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Empirical Essays on Women and Families

presented by

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*Date* May 23, 2001

# Empirical Essays on Women and Families

A thesis presented

by

Betsey Ayer Stevenson

to

The Economics Department

in partial fulfillment of the requirements
for the degree of
Doctor of Philosophy
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Empirical Essays on Women and Families

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## Abstract

This dissertation examines the effects on women and families of two major policy changes made during the 1970s. Chapter 1 examines Title IX of the Educational Amendments of 1972 finding that it led to a rapid increase in female athletic participation rates. Because Title IX required states to increase female athletic participation rates to approximately the same level as male rates, variation in the pre-Title IX male participation rates provides a useful instrument for examining the effects of the change in girls' athletic participation. I find that extending athletic opportunities to high school girls generates between 0.2 and 0.3 more years of educational attainment and more than a 10-percentage point increase in the probability of being employed.

Chapters two and three examine the effects of unilateral divorce laws. 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. While unilateral divorce laws have only small effects on marriage-market allocation, chapter two shows that there were profound effects on distribution. Suicide rates provide a quantifiable measure of well-being, and we find that female suicide rates fell by about a fifth when states liberalized access to divorce. Domestic violence against women declined by about a third, and

intimate homicide rates declined by a tenth, suggesting that these laws improved outcomes for women.

Chapter three examines the effects of unilateral divorce on the incentives to invest in marriage. Home ownership rates are used to examine the impact on joint investment in public goods within the marriage. I find a statistically significant decrease in home ownership rates of 2 to 3 percentage points. Investment in children is examined at the quantity margin. Newlyweds are found to be 7-8 percent less likely to have at least one child. A decrease in the probability of having more than three children is found for couples married 15 years or less. Investment in home-making skills has apparently decreased as hours in the labor force increases for women and decreases for men. In sum, chapters two and three show that legal institutions may have profound effects on outcomes within families.

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To
Robert Ayer Stevenson
1918-2000

# INTRODUCTION

Dramatic changes have occurred to American women and families during the past thirty years. While societal attitudes were shifting the government was also changing policies that had the potential to have a profound impact on women and families. The myriad of simultaneous events affecting women and children have made it difficult to accurately and completely analyze the impact of policies that occurred during this period. This dissertation examines two of policies from the 1970s–Title IX of the Educational Amendments of 1972 and "No-Fault" divorce laws – developing identification strategies to effectively isolate the impact of the policy on the outcomes of women.

Chapter 1 examines the effects of Title IX of the 1972 Educational Amendments to the 1964 Civil Rights Act which stated

> *No person in the United States shall, on the basis of sex, be excluded from participation in, be denied the benefits of, or be subjected to discrimination under any educational program or activity receiving financial assistance.*

This legislation passed one year after a Connecticut judge ruled that a school could legitimately provide a cross-country team only for male students. The judge, denying the female plaintiff's request to be allowed to try out for the team, stated "Athletic competition builds character in our boys. We do not need that kind of character in our girls."[1] Although attitudes about girls and sports had undoubtedly changed over the previous 20 years there was still extreme prejudice towards female athletics. Title IX banned such explicit discrimination against female athletes and forced states to provide the same opportunities to female students as were provided to male students.

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[1] *Title IX: 25 Years of Progress*: U.S. Department of Education.

Although the U.S. Department of Educations report on Title IX 25 years after its passage claimed that Title IX was a roaring success by showing the dramatic rise in female sports participation between 1972 and 1997, there has been no definitive study showing that Title IX *caused* the large increase in female sports participation. Chapter 1 examines the causal effect of Title IX on female athletic participation in two ways. First, the national data is examined providing strongly suggestive evidence that Title IX caused the rise in female sports participation. This evidence comes not from the dramatic rise between 1972 and 1997, but from that which occurred between 1972 – when the law was passed- and 1978-by when schools were required to be in compliance with Title IX. In fact, it is the lack of an increase between 1978 and 1992 that lends support to the argument that the rise in female sports participation was not part of a general social trend in female equality, but was the reaction to a change in legislative policy. This is further supported by the small rise that occurs after the Supreme Court ruled in 1992 that plaintiffs filing Title IX lawsuits may receive punitive damages.

The second piece of evidence comes from looking at male athletic participation rates over the same period both at the national and state level. At the national level, in contrast to the large, and discontinuous, jump that occurred in national female high school sports participation, there was no such change in male participation rates. The second piece of evidence comes from the enormous variation in the state male high school participation rates. Title IX required states to provide the same opportunities to boys that were provided to girls *whatever* those opportunities may be. In other words, a state with no male participation did not need any female participation. This variation at the state level provides not only a second way of assessing the impact of Title IX on female sports participation, but it generates a unique identification strategy for measuring the effects of high school sports participation on later outcomes.

Chapter 1 shows that the pre-Title IX rate of male athletic participation predicts the post-Title IX rate of female participation. As a result, the pre-Title IX rate of male participation is used as an instrumental variable to measure the causal benefits of participating in high school sports on the educational attainment and labor force participation of women. The experimental design used in chapter 1 allows me to isolate the treatment effects of sports participation (*caused* by participation). This is a novel contribution in that much of the existing research on the effects of high school sports participation has focused on sorting out the possible mediating mechanisms instead of dealing with the fact that students are not randomly assigned to participation in sports.

In sum chapter one finds that, despite being part of a sweeping trend of extending equal opportunities to girls in education, Title IX caused a dramatic increase in female high school sports participation and that the increased opportunities for girls to play sports led to higher educational attainments and labor force participation. Although it is undoubtedly true that more girls would be playing sports today than were playing in sports in 1972, it is clear that the increase would have been neither as large nor as rapid as that which occurred following Title IX.

Chapters two and three turn to what is colloquially known as the "no fault" revolution which swept the United States through the 1970s. The legal changes that arose dramatically changed the parameters of family law moving from a fault-based system to one of divorce on demand by either spouse. Despite the large amount of the population for whom this legal change may potentially impact, the true, long-run, effects of this change have yet to be fully understood. The public and economists have focused on the potential relationship between the legal changes and the concurrent rise divorce rates. However, the potential impact of unilateral divorce is clearly wider-ranging than simply that which it may have on divorce. Bargaining models of marriage argue that allocation within families is determined by threat points of which the ability to exit is one. Changing the parameters regarding one's ability to exit a marriage could potentially affect all marriages – not simply the ones that are at risk of terminating.

Chapter two is joint work with Justin Wolfers in which we attempt to evaluate unilateral divorce laws in terms of their affect on adult well-being. We pursue an identification strategy similar to that used in chapter one. Variation across states and through time is used to identify the potentially causal relationship between unilateral divorce and suicide, domestic violence, and spousal homicide. We find a decrease in female suicide, a decrease in domestic violence, and a decrease in the homicide of women by intimates. We believe that these findings reflect the fact that unilateral divorce laws make it easier for someone to leave a particularly bad situation.

Chapter three continues along the same vein as chapter two, evaluating the effects of unilateral divorce on marriage-specific investment. Theory states unambiguously that a rise in the divorce rate will cause a decrease in investment in marriage-specific capital. Furthermore, a change in when divorces occur (early versus later) in the marriage will affect investment propensities. Furthermore, bargaining models show that asymmetries will lead to changes in investment desire when the outside option is strengthened. Thus, there is good reason to think that the stock of marital capital should have been changed by the "no-fault" revolution.

Investment in homes, children, and home-making are investigated in chapter three. Home-making has become less fashionable in recent decades with a surge in married women's employment. However, chapter three is looking at the specific relationship between home-making an unilateral divorce laws. Examining marriages of various lengths provides some suggestive evidence of both an increase in female labor market hours and a decrease in male labor market hours stemming from the change in divorce laws. Investment in homes, a public good in a marriage, is easier to evaluate and unilateral divorce is shown to lead to an unambiguous decrease in home ownership rates The quantity of children is the trickiest investment at which to look, both because parents investment in quality as well as quantity, and because the quantity investment is largely irreversible. There is some evidence that smaller families have arisen as a result of unilateral divorce and that young couples post-pone having their first child.

In sum, this dissertation examines two pieces of public legislation and finds that they have profoundly impacted the lives of women in both their professional and family lives.

# Chapter 1
# Evidence on the Effects of Sports Participation
# Examining the Impact of Title IX

Abstract

Cross-sectional evidence suggests that high school athletes experience better outcomes than non-athletes, including higher educational attainment, more employment, and higher wages. Students self-select into athletics, however, so these may be selection effects rather than causal effects. This paper uses credibly exogenous variation in athletic participation caused by Title IX, federal legislation that led to dramatic increases in the number of American girls participating in high school sports. Between 1972 and 1978 U.S. high schools rapidly increased their female athletic participation rates (to approximately the same level as their male athletic participation rates) in order to comply with Title IX. This paper uses variation in the level of boys' athletic participation across states before Title IX as an instrument for the change in girls' athletic participation over the 1970s. I find that participation in high school sports generates between 0.2 and 0.3 more years of educational attainment and more than a 10-percentage point increase in the probability of being employed.

## 1. Introduction

Sports appear to be good for children. Ask any parent or teacher of a high school athlete and you are likely to hear an enthusiastic listing of the benefits of sports. Simple correlations indicate that children who participate in sports have better outcomes than those who do not. Data from the 1997 National Youth Risk Behavior Survey indicate that adolescents who play a sport are less likely to drink, smoke, use drugs, have sex, or have suicidal thoughts.[2] A recent medical study analyzing these data concluded, "the… positive relationships between sports participation and health behaviors suggest that physicians should actively encourage young people to… join sports teams."[3] At its heart, this recommendation is based on the assumption that those who participate in sports are *changed*, that is, that sports participation itself actually improves outcomes.

In 1961 James Coleman argued in *The Adolescent Society* that athletics were consuming too much attention in American high schools, shifting the focus away from the main mission of the schools. In the ensuing years researchers have tried to resolve the controversy over the costs and benefits of high school athletics. Many studies have found a positive relationship between participation in high school athletics and educational aspirations, educational attainment, and wages later in life.[4]

What remains elusive is whether such benefits are treatment effects (*caused* by participation) or merely selection effects (associated with the type of student who chooses to participate in athletics). Much of the existing research has focused on sorting out the possible mediating mechanisms instead of dealing with the fact that students are not randomly assigned to participation in sports. Athletes tend to be more extroverted, aggressive, and achievement oriented. Are these traits they bring to athletics or are these

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[2] Regressions using this data suggested that Caucasian adolescent girls who played on at least one sports team were 20 percent less likely to engage in sexual intercourse, 50 percent less likely to have had a pregnancy, one-third less likely to smoke, 20 percent less likely to use drugs, and 5 more likely to have never had an alcoholic drink, than those who were not on a sports team. Furthermore, the same regressions run comparing girls who reported participating in intensive exercise, but were not on a sports team with those who did not exercise, showed no significant relationships between exercise and behavioral outcomes for these, non-team, athletes.

[3] Pate *et al.* (2000).

[4] Rehberg and Schafer (1968); Spreitzer and Pugh (1973); Picou and Curry (1978); Hanks (1979); Long and Caudill (1991); Barron, Ewing, and Waddell (2000).

traits athletics brings to them? Are they learning skills for which they later earn higher wages? Or are the high skilled simply more likely to participate in sports?

To measure the causal benefits of participating in high school sports, one would want to randomly assign students to participation, or randomly assign different levels of athletic opportunities to different schools. Neither of these policy experiments exists, but there does exist a natural experiment that mimics the second policy experiment, at least for girls. In 1972 Congress enacted Title IX of the Educational Amendments, legislation that banned gender discrimination in federally-funded educational institutions. Compliance with Title IX can be characterized as requiring a school to raise its female athletic participation rate to near equality with its male athletic participation rate.[5] As a result, U.S. female high school athletic participation rose dramatically from 1 in 27 females in 1972 to 1 in 4 in 1978. In contrast, male participation remained relatively constant at 1 in 2. Although Title IX applied to every state, at the time of its passage there was considerable variation in male sports participation rates across states. (Female sports participation rates also varied, but were low everywhere. Most of the variation in the scale of the compliance problem, therefore, came from male participation.) In short, some states needed much larger increases in female sports participation than others. This paper uses the variation in states' mandated increases as a credibly exogenous source of variation in states' actual participation changes. As such, I identify credibly causal effects of athletic participation.

This paper focuses on the effects of *female* sports participation for two reasons. First, the experiment just described favors a focus on girls. Second, the benefits of sports

[5] Although the rules about compliance are in reality more complicated, a reasonable approximation is that compliance requires the female participation rate to equal the male participation rate.

participation are generally thought to be different for women than for men. Scholars have argued that since athletic participation is much less socially acceptable for females than it is for males, it is less important as a status-conferring mechanism.[6] While this may be true, status among one's peers is not the sole mediating mechanism by which athletic participation may confer benefits. Alternatively, women may gain interpersonal skills, confidence, work habits, or networks from sports that are useful in college and the labor market. Even if the effects of sports on individual females' traits were small, female athletic participation could improve women's outcomes by contributing to changing social norms about gender roles and perceptions about the perceived relative strengths of men and women.

The outline of the paper is as follows: I describe the likely motivations for playing sports in order to clarify the nature of the selection problem. I then use cross-sectional regressions to illustrate the association between participation in high school athletics and educational attainment and wages. Next, I discuss Title IX and the specific instrumental variables procedure that it generates. Using data from the Census of Population, I generate estimates of the effects of athletic participation on educational attainment and employment status that are credibly causal.[7]

## 2. Why Participate in Sports?

Broadly, there are two groups of potential mediating mechanisms through which athletics may influence academic and career outcomes. The first relies on the actual

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[6] Hanks (1979), Hanks and Eckland (1976), and Snyder and Spreitzer (1977).

[7] Extensions of this work will examine data from the (college) Freshman Survey tabulated by the Cooperative Institutional Research Program.

playing of a sport. Athletics is a highly regulated system in which social conflict is displayed in a positive light. From this, players learn how to compete. Participants also learn how to operate successfully under a formal code of rules and procedures. Furthermore, players are taught to function as a team. The development of these skills could be especially important for girls who must try to maneuver their way through traditionally male occupations later in life. Further, sports participation has direct physiological benefits that may be rewarded in the labor market.[8]

The second group of mediating mechanisms includes things that may occur because of athletic participation, but are not necessarily required by, or unique to, athletic participation. First, athletes may receive increased attention and encouragement from teachers, counselors, and other adults. Second, athletes may have larger, or more useful, social networks than non-athletes. Finally, because athletes often gain visibility amongst their peers, their self-esteem may rise and they may feel increased peer pressure to succeed.

In Gary Becker's seminal work on human capital he acknowledged the difficulty in conceptualizing ability. Conventional measures of ability, Becker argued, "while undoubtedly relevant at times, do not reliably measure the talents required to succeed in the economic sphere."[9] In other words, ability is multi-faceted. While some intellectual and academic abilities can be measured with standard IQ tests, other abilities are less easily measured by conventional tests. These attributes include the ability to

[8]For evidence on the relation between physiological characteristics and earnings see Averett and Sanders (1993) or Hamermesh and Biddle (1994).

[9] Becker, 1993 p. 97.

communicate, the ability to work well with others, competitiveness, assertiveness, and discipline.

Partitioning talent into these two components can help clarify how high school students choose among extra-curricular activities. Consider a high school student who has to decide how to allocate out-of-school time. Those possessed with an aptitude for academics may find reading or studying the most beneficial activity; those with strong motivation and aptitude in the interpersonal domain may find athletics most beneficial, and those with low ability in several domains may prefer activities such as watching television. Even if none of these activities generates human capital, they may generate private benefits to students because they signal (otherwise unobservable) abilities to employers and colleges. (A straightforward extension of a standard Spence-style signaling model to multiple abilities could generate such a separating equilibrium.) Note that even if abilities *were* observable to employers and colleges (though not to econometricians), a cross-sectional correlation between extra-curricular activities and outcomes would still be observed, so long as students with particular abilities enjoyed disproportionate (consumption) benefits from participating.[10]

In sum, athletes may earn more by signaling that they are motivated and competitive. Alternatively, a cross-sectional correlation may simply reflect unobserved background variables. Or athletics may foster the development of skills that increase productivity. These productivity-enhancing effects will be observed both in cross-

[10] Under the omitted variables interpretation, changes in the aggregate levels of athletic activity in a state yield no effect on wages within that state. The signaling model is subtler. Conceptualizing Title IX as a reduction in the cost of playing sport for women can yield predictions of either more or less efficient sorting of people of unknown abilities across jobs.

sectional data, and in the evolution through time of athletic participation rates and outcome measures in aggregate populations.

## 3. Association of Athletics with Educational and Labor Market Outcomes in Cross-Sectional Data

To better understand the association between sports participation and outcomes, it is instructive to analyze a cross-section of high school students. The National Longitudinal Survey of Youth (NLSY) is a nationally representative sample of 12,686 men and women aged 14 to 22 in 1979. In 1984, the respondents were asked retrospectively about their participation in a variety of high school extra-curricular activities, which can be grouped into three broad categories: vocational clubs, athletics, and other clubs.[11]

The relationship between sports participation and educational attainment is shown in Table 1.1.[12] Controlling for age, race, parent's education, and the urban status of the area in which the individual attended high school, being an athlete is associated with 0.6 of a year more schooling for girls (column 1). When controls are added for the state of residence at age 14, AFQT scores, and membership in the National Honor Society, the association is still statistically significant at the 1% level, but is reduced to approximately 0.4 of a year of schooling (column 3), still an educationally meaningful effect.

[11]There are many clubs for which the NLSY identifies as vocational. The remaining "other clubs" include student government, newspaper, yearbook, and other, primarily hobby, clubs.

[12]The sample is restricted to those who completed at least the $10^{th}$ grade, as attending some high school is necessary in order to participate in high school sports.

