Is Making Divorce Easier Bad for Children? The Long-Run Implications of Unilateral Divorce

Jonathan Gruber, *Massachusetts Institute of Technology and National Bureau of Economic Research*

I assess the long-run implications for children of growing up in a unilateral divorce environment, which increases the ease of divorce by not requiring the explicit consent of both partners. Using 40 years of census data to exploit the variation across states and over time in changes in divorce regulation, I confirm that unilateral divorce regulations do significantly increase the incidence of divorce. Adults who were exposed to unilateral divorce regulations as children are less well educated, have lower family incomes, marry earlier but separate more often, and have higher odds of adult suicide.

One of the most striking trends in postwar social indicators in the United States is the rise in divorce rates. Figure 1, updated from Friedberg (1998), illustrates the rate of divorce per 1,000 persons in the United States over time. After staying at low levels for many years, divorce rates began to rise precipitously in the mid-1960s, with the rate of divorce rising by over 200% in only 15 years. This "breakdown of the traditional family" has been decried in many circles, particularly due to its perceived negative implications for children. Indeed, there are large literatures in sociology,

I am grateful to Amber Batata, Jeffrey Hoffner, Becky Neuschatz, Kokkeong Puah, and Ebonya Washington for excellent research assistance, to John Johnson, William Johnson, Chris Mazingo, Justin Wolfers, and seminar participants at MIT and the National Bureau of Economic Research for helpful comments, and to the Smith Richardson Foundation for financial support. Contact the author at gruberj@mit.edu.

[*Journal of Labor Economics*, 2004, vol. 22, no. 4]

0734-306X/2004/2204-0003$10.00

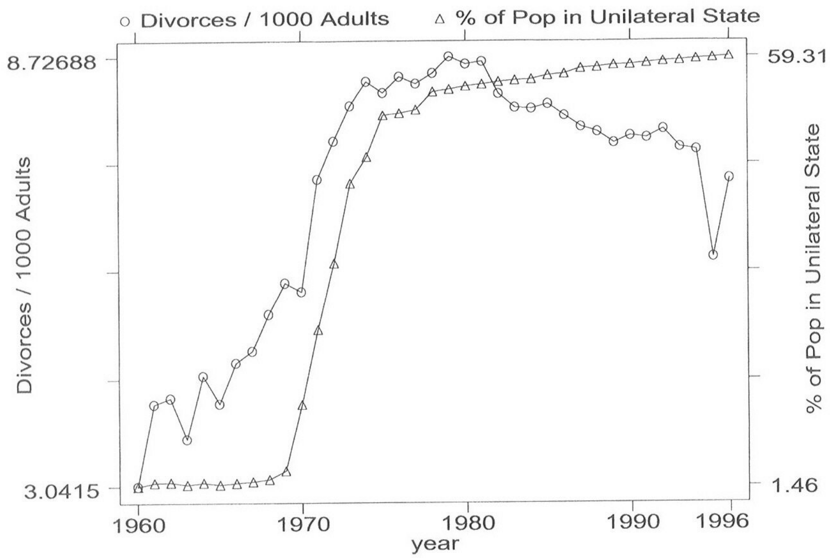

FIG. 1.—Divorce rates and exposure to unilateral divorce

developmental psychology, and economics that document the negative consequences for children of divorce, both as children and then later as adults.

A common villain in these criticisms is state regulations that increase the ease of divorce. The rise in divorce rates corresponds quite strikingly to the advent of state regulations that allowed for unilateral divorce—divorce that does not require the explicit consent of both partners. Unilateral divorce laws were passed in a number of states in the wake of the no-fault divorce revolution that moved the basis for divorce from the fault of one spouse to general "irreconcilable differences" (Weitzman 1985). These unilateral laws substantially increased the ease of divorce by allowing one partner to leave without obtaining the consent of the other. The second line in figure 1 shows the percent of persons living in states with unilateral divorce laws in place. Exposure to these unilateral divorce regimes rose in tandem with divorce rates over the 1970s.

As a result, critics of rising divorce rates have called for a reversal of the 1970s trend toward unilateral divorce regimes, in an effort to maintain the traditional two-parent family and improve child outcomes. Two states (Arizona and Louisiana) have passed "covenant marriage" laws, whereby the couple receives premarital counseling and signs a covenant that makes divorce more costly via separation periods with intensive counseling (although still possible unilaterally). These laws have been proposed in at least 17 other states as well. Too, there is a broader movement in religious communities to increase the stringency of the marriage contract (Nordinger 1998). In a recent debate over such regulations in Virginia, the Associated Press Wire, on February 14, 2000, quoted the legislation's author, Representative McDonnell, as saying that "he was concerned about the rising divorce rate and the impact it was having on families."

The key underlying assumption of this movement is that regulations that increase the ease of divorce have negative implications for children. This line of argument involves three key suppositions: that the increased ease of divorce under state regulations contributed to (or even fully caused) the rising rate of divorce in the United States, that divorce is actually bad for children relative to the counterfactual of maintaining potentially damaged marriages, and that changes in divorce regulation do not have any other impacts on families that may offset any direct influences through divorce, such as through the decision to enter into marriage or through changes in the nature of within-family bargaining. The existing evidence on the first of these suppositions is quite mixed, the evidence on the second has yet to convincingly address potential selection biases associated with the decision to divorce, and there is little empirical work on the third supposition. Thus, on net, there is no convincing evidence that unilateral divorce regulations actually have an adverse impact on child well-being.

The purpose of this article is to provide a careful assessment of the implications for children of making divorce easier, doing so in a framework that allows for integration of all of the issues noted above. I first show that there is a sizable and significant impact of unilateral divorce on the stock of divorced parents of childbearing age. I then directly examine the implications of growing up in a unilateral divorce regime for children when they themselves become adults, allowing for a long-run assessment of the ultimate consequences for children of growing up in an environment in which divorce is easier.

I primarily do so by using data on state divorce regulations, matched to information from the 1960, 1970, 1980, and 1990 censuses. This broad historical sweep allows me to consider both the direct impact of unilateral divorce regimes on the cohorts of adults at the time they are passed and the later impact on their children when the children themselves become adults. I can do so within models that account for both fixed state preferences toward divorce and changes in those preferences over time. I also augment these census findings with aggregate data on suicide rates by age and state.

I have several notable results, besides the finding noted above that unilateral divorce increases the stock of divorces. I find that adults who were exposed to unilateral divorce regulations as children are less well educated and have lower family incomes. They are also more likely themselves to be both married and separated, and both of these effects appear to reflect primarily a shift toward earlier marriage and separation. Women in these exposed cohorts are less attached to the labor force, while men are somewhat more attached; the timing of these effects appears to be consistent with a causal role for marriage. Both women and men are also more likely to take their own lives as adults if they are exposed to unilateral divorce as youths. Thus, exposure to easier divorce regulation as a youth appears to worsen adult outcomes along a number of dimensions, although the ultimate implications depend on the long-run impacts of earlier family formation among this cohort.

This article proceeds as follows. In Section I, I present background on the potential links between unilateral divorce laws and child outcomes. In Section II, I discuss my data and empirical strategy. Section III presents the results on the how unilateral divorce affects marital status. Section IV then examines the impact of exposure to unilateral divorce as a youth (and currently) on adult outcomes, and Section V concludes.

I. Background

As noted in the introduction, there are three key links between legislation that makes divorce easier and the outcomes of children. In this section, I review each link in turn. Before doing so, I provide some background on the unilateral divorce regulations that are the subject of this study.

A. Legislative Background

As nicely reviewed by Weitzman (1985), traditional state regulation in the United States provided for divorce only for such grounds as infidelity and physical abuse. Moreover, such divorce had to be mutually agreed upon by both partners. This law was widely viewed as inadequate, largely because of the enormous financial and emotional transaction costs that the establishment of fault placed on the divorce process. Indeed, marriages that were viewed by both parties as "broken" for mundane reasons could not be dissolved without more elaborate justification. Further, fault was viewed as a tool that was often used by one spouse (typically the wife) to "extort" excessive settlements from the other spouse. As Kay (1974, p. 50, quoted in Weitzman 1985), wrote: "It was unanimously agreed that elimination of the present grounds . . . would conform the law to prevailing reality, eliminate the existing evils of dissimulation, hypocrisy, and outright perjury, and end the use of conduct not formally alleged as a weapon in obtaining extortionate and frequently inequitable and unworkable concessions from the defending spouse."

These concerns led to a movement for reform of U.S. divorce laws. However, according to Weitzman, the reformers did not appear to recognize that they might be swinging the pendulum too far in the other

Table 1
Divorce Regulations across the States

State	No-Fault Date	Unilateral Date	State	No Fault Date	Unilateral Date
Alabama	1971	1971	Montana	1973	1973
Alaska	1935	1935	Nebraska	1972	1972
Arkansas	1937		Nevada	1931	1967
Arizona	1931	1973	New Hampshire	1971	1971
California	1970	1970	New Jersey	1971	
Colorado	1972	1972	New Mexico	1933	1933
Connecticut	1973	1973	New York	1967	
District of Columbia	1966		North Carolina	1910	
Delaware	1957	1968	North Dakota	1971	1971
Florida	1971	1971	Ohio	1974	
Georgia	1973	1973	Oklahoma	1953	1953
Hawaii	1965	1972	Oregon	1971	1971
Idaho	1945	1971	Pennsylvania	1980	
Illinois	1984		Rhode Island	1910	1975
Indiana	1973	1973	South Carolina	1969	
Iowa	1970	1970	South Dakota	1985	1985
Kansas	1969	1969	Tennessee	1963	
Kentucky	1962	1972	Texas	pre-1910	1970
Louisiana	1916		Utah	1943	1987
Maine	1973	1973	Vermont	1969	
Maryland	1969		Virginia	1960	
Massachusetts	1975	1975	Washington	1921	1973
Michigan	1972	1972	West Virginia	1969	
Minnesota	1933	1974	Wisconsin	pre-1910	1978
Mississippi	1978		Wyoming	1977	1977
Missouri	1974				

direction by removing the powerful property rights that mutual consent fault divorce gave to women. Indeed, most expert accounts of this period viewed this reform as an attempt to remove the legal inefficiencies of the divorce process, not as a tool of social policy.

