The Impact of Divorce Laws on Marriage-Specific Capital

Betsey Stevenson
bstevenson@post.harvard.edu

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Abstract

The "no-fault" revolution of the 1970s ushered in a wave of divorce law reform in which states began to grant divorce on demand by either spouse. This change in the marital contract affects the incentives to invest in marriage. Because states changed their laws in different years, there exists useful quasi-experimental variation with which to examine the effects of this change. While past research has focused on the effects of these reforms on marital formation and dissolution, or spousal bargaining, this paper focuses on the incentives for investment in marriage-specific capital. Data from the 1970 and 1980 censuses are analyzed to estimate the effect that marrying under, or living in, a unilateral divorce regime has on investment in marriage-specific capital. Three types of marriage-specific investment are analyzed: home ownership, children, and specialization in market versus non-market production. All three measures suggest that divorce regime appears to have real, significant effects on investment within marriage.

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1. Introduction

During the past thirty years, many states reformed their divorce laws so as to grant divorce on demand by either spouse. This legal change was part of a broader movement in which states began to recognize "irreconcilable differences" as a legitimate reason for divorce.[1] Initial research has focused on the extent to which liberalizing access to divorce led divorce rates to rise or altered spousal bargaining. Beyond these immediate effects, marriage and divorce laws set the parameters for intertemporal contracting between partners, and hence are likely to influence the incentives for investment in marriage-specific capital. Hence this paper focuses on the effects of family law regime on marriage-specific investments.

Marriage-specific capital is defined as an investment that either loses value or can be captured by one spouse when the marriage ends. Consider, for example, a wife who invests in her husband's human capital. Her investment is marriage-specific because the husband appropriates this human capital when the marriage ends. Although the value of the investment is unchanged, the wife's ability to earn a return on that investment falls. Alternatively, some investments – such as children – are public goods (or more precisely, their consumption is *non-rival* within the household). If the household dissolves, then the returns on this investment may diminish due to child custody restrictions. Finally, some investments—such as housing—are not intrinsically marriage-specific, but involve sufficiently large transaction costs that their value within the marriage is far greater than that when divorced.

While much of the initial debate over these laws focused on the consequences for the divorce rate, these general equilibrium bargaining and investment effects may be more important for evaluating the welfare consequences of divorce laws.

Divorce laws affect the incentive to invest in marriage-specific capital for several reasons.. First, if liberalizing access to divorce raises the divorce rate, then each spouse is less likely to reap the benefits of marriage-specific capital, reducing the incentive to jointly invest. Becker, Landes, and Michael (1977) first proposed this idea, arguing that "couples are reluctant to invest in skills or commodities 'specific' to their

[1] Weitzman (1985)

marriage if they anticipate dissolution." Therefore, if unilateral divorce caused an increase in the expected probability of divorce, then we should see a reduction in marriage-specific investment.

An alternative channel considers bargaining over rents within the relationship, which in standard bargaining models depends on the threat points of each partner – either the threat to divorce (particularly relevant under unilateral divorce laws, as in McElroy and Horney, 1981), or the threat to revert to non-cooperation within the household, as in Lundberg and Pollak (1994). This is important because once a marriage-specific investment has occurred, the returns are pure rents, and hence the incentive to jointly invest may depend upon the ability of the couple to commit to a specific distribution of future rents. For instance, in a regime which grants divorces only for cause or by mutual consent, a husband who has a good outside option (e.g., a good job, a new girlfriend) may be unable to use this option because his wife holds the relevant property right (the right to remarry). Under unilateral divorce, however, the outside option becomes more relevant because he can exercise it at any time. His bargaining position inside the marriage improves, and he is therefore able to demand a larger share of the couple's joint production. Because he cannot commit to *not* to take a larger share of goods, his wife is less likely to make marriage-specific investments because she would receive a smaller share of the proceeds. Thus, even with no change in the couple's actual divorce behavior, marriage-specific investment may change, with the sign of the predicted change depending upon the specific bargaining position of the two spouses and the time profile of expected future returns to the marital investment.

Finally, couples may use investment in marriage-specific capital strategically, over-investing today so as to constrain their future selves to prefer to remain married than to divorce. As such, robust investment in marriage-specific capital may be used to partially offset the incomplete enforcement of marriage contracts by the state. Thus, while the effects mediated through the divorce rate likely cause marriage-specific investment to fall, this latter channel suggests that it may rise, while the effects mediated by bargaining could go either way.

