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

BETSEY STEVENSON AND JUSTIN WOLFERS

This paper exploits the variation occurring from the different timing of divorce law reforms across the United States to evaluate how unilateral divorce changed family violence and whether the option provided by unilateral divorce reduced suicide and spousal homicide. Unilateral divorce both potentially increases the likelihood that a domestic violence relationship ends and acts to transfer bargaining power toward the abused, thereby potentially stopping the abuse in extant relationships. In states that introduced unilateral divorce we find a 8–16 percent decline in female suicide, roughly a 30 percent decline in domestic violence for both men and women, and a 10 percent decline in females murdered by their partners.

I. 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. During the next two decades, many states moved away from fault-based divorces, which were challenging the legal system, toward the less adversarial unilateral divorce.[1] In other words, in many states it became possible to seek the dissolution of a marriage without the consent of one's spouse.

However, many states began to question these changes in the

* This project has drawn on the advice of many generous friends and colleagues, including Olivier Blanchard, Margaret Brinig, David Cutler, Thomas Dee, David Ellwood, Leora Friedberg, Edward Glaeser, Claudia Goldin, Caroline Minter Hoxby, Christopher Jencks, Alan Krueger, Steven Levitt, Jeffrey Miron, Katherine Newman, Robert Putnam, David Weiman, Julie Wilson, and seminar participants at Harvard University, the MacArthur Network on Inequality and Social Interactions, Stanford University, University of Michigan, Princeton University, the London School of Economics, University of California at Berkeley, Columbia University, Yale University, University of Melbourne, and the Society of Labor Economists. Special thanks goes to Lawrence Katz for his guidance throughout the project. We have also benefited from the excellent research assistance of Eric Klotch, Amalia Miller, and Jason Grissom. Remaining errors are our own. We are grateful to the MacArthur Foundation and the Social Science Research Council for funding for this project. A previous version of this paper was circulated under the title, "'Til Death Do Us Part: Effects of Divorce Laws on Suicide, Domestic Violence and Intimate Homicide."

1. Historical accounts of the legislative movement to pass unilateral divorce focus on the difficulty of an adversarial system in which fault-based grounds for divorce need to be proved. While legitimate cases sometimes struggled to establish sufficient evidence in the face of a denying spouse, cases in which both couples wanted to divorce often involved fraudulent charges of adultery and abuse as spouses attempted to convince the court (usually successfully) that these were legitimate grounds for divorce [Jacob 1988].

The Quarterly Journal of Economics, February 2006

1990s and 2000s. Widespread concern over the decline of the American family has led many to point the finger at unilateral divorce laws, claiming that easy access to divorce undermines traditional family structures. Unfortunately, much of the public discussion centers on the consequences of *divorce* rather than the consequences of *divorce laws*.

Unilateral divorce allows marriages to end where one person wants out of the marriage and the other person wants to remain married. This paper seeks to answer the question: who benefited from this change and by how much? While models of the family that rely on a common preference function or internal threat points predict little change, external threat-point models tell us that unilateral divorce changes bargaining within marriage by improving the outside options of each spouse. As such, bargaining power, and therefore resources, shifts toward the person who most wants out of the marriage.

The people most likely to benefit from unilateral divorce are therefore those who stand to gain the most from having the option to exit their relationship. One possibility is that those in violent, potentially lethal, relationships have the most to gain when they can credibly threaten to exit the relationships. Unilateral divorce has two possible effects on these relationships. The first is that it allows them to end.[2] The second is that the threat of divorce may be sufficient to prevent future abuse in relationships that continue. Our focus in this paper is the effect of allowing unilateral divorce on these particularly bad marriages, potentially through both channels.

Without access to unilateral divorce, people "trapped" in a bad marriage had few choices. While they could leave the marriage without being granted a divorce, they would not be able to take any assets from the marriage and would be unable to remarry. We consider the possibility that violent relationships were more likely to end through suicide or homicide prior to unilateral divorce. Suicides could result from unhappiness: the value of continuing to live in the abusive relationship falling below the option value of staying alive.[3] Alternatively, those in abusive relationships may have used strategic suicide attempts as a way

2. While fault-based divorce does offer divorce for violent relationships, the violence must be proved in court. These cases were quite adversarial, and many abuse victims were likely afraid of the heightened threat during the trial.

3. For an economic model of suicide see Hamermesh and Soss [1974].

to get more resources transferred toward them.[4] Homicide may result either because the victim of abuse fights back with lethal force or because the abuse itself becomes lethal.