The association between athletic participation and educational attainment is similar for boys with male athletes achieving 0.8 years more schooling than non-athletes (Column 6). When controls for state of residence, AFQT scores, and membership in the National Honor Society are added (column 8) the magnitude is reduced to approximately 0.4 years. Although some previous studies have found no association between females' athletic participation and their educational attainment, the NLSY data indicate that the association is similar for males and females. This may reflect the fact that most of the girls in the NLSY data attended high school after Title IX was in effect, so that their participation tendencies were more like those of boys, whereas previous studies have largely looked at pre-Title IX sports participation.[13]

Previous literature has emphasized the effects of *athletic* participation on outcomes, assuming that athletics are more influential than other extracurricular activities in high school. Adding an indicator variable for an individual having participated any non-athletic club does not change the magnitude of the coefficient on athletics much, but the coefficient on the indicator variable itself is similar in magnitude to that on the indicator for athletic participation (columns 4 and 9). This is equally true for boys and girls.

When the category of non-athletic clubs is divided into vocational and non-vocational clubs, I find that the coefficient on vocational clubs is negative and statistically significant while the coefficient on non-vocational clubs is positive,

[13] Long and Caudill (1991) analyzed the effects of college sports participation using data from entering college freshman class in 1971, finding an increase in annual incomes of 4 percent for men. Although the estimated coefficient for women is economically comparable, the large standard errors (stemming from the small number of female sports participants in their sample) indicate that they simply lack sufficient statistical resolution to be able to distinguish effects of sports participation on wages for women.

statistically significant, and similar to the coefficient on athletics (columns 5 and 10). It is not surprising that the coefficient on vocational clubs is negative, if participating in such clubs indicates a desire to pursue a vocation rather than further schooling. This appears to be a clear case of sorting.

In short, the cross-sectional association between sports participation and educational attainment (which combines treatment and selection effects) appears to be similar for girls and boys. Furthermore, participation in non-athletic clubs has a similar association with educational attainment. Apparently, students who are actively engaged while in high school are more likely to get further education. Those who are nearing the end of their educational careers are less involved in high school activities. It is difficult to know whether any of this association is causal.

The association between high school activities and wages (approximately 9 years after high school, among those who work) is examined in Table 1.2.[14] Without controlling for education, playing a high school sport is associated with more than 10 percent higher wages for both men and women (column 1 and 6). When controls for education, AFQT scores, membership in the National Honor Society, and state fixed effects are added, the wage premium is reduced to around 7 percent for women and 6 percent for men, both of which are statistically significant (columns 3 and 8). When controls are added for participation in other clubs, we see that *only* high school sports participation consistently has a statistically significant association with wages (columns 4-5 and 9-10). The effect of athletics on women's wages is as large as that for men.

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[14] Wages are hourly wages measured in 1988 when the average respondent was 27 years old.

The fact that athletic participation (and only athletic participation among all clubs) is associated with wages suggests that sports have an especially strong correlation with a type of ability that is both an important determinant of wages and is not measured by other observable variables. Thus far, I have discussed only *associations* because sports participation is not randomly assigned in the NLSY, and it is unclear whether the coefficients recorded reflect causal effects or selection. I now turn to a natural experiment that relies on a credibly exogenous shock to female sports participation.

## 4. History of Title IX

On June 23, 1972 President Nixon signed into law Title IX of the Education Amendments to the 1964 Civil Rights Act.[15] Title IX stated that "No person in the United States shall, on the basis of sex, be excluded from participation in, be denied the benefits of, or be subjected to discrimination under any educational program or activity receiving financial assistance."[16] The two areas in which many schools still had explicitly discriminatory policies were sports and the enrollment of pregnant girls. Title IX banned such explicit discrimination against female athletes and pregnant students.[17] Thus, while Title IX formally applies to all areas of education, its most far-reaching implications have been to increase access to sports.[18]

[15] The legislative history of Title IX is shown in Appendix B.

[16] Historically, single sex schools were exempt from Title IX, as were military institutions, and religious institutions where Title IX was a violation of their religious beliefs.

[17] The potential for Title IX to impact sports participation was so great that the NCAA was behind an aggressive lobbying effort against its passage.

[18] Although Title IX applied to most activities of schools, other forms of explicit discrimination had been removed prior to Title IX. For instance, most of the male-only colleges and universities had become

A large, and discontinuous, jump in national female high school sports participation occurred in the early 1970s (Figure 1.1).[19] This increase in female participation appears to start with the passage of Title IX in 1972 and increase rapidly through to 1978, by which time the legislation required schools to be in compliance. Furthermore, the little evidence available in the earlier years indicates that girls throughout the United States had been virtually shut out of athletics prior to Title IX.

As is evident from Figure 1.1, schools immediately increased athletic opportunities for female students, despite the fact that schools were given until July 1978 to comply with the Title IX regulation. Although the regulation stipulating the procedures for the implementation of Title IX was not released until June 1975, most high school principals probably realized at the time the legislation passed that their schools would need to move toward roughly equal athletic participation rates among males and females.[20] Further, a school that had a high rate of male participation needed to achieve particularly large gains in female participation, unless it eliminated male sports teams.

Figure 1.2 shows that overall male high school athletic participation is largely unchanged over this period. National male participation rates remain around 50 percent. Figure 1.3 shows girls' participation as a share of all athletes. This fraction increased from 1971 when approximately 1 in 13 athletes were female to 1997 when 2 in 5 athletes

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coeducational prior to Title IX. Further, the rapid increase in women attending professional schools had also begun prior to Title IX (Goldin and Katz 2000).

[19] High school participation is measured as the total number of varsity players in all teams. If a person plays on two teams, they are counted as two participants.

[20] Title IX played a role in changing attitudes regarding the appropriateness of female sports participation – changes that were necessary for the increase to occur. In other words, even if schools expanded the *opportunities* overnight we would still expect a gradual increase in *participation* as social norms changed.

were female. Note that the most dramatic changes occurred from 1972-1978, corresponding with Title IX's timing. Given that male participation was steady, it is clear that the change in female share of athletes was generated almost entirely by an increase in women's participation.

In sum, the equality of opportunity and equality of provision mandates in Title IX induced schools to allow and/or encourage girls to pursue an interest in sports. Although enforcement of the law was (and still is) far from perfect, many schools made discrete and significant changes in the accessibility and attractiveness of high school sports for girls. Furthermore, scholars have argued that Title IX created a new norm about female athletics, which generated part of the dramatic increase in female athletic participation.[21]

## 5. Empirical Strategy

Title IX legislation provides an exogenous shock to female sports participation nationally. Moreover, the national shock can be combined with variation across states in *male* sports participation rates, prior to Title IX, to generate a useful identification strategy for estimating the effects of female high school sports participation.

Data on sports participation come from the National High School Athletic Participation Survey conducted by the National Federation of State High School Associations (NFSH). Each state, plus the District of Columbia, has its own high school association, which is responsible for gathering information from individual schools.[22]

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[21] Cahn 1994, Birrell & Cole 1994.

[22] Iowa appears to significantly under-report athletic participation through the 1970s.

The state associations record the number of athletes in each sport, by gender, and they report this information at the state level.[23]

The sports participation data provide the total number of team members in each state. To get sports participation *rates*, the raw numbers need to be divided by total high school enrollment by gender, for each state, for each year. However, high school enrollment by state and sex is not available. Instead I collect state level high school enrollment data from administrative sources and impute a gender division using graduation rates from the 5% Public Use Micro Sample (PUMS) of the 1990 Census of Population.[24]

Prior to Title IX's passage, individual states varied enormously in terms of the athletic participation rate of boys (Figure 1.4).[25] States with higher levels of male participation needed a higher level of female participation by 1978 to be in compliance with Title IX. Female participation rates also varied by state prior to Title IX, but the variation in girls' participation was much smaller than that in boys' participation (Figure 1.5). The difference between the maximum and minimum rates of girls' participation is less than one-sixth of the difference between the maximum and minimum rates of boys' participation. The participation rates in 1977-78 illustrate that while there were some

[23] Annual data exists for the number of participants for each gender, in each sport, and by state, for the academic years 1970-71, 1972-73, 1973-74, 1975-6, 1977-78 and every academic year thereafter.

[24] While this estimate has many problems, including the fact that state of birth is used to identify the state of high school and that people with GED degrees are counted as having graduated high school, this estimate should help control for any bias that may result from a change over time in female enrollment rates caused by the increasing athletic opportunities in high school. An alternative is to impute that half of all students are female. All results have been checked and found to be robust to using this alternative imputation procedure.

[25] Since participation counts all team members and many athletes play on more than one team, this number can be greater than one.

changes in male participation rates, the overwhelming change was the increase in female participation rates (Figures 1.6 and 1.7).

As a first stage equation, it might seem natural to regress a state's change in the girls' participation rates on the initial (1971) gap between the boys' and girls' participation rates:

$$\textit{Girls' participation}_s^{1978}\textit{-Girls' participation}_s^{1971}$$
$$= \alpha+\beta(\textit{Boys' participation}_s^{1971}\textit{-Girls' participation}_s^{1971}{}_s)+\varepsilon_s \quad (1)$$

However, there are two reasons why this specification is not the most desirable one. First, female sports participation rates in 1971 are measured with a great deal of error (perhaps because female athletics were not considered to be important). Putting a variable that contains a lot of classical measurement error on both sides of an equation produces biased coefficients. Second, the *initial level* of girls' participation is likely to be correlated with state norms regarding female education, female labor force participation, and women generally. The level of boys' sports participation prior to Title IX is far less likely to reflect such norms. Since most of the variation in states' compliance problems is generated by boys' participation and since *all* of the plausibly exogenous variation in states' compliance problems is generated by boys' initial participation, better first stage equations are:

$$\textit{Girls' participation}_s^{1978} = \alpha + \beta\,\textit{Boys' participation}_s^{1971} + \varepsilon_s \quad (2)$$

*or*

$$\textit{Girls' participation}_s^{1978}\textit{-Girls' participation}_s^{1971}=\alpha + \beta(\textit{Boys' participation}_s^{1971})+\varepsilon_s \quad (3)$$

Table 1.3 shows the results of estimating these two first stage equations. Columns 1 and 2 show the estimated coefficients from equations 2 and 3, respectively. The R-squared statistics indicate that the initial level of boys' participation is a strong instrument for either the level of girls' participation in 1978 or the change in girls' participation between 1971 and 1978. The coefficients indicate that a state that inherited a 10 percentage point higher rate of male sports participation increased female participation rates by 3 to 4 percentage points by 1978. The similarity of these specifications reflects the fact that female sports participation rates were close to zero in most states. Figure 1.8 shows that the rise in female sports participation rates from 1971 to 1978 is closely related to the pre-existing (1971) levels of participation.

To test that the relationships just described are not merely a coincidence, the third column shows the results of estimating an analogous "placebo" regression; boys' participation in 1981 is used to predict the change from 1981 to 1988 in girls' participation. As expected, boys' participation in 1981 does not have a statistically significant effect on the subsequent change in female athletic participation. In fact, the adjusted R-squared statistic is -.001.

Title IX was passed in 1972 and took full effect in the summer of 1978. Girls who graduated from high school in 1972 would have been unaffected by Title IX, while those who started high school in the fall of 1978 would have been fully exposed to the regulations mandated by Title IX. Given approximate high school starting and finishing ages of 14 and 18 respectively, those born before 1954 would have had no exposure to Title IX, while those born after 1964 would have had complete exposure. The cohort born between 1954 and 1964 had increasing exposure to Title IX, both because Title IX was gradually implemented over the six-year period from legislative passage to implementation and because there is more than one year of potential treatment. Thus, three cohorts can be identified: a non-treated cohort consisting of women born prior to

1954, a partially treated cohort comprised of individuals born between 1954 and 1964, and a treatment cohort consisting of those born after 1964.

In sum, the *combination* of pre-law variation in male sports participation rates and the timing of the Title IX legislation interact to generate a natural experiment in girls' sports participation. The instrumental variable yields plausibly exogenous variation in girls' participation unless there is an omitted variable that changes monotonically between 1971 and 1978 *and* that has state-level variation *in the rate of change* that is correlated with states' 1971 *levels* of *boys'* participation.

There remain two caveats about this identification strategy. First, Title IX did affect pregnant teens' enrollment in school, although pregnant teens constituted only a very small fraction of girls. Second, because Title IX increased athletic opportunities for women at college, the instrument may also pick up a rise in college athletic scholarships and athletic participation. However, much smaller shares of both male and female students participate in college athletics than in high school athletics. Even so, the reduced form results reflect the differential impact of Title IX across states.

## 6. The Effects of Athletic Participation on Education

I use data on 25-29 year olds from the 5% Public Use Micro Sample (PUMS) of the 1980 and 1990 Censuses of Population to look at educational attainment using the instrumental variables procedure just described.[26] The 25-29 year olds from the 1980 PUMS would have attended high school entirely before Title IX enactment if they had normal grade-for-age; the 25-29 year olds from the 1990 PUMS went to high school

[26] Data are for 49 states and the District of Columbia. Iowa was dropped from the sample due to reporting problems with the girls' sports participation. All results are robust to including Iowa in the sample.

entirely after Title IX compliance if they had normal grade-for-age.[27] I regress their educational attainment on the level of athletic participation that was characteristic of their cohort in their state:[28]

$$\textit{Years of Education}_{i,s,t} = \alpha + \beta\ \textit{Athletic Participation}_{s,t} + \sum_s \eta_s State_s + \sum_t \lambda_t Year_t + X_{i,s,t}\delta + \varepsilon_{i,s,t}$$

where $i$=individual, $s$=state, and $t$=year of census. **(4)**

The instrument for athletic participation is the pre-Title IX (1971) level of boys' participation for 1990 cohort observations and is zero for 1980 cohort observations (because the states' compliance problem was "zero" for this cohort). The standard errors are clustered by state-year cells.

Table 1.4 shows the instrumental variables estimates of the effects of female athletic participation on educational attainment. The first row in the first column of Table 1.4 suggests that a 10 percentage point increase in the female athletic participation rate in a state generates an increase in the average educational attainment among women of 0.021 years. An alternative way to interpret the estimate is to say that if *every* woman had been to induced to participate in athletics (and if none had been participating 1971) then educational attainment among women would have been 0.21 years higher. (This latter interpretation is less natural, given the source of variation in the data, as it requires one to extrapolate beyond the variation in the data.) This estimate controls for age, race, ability to speak English, disability, and state and time fixed effects. The second row reports the reduced form estimate of the relationship between Title IX compliance

[27] Mistakenly treating those who attend high school at an older age as untreated may lead to attenuation bias.

[28] State of birth is used as an indicator of the state in which an individual attended high school. This is likely to attenuate the estimated effects of Title IX

problems and educational attainment. The third row gives the estimate from the first stage (equation 3).[29]

The estimates in the next four columns examine whether the instrumental variables estimate is robust to changes in the specification. Column 2 shows that the estimated coefficients are unchanged by adding a cyclical control, the state's male unemployment rate, for each time period. Recall that, in order for an omitted variable to be driving the results, it would have to be changing over time in a way that is correlated with the initial level of boys' sports participation. For example, there was a tremendous increase in the educational attainment of African-American women over this time period. If there are changes in state racial composition that are coincidentally correlated with the initial level of boys' sports participation then the coefficient on sports participation may be inflated. Adding controls for changes in racial composition (year-times-race interactions) has little effect on both the estimated coefficient and the standard error (column 3).

Alternatively, one might be concerned about regional trends. Indeed Figure 1.4 shows that southern states had boys' participation rates in 1971 that were below the median. In column 4 of Table 1.4, controls are added for regional changes (year*region-of-birth indicator variables), raising the magnitude on the coefficient slightly.[30] Column five controls for both racial changes over time and regional changes over time. Again, there is little change in the magnitude of the coefficient, though there is an increase in the

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[29] The estimated coefficient here will differ from that in Table 3 both because of added controls and because these regression reflect census weights.

[30] Region is a saturated set of dummies for the nine U.S. regions, identified for an individual's state of birth..

standard error. In sum, the causal effect of playing sports on women's educational attainment appears to be quite robust.

Table 1.5 further analyzes the relationship between sports participation and post-secondary educational attainment. The first two columns in Table 1.5 regress sports participation on whether or not an individual received at least 2 years of education beyond high school. The first column shows that a 10 percentage point increase in girls' sports participation generates an increase of 0.9 percentage points in the probability of getting at least two years of college. The alternative interpretation is that if *every* girl had been induced to participation in athletics (and if no girls had been participating in 1971), then the female college attendance rate would have risen by 9 percentage points. As in Table 1.4, I test the sensitivity of the specification to the inclusion of regional and racial changes. Adding indicator variables for year*race and year*region has little impact.

I next look at the probability of attending at least four years of college. Although there is a positive and statistically significant effect of sports participation on the likelihood of achieving at least a college degree, these effects are not robust to the inclusion of either year-by-race and year-by-region interactions (columns 3 and 4).

To look further at post-secondary educational attainment I generated a variable indicating whether or not an individual has received schooling beyond undergraduate college. With controls for age, race, ability to speak English, disability status, male state unemployment, and state and year fixed effects, a 10 percentage point increase in girls' sports participation generates an increase of .14 percentage points in the probability of attending at least two years of schooling beyond college. With year-by-race and year-by-region effects included, the estimated coefficient doubles.

In sum, it appears that sports participation induced by Title IX had a large and statistically significant effect on female educational attainment. The reduced form results also indicate that states with bigger compliance problems (and thus bigger predicted increases in female sports participation) had bigger increases in educational attainment for women. The IV results suggest that a 10 percentage point rise in sports participation raises average years of education in a state by .02 to .03 years. Recall from the cross-sectional regressions in section 3 that female athletes had about 0.3 years more schooling than non-athletes. Thus, the causal effect found using an instrument for athletic participation is of a similar magnitude as one gets after controlling for many individual characteristics in cross-sectional data.