The first step in these reforms was moving to no-fault divorce, which was in place before 1950 in a number of states, while maintaining the mutual consent feature. Unilateral divorce, which allowed divorce with the consent of just one rather than both spouses, was rare before the late 1960s, but it was in place in most states by the mid-1970s. My article focuses on unilateral divorce, following the economics literature growing out of Peters (1986), which is discussed in more detail below. At the end of the article, I also briefly discuss (and dismiss) any distinct impact of no-fault divorce regulations per se.

I have documented the availability of unilateral divorce in each state from 1910 to the present, updating the legislative details in Friedberg (1998). The results are presented in table 1.[1] States could pass either un-

[1] There is some disagreement in both the economics and legal literatures on the appropriate coding of unilateral divorce. Table 1 is based on a careful state-by-state review of the actual divorce laws to ensure a consistent definition. An ap-

restricted unilateral divorce or unilateral divorce with the requirement that spouses live separated for some period of time (typically 1–5 years). I focus on unilateral divorce laws that do not include separation requirements; Friedberg finds that the impacts of laws with separation requirements on divorce flows is positive but much weaker than for unrestricted unilateral divorce.

B. Does Unilateral Divorce Affect Divorce Decisions?

The first supposition behind arguments that unilateral divorce is bad for children is that unilateral divorce leads to higher levels of divorce. As noted above, unilateral (and no-fault) divorce is perceived to be a cause of the dramatic rise in divorce rates in the United States. In addition to the obvious time series parallels, the theory behind such arguments is perceived to be quite straightforward: if there is one partner who wants to terminate the marriage, but the other does not, then a unilateral divorce will cause the marriage to end.

In a well-known article, Peters (1986) pointed out the flaw in this theory from the perspective of Coasian bargaining. If a marriage is a contract between two partners and one partner wants to end that contract, he or she can just pay their partner for that privilege. Thus, moving from multilateral to unilateral divorce does not change the fundamental likelihood of divorce; it simply changes the amount of payment that must be made from the partner who wants to leave to the partner who wants to stay. That is, under the typical presumption from the 1970s that men were the ones that wanted to terminate their marriage contracts, unilateral divorce would not lead to rising divorce; it would simply lead to lower alimony and child-support payments to the wives left behind.

Peters supported her theoretical argument with an empirical analysis of the impact of unilateral divorce regimes on divorce rates. She used a cross section of data on women to examine whether women were more likely to be divorced in states with unilateral divorce regimes, and she found no significant correspondence between the two.

This striking conclusion generated a number of follow-up analyses, yielding somewhat mixed results. Allen (1992) claimed that, for alternative specifications of the divorce variable and the model, there were impacts of unilateral divorce regimes on divorce, but Peters (1992) disputed his

pendix that gives the legislative cites and language for each state's law and that compares this definition to that used by other authors is available on request. There are ambiguities in coding for two small states (Delaware and Montana), but the results are not sensitive to alternative coding of these states. I only coded laws back to 1910, which is the period relevant for my empirical work, and so in two cases I do not report exact dates but rather that the law was in place in 1910.

law classification and his omission of important regional controls. Friedberg (1998) carefully revisited this question using panel data on divorce rates by state and year and found that, for the most detailed specification, there was an impact of unilateral divorce on divorce rates; similar analysis is found in Reilly and Evenhouse (1997). Wolfers (2000) shows that much of Friedberg's effect on flows of divorces arose from a large increase in divorces soon after the passage of unilateral divorce laws and that the long-run effect on the divorce flow is quite small. Further, Gray (1998), using the 1970 and 1980 censuses, does not find an impact of unilateral divorce on divorce rates.

C. Is Divorce Bad for Children?

The second supposition that drives criticisms of easier divorce regulations is that divorce is actually bad for children. This supposition is supported by an enormous literature, which can be only briefly summarized here. After reviewing 92 studies, Amato and Keith (1991) report that children of divorce have more difficulty than children in intact families adjusting both socially and psychologically. Surveys show that children of divorce are more likely to exhibit behavior that is antisocial, impulsive, or acting out. They are more likely to become delinquents (Matsueda and Heimer 1987; Zill, Morrison, and Coiro 1993), and they are likely to perform worse academically; Guidubaldi, Perry, and Cleminshaw (1984) find that first-, third-, and fifth-graders from divorced families as (compared with children of intact families) scored lower on I.Q., reading, spelling, and math tests. They are also more likely to suffer psychological symptoms such as dependency, low self-esteem, anxiety, and depression. Children whose parents have divorced score above clinical cutoffs on psychological tests of behavior problems twice as often (20% vs. 10%) in comparison to children from intact households. (See, e.g., Achenbach and Edelbrock 1983; Isaacs 1986; and Hetherington and Clingempeel 1989.)

The research on adolescents from divorced families also documents negative consequences. Adolescents with divorced parents are two to three times more likely to drop out of school, become pregnant, or engage in antisocial and delinquent behavior, and they score above clinical cutoffs on standardized tests of behavior (Achenbach and Edelbrock 1983). They begin to date and have sex at a younger age (Flewelling and Bauman 1990). Other researchers find that these youngsters are more aggressive, noncompliant, sexually active, and likely to use and abuse drugs and alcohol than adolescents from intact households (Dornbusch et al. 1985; Baumrin 1989; Doherty and Needle 1991). Adolescents whose parents have divorced are more likely to have a low academic performance and to drop out of school, even after one controls for socioeconomic status (Guidubaldi et al. 1984; Krein and Beller 1988).

A British longitudinal study of children shows that, by age 23, children with divorced parents are more likely to leave home because of friction (Cherlin, Kiernan, and Chase-Lansdale 1995). As compared with adults whose parents are not divorced, adult children of divorced parents are less likely to attend or complete college and they are more likely to be unemployed and on welfare. They are also likely to possess fewer resources (Keith and Finley 1988; McLeod 1991; Aquilino 1994; Cooney 1994; McLanahan and Sandefur 1994). Children with divorced parents tend to marry and cohabit at an earlier age, and they are more likely to terminate those marriages through separation or divorce (McLanahan and Bumpass 1988; Amato and Keith 1991; Kiernan and Hobcraft 1997; Feng et al. 1999; Kiernan and Cherlin 1999), although Wolfinger (1999) claims that transmission of divorce across generations is waning.

Thus, the negative implications of divorce for children are broadly supported by a large previous literature (and the even larger volume of research not summarized above). But a central limitation of these studies is that divorce is not an exogenous event with respect to other determinants of child outcomes. Another large literature on the determinants of divorce finds that divorce is strongly correlated with socioeconomic characteristics that also determine child outcomes, such as income and family size. For example, divorce rates are higher when men have experienced serious unemployment within the past 5 years (Ross and Sawhill 1975), and states with higher male earnings have lower divorce rates (Ferber and Sander 1989). Moreover, in theory, the implications of divorce for child well-being are ambiguous. While depriving the family of one potential earner and caregiver can clearly have negative implications, breaking up emotionally or physically harmful marriages can have benefits for children.

A number of the studies above attempt to control for socioeconomic characteristics in assessing the impact of divorce (e.g., McLanahan and Sandefur 1994). However, they do so armed with only a limited set of family background characteristics that might not fully capture underlying differences between families that do and do not divorce. Even conditional on background characteristics such as socioeconomic status, families that choose to divorce are different in unmeasured ways, and these differences can have important implications for children. For example, it may be the families with the weakest tastes for marriage that divorce, and this could lead to more unstable marriages among their children, not because of parental divorce per se, but rather because of inherited weak tastes for marriage.

What is required to appropriately identify the impacts of divorce is an exogenous instrument that causes some families to divorce and others not, based on a factor independent of the determinants of their children's outcomes. No previous study has been able to uncover such an instrument, making it somewhat hard to interpret causally this large literature. Two very recent studies have focused on a different shock to family resources, death of the household head (Corak 2001; Lang and Zagorsky 2001). They both find very modest impacts of parental death on child outcomes. However, death is a substantively different shock than divorce. Moreover, those families in which the head dies are also likely to be quite different from families in which the head does not die, so this does not necessarily solve the problem of omitted variables that plague observational studies of divorce outcomes.[2]

Moreover, the previous literature has focused on the impact of the average divorce, not of the marginal divorce that is induced by a change in the regulatory regime. Even relative to the effects of divorce on average, the divorces induced by a shift to unilateral divorce regulation may have larger or smaller negative implications for children. For example, the average divorce may be taking place for reasons of fault, such as spousal abuse or infidelity, while the marginal divorce that arises from a law change is due to spousal incompatibility. If marriages that end due to abuse or infidelity have worse or better implications for children than do marriages that end due to incompatibility, then unilateral law-induced divorces will have better or worse implications than the average divorce.

D. Are There Other (Potentially Offsetting) Impacts of Unilateral Divorce?

The third supposition behind these arguments is that there are not offsetting impacts of unilateral divorce on child well-being. In fact, there are at least three additional effects that unilateral divorce regulations may have on child well-being beyond the direct effects on the propensity to divorce; these effects may either offset or augment the direct divorce effects.

First, and most obvious, unilateral divorce can lower the incidence of separation, as families substitute official for de facto divorce; that is, parents who are living apart may now formalize a divorce. If it is having two parents in residence that is important for child development, then such a shift may be of little consequence for well-being.