This paper exploits the variation occurring from the different timing of divorce law reforms across the United States to evaluate how unilateral divorce changed family violence and whether the option provided by unilateral divorce reduced suicide and spousal homicide. Family violence surveys conducted in the mid-1970s, and again in the mid-1980s, provide basic detail about domestic violence by both men and women. Spousal homicide and suicide rates are examined for both men and women.

We find that states that passed unilateral divorce laws saw a large decline in both male- and female-initiated domestic violence. Between 1976 and 1985 states that had changed their divorce laws to allow unilateral divorce saw their overall and severe domestic violence rates fall by about one-third. The effect on domestic violence is large enough to imply that domestic violence was reduced not just by ending violent relationships, but by reducing the violence in extant relationships as well.

Our findings examining potential lethal ends to domestic violence—suicide and homicide—point to the benefits of unilateral divorce for women. We show that women murdered by intimates declined by 10 percent following the introduction of unilateral divorce. However, we note that an examination of the dynamic effects of the change by year indicate that there may have been a preexisting downward trend in women being killed by intimates in states that adopted unilateral divorce. We find no discernible effect of unilateral divorce laws on spousal homicide for men.

Suicide rates are examined for all men and women separately and by age category. To capture the full dynamic response of the suicide rate to the law change, we evaluate the effect for each year following the adoption of unilateral divorce. As with spousal homicide, our results show no discernible effect of unilateral divorce on male suicide. Female suicide is shown to fall following the adoption of unilateral divorce. Furthermore, the results indicate that female suicide rates continue to fall in unilateral divorce states for more than a decade following the legal change. Averaging the effects over the twenty years following

4. For a more complete discussion of strategic suicide, see Cutler, Glaeser, and Norberg [2001].

reform suggests an aggregate decline of 5–10 percent with larger long-run effects. We now turn to theory to better elucidate the key forces mediating these results.

II. Mediating Forces: Marriage, Divorce, and Bargaining within Marriage

Unilateral divorce laws may change behavior through two primary channels. First, they may lead to a change in divorce rates, allowing those to escape who were unable to either prove fault or persuade their spouse to grant them a divorce. And second, these laws redistribute property rights, and hence bargaining power, within the relationship. Becker [1993] has argued that the Coase theorem is the natural starting point for such an analysis.

In a Coasian analysis, 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 marriage is jointly suboptimal, and this efficient bargain will be obtained irrespective of the initial assignment of property rights. As such, the Coase theorem predicts that there are no "inefficient marriages," and a change in divorce law to allow unilateral divorce will have no effect on the divorce rate. Therefore, the first effect of unilateral divorce—allowing certain marriages to end that would not otherwise have ended—only occurs in cases where the Coase theorem is violated.[5]

Research has shown that the divorce rate was affected by the passage of unilateral divorce; Wolfers [2006] finds a small and transitory rise in divorce that dissipated within a decade. However, the magnitude of this effect suggests only a very small and gradual change in the stock of married couples.[6] Yet a small increase in divorce could reflect a large proportion of those in violent relationships divorcing, including those that might other-

5. The Coase Theorem requires costless bargaining, transferable utility, and no wealth effects.

6. Combining the estimates in Wolfers [2006] and Rasul [2004], the proportion of the population who are married declines by about 1–2 percent in the decade following reform (relative to the control states), with the effects becoming only slightly larger over the ensuing decade.

wise have ended lethally through suicide or homicide. A Coasian prediction of no change in the divorce rate requires costless bargaining, something that seems particularly unlikely to apply to those marriages where violence (rather than negotiation) is used to settle conflicting claims. By ending inefficient (and violent) marriages, unilateral divorce both reduces domestic violence and raises the expected value of life for the partner trapped in an inefficient marriage, thus reducing suicide.

Domestic violence, however, comes in varying degrees, and a large decline in overall domestic violence cannot simply be explained by increased divorce: over 10 percent of couples acknowledge using some amount of violence during a spousal conflict. This leads us to consider the second channel through which unilateral divorce may impact spousal violence: the distribution of bargaining power within marriage.