## 7. The Effect of Athletic Participation on Employment and Occupational Choice

If participating in athletics gives women skills that are particularly useful in the workforce, then women who had participated in athletics should be more likely to be employed.[31] To test this using the instrumental variables strategy, a variable indicating whether or not an individual reported being employed is generated for women ages 25-29 in both the 1980 and 1990 censuses. I regress whether or not a women reports being employed at the time of the census on the level of athletic participation that was characteristic of their cohort in their state:

$$Employed_{i,s,t} = \alpha + \beta \; Athletic\ Participation_{s,t} + \mathbf{X}_{i,s,t}\boldsymbol{\delta} + \sum_{s} \eta_s State_s + \sum_{t} \lambda_t Year_t + \varepsilon_{i,s,t} \quad (5)$$

[31] For instance, using the NLSY-79 women who played sports in high school were 5 percent more likely to be working 10 years later.

Following the previous section, I instrument for athletic participation with the interaction of boys' participation in 1971 and a post-Title IX dummy variable.

Regressions in Tables 6a and 6b also include controls for age, race, ability to speak English, disability status and state and year fixed effects. Column 1 of Table 1.6a shows that a 10-percentage point increase in girls' sports participation generates an increase of 1.4 percentage points in the probability of being employed. This result is robust to controlling for the state male unemployment rate (column 2). Adding controls for changes in racial composition (year*race interactions) and regional trends (year*region interactions) suggests that these basic results are robust (columns 3-5).

To further test this specification, regressions were run defining a woman as employed only if she reported having been employed for at least 42 weeks of the previous year. The estimated coefficients are similar to those using employment status at the time of the census survey (columns 6-9 of Table 1.6a). To analyze employment patterns further, Table 1.6b examines women who report working full-time (a typical work-week of 35+ hours). Controlling for age, race, ability to speak English, disability status, male state unemployment, and state and year fixed effects, a 10-percentage point increase in girls' sports participation generates an increase of 0.7 percentage points in the probability of working full-time (column 1). However, this estimate is not robust to the inclusion of controls (columns 2-4). For women who reported having worked full-time at least 42 weeks in the past year, column 5 shows that a 10 percentage point increase in the female athletic participation rate generates an increase of 0.9 percentage points in the probability of having worked full-time for at least 42 weeks in the previous year. These results are robust, albeit dampened, when year times region indicator variables are added (column 7). However, the result is not robust to the inclusion of year times race indicator variables (columns 6 and 8). In sum, the instrumental variables procedure yields suggestive evidence that the increase in sports participation caused by Title IX caused female employment rates to rise.

Recall that sports participation may teach competitiveness and assertiveness. As such, sports participation might have an effect on the type of career chosen. In other words, one might want to ask if girls who play sports are more likely to choose an occupation that was traditionally male. The difficulty in answering this question is that the increase in female employment rates just documented may confound identifying such trends. Women working in 1990 are a different group to those selected in 1980. If the marginal women who are induced by sports participation to become employed are different from the infra-marginal female worker in terms of her propensity to choose traditionally female occupations, then we might find that sports participation caused a decrease in the probability of choosing a previously male occupation.

Indeed the first column of Table 1.7 shows that a 10 percentage point increase in the female athletic participation rate leads to a 0.24 percentage point *decrease* in the probability of being employed in a job that was over 75 percent male in 1980. This estimate is robust to the inclusion of year-by-race indicator variables. However, it is not robust to the inclusion of year-by-region-of-birth indicator variables, and the coefficient becomes statistically indistinguishable from zero.

To get around the compositional changes generated by the supply shift, we can look for a group of women who did not have a large change in the probability of working. Of women who have no children, have never been married, and have a college degree, 91 percent were employed in 1980, compared with 93 percent in 1990. While, this measure may suffer from potential endogeneity as the decisions to neither marry, nor have children, and to get more education are all potentially influenced by sports participation, analyzing this group partly alleviates identification problems arising from the supply shift. Indeed, when controlling for year-by-race and year-by-region-of-birth effects, a 10 percentage point increase in the female sports participation rate generates an increase of .07 in the probability of being in a male dominated occupation for this group of women. However, the estimated coefficients generated for this group is also sensitive to the specification chosen - the result is not robust to eliminating region-specific trends (columns).

## 8. Conclusion

Despite the controversial nature of applying Title IX to athletics, there has been surprisingly little research done on the effects of Title IX on sports participation, and even less on the effects of female sports participation on later outcomes for women. Previous research, in both the economics and sociology literature, has found that participating in athletics, both at the high school and college level, translates into improved outcomes for men. While some of this research has attempted to look at women, the small number of women participating in athletics prior to 1972 made it near impossible to find significant effects. Furthermore, the existing literature has been hampered by a severe methodological problem in that sports participation is not randomly determined. As such, selection issues may swamp the previous findings of positive relationships between sports participation and outcome measures.

Title IX of the Education Amendments to the 1964 Civil Rights Act required schools to provide athletic opportunities for girls equal to those provided to boys. Before Title IX was enacted, states differed in the levels of athletic opportunities offered to boys, while negligible opportunities were offered to girls everywhere. Hence compliance required a larger increase in girls' sports participation in those states with historically strong sports programs for boys. Thus, the *interaction* of the Title IX legislation with pre-existing levels of boys' sports participation provides a credibly exogenous instrument for the change in girls' athletic participation over the 1972-78 period. Reduced form estimates suggest that this interaction is significantly related to changes in female educational attainment and employment status. Further, first stage regressions show that the instrument does indeed explain much of the variation through time in state-level measures of athletic participation. Thus, I conclude that athletic participation has important causal effects on women's educational and labor market outcomes. While alternative interpretations may point to the impact of Title IX on other things going on in the education system, these interpretations need to rely on the change in outcomes being correlated with the initial level of sports participation of boys.

My central estimates suggest that if a state's female sports participation rate rises 10-percentage points, then average levels of schooling in the state will rise by around .02-.03 years, and employment rates will rise by around 1 percentage point. Thus, while selection may explain some of the positive correlates of athletic participation found in cross-sectional analysis, there is some indication of important treatment effects as well.

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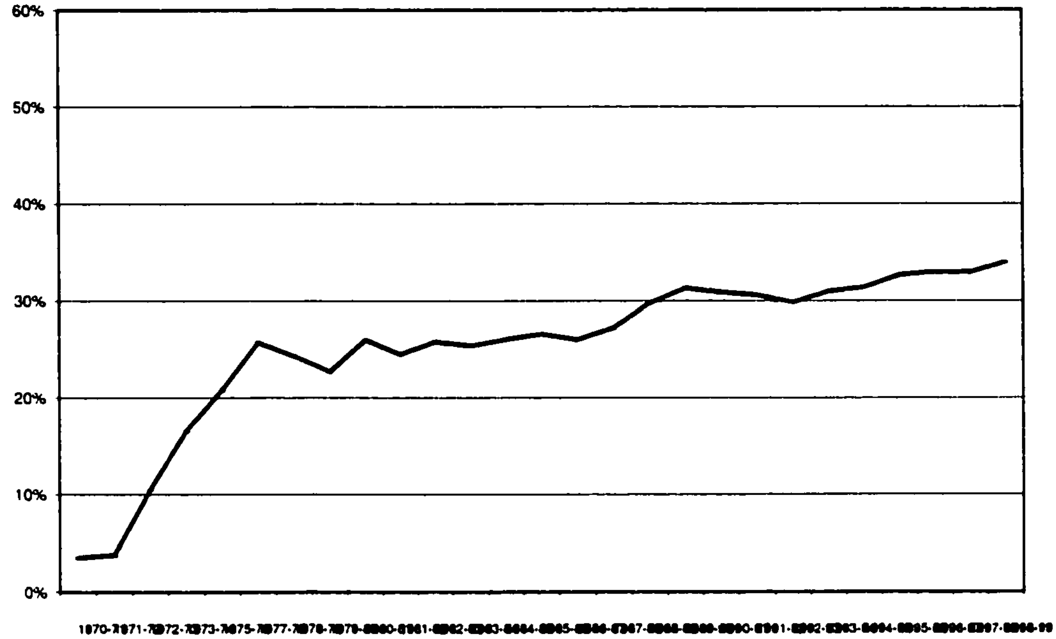
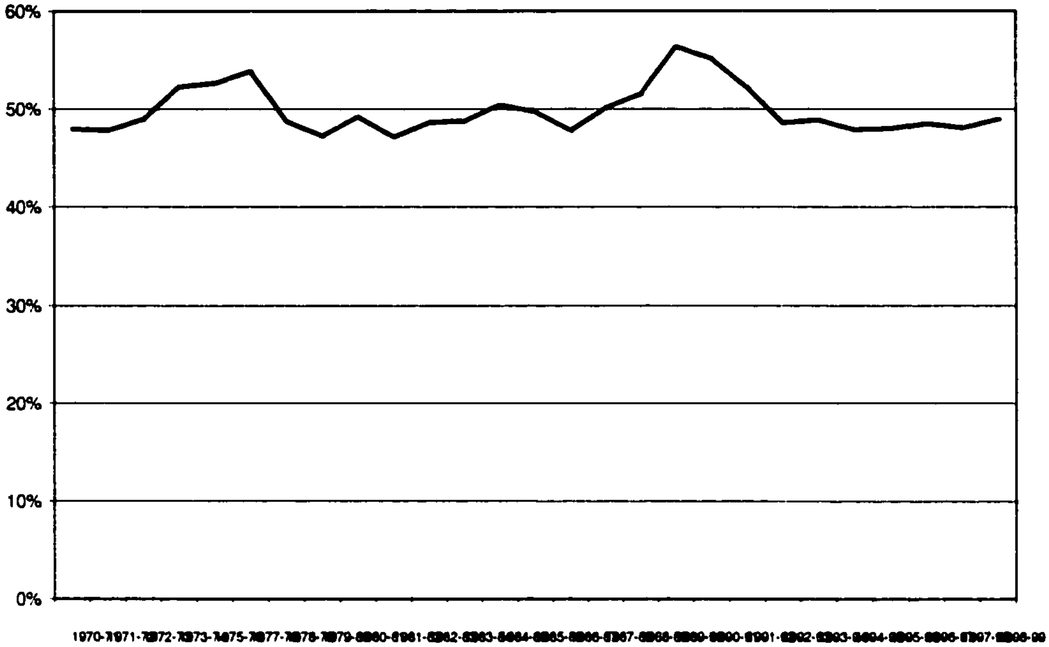
**Figure 1.1**
**Female High School Sports Participation**
**(As a Percentage of Female High School Enrollment)**



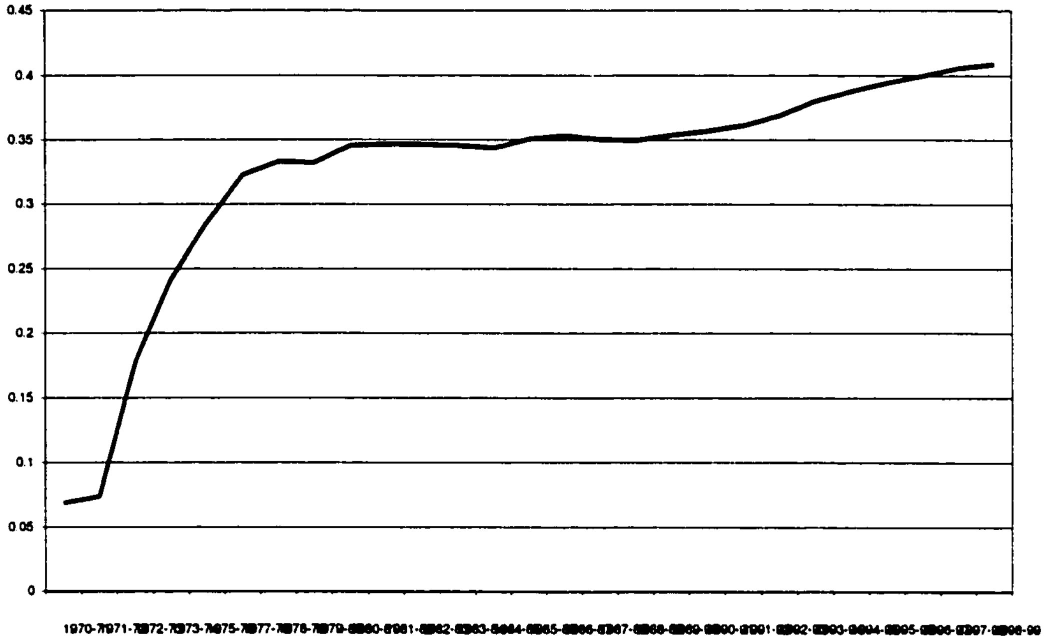

Source: Participation numbers are from the National Athletic Participation Survey (conducted by the National Federation of High Schools ). A participant is a varsity sport team member. (Individual students may be counted more than once if they play on multiple teams.) The participation rate is the sum of total team memberships in a year, divided by total high school enrollment given by the National Center for Education Statistics.

**Figure 1.2**
**Male High School Sports Participation**
**(As a Percentage of Male High School Enrollment)**



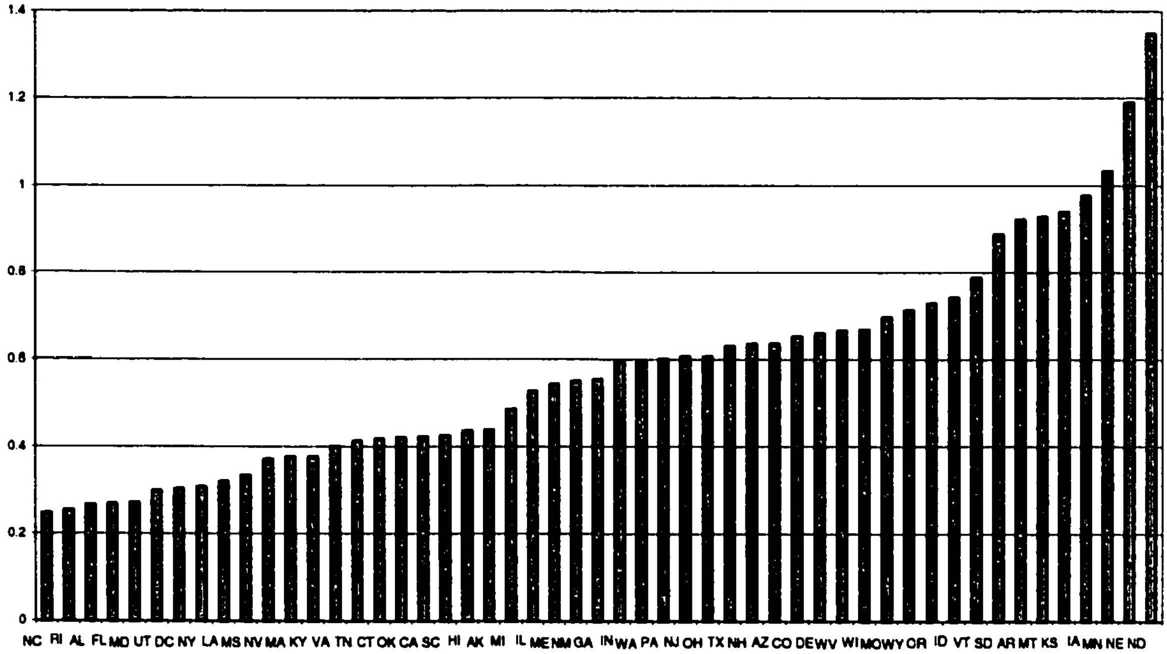

Source: Participation numbers are from the National Athletic Participation Survey (conducted by the National Federation of High Schools ). A participant is a varsity sport team member. (Individual students may be counted more than once if they play on multiple teams.) The participation rate is the sum of total team memberships in a year, divided by total high school enrollment given by the National Center for Education Statistics.

**Figure 1.3**
**Female High School Sports Participants**
**(As a Fraction of all Sports Participants)**



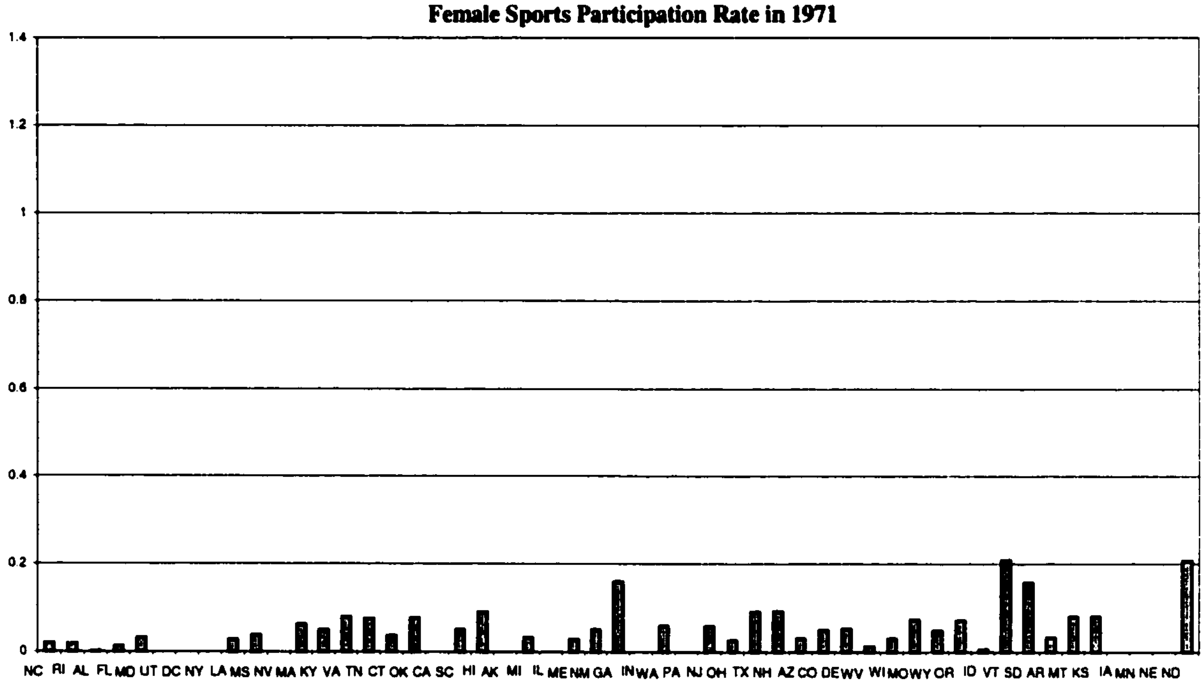

Source: Participation numbers are given by the National Federation of High Schools (Athletic Participation Survey). A participant is a varsity sport team member. (Individual students may be counted more than once if they play on multiple teams.) The participation rate is the sum of total team memberships in a year, divided by total high school enrollment given by the National Center for Education Statistics.

