UNTIL DEATH DO YOU PART: THE EFFECTS OF UNILATERAL DIVORCE ON SPOUSAL HOMICIDES

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This study examines how the widespread adoption of unilateral divorce influenced the prevalence of lethal spousal violence in the United States. These evaluations are based on fixed-effects specifications for spousal homicide counts from an annual panel of U.S. states from 1968 to 1978. The results indicate that unrestricted unilateral divorce laws had small and statistically insignificant effects on the amount of lethal spousal violence directed against wives. However, the easy access to divorce created by such laws increased spousal homicides of husbands by approximately 21%. These increases were concentrated in states where the division of marital property favored husbands. (*JEL* J12, J16, I18, K4)

I. INTRODUCTION

Policy makers throughout the United States have expressed an increased interest in placing stronger legal restrictions on the ability of spouses to dissolve marriages. In three states (Louisiana, Arizona, and Arkansas), this concern has led to the creation of an optional "covenant" marriage that can only be dissolved after a period of separation and counseling, a policy currently under consideration in several other states as well. Part of the logic behind these efforts is the view that the divorce law reforms from the prior decades were partially responsible for the sharply increased divorce rate in the United States. The period when divorce rates grew most rapidly (the late 1960s through the mid-1970s) does coincide roughly with widespread adoption of "unilateral" divorce laws. These new regulations allowed for the dissolution of a marriage without the mutual consent of the spouses.[1] Recent econometric studies have indicated that the introduction of unilateral divorce was partly responsible for the increase in U.S. divorce rates.[2] The other potential welfare consequences of unilateral divorce regimes that have attracted attention concern their implications for the financial status of family members, for children's educational and economic outcomes, and, more generally, for the distribution of bargaining

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1. In 1968, only 5 states had unilateral divorce laws, but by 1978, 32 states did. Since then, only two additional states have introduced these laws. I would like to thank Jonathan Gruber for providing data on these law changes (Gruber, 2000) that updates the information in Friedberg (1998).

2. For example, Friedberg (1998) found that unilateral divorce laws accounted for 17% of the overall increase in divorces between 1968 and 1988. An application of the Coase theorem suggests that the introduction of unilateral divorce laws would actually be irrelevant because in the presence of costless transfers between spouses, those who found it in their interest to divorce would have done so already. However, this prediction hinges on the absence of transaction costs, wealth effects, strategic behavior, market power, and asymmetric information (Becker, 1981; Peters, 1986).

ABBREVIATIONS

AFDC: Aid to Families with Dependent Children
CML: Conditional Maximum Likelihood
FBI: Federal Bureau of Investigation
ML: Maximum Likelihood
OLS: Ordinary Least Squares
SHR: Supplementary Homicide Reports
UCR: Uniform Crime Reporting

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power within marriages.[3] It is well established that divorce is economically injurious to women, but the evidence on whether unilateral divorce regimes have been similarly harmful is mixed (Gray, 1996; Jacob, 1989; Peters, 1986). Surprisingly, relatively little attention has been paid to how unilateral divorce laws may have influenced another distinct indicator of spousal well-being: the prevalence and patterns of spousal violence.[4]

This relative omission has been unfortunate given the high prevalence of domestic violence and its obvious policy relevance.[5] However, an assessment of the effects of unilateral divorce laws on spousal violence can also contribute to the broader literature on these policies by providing compelling indirect evidence on how these reforms influenced the distribution of bargaining power between spouses. In particular, the mix of reform-induced changes in the patterns of domestic violence can indirectly suggest who perceived themselves to be harmed by the sudden and dramatic shift in the nature of the marriage contract. This study addresses these questions by evaluating how the introduction and structure of unilateral divorce laws influenced counts of spousal homicides.[6] As the next section indicates, the potential links between legal restrictions on divorce and spousal homicides admit several theoretical possibilities. For example, for wives who found themselves trapped in a physically abusive marriage, the introduction of unilateral divorce laws may have facilitated household dissolutions and thereby reduced spousal homicides. Such a potential reduction in domestic violence was clearly one of the motivations for the many states that relaxed couples' access to divorce (Wardle, 1994). However, it is also plausible that the new divorce laws actually increased spousal violence. Because a divorce is often an economically catastrophic event for women, easier access to divorce may have therefore placed women in a weaker bargaining position and allowed husbands to extract more opportunistic violence from them (Brinig and Crafton, 1994). Similarly, the possibly increased threat of economic or physical harm to wives may have dramatically enhanced the attractiveness of killing their husbands.[7] The central contribution of this study is to provide empirical evidence on these policy-relevant but theoretically ambiguous questions by evaluating reduced-form models for annual state-level counts of spousal homicides. The results of these empirical models also inform these issues by assessing how the potential effects of unilateral divorce on spousal homicides varied by the gender of the victim and the state-specific approach to dividing marital property.[8]

The annual state-level counts of spousal homicides analyzed in this study were drawn from detailed victim-level data in the 1968–78 Supplementary Homicide Reports (SHR; Riedel et al., 1985, 1994). The SHR data are collected as part of the Federal Bureau of Investigation's (FBI) Uniform Crime Reporting (UCR) system, which regularly gathers

3. These issues are likely to be related. A reduction in bargaining power is one potential mechanism by which unilateral divorce laws may have harmed women. For example, when mutual consent was no longer necessary, the alimony payments negotiated by women may have fallen (Weitzman, 1985; Peters, 1986). A reduction in the bargaining power of women may also be one of the ways by which the availability of unilateral divorce harms children (Gruber, 2000).

4. One of the justifications for unilateral divorce was that it would reduce domestic violence. Brinig and Crafton (1994) present some evidence that relaxed access to divorce is associated with an *increase* in crisis calls to abuse centers. However, this empirical evidence has been criticized on several grounds, including the inappropriateness of known crisis calls as a measure of spousal abuse and the use of a possibly confounded cross-sectional identification strategy (Ellman and Lohr, 1997).

5. In 1996, over 12% of all the murder victims in the United States were killed by a family member (U.S. Department of Justice, 1997). By far the most frequent type of such family murders has been homicides among spouses. Nearly half of family murders are characterized as spousal homicides (U.S. Department of Justice, 1997). Tauchen et al. (1991) present descripitive statistics on the prevalence of nonlethal spousal violence.

6. Ellman and Lohr (1997) suggest that spousal homicides are an accurate indicator for general spousal abuse because they are reliably collected and positively correlated with nonlethal abuse. Regardless, lethal spousal violence is a policy-relevant outcome that can also provide suggestive indirect evidence on which spouses perceive themselves to be economically disadvantaged by changes in divorce law regimes.

7. A little-known but well-documented fact is that the amount of lethal spousal violence committed by women is surprisingly high in the United States (e.g., Zuger, 1998; Wilson and Daly, 1992). Over the 1968–78 period, husbands were the victims in nearly half of all spousal homicides (see Figure 1). However, recent declines in spousal homicides of husbands have been linked to increasing labor market opportunities for women (Gauthier and Bankston, 1997).

8. Gray (1996, 1998) emphasizes the possibly important role of the state-specific treatment of marital property, which economically favor particular spouses.

data on known criminal offenses from almost all jurisdictions in the United States. The empirical results reported here are based on Poisson and negative binomial regressions that explicitly recognize the count nature of these homicide data.[9] Additionally, the evaluations presented improve substantively on prior research by accommodating state fixed effects that condition on the confounding but unobserved cross-sectional variation in the determinants of spousal homicides.[10] To accommodate consistently both the count nature of the data and the biasing influence of unobserved, state-specific attributes, this study employs the conditional maximum likelihood (CML) procedures developed by Hausman et al. (1984) for fixed-effect models of count data.