Several related strands of the literature are worth noting. The effect of unilateral divorce on divorce rates has been a hotly contested debate with both theoretical and empirical work pointing in both

directions.[2] On the theory side, Becker, Landes and Michael (1977) argue that marital bargaining is "an excellent illustration of the Coase Theorem that the allocation of property rights or legal liability does not influence resource allocation when the parties involved can bargain with each other at little cost." By contrast, Peters (1988) argues that a "fixed wage" contract may better describe marital bargaining, and under such a contract the divorce rate is affected by divorce laws. Empirically, Gruber (2000) argues that census data show that the stock of divorced people rose significantly in unilateral divorce states. However, research by Wolfers (2003) revealed that, while the stock of the currently divorced may have risen, the probability of being an ever-divorced person is little changed by unilateral divorce laws. Friedberg (1998) notes that the flow of new divorces does in fact rise following a shift to unilateral divorce laws, although Wolfers (2003) shows that these effects are transitory and fade out within a decade. One reconciliation of these results is that unilateral divorce leads to earlier divorce and less remarriage. The implication of this interpretation is that divorce laws may affect the expected duration of a marriage without affecting the probability of dissolution. Because investment decisions reflect the NPV of future returns, the incentive to invest diminishes with a shortened horizon over which marriage-specific investments yield returns.

Papers by Parkman (1992), Gray (1998), and Chiappori, Fortin and Lacroix (2002) show important labor supply responses following these divorce law changes, which they interpret as reflecting bargaining effects. Stevenson and Wolfers (2003) provide related evidence on the redistribution of bargaining power within relationships, noting large declines in female suicide, domestic violence and intimate femicide following divorce law liberalization. Thus it seems plausible that divorce laws affected bargaining within marriage, which in turn may affect the ability of spouses to commit to share the returns to specific investment.

The only research looking directly at investment outcomes relates child outcomes to divorce laws. Gruber (2000) examines adult outcomes for children who grew up in unilateral divorce states, finding that such children have lower educational attainment and lower family incomes and, on average, married earlier

[2] Peters (1986), Peters (1992), and Wolfers (2003) all find that divorce rates did not much increase as a result of unilateral divorce. Allen (1992) and Friedberg (1998) find that they did.

and have an increased likelihood of separation. Johnson and Mazingo (2000) find similar results for women, but not for men. Both papers argue that the outcomes are not simply the result of having been more likely to grow up in a divorced household, but rather are due to deeper economic forces. For instance, Becker (1981) argues that children are a form of marriage-specific investment; viewed through this lens, one likely explanation for why adults exposed to unilateral divorce as children have lower educational attainment is that their parents simply invested less in their education. This logic reflects the view that children are a collective good within marriage, requiring the cooperative efforts of their parents in their production, and yielding non-rival consumption benefits. Outside of the marriage the utility from children decreases for the non-custodial parent due to decreased contact (and monitoring problems), and utility potentially decreases for the custodial parent by reducing their remarriage prospects.

As such, several of the more far-reaching effects of divorce regime may be mediated through the incentives for marriage-specific investment. This paper exploits the variation generated by the timing of divorce law reforms across the United States to evaluate changes in marriage-specific investment that are represented by home ownership, employment and children. Specifically, different states changed their divorce laws in different years. I exploit changes in each state's divorce regime over time, controlling for time invariant state characteristics and for factors that affect all states at a given time.

I investigate several outcomes because each allows me to test certain hypotheses about the way unilateral divorce affects marriage-specific investment. For instance, home ownership is a marriage-specific investment because couples pay large fixed costs for each house purchase and typically make investments in a home that are based on their preferences as a couple. These investments confer both financial and emotional benefits so long as the marriage is intact, but may be worth less to future owners (including one of the spouses by him or herself). For a couple that wants to make less marriage-specific investment, there is a simple alternative: renting. As an outcome, home ownership has the virtue of being easily observable, and the investment decision likely does reflect marital bargaining and the probability of divorce (as opposed to extraneous unobserved factors).

I also consider labor force participation of both men and women. Women who anticipate divorce should be less willing to invest in their husband's market skills, which retards the forces for household specialization. Men married to such women will be led to spend more time in non-market work and less time in market work as a result of their wives' decreased specialization in homemaking. I measure market participation by analyzing hours worked.

The final outcome that I investigate is investment in children. Investment in children is somewhat difficult to measure because parents can increase their investment along both the quantity and quality margins (more children versus more time and resources spent on each child; Becker and Lewis, 1973). Data limitations lead me to examine the quantity margin, specifically the likelihood that a couple has more than two children. For newlywed couples, I examine the probability that they have any children.[3]

I find signs of reduced investment in marriage-specific capital. There is about a 2 percentage point decrease in home ownership among couples in unilateral divorce states. Furthermore, for couples in unilateral divorce states, there is a 3-7 percentage point decrease in the probability of having more than two children. Evidence on newlyweds suggests a 7-8 percentage point decrease in the probability of having children in the first two years of marriage. Finally, there is suggestive evidence that female hours in the labor force rose and male hours declined as a result of the changing divorce regulations.

