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BARGAINING IN THE SHADOW OF THE LAW: DIVORCE LAWS AND FAMILY DISTRESS

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ABSTRACT

Over the past thirty years changes in divorce law have significantly increased access to divorce. The different timing of divorce law reform across states provides a useful quasi-experiment with which to examine the effects of this change. We analyze state panel data to estimate changes in suicide, domestic violence, and spousal murder rates arising from the change in divorce law. Suicide rates are used as a quantifiable measure of wellbeing, albeit one that focuses on the extreme lower tail of the distribution. We find a large, statistically significant, and econometrically robust decline in the number of women committing suicide following the introduction of unilateral divorce. No significant effect is found for men. Domestic violence is analyzed using data on both family conflict resolution and intimate homicide rates. The results indicate a large decline in domestic violence for both men and women in states that adopted unilateral divorce. We find suggestive evidence that unilateral divorce led to a decline in females murdered by their partners, while the data revealed no discernible effects for men murdered. In sum, we find strong evidence that legal institutions have profound real effects on outcomes within families.

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1. Introduction

In 1969, then Governor Ronald Reagan signed a bill creating unilateral divorce in California. This legislative change was one of the first in a series that increased access to divorce across the nation. In the ensuing decade, most states followed California's lead; although the specific family law reforms varied in each state, the end result was legislation that allowed unilateral divorce. In other words, in many states it became possible for a married person to seek the dissolution of their marriage without the consent of their spouse.

At the time, the legal changes that occurred were thought of more as a matter of procedural policy refinement, rather than a matter of social policy.[1] Despite the lack of intent, this legislative change has been an important force in altering social norms and perceptions about marriage and family. Consequently, divorce law has become an issue of social concern, with some states in recent years revisiting their earlier reforms, asking if perhaps they went too far. Unfortunately, the recent debate over tightening access to divorce is occurring with little knowledge of the effects of the initial changes on adult well-being.[2]

The existing work directly examining the effects of divorce law changes suggests that the reforms begun in 1969 may have caused divorce rates to rise.[3] Further, divorced people are known to exhibit a range of negative health and lifestyle characteristics, and the financial position of women typically deteriorates following a divorce.[4] These two facts have led some to argue that increasing access to divorce decreases well-being. However, there are several reasons why these arguments are misleading. First, the estimated correlation between divorce and poor

[1]Historical accounts of this legislative movement indicate that it was catalyzed by reformers who were interested in preserving the integrity of the legal system. The courts had become filled with cases involving fraudulent charges of adultery and abuse as spouses attempted to divorce when the state's laws did not provide for divorce any other way. Jacob (1988).
[2] For the effects on children, see Gruber (2000).
[3] Friedberg (1998), although Wolfers (2003) argues that these effects on the divorce rate were temporary.
[4] Holden and Smock (1991.

outcomes is unlikely to reflect a purely causal relationship.[5] Second, while divorce might be financially deleterious for women, introspection suggests that there are likely to be offsetting non-financial benefits (for at least one of the spouses). Further, there exists an important difference between the average divorce observed and the marginal divorces that are enabled by unilateral divorce (the latter being potentially welfare enhancing). Finally, such partial equilibrium assessments may be incomplete because unilateral divorce increases everyone's access to divorce, and even those who choose not to get divorced may be affected by the existence of this new option.

In the literature on the economics of the family there has been growing consensus on the need to take bargaining and distribution within marriage seriously. Such models of the family rely on a threat point to determine allocation within the household. The switch to a unilateral divorce regime redistributes power in a marriage, giving power to the person who wants out, and reducing the power previously held by the partner interested in preserving the marriage. Potentially, this may cause large changes in marital dynamics, whether or not there is an increasing tendency to actually exercise the divorce option. For instance, in a society in which people can leave abusive partners, spouses may be less likely to be abusive.

This paper exploits the variation occurring from the different timing of divorce law reforms across the United States to evaluate changes in suicide, domestic violence, and spousal murder rates in an attempt to measure some of the important effects of the "no-fault revolution." More specifically, we are examining suicide rates in an attempt to find a quantifiable measure of well-being. While variation in suicide rates only reflects changes at the extremes of the distribution, Di Tella, MacCulloch and Oswald (1997) show that aggregate suicide rates tend to co-move with other aggregate measures of subjective well-being. Family violence surveys conducted in the mid-1970s, and again in the mid-1980s, provide basic detail about domestic

[5] In fact, the causation may run the other way. For example, although alcoholism may result from divorce, it is also likely that alcoholics make undesirable spouses. Bedard and Deschenes (2003) present evidence

violence. Spousal murder rates are analyzed as a further quantifiable indicator of domestic violence.[6]

We find that states that passed unilateral divorce laws saw a large decline in both female suicide and domestic violence rates. Total female suicide declined by around 20% in states that adopted unilateral divorce. There is no discernable effect on male suicide. Our data on spousal conflict suggest that a large decline in domestic violence occurred in reform states. Furthermore, our results suggest a decline in women murdered by intimates, although the timing evidence is less supportive of this claim. As with suicide, there is no discernable effect on males murdered.

2. Mediating Forces: Divorce Rates and Bargaining within Marriage

Our analysis is concerned with changes in adult wellbeing occurring as a result of a shift to unilateral divorce (which permits divorce upon application by either spouse) from the pre-existing divorce laws (which typically required either the consent of both spouses or a demonstration of marital fault). There are two mechanisms through which a change in divorce law regime may affect indicators of spousal well-being. The first is by affecting the divorce rate. This direct mechanism traces the effects of easier access to divorce to higher divorce rates, through to the sorts of deleterious effects of divorce documented in the public health and sociological literatures.[7] If this were the only channel, then unilateral divorce laws would provide a useful instrumental variable for analyzing the adverse effects of divorce.

The second mechanism is by changing bargaining power and behavior within marriage. If the divorce regime affects the bargaining position of spouses in a way that changes intrafamily distribution then we expect to observe changes in spousal relations and wellbeing. Note that this mechanism may be important even if there is no effect of divorce regime on divorce rates.

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that the deleterious financial consequences of divorce are not robust to a more careful analysis of causation.

[6] Campbell (1992) provides evidence that domestic violence is a factor in most incidents of intimate homicide. Furthermore, existing estimates suggest that between one-quarter and one-half of women murdered are killed by their partner (Greenfeld et. al, 1998) making homicide of intrinsic interest.

[7] Waite (1995) documents a range of cross-sectional correlations between divorce and poor outcomes.

What can theory tell us about these two mechanisms? The first mechanism is mediated by rising divorce rates. Yet Becker (1981) argues that the Coase theorem applies to marital bargaining, and hence the divorce rate should be invariant to divorce regime. Under this view, unilateral divorce laws simply transfer a well-defined property right – the right to remarry – from the spouse who wants to remain married to the partner desiring a divorce. Efficient bargaining ensures that marriages only dissolve if continuing the marriage would be jointly sub-optimal, and this efficient bargain will obtain irrespective of the initial assignment of property rights. Recent empirical research suggests that liberalized divorce laws led to only a small and transitory rise in divorce that dissipated within a decade.[8]

The second channel reflects the impact of divorce laws on spousal bargaining over the distribution of marital rents. There are three canonical models of intrafamily distribution. The first is the *common preference* approach, which holds that families act *as if* maximizing a single utility function. This common preference can be motivated either by love (altruism, such that both spouses care equally about their own and their partner's satisfaction, as in Becker, 1981), or by the parties seeking to maximize a "social welfare function," agreed upon in a complete marriage contract.[9] The sharpest prediction of the common preference approach is that outcomes are invariant to the distribution of resources between spouses. By contrast, bargaining models hold that the presence of threat points determines intrafamily distribution. In the *separate spheres* bargaining model of Lundberg and Pollak (1993), these threat points are internal to the marriage. That is, the equilibrium distribution is maintained by the threat of reversion to a non-cooperative equilibrium involving, for example, burnt toast or sleeping on the sofa. Finally, the *exit threat* bargaining models of Manser and Brown (1980) and McElroy and Horney (1981) emphasize external threat points - specifically each party's best option outside the marriage. If this exit threat is binding, then changing opportunities outside the marriage will change the equilibrium

[8] See Wolfers (2003) which clarifies the interpretation of the results in Friedberg (1998).
[9] In the latter case the division of marital rents is agreed upon prior to the marriage.

distribution within the marriage. If the internal threat is binding, then such changes do not affect outcomes.[10]

To see how divorce laws affect the external threat point, note that prior to unilateral divorce, a partner wishing to dissolve the marriage could leave without their spouse's consent. However, in such a situation, a legal divorce is not granted and, as such, the right to remarry is forfeited. Under unilateral divorce the value of the exit threat increases for the unsatisfied spouse, as the right to remarry is retained regardless of the position of one's spouse. Thus, the exit threat model predicts that changes in divorce regimes will have real effects. If the divorce threat is sufficiently credible, it may directly affect intrafamily bargaining outcomes without the option ever being exercised. That is, there may be profound changes not mediated by higher divorce rates, and hence, unilateral divorce laws cannot be considered a valid instrument for divorce. Consequently, we estimate reduced form regressions that represent the effect of divorce regime on suicide, domestic violence, and spousal homicide.