Moreover, unilateral divorce may increase the incidence of marriage,

[2] The Corak paper also compares the characteristics of children from divorce before and after a policy change that made divorce easier. But this is a substantively different question than the one that I am asking. By focusing on the children of divorce, Corak examines a selected sample. For example, suppose that divorce has negative effects on average but that the marginal divorces that occur when the law changes are among higher-quality families; this would bias toward no effect among those in families getting divorced after vs. before the law. It is for this reason that I focus on all children in my analysis, not just the selected sample of children of divorce.

by reducing the barriers to exiting that marriage. That is, individuals who are reticent to enter marriage in a regime where unilaterally terminating that marriage is not possible may be more willing when they have a source of exit. Unilateral divorce may thereby lead to more "marital churning," with both more marriages and more divorces and little impact on the net stock of married couples (or of children living in two-parent households). If what matters for child well-being is being in a two-parent household, then even if divorce is rising in unilateral divorce regimes, child outcomes may be unaffected; of course, if marital instability per se is detrimental to child development, then child outcomes could worsen even if total marriage rates are constant. This point has not been considered by previous empirical studies, which have focused on divorce and not marriage as the outcome of interest, but it is discussed theoretically in Bougheas and Georgellis (1999).[3]

Finally, even if families see no change in marital status as a result of unilateral divorce, making divorce easier can change the nature of the bargaining relationships between husband and wife. If these relative positions of power have implications for child well-being, then this is an additional channel through which unilateral divorce can affect children. For example, there is a large literature that suggests that resources in the hands of women are more beneficial to children than are resources in the hands of men (e.g., Strauss and Thomas 1995; Lundberg, Pollak, and Wales 1997). Thus, if unilateral divorce weakens the within-household bargaining position of the wife, this may have negative implications for children who, under multilateral divorce, might have benefited from the relative power of their mothers. Of course, this effect could operate in the opposite manner if the wife's bargaining power is strengthened by unilateral divorce. There is little evidence to date as to whose bargaining power is increased on the margin by the availability of easier divorce regulations, although recent work does suggest that, in the unilateral world, most divorces are initiated by women.

E. Can the Effects of Divorce Be Estimated?

The goal of this article is to estimate the impact of a unilateral divorce regime on the marital status of parents and later life outcomes of children. I will not, however, be able to separate the two channels through which these effects may operate: the direct effects through divorce versus the indirect effects through family bargaining and the changing nature of marriage. This is an important point because it highlights that unilateral divorce laws are not useful instruments for evaluating the effects of di-

[3] The sign of this effect is not entirely obvious, however; for persons who desire to marry only if they can be assured that the marriage will not terminate, unilateral divorce may cause them to choose to forgo marriage altogether.

vorce. That is, moving to a unilateral divorce regime has impacts that go beyond any simple effect on the divorce rate. Effectively, in this analysis, I have one instrument, unilateral divorce laws, and two channels through which it might affect outcomes, through increasing divorce rates and through affecting family bargaining/formation. Thus, if I tried to use unilateral divorce laws as an instrument for divorce rates, I would misstate the impact on divorce, since my instrument would be capturing both channels of effects. So the results from this exercise should be interpreted as the reduced-form effects of making divorce easier on outcomes and not as the structural effects of divorce on outcomes.[4]

II. Data

The primary data for this analysis come from the 1960, 1970, 1980, and 1990 Public Use Micro Samples (PUMS) files from the U.S. Census. These data provide very large samples, so that information can readily be gleaned on the response to state divorce laws. Most important, the data also report both state of current residence and state of birth, and they cover the periods both during the unilateral reforms and many years thereafter, so that the effect of both current and youth exposure to unilateral divorce can be assessed. Due to the large samples and to the fact that the relevant legislative variables vary only at the state/year/age level, the data are collapsed into state/year/age cells for the analysis.

These data are parsed into three data files for the analysis. The first has information by state of residence and year for children 0–18 years old, so that data are cells by state, year, and child's age; all regressions are weighted by cell size to replicate the underlying microdata. This file contains information on the marital status of the parent with whom the child resides. The second data set is the parallel data set by state/year/age for adults of child-rearing age (25–50 years old), so as to examine the impact of unilateral divorce laws in one's state of residence on own divorce probabilities.

[4] A new paper by Johnson and Mazingo (2000), written at the same time as (and independently of) this article, also explores the implications of unilateral divorce for child outcomes as adults. These researchers follow a different strategy than the one employed here, using cross-sectional data from the 1990 census to compare the effects of unilateral divorce on those exposed to the law for different lengths of time as a child. However, given that childhood lasts for a fixed number of years, this approach cannot differentiate the impacts of the amount of time exposed to these laws from the age first exposed, making interpretation of these results somewhat difficult. Moreover, there may be important impacts of any exposure that are distinct from the amount of time exposed/age first exposed. The full impacts of youth exposure to unilateral divorce on outcomes are identified only by comparing those exposed at all to those not exposed, as I am able to do with my sample of multiple censuses. Nevertheless, I do also examine impacts by years of exposure as well below.

Table 2
Sample Means

	Female Adult	Male Adult	Child
Mother divorced			.0664
Mother separated			.0349
Mother never married			.0370
Father divorced			.0099
Father separated			.0033
Father never married			.0036
No. of observations			3,876
Married	.717	.726	
Divorced	.110	.082	
Separated	.034	.023	
Never married	.120	.166	
No. of children	2.024	1.169	
Years of education	11.683	11.933	
High school dropout	.189	.195	
High school graduate	.365	.308	
Some college	.216	.216	
College graduate	.231	.281	
Income per capita ($)	13,513.41	15,159.06	
Below poverty	.118	.086	
Work last year	.712	.946	
Weeks worked	29.758	45.040	
Earnings ($)	10,682.1	26,222.66	
No. of observations	159,884	159,487	
Suicides per 100,000	6.3	22.1	
No. of observations for suicide	23,868	23,868	

The third file is a data set for adults age 25–50 that is organized by age, state of birth, state of residence, and sex. This finer level of detail is necessary for us to examine jointly the impact of unilateral divorce regimes with which the individual grew up and unilateral divorce regimes within which he or she now resides. The data are also divided by sex because many of the outcomes I examine (e.g., labor force participation) differ significantly across the sexes. These data have information for each of these cells on marital status, family size, family income, individual education, individual work status, and individual earnings. Once again, these are all averages for the year/age/state of birth/state of residence/sex cell, and the regression is run at that level, weighted by cell size.

These census data are matched to information on the presence of unilateral divorce regimes across states and over time. The means of the first and third data sets are presented in table 2; the means in the second and third data sets for adults are identical, since the third is just a more finely parsed version of the second. Among children, 6.6% are living with a divorced mother, and roughly 1% are living with a divorced father. Among all adults of child-bearing age, 11% of females and 8.2% of males are divorced.

One weakness of the census analysis, however, is the potentially ambiguous nature of many of the outcome variables; if exposure to unilateral divorce leads women to be more likely to be married and home with children and less likely to work, is this good or bad? I therefore also assess the impact of exposure to unilateral regulations on a less ambiguous outcome measure: rates of suicide.[5]

Micro-data on all deaths in the United States are available from the Vital Statistics Mortality data. These data have recorded state of birth since 1978, as well as information on cause of death, age, and sex. Therefore we can use these data to create annual counts of deaths by age, sex, and state of birth cells from 1978 to 1996. Since we do not have a good population denominator by age, sex, and state of birth for each year, and since the counts of these events are quite small (e.g., there are only on average about two suicides per year in each female cell), I use a negative binomial count model, as in Dee (1999) or Dee and Evans (2001). I also report results from a population shares model, where I normalize by the state/age/year/state of residence population; the general pattern of results is consistent regardless of the measure used. Since these data are not first collected until 1978, I consider in these suicide models the impact of only exposure as a youth and not of current exposure to divorce regulations. Means of the suicide rates are shown in table 2 as well: among females, there are 6.26 suicides per 100,000 25–49-year-olds, and among males the rate is 22.1 per 100,000.

III. Do Unilateral Divorce Laws Affect Marital Status?

In this section, I address the first of the predicate questions raised above: does the availability of unilateral divorce actually affect marital status? I examine both the impact on the likelihood that adults are divorced and the impact on other marital states that may be affected by this shift in legal regimes. To assess the impact of unilateral divorce regulations on marital status, I run regressions of the form:

$$\begin{aligned} \text{DIVORCE}_{ajt} &= \alpha + \beta_1 \text{UNILAT}_{jt\text{-}1} + \beta_2 \text{RACE}_{ajt} \\ &\quad + \beta_3 \eta_a + \beta_4 \delta_j + \beta_5 \tau_{t+} \beta_6 \eta_a^{*} \tau_t + \epsilon, \end{aligned} \tag{1}$$

where a indexes ages, j indexes states, and t indexes years; DIVORCE is the cell mean divorce rate (or some other marital status indicator); UNILAT is a dummy for the presence of a unilateral reform law in the year before the census year (since the census is carried out in the spring of the year and many of the questions that I will use refer to the previous year); RACE are dummies for % black and % white, respectively, in the cell; η_a, δ_j, and τ_t are full sets of dummies for age group, state, and year,

[5] In earlier work, I examined impacts on arrest rates as well; the signs were all consistent with the general pattern of results here, but the estimates are consistently insignificant when the standard errors are appropriately corrected.

respectively; and $\eta_a * \tau_t$ is a full set of age*year interactions to allow for differential time patterns by age. This model controls for fixed factors that vary by age, location, or year, and is identified by the passage of unilateral divorce laws over time.

One concern with this approach, however, is that there may be trends in marital status that are correlated with the passage of unilateral divorce laws. That is, unilateral divorce may pass where divorce is rising, rather than the opposite causal interpretation. The evidence in Wolfers (2000) suggests that there are not large preexisting trends in divorce rates in the unilateral divorce states, once data back to 1960 are used. Nevertheless, I will attempt to address this concern by including in the model, along with state fixed effects, state-specific trends. This is not a perfect solution given these census data, since there are only four underlying time-series observations, so fixed trend models are fairly demanding. Moreover, if the endogeneity is in the short run, then trends that measure decade time spans (as with the census) will miss them. But this is the best feasible approach to the problem and, if the results are robust to the inclusion of trends, it offers some comfort that they are not driven by endogeneity of the laws.

Another econometric concern is that the standard errors in this regression may be seriously misstated in the presence of serial correlation of outcomes within a state over time. Therefore, following the suggestion of Bertrand, Duflo, and Mullainathan (2004), I correct the standard errors by clustering on state of residence. This allows for an arbitrary correlation within state cells over time to capture any autocorrelation in outcomes.

The results of running regressions such as this for various measures of marital status are shown in table 3. The first set of columns is for women, and the second set for men. Within each set of columns, I show results with and without state-specific trend controls. Within each cell, I present the coefficient, the standard error (in parentheses), and proportional impact (relative to means in table 2). The coefficients are all multiplied by 100 for ease of interpretability. Each cell in the table is from a separate regression.