**Figure 1.4**
**Male Sports Participation Rate in 1971**



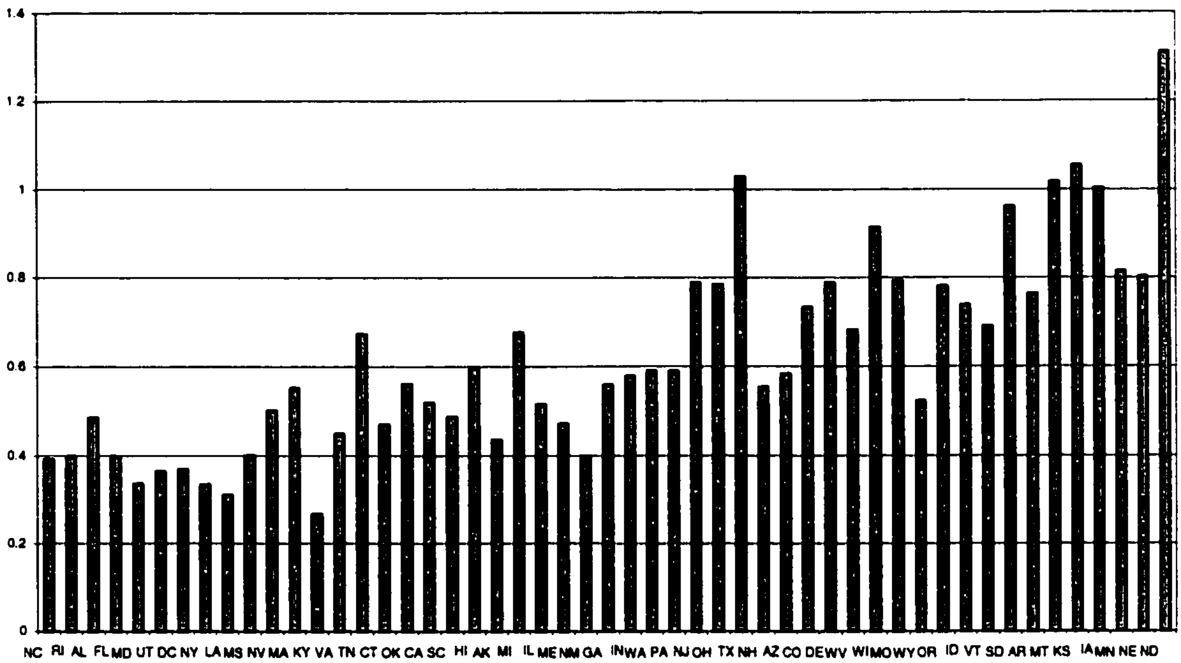

Source: State numbers for 1971 are calculated by the author from state-level participation numbers by sport (provided by the National Federation of High Schools Athletic Participation Survey). The participation numbers are divided by an estimate of the state's high school population. These are estimated using state-level high school enrollment data from the National Center for Education Statistics (NCES), with a gender split imputed using graduation rates by state of birth from the 5% Public Use Micro Sample (PUMS) of the 1990 Census of Population.

**Figure 1.5**
**Female Sports Participation Rate in 1971**



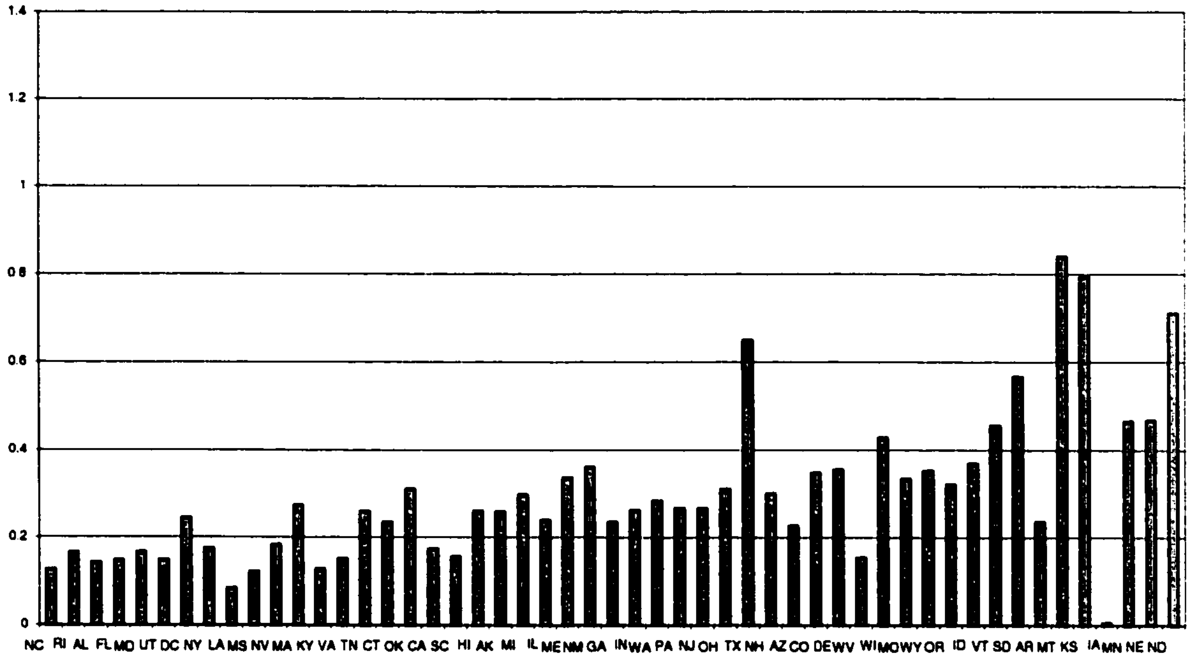

Source: State numbers for 1971 are calculated by the author from state-level participation numbers by sport (provided by the National Federation of High Schools Athletic Participation Survey). The participation numbers are divided by an estimate of the state's high school population. These are estimated using state-level high school enrollment data from the National Center for Education Statistics (NCES), with a gender split imputed using graduation rates by state of birth from the 5% Public Use Micro Sample (PUMS) of the 1990 Census of Population. The states are sorted in order of increasing level of boys' sports participation in 1971 (see Figure 1.4).

**Figure 1.6**
**Boys Participation 1977**



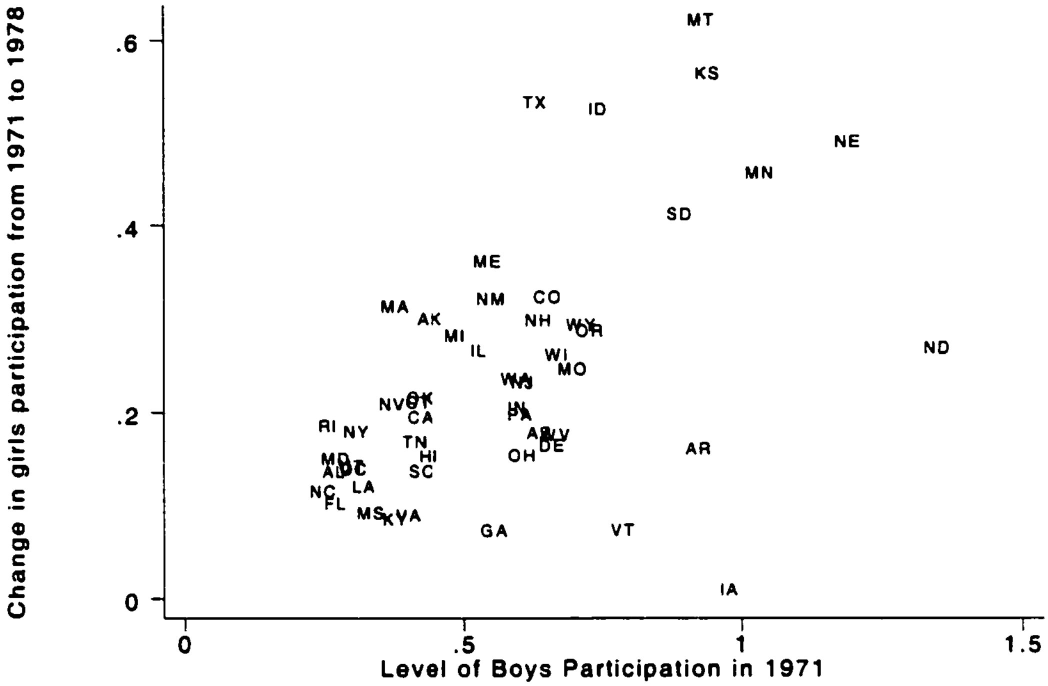

Source: State numbers for 1971 are calculated by the author from state-level participation numbers by sport (provided by the National Federation of High Schools Athletic Participation Survey). The participation numbers are divided by an estimate of the state's high school population. These are estimated using state-level high school enrollment data from the National Center for Education Statistics (NCES), with a gender split imputed using graduation rates by state of birth from the 5% Public Use Micro Sample (PUMS) of the 1990 Census of Population. The states are sorted in order of increasing level of boys' sports participation in 1971 (see Figure 1.4).

**Figure 1.7**
**Girls Participation 1977**




Source: State numbers for 1971 are calculated by the author from state-level participation numbers by sport (provided by the National Federation of High Schools Athletic Participation Survey). The participation numbers are divided by an estimate of the state's high school population. These are estimated using state-level high school enrollment data from the National Center for Education Statistics (NCES), with a gender split imputed using graduation rates by state of birth from the 5% Public Use Micro Sample (PUMS) of the 1990 Census of Population. The states are sorted in order of increasing level of boys' sports participation in 1971 (see Figure 1.4).

Figure 1.8




Source: State numbers for 1971 are calculated by the author from state-level participation numbers by sport (provided by the National Federation of High Schools Athletic Participation Survey). The participation numbers are divided by an estimate of the state's high school population. These are estimated using state-level high school enrollment data from the National Center for Education Statistics (NCES), with a gender split imputed using graduation rates by state of birth from the 5% Public Use Micro Sample (PUMS) of the 1990 Census of Population.

**TABLE 1.1**
**EFFECTS OF HIGH SCHOOL PARTICIPATION IN EXTRA-CURRICULAR ACTIVITIES ON EDUCATIONAL ATTAINMENT**

| | Dependent Variable: Years of Education[a] (OLS) | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| **Independent Variable** | **Female** | | | | | **Male** | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Athletics[b] | .634 | .653 | .371 | .323 | .276 | .836 | .804 | .417 | .399 | .387 |
| | (.054) | (.055) | (.049) | (.050) | (.050) | (.055) | (.056) | (.048) | (.048) | (.048) |
| All Non-Athletic Clubs[b] | | | | .287 | | | | | .210 | |
| | | | | (.052) | | | | | (.048) | |
| Vocational Clubs[b] | | | | | -.096 | | | | | -.277 |
| | | | | | (.049) | | | | | (.056) |
| Non-vocational Clubs[b] | | | | | .384 | | | | | .427 |
| | | | | | (.050) | | | | | (.050) |
| **Controls**[c] | | | | | | | | | | |
| Race | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Parents' Education[d] | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Urban/Rural status | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Age | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| State at age 14 | | ✓ | ✓ | ✓ | ✓ | | ✓ | ✓ | ✓ | ✓ |
| AFQT Score | | | ✓ | ✓ | ✓ | | | ✓ | ✓ | ✓ |
| National Honor Society | | | ✓ | ✓ | ✓ | | | ✓ | ✓ | ✓ |
| Number of Observations | 5148 | 5148 | 5148 | 5148 | 5148 | 4932 | 4932 | 4932 | 4932 | 4932 |
| Adjusted R-squared | .21 | .22 | .40 | .41 | .40 | .23 | .24 | .47 | .47 | .48 |

Source: Author's calculations based on data from National Longitudinal Survey of Youth, 1979 (NLSY79).
(Standard errors in parentheses.)
[a] Educational attainment is that achieved by 1996. Sample is restricted to those who completed at least 10[th] grade.
[b] Participation in extra-curricular activities was asked in 1984. *Athletics* is an indicator variable for an individual having participated in high school sports. *Non-athletic clubs* include vocational clubs, student government, newspaper, yearbook, and other, primarily hobby, clubs. This variable is then further divided into two collectively exhaustive and mutually exclusive categories: *vocational* and *non-vocational* clubs.
[c] All control variables are included as a saturated set of dummy variables.
[d] *Parent's education* reflects the highest grade completed by either parent.

**TABLE 1.2**
**EFFECTS OF HIGH SCHOOL PARTICIPATION IN EXTRA-CURRICULAR ACTIVITIES ON LOG HOURLY WAGES**

| **Independent Variable** | **Dependent Variable: Log Hourly Wages[a] (OLS)** | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | **Female** | | | | | **Male** | | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Athletics[b] | .126$^{***}$ | .078$^{***}$ | .067$^{***}$ | .067$^{***}$ | .068$^{***}$ | .105$^{***}$ | .069$^{***}$ | .055$^{***}$ | .057$^{***}$ | .058$^{***}$ |
| | (.019) | (.019) | (.019) | (.019) | (.019) | (.021) | (.021) | (.022) | (.022) | (.022) |
| All Non-Athletic Clubs[b] | | | | .001 | | | | | -.036$^{*}$ | |
| | | | | (.021) | | | | | (.021) | |
| Vocational Clubs[b] | | | | | -.019 | | | | | -.038 |
| | | | | | (.020) | | | | | (.025) |
| Non-vocational Clubs[b] | | | | | .002 | | | | | -.023 |
| | | | | | (.019) | | | | | (.023) |
| **Controls[c]** | | | | | | | | | | |
| Race | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Parents' Education[d] | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Urban/Rural status | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Age | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| State at age 14 | | ✓ | ✓ | ✓ | ✓ | | ✓ | ✓ | ✓ | ✓ |
| AFQT Score | | | ✓ | ✓ | ✓ | | | ✓ | ✓ | ✓ |
| National Honor Society | | | ✓ | ✓ | ✓ | | | ✓ | ✓ | ✓ |
| Number of Observations | 3724 | 3724 | 3724 | 3724 | 3724 | 3712 | 3712 | 3712 | 3712 | 3712 |
| Adjusted R-squared | .13 | .18 | .21 | .21 | .21 | .09 | .11 | .13 | .13 | .13 |

Source: Author's calculations based on data from National Longitudinal Survey of Youth, 1979 (NLSY79).
(Standard errors in parentheses.) $^{***}$, $^{**}$ and $^{*}$ indicate statistically discernible from zero at the 1%, 5% and 10% levels, respectively.
[a] Log hourly wages are as reported in 1988 when the average respondent is 28 years old. Sample is restricted to those who completed at least 10th grade, are employed, and have a valid hourly wage observation.
[b] Participation in extra-curricular activities was asked in 1984. *Athletics* is an indicator variable for an individual having participated in high school sports. *Non-athletic clubs* include vocational clubs, student government, newspaper, yearbook, and other, primarily hobby, clubs. This variable is then further divided into two collectively exhaustive and mutually exclusive categories: *vocational* and *non-vocational* clubs.
[c] All control variables are included as a saturated set of dummy variables.
[d] Parent's education reflects the highest grade completed by either parent.

**TABLE 1.3**
**THE RELATIONSHIP BETWEEN THE CHANGE IN GIRLS SPORTS PARTICIPATION AND THE PRE-EXISTING LEVEL OF BOYS SPORTS PARTICIPATION**

| Independent Variable | Change in Girls' participation Rate: 1971-1978 (1) | Girls' participation Rate in 1978 (2) | PLACEBO Change in Girls' participation Rate: 1981-1988 (3) |
|---|---|---|---|
| Boys' participation in 1971 | .280*** (.067) | .353*** (.068) | |
| Boys' participation in 1981 | | | .080 (.083) |
| Adjusted $R^2$ | .25 | .34 | -.001 |
| Number of Observations | 51 | 51 | 51 |

***, **, and * indicate statistically discernible from zero at the 1%, 5% and 10% levels, respectively.

Source: Participation rates are calculated by the author using total participation numbers from High School Athletics Participation Survey (conducted by the National Federation of High Schools). A participant is a varsity sport team member. (Individual students may be counted more than once if they play on multiple teams.) The participation rate is the sum of total team memberships in a state in a year, divided by an estimate of the state's high school population. These are estimated using state-level high school enrollment data from the National Center for Education Statistics (NCES), with a gender division is imputed using graduation rates by state of birth from the 5% Public Use Micro Sample (PUMS) of the 1990 Census of Population.