The results of these evaluations demonstrate that the effect of unilateral divorce laws on spousal homicides varied sharply by the gender of the victim and the design of law. For example, these results suggest that the widespread adoption of unrestricted unilateral divorce had relatively small and statistically insignificant effects on the number of wives killed by their husbands. In contrast, the introduction of unilateral divorce laws had large and statistically significant effects on the number of husbands killed by their wives. More specifically, these results demonstrate that the introduction of unilateral divorce laws is associated with an increase of roughly 21% in spousal homicides of husbands. Notably, the estimated increases in murdered husbands were concentrated in the unilateral divorce states where the treatment of marital property favored the husbands. These dramatic increases in lethal spousal violence against husbands underscore an important and overlooked consequence of unilateral divorce laws. However, these empirical results also provide novel evidence that disagreements over marital dissolution and economic assets were an important determinant of the spousal violence committed by women.[11] Furthermore, this evidence also speaks to the controversial question of whether women lost bargaining power and were substantially disadvantaged by the introduction of less effective marriage contracting. The finding that spousal homicides of husbands increased sharply after the introduction of unilateral divorce laws provides provocative indirect evidence that this was indeed a widespread perception among married women, at least in states that did not have generous community property provisions for the division of marital assets.

II. DOMESTIC VIOLENCE AND UNILATERAL DIVORCE

The seminal research on household decision making by Samuelson (1956) and Becker (1981) presumed at least some degree of cooperation and altruism among family members. However, the household bargaining models developed more recently by Manser and Brown (1980) and McElroy and Horney (1981) reflect a less altruistic environment and provide a more appropriate framework for considering decisions regarding domestic violence. The within-family distribution of outcomes in such Nash bargaining models is influenced in part by each spouse's "threat point": the reservation level of utility associated with divorce.[12] Tauchen et al. (1991) extended these bargaining models to incorporate the determination of domestic violence. In their framework, husbands engage in "instrumental" violence that discourages undesirable behavior by wives, as well as in "expressive" violence that is directly and psychologically gratifying to husbands. In equilibrium, the amount of domestic violence

9. Ordinary least squares (OLS) models generate qualitatively similar results, which are discussed here. However, because the state-year data on spousal homicides are exclusively nonnegative integers that are often small in value, conventional OLS procedures provide consistent but possibly inefficient results. Negative binomial regressions improve on the basic Poisson procedure by accommodating the possibility of overdispersion in the homicide counts (i.e., $\text{var}(y) \geq E(y)$).

10. There is considerable cross-sectional variation in homicide patterns in the United States (e.g., Butterfield, 1998). However, prior evaluations in this area have not directly addressed the bias introduced by unobserved state-specific attributes (Brinig and Crafton, 1994; Ellman and Lohr, 1997). The results presented here demonstrate that omitting state-specific controls can lead to highly misleading inferences.

11. In particular, the fact that unilateral divorce did not increase spousal homicides of wives suggest that the increase in spousal murders of husbands was not simply motivated by self-defense. This is also suggested by the heterogeneity in these effects with respect to the spouse-specific treatment of marital property.

12. Lundberg and Pollak (1993) propose a similar model in which the threat point is an "uncooperative marriage" instead of divorce.

essentially constitutes a nonpecuniary transfer between spouses.[13] The amount of violence endured by wives is constrained in part by their threat points. Farmer and Tiefenthaler (1997) develop a similar model and note that the availability of support outside the family (e.g., shelters, divorce settlements) decreases the level of violence endured by women by effectively raising their credible threat points.

How might we expect the introduction of unilateral divorce to influence the equilibrium level of domestic violence in such models? One straightforward prediction is that the availability of unilateral divorce would reduce the prevalence of domestic violence by making it easier for wives to extract themselves from abusive situations (or to threaten to do so). Within the context of these bargaining models, this prediction could be readily obtained by allowing unilateral divorce to raise the wife's threat point through a reduction in the costly obstacles to household dissolution. Domestic violence could therefore be reduced both through divorce as well as by more effective bargaining among the wives who remained in intact marriages. However, the opposite prediction regarding divorce laws and domestic violence may also be quite consistent with these bargaining models. Pollak (1985) notes that the absence of effective long-term contracting can allow the party in a strong strategic position to make opportunistic gains at the expense of the other party. Brinig and Crafton (1994) suggest this observation applies to the case of unilateral divorce. More specifically, they suggest that, when mutual consent is no longer required, women suffer particularly strong negative wealth effects on divorce.[14] Relaxed access to divorce would therefore imply that wives have a lower expected utility outside of marriage (i.e., a lower credible threat point). This may allow husbands to extract higher transfers from them in the form of more opportunistic violence.

Notably absent in all of these models is a consideration of the spousal violence committed by wives. This omission is arguably appropriate in the case of nonlethal violence, which appears to be almost exclusively committed by men. However, the surprisingly high share of spousal homicides that are committed by wives (Wilson and Daly, 1992) suggests that consideration of this lethal violence may be particularly informative. Within the context of this study, the key question is whether theoretical models of domestic violence can inform how spousal homicides of husbands might change in response to the introduction of unilateral divorce. As with violence directed against wives, it is straightforward to show that the theoretical predictions are again ambiguous. For example, the available stylized evidence suggests that wives sometimes kill their husbands out of self-defense (Wilson and Daly, 1992). Therefore, if unilateral divorce raises the threat points of wives and allows them to bargain down the amount of violence they endure, they should find it less necessary to kill their husbands in self-defense. In contrast, if unilateral divorce increased the amount of opportunistic violence wives endured, they may also be more likely to slay their husbands out of self-defense. There is also stylized evidence that wives murder their husbands in part because of their limited economic alternatives.[15] Given this motivation, the introduction of unilateral divorce could also conceivably increase the number of husbands slain by their wives. For example, if unilateral divorce placed wives in an economically perilous situation with regard to divorce, they may find the opportunity cost of murdering their husbands to be substantially lower.

The reduced-form empirical evidence presented in the remainder of this article informs the theoretical ambiguities discussed here. More specifically, this is achieved by empirical evaluations that identify how the introduction of unilateral divorce influenced counts of spousal homicides. The discussion of these results attempts to make specific distinctions among these theoretical perspectives by examining the heterogeneous responses with respect to gender of the victims as well as with respect to the state-specific treatment of marital property, which

13. For example, these models suggest that in the "standard" case where the wife's utility constraint is binding and she receives income transfers, an increase in the husband's income allows him to "buy" violence from his spouse.

14. As already noted, this is actually a controversial empirical issue that is informed by the results presented here.

15. The role of economic opportunity is underscored by evidence linking increased labor force participation with sharp reductions in spousal homicides of husbands (Gauthier and Bankston, 1997).

influenced the economic consequences of divorce in distinctive ways for husbands and wives. An additional caveat regarding dynamic responses to the changes in divorce laws should be noted. The long-term effect of unilateral divorce laws on spousal homicides may have been attenuated as current and future spouses adjust their behavior to the new realities of the marriage contract. For example, current and future wives who knew they could be economically threatened by the new divorce law regimes may have adjusted their household and labor market activities (e.g., labor force participation and human capital acquisition) to make themselves less subject to opportunistic violence by their husbands as well as less likely to kill their husbands. To avoid confounding such responses with the immediate effects of unilateral divorce on spouses who were "surprised" by the changed marriage contract, this study focuses on a relatively short state-level panel from the 1968–78 period.[16]

III. DATA

The FBI gathers information on known criminal offenses from local law enforcement agencies through its UCR system. As part of the UCR program, local jurisdictions are routinely asked to provide additional details on homicides by completing SHRs. The annual victim-level SHR data on homicides over the 1968–78 period were collected and, to the extent possible, standardized by Riedel et al. (1985, 1994).[17] There are a number of well-known concerns with such UCR data. For example, some crimes are unreported, and a small number of jurisdictions do not participate in the UCR program or submit SHR questionnaires. However, the underreporting associated with murders is particularly low and the response rate for the SHR questionnaire is quite high over this period (typically there are SHR data on over 95% of known murders). Furthermore, there is little reason to expect that the modest underreporting implied by these caveats introduces a confounding bias into the key inferences addressed in this study.[18]

The victim-level data on homicides from these SHRs were aggregated to generate counts of spousal homicides by state, year, and gender of the victim.[19] Unfortunately, the coding of spousal homicides varied somewhat over this period. Over the 1968–75 period, the SHR coding procedures identified spousal homicides but did not distinguish among those committed by common-law spouses and ex-spouses.[20] However, the SHR coding procedures for the 1976–78 period did make this distinction. In an attempt to be consistent in the panel data set, the counts of spousal homicides from the 1976–78 period also include those committed by common-law and ex-spouses. Casual observation of the trend in homicide counts over this period suggests this construction does not produce a sharp time-series break (Figure 1). More important, the key results from this study are replicated in models that only use the 1968–75 and 1968–72 data.[21] The descriptive statistics for these count data indicate that on average nearly 19 wives were killed by their husbands in a given state and year (Table 1). Spousal homicides of husbands were similarly frequent with an average of nearly 17 in a given state and year over this period (Table 1). As noted earlier, the surprisingly high frequency of lethal domestic violence by women in the United States over this period is a well documented though not

16. This time period covers the period just before and after the bulk of unilateral reforms. Some of the models address this issue in further detail by examining the results based on even shorter panels.