I find that there is a very sizable and significant impact of unilateral divorce regulations on the likelihood of being divorced. For women, unilateral divorce being in place raises the odds of divorce by 0.0127 percentage points, or 11.6%. For men, the increase is 0.0095 percentage points, or 11.6%. The results are even stronger when state-specific trends are included, with the coefficient for females rising to 0.014 percentage points (12.7%) and for men rising to 0.0096 percentage points (11.7%).

On the other hand, there is no robust evidence on the odds of being either separated or never married. For women, there is some suggestion of a reduction in the odds of being never married, but it is highly insig-

Table 3
The Impact of Unilateral Divorce on Marital Status

	Adult Female		Adult Male	
Adult is . . .	No Trend	Trend	No Trend	Trend
Divorced	1.277 (.395) [.116]	1.396 (.675) [.127]	.948 (.377) [.116]	.961 (.661) [.117]
Separated	.216 (.196) [.063]	−.142 (.325) [.042]	−.057 (.120) [−.025]	−.097 (.279) [−.042]
Never married	−.613 (.703) [−.051]	−.255 (.474) [−.021]	−.37 (.840) [−.022]	−.088 (.608) [−.005]
No. of observations	5,304	5,304	5,304	5,304
	Child Is Living with Mother		**Child Is Living with Father**	
Parent that the child lives with is . . .	No Trend	Trend	No Trend	Trend
Divorced	.606 (.244) [.091]	.963 (.409) [.145]	.192 (.070) [.194]	.11 (.104) [.111]
Separated	.003 (.195) [0]	−.251 (.295) [−.072]	−.016 (.036) [−.048]	−.001 (.042) [−.003]
Never married	−1.003 (.340) [−.391]	−.133 (.290) [−.036]	−.088 (.047) [−.244]	−.097 (.069) [−.269]
No. of observations	3,876	3,876	3,876	3,876

NOTE.—Standard errors, adjusted for clustering at the state of residence level, are in parentheses; percentage impacts are in square brackets. All coefficients are multiplied by 100. Each coefficient is from a separate regression that includes the following: race, state of residence dummies, age dummies, year dummies, and age*year dummy interactions. The top of the table uses data on adults, examining own marital status. The second panel uses data on children, examining the marital status of the parent with whom they reside. The first column in each panel excludes state specific linear time trends; the second column includes those trends.

nificant and small when state trends are included; for men, the effect is wrong-signed with trends included.

These results show the effects on parents of child-bearing age. However, it is possible that the impacts of divorce reform may be different for parents who actually have children. So the next panel of table 3 focuses on the marital status of parents of children in the data set. The regression run is the same as above, except that the sample is now children age 0–18 and the dependent variables are whether their mother or father are divorced, separated, or never married.

The results for the parents of children echo those for adults of child-bearing age. Controlling for state-specific trends, there is a 0.0096 percentage point increase in the odds that a child is living with a divorced mother, which is 14.5% of baseline, and a 0.0011 percentage point increase in the odds of living with a divorced father, which is 11.1% of baseline. Overall, the odds of living with a divorced parent rise by 0.0107 percentage points, which is quite similar to the odds for adults of being divorced. These child-based results offer more evidence for increased entry into marriage when divorce is made easier. In the model without trends, there is a very large impact on the odds of living with a never-married mother or father; however, both results are insignificant when trends are included.[6]

Thus, there is clear evidence from the census data that making divorce easier increases the stock of divorced women and men and that as a result children are more likely to be living with a divorced parent. There is mixed evidence for an offsetting impact on other marital decisions. Overall, however, these findings imply that the rise in unilateral divorce regulations can explain only a very small share of the overall rise in divorce shown in figure 1. In 1960, 3.3% of women age 25–49 were divorced; by 1990, this had risen to 13.1%. Given that the percentage of women exposed to unilateral divorce regulation rose by 61% and that exposure to unilateral regulations raises the odds of being divorced by 1.4%, the rise in unilateral regulation can explain less than 10% of the overall rise in the stock of divorced women.

IV. Impact of Unilateral Divorce Laws on Outcomes

I now turn to assessing the impact of being exposed to unilateral divorce regulations as a youth on adult outcomes. To do so, I turn to data that are created by both state of residence and state of birth, and I run expanded regressions of the form:

$$\begin{aligned} \text{OUTCOME}_{ajbt} = {} & \alpha + \beta_1 \text{UNILAT}_{jt-1} + \beta_2 \text{KIDUNI}_{abt} \\ & + \beta_3 \text{RACE}_{ajt} + \beta_4 \eta_a + \beta_5 \sigma_b \\ & + \beta_6 \delta_j + \beta_7 \tau_t + \beta_8 \eta_a^* \tau_t + \epsilon, \end{aligned} \tag{2}$$

where, in addition to the other indices, b indexes state of birth, OUTCOME is one of the measures of outcomes, KIDUNI is a dummy for having a unilateral divorce law in your state of birth before you were age 18, and σ_b is a full set of state-of-birth dummies. Thus, this regression framework allows for both effects of contemporaneous and youth laws on outcomes. Once again, I also explore the sensitivity of results to state-specific trends, where now I include trends for both state of birth and

[6] One problem with these data is that they only measure marital status at a point in time. As a result, adults may have been divorced and remarried, and children may have thus been exposed to some time in a divorced family, but it will not be captured here. I can explore this in the 1970 and 1980 censuses, which, along with current marital status, asked about previous marital status. The basic results for current marital status are similar when restricted to these censuses. There is in fact a negative and insignificant impact on the odds of being married and previously divorced, which actually offsets somewhat the positive impact on being divorced.

state of residence. Also, the standard errors are now corrected for intracell correlation within state of residence*state of birth*year cells.

A. Census Results

The results of running these regressions for females age 25–50 are presented in table 4. The first and third columns show the results of estimating equation (2) without trends, and they present the key coefficients on KIDUNI and UNILAT; the second and fourth columns show the results of estimating equation (2) with trends, and once again they present those same coefficients. So there are two regressions presented per row.

I show the results for several sets of variables, denoted in the various parts of the table. Since there are many outcome variables and their effects are likely to be related, I first review below the full set of findings and then turn to discussing their implications. For regressions where the dependent variable is a cell mean of a discrete variable (e.g., marital status measures), the coefficients on unilateral variables are multiplied by 100; for continuous variables (e.g., number of children, years of education, earnings), the coefficients are directly reported (and not multiplied by 100).

The first group of results shows the impact on marital status and number of children; the coefficients for current divorce law parallel those shown in table 3, and the results are indeed quite similar, suggesting little bias from examining the current laws in a vacuum (ignoring youth laws). However, the coefficients for laws as a youth are strikingly different: I find that unilateral divorce as a youth is associated with no rise in the odds of being divorced but is associated with a much higher likelihood of being married. The coefficient with state-specific trends included indicates that being exposed to unilateral divorce as a youth raises the odds of being married by 0.0066 percentage points, or 0.9% of the sample mean. There is a correspondingly much lower likelihood of being never married. There is also a significant rise in the odds of being separated. These results are all very robust to the inclusion of state-specific time trends.

Corresponding to the increase in the odds of being married, there is a rise in the number of children on average associated with being exposed to unilateral divorce as a youth. This result is somewhat sensitive to the inclusion of trends, with the coefficient doubling to 0.021 more children if exposed to a unilateral divorce regime as a youth and becoming significant.

The next group of the table examines the impact of unilateral divorce on educational attainment. There is in fact a significant decline in years of education attained for those exposed to unilateral divorce as a youth, with exposure associated with 0.065 fewer years of education (0.6% of

Table 4
Unilateral Divorce and Outcomes as Adults, Females

	Unilateral as Youth		Unilateral as Adult	
	No Trend	Trend	No Trend	Trend
Family structure:				
Married	.591	.664	-1.263	-.734
	(.274)	(.361)	(.808)	(1.079)
	[.008]	[.009]	[−.018]	[−.010]
Divorced	.058	.007	.942	1.073
	(.370)	(.350)	(.262)	(.517)
	[.005]	[.001]	[.086]	[.098]
Separated	.347	.326	.119	-.166
	(.110)	(.092)	(.163)	(.334)
	[.102]	[.096]	[.035]	[−.049]
Never married	−1.198	−1.222	.136	−.16
	(.621)	(.672)	(.602)	(.385)
	[−.1]	[−.102]	[.011]	[−.013]
No. of children	.011	.021	−.054	−.017
	(.011)	(.009)	(.029)	(.017)
	[.005]	[.010]	[−.026]	[−.008]
Educational attainment:				
Years of education	−.076	−.065	−.132	−.025
	(.044)	(.033)	(.076)	(.039)
	[−.006]	[−.006]	[−.011]	[−.002]
High school dropout	.837	.622	2.7	.195
	(.726)	(.509)	(.962)	(.516)
	[.044]	[.033]	[.142]	[.010]
High school graduate	1.05	1.16	−4.26	−1.28
	(.806)	(.553)	(2.003)	(.959)
	[.029]	[.032]	[−.117]	[.035]
Some college	−.235	−.284	.932	2.46
	(.415)	(.269)	(.51)	(1.533)
	[−.011]	[−.013]	[.043]	[.114]
College graduate	−1.656	−1.497	.633	−1.378
	(.526)	(.443)	(1.277)	(.764)
	[−.072]	[.065]	[.027]	[−.06]
Living standards:				
Income per capita	−521.35	−431.63	213.45	54.21
	(197.01)	(148.36)	(395.67)	(807.64)
	[−.039]	[−.032]	[.016]	[.004]
Below poverty	.026	.091	.557	.343
	(.265)	(.213)	(1.411)	(1.384)
	[.002]	[.008]	[.047]	[.029]
Labor supply:				
Work last year	−.76	−.478	1.284	2.197
	(.296)	(.222)	(.64)	(.874)
	[−.011]	[−.007]	[.018]	[.031]
Weeks worked	−.099	−.139	.65	.262
	(.093)	(.075)	(.167)	(.166)
	[−.003]	[−.005]	[.022]	[.009]
Earnings	−336.35	−246.28	198.77	91.01
	(145.03)	(108.01)	(387.29)	(595.17)
	[−.031]	[−.023]	[.019]	[.009]

NOTE.—Standard errors, adjusted for clustering at the state of birth level, are in parentheses; percentage impacts are in square brackets. Coefficients on dummy variables are multiplied by 100; coefficients on continuous variables are unchanged. The first and third columns are from one regression, which excludes state-specific time trends; the second and fourth columns are from a second regression, which includes linear time trends for both state of residence and state of birth. Each regression also includes the following: race, state of residence dummies, state of birth dummies, age dummies, year dummies, and age*year dummy interactions.

mean). This impact appears to arise mostly from a significant and sizable increase in the odds of being a high school graduate and a corresponding large decline in the odds of being a college graduate. There is a also a large increase in the odds of being a high school dropout, but it is not significant. There is little net effect on the odds of getting some college education (but not a degree), but this result is hard to interpret since there are both flows into this group (from reductions in college completion) and flows out (from reductions in college attainment).