Although theory predicts that real effects can flow from divorce reform, signing these effects is much harder. An increase in access to divorce could decrease suicide rates simply because suicide and divorce might be substitutes. That is, while the misery of living in an abusive or otherwise unhappy relationship may result in suicide, the option of divorce and remarriage may avert this course of events.[11] Alternatively, rising divorce or the threat of divorce may increase the number of unhappy or abandoned spouses, raising the temptation of suicide.

Similarly, domestic violence may decrease because the threat to leave if abused becomes credible under the unilateral divorce framework. If the abuser wishes to continue the marriage then this threat may be sufficient to prevent abusive behavior. Note that this change in behavior results from the change in bargaining power and, as such, can occur without any observed change

[10] As Lundberg and Pollak acknowledge both threat points may be possible, and therefore the relevant threat point is determined by institutional frameworks and individual utility functions (1993, p. 1001).
[11] For an analysis of suicide that emphasizes the option value of staying alive, see Hamermesh and Soss (1974).

in divorce propensities. Finally, most spousal homicides occur in the context of abusive relationships,[12] and hence any policy that reduces the barriers to exiting such a relationship reduces the probability of both abuse and spousal homicide.

Countering these forces, there are several reasons why unilateral divorce may raise spousal violence. The first is that without a legal system that enforces the marriage contract, individuals may substitute private for public enforcement of their marriage contracts. Under the consent divorce framework, spouses "owned" each other, and this ownership was enforced by the state through legal sanction. Under unilateral divorce this perceived property right is threatened; hence, there exists the possibility that private enforcement, through violence, will substitute for state sanctions.[13] The resulting increase in domestic violence may also lead to an increase in murder. Finally, the intense emotional distress and personal tumult associated with divorce proceedings might provoke an increase in domestic violence and murder. That is, unilateral divorce, in so much as it leads to an increase in the number of highly charged divorces, creates more violent situations.[14]

3. Empirical Strategy

We follow Friedberg's (1998) coding of state divorce regimes and the dates of divorce reforms. It should be noted that there are actually degrees of unilateral divorce, in that legislation might allow unilateral divorce conditional upon a separation period. We code states both with and without separation requirements as unilateral divorce regimes.[15]

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[12] Campbell (1992).

[13] Miron (1998) makes this argument in terms of drug contracts, arguing that when the state refuses to enforce such contracts private enforcement mechanisms such as gang violence will replace court sanctions.

[14] The study that is closest in spirit to the present paper is Gillis' (1996) assessment of the effects of a previous liberalization of divorce law which introduced divorce in mid-19th century France. The reform allowed judicial separation to be granted in response to adultery or the threat or occurrence of serious violence. Gillis analyses time series data on divorce and "deadly domestic quarrels," finding that the overall effect throughout this period was a decline in murder rates but a small increase in "spontaneous" murders.

[15] Around one-third of states have separation requirements, ranging from six months to five years.

Of the fifty states, five are yet to adopt any form of unilateral divorce: Arkansas, Delaware, Mississippi, New York and Tennessee. Of the forty-six states that currently have unilateral divorce regimes, nine had adopted some variant of unilateral divorce before the no-fault revolution of the early 1970s. Along with the thirty-seven remaining states we include the District of Columbia, which adopted unilateral divorce in 1977. Consequently, we effectively have thirty-seven "experiments" of changing divorce laws. The remaining fourteen states are included as controls. Table 1 lists the year that each state changed its divorce regime to allow unilateral divorce.

We use the natural variation resulting from the different timing of the adoption of unilateral divorce laws across states to estimate the effects of these laws on murder, suicide, and domestic violence rates for men and women independently. Consequently, we use state-based panel estimation, including both state and time fixed effects in all regressions. A dummy variable indicating whether the state currently has a unilateral divorce regime is our variable of interest. The dependent variable is the annual suicide, domestic violence, or murder rate. Where possible, we report our coefficients as elasticities (evaluated at the unweighted cell mean). That is, the reported results are interpreted as the percentage change in the relevant rate stemming from the change to unilateral divorce. Appendix A provides summary statistics.

4. Suicide Results

Data on suicide comes from the National Center for Health Statistics (NCHS).[16] The NCHS data are a census of death certificates, which code the cause of death for all deceased persons. There are broad codes for suicide, as well as a more detailed coding structure that includes data on the method of suicide. Individual data on gender, state of residence, and age of

[16] Suicide data for 1964-1967 were hand entered from annual editions of the NCHS report "Vital Statistics: Mortality, Vol.2". Data for 1968-78 are calculated from ICPSR Study No. 8224, "Mortality Detail Files: External Cause Extract, 1968-78", PI: National Center for Health Statistics. Data from 1979-96 have been downloaded from the Center for Disease Control's Wonder system which accesses the NCHS "Compressed

death are also collected.[17] By examining the period from 1964 through to 1996, we can both robustly identify suicide rates before the adoption of unilateral divorce laws, and trace their evolution over the following years.

Note that the dependent variable is the suicide rate of *all persons*, not just those who have been married. We analyze this variable both because of data limitations (the NCHS begin coding marital status in 1978) and to avoid endogeneity problems posed by the possibility that marriage decisions may respond to divorce regime.

We employ OLS to estimate:

$$Suicide\ rate = \sum_k \beta_k Unilateral_{s,t}^k + \sum_s \eta_s State_s + \sum_t \lambda_t Year_t + Controls_{s,t} + \varepsilon_{s,t}$$

where $Unilateral^k$ refers to a series of dummy variables set equal to one if a state had adopted unilateral divorce *k* years ago. Thus the results, shown in Table 2, map out the full dynamic response of the suicide rate to the law change.

There is a large and statistically significant reduction in the female suicide rate following the change to unilateral divorce. Further, this effect seems to grow as the effects of divorce law reform percolate through society, presumably reflecting couples learning about the new law, social norms about the family adjusting, and spouses starting to understand their new rights. Averaging the effects over the twenty years following reform suggests an aggregate decline of 5-10%. For male suicides, these estimates indicate no discernible effect.

We test the sensitivity of our specification to a range of controls, including a proxy for the evolving economic power of women (the ratio of male-to-female employment rates), indicators of the business cycle (state income per capita and unemployment), welfare generosity (the maximum AFDC payment for a family of four, and the share of the state population on the welfare rolls), the availability of abortion, and the racial and age composition of the state. While

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Mortality Files" (http://wonder.cdc.gov/). Apart from minor revisions to the International Classification of Diseases, these data are consistently coded.

[17] Our population data, downloaded from www.census.gov, are not coded by gender; the evolution of gender shares in each state are imputed from the March CPS files (for the population aged 14 or over).

we find that some of these controls are significant explanators of the suicide rate, column 2 shows that they barely change the estimated effect of unilateral divorce. This column represents our preferred estimate, suggesting an average decline in female suicide of around eight percent, and a long run decline that is perhaps double that. Weighted least squares results are also broadly similar (column 3). We add state-specific time trends in column 4, finding that their inclusion causes the standard errors to increase. For women, the specification including state-specific time trends is not precisely estimated enough to reject either a null that the pattern of coefficients follows that shown in columns 1-3, or a null of no effect. The point estimates, while smaller, are still large. For males, including state-specific trends is suggestive of a decline in male suicide rates following the advent of unilateral divorce.

In further robustness testing (not shown), we ran each of our baseline regressions omitting in turn individual states or years, finding that particular states or years do not unduly influence our results. Robust estimation procedures, including median regression, yielded similar results. Further, while OLS implicitly gives equal weight to each of our thirty-seven divorce reform experiments, we also found similar results using population-weighted least squares and generalized least squares. We also experimented with the control group, dropping those states that did not change their laws from the estimation. We found that estimating off only the variation due to the different timing of reform was sufficient to identify the noted large decline in female suicide. This specification was also suggestive of a decline in male suicide.