2. Empirical Strategy

To identify the effects of unilateral divorce on marriage-specific investment, I exploit variation in the timing of states' divorce regimes. Table 1 shows the year that unilateral divorce was passed in each state. Currently 34 states allow for unrestricted unilateral divorce, and of these, 29 changed their law between 1967 and 1978.[4] Nine other states passed unilateral divorce laws with separation requirements

[3] Newlyweds are defined as those married two years or less.
[4] Utah and South Dakota adopted unrestricted unilateral divorce in the mid-1980s. The other three states had pre-existing unrestricted unilateral divorce.

during this period.[5]

I use variation in the state and year of marriage to see if the presence of unilateral divorce laws discourages investment in marital capital. Using such variation (within states, over time) raises the question of how to measure the divorce regime that applies to each marriage. Ideally, one would like to know, for each potential marriage-specific investment, what divorce regime was prevailing and what divorce regime was expected to prevail. But the timing of marriage-specific investment is quite flexible (children, house purchases, and departure from the labor force may all be delayed). As a result, we would not know what divorce regime to assign to couples who do not make the marriage-specific investment. As an example, consider a home purchase. For couples who actually purchase a home, we could assign the divorce regime prevailing in the year in which the house was purchased. But, for renters, we would be at a loss to choose the year in which to measure the regime.

In short, I want a measure of the divorce regime that accurately reflects a couples' experience but that can also be recorded symmetrically for couples who do and do not invest. I use two such measures. The first is the divorce regime that prevailed at the time the marriage occurred. The second is the share of married years in which the couple has been exposed to unilateral divorce laws. The first measure emphasizes the initial conditions of the marriage and early years, in which many key marriage-specific investments are made. The second measure emphasizes current conditions and the effects of continuous exposure. Data from the 1970 and 1980 censuses on the age of first marriage can be used to calculate the year of marriage for individuals currently in their first marriage.[6] Data limitations lead me to restrict my analysis to first marriages.

Because divorce laws may change selection both into and out of marriage, focusing on currently married couples induces potentially confounding influences. Theory does not immediately help us predict the net effect of these influences. For example, bad marriages may end earlier after unilateral divorce, so

[5] Gruber (2000) codes only those states with no separation requirement as unilateral. This paper will follow Gruber by looking at unrestricted unilateral divorce and will also consider a middle ground definition where states with a one year separation requirement are also included as unilateral.

[6] Whether an individual is in a first or subsequent marriage is identified by the census only through to 1980.

there will be more "bad marriages" prior to unilateral divorce. If bad marriages have lower marriage-specific investment, then, even if no one changes their investment behavior, regressions examining the effect of unilateral divorce on marital investment will show an *increase* in marriage-specific investment. On the other hand, couples may be more likely to marry when they know that they can exit the marriage more easily.[7] In other words, match quality may go down because the cost of a bad match falls. Marriages that are worse matches have a higher expectation of divorce and, therefore, should have less marriage-specific investment. This effect on selection into marriage will lead regression estimates to suggest that unilateral divorce lowers investments.

Because selection into marriage and selection out of marriage both generate potential biases in estimates of the effect of unilateral divorce on marriage-specific investments, I consider two sub-populations that each serve to minimize one of the forms of selection bias. First, I consider individuals in the first two years of marriage. These newlyweds have been married such a short time that selection *out of marriage* is unlikely to have taken place.[8] Therefore, regressions based on newlyweds should not contain bias from the disappearance of bad marriages from the sample. In this case the regression strategy is to compare newlyweds from the 1980 census with those from the 1970 census, across both types of divorce regimes. Of the 29 law changes, 26 occurred between 1970 and 1978, and these will be the treatment states in these regressions.

The second sub-population that I consider consists of those who are married under a fault-divorce regime, but then spend most of their married lives under a unilateral divorce regime.[9] Such marriages are not contaminated by the effects that divorce laws may have on selection *into marriage*. Therefore, these regressions are not contaminated by changes in the proportion of the population in weakly committed relationships. To create this sub-population, individuals married 1-2 years before the legal change occurs

[7] Alternatively, individuals know that a potential spouse is more likely to want to divorce. Since divorces are emotionally and financially costly some individuals may be more cautious about entering a marriage.
[8] Even if there was not this bias, one would expect the effect of unilateral divorce to be greatest on the marital investment of young couples and to disappear over many years of marriage, as the probability of divorce (and therefore the effect of divorce law) decreases with length of marriage.
[9] The actual state in which the marriage took place is unknown. Only the year of marriage, state of birth and current state of residence are known.

are identified as the treatment group, so that the differences between the people married just before unilateral divorce and the equivalent cohort in the 1970s sample (in terms of years married) is compared with the differences in the non-treatment states. Since the treatment here is changing the divorce law quickly after the marriage occurs, only the non-reform, non-unilateral states are used as controls.[10] For example, Massachusetts changed to a unilateral divorce regime in 1975, so those married in 1973 or 1974 would be considered the population exposed to unilateral divorce in Massachusetts. The outcome variables for this group will be compared with the outcome variables for those married in 1963 or 1964 in Massachusetts from the 1970 census. States that did not have unilateral divorce and did not reform divorce laws will act as controls for all other states for all years of marriage represented by the treatment states.

When I examine the fraction of a marriage that was spent under unilateral divorce laws, I allow the estimated effects to be non-linear in exposure. This is done by using indicator variables for 25 percent intervals of being exposed to unilateral divorce.

Table 2 shows summary statistics for each of the relevant sample groups.