**TABLE 1.4**
**INSTRUMENTAL VARIABLES ESTIMATES OF THE EFFECTS OF FEMALE ATHLETIC PARTICIPATION ON EDUCATIONAL ATTAINMENT**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| **Wald Estimator (IV)** | .209** | .221** | .199* | .311* | .300 |
| **Causal Effect of Sports Participation** [a] | (.102) | (.100) | (.112) | (.171) | (.187) |
| **Reduced Form Results: Differential Effects of** | .089*** | .092*** | .082* | .094*** | .090** |
| **Title IX on Years of Education, by State** [b] | (.037) | (.036) | (.043) | (.036) | (.039) |
| **First-Stage Results: Changes in Female Sports** | .426 | .410 | .412 | .301 | .301 |
| **Participation Generated by Title IX** [c] | (.057) | (.050) | (.050) | (.088) | (.087) |
| **Controls** | | | | | |
| **Year*Race Fixed Effects** | NO | NO | YES | NO | YES |
| **Year*Region of Birth Fixed Effects** | NO | NO | NO | YES | YES |
| **State Male Unemployment Rate** | NO | YES | YES | YES | YES |
| **Observations** | 844509 | 844509 | 844509 | 844509 | 844509 |

Standard errors (shown in parentheses) are clustered at the level of 100 state-year cells. ***, **, and * indicate statistically significant at the 1%, 5% and 10% levels.

Source: 1980 and 1990 Censuses of Population, IPUMS, 5% sample (Ruggles and Sobek 1997). Data are for 49 states plus the District of Columbia. Iowa is excluded because it does not report girls sports participation in 1978. Data are for women aged 25-29 conditional on having completed 10th grade.

Specifications:

[a] IV estimates of causal effects of rising state female sports participation rates:

$$Years\ of\ Schooling_{i,s,t} = \alpha + \beta\ Female\ Athletic\ Participation^{IV}_{s,t} + \sum_s \eta_s State_s + \sum_t \chi_t Year_t + \mathbf{X}_{i,s,t}\lambda + \varepsilon_{i,s,t}$$

[b] Reduced Form Results: Relationship between changing girls' educational outcomes, and the pre-existing levels of boys sports participation:

$$Years\ of\ Schooling_{i,s,t} = \alpha + \beta\,(Post\ Title\ IX\ Cohort_t * Boys\ Athletic\ Participation^{1971}_s) + \sum_s \eta_s State_s + \sum_t \chi_t Year_t + \mathbf{X}_{i,s,t}\lambda + \varepsilon_{i,s,t}$$

[c] 1st Stage Regression: Changes in girls sports participation by state generated by the interaction of Title IX and pre-existing levels of boys sports participation:

$$Female\ Athletic\ Participation_{i,s,t} = \alpha + \beta\,(Post\ Title\ IX\ Cohort_t * Boys\ Athletic\ Participation^{1971}_s) + \sum_s \eta_s State_s + \sum_t \chi_t Year_t + \mathbf{X}_{i,s,t}\lambda + \varepsilon_{i,s,t}$$

All regressions include as controls a saturated set of dummy variables for state of birth, year of sample, age, race, ability to speak English , and disability level.

# TABLE 1.5
## INSTRUMENTAL VARIABLES ESTIMATES OF THE EFFECTS OF FEMALE ATHLETIC PARTICIPATION ON THE PROBABILITY OF ATTENDING COLLEGE

| | Attended at least two years of college | | Attended at least four years of college | | Post-college education | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **Wald Estimator (IV)** | .088** | .131** | .026* | -.031 | .014* | .031** |
| **Causal Effect of Sports Participation** [a] | (.039) | (.065) | (.015) | (.021) | (.005) | (.015) |
| **Reduced Form Results: Differential Effects of Title** | .037** | .039*** | .011* | -.009 | .006*** | .009*** |
| **IX on College-going propensities, by State** [b] | (.015) | (.012) | (.006) | (.007) | (.002) | (.002) |
| **First-Stage Results: Changes in Female Sports** | .410 | .301 | .410 | .301 | .410 | .301 |
| **Participation Generated by Title IX** [c] | (.050) | (.087) | (.050) | (.087) | (.050) | (.087) |
| **<u>Controls</u>** | | | | | | |
| **Year*Race Fixed Effects** | NO | YES | NO | YES | NO | YES |
| **Year*Region of Birth Fixed Effects** | NO | YES | NO | YES | NO | YES |
| **State Male Unemployment Rate** | YES | YES | YES | YES | YES | YES |
| **Observations** | 844509 | 844509 | 844509 | 844509 | 844509 | 844509 |

Standard errors (shown in parentheses) are clustered at the level of 100 state-year cells. ***, **, * indicate statistically significant at the 1%, 5% and 10% levels.

<u>Source:</u> 1980 and 1990 Censuses of Population (IPUMS) 5% sample (Ruggles and Sobek 1997). Data are for 49 states plus the District of Columbia. Iowa is excluded because it does not report girls sports participation in 1978. Data are for women aged 25-29 conditional on having completed 10th grade.

<u>Specifications:</u> (Linear probability model)

[a] IV estimates of causal effects of rising state female sports participation rates:

$$\textit{Attended college}_{i,s,t} = \alpha + \beta\ \textit{Female Athletic Participation}^{IV}_{s,t} + \sum_s \eta_s State_s + \sum_t \chi_t Year_t + \mathbf{X}_{i,s,t}\lambda + \varepsilon_{i,s,t}$$

[b] Reduced Form Results: Relationship between changing girls' educational outcomes, and the pre-existing levels of boys sports participation:

$$\textit{Attended college}_{i,s,t} = \alpha + \beta\ (\textit{Post Title IX}_t * \textit{Boys Athletic Participation}^{1971}_s) + \sum_s \eta_s State_s + \sum_t \chi_t Year_t + \mathbf{X}_{i,s,t}\lambda + \varepsilon_{i,s,t}$$

[c] 1st Stage Regression: Changes in girls sports participation by state generated by the interaction of Title IX and pre-existing levels of boys sports participation:

$$\textit{Female Athletic Participation}_{i,s,t} = \alpha + \beta\ (\textit{Post Title IX}_t * \textit{Boys Athletic Participation}^{1971}_s) + \sum_s \eta_s State_s + \sum_t \chi_t Year_t + \mathbf{X}_{i,s,t}\lambda + \varepsilon_{i,s,t}$$

All regressions include as controls a saturated set of dummy variables for state of birth, year of sample, age, race, ability to speak English, and disability level.

**TABLE 1.6a**
**INSTRUMENTAL VARIABLES ESTIMATES OF THE EFFECTS OF FEMALE ATHLETIC PARTICIPATION ON EMPLOYMENT STATUS**

| | Employed at time of the survey 1=Working, 0=Not Working | | | | | Full-year Worker Employed at least 42 weeks in past year | | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| **Wald Estimator (IV)** | .143*** | .152*** | .090*** | .170** | .112** | .133*** | .070*** | .126** | .068 |
| **Causal Effect of Sports Participation** [a] | (.036) | (.032) | (.025) | (.068) | (.057) | (.033) | (.026) | (.056) | (.046) |
| **Reduced Form Results: Differential Effects of Title** | .061*** | .063*** | .037*** | .051*** | .034*** | .055*** | .029*** | .038*** | .020** |
| **IX on Employment, by State** [b] | (.011) | (.010) | (.008) | (.011) | (.010) | (.011) | (.009) | (.011) | (.010) |
| **First-Stage Results: Changes in Female Sports** | .426 | .410 | .412 | .301 | .301 | .410 | .412 | .301 | .301 |
| **Participation Generated by Title IX** [c] | (.057) | (.050) | (.050) | (.088) | (.087) | (.050) | (.050) | (.088) | (.087) |
| **Controls** | | | | | | | | | |
| **Year*Race Fixed Effects** | NO | NO | YES | NO | YES | NO | YES | NO | YES |
| **Year*Region of Birth Fixed Effects** | NO | NO | NO | YES | YES | NO | NO | YES | YES |
| **State Male Unemployment Rate** | NO | YES | YES | YES | YES | YES | YES | YES | YES |
| **Observations** | 844509 | 844509 | 844509 | 844509 | 844509 | 844509 | 844509 | 844509 | 844509 |

Standard errors (shown in parentheses) are clustered at the level of 100 state-year cells. ***, **, * indicate statistically significant at the 1%, 5% and 10% levels.

Source: 1980 and 1990 Censuses of Population (IPUMS) 5% sample (Ruggles and Sobek 1997). Data are for 49 states plus the District of Columbia. Iowa is excluded because it does not report girls sports participation in 1978. Data are for women aged 25-29 conditional on having completed 10th grade.

Specifications: (Linear probability model)

[a] IV estimates of causal effects of rising state female sports participation rates:

$$Employed_{i,s,t} = \alpha + \beta \; Female\ Athletic\ Participation^{IV}_{s,t} + \sum_s \eta_s State_s + \sum_t \chi_t Year_t + \mathbf{X}_{i,s,t}\lambda + \varepsilon_{i,s,t}$$

[b] Reduced Form Results: Relationship between changing women's labor market outcomes, and the pre-existing levels of boys sports participation:

$$Employed_{i,s,t} = \alpha + \beta \; (Post\ Title\ IX_t \;*\; Boys\ Athletic\ Participation^{1971}_s) + \sum_s \eta_s State_s + \sum_t \chi_t Year_t + \mathbf{X}_{i,s,t}\lambda + \varepsilon_{i,s,t}$$

[c] 1st Stage Regression: Changes in girls sports participation by state generated by the interaction of Title IX and pre-existing levels of boys sports participation:

$$Female\ Athletic\ Participation_{i,s,t} = \alpha + \beta \; (Post\ Title\ IX_t \;*\; Boys\ Athletic\ Participation^{1971}_s) + \sum_s \eta_s State_s + \sum_t \chi_t Year_t + \mathbf{X}_{i,s,t}\lambda + \varepsilon_{i,s,t}$$

All regressions include as controls a saturated set of dummy variables for state of birth, year of sample, age, race, ability to speak English, and disability level.

**TABLE 1.6b**
**INSTRUMENTAL VARIABLES ESTIMATES OF THE EFFECTS OF FEMALE ATHLETIC PARTICIPATION ON EMPLOYMENT STATUS**

| | Full-time Worker<br>Usually works at least 35 hours per week | | | | Full-time, Full-year Worker<br>Employed at least 42 weeks in the past year and usually work at least 35 hours per week | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Wald Estimator (IV)**<br>**Causal Effect of Sports Participation** [a] | .067*<br>(.036) | .015<br>(.029) | .049<br>(.034) | .002<br>(.030) | .093***<br>(.033) | .034<br>(.027) | .069*<br>(.038) | .016<br>(.033) |
| **Reduced Form Results: Differential Effects of Title IX on Employment, by State** [b] | .028**<br>(.014) | .006<br>(.012) | .014*<br>(.008) | .001<br>(.009) | .039***<br>(.012) | .014<br>(.010) | .021***<br>(.008) | .005<br>(.009) |
| **First-Stage Results: Changes in Female Sports Participation Generated by Title IX** [c] | .410<br>(.050) | .412<br>(.050) | .301<br>(.088) | .301<br>(.087) | .410<br>(.050) | .412<br>(.050) | .301<br>(.088) | .301<br>(.087) |
| **Controls** | | | | | | | | |
| **Year*Race Fixed Effects** | NO | YES | NO | YES | NO | YES | NO | YES |
| **Year*Region of Birth Fixed Effects** | NO | NO | YES | YES | NO | NO | YES | YES |
| **State Male Unemployment Rate** | YES | YES | YES | YES | YES | YES | YES | YES |
| **Observations** | 844509 | 844509 | 844509 | 844509 | 844509 | 844509 | 844509 | 844509 |

Standard errors (shown in parentheses) are clustered at the level of 100 state-year cells. ***, **, and * indicate statistically significant at the 1%, 5% and 10% levels.

Source: 1980 and 1990 Censuses of Population (IPUMS) 5% sample (Ruggles and Sobek 1997). Data are for 49 states plus the District of Columbia. Iowa is excluded because it does not report girls sports participation in 1978. Data are for women aged 25-29 conditional on having completed 10th grade.

Specifications: (Linear probability model)

[a] IV estimates of causal effects of rising state female sports participation rates:

$$Employed_{i,s,t} = \alpha + \beta \text{ } Female\ Athletic\ Participation_{s,t}^{IV} + \sum_s \eta_s State_s + \sum_t \chi_t Year_t + \mathbf{X}_{i,s,t}\lambda + \varepsilon_{i,s,t}$$

[b] Reduced Form Results: Relationship between changing women's labor market outcomes, and the pre-existing levels of boys sports participation:

$$Employed_{i,s,t} = \alpha + \beta \text{ } (Post\ Title\ IX_t * Boys\ Athletic\ Participation_s^{1971}) + \sum_s \eta_s State_s + \sum_t \chi_t Year_t + \mathbf{X}_{i,s,t}\lambda + \varepsilon_{i,s,t}$$

[c] 1st Stage Regression: Changes in girls sports participation by state generated by the interaction of Title IX and pre-existing levels of boys sports participation:

$$Female\ Athletic\ Participation_{i,s,t} = \alpha + \beta \text{ } (Post\ Title\ IX_t * Boys\ Athletic\ Participation_s^{1971}) + \sum_s \eta_s State_s + \sum_t \chi_t Year_t + \mathbf{X}_{i,s,t}\lambda + \varepsilon_{i,s,t}$$

All regressions include as controls a saturated set of dummy variables for state of birth, year of sample, age, race, ability to speak English , and disability level.

## TABLE 1.7
## INSTRUMENTAL VARIABLES ESTIMATES OF THE EFFECTS OF THE CHANGE IN FEMALE ATHLETIC PARTICIPATION ON TYPLE OF OCCUPATION

| | Work in a "male occupation" defined as one in which 75% or more employees, ages 25-29 in 1980, were male | | | | Work in a "male occupation"– sample restricted to college educated, unmarried, women without children | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Wald Estimator (IV)** | -.024*** | -.023*** | -.006 | -.005 | -.036 | .010 | .154* | .069* |
| **Causal Effect of Sports Participation** [a] | (.007) | (.007) | (.016) | (.016) | (.026) | (.051) | (.094) | (.041) |
| **Reduced Form Results: Differential Effects of Title** | -.010*** | -.009*** | -.002* | -.001 | -.014 | -.013 | .018*** | .020** |
| **IX on Employment, by Stat e** [b] | (.003) | (.003) | (.004) | (.005) | (.010) | (.010) | (.010) | (.010) |
| **First-Stage Results: Changes in Female Sports** | .410 | .412 | .301 | .301 | .410 | .412 | .301 | .301 |
| **Participation Generated by Title IX** [c] | (.050) | (.050) | (.088) | (.087) | (.050) | (.050) | (.088) | (.087) |
| **Controls** | | | | | | | | |
| **Year*Race Fixed Effects** | NO | YES | NO | YES | NO | YES | NO | YES |
| **Year*Region of Birth Fixed Effects** | NO | NO | YES | YES | NO | NO | YES | YES |
| **State Male Unemployment Rate** | YES | YES | YES | YES | YES | YES | YES | YES |
| **Observations** | 576008 | 576008 | 576008 | 576008 | 64106 | 64106 | 64106 | 64106 |

Standard errors (shown in parentheses) are clustered at the level of 100 state-year cells. ***, **, and * indicate statistically significant at the 1%, 5% and 10% levels.

Source: 1980 and 1990 Censuses of Population (IPUMS) 5% sample (Ruggles and Sobek 1997). Data are for 49 states plus the District of Columbia. Iowa is excluded because it does not report girls sports participation in 1978. Data are for women aged 25-29 conditional on having completed 10th grade.

Specifications: (Linear probability model)

[a] IV estimates of causal effects of rising state female sports participation rates:

$$\textit{Employed in Male Occupation}_{i,s,t} = \alpha + \beta \ \textit{Female Athletic Participation}^{IV}_{s,t} + \sum_s \eta_s State_s + \sum_t \chi_t Year_t + \mathbf{X}_{i,s,t}\lambda + \varepsilon_{i,s,t}$$

[b] Reduced Form Results: Relationship between changing women's labor market outcomes, and the pre-existing levels of boys sports participation:

$$\textit{Employed in Male Occupation}_{i,s,t} = \alpha + \beta \ (\textit{Post Title IX}_t \ * \textit{Boys Athletic Participation}^{1971}_s) + \sum_s \eta_s State_s + \sum_t \chi_t Year_t + \mathbf{X}_{i,s,t}\lambda + \varepsilon_{i,s,t}$$

[c] 1st Stage Regression: Changes in girls sports participation by state generated by the interaction of Title IX and pre-existing levels of boys sports participation:

$$\textit{Female Athletic Participation}_{i,s,t} = \alpha + \beta \ (\textit{Post Title IX}_t \ * \textit{Boys Athletic Participation}^{1971}_s) + \sum_s \eta_s State_s + \sum_t \chi_t Year_t + \mathbf{X}_{i,s,t}\lambda + \varepsilon_{i,s,t}$$

All regressions include as controls a saturated set of dummy variables for state of birth, year of sample, age, race, ability to speak English , and disability level.