While Coase predicts a change in distribution toward those who want out of the marriage (this redistribution is the set of side payments required to enforce an efficient bargain), the effects of redistribution depends on the underlying model of intrahousehold distribution. Existing theory is conflicted about whether a redistribution of resources within a family will affect individual members' shares of resources. Both the *common preference* approach to within-family distribution and internal threat point (*separate spheres*) bargaining models argue that the change in property rights within a marriage should have no effect on within-household distribution.[7] The former rules out spousal bargaining by positing a joint utility function (perhaps love yields perfect altruism and hence a common preference). As such, the Coase theorem predictions about outcomes will hold (the common preference model posits that households maximize a joint utility function, and as such, divorce rates would be invariant to divorce law); however, distribution will remain unchanged. Internal threat point models argue that distribution is determined through bargaining; however, the relevant threat points are reversion to a noncooperative equilibrium (such as sleeping on the couch) within the marriage and are invariant to a change in outside options. Unilateral divorce laws do not affect these threat

7. For information on bargaining models that rely on threat points that are internal to the marriage, see Lundberg and Pollak [1993].

points, and hence do not change the distribution of resources within a household.

By contrast, *exit threat* bargaining models emphasize each spouse's best option outside the marriage as the relevant parameters determining the intrahousehold distribution. Under a consent divorce regime the relevant exit threat is to leave the marriage, albeit with no opportunity to remarry, nor with a legal claim to a share of the couple's joint assets. Unilateral divorce laws provide for a more attractive outside option, which likely affects the resulting bargain inside the marriage. Alternatively phrased, bargaining power, and thus resources, should be redistributed toward those for whom unilateral divorce provides a credit threat to exit the marriage.

If the redistribution of property rights caused by unilateral divorce laws does change within-household bargaining, we should see effects arising out of that redistribution. If unilateral divorce laws redistribute bargaining power toward abused spouses, presumably abused spouses will use their increased bargaining power to demand less abuse. Furthermore, redistribution should have the largest impact on those for whom the marginal utility of an extra dollar is the highest. Such relationships might involve highly skewed distribution. These are also the relationships in which one might expect to observe extreme attempts to redistribute resources. Cutler, Glaeser, and Norberg [2001] suggest that "strategic" suicide attempts may be designed to signal unhappiness with the current intrahousehold allocation, and to threaten the abuser with a bad outcome if it is not rectified. If the threat is successful, it leads to a redistribution of resources toward the suicidal spouse. Strategic suicide must be (occasionally) credible to be effective as a threat, and as such, must result some proportion of the time in actual suicides. By transferring bargaining power toward the person who is enduring violence, they can use this increased power to negotiate less violence. As such, this increased power also reduces the marginal value of strategic suicide attempts (assuming decreasing returns to lowering violence), thereby reducing both attempts and actual suicides ("failed" attempts).

Finally, most spousal homicides occur in the context of abusive relationships [Campbell 1992], and hence any policy that reduces domestic violence is likely to reduce the probability of spousal homicide. We now turn to exploring these potential effects empirically.

III. Empirical Strategy and Data

We follow Friedberg's [1998] coding of state divorce regimes and the dates of divorce reforms.[8] 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.[9] Of the 50 states, 5 are yet to adopt any form of unilateral divorce: Arkansas, Delaware, Mississippi, New York, and Tennessee. Of the 45 states that currently have unilateral divorce regimes, 9 had adopted some variant of unilateral divorce before the no-fault revolution of the early 1970s. Along with the 36 remaining states we include the District of Columbia, which adopted unilateral divorce in 1977. Consequently, we effectively have 37 "experiments" of changing divorce laws. The remaining fourteen states are included as controls.

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 suicide, domestic violence, and homicide rates for women and men 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 allows unilateral divorce 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.[10]

Data on suicide come from the National Center for Health Statistics (NCHS).[11] The NCHS data are a census of death cer-

8. Results are consistent with alternative coding of the dates of the legal reforms to unilateral divorce.

9. Around one-third of states have separation requirements, ranging from six months to five years. Results are consistent with alternative treatment of separation requirements.

10. Summary statistics are available in Stevenson and Wolfers [2003].

11. Suicide data for 1964–1967 were hand entered from annual editions of the NCHS report "Vital Statistics: Mortality, Vol. 2." Data for 1968–1978 are calculated from ICPSR Study No. 8224, "Mortality Detail Files: External Cause Extract, 1968–78," PI: National Center for Health Statistics. Data from 1979–1996 have been downloaded from the Center for Disease Control's Wonder system which accesses the NCHS "Compressed Mortality Files" (http://wonder.cdc.gov/). Apart from minor revisions to the International Classification of Diseases, these data are consistently coded.

tificates, 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 death are also collected.