3. The Effects of Unilateral Divorce on Investment in a Home

The home of a married couple typically represents their most valuable joint asset and involves large transaction costs, making the purchase decision costly to reverse. Home ownership is an investment that is jointly beneficial when married, but one that has ready substitutes – rental units. Furthermore, couples jointly make choices about how much to invest in the home. Home ownership clearly represents more investment in marriage-specific capital than does renting. This specifically reflects both substantial transaction costs in buying and selling a home and improvements that reflect a couple's idiosyncratic tastes.

The census identifies whether a couple lives in a rental unit or a home that they own. I use an indicator variable for home ownership as my dependent variable. In keeping with the empirical strategy above, I estimate three regressions.

[10] In this context a marriage cohort are all couples with the same number of years married on census night.

The first regression considers only the population of newlyweds. The independent variable of interest is an indicator of whether or not unilateral divorce prevailed at the time of the marriage.[11] The regression run is:

$$Own\ home_{i,s,t} = a + b\ unilateral_{s,t} + l\ Census_t + \sum_s h_s state + X_{i,s,t} j + e_{s,t} \qquad (1)$$

where standard errors are clustered at the level of state*census-year . The *X* matrix includes controls for individual characteristics that are not likely to be affected by unilateral divorce, including: race, region, metropolitan status, and state effects. I also include year fixed effects. I do not control for variables that might be affected by unilateral divorce. For instance, one might want to control for family income in a home ownership regression, but family income is likely to be affected by unilateral divorce if women are more likely to work outside the home. I estimate this regression separately for women and men and report the results in the first two columns of the first row of Table 3a. Including only states with unrestricted unilateral divorce in the treatment group, the estimated coefficients, though negative, are not statistically significant. In columns 4-6 the treatment group is expanded to include unilateral divorce with a one year separation requirement.[12] Using this definition of unilateral divorce, the estimated coefficient on home ownership increases slightly, and is weakly significant.

The second set of regressions (bottom of Table 3a) addresses the bias caused by divorce laws changing selection into marriage. Here I compare individuals who were married in the two years prior to the law change with the corresponding cohort (in terms of years married) from the 1970 census, and with control states that did not change their laws. Because of the time variation in states' passage of divorce reform, this method compares, for Massachusetts, the difference between people who were married in 1974 with those married in 1964, while the difference for California is between people married in 1969 and 1959. However, non-reforming, non-unilateral states, such as New York, serve as controls for both Massachusetts and California by including people from all four of the mentioned marriage dates. This

[11] State of current residence is used for state of marriage introducing measurement error from the potential misclassification.

allows the non-reform states to assist in the identification of both length of marriage and year of census effects. Thus, the following regression is run:

$$Own\ home_{i,s,t} = a + b\ unilateral_{s,t} + l Census_t + \sum_s h_s state + \sum_t g_t Years\ Married + X_{i,s,t} j + e_{s,t} \qquad (2)$$

where *Unilateral* is equal to one if an individual was married in the two years prior to unilateral divorce becoming law and zero otherwise. The estimated coefficients from this regression are broadly similar to those from the previous regression. Considering all couples married a year or two before unilateral divorce is passed, there is a 2 percentage point decrease in home ownership rates for both men and women. When the sample is narrowed to those married 12 years or fewer, the estimated coefficient increases to almost 3 percentage points. Expanding the treatment group to include unilateral divorce with a one-year separation yields estimated coefficients that are statistically significant but slightly smaller. As unilateral divorce with a one-year separation requirement is a weaker form of unilateral divorce, theory would predict that it should have a smaller effect on investment in marital capital. When the sample is narrowed further to those married seven years or fewer, the number of treatment states shrinks and the precision of the estimates falls substantially.

The third set of regressions (Table 3b) considers the percent of married life spent under a unilateral divorce regime for those married under a consent divorce regime. Dummy variables are constructed indicating 1 to 24 percent, 25 to 49 percent, 50 to 74 percent, 75 to 99 percent, and 100 percent spent in a state allowing for unilateral divorce. Thus the following regression is run:

$$Own\ home_{i,s,t} = a + \sum_p b_p\ unilateral_{s,t} + l Census_t + \sum_s h_s state + \sum_t g_t Years\ Married + X_{i,s,t} j + e_{s,t} \qquad (3)$$

where *p* represents the percentile groups of unilateral divorce. The results of this regression run for all married couples are reported in the first column of Table 3b. Subsequent columns report the results for couples married 15 years or fewer, 12 years or fewer, and 7 years or fewer. When the sample includes all couples, there is a statistically significant decrease in home ownership rates of about one percentage point.

[12] Seven states changed their law between 1965 and 1980 to allow for unilateral divorce conditional on a one-year separation.

There is no discernable relationship between the pattern of coefficients and the length of exposure to a unilateral divorce regime. Restricting the sample to the various marriage lengths provides little additional insight.

4. Investment in Household Specialization

Specialization within the family generally means that one person in a marriage specializes in the market sector and the other person specializes in the non-market sector. These specialized skills are highly complementary within a marriage, but less useful when single. Although market- or non-market skills may be transferable to another marriage, they will go under-utilized during any period that either partner is single. Alternatively, if spouses cannot commit to sharing future rents from skill formation, then each will be less willing to invest in the skills of the other. Therefore, an increase in the expectation of divorce should lead to more two-earner couples (more equitable investment in both market and nonmarket skills).