Timing evidence might speak to a causal interpretation of these results. We are particularly interested in whether the change in suicide post-dated the change in divorce regime, and whether adjustment to the new regime seems plausible. In order to examine these potential lags, we added a series of leads to our preferred specification, coding dummies for whether unilateral divorce will become law in 1-2 years, 3-4 years and so on, with leads beyond 10 years coded to the 9-10 year group. The leads are included to check if the timing of the decrease in suicide rates corresponds with the change to unilateral divorce. The estimated coefficients are shown in Figures 1 and 2 (the graphs are normalized so that the pre-change effects are centered on zero).

Firstly, note that the coefficients on the dummies indicating the period prior to the divorce law reform are all close to zero, and in no case are they (individually or jointly) statistically distinguishable from zero. This speaks clearly to a causal interpretation of these results. Secondly, reinforcing our baseline results, the graphs show that there was a large and statistically significant decrease in female suicide rates, and no discernable affect on male rates. The smooth and plausible shape of the response of female suicide suggests that our treatment effect is reasonably well identified. The graph illustrates that the full effects of the change in divorce laws take almost twenty years to percolate through society.

While the death certificate data do not continuously code marital status, we can disaggregate our main results by age.[18] For brevity we only show the results for women because even disaggregating by age, we still find no effects for men. Figure 3 shows our results by age group for women, mapping the estimated response of suicide rates following the change in divorce regimes, where the analysis in Figure 1 is repeated separately for women in each of eleven different age groups. These age groups comprise unequal shares of the population, and so in each case coefficients are scaled by their share of the US population, allowing these figures to be added to yield the aggregate effect (shown in the bottom right panel). For teens, the effect is a relatively precisely estimated zero, reflecting both the lack of correlation between teen suicide and divorce laws and the relatively small number of teen suicides. The second row of Figure 3 shows that prime-age women account for the bulk of the main effect, with unilateral divorce substantially reducing the suicide rates of women in each of the age groups from 25-65. Turning to the elderly, it appears that unilateral divorce laws had little effect on suicide decisions, although there may be some impact on women aged 65-74 (these estimates are sufficiently imprecise as to be consistent with either no effect or a meaningful decline). The bottom right panel shows that adding the effects across the eleven panels yields a set of estimates that are consistent with those shown in Figure 1, suggesting that these initial aggregate results are robust to the inclusion of age-specific state and year effects (and to the interaction of age with the other controls). Overall the observed correlation between the adoption of unilateral divorce and the decline in female suicide seems extremely robust, and we can be confident that neither youth nor the elderly drive the observed correlation between female suicide and divorce regime – a result that is broadly consistent with the causal interpretation we offer below.

5. Interpretation

It is useful to think about the role that divorce itself is playing in generating these observed changes in suicide rates. Two interpretations seem particularly relevant. The first is that the reduction in female suicide reflects both women escaping from bad marriages and the redistribution of power within marriages that results from increased access to divorce. Under this interpretation unilateral divorce has both direct effects on bargaining within marriage, and effects that are mediated through increased divorce.

A more restrictive interpretation is that our results are simply the reduced form representation of an instrumental variables regression in which unilateral divorce laws are an instrument for higher divorce rates. This IV interpretation assumes that the decrease in female suicide is solely the result of higher divorce rates and does not reflect bargaining within marriage. With no direct evidence as to the presence or absence of such a channel, we are reluctant to embrace this more restrictive interpretation. Further, the indirect evidence that we have speaks, albeit weakly, against this view. Specifically, note that in Figure 1 there is no immediate spike in

[18] We are grateful to an anonymous referee for suggesting this analysis.

suicide following the regime change. By contrast, a spike in divorce typically follows a shift in regimes – as the courts cater to pent-up demand for unilateral divorces.[19]

Finally, Wolfers (2003) casts doubt on Friedberg's (1998) assessment of the relationship between divorce laws and divorce rates. Friedberg's result relies upon the inclusion of state-specific time trends that are calculated on data that include only a few years of data prior to the legal change and, in some cases, only one year. As a result the estimated state-specific time trends reflect the dynamic response of the divorce rate to the regime change, rather than pre-existing divorce trends in each state. By partialling out a downward trend in divorce in unilateral divorce states, Friedberg finds an artificially large rise in divorces following the legal change.[20] When these trends are omitted, or are calculated so as to reflect only pre-existing trends in a state's divorce rate, Wolfers concludes that the adoption of unilateral divorce caused the divorce rate to be higher for a decade, and then lower in the ensuing decade. By contrast, Figure 1 suggests that suicides decline steadily over the twenty years following divorce law reform. The short-term rise in divorce and the steady decline in suicide seem somewhat difficult to reconcile if divorce is the main mediating mechanism. These interpretation issues remain relevant as we now turn our attention to the relationship between unilateral divorce and intimate homicide and domestic violence.

[19] This heuristic argument is equivalent to the type of argument in formal over-identification tests. The first stage regression is formally over-identified in that we have separate instruments for a rise in divorce based on dummies of whether the legal change occurred in the last two years, 3-4 years ago etc. The heuristic argument is that the large change in divorce generated in the first two years after a regime change yields only a small decline in suicide rates, a result that is difficult to reconcile with smaller changes in divorce rates in ensuing years that yield much larger effects on suicide. Of course, appealing to a specific constellation of lags between the rise in divorce and the decline in suicide can rationalize this result away.

[20] The estimated trend is negative in unilateral divorce states relative to the controls because the divorce rate spikes up immediately following the change in regime, and then declines smoothly, asymptoting toward a smaller long-run effect.

6. Domestic Violence

The most credible cross-state data on domestic violence are the landmark Family Violence Surveys undertaken by sociologists Straus and Gelles in 1976 and again in 1985.[21] These data come from household interviews that ask how couples resolve conflict.[22] This type of survey instrument typically yields higher estimates of domestic violence than police reports or crime victimization surveys because the victim need not perceive the act as domestic violence and/or a crime for it to be recorded. While still an imperfect survey instrument, Markowitz (1999, p.8) argues that this methodology is currently "the best available technique for collecting truthful information on domestic violence."

The two available surveys yield cross-sectional data for 1976, by which time thirty-one states had recently changed their divorce laws, and again for 1985, by which time all thirty-seven regime changes identified in Figure 1 had occurred. This timing is somewhat unfortunate in that it is unclear how the differential timing of reform across states would translate into differential changes in domestic violence rates over the 1976-85 period. Yet, although the differential cross-state timing in reform yields little analytical leverage, we can compare changes in violence rates among our thirty-seven states that constituted the "no-fault revolution" with two alternative control groups: the five states that are yet to adopt unilateral divorce (AR, DE, MS, NY and TN), and the nine states whose pre-existing regime involved unilateral divorce (AK, LA, MD, NC, OK, UT, VA, VT and WV).[23] If there is an underlying relationship between domestic violence and divorce regime, we would expect to observe changing violence propensities in the treatment group relative to the controls. Because the survey universe consists only of couples living in a conjugal unit, we are limited to analyzing rates of domestic violence within intact marriages.

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[21] Crime victimization survey data both lack state identifiers and are not available for the relevant time period. Police reports suffer both from serious problems of under-reporting and, more importantly, changes in social norms regarding reporting over the relevant time period.

[22] Murray A. Straus and Richard J. Gelles "Physical Violence in American Families". The 1976 and 1985 surveys are ICPSR studies 9211 and 7733, respectively.

Thus, we cannot disentangle whether the estimated effects reflect a decreasing propensity towards spousal violence, or an increasing propensity for abused spouses to exit their marriages.

Table 3 shows illustrative differences-in-differences estimates of the effects of unilateral divorce on domestic violence.[24] The first row of Panel A tells us that between 1976 and 1985 domestic violence towards women declined by 1.7 percentage points in reform states, while it rose 2.5 percentage points in the control states. Thus, the difference-in-difference estimate suggests that the treatment—adoption of unilateral divorce—led domestic violence rates to decline by 4.3 percentage points, or by around one-third, over the 1976-85 period. Panel B shows a slightly smaller, statistically insignificant decline in wife-to-husband violence. These magnitudes are clearly important and lead us to the micro-data to probe this result more intensively.