Data on domestic violence are from the landmark Family Violence Surveys undertaken by sociologists Murray A. Straus and Richard J. Gelles in 1976 and again in 1985.[12] These data are gathered using household interviews that ask how couples resolve conflict. 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 or a crime for it to be recorded.[13] While still an imperfect survey instrument, Markowitz [2000, p. 286] argues that this methodology is currently "the best available technique for collecting truthful information on domestic violence."

Data on homicide come from the FBI Uniform Crime Reports (UCR).[14] 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 these 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 underreporting 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 consistent, and

12. The 1976 and 1985 surveys are ICPSR studies 7733 and 9211, respectively.

13. Crime victimization survey data lack state identifiers and are not available for the relevant time period. Police reports suffer from serious problems of underreporting and changes in social norms regarding reporting over the relevant time period.

14. Data for 1968–1975 are from ICPSR Study No. 8676, "Trends in American Homicide, 1968–1978: Victim-Level Supplementary Homicide Reports," [Riedel and Zahn 1994]. Data for 1976–1994 are extracted from ICPSR Study No. 6754, "Uniform Crime Reports [United States]: Supplementary Homicide Reports, 1976–1994" [Fox 1996]. A detailed appendix discussing the consistency of these data is available from the authors.

hence the rest of our analysis uses the FBI data 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.[15] 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 nondomestic romantic partners and other family members (particularly children). However, there are data problems constructing such a series that is consistent across coding breaks. As such, we estimate our results for several definitions of intimate homicide.

IV. Suicide Results

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. By analyzing the suicide rate of all persons, our coefficient captures the effect of unilateral divorce on suicidality through both channels: those who remain married and those who exit their relationships.

15. 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 nonparticipation 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. One outlier to this is Illinois where the Chicago Police Department failed to report any murders in 1984, 1985, November 1986–May 1987, July 1987–December 1987 and July 1990–December 1990. As it is implausible that there were no murders during these periods, we omit Illinois from our homicide samples.

We employ OLS to estimate

$$\textit{Suicide rate}_{s,t} = \sum_k \beta_k \textit{Unilateral}^k_{s,t} + \sum_s \eta_s \textit{State}_s + \sum_t \lambda_t \textit{Year}_t + \textit{Controls}_{s,t} + \varepsilon_{s,t}.$$

$\textit{Unilateral}^k$ refers to a series of dummy variables set equal to one if a state had adopted unilateral divorce k years ago. Thus, 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. As such, they map out the full dynamic response of the suicide rate to the law change.

The first and third columns of Table I report baseline results without including demographic and social policy controls for men and women, respectively. The second and fourth columns add a full set of controls, including a proxy for the evolving economic power of women (the ratio of male-to-female employment rates), business cycle indicators (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.[16] While we find that some of these controls are significant explanators of the suicide rate, their inclusion has little effect on our parameter of interest—the estimated effect of unilateral divorce.

Table I shows that there is a large and statistically significant reduction in the female suicide rate following the change to unilateral divorce. Further, this effect grows over time with the full effects of unilateral divorce on female suicide occurring fifteen to twenty years following the adoption of unilateral divorce. Averaging the effects over the twenty years following reform suggests an aggregate decline of 8 percent–10 percent in female suicide and a long-run decline that is much larger. For male suicides, Table I reveals no discernible effect. It should be noted that the male suicide rate is four times larger than that for women; thus, these results falsify neither moderately large positive nor negative effects on men committing suicide.

We test the sensitivity of our results to a number of alterna-

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

TABLE I
EFFECTS OF UNILATERAL DIVORCE ON SUICIDE RATES (PERCENT CHANGE)