The next group of table 4 examines the impact on living standards. There is a significant deterioration in average income per capita for those women exposed to unilateral divorce as a youth, with exposure associated with a reduction in income per capita of $431, or 3.2%. However, there is no effect on the percentage of the cell living below the poverty line, suggesting that the reductions in income are concentrated in middle- and higher-income families.[7]

The next group of table 4 shows the impact of unilateral divorce on labor supply. Being exposed to unilateral divorce as a youth significantly lowers labor supply. The odds of being employed fall by 0.48%, which is 0.7% of the sample mean; weeks worked fall by 0.14, which is 0.5% of the sample mean; and earnings fall by $246, which is 2.3% of the sample mean. The much larger impact on earnings than on labor supplied implies that wage rates are falling as well; direct examination of hourly wages yields a negative but insignificant coefficient.

Table 5 shows corresponding results for males. The basic pattern of results for the first three groups is quite similar: increased marriage probabilities, reduced educational attainment, and reduced living standards. However, the effects on labor supply are quite different: being exposed to unilateral divorce as a youth appears to increase labor supply for men, albeit not significantly.

The second set of columns in both tables 4 and 5 shows the results of current unilateral divorce regimes. In both cases, except for marital status, there are relatively few significant coefficients. This reflects the much more limited variation in the current unilateral regime than in the state-of-birth unilateral regime.

B. Interpretation

This panoply of results paints an interesting picture of the impact of being exposed to unilateral divorce laws as a youth. Exposure to unilateral divorce leads, for both men and women, to more marriage (but more

[7] While easy to interpret, income per capita measures ignore economies of scale from changes in family size. However, the results are very similar if I use instead a measure of the ratio of family income to the poverty line that uses census equivalence scales to adjust.

Table 5
Unilateral Divorce and Outcomes as Adults, Males

	Unilateral as Youth		Unilateral as Adult	
	No Trend	Trend	No Trend	Trend
Family structure:				
Married	.555 (.417) [.008]	.625 (.531) [.009]	−.926 (.945) [−.013]	−.693 (1.292) [−.010]
Divorced	.027 (.349) [.003]	−.107 (.328) [−.013]	.723 (.276) [.088]	.689 (.481) [.084]
Separated	.248 (.078) [.108]	.261 (.067) [.113]	−.116 (.121) [−.050]	−.065 (.271) [−.028]
Never married	−.897 (.773) [−.054]	−.839 (.856) [−.051]	.315 (.802) [.019]	.121 (.672) [.007]
No. of children	.037 (.015) [.032]	.046 (.017) [.040]	−.074 (.024) [−.063]	−.054 (.022) [−.046]
Educational attainment:				
Years of education	−.079 (.057) [−.007]	−.072 (.045) [−.006]	−.076 (.083) [−.006]	.041 (.059) [.003]
High school dropout	1.05 (.625) [.054]	.889 (.433) [.046]	1.584 (.834) [.081]	−.272 (.479) [−.014]
High school graduate	.633 (.558) [.021]	.781 (.377) [.025]	−3.627 (1.586) [−.118]	−1.329 (.88) [−.043]
Some college	−.364 (.228) [−.017]	−.335 (.168) [−.016]	.575 (.422) [.027]	2.484 (1.183 [.115]
College graduate	−1.32 (.465) [−.047]	−1.335 (.546) [−.048]	1.468 (1.128) [.052]	−.883 (.596) [−.031]
Living standards:				
Income per capita	−431.96 (206.8) [−.028]	−266.72 (155.87) [−.018]	237.45 (450.46) [.016]	255.44 (787.2) [.017]
Below poverty	−.08 (.202) [−.009]	.057 (.148) [.007]	.398 (1.33) [.046]	.502 (1.11) [.058]
Labor supply:				
Work last year	.083 (.142) [.001]	.161 (.151) [.019]	.301 (.203) [.035]	.259 (.424) [.030]
Weeks worked	−.093 (.051) [−.002]	−.009 (.006) [−.000]	−.166 (.111) [−.004]	.104 (.219) [.002]
Earnings	93.65 (268.13) [.004]	418.8 (361.27) [.016]	−54.36 (414.36) [−.002]	309.73 (1475.12) [.012]

NOTE.—Standard errors, adjusted for clustering at the state of birth level, are in parentheses; percentage impacts are in square brackets. Coefficients on dummy variables are multiplied by 100; coefficients on continuous variables are unchanged. The first and third columns are from one regression, which excludes state-specific time trends; the second and fourth columns are from a second regression, which includes linear time trends for both state of residence and state of birth. Each regression also includes the following: race, state of residence dummies, state of birth dummies, age dummies, year dummies, and age*year dummy interactions.

separation as well), less education, and lower family incomes. For women, being exposed to unilateral divorce leads to lower labor force attachment and earnings; for men, labor force attachment and earnings actually rise, albeit insignificantly.

Distinguishing causal pathways for these effects, however, is somewhat difficult, as educational attainment, marital status, and labor force attachment are all jointly determined. Since, in the raw data, there is a strong positive association between education and marriage, it seems likely that the increase in marriage and reduction in education are both direct effects of unilateral divorce, rather than one being a secondary effect of the other. The increase in marriage is not inconsistent with the previous literature, which found earlier marital formation for children of divorce; in these cross-sections of a given set of ages, a higher likelihood of being married on average could result from either increased odds of marriage at every age or from a shift forward in the timing of marriage. I return to this point below.

The reduction in education could arise from at least two channels. The first is liquidity constraints: to the extent that children of divorce have fewer resources, they may be unable to afford higher education. The second is stress in childhood that leads to worse performance in school as a youth, with resulting ramifications for ultimate educational attainment. The fact that, for women at least, the impact on high school graduation is much larger than on dropping out from high school would be consistent with liquidity constraints operating particularly on the college attendance margin; this is not true for men, however, where there are equal effects on high school dropping out and on high school graduation.[8]

The reduction in labor force attachment and earnings for women could arise directly through unilateral divorce impacts or indirectly through either of the marriage or education channels. The direction of the impacts is inconsistent with a causal pathway through education, as both males and females are suffering similar reductions in educational attainment, but female labor supply falls while male labor supply rises. On the other

[8] A liquidity constraints explanation for reduced educational attainment would suggest that the effect would be largest where the costs of higher education are the highest. To assess whether this is true, I obtained data on public university tuition levels across the states and estimated models where I allowed the education effects to vary by underlying public tuition levels. (I am grateful to David Card for providing these data.) The data are far from ideal for my purposes; they only go back to 1972, whereas most of my sample graduated from high school well before then. I therefore made a ranking of the states in terms of their tuition levels and used the average ranking over the 1972–83 period as my regressor for all cohorts, assuming that states are either a consistently high- or low-tuition state. Doing so, I found no evidence of stronger reductions in college attainment from unilateral divorce where tuition ranking was higher. However, this test is not a strong one due to the limitations of the tuition data for my purposes.

hand, the direction is consistent with the reduction in female labor supply and increase in male labor supply that would typically arise from marriage. However, even here the magnitudes are too large to be explained by typical estimates of the effect of marriage on labor supply. For example, cross-sectional estimates in our census data show that being married lowers earnings by 70% for females; at this magnitude, a 0.66% rise in the odds of marriage cannot explain a 2.3% decline in earnings. On the other hand, these cross-sectional estimates suffer from a number of selection biases, which make it hard to use them to infer the indirect effect of unilateral divorce through marriage on labor supply.

The central interpretive issue with these results is the mechanisms through which unilateral divorce regulation leads to later outcomes: is it solely through increased divorce, or through the other mechanisms discussed above? As noted earlier, it is impossible to answer this question precisely, given that I only have one instrument and two channels of effects. But back-of-the-envelope calculations from the estimates in tables 3 and 4 suggest that the effects of divorce must be enormous if unilateral divorce has its impacts through the divorce channel only. For example, I find in table 3 that unilateral divorce regulations increase the odds for a child living with a divorced parent by 0.0107 percentage points. Putting this together with the findings in table 4 and assuming that the impacts of unilateral divorce arise through increased divorce only would imply that coming from a divorced family raises for females the odds of being married by 62 percentage points (84%), lowers education by 6.1 years (56%), lowers the odds of graduating from high school by 308%, lowers average incomes per capita by $40,340 (300%), lowers the odds of work by 65%, and lowers earnings by $23,016 (215%).[9]

Some of these effects are in the range of estimates from the observational literature on the impacts of divorce on child outcomes. Furstenberg and Teiler (1994), for example, find that female children of divorce are 192% more likely to have dropped out from high school and 205% less likely to be working. However, the natural presumption is that these previous estimates, which take divorce as exogenous, should be overstating the true impact of divorce, since divorce is likely to be correlated with unobserved negative determinants of outcomes. So the fact that the estimated impacts here, if they occur solely through divorce, are even larger than those from this previous literature clearly indicates that these effects are not driven solely by parental divorce.

[9] Of course, to the extent that some children exposed to divorce are now in remarried households, this "first stage" coefficient will understate the net effect of divorce exposure, so that these implied instrumental variables (IV) coefficients are too large. However, as noted earlier, data from 1970 and 1980 do not suggest a rise in the odds of living with a remarried mother when exposed to unilateral divorce, so this does not seem an empirically important consideration.