As with home ownership, I run three regressions to determine the effect of unilateral divorce on hours worked, following the procedures outlined above. The top panel of Table 4a shows the effects of divorce laws on the working hours of newlyweds. The effect on male hours is negligible and statistically insignificant. The estimate for women suggests an increase of a half-hour of work per week for women newly married in unilateral divorce states.

The second group considered is those married just before unilateral divorce becomes law. The bottom panel of Table 4a shows that for those married 15 or fewer or 12 or fewer years, there is no discernable effect on either male or female hours worked. However, when the sample is narrowed to marriages lasting 7 years or fewer, there is a statistically significant increase in women's hours worked and a similar, statistically significant decrease in male hours – despite the lack of precision arising from the small sample. To examine this further I use the third regression strategy, which considers all couples and looks at the fraction of the marriage that was spent under unilateral divorce laws. Table 4b shows that women who have spent some of their marriage in unilateral divorce states tend to work more hours than do those who have spent none of their marriage in unilateral divorce states. Similarly, men who have been

exposed to unilateral divorce work fewer hours than their counterparts in non-unilateral states, although, as with the home ownership results, the pattern of coefficients suggests that the length of exposure is less relevant.

5. Investment in Children

According to Becker "the most obvious and dominant example of marriage-specific investment is children"[13]. Children are produced in households by husbands and wives investing time and resources in them. One aspect of the return on children is the love, attention, and pride that they give their parents. The ability to extract these returns diminishes upon divorce because parents, particularly the non-custodial parent, spend less time with their children. Furthermore, children may be a hindrance to remarriage and an unpleasant reminder of the first marriage. Accordingly when the contractual bonds of marriage are weakened, couples may choose to reduce either the total number of children conceived in the marriage or investment in the children they do have.

The census identifies the total number of children ever born for women and the total number of children in the home for both women and men. In order to use a metric that is comparable for men and women, the regressions on children will look at the number of children in the home. However, since the couples examined have all been married 15 years or fewer, the two measures are similar. In fact, running the regressions for women using the data from children ever born provides estimates that are consistent with those generated using the data on children in the home.

As in the previous sections, I start by focusing on newlyweds, and for this group it only really makes sense to consider whether the couple has had at least one child. I run a linear probability model and find large negative effects (shown in Table 5a), with unilateral divorce laws reducing early fertility by 7 to 8 percentage points. Although this suggests a fertility decline of about 20 percent, it is likely that most of this decline represents a postponement of children rather than a true decline in the expected probability of ever having one child.

[13] Becker, 1974. p. S23

The second regression returns to examining those married in the two years prior to the law change. Recall that using this sample eliminates the selection bias resulting from selection into marriage, but is contaminated by selection bias out of marriage. The form of bias present is particularly important when considering children, as the timing with children is less flexible than other investments and much less reversible. For this group I consider three measures of the effect of unilateral divorce on family size. The dependent variables are an indicator of whether or not the couple has at least one child, an indicator for at least two children, and an indicator for at least three children. The effects on having children are somewhat uninformative: Since the treatment group considered here consists of those who were married one or two years *before* the law changed, it is most likely that they already had their first child at the time unilateral divorce became the law. However, after the law changes, married couples have to decide whether or not to separate as a result of the eased divorce regulations. If unilateral divorce increases the divorce probability of those without kids relative to those with kids, then the estimated effect of unilateral divorce will be an *increase* in the probability of having kids. Indeed, looking at the probability of having one child for those married 15 years or fewer (Table 5a), there is a statistically significant increase of 1.5 percentage points in the probability of having at least one child. When the sample is narrowed to those with 12 years of marriage or fewer, this estimated coefficient increases to three percentage points. These positive coefficients are likely to be reflecting the different tendencies to select out of marriage for those with and without kids.

As these couples have only been married one or two years at the time the law changes, they are much less likely to have already had two or three children by then. However, the selection bias arising from those selecting out of marriage will still exist. Looking at the probability of two or more children for couples married 15 years or fewer, we see that the coefficient is negative, but not statistically significant. However, the estimated coefficient on having three or more children is large and statistically significant. For couples married 15 years or fewer, unilateral divorce makes them 4 percentage points less likely to have three or more children. The estimated coefficient increases to 5 percentage points when the sample is

narrowed to those married 12 years or fewer. These estimates suggest either large changes in childbearing, or differential effects of divorce laws on marital dissolution by family size.

The third set of regressions, reported in Table 5b, considers all marriages, using indicator variables for the amount of the marriage that was spent under a unilateral divorce regime. These results are broadly in line with the previous table, finding small or positive effects on the probability of having any children, but large negative effects on the likelihood of having larger families the longer that one is exposed to unilateral divorce. The treatment effects are closer to constant, than linear in exposure. However, considering couples married in a unilateral divorce state (100 percent) there is a statistically significant increase of about 2 percentage points. This most likely reflects the bias arising from selection *into* marriage.