Table 4 analyzes household-level data in which the dependent variable, *Domestic Violence*, is a dummy indicating whether the specified type of violence occurred within each household. We estimate the micro-data analog of our differences-in-differences estimate:

$$Domestic\,Violence_{i,s,t} = \beta(Treatment_s \times Year_t^{1985}) + \delta Treatment_s + year\ effects_t\ (+state\ effects_s + controls_i) + \varepsilon_{i,s,t}$$

where *Treatment* is a dummy variable that is equal to one if the state is coded in Table 1 as a reform state, and is zero otherwise.

The first row of Table 4 shows the mean rates of violence across households. Perhaps surprisingly, men are as likely to be physically abused by their spouses as women are. The next row simply reproduces the differences-in-differences estimates from Table 3, for each of the four categories of spousal abuse. Extremely large declines in violence are found for each abuse indicator. Adding state fixed effects in the next row sharpens these estimates somewhat, and

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[23] The 1976 survey did not sample from all states, and hence we are forced to omit the following states from our analysis: AK, AR, DC, DE, HI, IA, KY, MA, ND, NH, NM, NV, RI, SD, WY.

[24] The definition of domestic violence follows Gelles and Straus (1994). That is, we code domestic violence as occurring if there has been any incident over the last year in which a person threw something at

these large effects are all found to be statistically significant. The following four rows show that these results are robust to the inclusion of state fixed effects, a rich set of individual-level controls, the set of within-state time-varying economic and social policy controls shown in Table 2, and also the use of a probit estimator. Further, dropping specific states from the sample did not appreciably change these results.

Comparing these declines in violence rates with their base rates, domestic violence appears to have declined by somewhere between a quarter and a half between 1976 and 1985 in those states that reformed their divorce laws during the "no-fault revolution." We now turn to an alternative indicator of domestic violence—intimate homicide—to further probe the robustness of these results.

7. Intimate Homicide

Our data on homicide come from the FBI Uniform Crime Reports (UCR).[25] The UCR data are derived using a voluntary police agency-based reporting system. The Supplementary Homicide Reports of the UCR provide *incident-level* information on criminal homicides, including data describing the date and location of the incident, as well as a range of information on both the offender and the victim. The particular richness of this data is that it codes the relationship of the victim to the murderer, where known.

Because the FBI data rely on police reporting, there are often problems of under-reporting or downgrading of crimes. However, the nature of homicide means that both of these problems are minimized. The FBI counts of total murders each year by state were checked against the independently gathered NCHS murder count. Generally, these two data sources were

their partner, pushed, grabbed, shoved, slapped, kicked, bit, hit with fist, hit or tried to hit with object, beat up, or threatened or used a gun or knife against their partner.

[25] Data for 1968-75 are from ICPSR Study No. 8676, "American Homicide, 1968-1978: Victim-Level Supplementary Homicide Reports", Marc Riedel and Margaret Zahn (1994). Data for 1976-94 are extracted from ICPSR Study No. 6754, "Uniform Crime Reports [United States]: Supplementary Homicide Reports, 1976-1994", James Alan Fox (1996). The consistency of these data is discussed in Appendix B.

consistent, and hence the rest of our analysis uses the FBI data,[26] which include their coding of victim-perpetrator relationships.

Nonetheless, there remains a range of problems when working with these data. First, the participation of agencies is not completely consistent, and when an agency fails to report in a particular month, we cannot tell whether this reflects laxity with paperwork or that there were no murders to report.[27] Second, there are various coding breaks arising from the changing definitions of victim-perpetrator relationship, causing a minor break in 1972, and a more important break in 1976. These coding breaks present a problem for our analysis because, conceptually, we would like to capture any relationship that may be affected by changes in family law. Such relationships include, along with spouses, domestic and non-domestic romantic partners and other family members (particularly children). However, there are data problems constructing such a series that is consistent across coding breaks.[28] In this section we will examine three successively broader definitions of intimate homicide. The narrowest only includes spousal homicide, the next group includes homicides committed by any family member or romantic interest, and finally we expand our treatment group to our broadest categorization, which includes all homicides committed by non-strangers.

The defect of the broader measures is that the treatment group is defined to include many relationships that are not affected by the treatment of unilateral divorce. The defect of narrower measures is that police classifications of victim-perpetrator relationships as "spousal" are likely to have changed over time, in a way that is correlated with family law regimes, leading to (difficult

[26] The FBI data for Illinois are quite different from the Death Certificate data. Looking closely at the data, we find that the Chicago Police Department failed to report any murders in 1984, 1985, November 1986-May1987, July 1987-December 1987 and July 1990-December 1990. It is implausible that there were no murders in these periods, and hence we believe the FBI data to be wrong. Thus we omitted Illinois from our homicide samples.

[27] When there are no data for an entire state, for a whole year, this could reflect either that the state was not participating in the reporting program, or that there were no murders in that state-year. We assume non-participation when a zero murder count would lie outside a three-standard error confidence band for that state, and infer a number by linear interpolation. Otherwise we assume a zero murder count. These adjustments affect 37 of our 2754 state-year-sex observations.

[28] In Appendix B we outline our attempts to construct consistent series.

to sign) bias issues.[29] Further, identifying intimates narrowly, such as by “spouses”, is more likely to suffer from endogeneity problems as the legal status that people choose for their relationships may change with changes in the legal regime.

For women murdered, Table 5 suggests a large and significant decline following the adoption of unilateral divorce for all three definitions of intimate homicide, with column 1 suggesting declines on the order of around 10 percent. Column 2 shows that this estimate is robust to adding a rich set of controls, including not only the economic, social policy and demographic variables previously considered, but also a set of criminal justice variables including a death penalty indicator, Donahue and Levitt’s Effective Abortion Rate, and the share of the state’s population in prison population rate, lagged one year.

The results for males murdered are imprecisely estimated and would admit large effects in either direction. The estimates change substantially across different definitions of intimate homicide, and adding controls leads to moderate changes in the estimates.[30]

As with the suicide data, timing evidence might assist us in interpreting our results. Therefore, we once again replace the single dummy variable *Unilateral* in the baseline model with several dummy variables indicating the number of years since (or until) the law went (goes) into effect. We run this regression for all three categories of intimate homicide. The estimated coefficients for females murdered are shown in Figure 4. For clarity, standard error bands are not shown, but as a rough indicator, estimated standard errors for each lead, or lag, plotted are around twice that shown in the corresponding row of Table 5. The imprecision with which we estimate

[29] While the coding of married partners as “spouses” presents no difficulty, coding of common-law marriages, cohabiting couples, same-sex couples, romantic partners and separated spouses is likely to have changed over time. Although these groups may be small compared to the whole population, we do not know if this is true of the homicidal population. All that is known with certainty is that a homicidal member from one of the above groups would not have been coded as a stranger, which is the motivation for looking at the broadest of our definitions of the treatment group.

[30] Thomas Dee (2003) has also analyzed these data, employing count data methods on a short (1968-1978) panel. He finds a large increase in males murdered by their spouses. His paper contains a reconciliation of his results with ours, which largely turns on his shorter sample period, coding of intimate homicide, and functional form. As we shall see, the effects on men appear to be extremely sensitive to small changes in specification, undermining our confidence in any estimate.

effects on males murdered is sufficiently large that we omit them from the rest of the analysis of intimate homicide.

Figure 4 confirms the initial findings of a decrease in women murdered in the period following the passage of divorce law reforms. However, the timing evidence is somewhat worrying, and the reader is left to judge whether the decline in homicide pre-dated the law change to an extent that undermines our results. This raises the possibility that our regression results may be picking up the effects of some alternative phenomenon that pre-dated divorce law reform.

The fact that family law affects behavior between intimates but not between strangers provides an opportunity to further probe these results. Specifically, *non*-intimate homicide may serve as an ideal placebo group. Column 3 of Table 5 shows the differences-in-differences (panel) estimates for the non-intimate homicide placebo group (that is, the dependent variable is the aggregate homicide rate, less the relevant definition of intimate homicide). These results suggest that there is a negative correlation between non-intimate homicide and divorce laws, albeit not a statistically significant one. These results also give us a chance to assess an alternative counterfactual: instead of assuming that, in the absence of divorce reform, intimate homicide would remain unchanged (as in the first two columns), the differences-in-differences-in-differences in column four assumes that the change in non-intimate homicide is the relevant baseline. These triple difference estimates suggest that intimate femicide declined when compared with this counterfactual, but that this difference is not statistically significant. For men, the estimates remain both imprecise and sensitive to changes in definition. Finally, other crime measures provide a further set of interesting placebos, and these results (shown in the bottom panel of Table 5) generally show little correlation between state crime trends and divorce laws.