Table 6
Unilateral Exposure as Youth and Adult Suicide

	Females		Males	
	No Trend	Trend	No Trend	Trend
Number of suicides, negative binomial	13.054	11.189	10.988	9.877
	(5.709)	(5.324)	(4.946)	(4.58)
	[.060]	[.051]	[.015]	[.013]
Suicide rate (per 10,000), OLS	.678	.643	1.144	1.189
	(.371)	(.34)	(.75)	(.666)
	[.108]	[.102]	[.052]	[.054]
No. of observations	23,868	23,868	23,868	23,868

NOTE.—Standard errors, adjusted for clustering at the state of birth level, are in parentheses; percentage impacts are in square brackets. The first and second columns show results for females, with and without separate trend terms for each state of birth. The first row shows results from a negative binomial model estimated on counts of suicides; the second row shows results from the suicide rate model estimated on suicides as a share of (state of residence) population. Each regression also includes the following: state of birth dummies, age dummies, year dummies, and age*year dummy interactions.

Rather, the estimated results likely reflect the effects of the laws on two other groups. The first, as noted above, is couples who do not divorce but in whom relative bargaining positions change when these laws are passed. Since this group is much larger than the set of couples who are induced to divorce by the laws, even relatively small effects in married couples could have nontrivial aggregate impacts such as those shown in tables 4 and 5. The second is those parents who would have been divorced even in the absence of these laws but whose financial circumstances are affected by the laws. For example, if the laws lowered the resources available to women of divorce and women are more generous to their children then are men, then children's outcomes may be worsening even among families whose divorce decisions are not affected by the laws. Unfortunately, I cannot test directly for effects through these other channels, but the magnitudes here strongly suggest that there is some reaction along these dimensions.

C. Effects on Suicide

As discussed, it is desirable to consider as well outcomes that are not available in census data and that provide a potentially more unambiguous picture of the welfare consequences of easier divorce. So I now turn to examining the impact of unilateral divorce on suicide rates.

Table 6 presents the suicide rate results. Here we only have data for 1978 forward, and so we do not include variables for current unilateral divorce exposure (since there is so little variation in our sample period). The first row of the table shows the results first for females and then for males, first excluding and then including state trends, using the negative binomial model. As a specification check on this approach, the second row instead shows effects on suicide rates, where the denominator is state of residence/year/sex/age population counts. The negative binomial model is not weighted; the population rate model is weighted by cell population count.

As table 6 shows, we find a significant and sizable impact on suicide rates for both males and females that is robust to both state trends and to estimation methodology. The negative binomial model shows that, in the model with state trends, there is a rise in the number of females' suicides of 0.11, or roughly 5%; for males, the estimate is 0.099, or only 1.3%. The effects are much larger from the models that normalize by state-of-residence population, with an increase of 0.64 per 100,000 for females, or about 10%, and an increase of 1.189 per 100,000 for males, or roughly 5%. These impacts are quite large given the small underlying impact on divorce rates, but they are relatively small in terms of implied additional suicide per additional divorce: for every 1,000 additional children who are exposed to divorce as a result of unilateral divorce regulations, there are approximately 0.3 additional suicides per year when these children are adults.[10] Given the potential for family bargaining impacts as well as impacts directly through divorce, these results do not appear on their face implausible.

D. Exposure to Laws

One interesting question is whether the amount of exposure to unilateral divorce laws strengthens the effects that have been shown thus far. To the extent that additional years of exposure to unilateral divorce regimes raises the odds that parents divorce, it could lead to stronger impacts on the outcome of children who grow up in unilateral divorce regimes. On the other hand, if there is a pent-up stock of divorce demand that is satisfied shortly after the unilateral divorce law is passed and then divorces decline again, there may not be a strong relationship with exposure. Wolfers (2000) suggests that such a "blip" may occur over the first 8 years that a unilateral divorce law is in place.

Examining impacts by amount of exposure also has the potential to help determine the causal pathways of the effects that were shown above, in two senses. First, if the time pattern of impacts of later outcomes matches that of exposure to divorce, then it lends some credence to the notion that the effects are occurring through the divorce channel and not other (e.g., household bargaining) channels. Second, if the time pattern of some effects (e.g., marriage) matches that of others (e.g., labor supply), it provides some more evidence that can be helpful in interpreting causal pathways of effects of unilateral divorce.

To examine exposure effects, I calculate the number of years that each

[10] This calculation uses the more reliable negative binomial coefficient, but it applies population counts by sex*age*state of residence cells.

Table 7
Amount of Exposure to Unilateral Divorce Regulation and Marital Status

	Adult Respondent Is Female			Adult Respondent Is Male		
	1–4 Years	5–8 Years	9+ Years	1–4 Years	5–8 Years	9+ Years
Adult respondent is:						
Divorced	.568	1.677	2.08	.521	1.183	1.724
	(.613)	(.688)	(1.198)	(.567)	(.659)	(1.112)
	[.052]	[.152]	[.189]	[.064]	[.144]	[.210]
Separated	−.19	−.161	−.33	−.179	−.107	−.271
	(.33)	(.361	(.473)	(.279)	(.313)	(.429)
	[−.056]	[−.047]	[−.097]	[−.078]	[−.047]	[−.118]
Never married	.163	−.245	.382	−.02	.01	.609
	(.409)	(.517)	(.628)	(.379)	(.652)	(.592)
	[.014]	[−.020]	[.032]	[−.001]	[.001]	[.037]
	Child Lives with Mother			Child Lives with Father		
Parent is:	1–4 Years	5–8 Years	9+ Years	1–4 Years	5–8 Years	9+ Years
Divorced	.736	1.177	1.088	.005	.172	.263
	(.222)	(.278)	(.399)	(.035)	(.049)	(.081)
	[.111]	[.177]	[.164]	[.005]	[.174]	[.266]
Separated	−.128	−.598	−.715	.016	−.021	−.045
	(.205)	(.306)	(.3)	(.025)	(.034)	(.035)
	[−.037]	[−.171]	[−.205]	[.048]	[−.063]	[−.135]
Never married	−.196	.282	.772	−.106	−.152	−.208
	(.193)	(.291)	(.32)	(.056)	(.097)	(.124)
	[−.053]	[.076]	[.209]	[−.294]	[−.422]	[−.578]

NOTE.—Standard errors, adjusted for clustering at the state of residence level, are in parentheses; percentage impacts are in square brackets. All coefficients are multiplied by 100. Each set of coefficients is from a separate regression that includes the following: race, state of residence dummies, age dummies, year dummies, age*year dummy interactions, and state-specific linear time trends. The top portion of the table uses data on adults, examining own marital status, in the first three columns for women, and in the second three columns for men. The second panel uses data on children, examining the marital status of the parent with whom they reside; in the first three columns the dependent variable is for residing with a mother who is divorced/separated/never married, the last three columns are parallel for a child living with a father. Each set of three columns is from the same regression that replaces UNILAT dummy with dummies for exposure for 1–4, 5–8, and 9 or more years.

adult in the sample was exposed to unilateral divorce as an adult (after age 18) and as a youth (up through age 18). I then divide these current unilateral and unilateral as youth dummies into three exposure categories: 1–4 years of exposure, 5–8 years of exposure, and 9 or more years of exposure. It is important to note that, by the nature of the construction of years exposed, the exposure effects of laws as a youth pick up two effects: amount of time exposed and age first exposed. That is, an individual who is exposed for 8 years is by definition first exposed at age 10. This makes it difficult to interpret the effects below as effects of additional exposure rather than as affects of being exposed at a younger age.[11] Thus, this analysis complements, rather than replaces, the earlier findings.

Table 7 first explores the impact of additional exposure on marital status,

[11] The framework used in this subsection mirrors that used by Johnson and Mazingo (2000) in their recent working paper. The results are broadly consistent with their findings.

Table 8
Amount of Exposure to Unilateral Divorce Regulation and Adult Outcomes

	Females			Males		
	1–4 Years	5–8 Years	9+ Years	1–4 Years	5–8 Years	9+ Years
Family structure:						
Married	.525	.428	1.171	.481	.264	1.219
	(.324)	(.336)	(.633)	(.404)	(.563)	(.981)
	[.007]	[.006]	[.016]	[.007]	[.004]	[.017]
Divorced	.031	.051	.046	−.041	−.032	−.189
	(.279)	(.338)	(.453)	(.264)	(.272)	(.471)
	[.003]	[.005]	[.004]	[−.005]	[−.004]	[−.023]
Separated	.206	.35	.622	.204	.25	.404
	(.086)	(.101)	(.162)	(.053)	(.077)	(.12)
	[.061]	[.103]	[.183]	[.089]	[.109]	[.176]
Never married	−.93	−1.092	−2.158	−.699	−.541	−1.51
	(.508)	(.62)	(1.121)	(.661)	(.806)	(1.459)
	[−.078]	[−.091]	[−.18]	[−.042]	[−.033]	[−.091]
No. of children	.008	.021	.056	.034	.042	.08
	(.009)	(.012)	(.017)	(.014)	(.022)	(.023)
	[.004]	[.010]	[.028]	[.029]	[.036]	[.026]
Educational attainment:						
Years of education	−.052	−.062	−.088	−.058	−.064	−.092
	(.033)	(.032)	(.038)	(.043)	(.040)	(.052)
	[−.005]	[−.005]	[−.008]	[−.005]	[−.005]	[−.008]