6. Conclusion

Clearly there is more to divorce regulation than simply the effect on those who choose to divorce. This paper has demonstrated that investment in marriage-specific capital is affected by the legal regime in place. People invest in their marriages to the extent that they expect them to stay intact, or the extent to which their partners can credibly commit to sharing the fruits of such investments. There is evidence that couples are less likely to invest in a major public good – their own homes. The effects on the quantity of children are complicated due to various forms of selection bias, but show an unambiguous decrease in the probability of having three or more children for couples who are exposed to unilateral divorce regulation after they marry. Finally, young couples appear to delay having their first child, perhaps because they are waiting to see if their marriage will succeed before making such an irreversible decision.

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

	Unilateral		Unilateral with Separation Requirement					Unilateral		Unilateral with Separation Requirement			
			1 Year	1.5-2 years	3 years	5 or more				1 Year	1.5-2 years	3 years	5 or more
Alabama	1971						Montana	1973	*				
Alaska	1935						Nebraska	1972					
Arizona	1973					1931	Nevada	1967	*			1939	1931
Arkansas		*		1991	1937		New Hampshire	1971					
California	1970						New Jersey				1971		1907
Colorado	1972	*					New Mexico	1933	*				
Connecticut	1973						New York						
Delaware	1968	*					North Carolina			1965	1933		
DC			1977				North Dakota	1971					
Florida	1971						Ohio			1982	1974		
Georgia	1973						Oklahoma	1953					
Hawaii	1972	*		1965			Oregon	1971	*				
Idaho	1971					1945	Pennsylvania				1988	1980	
Illinois				1984			Rhode Island	1975	*				1910
Indiana	1973						South Carolina			1979		1969	
Iowa	1970						South Dakota	1985					
Kansas	1969						Tennessee		*		1963		
Kentucky	1972					1916	Texas	1970	*			1967	1941
Louisiana			1979	1938			Utah	1987				1943	
Maine	1973						Vermont		*	1972	1971	1969	
Maryland		*		1983	1973	1969	Virginia			1975	1964	1960	
Massachusetts	1975						Washington	1973			1965		1921
Michigan	1972						West Virginia		*	1977	1969		
Minnesota	1974					1933	Wisconsin	1978	*				
Mississippi							Wyoming	1977					
Missouri		*		1974									

Source: Gruber (2000)

* Indicates that coding disagrees with Friedberg (1998)

Table 2 Summary Statistics

	Women		Men	
	1970	**1980**	**1970**	**1980**
Married	60.4%	55.3%	66.3%	60.9%
Separated	2.3%	2.6%	1.5%	1.9%
Divorced	4.0%	7.1%	2.8%	5.2%
Never Married	20.5%	22.8%	26.3%	29.5
In First Marriage	52.1%	46.5%	56.9%	50.7%
Newlyweds	9.5%	9.0%	9.4%	9.0%
Of those in First Marriages				
Age at First Marriage	21.6	21.6	24.4	24.2
	(5.4)	(5.1)	(6.0)	(5.6)
Number of years Married	20.2	21.2	20.2	21.2
	(14.2)	(15.0)	(14.2)	(14.9)
Own Home	70.6%	78.2%	70.8%	78.3%
Hours Worked	12.4	15.6	34.8	33.7
	(18.0)	(19.0)	(20.1)	(21.3)
Children ever born	2.4	2.3		
	(2.0)	(1.8)		
At least 2 children in house	43.3%	38.7%	43.4%	38.6%
(Nos. in brackets are children ever born)	(66.4%)	(65.7%)		
At least 3 children in house	23.5%	16.4%	23.6%	16.5%
(Nos. in brackets are children ever born)	(40.9%)	(37.5%)		
Newlyweds In First Marriages				
Age of Marriage	21.4	22.3	23.8	24.3
	(6.4)	(5.6)	(6.4)	(6.2)
Own Home	30.0%	40.9%	28.4%	40.7%
Hours Worked	16.7	21.4	32.3	36.8
	(19.1)	(19.6)	(19.8)	(18.2)
At least one child	37.9%	34%	38.7%	35.5%
At least one child ever born	40.2%	36%		
Couples Married 1 or 2 years before the law changes and the control groups				
All (Married 15 years or less)				
Own Home	56.7	73.6	57.0	73.8
Hours Worked	12.3	16.9	38.5	40.3
	(17.7)	(19.0)	(17.2)	(16.4)
At least one child	77.0	82.1	77.0	81.8
At least two children	54.8	57.5	55.1	57.7
Married 12 years or less				
Own home	53.3	71.6	53.7	71.9
Hours Worked	12.4	16.9	38.1	40.1
	(17.7)	(19.0)	(17.3)	(16.5)
At least one child	74.6	80.1	74.5	79.8
At least two children	50.1	53.2	50.7	53.7
Married 7 years or less				
Own home	42.6	63.3	42.7	63.6
Hours Worked	13.7	17.9	36.6	39.3
	(18.3)	(19.3)	(17.8)	(16.9)
At least one child	63.9	69.9	63.9	69.9
At least two children	31.7	34.6	32.6	35.6

*Actual state in which marriage occurred is unknown; Coded to current state of residence.