8. Conclusion

Our analysis examines indicators of adult well-being following a regime shift to unilateral divorce from the pre-existing divorce laws. We have attempted to measure specific changes in family distress resulting from the radical changes made over the past thirty years to divorce laws. These changes led to one spouse being able to obtain a divorce without his or her partner's consent. Examining state panel data on suicide, domestic violence, and murder, we find a striking decline in female suicide and domestic violence rates arising from the advent of unilateral divorce. Total female suicide declined by around 20% in the long run in states that adopted unilateral divorce. We believe that this decline is a robust and well-identified result, and timing evidence speaks clearly to this interpretation. There is no discernable effect on male suicide.

Data on conflict resolution reveal large declines in domestic violence committed by, and against, both men and women in states that adopted unilateral divorce. Furthermore, we find suggestive evidence of a decline in females murdered by intimates, although the timing evidence makes this a more suspect result. As with suicide, there is no discernable effect on males murdered, although this reflects the imprecision and volatility of our estimates.

While our results are open to the interpretation that the large declines identified are the result of changing divorce rates, we believe that this is only part of the story. Indeed it is difficult to reconcile the timing of these outcomes with the response of the divorce rate to these reforms. A more complete story takes changes in marital dynamics into account. Unilateral divorce changed the bargaining power in marriages and therefore impacted many marriages– not simply the extra few divorces enabled by unilateral divorce. Speculating on the policy implications of emerging models of the family, Lundberg and Pollak (1993, p.992) argued that the possibility of "the dependence of intrafamily distribution on the well-being of divorced individuals provides a mechanism through which government policy can affect distribution within marriage." The mechanism examined in this paper is a change in divorce regime and we interpret the evidence collected here as an empirical endorsement of the idea that family law provides a potent tool for affecting outcomes within families.

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Appendix A: Summary Statistics

	Mean	Standard Deviation	Min.	Max.	*n*
<u>Suicide Rates</u>					
Female Suicide Rate (Suicides per million women in the state per year)					
Total	54.4	18.8	9.2	183.4	1683 state-years
within states		12.0			33 years (1964-96)
Male Suicide Rate (Suicides per million men in the state per year)					
Total	202.2	53.5	74.5	435.4	1683 state-years
within states		28.0			33 years (1964-96)
<u>Homicide Rates: FBI Count</u>					
by Spouses					
Women killed by spouses per million women in the state each year					
Total	7.3	4.7	0.0	36.9	1323 state-years
within states		3.4			27 years (1968-94)
Men killed by spouses per million men in the state each year					
Total	5.5	5.4	0.0	43.9	1323 state-years
within states		3.7			27 years (1968-94)
by Family Members					
Women killed by intimates per million women in the state each year					
Total	14.7	7.9	0.0	63.2	1323 state-years
within states		5.2			27 years (1968-94)
Men killed by intimates per million men in the state each year					
Total	18.4	13.4	0.0	95.9	1323 state-years
within states		8.0			27 years (1968-94)
by Non-Strangers					
Women killed by non-strangers per million women in the state each year					
Total	21.2	10.9	0.0	87.5	1323 state-years
within states		6.6			27 years (1968-94)
Men killed by non-strangers per million men in the state each year					
Total	56.9	35.7	0.0	178.2	1323 state-years
within states		15.3			27 years (1968-94)
All Murders					
Women Murdered per million women in each state each year					
Total	32.8	16.6	0	134.9	1323 state-years
within states		9.3			27 years (1968-94)
Men Murdered per million men in each state each year					
Total	92.6	56.2	0	314.4	1323 state-years
within states		22.6			27 years (1968-94)

Appendix A Continued

	Mean	Standard Deviation	Min.	Max.	*n*
Domestic Violence#					
Incidence of Overall Husband-to-wife abuse per hundred couples in each state each year					
Total	13.0	9.1	0.0	66.7	72 state-years
within states		6.3			2 years (1976, 1985)
Incidence of Severe Husband-to-wife abuse per hundred couples in each state each year					
Total	4.2	4.2	0.0	25.0	72 state-years
within states		3.1			2 years (1976, 1985)
Incidence of Overall Wife-to-husband abuse per hundred couples in each state each year					
Total	12.7	9.0	0.0	66.7	72 state-years
within states		6.6			2 years (1976, 1985)
Incidence of Severe Wife-to-husband abuse per hundred couples in each state each year					
Total	5.5	6.8	0.0	50.0	72 state-years
within states		4.7			2 years (1976, 1985)
Unilateral Divorce Regime (=1 if unilateral, 0 if consent divorce)					
Total	.69	.46	0	1	1683 state-years
within states		.38			33 years (1964-96)

* Homicide data excludes IL (missing data), DC (outlier)

Domestic violence data excludes AK, AR, DC, DE, HI, IA, KY, MA, ND, NH, NM, NV, RI, SD, WY due to missing observations in 1976 survey. For definitions of severe and overall abuse, see Table 5.

Appendix B: Coding of FBI Murder Data

Our data on homicide come from the FBI Uniform Crime Reports (UCR).[31] The UCR data are derived using a voluntary police agency-based reporting system. The Supplementary Homicide Reports of the UCR provide *incident-level* information on criminal homicides, including data describing the date and location of the incident, and a range of information on both the offender and the victim. This data codes the relationship of the victim to the murderer, where known. We would like to be able to look exclusively at murders that may be affected by unilateral divorce. Therefore we are looking for cases in which the perpetrator maybe motivated by the laws pertaining to divorce – intimate homicides. A useful definition of the treatment group should include intimates such as spouses, ex-spouses, other partners, and other family members. However, relationships such as common-law spouse, boyfriend/girlfriend, and even ex-spouse are not consistently coded through our sample. While time-fixed effects will effectively difference out inconsistencies that are common across states, we are worried that family law reform may have changed the common meanings of certain definitions of the treatment group. (For instance the distinction between marriage, common-law marriage and live-in partners has changed with the social meaning of these terms, which has in turn been affected by family law.[32]) As such we can think of a bias/efficiency tradeoff. The most efficient strategy is based on a narrow definition of intimate homicide that includes only spouses, but runs the risk that this category is not well-defined through the sample period. At the other extreme, perhaps the safest identification strategy is to assume that the legal regime did not affect murder between strangers, but did affect murder where the victim is known to the murderer – in any capacity.

We employ three definitions of the treatment group – ranging from that which we are most interested in (spousal murder) to definitions which are less likely to suffer from coding-induced biases (stranger/non-stranger). The definitions used are outlined in the following table.

Alternative definitions of the "Treatment Group"

Classification		Treatment group	Control group
Spouses			
	1968-72	"Spouse kills spouse"	All other*
	1972-75	"Spouse kills spouse"	All other*
	1976-94	"Husband", "Wife", "Common-law Husband", "Common-law Wife"	All other*
Family			
	1968-72	"Spouse kills spouse", "Parent kills child", "Child kills parent", "Other family situation", "Love Triangle"	All other*
	1972-75	"Spouse kills spouse", "Parent kills child", "Child kills parent", "Relative kills relative", "Other family situation", "Love Triangle"	All other*
	1976-94	"Husband", "Wife", "Common-law Husband", "Common-law Wife", "Mother", "Father", "Son", "Daughter", "Brother", "Sister", "In-law", "Stepfather", "Stepmother", "Stepson", "Stepdaughter", "Other family", "Boyfriend", "Girlfriend", "Ex-husband", "Ex-wife", "Homosexual relationship"	All other*
Known			
	1968-72	"Spouse kills spouse", "Parent kills child", "Child kills parent", "Other family situation", "Love triangle", "Money", "Revenge", "Other argument"	All other*
	1972-75	"Spouse kills spouse", "Parent kills child", "Child kills parent", "Relative kills relative", "Other family situation", "Love Triangle", "Argument/money", "Other arguments"	All other*
	1976-94	All other	"Stranger", "Unknown Relationship"

* Note that "All other" includes "Murder reason unknown", "Not stated", "Not coded" and "Unknown relationship".

[31] Data for 1968-75 are from ICPSR Study No. 8676, "American Homicide, 1968-1978: Victim-Level Supplementary Homicide Reports", Marc Riedel and Margaret Zahn (1994). Data for 1976-94 are extracted from ICPSR Study No. 6754, "Uniform Crime Reports [United States]: Supplementary Homicide Reports, 1976-1994", James Alan Fox (1996). The consistency of these data is discussed in Appendix B.

[32] An additional problem is that we cannot infer that the share of murderer-victim relationships that are coded are representative of those for which no code was recorded.