17. These yearly data files are available through the Inter-University Consortium for Political and Social Research (ICPSR Study 8676).

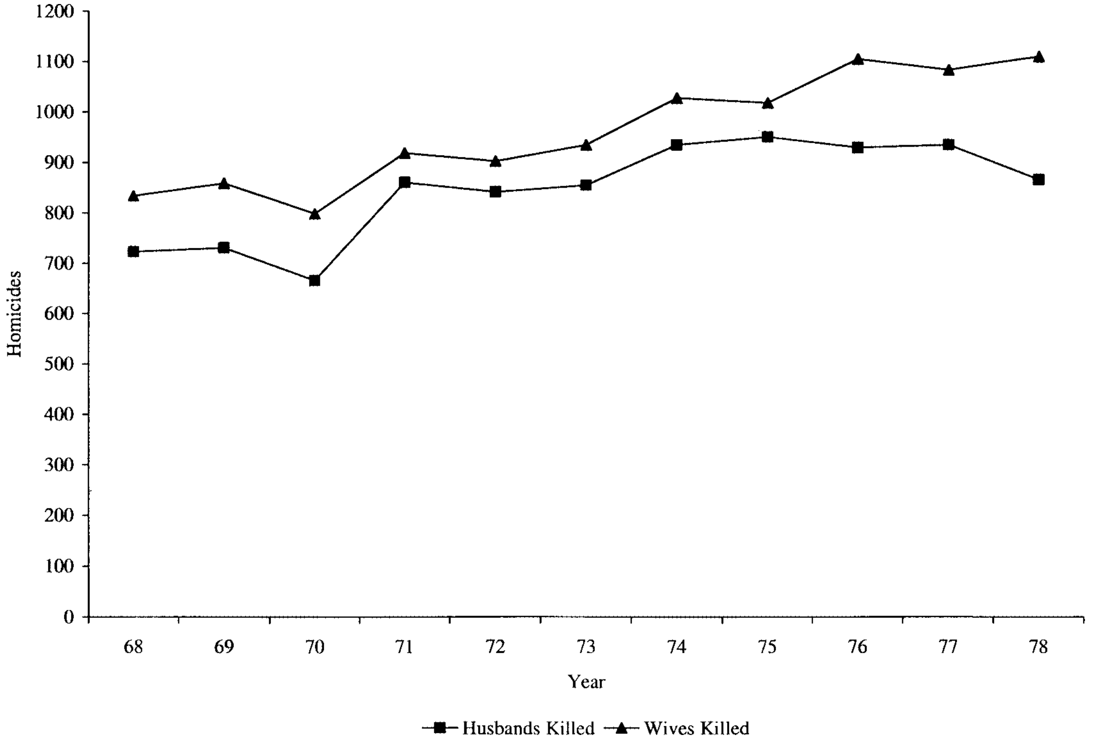
18. This is particularly so given the pattern of results and that the specifications condition on the unobserved state-specific and year-specific determinants that may reflect reporting propensities. Also, I found that all state-year cells contained nonzero total homicides, except for Vermont in 1969. The lack of reported murders for Vermont in 1969 may be accurate; totals of only five, four, and three homicides were reported for 1968, 1970, and 1971, respectively. Nonetheless, this study's results are robust to simply excluding all data from Vermont.

19. These data include the District of Columbia. Therefore, there are 561 state-year observations of spousal homicide counts for victims of each gender.

20. Though it might have been intriguing to distinguish the empirical results among these types of spouses, this aggregation is not problematic. In fact, it ensures that there is not a merely mechanical reduction in observed spousal homicides associated with the law-induced increase in divorces.

21. This is not entirely surprising because year fixed effects should purge the shared variation that might distinguish these periods. However, the persistence of the empirical results in shorter panels also points to their robustness.

FIGURE 1
Total Spousal Homicides by Year and Gender, 1968–78 SHRs



widely known phenomenon (e.g., Wilson and Daly, 1992; Zuger, 1998).[22]

The 1968–78 period for which these spousal homicide counts are defined was characterized by considerable within-state variation in divorce laws. Almost all states adopted laws over this period that eased restrictions on divorce. In particular, most states adopted unilateral divorce laws that allowed a spouse to end a marriage without the consent of the other.[23] In the most basic evaluations presented here, the variation in divorce laws is represented by a simple binary indicator for unrestricted unilateral divorce in a given state and year. The descriptive statistics in Table 1 indicate that 41.5% of the state-year observations of homicidal counts occurred under such a divorce-law regime. However, other evaluations address the potential effects of more detailed attributes of these laws. For example, a few states approved unilateral divorce but only on the condition that the partners had lived apart for a fixed period of time. Because they do not necessarily reduce the costs associated with obtaining a divorce, prior studies (e.g., Peters, 1986; Gray, 1998) have generally assumed that such laws are effectively equivalent to mutual-consent divorce laws. However, this assumption may not be appropriate in this context. For example, laws that allow unilateral

22. However, as noted, the share of "intimate partner" homicides committed by women has been declining in recent years (Gauthier and Bankston, 1997). Therefore, although the 1968–78 period is relevant given the considerable within-state variation in divorce laws, issues related to extrapolating these inferences to the current period may prove important and are discussed in section V.

23. Five states allowed unilateral divorce in 1968. By 1978, 32 had such laws. Over the subsequent years, only two additional states introduced these reforms. Some states also eased restrictions on divorce over this period by eliminating the need to demonstrate the fault of a particular spouse. However, as in other recent studies (Peters, 1986; Friedberg, 1998; Gray, 1998; Gruber, 2000; Johnson and Mazingo, 2000), the evaluations presented here emphasize the distinction between mutual consent and unilateral divorce law regimes.

TABLE 1
Descriptive Statistics, State-Level Panel Data, 1968–78

Variable	Mean (SD)
Wives killed by husbands	18.9 (23.1)
Husbands killed by wives	16.6 (22.6)
Unilateral divorce	.415 (.493)
Unilateral divorce–community property	.107 (.309)
Unilateral divorce–equitable distribution	.262 (.440)
Unilateral divorce–common law	.046 (.210)
Unilateral divorce with separation requirements	.289 (.454)
Real AFDC expenditures per recipient	1,391 (472)
Law enforcement officers (FTE) per 1,000 persons	2.28 (.92)
Death penalty	.514 (.491)
Unemployment rate	.056 (.020)
Real state personal income per capita (in thousands)	10.40 (1.77)
Stranger homicides per capita	.026 (.023)
Population (in thousands)	4,135 (4,385)
Number of observations	561

Notes: Standard deviations are reported in parentheses. The counts of spousal homicides include common-law spouses, ex-wives, and ex-husbands.

divorce but only after a period of separation may reduce lethal spousal violence against wives by allowing them to extract themselves from an abusive relationship while avoiding immediate and potentially volatile confrontations over the potential dissolution of a marriage. More specifically, under such a regime, the decision to leave a potentially violent spouse does not imply that unilateral divorce can begin immediately and therefore may reduce the prevalence of homicidal confrontations among spouses. Because of this possibility, some of the evaluations presented here distinguish states that allow unilateral divorce after a period of separation from states that continued to allow divorce based only on mutual consent.