	Female suicides		Male suicides	
Column no.	(1f)	(2f)	(1m)	(2m)
Year of change	1.6%	1.3%	−0.8%	−1.4%
	(3.8)	(3.4)	(2.2)	(2.1)
1–2 years later	−1.5%	−1.4%	1.2%	0.5%
	(3.7)	(3.5)	(1.5)	(1.4)
3–4 years later	−1.5%	−1.1%	0.0%	−0.9%
	(3.1)	(3.1)	(1.6)	(1.5)
5–6 years later	−3.0%	−2.0%	0.4%	−0.2%
	(2.9)	(2.9)	(1.5)	(1.5)
7–8 years later	−8.0%	−6.6%	−1.0%	−1.3%
	(3.0)	(3.0)	(1.8)	(1.8)
9–10 years later	−10.0%	−8.5%	−3.5%	−3.9%
	(3.0)	(3.0)	(1.7)	(1.7)
11–12 years later	−11.9%	−10.2%	−2.2%	−2.6%
	(3.1)	(3.2)	(2.0)	(2.0)
13–14 years later	−12.8%	−11.1%	−3.2%	−3.6%
	(3.2)	(3.1)	(2.0)	(2.0)
15–16 years later	−13.3%	−11.7%	−1.6%	−2.0%
	(3.7)	(3.6)	(2.0)	(1.9)
17–18 years later	−16.4%	−13.9%	−1.6%	−1.9%
	(3.6)	(3.6)	(2.1)	(2.0)
≥19 years later	−18.7%	−16.4%	−3.9%	−4.3%
	(3.2)	(3.3)	(2.0)	(2.0)
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%	−1.5%	−2.0%
	(2.3)	(2.3)	(1.3)	(1.3)
F-test of joint significance	p = 0.00	p = 0.00	p = 0.36	p = 0.37
Control variables				
State and year fixed effects	√	√	√	√
Economic, demographic, and social policy controls#		√		√

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 fourteen years or greater.)

tive specifications.[17] We examine the sensitivity of our baseline regressions to the time period and sample chosen by 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, also yield similar results. Further, while OLS implicitly gives equal weight to each of our 37 divorce reform experiments, we also found similar results using population-weighted least squares and generalized least squares.

In further robustness testing, we tested our results to the inclusion of state-specific time trends finding that their inclusion causes the standard errors to increase. For women, the specification including state-specific time trends yields point estates that are roughly similar to, but slightly smaller than, those shown in Table I. However, the increase in standard errors yields results that are not precisely estimated enough to reject either a null that the pattern of coefficients follows that shown in Table I or a null of no effect. For males, including state-specific trends is suggestive of a decline in male suicide rates following the advent of unilateral divorce. We also experimented with the control group, dropping those states that did not change their divorce 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 postdated the change in divorce regime, and whether adjustment to the new regime seems plausible. Additionally, if divorce law is directly affecting suicidality, it should primarily affect prime-age women rather than teens and the elderly. In order to examine these issues, 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 ten years coded to the 9–10 year group. Again, we find no discernible effect on male suicide. For female suicides, 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.

17. Several of the specification tests discussed can be found in Stevenson and Wolfers [2003].

We also disaggregate our main results by age. Figure I reports these regressions for 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 U. S. 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 I 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). Overall, the observed correlation between the adoption of unilateral divorce and the decline in female suicide seems robust, and we can be confident that neither youth nor the elderly drive the observed correlation between female suicide and divorce regime.

V. Domestic Violence and Homicide

While the Strauss and Gelles [1994] data on domestic violence are plausibly the best available data, the timing of the surveys is not ideal for evaluating the effect of unilateral divorce on domestic violence. These surveys provide cross-sectional data for 1976, by which time 31 states had recently changed their divorce laws, and again for 1985, by which time 37 states (including Washington, DC) had changed their laws to allow unilateral divorce. 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 1976–1985 period. Although the differential cross-state timing in reform yields little analytical leverage, we can compare changes in violence rates among our 37 states that changed their divorce laws to allow unilateral divorce with 2 alternative control groups: the 5 states that are yet to adopt unilateral divorce (AR, DE, MS, NY, and TN), and the 9 states whose preexisting regime involved