High school dropout	.504 (.48) [.027]	.549 (.501) [.029]	.837 (.659) [.044]	.629 (.401) [.032]	.852 (.413) [.044]	1.435 (.581) [.074]
High school graduate	.826 (.519) [.023]	1.27 (.572) [.0345]	1.815 (.692) [.05]	.643 (.341) [.021]	.72 (.453) [.023]	.928 (.469) [.030]
Some college	−.086 (.213) [−.004]	−.469 (.335) [−.022]	−.395 (.471) [−.018]	−.089 (.19) [−.004]	−.561 (.229) [−.026]	−.44 (.267) [−.020]
College graduate	−1.244 (.388) [−.054]	−1.35 (.508) [−.058]	−2.257 (.562) [−.098]	−1.183 (.473) [−.042]	−1.011 (.556) [−.036]	−1.922 (.733) [−.068]
Living standards:						
Income per capita	−189.69 (128.76) [−.014]	−461.46 (171.29) [−.034]	−724.26 (210.44) [−.053]	−141.23 (133.88) [−.009]	−254.24 (190.61) [−.017]	−610.26 (222.28) [−.040]
Below poverty	−.016 (.28) [−.001]	−.006 (.307) [−.001]	.405 (.404) [.034]	−.074 (.16) [−.009]	.085 (.268) [.01]	.317 (.281) [.037]
Labor supply:						
Work last year	−.303 (.22) [−.004]	−.255 (.244) [−.004]	−.902 (.314) [−.013]	.127 (.144) [.001]	.231 (.16) [.002]	.102 (.202) [.001]
Weeks worked	−.06 (.057) [−.002]	−.056 (.104) [-.002]	−.385 (.122) [−.013]	.005 (.081) [.000]	−.067 (.061) [−.001]	−.121 (.089) [.003]
Earnings	−45.17 (100.21) [−.042]	−238.17 (131.31) [−.022]	−616.32 (163.92) [.058]	310.17 (280.43) [.012]	368.27 (293.69) [.014]	592.12 (489.56) [.023]

NOTE.—Standard errors, adjusted for clustering at the state of birth level, are in parentheses; percentage impacts are in square brackets. All coefficients are multiplied by 100. Each set of coefficients is from a separate regression that includes the following: race, state of residence dummies, age dummies; year dummies, age*year dummy interactions, and state-specific linear time trends. The first set of columns presents results for women, the second set for men. Each set of three columns is from same regression that replaces UNILAT dummy with dummies for exposure for 1–4, 5–8, and 9 or more years.

mirroring table 3. For presentational simplicity, I show only the results with state-specific trends. The basic finding from both female divorce and for the odds of living with a divorce mother is that there appears to be an increasing effect from 1–4 to 5–8 years of exposure, but there is little additional effect beyond 8 years. This is consistent with Wolfers's (2000) conclusion. For men and for the odds of living with a divorced father, there is more of a monotonic increase over all three ranges.

I also find an interesting pattern of exposure effects for the variables measuring unilateral exposure as a youth, as shown in table 8; the first set of columns shows results for females and the second set for males, and all results include state-specific trends (as well as parallel exposure categories as an adult). For the marital status and labor supply variables, the impacts are roughly constant or only slightly increasing for 1-4 and 5-8 years of exposure, and then grow substantially after 8 years. The pattern of effects on years of education is fairly flat, but when subcategories of education are examined, there emerges the same pattern of relatively flat effects through 8 years and then a significant rise for 9 or more years of exposure.

Thus, this exposure timing evidence appears to confirm that channels other than increased divorce are at least partly responsible for the impacts of unilateral divorce that we observe. Despite an impact of unilateral divorce on divorce levels that is fairly flat after 8 years, the impacts on marriage and labor supply increase significantly with 8 or more years of exposure.

In terms of causal channels among the impacts of unilateral divorce, these findings appear to offer further suggestion of a causal effect of marriage patterns on labor supply. Reductions in earnings for women, and increases for men, are occurring mostly after 8 years of exposure, which is exactly when the marriage effect grows.

E. Age Effects on Marriage

As shown above, exposure to unilateral divorce as a youth leads to a higher likelihood of being married. However, in these cross-sections of a given set of ages, a higher likelihood of being married on average could result from either increased odds of marriage at every age or from a shift forward in the timing of marriage. I explore this issue in table 9. I show the results, for the marriage variables, of models that interact the current unilateral law and unilateral as youth dummies with age. I focus solely on results with state-specific trends. The top portion of the table shows the results for women; the bottom portion shows the results for men.

In fact, I find that the effects of exposure to unilateral divorce as a youth on marriage appear to mostly arise through marriage timing. The age interaction with the unilateral-as-youth dummy is negative, signifi-

Table 9
Age Pattern of Effects of Unilateral Divorce on Marital Status

	Unilateral as Youth		Unilateral as Adult	
	Unilateral	Unilateral * (Age − 24)	Unilateral	Unilateral * (Age − 24)
Female:				
Married	1.496	−.133	−.782	−.005
	(.429)	(.042)	(1.479)	(.051)
	[.021]	[−.002]	[−.011]	[0]
Divorced	.406	−.037	.788	.026
	(.188)	(.018)	(.303)	(.038)
	[.037]	[−.003]	[.072]	[.002]
Separated	.427	−.026	−.073	−.011
	(.081)	(.009)	(.334)	(.01)
	[.126]	[−.008]	[−.021]	[−.003]
Never married	−2.26	.184	−.28	.025
	(.538)	(.051)	(1.139)	(.084)
	[−.188]	[.015]	[−.023]	[.002]
Male:				
Married	1.745	−.17	−.851	.004
	(.593)	(.057)	(1.793)	(.062)
	[.024]	[−.002]	[−.012]	[0]
Divorced	.239	−.019	.306	.037
	(.237)	(.028)	(.225)	(.033)
	[.029]	[−.002]	[.037]	[.005]
Separated	.279	−.014	.043	−.012
	(.076)	(.007)	(.256)	(.007)
	[.121]	[−.006]	[.019]	[−.005]
Never married	−2.256	.199	.479	−.022
	(.746)	(.066)	(1.554)	(.096)
	[−.136]	[.012]	[.029]	[−.001]

NOTE.—Standard errors, adjusted for clustering at the state of birth level, are in parentheses; percentage impacts are in square brackets. All coefficients are multiplied by 100. Each set of coefficients is from a separate regression that includes the following: race, state of residence dummies, age dummies, year dummies, age*year dummy interactions, and state-specific linear time trends. Each row reports coefficients from one regression that includes current exposure, youth exposure, and interactions of each exposure dummy with (current age − 24).

cant, and sizable; it indicates that the impact at age 25 is .015 percentage points, but that, by age 36, the impact is roughly zero. That is, unilateral divorce exposure as a youth raises the odds of being married at younger ages but actually lowers them at older ages. Moreover, there are positive and large impacts on being divorced (not significant) and separated (significant) at younger ages that also fade with time. The pattern of results is very similar for males. It is also interesting to note that, for current exposure to unilateral divorce regimes, there are only very modest increases in the impact of unilateral divorce on divorce propensities with age for women, although more sizable increases with age for men.

Thus, the findings here echo the conclusions of the observational literature on intergenerational effects of divorce cited earlier: exposure to easier divorce as a child leads to earlier marriage, but it leads to more marital instability as well. By the time that adults are in their late thirties, in fact, there is little net impact on marital status. This suggests that these children of easier divorce laws, perhaps reacting to increased parental marital instability induced by easier divorce laws, move more quickly to form their own unions once they leave the home but that these unions end up being less stable than do marriages of children not exposed to unilateral divorce.

Moreover, in results not reported, I have investigated the age interaction effects on labor supply, and they continue to be consistent with a causal pathway through marriage: both the reduction in labor supply for women and the increase in labor supply for men fade with age. Thus, while the magnitudes of the labor supply effects appear too large to be explained by marriage impacts, the pattern of results continues to suggest that causal pathway.

F. No-Fault versus Unilateral Divorce

This analysis has followed the previous economics literature in focusing on the impact of unilateral divorce regulations as the key regime change and not the presence of no-fault divorce per se. This is because it is unilateral divorce that leads to the important shift in property rights that may both raise divorce rates and strongly shift bargaining power within the family. No-fault divorces may have reduced transactions costs, but there was no fundamental change to the nature of the family bargaining structure that should have a major impact on divorces or outcomes.

Nevertheless, if the transactions costs reduced by no-fault divorce were large, it is possible that these reforms could have also affected outcomes. I have therefore estimated all of the models reported thus far, including alongside the measures of unilateral divorce measures of exposure to no-fault divorce regimes, both concurrently and as a youth; these coefficients are separately identified by the gaps between no-fault and unilateral divorce dates shown in table 1.

I find that there is a marginally significant effect of no-fault regulations on the odds of divorce but that this impact is much smaller than the effects of unilateral regulations; for example, there is a rise in the odds of divorce of 0.0035 percentage points for women and of 0.0017 percentage points for men, effects which are roughly one-quarter as large as the impacts of unilateral regulations; neither estimate is statistically significant. In terms of outcomes of adult males and females, the effects of no-fault exposure as a youth (or currently) are almost always insignificant, while the effects of unilateral exposure are very similar to those reported earlier. Thus, the intuition of the economics literature is confirmed; there is little impact of reducing transactions costs on outcomes, while there are large impacts of shifting property rights.

G. Specification Checks

I have also pursued a series of specification checks to assess whether I am truly uncovering a causal impact of unilateral divorce regulations. One concern with these results is that I only measure state of birth, while the relevant variable is years of exposure as a youth. This could potentially lead to the type of selective migration bias discussed by Heckman, Layne-Farrar, and Todd (1996), whereby migration decisions are related to divorce regimes. I can address this bias straightforwardly in my context by assessing whether there is a differential impact on those who still live in their state of birth; while it is possible that those individuals lived somewhere else for their youth, it is extremely unlikely. Doing so, I obtain results that are very similar to the main results reported in tables 4 and 5. Thus, it appears that selective migration cannot explain these results.

Another concern is that there are somehow other omitted state policy variables that are correlated with the passage of unilateral divorce regulations. One obvious candidate is welfare generosity; if states were reducing welfare generosity at the same time that they were making divorce easier, then the long-term negative effects on children I observe could be through welfare policy changes. I have tested this hypothesis directly by modeling state maximum welfare payments as a function of a unilateral divorce law dummy, controlling for state and year fixed effects.[12] The coefficient on unilateral laws in such a regression is close to zero and highly insignificant, suggesting no correlation between divorce laws and welfare generosity.

Another candidate is education spending. It is possible that unilateral divorce laws were being passed in states where there was less spending on education, once again leading to more adverse child outcomes. I have gathered data on state spending on education for 5-year intervals from 1955 to 1990, and I once again find no significant correlation with the presence of unilateral divorce laws.

Finally, there is a concern that the results are driven by outlier states. The obvious candidate is California. California was going through a multitude of social changes at the time of its unilateral divorce reform, including radical changes to its educational finance system and the legalization of abortion. The direction and magnitude of these effects for future child outcomes is, on net, ambiguous, but it is important to assess if this state is driving the results. In fact, California is a significant outlier, and the results from tables 4 and 5 are significantly weakened when those born in California are excluded from the analysis. However, the basic pattern of results remains.