## APPENDIX A
## SUMMARY STATISTICS

| | | Mean | Standard Deviation | Min | Max |
|---|---|---|---|---|---|
| Boys' Athletic Participation 1971 | | .570 | .247 | .248 | 1.35 |
| Girls' Athletic Participation 1971 | | .048 | .050 | 0 | .208 |
| Boys' Athletic Participation 1978 | | .551 | .180 | .253 | 1.03 |
| Girls' Athletic Participation 1978 | | .286 | .145 | .111 | .697 |
| Years of Schooling | 1980 | 13.4 | .263 | 13..0 | 14.0 |
| | 1990 | 13.4 | .229 | 13.1 | 13.9 |
| Attended at least 2 years of college | 1980 | .357 | .054 | .261 | .494 |
| | 1990 | .322 | .056 | .235 | .437 |
| Attended at least 4 years of college | 1980 | .212 | .041 | .156 | .309 |
| | 1990 | .225 | .045 | .155 | .341 |
| Attended school beyond college | 1980 | .035 | .012 | .018 | .077 |
| | 1990 | .034 | .011 | .015 | .070 |
| Percent Employed (At time of Census Interview) | 1980 | .642 | .045 | .499 | .734 |
| | 1990 | .725 | .042 | .645 | .805 |
| Percent Employed Full-Year (Worked at least 42 weeks in the past year.) | 1980 | .476 | .050 | .332 | .592 |
| | 1990 | .590 | .050 | .495 | .674 |
| Percent Employed Full-Time (Usual hours of at least 35 per week.) | 1980 | .589 | .049 | .395 | .697 |
| | 1990 | .652 | .042 | .521 | .746 |
| Percent Employed Full-Year Full-Time (Worked 35+ hours for at least 42 weeks in the past year.) | 1980 | .413 | .050 | .252 | .535 |
| | 1990 | .509 | .050 | .382 | .606 |
| Percent Employed (At time of Census Interview, conditional on being never married, with no children, and a college degree) | 1980 | .912 | .023 | .862 | .977 |
| | 1990 | .932 | .020 | .903 | 1.00 |

Source: 1980 and 1990 Censuses of Population (IPUMS) 5% sample (Ruggles and Sobek 1997). Data are for 49 states plus the District of Columbia. Iowa is excluded from the sample because it does not report girls sports participation in 1978.

Notes: Data are for women aged 25-29 conditional on having completed 10th grade.

## APPENDIX B
## HISTORY OF TITLE IX LEGISLATION, REGULATION AND POLICY INTERPRETATION[32]

*"No person in the United States shall, on the basis of sex, be excluded from participation in, be denied the benefits of, or be subjected to discrimination under any education program or activity receiving Federal financial assistance."*

| | |
|---|---|
| 1972 | Congress enacts Title IX of the Educational Amendments of 1972, prohibiting sex discrimination in any education program or activity, within any institution receiving any type of Federal financial assistance. Historically single-sex, religious, and military schools are exempt from Title IX. *(Title 20 U.S.C. Sections 1681-1688)* Signed into law on June 23, 1972 by President Nixon. |
| 1974 | On May 20, 1974, Senator Tower introduced an amendment to exempt revenue-producing sports from any measure of Title IX compliance. The amendment was rejected. |
| 1974 | In lieu of the proposed Tower Amendment, Senator Javits introduced, in July 1974, a proposal stating that HEW must issue Title IX regulation including "with respect to intercollegiate athletic activities, reasonable provisions considering the nature of particular sports." |
| 1975 | HEW issues final Title IX regulation, which includes compliance measures and provisions prohibiting sex discrimination in athletics. |
| 1975 | On June 4, 1975 the present Title IX compliance regulation was transmitted to Congress. The next day (June 5, 1975) Senator Helms introduced a resolution in the Senate disapproving of the entire Title IX legislation. On June 17, 1975, Rep. Martin introduced two resolutions in the House. The first was similar to Senator Helms resolution and the second attempted to exclude intercollegiate athletics from Title IX. On July 16, 1975, Senators Laxalt, Curtis & Fannin introduce resolutions in the attempting to exclude intercollegiate athletics from Title IX. None of the attempts to curtail Title IX were adopted. |
| 1975 & 1977 | Two bills attempt to exclude revenue-producing sports from full-compliance with Title IX; both die in committees before reaching the House or Senate floors. Senator Helms introduces a bill in an attempt to prohibit the application of Title IX regulations to athletics where participation in those athletic activities are not a required part of the educational institution's curriculum. Senator Helms reintroduces the bill again in 1977. |
| 1978 | HEW issues proposed policy, in this policy the compliance is defined as substantially equal average per capita expenditures for men and women athletes and the potential for future expansion of opportunity and participation for women. |
| 1979 | Final policy interpretation is used by HEW. The policy focuses on institution's obligation to provide equal opportunity, rather than relying exclusively on a single compliance standard. |

[32] Information provided by *http://bailiwick.lib.uiowa.edu/ge/history.html*

| | |
|---|---|
| 1980 | Department of Education is established and given oversight of Title IX through the Office for Civil Rights (OCR). |
| 1981 | U.S. Department of Education releases a memo stating that Title IX prohibits sexual harassment. As such, any institution covered by Title IX must set up procedures for sexual harassment claims. |
| 1984 | The Supreme Court ruled in *Grove City vs. Bell* that institutions that discriminated on the basis of sex, race, age, national origin, or disability could continue to receive federal funding as long as the discrimination occurred only within non-federally funded programs. This decision removed the applicability of Title IX to many athletics programs. |
| 1988 | Congress reverses *Grove City vs. Bell* with the *Civil Rights Restoration Act*, which becomes law on 3/22/88 (after overriding a Presidential veto by President Ronald Reagan). With this legislation Congress clarified its intent that Title IX should apply to all educational institutions which receive any type of Federal financial assistance, whether it be direct or indirect.. |
| 1990 | Title IX Investigation Manual is published by the U.S. Dept. of Education through the Office for Civil Rights. |
| 1992 | On February 2, 1992 the Supreme Court ruled unanimously in *Franklin vs. Gwinnett County Public Schools* that plaintiffs filing Title IX lawsuist may receive punitive damages in cases where intentional action to avoid Title IX compliance is shown. |
| 1994 | Senator Mosley-Braun (S. 1468) and Rep. Collins (H.R. 921) sponsor the *Equity in Athletics Disclosure Act (EADA)*, stating that any co-educational institution of higher education participating in any Federal student financial aid program must make public information their regarding intercollegiate athletics program by publishing an annual report. The legislation passes and the first EADA disclosure report is due no later than October 1, 1996. |
| 1996 | OCR issues clarifications of three-part "Effective Accommodation Test" |
| 1996 | The first EADA report is due. As such, all institutions must have publicly available information on their intercollegiate athletics department. |

# APPENDIX C
## INSTRUMENTAL VARIABLES PLACEBO ESTIMATES OF THE EFFECTS OF FEMALE ATHLETIC PARTICIPATION ON THE EDUCATIONAL ATTAINMENT OF THE NON-TREATED

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| **Wald Estimator (IV)** | -.004 | .018 | -.117 | -.031 | -.135 |
| **Causal Effect of Sports Participation** [a] | (.145) | (.146) | (.135) | (.272) | (.263) |
| **Reduced Form Results: Differential Effects of** | -.002 | .007 | -.045 | -.008 | -.035 |
| **Title IX on Years of Education, by State** [b] | (.059) | (.057) | (.052) | (.071) | (.070) |
| **First-Stage Results: Changes in Female Sports** | .406 | .389 | .385 | .258 | .258 |
| **Participation Generated by Title IX** [c] | (.056) | (.049) | (.049) | (.091) | (.090) |
| **Controls** | | | | | |
| **Year*Race Fixed Effects** | NO | NO | YES | NO | YES |
| **Year*Region of Birth Fixed Effects** | NO | NO | NO | YES | YES |
| **State Male Unemployment Rate** | NO | YES | YES | YES | YES |
| **Observations** | 131544 | 131544 | 131544 | 131544 | 131544 |

Standard errors (shown in parentheses) are clustered at the level of 100 state-year cells.

Source: 1980 and 1990 Censuses of Population, IPUMS, 5% sample (Ruggles and Sobele 1997). Data are for 49 states plus the District of Columbia. Iowa is excluded because it does not report girls sports participation in 1978. Data are for women aged 40 conditional on having completed 10th grade.

Specifications:

[a] IV estimates of causal effects of rising state female sports participation rates:

$$Years\ of\ Schooling_{i,s,t} = \alpha + \beta\ Female\ Athletic\ Participation^{IV}_{s,t} + \sum_s \eta_s State_s + \sum_t \chi_t Year_t + \mathbf{X}_{i,s,t}\lambda + \varepsilon_{i,s,t}$$

[b] Reduced Form Results: Relationship between changing girls' educational outcomes, and the pre-existing levels of boys sports participation:

$$Years\ of\ Schooling_{i,s,t} = \alpha + \beta\,(Post\ Title\ IX\ Cohort_t * Boys\ Athletic\ Participation^{1971}_s) + \sum_s \eta_s State_s + \sum_t \chi_t Year_t + \mathbf{X}_{i,s,t}\lambda + \varepsilon_{i,s,t}$$

[c] 1st Stage Regression: Changes in girls sports participation by state generated by the interaction of Title IX and pre-existing levels of boys sports participation:

$$Female\ Athletic\ Participation_{i,s,t} = \alpha + \beta\,(Post\ Title\ IX\ Cohort_t * Boys\ Athletic\ Participation^{1971}_s) + \sum_s \eta_s State_s + \sum_t \chi_t Year_t + \mathbf{X}_{i,s,t}\lambda + \varepsilon_{i,s,t}$$

All regressions include as controls a saturated set of dummy variables for state of birth, year of sample, race, ability to speak English , and disability level.

## APPENDIX D
## INSTRUMENTAL VARIABLES PLACEBO ESTIMATES OF THE EFFECTS OF FEMALE ATHLETIC PARTICIPATION ON THE EMPLOYMENT STATUS OF THE NON-TREATED

| | Employed at time of the survey 1=Working, 0=Not Working | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| **Wald Estimator (IV)** | .040 | .055 | .020 | .046 | .013 |
| **Causal Effect of Sports Participation** [a] | (.038) | (.037) | (.035) | (.066) | (.061) |
| **Reduced Form Results: Differential Effects of Title** | .016 | .021 | .008 | .012 | .003 |
| **IX on Employment, by State** [b] | (.015) | (.014) | (.013) | (.016) | (.015) |
| **First-Stage Results: Changes in Female Sports** | .410 | .301 | .410 | .301 | .410 |
| **Participation Generated by Title IX** [c] | (.050) | (.087) | (.050) | (.087) | (.050) |
| **Controls** | | | | | |
| **Year*Race Fixed Effects** | NO | NO | YES | NO | YES |
| **Year*Region of Birth Fixed Effects** | NO | NO | NO | YES | YES |
| **State Male Unemployment Rate** | NO | YES | YES | YES | YES |
| **Observations** | 131544 | 131544 | 131544 | 131544 | 131544 |

Standard errors (shown in parentheses) are clustered at the level of 100 state-year cells.
Source: 1980 and 1990 Censuses of Population (IPUMS) 5% sample (Ruggles and Sobele 1997). Data are for 49 states plus the District of Columbia. Iowa is excluded because it does not report girls sports participation in 1978. Data are for women aged 40 conditional on having completed 10$^{th}$ grade.
Specifications: (Linear probability model)
[a] IV estimates of causal effects of rising state female sports participation rates:

$$Employed_{i,s,t} = \alpha + \beta \ Female\ Athletic\ Participation_{s,t}^{IV} + \sum_s \eta_s State_s + \sum_t \chi_t Year_t + \mathbf{X}_{i,s,t}\lambda + \varepsilon_{i,s,t}$$

[b] Reduced Form Results: Relationship between changing women's labor market outcomes, and the pre-existing levels of boys sports participation:

$$Employed_{i,s,t} = \alpha + \beta \ (Post\ Title\ IX_t \ * Boys\ Athletic\ Participation_s^{1971}) + \sum_s \eta_s State_s + \sum_t \chi_t Year_t + \mathbf{X}_{i,s,t}\lambda + \varepsilon_{i,s,t}$$

[c] 1$^{st}$ Stage Regression: Changes in girls sports participation by state generated by the interaction of Title IX and pre-existing levels of boys sports participation:

$$Female\ Athletic\ Participation_{i,s,t} = \alpha + \beta \ (Post\ Title\ IX_t \ * Boys\ Athletic\ Participation_s^{1971}) + \sum_s \eta_s State_s + \sum_t \chi_t Year_t + \mathbf{X}_{i,s,t}\lambda + \varepsilon_{i,s,t}$$

All regressions include as controls a saturated set of dummy variables for state of birth, year of sample, race, ability to speak English, and disability level.

## Chapter 2

# Effects of Divorce Laws on Suicide, Domestic Violence and Spousal Murder

Joint work with Justin Wolfers

### Abstract

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

## 1. Introduction

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

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

The existing work directly examining the effects of divorce law changes suggests that the reforms begun in 1969 may have caused divorce rates to rise.[3] Proponents of more restrictive divorce laws typically combine this finding with the observation that divorced people are known to exhibit a range of negative health and lifestyle outcomes to argue that any policy that increases access to divorce necessarily decreases well-being. This argument is further backed up with research showing that the financial position of women typically deteriorates following a divorce.[4] However, there are several reasons why these arguments are misleading. First, the estimated correlation between divorce

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[1]Historical accounts of this legislative movement indicate that it was catalyzed by reformers who were interested in preserving the integrity of the legal system. The courts had become filled with cases involving fraudulent charges of adultery and abuse as spouses attempted to divorce when the state's laws did not provide for divorce any other way. Jacob (1988).

[2] For the effects on children, see Gruber (2000).

[3] Friedberg (1998), although Wolfers (2000) presents evidence raising concerns about the robustness and interpretation of these findings.

[4] Holden and Smock (1991).

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

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

This paper exploits the variation occurring from the different timing of divorce law reforms across the United States, to evaluate changes in suicide, domestic violence, and spousal murder rates in an attempt to measure some of the important effects of the

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[5] In fact, the causation may run the other way. For example, although alcoholism may result from divorce, it is also likely that alcoholics make undesirable spouses.

"no-fault revolution". More specifically, we are examining suicide rates in an attempt to find a quantifiable measure of happiness and well-being. While variation in suicide rates only reflects changes at the extremes of the happiness distribution, Di Tella, MacCulloch and Oswald (1997) show that aggregate suicide rates tend to co-move with other aggregate measures of subjective well-being. Family violence surveys conducted in the mid-1970s, and again in the mid-1980s, provide basic detail about domestic violence. Spousal murder rates are analyzed as a further quantifiable indicator of domestic violence.[6]

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

## 2. Mediating Forces: Divorce Rates and Bargaining within Marriage

Our analysis is concerned with changes in adult well-being occurring as a result of a shift to unilateral divorce (which permits divorce upon application by either spouse) from the pre-existing divorce laws (which typically required either the consent of both spouses or a demonstration of marital fault). There are two mechanisms through which a

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

change in divorce law regime may affect indicators of spousal well-being. The first is by affecting the divorce rate. This direct mechanism traces the effects of easier access to divorce to higher divorce rates, through to the sorts of deleterious effects of divorce documented in the public health and sociological literatures.[7] If this were the only channel, then unilateral divorce laws would provide a useful instrumental variable for analyzing the adverse effects of divorce.

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

What can theory tell us about these two mechanisms? The first mechanism is mediated by rising divorce rates. The most prominent theoretical finding regarding the effect of divorce law regime is Becker's (1981) argument that divorce rates should be the same under either a unilateral or consent divorce regime. His argument is a straightforward application of the Coase Theorem, which holds that, under certain conditions, outcomes should not vary according to who holds a particular property right. When divorce law changes from a consent to a unilateral framework an important property right - the right to remarry - is transferred from the spouse who wants to remain married to the partner desiring a divorce. According to the Coase Theorem this will not affect the decision to divorce. If the Coase theorem holds then there should be no change

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

in adult well-being occurring through the first mechanism, since there should be no change in the divorce rate.

However, the Coase theorem relies on four strong assumptions: contractability, costless bargaining, symmetric information, and transferability, all of which are potentially problematic with respect to divorce negotiations. While previous research suggested that an increase in divorce rates occurred as a result of the change to unilateral divorce, conflicting evidence indicates that the increase in divorce rates was only transitional, disappearing after a decade.[8] Either way it is possible that changes in well-being may be mediated by induced changes in divorce propensities.

The second channel reflects the impact of divorce laws on spousal bargaining over the distribution of marital rents. There are three canonical models of intrafamily distribution. The first is the *common preference* approach, which holds that families act *as if* maximizing a single utility function. This common preference can be motivated by either love (altruism, such that both spouses care equally about their own and their partner's satisfaction, as in Becker, 1981), or the parties seeking to maximize a "social welfare function," agreed upon in a complete marriage contract.[9] The sharpest prediction of the common preference approach is that outcomes are invariant to the distribution of resources between spouses. By contrast, bargaining models hold that the presence of threat points determines intrafamily distribution. In the *separate spheres* bargaining model of Lundberg and Pollak (1993), these threat points are internal to the marriage. That is, the equilibrium distribution is maintained by the threat of reversion to a non-

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[8] See Friedberg (1998) and the response in Wolfers (2000).

[9] In the latter case the division of marital rents is agreed upon prior to the marriage.

cooperative equilibrium involving, for example, burnt toast or sleeping on the sofa. By contrast, the *exit threat* bargaining models of Manser and Brown (1980) and McElroy and Horney (1981) emphasize external threat points - specifically each party's best option outside the marriage. If this exit threat is binding, then changing opportunities outside the marriage will change the equilibrium outcome within the marriage. If the internal threat is binding, then such changes do not affect outcomes.[10]

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

Although theory predicts that real effects can flow from divorce reform, signing these effects is much harder. An increase in access to divorce could decrease suicide

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[10] As Lundberg and Pollak acknowledge both threat points may be possible, and therefore the relevant threat point is determined by institutional frameworks and individual utility functions (1993, p. 1001).

rates simply because suicide and divorce might be substitutes. That is, while the misery of living in an abusive, or otherwise unhappy relationship may result in suicide, the option of divorce and remarriage may avert this course of events.[11] Alternatively, if more divorces occur as a result of the legal change then there will be an increase in abandoned, and unhappy, spouses, thereby raising the temptation of suicide.

Similarly, murder may be used to escape from a bad marriage. As less costly ways to escape the marriage arise, we would expect to see substitution away from spousal murder. Both murder and other forms of domestic violence may decrease simply because the threat to leave if beaten becomes credible under the unilateral divorce framework. If the abuser wishes to continue the marriage then this threat may be sufficient to prevent abusive behavior. Note that this change in behavior results from the change in bargaining power and, as such, can occur without any observed change in divorce propensities. Finally, most spousal homicides occur in the context of abusive relationships,[12] and hence any policy that reduces the barriers to exiting such a relationship reduces the probability of both abuse and spousal homicide.