Another potentially insightful set of distinctions is examined by evaluating the possibility of heterogeneous effects associated with each reform state's treatment of marital property in divorce settlements. Gray (1998) argued that the laws governing each state's treatment of marital property have an important influence on the financial vulnerability of particular spouses.[24] Because such financial vulnerability can dramatically influence each spouse's reservation level of utility outside the marriage, marital property laws could have important effects on the prevalence and composition of homicidal spousal violence. In general, there are three types of relevant marital property laws (Gray, 1998). Most states that adopted unilateral divorce also had equitable distribution treatment of marital property under which courts have jurisdiction to distribute property (Table 1). However, in common-law states, the distribution of marital assets generally favors the husband, whereas in community-property states, wives are generally favored (Gray, 1998). One caveat with regard to constructing the key variables this finely is that the available sample variation can become quite limited and state-specific. In particular, only 4.6% of the state-year observations occur in states with both unilateral divorce and the common law treatment of marital property that ostensibly favors husbands (Table 1).[25] Fortunately, the subsequent results indicate that this limited sample variation does not substantively attenuate the statistical power of the research design, which is described later.

IV. SPECIFICATIONS

The empirical results presented here are based on Poisson and negative binomial regressions. As already noted, these regression models, which explicitly recognize the count nature of the data, may be able to generate more reliable inferences. A basic count specification would begin by assuming that

24. Weitzman (1985) discusses these types of marital property regimes in more detail.

25. Only four states were simultaneously characterized over this period by unrestricted unilateral divorce and common-law treatment of marital property (Alabama, Georgia, Florida, and Rhode Island). This suggests that some caution should be exercised in the interpretation of this variable because it could reflect state-specific responses unrelated to the treatment of marital property.

the count of spousal homicides in state s and year t, y_{st}, has a Poisson density:

$$\text{(1)} \qquad \mathrm{pr}(y_{st}) = \lambda_{st}^{y_{st}} \exp(\lambda_{st})/y_{st}!$$

The conventional maximum likelihood approach to Poisson regressions would then assume that the mean of the distribution (i.e., λ_{st}) has a functional form such that $\ln(\lambda_{st}) = \mathbf{X}_{st}\boldsymbol{\beta}$ where $\mathbf{X}_{st}$ is a $(1 \times k)$ vector of regressors that characterize state s in year t.[26] However, to control for unobserved state fixed effects (i.e., α_s), most of the models presented here instead assume that $\ln(\lambda_{st}) = \alpha_s + \mathbf{X}_{st}\boldsymbol{\beta}$ and that equation (1) represents the density of y_{st} conditional on α_s and $\mathbf{X}_{st}$. Hausman et al. (1984), following Andersen (1970), develop a CML approach to estimating the likelihood function based on this density. More specifically, a key feature of this approach is defining the joint distributions of the annual counts within each state (i.e., $y_s \equiv (y_{s1}, y_{s2}, \ldots, y_{sT})$) conditional on $\Sigma_t y_{st}$. This joint distribution has the following form (Hausman et al., 1984):

$$\text{(2)} \quad \mathrm{pr}(y_s|\Sigma_t y_{st}) = ((\Sigma_t y_{st})!/(\Pi_t y_{st}!))\Pi_t p_{st}^{y_{st}}$$

where

$$\begin{aligned} \text{(3)} \quad p_{st} &\equiv \lambda_{st}/\Sigma_t \lambda_{st} \\ &= \exp(\alpha_s + \mathbf{X}_{st}\boldsymbol{\beta})/\Sigma_t \exp(\alpha_s + \mathbf{X}_{st}\boldsymbol{\beta}) \\ &= \exp(\mathbf{X}_{st}\boldsymbol{\beta})/\Sigma_t \exp(\mathbf{X}_{st}\boldsymbol{\beta}). \end{aligned}$$

Ignoring the constant terms, the log likelihood based on this conditional approach is simply $\Sigma_s \Sigma_t y_{st} \log(p_{st})$. The usefulness of this conditional approach is evident in equation (3), which indicates that the unobserved, state-specific determinants of y_{st} are completely eliminated from the estimating procedure.

Nonetheless, the use of this evaluation strategy raises another important specification issue. A well-known and potentially unwarranted restriction implicit in Poisson regression models is the assumption of equal means and variances (i.e., $E(y_{st}) = \lambda_{st} = \mathrm{var}(y_{st})$). In empirical applications, it is typically found that count data are in fact characterized by overdispersion (i.e., $\mathrm{var}(y_{st}) > E(y_{st})$). Casual observation of the means and standard deviations in Table 1 indicates that the state-level counts of spousal homicides are highly overdispersed as well.[27] Under rather weak conditions the presence of overdispersion does not introduce inconsistency into the estimated regression coefficients, but it does lead to inconsistent standard errors that overstate the estimated coefficients' precision. Standard corrections for the presence of overdispersion are typically based on negative binomial regressions. In conventional negative binomial regressions, it is assumed that the distribution of y_{st} conditional on λ_{st} is Poisson and that λ_{st} follows a gamma distribution (e.g., Grogger, 1990). The implied likelihood function based on the mixture distribution of y_{st} effectively accommodates overdispersion through the introduction of a "nuisance" parameter that reflects the stochastic nature of λ_{st} and is estimated along with the regression coefficients. Hausman et al. (1984) extend the CML approach to develop a negative binomial regression that includes fixed effects and accommodates overdispersion. Given the strong evidence of overdispersion in these data, evaluations based on that approach are also reported here.[28] As an additional check on the robustness of the results from these models, I also discuss the results from a simple OLS version of this model in which the natural log of the homicide counts is the dependent variable and the independent variables include state and year fixed effects as well as other state-year regressors.[29]

These basic evaluation strategies improve substantively on earlier empirical research (Brinig and Crafton, 1994; Ellman and Lohr, 1997) in two ways. One is by employing

26. This conventional functional form for λ_{st} ensures against negative predicted values.

27. Unfortunately, conventional tests for overdispersion, which are based on predicted values from Poisson regressions (e.g., Cameron and Triveldi, 1990), cannot be employed for these fixed-effects specifications because the fixed effects are not estimated. However, tests based on conventional Poisson regressions confirm the presence of highly significant overdispersion and the results of the negative binomial regressions reported here provide some indication of the importance of this overdispersion for this study's key inferences.

28. In developing a joint distribution for a state's homicide counts, the nuisance parameter that measures the extent of overdispersion is, like the state fixed effects, eliminated and therefore is not estimated (Hausman et al., 1984).

29. Following Hausman et al. (1984), this ad hoc specification also sets $\ln(y_{st})$ equal to zero and includes a dummy variable for observations with zero homicides.

regression procedures that explicitly recognize the count nature of the data. A second, critical improvement is the inclusion of state fixed effects that purge the confounding influence of unobserved cross-state heterogeneity. The strong biases introduced by ignoring unobserved state-specific determinants are illustrated by comparing the results of fixed-effects models to the results of conventional specifications that omit these controls. However, the recognition of unobserved state fixed effects does not obviate all concerns about important specification issues like the role of omitted variables. This study addresses the possibly confounding influence of omitted variables in these evaluations by first presenting the results of sparse specifications where $\mathbf{X}_{st}$ includes only the binary indicator(s) representing the divorce laws in state s and year t, year fixed effects, and the natural log of the state-year population estimates from the U.S. Census Bureau. The year fixed effects provide unambiguous controls for the shared time-series variation in spousal homicides. The use of the log-transformed population as a regressor is an unrestrictive modification of the Poisson model similar to those used in applications where the observed counts vary by exposure time instead of population size (Winkelmann and Zimmermann, 1995). As an aside, the use of the population variable as a regressor does raise the reasonable question of whether spousal homicides could also be modeled as a percentage of the population. That approach is rejected here because there are several reasons to believe that such a research design would have weak statistical power. The relevant denominator for such a rate (i.e., the ever married population in a given state and year) was not routinely estimated by the U.S. Census Bureau over this period. And the use of plausible proxies (e.g., estimates of the total population) might introduce considerable measurement error. The relative magnitude of this measurement error would be particularly large given that the counts of spousal homicides are quite small in relation to states' populations.[30] Regardless, the results reported here are robust to the exclusion of the population variable, which suggests that scale effects associated with the changes in state populations do not have a confounding influence on this study's key inferences. In fact, in some specifications the inclusion of such regressors appears only to increase the precision of the evaluation parameters of interest substantially.