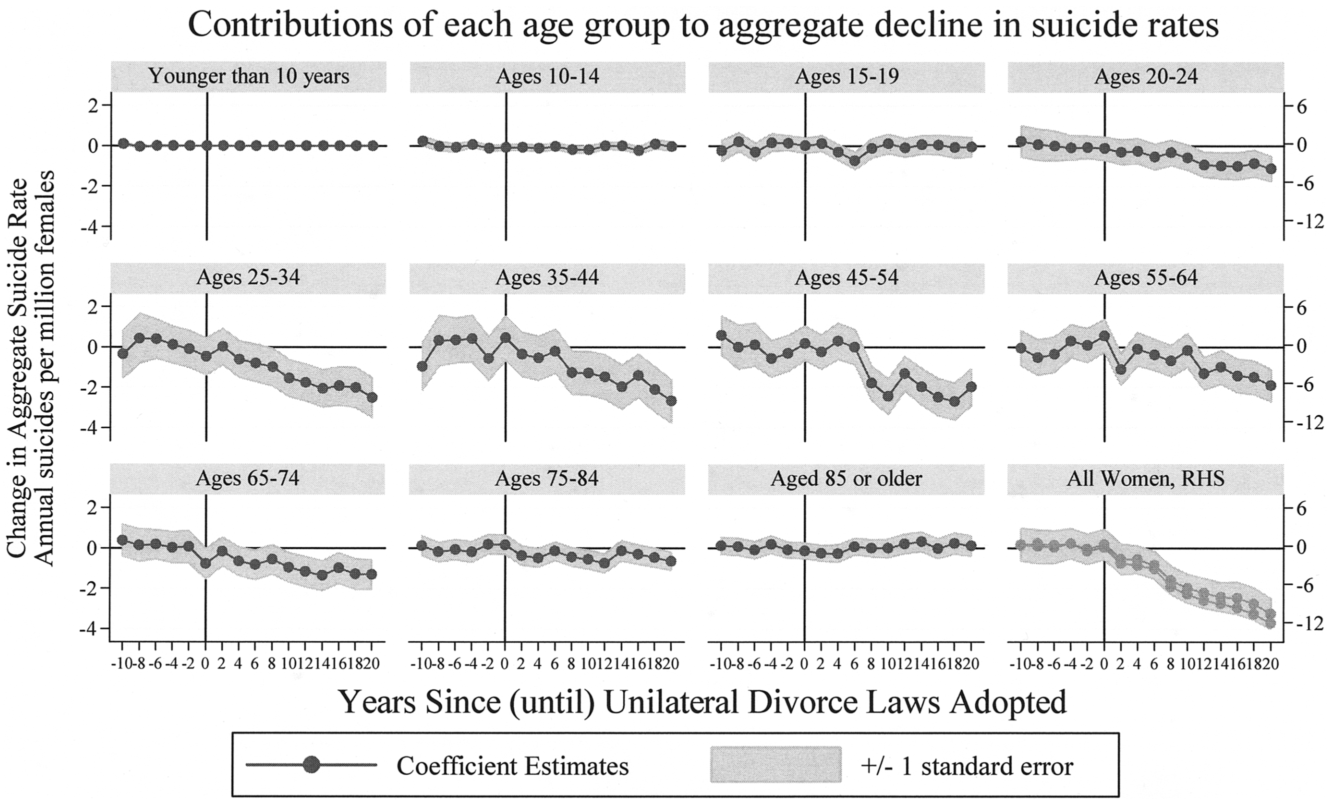

FIGURE I

Effects of Unilateral Divorce Laws on Female Suicide

Each panel reports results from a separate regression, including all controls listed in Table I and state and year fixed effects. Bottom right panel: top line shows results from age-aggregated regression. Bottom line sums the results from preceding panels. Scale is on the right-hand side.

unilateral divorce (AK, LA, MD, NC, OK, UT, VA, VT, and WV).[18] 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. Thus, we cannot directly disentangle whether the estimated effects reflect a decreasing propensity toward spousal violence, or an increasing propensity for abused spouses to exit their marriages.

Table II 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.[19] We estimate

$$Domestic\ Violence_{i,s,t} = \beta(Treatment_s \times Year_t^{1985}) + \delta Treatment_s$$

$$\sum_t \lambda_t\, Year_t + \left[\sum_s \eta_s\, state_s + controls_i\right] + \varepsilon_{i,s,t},$$

where *Treatment* is a dummy variable that is equal to one if the state adopted unilateral divorce prior to 1985, and is zero otherwise and β is the difference-in-difference estimator.

The first row of Table II shows the mean rates of violence across households. Perhaps surprisingly, men are as likely to be physically abused by their spouses as women are.[20] The next row shows difference-in-difference estimates of the effects of unilateral divorce on domestic violence. Domestic violence toward women declined by 1.7 percentage points in reform states and 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 1976–1985 period. Adding state fixed effects in the next row sharpens

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

19. The definition of domestic violence follows Straus and Gelles [1994] who code domestic violence as occurring if there has been any incident over the last year in which a person threw something at 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.

20. One reason that physical abuse may be perceived as occurring more often by men toward women is that assaults by men are seven times more likely to result in injuries that require medical treatment. See Stets and Straus [1990].