Table 1: Year of Introduction of Unilateral Divorce Laws, by State

Pre-existing Unilateral Divorce statutes (predate beginning of sample in 1964):

Alaska, Louisiana, Maryland, North Carolina, Oklahoma, Utah, Virginia, Vermont, West Virginia

States adopting Unilateral Divorce Laws:

1969	Kansas, South Carolina
1970	Iowa
1971	Alabama, Colorado, Florida, Idaho, New Hampshire, New Jersey, North Dakota
1972	Kentucky, Michigan, Nebraska
1973	Arizona, Connecticut, Georgia, Hawaii, Indiana, Maine, Missouri, New Mexico, Nevada, Oregon, Washington
1974	Minnesota, Ohio, Texas
1975	Massachusetts, Montana
1976	Rhode Island
1977	Washington DC, Wisconsin, Wyoming
1980	Pennsylvania
1984	Illinois
1985	South Dakota

Continuing Consent Divorce States (as of 1996):

Arkansas, Delaware, Mississippi, New York, Tennessee

Source: Friedberg (1998)

Table 2: Effects of Unilateral Divorce on Suicide Rates (% change)

	Female Suicides				*Male Suicides*			
Column No.	*(1f)*	*(2f)*	*(3f)*	*(4f)*	*(1m)*	*(2m)*	*(3m)*	*(4m)*
Year of Change	1.6%	1.3%	2.1%	2.2%	-0.8%	-1.4%	1.6%	-2.3%
	(3.8)	(3.4)	(4.3)	(3.2)	(2.2)	(2.1)	(1.5)	(2.1)
1-2 years later	-1.5%	-1.4%	3.0%	-0.4%	1.2%	0.5%	3.0%	-0.8%
	(3.7)	(3.5)	(3.4)	(3.7)	(1.5)	(1.4)	(1.2)	(1.6)
3-4 years later	-1.5%	-1.1%	0.8%	0.3%	0.0%	-0.9%	1.1%	-2.6%
	(3.1)	(3.1)	(2.5)	(4.0)	(1.6)	(1.5)	(1.2)	(1.8)
5-6 years later	-3.0%	-2.0%	-1.3%	-0.3%	0.4%	-0.2%	0.9%	-2.3%
	(2.9)	(2.9)	(2.5)	(4.2)	(1.5)	(1.5)	(1.3)	(2.1)
7-8 years later	-8.0%	-6.6%	-5.0%	-4.8%	-1.0%	-1.3%	0.7%	-4.0%
	(3.0)	(3.0)	(2.7)	(4.8)	(1.8)	(1.8)	(1.3)	(2.5)
9-10 years later	-10.0%	-8.5%	-8.5%	-6.2%	-3.5%	-3.9%	-0.7%	-6.8%
	(3.0)	(3.0)	(2.6)	(5.3)	(1.7)	(1.7)	(1.3)	(2.8)
11-12 years later	-11.9%	-10.2%	-5.6%	-7.0%	-2.2%	-2.6%	0.5%	-5.6%
	(3.1)	(3.2)	(2.3)	(6.1)	(2.0)	(2.0)	(1.3)	(3.3)
13-14 years later	-12.8%	-11.1%	-9.6%	-7.3%	-3.2%	-3.6%	1.4%	-7.0%
	(3.2)	(3.1)	(2.9)	(6.9)	(2.0)	(2.0)	(1.4)	(3.6)
15-16 years later	-13.3%	-11.7%	-10.1%	-7.3%	-1.6%	-2.0%	1.3%	-5.6%
	(3.7)	(3.6)	(2.7)	(7.7)	(2.0)	(1.9)	(1.5)	(4.0)
17-18 years later	-16.4%	-13.9%	-14.5%	-9.5%	-1.6%	-1.9%	1.7%	-5.7%
	(3.6)	(3.6)	(3.0)	(8.7)	(2.1)	(2.0)	(1.6)	(4.5)
≥19 years later	-18.7%	-16.4%	-19.3%	-11.2%	-3.9%	-4.3%	-3.2%	-8.5%
	(3.2)	(3.3)	(2.9)	(9.6)	(2.0)	(2.0)	(1.5)	(5.0)
Mean Suicide Rate	54 suicides per million women				202 suicides per million men			
Average effect over the 20 years following divorce law reform	-9.7%	-8.3%	-7.0%	-5.4%	-1.5%	-2.0%	0.7%	-4.9%
	(2.3)	(2.3)	(1.9)	(5.6)	(1.3)	(1.3)	(1.0)	(2.8)
F-test of joint significance	p=0.00	p=0.00	p=0.00	p=0.82	p=0.36	p=0.37	p=0.00	p=0.35
Estimation method	OLS	OLS	WLS	OLS	OLS	OLS	WLS	OLS
<u>Control variables</u>								
State and year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Economic, demographic and social policy controls[#]		✓	✓	✓		✓	✓	✓
State-specific time trends				✓				✓

Sample 1964-1996, n=1683.

Dependent variable is the aggregate state suicide rate by year. Coefficients are reported as the percentage change in the suicide rate due to the adoption of unilateral divorce laws the stated number of years ago; this elasticity is calculated using the unweighted cell mean as the base. Robust standard errors are in parentheses.

[#] Controls include the maximum AFDC rate for a family of four, the natural log of state personal income per capita, the unemployment rate, the female-to-male employment rate, age composition variables indicating the share of states' populations aged 14-19, and then ten-year cohorts beginning with age 20 up to a variable for 90+, and the share of the state's population that is black, white and other. (Employment status, age and race data are constructed from Unicon's March CPS files, and refer to the population aged 14 years or greater.)

Table 3: Differences-in-Differences: Effects of Divorce Reform on Domestic Violence

Panel A: Husband to Wife Violence

	1976	1985	Difference *(1985-1976)*
Treatment *(Adopted Unilateral Divorce)*	12.8% (0.9)	11.1% (0.7)	-1.7 (1.1)
Control *(No regime change)*	10.0% (1.0)	12.6% (1.1)	+2.5 (1.5)
Difference *(Treatment-Control)*	+2.8** (1.4)	-1.5 (1.3)	**-4.3**** **(1.9)** **[-36%]**

Panel B: Wife to Husband Violence

	1976	1985	Difference *(1985-1976)*
Treatment *(Adopted Unilateral Divorce)*	11.9% (0.9)	11.9% (0.5)	+0.0 (1.0)
Control *(No regime change)*	10.2% (1.0)	12.8% (1.0)	+2.7* (1.5)
Difference *(Treatment-Control)*	+1.8% (1.3)	-0.9% (1.2)	**-2.7** **(1.8)** **[-24%]**

Sample: n_{1976}=2102; n_{1985}=3874 (includes cross-section and state over-samples, excludes observations from states that are not present in the 1976 data; sampling weights are applied).

*, **, *** denote significance at the 10%, 5%, and 1% levels respectively.

(Robust standard errors in parenthesis; standard errors corrected for clustering within 72 state-year cells)

[Estimate of percent change in violence, in square brackets. ie coefficient evaluated at cell mean]

Dependent variable is a dummy variable set equal to one if the household reports a violent incident as having occurred between spouses over the preceding year, and zero otherwise. Following Gelles and Straus (1994), violent acts include any incident in which one spouse threw something at partner, pushed grabbed or shoved, slapped, kicked, bit, hit with fist, hit or tried to hit with something, beat up partner, threatened with gun or knife, or used a gun or knife.

Table 4: Effects of Unilateral Divorce on Domestic Violence

	Overall Violence[(a)]		Severe Violence[(a)]	
	Husband to Wife	**Wife to Husband**	**Husband to Wife**	**Wife to Husband**
	Average Incidence of Each Type of Violence			
	11.7%	11.9%	3.4%	4.6%
Estimated Change in Violence Rates in Treatment states relative to Control states				
OLS (Diffs-in-diffs)	-4.3%**	-2.7%	-1.1%	-2.9%***
	(1.9)	(1.8)	(1.3)	(1.0)
add state fixed effects	-5.5%***	-3.2%**	-2.0%**	-3.6%***
	(1.8)	(1.5)	(0.9)	(0.7)
add individual controls[b]	-4.8%***	-1.9%	-1.8%*	-3.4%***
	(1.7)	(1.4)	(1.0)	(0.9)
add state-level time-varying controls[c]	-3.8%**-	-1.8%	-1.8%	-3.0%***
	(1.8)	(1.3)	(1.0)	(0.7)
Probit with individual controls[b]	-4.7%***	-2.0%	-1.2%*	-2.1%***
	(1.6)	(1.3)	(0.7)	(0.7)

Sample: n_{1976}=2102; n_{1985}=3874 (includes cross-section and state over-samples, excludes observations from states that are not present in the 1976 data; sampling weights are applied)

*, **, *** denote significance at the 10%, 5%, and 1% levels respectively.