Are there other state-year variables whose omission might bias these evaluations? The compelling ad hoc evidence that the within-state timing of unilateral divorce laws was independently given (e.g., Friedberg, 1998) suggests that this is unlikely. Nonetheless, this study directly evaluates the robustness of results based on these sparse specifications by estimating models that introduce several other possibly salient variables that varied within states over time. For example, some specifications introduce the state-by-year unemployment rate and the real state personal income per capita because the economic opportunities and strain created by cyclic variation in employment and earnings could conceivably influence patterns of lethal domestic violence.[31] Another possibly important determinant that is included in these evaluations is the level of welfare generosity within a given state and year. The level of welfare support could conceivably influence a woman's propensity for committing lethal violence as well as for victimization by changing the reservation level of utility outside the marriage. This study adopts real Aid to Families with Dependent Children (AFDC) expenditures per recipient as a measure of the welfare generosity in a given state and year. It is also conceivable that the prevalence of spousal homicides is influenced by the probability of detection and by the level of punishment. These potential determinants are represented here by per-person counts of state and local law enforcement officers (full-time equivalents) and by a binary indicator for the presence of the death penalty.[32]

30. Over 50% of the state-year-gender observations have counts of 10 or fewer spousal homicides.

31. The likely effect of this variable is theoretically ambiguous. For example, economic expansions might allow husbands to "buy" more violence but could also raise the threat points of wives.

32. There was considerable variation in death penalties over this period. A 1972 U.S. Supreme Court decision effectively invalidated death penalties as typically constituted. However, states began instituting newly tailored death penalty statutes shortly thereafter. The number of executions could also serve as a reasonable proxy. However, between 1968 and 1978, there was only one state execution (Utah in 1977). The limited application of the death penalty over this period suggests that this variable may not be particularly relevant.

An important concern with some of these additional regressors is that they could be endogenously determined. Nonetheless, their inclusion in some of these evaluations provides meaningful evidence on the robustness of this study's key inferences. As an additional robustness check, some models introduce a broad proxy for the omitted variables associated with increased violence, the rate of homicides committed by unknown assailants. A final set of robustness checks is based on state laws relating to firearm use and availability. For example, over this period, a number of states introduced minimum sentencing rules or extra prison terms for felonies involving guns. Moody and Marvell (1995) found that such firearm sentence enhancement laws had no detectable influence on homicides or other crimes. Nonetheless, because they did not address spousal homicides specifically, some of the models discussed here also introduce a binary indicator for the adoption of a firearm sentence enhancement law.[33] As many as 14 states may have also instituted concealed weapon laws and waiting periods for firearm purchases over this period. The possibly confounding influence of these changes is examined by replicating the key results in models that simply omit these states.[34]

V. RESULTS

This section presents the key results of Poisson and negative binomial regressions where the dependent variables are state-year-gender counts of spousal homicides from the 1968–78 period. In both models the reported coefficients can be interpreted as the mean proportionate change in the dependent variable associated with a one-unit change in the independent variable.

33. Three states (IN, NE, and NM) were excluded from these checks because Moody and Marvell (1995) only indicate that their firearm enhancement laws became effective "before 1970."

34. I would like to thank David Mustard for generously providing this information. Unfortunately, the exact effective date for many of these older regulations is unclear so these events cannot be represented as regressors. However, the robustness of the evaluation results to the exclusion of these states suggests strongly that their omission is not confounding and confirms prior evidence that the within-state adoption and timing of these divorce reforms was independently given.

The Importance of Unobserved State-Specific Determinants

One of the novel contributions of this study is the introduction of state fixed effects, which provide unambiguous controls for the state-specific but unobserved determinants of lethal spousal violence. To provide continuity with prior research, this section addresses briefly whether the conventional omission of these controls in prior studies could have led to biased inferences. More specifically, the relevance of these controls is illustrated by comparing the results of conventional maximum likelihood (ML) estimates that omit state fixed effects to the CML estimates that do not. The key results of these regressions are reported in Table 2. These models uniformly represent unilateral divorce laws by two binary indicators: one for an unrestricted law and another for a law that was subject to a separation requirement. All of the sparse specifications reported in Table 2 also include as regressors year fixed effects and the natural log of the state-year population estimates. The results in the top part of Table 2 are based on spousal homicides of wives. The models that omit state fixed effects suggest that unilateral divorce led to large and statistically significant increases in counts of murdered wives. For example, the Poisson and negative binomial models suggest that both types of unilateral divorce significantly increased the number of murdered wives by amounts ranging from 27% to 34%. These results parallel those reported by Brinig and Crafton (1994), who reported a significant partial correlation between the cross-state variation in no-fault divorce laws and crisis calls from victims of spousal abuse. However, the fixed-effects models indicate that these results are quite sensitive to the presence of unobserved and state-specific determinants of spousal violence. More specifically, the fixed-effects versions of Poisson and negative binomial regressions indicate that both types of unilateral divorce laws had relatively small and statistically insignificant effects on spousal homicides of wives.[35]

The empirical models for spousal homicides of husbands similarly underscore

35. Though these estimates have fairly wide confidence intervals, the large estimates based on cross-state variation can be rejected.

TABLE 2
ML and CML Estimates of the Effects of Unilateral Divorce Laws on Spousal Homicides, Poisson and Negative Binomial Regressions with and without State Fixed Effects

Regression model	State fixed effects?	Estimated effects: Unilateral divorce	Estimated effects: Unilateral divorce with separation requirement
Dependent variable: wives killed by husbands			
ML–Poisson	no	.323* (.026)	.338* (.027)
ML–negative binomial	no	.268* (.066)	.328* (.064)
CML–Poisson	yes	.070 (.046)	.014 (.049)
CML–negative binomial	yes	.061 (.059)	.002 (.063)
Dependent variable: husbands killed by wives			
ML–Poisson	no	.528* (.028)	.537* (.030)
ML–negative binomial	no	.388* (.092)	.475* (.088)
CML–Poisson	yes	.214* (.051)	.040 (.052)
CML–negative binomial	yes	.226* (.072)	.039 (.076)

Notes: All specifications include year fixed effects and the log of the state-year population. Standard errors are reported in parentheses.
*Statistically significant at the 1% level.

the potentially confounding, positive bias imparted by the unobserved state-specific determinants of spousal violence. The ML estimates, which effectively rely on the cross-state variation in unilateral divorce laws, suggest that both restricted and unrestricted unilateral divorce led to very large and statistically significant increases in counts of murdered husbands. But in the fixed-effects models, only the introduction of unrestricted unilateral divorce laws appears to be associated with statistically significant increases in murdered husbands (21%–23%). Like the results for wives murdered, these results demonstrate that the omission of state fixed effects imparts a positive bias, particularly in Poisson regressions. Also, another ad hoc indication that state fixed effects provide important controls is that when they are included, the Poisson and negative binomial regressions generate the qualitatively similar point estimates that would be expected from well-specified empirical models.