Table 3a
DIVORCE REGIME AND HOME OWNERSHIP

$$(Own\ Home)_{i,s} = a + b\,Unilateral + \sum_s h_s State_s + \sum_t c_t Year\ Married_t + X_{i,s} l + e_{s,t}$$

	Unilateral is a dummy variable indicating unrestricted unilateral divorce			Unilateral divorce is a dummy variable indicating unilateral divorce with a one year or less separation required		
	Women	Men	Combined	Women	Men	Combined
	(1)	(2)	(3)	(4)	(5)	(6)
Newlyweds	-.008	-.006	-.007	-.013*	-.011*	-.012*
	(.007)	(.006)	(.006)	(.007)	(.007)	(.007)
Couples Married 1or 2 years before the law changes						
Married 15 years or less (All law changes)	-.021***	-.023***	-.022***	-.014*	-.017**	-.016**
	(.007)	(.007)	(.006)	(.008)	(.008)	(.008)
Married 12 years or less	-.026***	-.028***	-.027***	-.020***	-.023***	-.021***
	(.008)	(.008)	(.009)	(.008)	(.009)	(.008)
Married 7 years or less	-.009	-.009	-.009	-.004	-.006	-.005
	(.020)	(.022)	(.021)	(.012)	(.012)	(.012)

***, **, and * indicate statistically discernible from zero at the 1%, 5% and 10% levels, respectively.
Source: 1970 and 1980 Censuses of Population, IPUMS, (Ruggles and Sobek 1997).

Notes: Standard errors are clustered at the level of state*year of census cells. All regressions include a saturated set of dummy variables for state of residence, year of census, years of marriage, region , and metropolitan status. Combined regressions control for gender.

Table 3b
DIVORCE REGIME AND HOME OWNERSHIP

Percent of Marriage Spent under Unilateral Divorce Regime	All Marriages		Married 15 years or less		Married 12 years or less		Married 7 years or less	
	Women	Men	Women	Men	Women	Men	Women	Men
1 to <25 percent	-.022*** (.003)	-.021*** (.003)	-.009 (.009)	-.005 (.008)	-.005 (.011)	-.002 (.010)	-.018 (.021)	-.007 (.017)
25 to < 50 percent	-.007*** (.003)	-.009*** (.003)	-.005 (.006)	-.010 (.007)	.010 (.008)	-.005 (.009)	.025** (.011)	.017 (.014)
50 to <75 percent	-.007** (.002)	-.008*** (.003)	-.022*** (.005)	-.025*** (.005)	-.019*** (.006)	-.020*** (.007)	.016 (.010)	.006 (.009)
75 to < 100 percent	-.011*** (.004)	-.009** (.004)	-.015*** (.005)	-.016*** (.005)	-.015*** (.006)	-.017*** (.006)	.005 (.011)	-.008 (.010)
100 percent	-.016*** (.004)	-.016*** (.004)	-.012*** (.003)	-.012*** (.003)	-.012*** (.004)	-.013*** (.004)	-.011*** (.004)	-.012*** (.004)

***, **, and * indicate statistically discernible fro m zero at the 1%, 5% and 10% levels, respectively.
Source: 1970 and 1980 Censuses of Population, IPUMS, (Ruggles and Sobek 1997).

Notes: Standard errors are clustered at the level of state*year of marriage cells. All regressions include a saturated set of dummy variables for state of residence, year of census, years of marriage, age, race, region , and metropolitan status.

Table 4a
DIVORCE REGIME AND HOURS WORKED

	Unilateral is a dummy variable indicating unrestricted unilateral divorce	
	Women	Men
	(1)	(2)
Newlyweds	.489*	-.008
	(.264)	(.047)
Couples Married 1-2 years before the law changes		
Married 15 years or less	.214	.244
(All law changes)	(.215)	(.316)
Married 12 years or less	.015	.178
	(.254)	(.326)
Married 7 years or less	.948***	1.21*
	(.327)	(.711)

***, **, and * indicate statistically discernible from zero at the 1%, 5% and 10% levels, respectively.
Source: 1970 and 1980 Censuses of Population, IPUMS, (Ruggles and Sobek 1997).

Notes: Standard errors are clustered at the level of state*year of marriage cells. All regressions include a saturated set of dummy variables for state of residence, year of marriage, race, region , and urban status. Combined regressions control for gender.

Table 4b
DIVORCE REGIME AND HOURS WORKED

Percent of Marriage Spent under Unilateral Divorce Regime	Married 15 years or less		Married 12 years or less		Married 7 years or less	
	Women	Men	Women	Men	Women	Men
1 to <25 percent	.004 (.035)	-.515** (.269)	.011 (.008)	-.621** (.311)	-.021 (.094)	-.227 (.564)
25 to < 50 percent	.029 (.029)	-.666*** (.258)	.071 (.007)	-1.22*** (.339)	-.034 (.056)	-2.01*** (.607)
50 to <75 percent	.039** (.019)	-.278 (.231)	.017 (.005)	-.370 (.270)	-.012 (.068)	-.699 (.524)
75 to < 100 percent	.063*** (.020)	-.383 (.263)	.070 (.006)	-.383* (.263)	-.159*** (.060)	-.675* (.388)
100 percent	.027* (.016)	-.181 (.261)	-.002 (.005)	-.181 (.261)	.040* (.022)	.294 (.351)

***, **, and * indicate statistically discernible from zero at the 1%, 5% and 10% levels, respectively.
Source: 1970 and 1980 Censuses of Population, IPUMS, (Ruggles and Sobek 1997).