TABLE II
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 oversamples, excludes observations from states that are not present in the 1976 data; sampling weights are applied). Robust standard errors are in parentheses, corrected for clustering within 72 state-year cells. *, **, *** denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively. All regressions include year 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.

these estimates somewhat, and these large effects are all found to be statistically significant. The following three rows show that these results are robust to the inclusion of a rich set of individual-level controls, the set of within-state time-varying economic and social policy controls used in Table I, 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. We now turn to an alternative indicator of spousal abuse—intimate homicide—to further probe the robustness of these results.

We consider several 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 nonstrangers. 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, possibly in a way that is correlated with family law regimes, leading to (difficult to sign) bias issues.[21] 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.

Table III suggests a large and significant decline in intimate femicide 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. Dee [2003] has also analyzed these data, employing

21. While the coding of married partners as "spouses" presents no difficulty, coding of common-law marriages, cohabiting couples, romantic partners, and separated spouses is likely to have changed over time. Although these groups may be small compared with 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.

TABLE III
EFFECT OF UNILATERAL DIVORCE ON INTIMATE HOMICIDE (PERCENT CHANGE)

	No controls	Including controls#		
	Intimate homicide (1)	Intimate homicide (2)	Placebo nonintimate homicide (3)	Diffs-in-diffs-in-diffs (intimate less nonintimate) (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)

Sample: 1968–1994. Sample excludes Illinois due to missing observations from Chicago Police Department. Also excludes Washington, DC as an outlier: n = 1323. Robust standard errors are reported in parentheses. *, **, and *** denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively. 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.

count data methods on a short (1968–1978) panel. He finds a large increase in males murdered by their spouses.[22] The sensitivity of both sets of results to small changes in specification makes us reluctant to draw strong conclusions in either direction for male homicide.

The results for female homicide are more robust, and we turn

22. 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.

to timing evidence to assist us in interpreting these results. As with the suicide data, 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 II. 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 III.

Figure II 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 predated 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 an alternative phenomenon that predated divorce law reform.

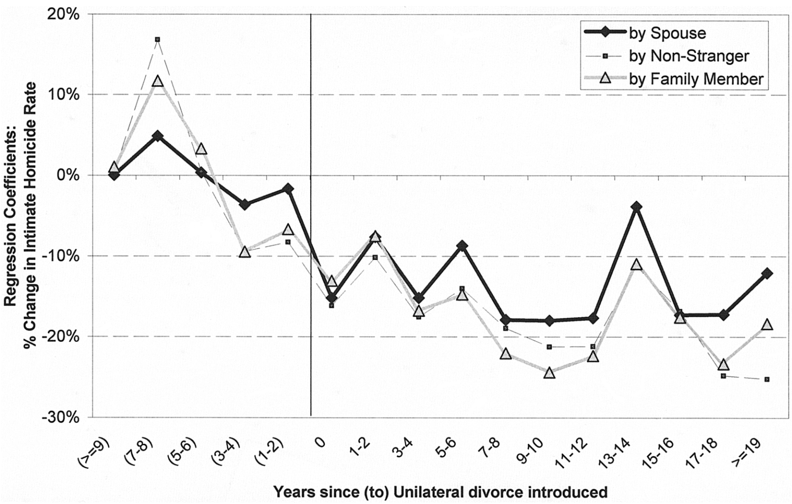

FIGURE II

Effect of Unilateral Divorce on Females Murdered by Intimates

Figure II shows the estimated coefficients (evaluated as elasticities at the unweighted cell means) from three regressions, each focusing 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.

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 III shows the differences-in-differences (panel) estimates for the nonintimate 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 nonintimate 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 (4) assumes that the change in nonintimate 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 generally show little correlation between state crime trends and divorce laws. These results are reported in Stevenson and Wolfers [2003].

VI. Conclusion

Our analysis examines the effect of unilateral divorce laws on measures of extreme marital distress. Changes in divorce law 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 percent 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 discernible 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 these results are not as convincing. As with suicide, there is no discernible 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 marriage and divorce rates, we believe that this is only part of the story. The timing evidence suggests that changes in the divorce rate explain little of our findings as the long-term effect of unilateral divorce on divorce rates is smaller than the short-term impacts while suicide rates show the opposite pattern. This suggests important roles for changes in marital formation and bargaining within marriage. If unilateral divorce were causing people to make better matches, then this effect would show up slowly over time as the stock of married people shifted toward those married after unilateral divorce. Beyond this, a more complete account must take changes in marital dynamics into account. Unilateral divorce changed the distribution of bargaining power within marriages, and therefore impacted many marriages.