Fixed Effects Results

The results from the previous section provided important evidence that the unobserved state-specific determinants of spousal violence can exert a confounding influence on policy evaluations. Those results also provided some novel and suggestive evidence regarding the effects of unilateral divorce on spousal homicides. By presenting the results of more varied fixed-effects specifications, this section evaluates the impact of unilateral divorce laws on spousal homicides more completely. Table 3 begins by presenting the results of Poisson and negative binomial regressions where the dependent variable is spousal homicides of wives. In these models, unilateral divorce laws are represented simply by a binary indicator for the adoption of an unrestricted law and by a second binary indicator for the adoption of unilateral divorce conditional on fixed period of separation. In addition to conditioning on state fixed effects, all of the models in Table 3

TABLE 3
CML Estimates of Fixed Effects Spousal Homicide Models, Homicides of Wives

	Poisson regressions						Negative binomial regressions					
Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**
Unilateral divorce	.065	.070	.077*	.081*	.084*	.088*	.060	.061	.057	.060	.058	.062
	(.043)	(.046)	(.043)	(.047)	(.043)	(.047)	(.053)	(.059)	(.053)	(.059)	(.053)	(.059)
Unilateral divorce with separation requirements	—	.014	—	.011	—	.012	—	-.002	—	.008	—	.009
		(.049)		(.050)		(.050)		(.063)		(.063)		(.062)
Unemployment rate	—	—	2.4*	2.4*	2.3	2.3	—	—	2.7	2.6	2.6	2.6
			(1.4)	(1.4)	(1.4)	(1.5)			(1.9)	(1.9)	(1.9)	(1.9)
Real state personal income per capita	—	—	.18**	.18**	.18**	.18**	—	—	.16**	.16**	.16**	.16**
			(.05)	(.05)	(.05)	(.05)			(.05)	(.05)	(.05)	(.05)
Real AFDC expenditures per recipient	—	—	.0001	.0001	.0001	.0001	—	—	.0001	.0001	.0001	.0001
			(.0001)	(.0001)	(.0001)	(.0001)			(.0001)	(.0001)	(.0001)	(.0001)
Law enforcement officers (FTE) per 1,000 persons	—	—	.04	.05	.07	.07	—	—	−.04	−.04	−.03	−.03
			(.08)	(.08)	(.08)	(.08)			(.10)	(.10)	(.10)	(.10)
Death penalty	—	—	.01	.01	.001	.001	—	—	.01	.01	.01	.005
			(.04)	(.04)	(.04)	(.04)			(.05)	(.05)	(.05)	(.05)
Stranger homicides per capita	—	—	—	—	2.6*	2.6*	—	—	—	—	1.1	1.1
					(1.4)	(1.4)					(1.8)	(1.8)
ln(Population)	1.0**	1.0**	.39	.41	.33	.34	.87**	.87**	.73**	.73**	.70**	.70**
	(.29)	(.30)	(.34)	(.34)	(.34)	(.34)	(.16)	(.16)	(.21)	(.22)	(.21)	(.22)
Log likelihood	−1,356	−1,356	−1,348	−1,348	−1,346	−1,346	−1,317	−1,317	−1,312	−1,312	−1,311	−1,311

Notes: All specifications condition on state and year fixed effects. Standard errors are reported in parentheses.
* and ** indicate significance at the 10% and 1% level, respectively.

include as explicit regressors year fixed effects and the natural log of the state-year population. The subsequent models in Table 3 then introduce as additional regressors the unemployment rate, real state personal income per capita, real AFDC expenditures per recipient, law enforcement officers per person, a binary indicator for the death penalty, and the rate of homicides by strangers.

The estimated coefficients from these regressions are strikingly uniform with respect to both the choice of regression procedure and the introduction of additional regressors. More specifically, these regressions generally indicate that both types of unilateral divorce laws had relatively small and statistically insignificant effects on spousal homicides of wives. In some of the Poisson regressions, unilateral divorce is weakly associated with increased spousal violence against wives. However, given the overdispersion in the count variable, the larger standard errors implied by the negative binomial model are preferred. Another striking feature of these results is that increases in per capita income appear to increase the number of spousal homicides of wives.[36] The within-state changes in welfare generosity, the size of the police force and the presence of a death penalty all have uniformly small and statistically insignificant effects on these counts. The rate of stranger homicides per capita is positively associated with lethal violence against wives but is weakly significant and only in the Poisson regressions.

As already noted, the surprisingly high prevalence of spousal violence against men is a well-documented but little-known phenomenon (e.g., Wilson and Daly, 1992; Zuger, 1998). Roughly 47% of spousal homicides over this 1968–78 period were committed by wives. The results in Table 4 provide more complete evidence on whether state divorce policies influenced the pattern of lethal domestic violence against husbands. As with the prior evaluations, these models generate results that are strikingly uniform with respect to the regression method and the introduction of included regressors. More specifically, these regressions indicate that unrestricted unilateral divorce increased spousal homicides of husbands by 20% to 25%. Even in the negative binomial regressions, the estimated increases in slain husbands are quite precise (p-values less than 0.01). These models also suggest that unilateral divorce subject to a separation requirement led to more modest and statistically insignificant effects. The Poisson regressions suggest that some of the other control variables also have a significant influence on homicides of husbands (e.g., welfare generosity, income variation, stranger homicides per capita). However, in the negative binomial regressions, only the proxy for omitted determinants for violence (stranger homicides per capita) is statistically significant at the 5% level.

The results in Table 3 indicate that the adoption of unilateral divorce appeared to have no detectable effect on the prevalence of male-on-female spousal violence. However, the results in Table 4 indicate that the easy access to divorce created by these new regulations increased female-on-male spousal homicides by roughly 21%. These results appeared to be quite robust to the inclusion of additional controls. However, I also examined the robustness of these results in a number of other ways. One is by replicating the evaluations for the shorter sample periods, 1968–75 and 1968–72. As noted, focusing on these shorter periods is useful in part because the coding of spousal homicides in the SHRs was more consistent within these periods. The key results from these evaluations are reported in Table 5. Like the results in Tables 3 and 4, the estimates from the shorter panels indicate that the only statistically significant effect of these reforms was to increase spousal homicides of husbands. Although the estimated effect in the shortest panel is smaller (a 17.7% increase) and somewhat less precise, this law-induced change is still qualitatively large and statistically significant at the 5% level.

I also conducted additional robustness checks by exploiting the available data on three state laws related to firearms. For example, negative binomial models that include a control for the presence of firearm sentence enhancement laws suggest that the introduction of unilateral divorce had a

36. These results imply that an increase in state personal income per capita of $1,000 would generate a 16% increase in spousal homicides of wives. This result is consistent with the views that macroeconomic expansions lead to stress-induced increases in violence and an increased ability to "purchase" spousal violence.

TABLE 4
CML Estimates of Fixed Effects Spousal Homicide Models, Homicides of Husbands

	Poisson regressions						Negative binomial regressions					
Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**
Unilateral divorce	.199***	.214***	.214***	.238***	.232***	.255***	.209***	.226***	.217***	.243***	.228***	.250***
	(.047)	(.051)	(.048)	(.052)	(.048)	(.053)	(.065)	(.072)	(.064)	(.071)	(.064)	(.071)
Unilateral divorce with separation requirements	—	.040	—	.059	—	.057	—	.039	—	.062	—	.056
		(.052)		(.053)		(.053)		(.076)		(.075)		(.075)
Unemployment rate	—	—	−.87	−1.0	1.3	−1.5	—	—	−4.0*	−4.0*	−3.8*	−3.9*
			(1.6)	(1.6)	(1.6)	(1.6)			(2.2)	(2.2)	(2.2)	(2.2)
Real state personal income per capita	—	—	.22***	.22***	.21***	.21***	—	—	.06	.06	.09	.09
			(.05)	(.05)	(.05)	(.05)			(.06)	(.06)	(.07)	(.07)
Real AFDC expenditures per recipient	—	—	.0003***	.0003***	.0003**	.0003**	—	—	.0002	.0002	.0002	.0002
			(.0001)	(.0001)	(.0001)	(.0001)			(.0002)	(.0002)	(.0002)	(.0002)
Law enforcement officers (FTE) per 1,000 persons	—	—	.15	.14	.25**	.25**	—	—	−.04	−.05	.08	.08
			(.10)	(.10)	(.10)	(.10)			(.13)	(.13)	(.13)	(.14)
Death penalty	—	—	−.01	−.01	−.04	−.05	—	—	.03	.03	−.01	−.01
			(.05)	(.05)	(.05)	(.05)			(.06)	(.06)	(.07)	(.06)
Stranger homicides per capita	—	—	—	—	8.1***	8.1***	—	—	—	—	5.9**	5.8**
					(1.6)	(1.6)					(2.4)	(2.5)
ln(Population)	.56*	.61*	−.17	−.20	−.36	−.28	.74***	.75***	.75***	.78***	.49*	.53*
	(.33)	(.33)	(.37)	(.38)	(.37)	(.38)	(.18)	(.18)	(.19)	(.19)	(.29)	(.29)
Log likelihood	−1,314	−1,314	−1,296	−1,296	−1,283	−1,283	−1,246	−1,246	−1,241	−1,241	−1,238	−1,238