(Robust standard errors in parentheses, corrected for clustering within 72 state-year cells).

All regressions include year fixed effects and either state fixed effects, or treatment/control fixed effects.

Dependent variable is a dummy variable set equal to one if the household reports a violent incident as having occurred between spouses over the preceding year, and zero otherwise. Thus, reported coefficients reflect the change in the relevant spousal violence rate in treatment relative to control states – in percentage points. To assess these changes in percentage terms, compare the reported coefficient with the corresponding term in the first row. Each entry reflects a separate regression.

[a] Severe violence is defined as kicked, bit, hit with fist, hit or tried to hit with something, beat up partner, threatened with gun or knife, or used a gun or knife, in the past year. Overall violence also includes threw something at partner, pushed grabbed or shoved, and slapped. (Follows Gelles and Straus, 1994.)

[b] Individual controls include a saturated set of dummies for respondent's age, race and gender, and the educational attainment and current labor force status of both husband and wife. These regressions also include state-fixed effects.

[c] State-level time-varying controls include the maximum level of AFDC for a family of four in that state-year, the proportion of the population on welfare, the ratio of female to male employment rates, the state unemployment rate and log personal income per capita.

Table 5: Effect of Unilateral Divorce on Intimate Homicide (% change)

	No Controls	Including Controls#		
	Intimate Homicide	Intimate Homicide	Placebo Non-Intimate Homicide	Diffs-in-Diffs-in-Diffs (Intimate less Non-intimate)
	(1)	*(2)*	*(3)*	*(4)*
	Women murdered by Intimates			
By Spouse	-10.5%*	-12.6%**	-3.7%	-7.2%
	(5.9)	(6.0)	(3.5%)	(6.9)
By Family	-8.9%**	-8.8%**	-3.1%	-5.6%
	(4.4)	(4.4)	(4.2)	(6.1)
By Known	-8.7%**	-8.5%**	-0.1%	-7.9%**
	(3.7)	(3.6)	(5.2)	(6.3)
	Men Murdered by Intimates			
By Spouse	12.3%	3.9%	-2.2%	10.9%
	(9.2)	(9.0)	(2.8)	(9.6)
By Family	1.9%	-4.3%	-1.3%	0.2%
	(5.3)	(5.3)	(3.0)	(5.9)
By Known	-2.0%	-5.0%	2.7%	-4.1%
	(3.1)	(3.1)	(4.3)	(5.2)
	Alternative Placebos: "Effects" on Other FBI Index Crimes			
Forcible rape	-0.0%	3.3%		
	(2.7)	(2.4)		
Robbery	-2.1%	3.1%		
	(2.9)	(2.8)		
Aggravated assault	-2.2%	5.9%**		
	(3.0)	(2.7)		
Burglary	-0.3%	0.7%		
	(1.6)	(1.6)		
Larceny-Theft	-1.5%	-0.3%		
	(1.1)	(1.2)		
Auto theft	-7.9%***	-2.0%		
	(2.6)	(2.6)		

Sample: 1968-94. Sample excludes Illinois due to missing observations from Chicago Police Department. Also excludes Washington DC as an outlier: *n*=1323.

*, ** and *** denote significance at the 10%, 5%, and 1% levels respectively. (Robust standard errors in parentheses.) Dependent variable is the annual intimate homicide rate in each state. Each cell reports the estimated effect of unilateral divorce laws from a separate regression. The rows focus on different definitions of "intimate homicide", while columns report different specifications. Reported coefficients reflect the percentage change in the relevant homicide rate attributed to Unilateral Divorce laws; calculated using the unweighted cell mean as the base. All regressions include (significant) state and year fixed effects.

Controls include an indicator variable for the death penalty, the Donahue and Levitt Effective Abortion Rate, and the state incarceration rate, once lagged, as well as the AFDC rate for a family of four, the natural log of state personal income per capita, the unemployment rate, the female-to-male employment rate, age composition variables indicating the share of states' populations aged 14-19, and then ten-year cohorts beginning with age 20 up to a variable for 90+, and the share of the state's population that is black, white and other.

Figure 1

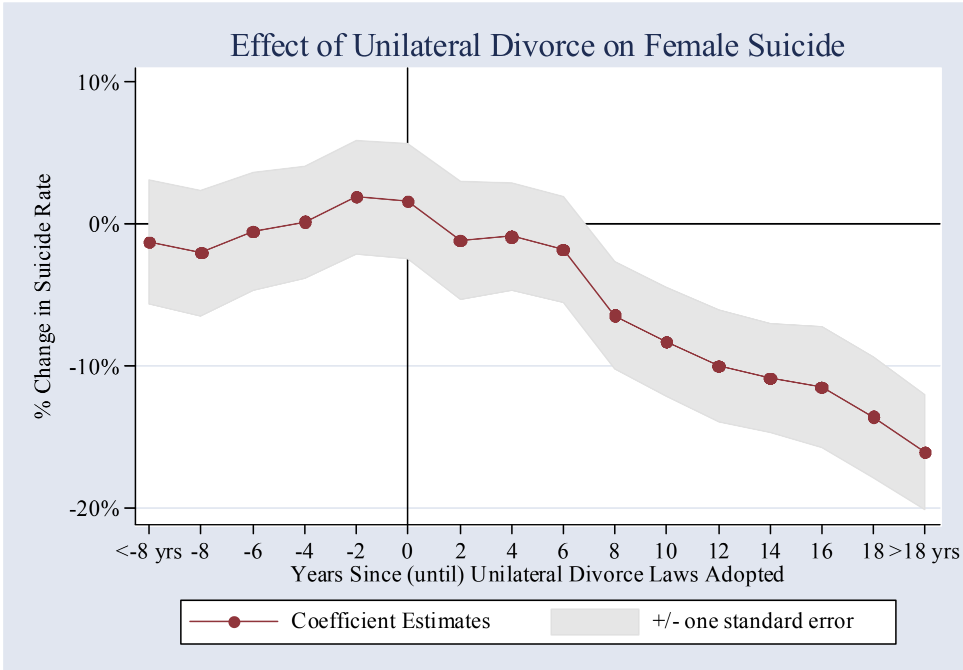
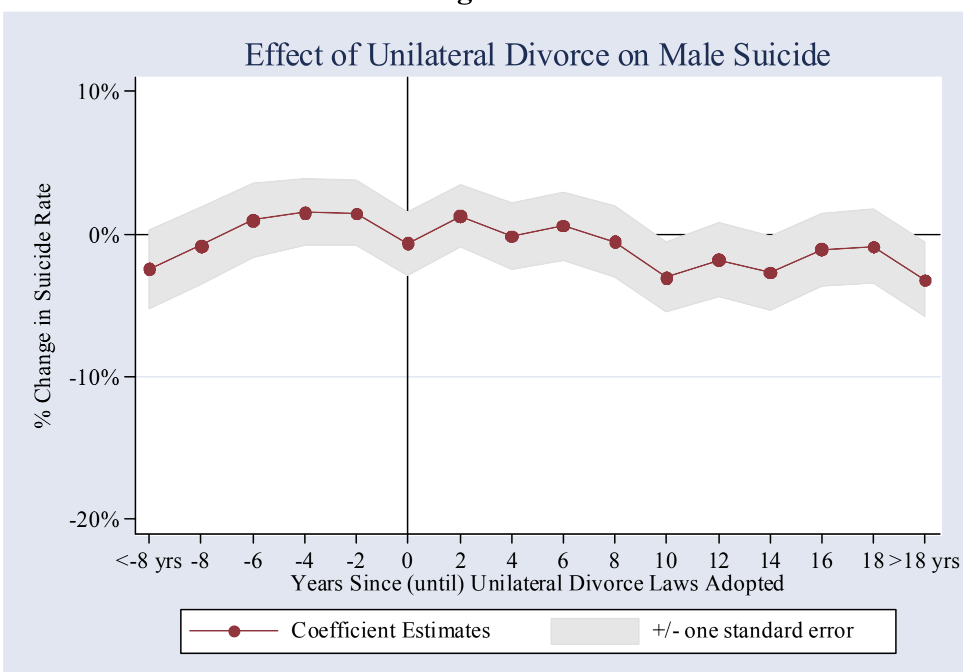


Figure 2



Notes: Figures 1 and 2 show the estimated coefficients (evaluated as elasticities at the unweighted cell mean) from regressing the suicide rate on dummy variables for whether unilateral divorce laws have been in effect for 1-2 years, 3-4 years, 5-6 years etc; as shown, dummies are also included for similar leads. State and year fixed effects are also included.