Countering these forces, there are several reasons why unilateral divorce may raise intimate homicide and domestic violence rates. The first is that without a legal system that enforces the marriage contract, individuals may substitute private for public enforcement of their marriage contracts. Under the consent divorce framework, spouses "owned" each other, and this ownership was enforced by the state through legal sanction.

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[11] For an analysis of suicide that emphasizes the option value of staying alive, see Hamermesh and Soss (1974).

[12] Campbell (1992).

Under unilateral divorce this perceived property right is threatened; hence, there exists the possibility that private enforcement, through violence, will substitute for state sanctions.[13] The resulting increase in domestic violence may also lead to an increase in murder. Finally, the intense emotional distress and personal tumult associated with divorce proceedings might provoke an increase in domestic violence and murder. That is, unilateral divorce, in so much as it leads to an increase in the number of highly charged divorces, creates more violent situations.[14]

## 3. Empirical Strategy

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

Of the fifty states, four are yet to adopt any form of unilateral divorce: Arkansas, Delaware, Mississippi, and New York. Of the forty-six states that currently have unilateral divorce regimes, ten had adopted some variant of unilateral divorce before the

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

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

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

no-fault revolution of the early 1970s. Along with the thirty-six remaining states we include the District of Columbia, which adopted unilateral divorce in 1977. Consequently, we effectively have thirty-seven "experiments" of changing divorce laws. The remaining fourteen states are included as controls. Table 1 lists the year that each state changed its divorce regime to allow unilateral divorce.

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

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

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

**States adopting Unilateral Divorce Laws:**

| | |
|---|---|
| **1969** | Kansas, South Carolina |
| **1970** | California, Iowa |
| **1971** | Alabama, Colorado, Florida, Idaho |
| **1972** | Kentucky, Michigan, Nebraska |
| **1973** | Arizona, Connecticut, Georgia, Hawaii, Indiana, Maine, Missouri, New Mexico, Nevada, Oregon, Washington |
| **1975** | Massachusetts, Montana |
| **1976** | Rhode Island |
| **1977** | Washington DC, Wisconsin |
| **1980** | Pennsylvania |
| **1984** | Illinois |
| **1985** | South Dakota |

**Continuing Consent Divorce States (as of 1996):**

Arkansas, Delaware, Mississippi, New York, Tennessee

Source: Friedberg (1998)

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

## 4. Suicide Results

Data on suicide comes from the National Center for Health Statistics (NCHS).[16] The NCHS data are a census of death certificates, which code the cause of death for all deceased persons. There are broad codes for suicide, as well as a more detailed coding structure that includes data on the method of suicide. Individual data on gender, state of residence, and age of death are also collected.[17] By examining the period from 1964

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[16] Suicide data for 1964-1967 were hand entered from annual editions of the NCHS report "Vital Statistics: Mortality, Vol.2". Data for 1968-78 are calculated from ICPSR Study No. 8224, "Mortality Detail Files: External Cause Extract, 1968-78", PI: National Center for Health Statistics. Data from 1979-96 have been downloaded from the Center for Disease Control's Wonder system which accesses the NCHS "Compressed Mortality Files" (http://wonder.cdc.gov/). Apart from minor revisions to the International Classification of Diseases, these data are consistently coded.

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

through to 1996, we can both robustly identify suicide rates before the adoption of unilateral divorce laws, and trace their evolution over the following years.

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

We employ OLS with robust standard errors to estimate for men and women separately:

$$\textit{Suicide rate}_{s,t} = \alpha + \beta \; \textit{Unilateral}_{s,t} + \sum_s \eta_s \textit{State}_s + \sum_t \lambda_t \textit{Year}_t + \varepsilon_{s,t}$$

where *Unilateral* is a dummy variable that is equal to one if a state has unilateral divorce in that particular year, and is equal to zero otherwise.

As can be seen in Table 2, there is a large and statistically significant reduction in the female suicide rate following the change to unilateral divorce, with the point estimate suggesting nearly a six-percent decline. For male suicides, the specific point estimate suggests that there is no discernible effect, however the confidence interval is sufficiently wide as to be consistent with a meaningful effect in either direction.

**Table 2: Effects of Unilateral Divorce on Suicide Rates**

| | No Controls | | State-level controls | |
|---|---|---|---|---|
| | Female | Male | Female | Male |
| | *(1)* | *(2)* | *(3)* | *(4)* |
| **Mean Suicide Rate** | 54 per year per million women | 202 per year per million men | 54 per year per million women | 202 per year per million men |
| **Effect of Unilateral Divorce** | -5.6%*** (2.2) | -0.8% (1.2) | -5.6%** (2.3) | -1.6% (1.1) |
| **Adj. $R^2$** | **.667** | **.816** | **.669** | **.822** |
| **State fixed effects** | ✓ | ✓ | ✓ | ✓ |
| **Year fixed effects** | ✓ | ✓ | ✓ | ✓ |
| **Female-to-Male Employment Rate** | | | ✓ | ✓ |
| **Unemployment Rate** | | | ✓ | ✓ |
| **Age composition** | | | ✓ | ✓ |
| **Racial composition** | | | ✓ | ✓ |

Sample 1964-1996, n=1683.

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

Dependent variable is the suicide rate. Coefficients are reported as the percentage change in the suicide rate due to Unilateral divorce laws; this elasticity is calculated using the unweighted cell mean as the base. Robust standard errors are in parentheses. All controls are state-level aggregates, constructed from the Unicon March CPS files. For consistency, CPS variables refer to the population aged 14 years or greater. Controls for age composition indicate the share of states' populations aged 14-19, and then ten-year cohorts beginning with age 20-29, to a variable for 90+. Controls for racial composition include variables describing the share of the state's population that is black, white and other.

We test the sensitivity of our specification to a range of controls, including a proxy for the evolving strength of the feminist movement, indicators of the business cycle, and the racial and age composition of the state. While we find that some of these controls are significant explanators of the suicide rate, Table 2 shows that they barely change the estimated effect of unilateral divorce.

Timing evidence might speak to a causal interpretation of these results. We are particularly interested in whether the change in suicide post-dated the change in divorce regime, and whether adjustment to the new regime seems plausible. Presumably there are important lags in people understanding the new law, in spouses understanding their new bargaining power, in murder and suicide decisions, and probably most importantly, for all of this to affect social norms about marriage, domestic violence and divorce. In order to examine these potential lags, we replace the single dummy variable *Unilateral* in our baseline regression with eleven dummy variables. The first dummy indicates the year that the divorce law was changed and the remaining ten dummies indicate whether unilateral divorce has been in effect for 1-2 years, 3-4 years, 5-6 years and so on; periods beyond 20 years are coded to the 19-20 groups so as to capture the long run effect of the policy change.

Equally important, we look at leads, coding dummies for whether unilateral divorce will become law in 1-2 years, 3-4 years and so on, with leads beyond 10 years coded to the 9-10 year group. The leads are included to check if the timing of the decrease in suicide rates corresponds with the change to unilateral divorce. The estimated coefficients are shown in Figures 1 and 2 (the graphs are normalized so that the pre-change effects are centered on zero).

**Figure 1**

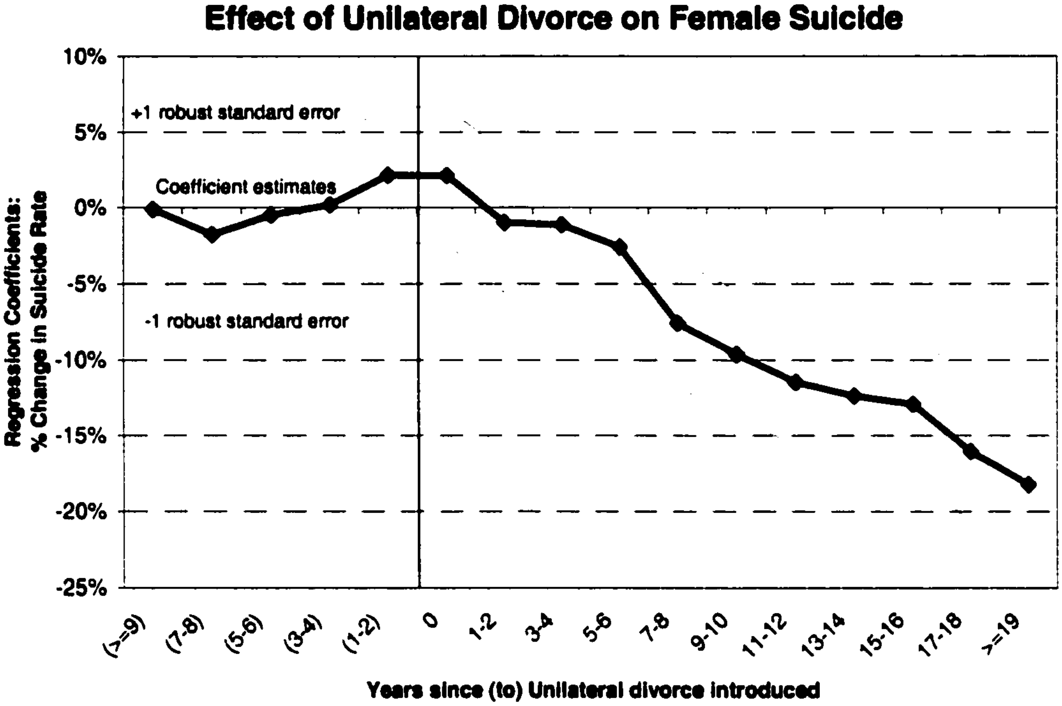
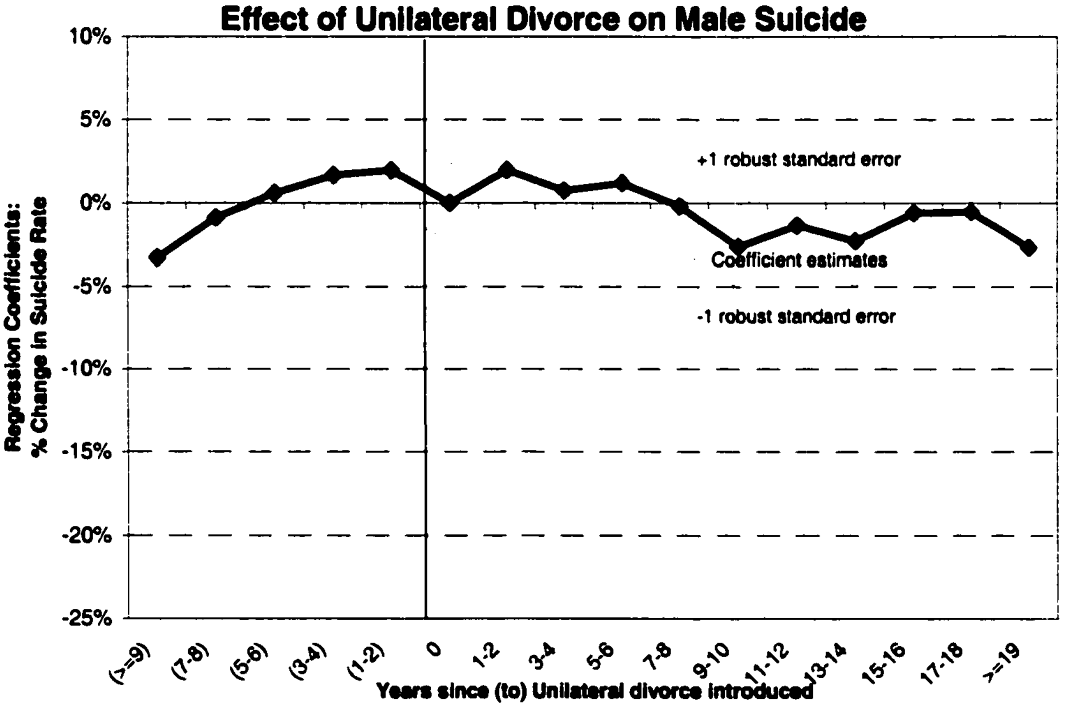



**Figure 2**




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

Firstly, note that the coefficients on the dummies indicating the period prior to the divorce law reform are all close to zero, and in no case are they (individually or jointly) statistically distinguishable from zero. This speaks clearly to a causal interpretation of these results. Secondly, reinforcing our baseline results, the graphs show that there was a large and statistically significant decrease in female suicide rates, and no discernable affect on male rates. The smooth and plausible shape of the response of female suicide suggests that our treatment effect is reasonably well identified. The graph illustrates that the full effects of the change in divorce laws take almost twenty years to percolate through society. Furthermore, because our baseline regressions in Table 2 average the effect of unilateral divorce through the entire post-divorce period (including the transition), they understate the full long-run impact. With female suicides, we see that the long run effect is more than twice that shown in Table 2 – close to a 20 per cent decline.

**Table 3: Effects of Unilateral Divorce on Suicide Rates**

| | Female Suicides | | | | Male Suicides | | | |
|---|---|---|---|---|---|---|---|---|
| Column No. | (1f) | (2f) | (3f) | (4f) | (1m) | (2m) | (3m) | (4m) |
| **Year of Change** | 1.6%<br>(3.8) | 1.4%<br>(3.8) | 7.2%<br>(8.7) | 2.8%<br>(3.2) | -0.8%<br>(2.2) | -1.0%<br>(2.2) | 2.9%<br>(2.2) | -1.8%<br>(2.2) |
| **1-4 years later** | -1.6%<br>(2.8) | -1.5%<br>(2.8) | 6.8%<br>(4.5) | 0.5%<br>(3.2) | 0.6%<br>(1.3) | 0.3%<br>(1.2) | 3.5%<br>(1.4) | -0.9%<br>(1.4) |
| **5-8 years later** | -5.4%<br>(2.6) | -5.3%<br>(2.6) | 0.4%<br>(3.2) | -1.7%<br>(3.8) | -0.1%<br>(1.5) | -0.5%<br>(1.5) | 2.0%<br>(1.3) | -2.0%<br>(1.9) |
| **9-12 years later** | -10.1%<br>(3.0) | -10.5%<br>(2.7) | -6.4%<br>(3.0) | -5.1%<br>(4.7) | -2.7%<br>(1.6) | -3.1%<br>(1.6) | 0.0%<br>(1.4) | -4.7%<br>(2.5) |
| **13-16 years later** | -12.8%<br>(2.9) | -12.6%<br>(3.0) | -12.9%<br>(3.6) | -5.0%<br>(5.9) | -2.5%<br>(1.8) | -3.1%<br>(1.8) | -0.1%<br>(1.6) | -4.5%<br>(3.2) |
| **≥ 17 years later** | -17.6%<br>(2.9) | -17.1%<br>(3.0) | -25.2%<br>(3.9) | -7.5%<br>(7.5) | -3.0%<br>(1.9) | -3.9%<br>(1.9) | -4.6%<br>(1.8) | -5.0%<br>(3.9) |
| **F-test of joint significance** | p=0.00 | p=0.00 | p=0.00 | p=0.54 | p=0.26 | p=0.16 | p=0.00 | p=0.53 |
| **Estimation method** | OLS | OLS | WLS | OLS | OLS | OLS | WLS | OLS |
| **<u>Control variables</u>** | | | | | | | | |
| **State and year fixed effects** | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| **Female-to-Male Emp. Rate** | | ✓ | ✓ | ✓ | | ✓ | ✓ | ✓ |
| **Unemployment Rate** | | ✓ | ✓ | ✓ | | ✓ | ✓ | ✓ |
| **Age composition** | | ✓ | ✓ | ✓ | | ✓ | ✓ | ✓ |
| **Racial composition** | | ✓ | ✓ | ✓ | | ✓ | ✓ | ✓ |
| **State-specific time trends** | | | | ✓ | | | | ✓ |

Sample 1964-1996, n=1683.

Dependent variable is the suicide rate. Coefficients are reported as the percentage change in the suicide rate due to the adoption of Unilateral divorce laws the stated number of years ago; this elasticity is calculated using the unweighted cell mean as the base. Robust standard errors are in parentheses. All controls are state-level aggregates, constructed from the Unicon March CPS files. For consistency, CPS variables refer to the population aged 14 years or greater. Controls for age composition include variables indicating the share of states' populations aged 14-19, and then ten-year cohorts beginning with age 20 up to a variable for 90+. Controls for racial composition include variables describing the share of the state's population that is black, white and other.

Clearly, the dynamics of the adjustment process are important, and Table 3 presents our preferred results, mapping out the dynamic effect of divorce law reform. This table shows the coefficients for a specification similar to that used in Figure 1, although for ease of exposition the specification is simplified so that there are dummy variables for the first four years after unilateral divorce has been in place and four-year intervals thereafter. We subjected this specification to significant robustness checking. Column one shows the basic results. Column two shows that these results are robust to the inclusion of a range of controls. Weighted least squares results are also broadly similar (column 3). We add state-specific time trends in column 4, finding that their inclusion causes the standard errors to increase. For women, the specification including state-specific time trends is not precisely estimated enough to reject either the pattern of coefficients follows that shown in columns 1-3, or the null of no effect. For males, including state-specific trends is suggestive of a decline in male suicide rates following the advent of unilateral divorce.