Notes: All specifications condition on state and year fixed effects. Standard errors are reported in parentheses.
*, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

TABLE 5
CML Estimates of Fixed Effects Negative Binomial Models for Spousal Homicides by Sample Years

	Estimated effects	
Sample years	**Unilateral divorce**	**Unilateral divorce with separation requirements**
Dependent variable: wives killed by husbands		
1968–1978	.061	.008
	(.059)	(.062)
1968–1975	.057	−.014
	(.064)	(.070)
1968–1972	.107	−.025
	(.079)	(.098)
Dependent variable: husbands killed by wives		
1968–1978	.247**	.063
	(.071)	(.075)
1968–1975	.221**	.045
	(.075)	(.081)
1968–1972	.177*	−.014
	(.083)	(.091)

Notes: All specifications condition on state and year fixed effects, the state unemployment rate, real state personal income per capita, and the natural log of the state-year population. Standard errors are reported in parentheses.

* and ** indicate significance at the 5% and 1% level, respectively.

statistically insignificant effect on male-on-female spousal homicides (p-value $= 0.215$) but increased female-on-male spousal homicides by 25% (p-value $= 0.001$). Models that exclude states with possible variation over this period in their laws on concealed weapons and waiting periods for firearm sales also generate results similar to those in Tables 3 and 4. More specifically, these models suggest that unilateral divorce had statistically insignificant effects on spousal homicides of wives (p-value of 0.486) but increased spousal homicides of husbands by 28.8% (p-value of 0.001). I also replicated the basic results in Tables 3 and 4 in simple two-way fixed-effects model estimated by OLS. The results again suggested that unilateral divorce laws had an insignificant effect on spousal homicides of wives (p-value $=$ 0.198) but generated a statistically significant increase of 17.1% in spousal homicides of husbands (p-value $= 0.047$).

It should be noted that a new study by Stevenson and Wolfers (2000) also presents fixed-effects evaluations of how unilateral divorce influenced spousal homicides but reports results quite different from those presented here.[37] However, their research design differs from that adopted here in at least three ways. First, they use a much longer panel that extends from 1968 to 1994. Second, they model homicide rates (denominated by state-year-gender population estimates) instead of homicide counts. Third, their representation of the state laws differs, in part because they do not distinguish states that introduced unrestricted unilateral divorce from those that only allowed it after a fixed period of separation.[38] In an attempt to assess the empirical relevance of these differences, I replicated their data set and reported results.[39] In examining the evaluation results based on these data, I found that their results were sensitive to the use of homicide rates instead of counts as well as to their representation of the state laws. For example, a count-data model based on their data suggests that unilateral divorce had smaller and statistically insignificant effects on spousal homicides of wives. An OLS model of spousal homicide rates indicates that the prevalence of slain husbands increased significantly (by roughly 18%) after the introduction of unilateral divorce laws that were not subject to separation requirements.

However, using homicide rates instead of counts may create important signal-to-noise problems because spousal homicides are typically quite low relative to the entire state-year population estimates.[40] Furthermore, an

37. Specifically, their evaluations suggest that unilateral divorce reduced the number of females murdered by their spouses by roughly 10% and had positive but statistically insignificant effects on the number of husbands murdered by their wives.

38. Their study also uses the law classifications reported by Friedberg (1998). This study uses the updated classifications reported by Gruber (2000).

39. It should be noted that their SHR data for the 1976–94 period (ICPSR Study #6754) only includes "criminal" homicides and excludes justifiable homicides and negligent manslaughters, which may disproportionately involve spouses (in particular, female-on-male violence).

40. For example, Stevenson and Wolfers (2000) report an average of just 7.3 females murdered by spouses per million women in the state. Nearly 10% of their state-year-gender cells have zero counts. The median state-year count of spousal homicides of wives is only 10 and of husbands, only 5.

approach based on 1968–94 state-level panel data may also be problematic because it extends well beyond the within-state variation in divorce laws. According to the law coding used by Stevenson and Wolfers (2000), 25 of the 37 within-state law changes had already occurred within just the first six years of the panel (i.e., by 1973). By 1977, 34 of the 37 states had adopted unilateral divorce; the remaining three changes had all occurred by 1985. The use of much shorter panels should also be conceptually important for interpreting the evaluation results because it allows us to isolate the effect of unilateral divorce among couples who had been surprised by the dramatic shift in their marriage contract. Longer panels encompass periods when the stock of ever-married people may have changed along with the circumstances under which they chose to form a marriage contract and make related human capital decisions.

Stevenson and Wolfers (2000) deal somewhat with the former issue by using homicide rates defined more broadly to include homicides by a family member and homicides by any nonstranger. In models based on these data, they get similar results: unilateral divorce reduced each homicide rate for females by 9%. However, the constancy of the effect sizes associated with these different homicide rates is actually quite surprising. For females, the broader homicide rates are on average two to three times larger than the spousal homicide rates. It is odd, therefore, that unilateral divorce should still appear to generate a 9% to 10% reduction in these rates because they include many homicides for which the new divorce laws should have clearly been much less relevant (e.g., homicide by a known assailant who is not a family member or boyfriend).

Heterogeneity with Respect to the Treatment of Marital Property

The evaluation results from Tables 2, 3, 4, and 5 suggest that unrestricted unilateral divorce did not reduce lethal spousal violence against women but instead lead to a sharp increase in lethal violence against husbands. However, these results may be somewhat incomplete because they do not address how the adoption of unilateral divorce interacted with the state-specific treatment of marital property. For example, unrestricted unilateral divorce could have lead to increases in opportunistic violence against women only in the states where husbands' bargaining positions were also enhanced by a favorable division of property in divorces. Alternatively, it would also be useful to examine whether the observed increases in slain husbands were larger in states where the economic consequences of divorce were particularly damaging to women. The regression results presented in Tables 6 and 7 address these questions. These results are based on negative binomial regressions that provide conservative standard errors and condition on year fixed effects, the natural log of the state-year population and the macroeconomic controls.[41]

TABLE 6
CML Estimates of Fixed Effects Negative Binomial Models for Spousal Homicides of Wives, by Property Treatment

Variable	(1)	(2)	(3)
Unilateral divorce	.062 (.059)	—	—
By treatment of marital property			
Community property	—	.063 (.082)	.064 (.093)
Equitable distribution	—	.017 (.072)	.017 (.075)
Common law	—	.143 (.099)	.143 (.100)
Unilateral divorce with separation requirements	.008 (.062)	—	.001 (.065)
Log likelihood	−1,312	−1,312	−1,312

Notes: All specifications condition on state and year fixed effects, the state unemployment rate, real state personal income per capita, and the natural log of the state-year population. Standard errors are reported in parentheses.

The results in Table 6 focus on spousal homicides of wives. Like the prior results, these models suggest that unrestricted unilateral divorce had no effect on spousal homicides of wives. Consideration of state laws regarding the treatment of marital property or the existence of fault grounds for property division does not alter these conclusions. It is suggestive of opportunistic violence

41. These results of these relatively sparse specifications are robust to the inclusion of the other insignificant regressors.