Figure 3

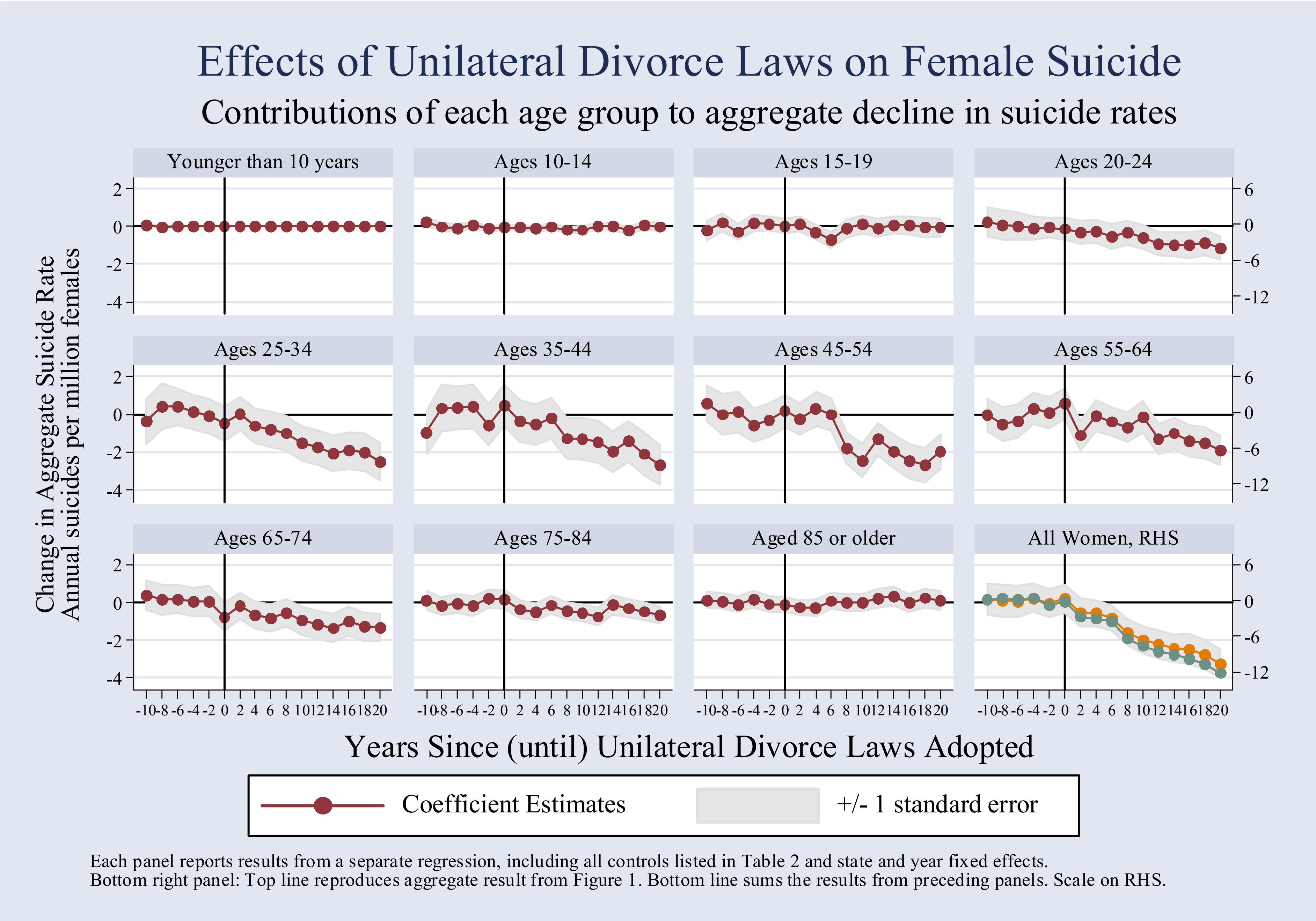
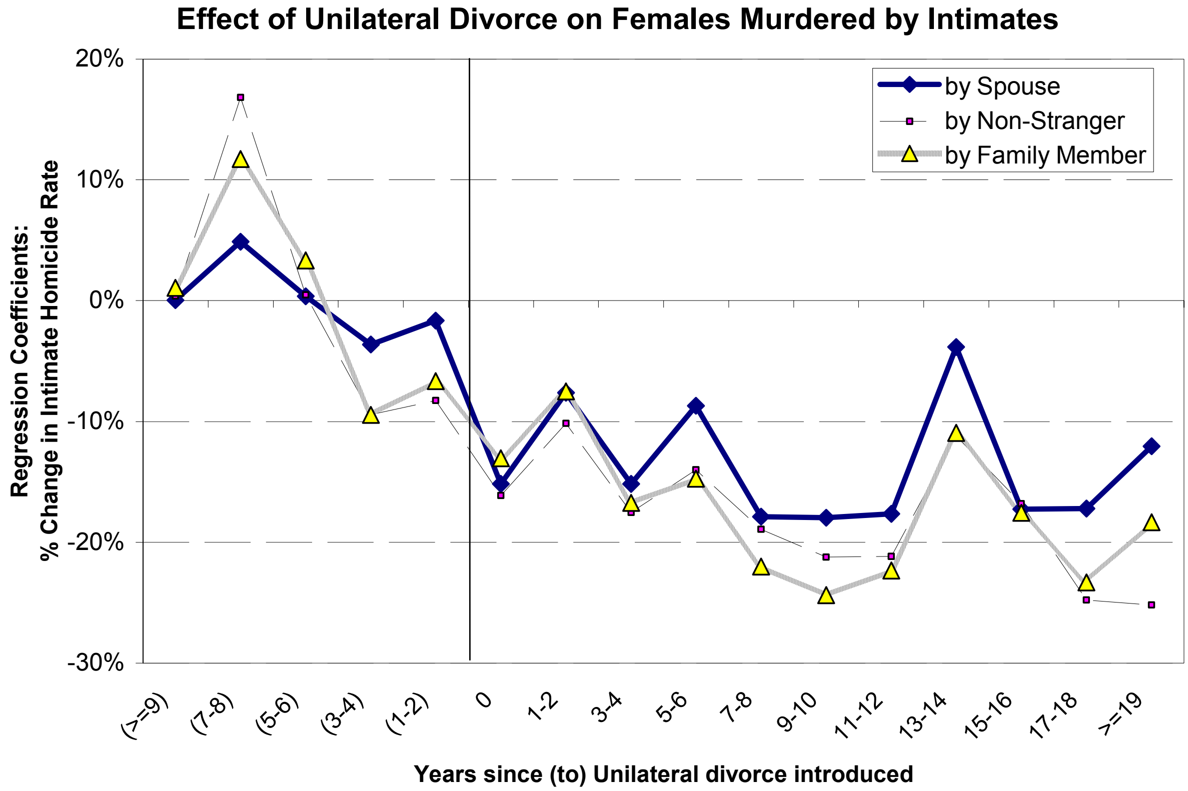

Effects of Unilateral Divorce Laws on Female Suicide
Contributions of each age group to aggregate decline in suicide rates
Younger than 10 years
Ages 10-14
Ages 15-19
Ages 20-24
Ages 25-34
Ages 35-44
Ages 45-54
Ages 55-64
Ages 65-74
Ages 75-84
Aged 85 or older
All Women, RHS
Change in Aggregate Suicide Rate
Annual suicides per million females
Years Since (until) Unilateral Divorce Laws Adopted
Coefficient Estimates
+/- 1 standard error
Each panel reports results from a separate regression, including all controls listed in Table 2 and state and year fixed effects.
Bottom right panel: Top line reproduces aggregate result from Figure 1. Bottom line sums the results from preceding panels. Scale on RHS.

Figure 4



Notes: Figure 4 shows the estimated coefficients (evaluated as elasticities at the unweighted cell means) from three regressions, each focussing on a different definition of the female intimate homicide rate. Each line plots the coefficients on dummies indicating whether unilateral divorce laws have been in effect for 1-2 years, 3-4 years, 5-6 years etc; as shown, dummies are also included for similar leads. State and year fixed effects are also included.