[12] I am grateful to Robert Moffitt for preparing the data on state maximum welfare payments. These data are available for 1960, 1964, and 1968 onward; I estimate my regression over the 1960–90 period.

Thus, overall, the census results are fairly robust to sensitivity checks. The largest impact appears to be the effect of California, which reduces the magnitudes but does not alter the basic message of the census findings.

V. Conclusions

Does making divorce easier have negative long-run implications for children? The results in this article suggest that the answer is a qualified yes. Those who are exposed to unilateral divorce regulations are more likely to be living with divorced parents as youths. As adults, they are less educated and have lower family incomes. These lower family incomes appear to largely arise because of earlier marriage, more children, and reduced labor force attachment by women. These exposed children are also more likely to commit suicide. The evidence suggests that the reduction in female labor supply arises through this increase in marriage propensities. But these earlier marriages also appear to be more likely to end in separation and divorce; on net, there is little impact on marital status at middle ages, but there is more marital churning at younger ages.

Thus, the qualification: children of unilateral divorce are living with worse living standards later in life, but this is largely because the females in this group are staying at home with children rather than working at younger ages. Therefore, the question of whether making divorce easier is bad for children is fundamentally determined by the welfare impacts of this earlier family formation and resulting reduction in labor supply. In other words, the answer to this question gets pushed back yet another generation: is it beneficial or detrimental for the "grandchildren" of unilateral divorce to be born to younger mothers, with lower incomes but more maternal time in the home? Given that the children of unilateral divorce regulations are not only marrying but also dissolving marriages earlier and more frequently, it seems likely that this was on net detrimental to this "grandchild" cohort. However, further research is required to confirm this conclusion.

Moreover, further research is necessary to understand the mechanisms behind the results uncovered here. For example, the impacts on adult outcomes appear simply too large to be explained by increased exposure to divorce as a youth; clearly, these laws had other effects that affected the upbringing of youths. Trying to get inside the "black box" of intact marriages, particularly with respect to issues of bargaining power, is central to assessing these causal pathways.

References

Achenbach, Thomas M., and Craig S. Edelbrock. 1983. A manual for the child behavior checklist and revised child behavior profile. Burlington, VT: University of Vermont, Department of Psychiatry.

Allen, Douglas. 1992. Marriage and divorce: A comment on Peters. *American Economic Review* 82, no. 3:677–85.

Amato, Paul R., and Bruce Keith. 1991. Parental divorce and the well-being of children: A meta-analysis. *Psychological Bulletin* 110, no. 1: 26–46.

Aquilino, William S. 1994. Impact of childhood family disruption on young adults' relationships with parents. *Journal of Marriage and the Family* 56, no. 2:295–313.

Baumrind, Diana. 1989. Sex-differentiated socialization effects in childhood and adolescence in divorced and intact families. Paper presented at the biennial meetings of the Society for Research on Child Development, Kansas City, MO.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. How much should we trust differences-in-differences estimates? *Quarterly Journal of Economics* 119, no. 1:249–75.

Bougheas, Spiros, and Yannis Georgellis. 1999. The effect of divorce costs on marriage formation and dissolution. *Journal of Population Economics* 12, no. 3:489–98.

Cherlin, Andrew J., Kathleen E. Kiernan, and P. Lindsay Chase-Lansdale. 1995. Parental divorce in childhood and demographic outcomes in young children. *Demography* 32, no. 3:299–315.

Cooney, Theresa M. 1994. Young adults' relations with parents: The influence of recent parental divorce. *Journal of Marriage and the Family* 56, no. 1:45–56.

Corak, Miles. 2001. Death and divorce: The long-term consequences of parental loss on adolescents. *Journal of Labor Economics* 19, no. 3: 682–715.

Dee, Thomas. 1999. The complementarity of teen smoking and drinking. *Journal of Health Economics* 18, no. 6:769–93.

Dee, Thomas, and William Evans. 2001. Teens and traffic safety. In *Risky behavior among youths: An economic analysis*, ed. Jonathan Gruber, pp. 121–66. Chicago: University of Chicago Press.

Doherty, William J., and R. H. Needle. 1991. Psychological adjustment and substance abuse among adolescents before and after a parental divorce. *Child Development* 62:328–37.

Dornbush, Sanford, J. Carlsmith Merrill, Steven Bushwall, Philip L. Ritter, Herbert Leiderman, Albert H. Hastorf, and Ruth T. Gross. 1985. Single parents, extended households, and the control of adolescents. *Child Development* 56, no. 2:326–41.

Feng, Du, Roseanne Giarruso, Vern Bentson, and Nancy Frye. 1999. Intergenerational transmission of marital quality and marital instability. *Journal of Marriage and the Family* 61, no. 2:451–63.

Ferber, Marianne, and William Sander. 1989. Of women, men, and divorce: Not by economics alone. *Review of Social Economy* 47, no. 1:15–26.

Flewelling, Robert L., and Karl E. Bauman. 1990. Family structure as predictor of initial substance use and sexual intercourse in early adolescence. *Journal of Marriage and the Family* 52:171–81.

Friedberg, Leora. 1998. Did unilateral divorce raise divorce rates? Evidence from panel data. *American Economic Review* 88, no. 3:608–27.

Furstenberg, Frank F., and Julien O. Teiler. 1994. Reconsidering the effects of marital disruption: What happens to children of divorce in early adulthood? *Journal of Family Issues* 15, no. 2:173–90.

Gray, Jeffrey. 1998. Divorce law changes household bargaining and married women's labor supply. *American Economic Review* 88, no. 3:628–42.

Guidubaldi, John, Joseph D. Perry, and Helen K. Cleminshaw. 1984. The legacy of parental divorce: A nationwide study of family status and selected mediating variables on children's academic and social competencies. In *Advances in clinical child psychology*, vol. 7, ed. Benjamin B. Lahey and Alan E. Kazdin, pp. 109–51. New York: Plenum.

Heckman, James, Anne Layne-Farrar, and Petra Todd. 1996. Does measured school quality really matter? An examination of the earnings-quality relationship. In *Does money matter?* ed. Gary Burtless, pp. 192–289. Washington, DC: Brookings Institution.

Hetherington, Eileen M., and William G. Clingempeel. 1992. Coping with marital transitions: A family systems perspective. *Monographs of the Society for Research in Child Development*, 57, nos. 2–3:1–238.

Isaacs, Marla. 1986. The visitation schedule and child adjustment: A three-year study. *Family process* 27, no. 2:252–56.

Johnson, John, and Chris Mazingo. 2000. The economic consequences of unilateral divorce for children. Working Paper no. 00-0112, University of Illinois, CBA Office of Research, Urbana.

Kay, Herma Hill. 1977. The California background. Unpublished manuscript, written for the California Divorce Law Research Project, Center for the Study of Law and Society, University of California, Berkeley.

Keith, Verna M., and Barbara Finley. 1988. The impact of parental divorce on children's educational attainment, marital timing, and likelihood of divorce. *Journal of Marriage and the Family* 50, no. 3:797–809.

Kiernan, Kathleen, and Andrew Cherlin. 1999. Parental divorce and partnership dissolution in adulthood: Evidence from a British cohort study. *Population Studies* 53, no. 1:39–48.

Kiernan, Kathleen, and John Hobcraft. 1997. Parental divorce during childhood: Age at first intercourse, partnership, and parenthood. *Population Studies* 51, no. 1:41–55.

Krein, Sheila F., and Andrea H. Beller. 1988. Educational attainment of children from single-parent families: Differences by exposure gender and race. *Demography* 25, no. 2:221–34.

Lang, Kevin, and Jay Zagorsky. 2001. Does growing up with a parent absent really hurt? *Journal of Human Resources* 36, no. 2:253–73.

Lundberg, Shelly, Robert Pollak, and Terence Wales. 1997. Do husbands and wives pool their resources? Evidence from the United Kingdom child benefit. *Journal of Human Resources* 32, no. 3:463–80.

Matsueda, Ross L., and Karen Heimer. 1987. Race, family structure, and delinquency: A test of differential association and social control theories. *American Sociological Review* 52, no. 6:826–40.

McLanahan, Sara, and Larry Bumpass. 1988. Intergenerational consequences of family disruption. *American Journal of Sociology* 94, no. 1: 130–52.

McLanahan, Sara, and Gary Sandefur. 1994. *Growing up with a single parent.* Cambridge, MA: Harvard University Press.

McLeod, Jane D. 1991. Childhood parental loss and adult depression. *Journal of Health and Social Behavior* 32, no. 3:205–20.

Nordinger, Pia. 1998. The anti-divorce revolution. *Weekly Standard*, March 2.

Peters, H. Elizabeth. 1986. Marriage and divorce: Informational constraints and private contracting. *American Economic Review* 76, no. 3: 437–54.

———. 1992. Marriage and divorce: Reply. *American Economic Review* 82, no. 3:686–93.

Reilly, Siobhan, and Eirik Evenhouse. 1997. Divorce laws and divorce rates: Evidence from panel data. Photocopy, Vanderbilt University.

Ross, Heather L., and Isabel V. Sawhill. 1975. *Time of transition: The growth of families headed by women.* Washington, DC: Urban Institute.

Strauss, John, and Duncan Thomas. 1995. Human resources: Empirical modeling of household and family decisions. In *Handbook of development economics*, ed. Jere Behrman and T. N. Srinivasan, vol. 3A, pp. 1883–2023. Amsterdam: Elsevier.

Weitzman, Lenore. 1985. *The divorce revolution.* New York: Free Press.

Wolfers, Justin. 2000. Did unilateral divorce raise divorce rates? Reassessing the evidence. Photocopy, Harvard University.

Wolfinger, Nicholas. 1999. Trends in the intergenerational transmission of divorce. *Demography* 36, no. 3:415–20.

Zill, Nicholas, Donna R. Morrison, and Mary J. Coiro. 1993. Long term effects of parental divorce on parent-child relationships adjustment and achievement in young adulthood. *Journal of Family Psychology* 7, no. 1:91–103.