TABLE 7
CML Estimates of Fixed Effects Negative Binomial Models for Spousal Homicides of Husbands, by Property Treatment

Variable	(1)	(2)	(3)
Unilateral divorce	.247** (.071)	—	—
By treatment of marital property			
Community property	—	.093 (.098)	.116 (.113)
Equitable distribution	—	.307** (.091)	.319** (.095)
Common law	—	.245* (.114)	.248* (.115)
Unilateral divorce with separation requirements	.063 (.075)	—	.032 (.079)
Log likelihood	−1,242	−1,241	−1,241

Notes: All specifications condition on state and year fixed effects, the state unemployment rate, real state personal income per capita, and the natural log of the state-year population. Standard errors are reported in parentheses.

* and ** indicate significance at the 5% and 1% level, respectively.

that the point estimates are larger in states where property treatment putatively favors husbands (i.e., common-law states). However, these estimates are highly imprecise. The regression results in Table 7 provide important evidence on whether the reform-driven increases in slain husbands varied by each state's treatment of marital property. These results suggest that the increase in lethal spousal violence against husbands did vary in a striking but plausible way with the property laws. For example, in the community property states whose property laws favored wives, the introduction of unilateral divorce was actually associated with relatively small and statistically insignificant changes in counts of murdered husbands. In contrast, in the equitable distribution and common-law states, where the distribution of marital property looks with more favor on the husbands, the adoption of unilateral divorce was associated with substantially larger increases in spousal homicides. More specifically, the negative binomial regressions suggest that in these states, unrestricted unilateral divorce increased the annual counts of slain husbands by a statistically significant 25%–32%.

Discussion

These evaluations provide evidence that an important and unintended consequence of unrestricted unilateral divorce laws was a sharp increase in the number of husbands killed by their wives. But these evaluations also indicate that those same divorce laws had small and statistically insignificant effects on the amount of lethal spousal violence directed at women. These findings are clearly important for understanding and evaluating the extensive state-level experiences with relaxed access to divorce. This is partly because these results suggest that unilateral divorce laws may not have been successful in reducing the prevalence of lethal spousal violence against women. However, that inference should be made with some caution because the long-term response among spouses could conceivably be quite different. A virtue of this study's focus on the near-term effects of unilateral divorce is that it allows us to identify the behavioral responses among spouses for whom a strong marriage contract suddenly and unexpectedly weakened. In particular, the reform-induced changes in the patterns of spousal homicides over this period provides unique, indirect evidence on how unilateral divorce changed the distribution of bargaining power within marriages and whether this change was particularly harmful to the wives who had anticipated a stronger marriage contract.

What do these results tell us about easier access to divorce, the nature of household bargaining and domestic violence? First, the evidence of increased spousal murders of husbands suggests that unilateral divorce lowered wives' reservation levels of utility outside marriage, their credible threat points. But the fact that these reforms did not generate a detectable increase in spousal killings of wives implies that husbands did not exploit these changed threat points by engaging in more opportunistic spousal violence. These empirical results contradict the prior cross-sectional evidence, which suggested that easy access to divorce did allow husbands to extract more opportunistic violence from women. However, the results presented in Table 2 reconciled this conflicting empirical evidence by demonstrating that the inferences from prior cross-sectional evaluations appear to be biased because they confound the effects of the new divorce regulations with the unobserved and state-specific determinants of domestic violence.

This study's results make a second contribution to our understanding of domestic violence by providing evidence that relaxed access to divorce created a threat of economic harm that influenced the understudied phenomenon of female-on-male spousal violence. In particular, these results imply that wives' threat points (i.e., their reservation levels of utility outside of marriage) were important determinants of the lethal domestic violence they committed and were closely related to their postdivorce economic well-being. Two dimensions of these results support this unique economic interpretation of the links between unilateral divorce and the increased spousal homicides of husbands. First and foremost, the heterogeneity of the increases in slain husbands with respect to the marital property laws indicates that economic deprivation was indeed a key determinant of the increases in spousal homicides committed by wives. The increases in murdered husbands were concentrated in those states where laws on the division of marital property generally favored husbands over wives. Second, we can largely dismiss the alternative interpretation that more wives killed their husbands simply in acts of self-defense against increased opportunistic violence. The small and statistically insignificant links between unilateral divorce and lethal spousal violence directed at wives suggests that this interpretation is not easily tenable. This study's results make a third contribution to our understanding of divorce regulations by providing unique evidence on the controversial empirical question of whether women were particularly disadvantaged by the introduction of divorce law reforms (Gray, 1996; Peters, 1986; Jacob, 1989). The increase in lethal spousal violence identified in this study provides compelling indirect evidence that women did perceive themselves to be economically disadvantaged by a new divorce law regime that allowed their husbands to dissolve their marriage without first establishing with them terms of mutual consent.

Any extrapolation of the impact that divorce law reforms had in the 1960s and 1970s to the current debate over rescinding these reforms will necessarily be qualified because of other substantive changes in the legal, cultural, and economic environment since that time. But given that important caveat, these results do provide some guidance to states that are considering rescinding earlier divorce law reforms. First, these results clearly suggest that such changes will not influence the amount of spousal violence directed against women. A naive reading of the results presented here might also suggest that newly restricted access to divorce would reduce lethal spousal violence against husbands. However, a caveat against extrapolating the results to the current period is particularly appropriate because a large share of the current stock of wives made decisions about their own human capital and family-specific investments under a weaker marriage contract. Recently married women may have adjusted their behavior over time to attenuate the dire economic consequences of unilateral divorce and the new realities of the weakened marriage contract. This would imply that little or no reduction in the current number of slain husbands can be generated by again restricting access to divorce. Instead, the increase in slain husbands identified here suggests that there may be more subtle social gains to again restricting access to divorce. More specifically, the ability to engage in more effective long-term marriage contracting appears to have been a severe economic loss for wives. This loss was plausibly concentrated in states that did not provide wives a legal environment that led to generous property settlements or alimony. Its reinstatement may generate benefits for wives that are commensurate with the benefits of community property provisions because it would allow them to bargain effectively for more generous terms on divorce. Furthermore, these results clearly suggest that unilateral divorce reduced the family-centered bargaining power of wives. These reductions may have contributed to the pejorative effects that relaxed access to divorce appears to have had on children's development (Gruber, 2000; Johnson and Mazingo, 2000). These observations imply that a stronger marriage contract could protect the economic well-being of women and promote child-specific family investments.

VI. CONCLUSIONS

The ongoing controversy over state policies that influence couples' access to divorce has focused on a broad variety of measures for economic and psychological well-being. However, relatively little attention has been paid to how such policies might influence patterns of domestic violence and how these relationships might inform our understanding of household bargaining and the social consequences of unilateral divorce. Theoretical models of household bargaining offer only ambiguous predictions of how relaxed access to divorce might influence the prevalence of domestic violence. For example, it is reasonable to suppose a priori that such laws reduced domestic violence against wives by facilitating the dissolution of abusive marriages. However, it is also plausible to suspect that an increased threat of divorce and possible economic hardship allowed husbands to extract more opportunistic violence from wives. Furthermore, an increased threat of divorce and economic hardship may have encouraged more wives to kill their husbands. This study provided empirical evidence on these policy-relevant questions by evaluating how the introduction and design of unilateral divorce laws influenced counts of spousal homicides over the 1968–78 period.

The empirical results presented here also improved on the limited prior evidence by evaluating models that recognized the count nature of the data and by employing specifications that unambiguously purged the confounding influence of unobserved state-specific and year-specific determinants. The results of the fixed-effects count data models indicated that the impact of unilateral divorce laws varied by their design and the gender of the victim. More specifically, the widespread adoption of unilateral divorce laws had relatively small and statistically insignificant on the number of wives murdered by their husbands. These empirical models also demonstrated that the introduction of unilateral divorce laws led to a statistically significant increase of roughly 21% in the number of husbands killed by their wives. Notably, the increases in spousal homicides of husbands were concentrated in the states with marital property laws that favored husbands. These results provide important evidence on the intended and unintended effects of unilateral divorce on the prevalence of domestic violence. The pattern of these results also provides new empirical insights into the changes in household bargaining power as well as compelling indirect evidence on the controversial question of whether women perceived themselves to be economically disadvantaged by the widespread introduction of unilateral divorce.

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