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.

WHARTON, UNIVERSITY OF PENNSYLVANIA
WHARTON, UNIVERSITY OF PENNSYLVANIA, AND CEPR, IZA, AND NBER

REFERENCES

Becker, Gary S., *A Treatise on the Family* (Cambridge, MA: Harvard University Press, 1993).

Campbell, Jacquelyn C., "If I Can't Have You, No One Can: Power and Control in Homicide of Female Partners," in Jill Radford and Diana E. H. Russell, eds., *Femicide: The Politics of Women Killing* (New York, NY: Twayne, 1992).

Cutler, David M., Edward M. Glaeser, and Karen Norberg, "Explaining the Rise in Youth Suicide," in Jonathan Gruber, ed., *Risky Behavior Among Youths: An Economic Analysis* (Chicago, IL: University of Chicago Press, 2001).

Dee, Thomas S., "Until Death Do You Part: The Effects of Unilateral Divorce on Spousal Homicides," *Economic Inquiry,* XLI (2003), 163–183.

Fox, James A., "Uniform Crime Reports [United States]: Supplementary Homicide Reports, 1976–1994," ICPSR Study 6754, Boston, MA: Northeastern University, College of Criminal Justice, 1996.

Friedberg, Leora, "Did Unilateral Divorce Raise Divorce Rates? Evidence from Panel Data," *American Economic Review,* LXXXVIII (1998), 608–627.

Gelles, Richard J., and Murray A. Straus, PHYSICAL VIOLENCE IN AMERICAN FAMILIES, 1985 [Computer file], 3rd ICPSR release. Durham, NH: University of New Hampshire Family Research Laboratory [producer], 1988; Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 1994.

Hamermesh, Daniel S., and Neal M. Soss, "An Economic Theory of Suicide," *Journal of Political Economy,* LXXXII (1974), 83–98.

Jacob, Herbert, *Silent Revolution: The Transformation of Divorce Law in the United States* (Chicago, IL: University of Chicago Press, 1988).

Lundberg, Shelly, and Robert A. Pollak, "Separate Spheres Bargaining and the Marriage Market," *Journal of Political Economy,* CI (1993), 988–1010.

Markowitz, Sara, "The Price of Alcohol, Wife Abuse, and Husband Abuse," *Southern Economic Journal,* LXVII (2000), 279–303.

McElroy, Marjorie B., and Mary Jean Horney, "Nash-Bargained Household Decisions: Toward a Generalization of the Theory of Demand," *International Economic Review,* XXII (1981), 333–349.

Rasul, Imran, "The Impact of Divorce Laws on Marriage," mimeo, University of Chicago, 2004.

Riedel, Marc, and Margaret Zahn, "Trends in American Homicide, 1968–1978: Victim-Level Supplementary Homicide Reports," ICPSR Study 8676, Ann Arbor, MI: Inter-university Consortium for Political and Social Research, 1994.

Stets, Jan E., and Murray A. Straus, "Gender Differences in Reporting Marital Violence and its Medical and Psychological Consequences," in Murray A. Straus and Richard J. Gelles, eds., *Physical Violence in American Families: Risk Factors and Adaptations to Violence in 8,145 Families* (New Brunswick, NJ: Transaction Publishers, 1990).

Stevenson, Betsey, and Justin Wolfers, "Bargaining in the Shadow of the Law: Divorce Laws and Family Distress," NBER Working Paper No. 10175, 2003.

Straus, Murray, and Richard J. Gelles, PHYSICAL VIOLENCE IN AMERICAN FAMILIES, 1976 [Computer file], conducted by Murray A. Straus, University of New Hampshire, and Richard J. Gelles, University of Rhode Island, 2nd ICPSR ed., Ann Arbor, MI: Inter-university Consortium for Political and Social Research [producer and distributor], 1994.

United States Department of Health and Human Services, National Center for Health Statistics, "Mortality Detail File: External Cause Extract, 1968–1978, 1979–1980," ICPSR Study 8224, Ann Arbor, MI: Inter-university Consortium for Political and Social Research, 1985.

Wolfers, Justin, "Did Unilateral Divorce Raise Divorce Rates? A Reconciliation and New Results," *American Economic Review* (2006), forthcoming.