Notes: Standard errors are clustered at the level of state*year of marriage cells. All regressions include a saturated set of dummy variables for state of residence, year of census, years of marriage, age, race, region , and metropolitan status.

Table 5a
DIVORCE REGIME AND NUMBER OF CHILDREN

	Unilateral is a dummy variable indicating unrestricted unilateral divorce		Unilateral divorce is a dummy variable indicating unilateral divorce with a one year or less separation required	
	Women (1)	Men (2)	Women (3)	Men (4)
Newlyweds				
At least one child in the household	-.076*** (.031)	-.065** (.030)	-.085*** (.031)	-.071** (.030)
At least one child ever born	-.075*** (.030)		-.084*** (.030)	
Couples Married 1 or 2 years before the law changes				
Married 15 years or less (All law changes)				
At least one child in the household	.015** (.007)	.018*** (.007)	.015** (.007)	.018*** (.007)
Two or more children	-.010 (.009)	-.011 (.008)	-.010 (.009)	-.011 (.008)
Three or more children	-.043*** (.010)	-.040*** (.009)	-.043*** (.010)	-.040*** (.009)
Married 12 years or less				
At least one child in the household	.030*** (.008)	.032*** (.007)	.030*** (.008)	.032*** (.007)
Two or more children	.005 (.010)	.003 (.009)	.005 (.010)	.003 (.009)
Three or more children	-.052*** (.010)	-.050*** (.010)	-.052*** (.010)	-.050*** (.010)
Married 7 years or less				
At least one child in the household	.004 (.010)	.017* (.008)	.004 (.009)	.017* (.008)
Two or more children	-.023 (.025)	-.019 (.025)	-.023 (.025)	-.019 (.025)
Three or more children	-.034* (.018)	-.031 (.020)	-.034* (.019)	-.031 (.020)

***, **, and * indicate statistically discernible from zero at the 1%, 5% and 10% levels, respectively.
Source: 1970 and 1980 Censuses of Population, IPUMS, (Ruggles and Sobek 1997).

Notes: Standard errors are clustered at the level of state*year of census cells. All regressions include a saturated set of dummy variables for state of residence, year of marriage, race, region , and metropolitan status.

Table 5b
DIVORCE REGIME AND NUMBER OF CHILDREN

Percent of Marriage Spent under Unilateral Divorce Regime	At Least One Child		At Least Two Children		At Least Three Children	
	Women (1)	Men (2)	Women (3)	Men (4)	Women (5)	Men (6)
Married 15 Years or less						
1 to <25 percent	.005 (.005)	.007 (.005)	.006 (.008)	.009 (.008)	-.028*** (.011)	-.028*** (.010)
25 to < 50 percent	.021*** (.006)	.015*** (.006)	.009 (.007)	.007 (.008)	-.061*** (.008)	-.060*** (.007)
50 to <75 percent	.022*** (.003)	.021*** (.004)	.006 (.005)	.007 (.005)	-.073*** (.006)	-.070*** (.005)
75 to < 100 percent	.013*** (.004)	.011*** (.004)	.008 (.006)	-.010* (.005)	-.061*** (.006)	-.060*** (.005)
100 percent	-.011*** (.003)	-.011*** (.003)	-.002 (.005)	-.004 (.004)	.018*** (.005)	.016*** (.005)
Married 12 Years or less						
1 to <25 percent	-.001 (.007)	.001 (.006)	-.003 (.010)	-.000 (.010)	-.029** (.014)	-.028** (.012)
25 to < 50 percent	.005 (.009)	-.008 (.008)	-.024** (.010)	-.027*** (.010)	-.057*** (.014)	-.059*** (.012)
50 to <75 percent	.018*** (.005)	.017*** (.005)	-.008 (.007)	-.004 (.007)	-.082*** (.008)	-.077*** (.007)
75 to < 100 percent	.015*** (.005)	.012*** (.004)	-.009 (.006)	-.012** (.006)	-.064*** (.006)	-.065*** (.005)
100 percent	-.006** (.003)	-.008** (.003)	.003 (.005)	-.000 (.005)	.015*** (.005)	.011** (.005)

***, **, and * indicate statistically discernible from zero at the 1%, 5% and 10% levels, respectively.
Source: 1970 and 1980 Censuses of Population, IPUMS, (Ruggles and Sobek 1997).

Notes: Standard errors are clustered at the level of state*year of marriage cells. All regressions include a saturated set of dummy variables for state of residence, year of census, years of marriage, age, race, region , and metropolitan status.