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

**Interpretation**

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

A more restrictive interpretation is that our results are simply the reduced form representation of an instrumental variables regression in which unilateral divorce laws are an instrument for higher divorce rates. This IV interpretation assumes that the decrease in female suicide is solely the result of higher divorce rates and does not reflect bargaining within marriage. With no direct evidence as to the presence or absence of such a channel, we are reluctant to embrace this more restrictive interpretation. Further, the indirect evidence that we have speaks weakly against this view. Specifically, note that in Figure 1 that there is no immediate spike in suicide following the regime change. By contrast, a spike in divorce typically follows a shift in regimes – as the courts cater to pent-up demand for unilateral divorces.[18]

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[18] This heuristic argument is equivalent to the type of argument in formal over-identification tests. The first stage regression is formally over-identified in that we have separate instruments for a rise in divorce based on dummies of whether the legal change occurred in the last two years, 3-4 years ago

Finally, Wolfers (2001) casts doubt on Friedberg's (1998) assessment of the relationship between divorce laws and divorce rates. Friedberg's result relies upon the inclusion of state-specific time trends that are calculated on data that include only a few years of data prior to the legal change and, in some cases, only one year. As a result the estimated state-specific time trends reflect the dynamic response of the divorce rate to the regime change, rather than pre-existing divorce trends in each state. By partialling out a downward trend in divorce in unilateral divorce states, Friedberg finds an artificially large rise in divorces following the legal change.[19] When these trends are omitted, or are calculated so as to reflect only pre-existing trends in a state's divorce rate, Wolfers concludes that the adoption of unilateral divorce caused the divorce rate to be higher for a decade, and then lower in the ensuing decade. By contrast, Figure 1 suggests that suicides decline steadily over the twenty years following divorce reform. Thus, either divorce is not the sole intermediating step, or somehow the suicide data does a better job than the divorce data at revealing the underlying relationship between divorce rates and divorce laws. These interpretation issues remain relevant as we now turn our attention to the relationship between unilateral divorce and intimate homicide and domestic violence.

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etc. The heuristic argument is that the large change in divorce generated in the first two years after a regime change yields only a small decline in suicide rates, a result that is difficult to reconcile with smaller changes in divorce rates in ensuing years that yield much larger effects on suicide. Of course, appealing to a specific constellation of lags between the rise in divorce and the decline in suicide can rationalize this result away.

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

## 5. Domestic Violence

The most credible cross-state data on domestic violence are the landmark Family Violence Surveys undertaken by sociologists Straus and Gelles in 1976 and again in 1985.[20] These data come from household interviews that ask how couples resolve conflict.[21] This type of survey instrument typically yields higher estimates of domestic violence than police reports or crime victimization surveys because the victim need not perceive the act as domestic violence and/or a crime for it to be recorded. While still an imperfect survey instrument, Markowitz (1999, p.8) argues that this methodology is currently "the best available technique for collecting truthful information on domestic violence".

The two available surveys yield cross-sectional data for 1976, by which time thirty-one states had recently changed their divorce laws, and again for 1985, by which time all thirty-seven regime changes identified in Figure 1 had occurred. This timing is somewhat unfortunate in that it is unclear how the differential timing of reform across states would translate into differential changes in domestic violence rates over the 1976-85 period. Yet, although the differential cross-state timing in reform yields little analytical leverage, we can compare changes in violence rates among our thirty-seven states that constituted the "no-fault revolution" with two alternative control groups: the five states that are yet to adopt unilateral divorce (AR, DE, MS, NY and TN), and the nine states whose pre-existing regime involved unilateral divorce (AK, LA, MD, NC,

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

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

OK, UT, VA, VT and WV).[22] If there is an underlying relationship between domestic violence and divorce regime, we would expect to observe changing violence propensities in the treatment group relative to the controls. Because the survey universe consists only of couples living in a conjugal unit, we are limited to analyzing rates of domestic violence within intact marriages. Thus, we cannot disentangle whether the estimated effects reflect a decreasing propensity towards spousal violence, or an increasing propensity for abused spouses to exit their marriages.

Table 4 shows illustrative differences-in-differences estimates of the effects of unilateral divorce on domestic violence.[23] The first row of Panel A tells us that between 1976 and 1985 domestic violence towards women declined by 1.7 percentage points in reform states, while it rose 2.5 percentage points in the control states. Thus, the difference-in-difference estimate suggests that the treatment - adoption of unilateral divorce - led domestic violence rates to decline by 4.3 percentage points, or by around one-third, over the 1976-85 period. Panel B shows a slightly smaller, statistically insignificant decline in wife-to-husband violence. These magnitudes are clearly important and lead us to the micro-data to probe this result more intensively.

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

[23] The definition of domestic violence follows Gelles and Straus (1994). That is, we code domestic violence as occurring if there has been any incident over the last year in which a person threw something at their partner, pushed, grabbed, shoved, slapped, kicked, bit, hit with fist, hit or tried to hit with object, beat up, or threatened or used a gun or knife against their partner.

## Table 4

**Differences-in-Differences: Effects of Divorce Reform on Domestic Violence**

***Panel A: Husband to Wife Violence***

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

***Panel B: Wife to Husband Violence***

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

Sample: $n_{1976}$=2102; $n_{1985}$=3874 (includes cross-section and state over-samples, excludes observations from states that are not present in the 1976 data; sampling weights are applied). *, **, *** denote significance at the 10%, 5%, and 1% levels respectively. (Robust standard errors in parenthesis; standard errors corrected for clustering within 72 state-year cells)

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

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

Table 5 analyzes household-level data in which the dependent variable, *Domestic Violence* is a dummy indicating whether the specified type of violence occurred within each household. We estimate the micro-data analog of our differences-in-differences estimate:

$$\begin{aligned} \textit{Domestic Violence}_{i,s,t} &= \beta(\textit{Treatment}_s \times \textit{Year}_t^{1985}) + \delta \textit{Treatment}_s + \textit{year effects}_t \\ &\quad (+\textit{state effects}_s + \textit{controls}_i) + \varepsilon_{i,s,t} \end{aligned}$$

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

The first row of Table 5 shows the mean rates of violence across households. Perhaps surprisingly, men are as likely to be physically abused by their spouses as women are. The next row simply reproduces the differences-in-differences estimates from Table 4, for each of the four categories of spousal abuse. Extremely large declines in violence are found for each abuse indicator. Adding state fixed effects in the next row sharpens these estimates somewhat, and these large effects are all found to be statistically significant. The following two rows show that these results are robust to the inclusion of a rich set of individual-level controls. Dropping specific states from the sample did not appreciably change these results.

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

**Table 5: Effects of Unilateral Divorce on Domestic Violence**

| | Overall Violence[a] | | Severe Violence[a] | |
|---|---|---|---|---|
| | Husband to Wife | Wife to Husband | Husband to Wife | Wife to Husband |
| | Average Incidence of Each Type of Violence | | | |
| | 11.7% | 11.9% | 3.4% | 4.6% |
| **Estimated Change in Violence Rates in Treatment states relative to Control states** | | | | |
| **OLS (Diffs-in-diffs)** | -4.3%** | -2.7% | -1.1% | -2.9%*** |
| | (1.9) | (1.8) | (1.3) | (1.0) |
| **add state fixed effects** | -5.5%*** | -3.3%** | -2.0%** | -3.6%*** |
| | (1.8) | (1.5) | (1.0) | (0.7) |
| **add individual controls**[b] | -4.8%*** | -2.0% | -1.8%* | -3.3%*** |
| | (1.7) | (1.4) | (1.0) | (0.9) |
| **Probit with individual controls**[b] | -4.5%*** | -2.1% | -1.2%* | -2.0%*** |
| | (1.5) | (1.3) | (0.7) | (0.7) |

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

*,**,*** denote significance at the 10%, 5%, and 1% levels respectively. (Robust standard errors in parentheses, corrected for clustering within 72 state-year cells). All regressions include year fixed effects and either state fixed effects, or treatment/control fixed effects.

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

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

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

## 6. Intimate Homicide

Our data on homicide come from the FBI Uniform Crime Reports (UCR).[24] The UCR data are derived using a voluntary police agency-based reporting system. The Supplementary Homicide Reports of the UCR provide *incident-level* information on criminal homicides, including data describing the date and location of the incident, and a range of information on both the offender and the victim. The particular richness of this data is that it codes the relationship of the victim to the murderer, where known.

Because the FBI data rely on police reporting there are often problems of under-reporting or downgrading of crimes. However, the nature of homicide means that both of these problems are minimized. The FBI counts of total murders each year by state were checked against the independently gathered NCHS murder count. Generally, these two data sources were consistent, and hence the rest of our analysis uses the FBI data,[25] which include their coding of victim-perpetrator relationships.

Nonetheless, there remains a range of problems working with these data. First, the participation of agencies is not completely consistent, and when an agency fails to report in a particular month, we cannot tell whether this reflects laxity with paperwork or

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

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

that there were no murders to report.[26] Second, there are various coding breaks arising from the changing definitions of victim-perpetrator relationship, causing a minor break in 1972, and a more important break in 1976. These coding breaks present a problem for our analysis because, conceptually, we would like to capture any relationship that may be affected by changes in family law. Such relationships include, along with spouses, domestic and non-domestic romantic partners, and other family members (particularly children). However, there are data problems constructing such a series that is consistent across coding breaks.[27] In this section we will examine three successively broader definitions of intimate homicide. The narrowest only includes spousal homicide, the next group includes homicides committed by any family member or romantic interest, and finally we expand our treatment group to our broadest categorization, which includes all homicides committed by non-strangers.

The defect of the broader measures is that the treatment group is defined to include many relationships not affected by the treatment of unilateral divorce. The defect of narrower measures is that police classifications of victim-perpetrator relationships as "spousal" are likely to have changed over time, in a way that is correlated with family law regimes, leading to (difficult to sign) bias issues.[28] Further, identifying intimates

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

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

[28] While the coding of married partners as "spouses" presents no difficulty, coding of common-law marriages, cohabiting couples, same-sex couples, romantic partners and separated spouses is likely to have changed over time. Although these groups may be small compared to the whole population, we do not know if this is true of the homicidal population. All that is known with certainty is that a

narrowly, such as by "spouses", is more likely to suffer from endogeneity problems as the legal status that people choose for their relationships may change with changes in the legal regime.

For women murdered, Table 6 suggests a large and significant decline following the adoption of unilateral divorce for all three definitions of intimate homicide. The results for males murdered are imprecisely estimated, and would admit large effects in either direction. Once again, we find that adding controls has little effect on the coefficient of interest.

As with the suicide data, timing evidence might assist us in interpreting our results. Therefore, we once again replace the single dummy variable *Unilateral* in the baseline model with several dummy variables indicating the number of years since (or until) the law went (goes) into effect. We run this regression for all three categories of intimate homicide. The estimated coefficients for females murdered are shown in the Figure 3. For clarity, standard error bands are not shown, but as a rough indicator, estimated standard errors for each lead, or lag, plotted are around twice that shown in the corresponding row of Table 6. The imprecision with which we estimate effects on males murdered is sufficiently large that we omit them from the rest of the analysis of intimate homicide.

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homicidal member from one of the above groups would not have been coded as a stranger, which is the motivation for looking at the broadest of our definitions of the treatment group.

**Table 6: Effect of Unilateral Divorce on Intimate Homicide (% change)**

| Dependent Variable | Mean Annual Homicide Rate *(Homicides per million men or women)* | Effect of Unilateral Divorce Laws | |
|---|---|---|---|
| | | No controls | Including controls[#] |
| **Females Murdered** | | | |
| **by Spouse** | 7.3 | -10.5%* (5.9) | -10.5%* (6.1) |
| **by Family Member** | 14.7 | -8.9%** (4.4) | -9.3%** (4.4) |
| **by Non-Stranger** | 21.2 | -8.7%** (3.7) | -8.8%** (3.7) |
| **Males Murdered** | | | |
| **by Spouse** | 5.5 | +12.3% (9.2) | +8.4% (9.3) |
| **by Family Member** | 18.4 | +1.9% (5.3) | -0.9% (5.3) |
| **by Non-Stranger** | 56.9 | -2.0% (3.1) | -2.8% (3.1) |

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

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

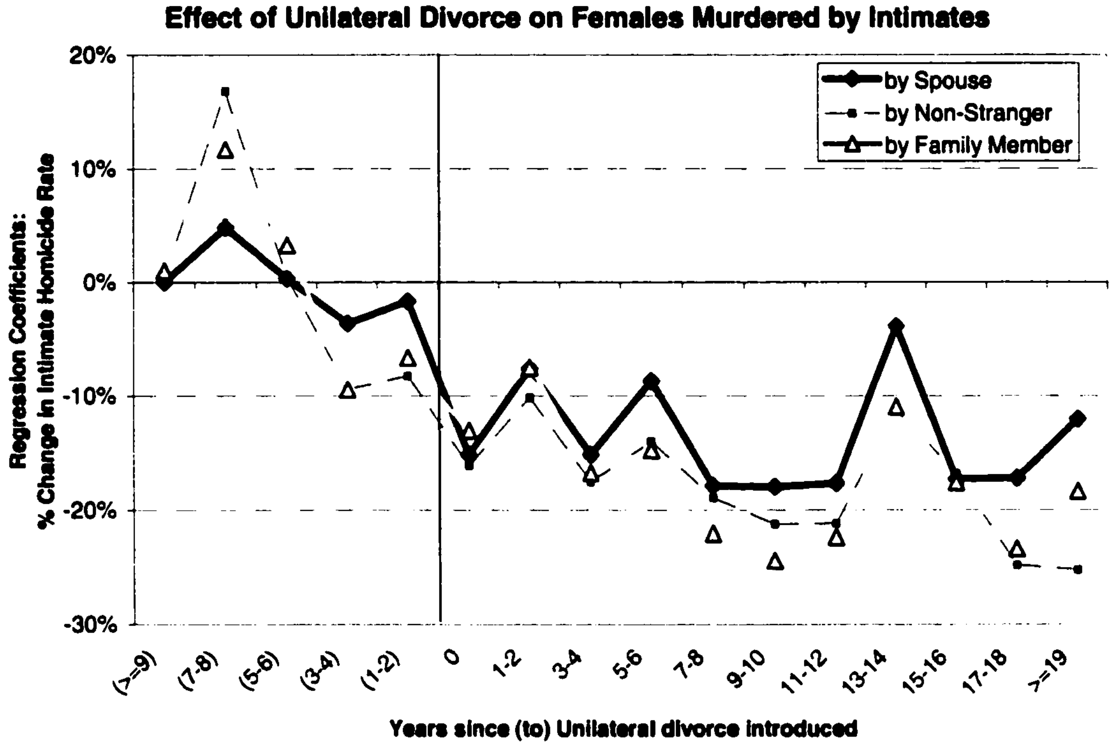
(Robust standard errors in parentheses.)

Dependent variable is the annual intimate homicide rate in each state. Each row reports a separate regression, focussing on a different definition of "intimate homicide". Reported coefficients reflect the percentage change in the relevant homicide rate attributed to Unilateral Divorce laws; calculated using the unweighted cell mean as the base.

All regressions include (significant) state and year fixed effects.

[#] Controls include the state unemployment rate, female-to-male employment rates, and the age and race structure of the state's population.

Figure 3




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

Figure 3 confirms the initial findings of a decrease in women murdered in the period following the passage of divorce law reforms. However, the timing evidence is somewhat worrying, and the reader is left to judge whether the decline in homicide pre-dated the legal change to an extent that undermines our results. This raises the possibility that our regression results may be picking up the effects of some alternative phenomenon that pre-dated divorce law reform. Although, as can be seen in Table 6, the coefficients were basically unchanged by the addition of controls.

The fact that family law affects behavior between intimates but not between strangers, provides an opportunity to further probe these results. Specifically, *non*-intimate homicide may serve as an ideal placebo group for a differences-in-differences-in-differences estimate. Table 7 provides simple differences-in-differences (panel) estimates for both the treatment and placebo groups.

The first column simply reproduces results familiar from Table 6, where the dependent variable is a particular definition of intimate homicide. Again we see that the decline in females murdered is statistically different from zero for all three definitions of "intimate". The second column reports the results from analogous regressions, where the dependent variable is equal to the aggregate homicide rate, less the corresponding intimate homicide rate. That is, it reports the effect of unilateral divorce on non-intimate homicide. In each case we find that homicide rates among the placebo groups are not statistically significantly related to divorce regime.

These results also give us a chance to assess an alternative counterfactual. Instead of assuming that in the absence of divorce reform that intimate homicide would remain unchanged (as in the first column), a differences-in-differences-in-differences estimate assumes that the change in non-intimate homicide is the relevant baseline. The final column of Table 7 shows these estimates, finding that intimate homicide declined when compared with this counterfactual, but that this difference is not statistically significant.

**Table 7: "Effect" of Unilateral Divorce on Treatment and Placebo Groups**

| | Dependent Variable: Women Murdered, by Relationship to Perpetrator | | |
|---|---|---|---|
| **Definition of "Intimate"** | **Intimate homicide** ***(Treatment)*** | **Non-intimate homicide** ***(Placebo)*** | **Diff-in-Diff-in-Diff Estimate** ***(Treatment-Placebo)*** |
| **by Spouse** | -10.5%* | -5.9% | -4.6% |
| | (5.9) | (3.6) | (6.9) |
| **by Family Member** | -8.9%** | -5.2% | -3.8% |
| | (4.4) | (4.2) | (6.0) |
| **by Non-Stranger** | -8.7%** | -3.4% | -5.3% |
| | (3.7) | (5.1) | (6.3) |

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

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

(Robust standard errors in parentheses.)

Dependent variable is the annual intimate, or non-intimate homicide rate in each state. Each row reports a separate regression, focussing on a different definition of "intimate homicide". Non-intimate homicide (the placebo category) includes all homicides not fitting the particular definition of intimate homicide. Reported coefficients reflect the percentage change in the relevant homicide rate attributable to Unilateral divorce (calculated using the unweighted cell mean as the base).

All regressions include (significant) state and year fixed effects.

## 7. Conclusion

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

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

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

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Appendix A: Summary Statistics

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

**Appendix A Continued**

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

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

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

## Appendix B: Coding of FBI Murder Data

Our data on homicide come from the FBI Uniform Crime Reports (UCR).[29] The UCR data are derived using a voluntary police agency-based reporting system. The Supplementary Homicide Reports of the UCR provide *incident-level* information on criminal homicides, including data describing the date and location of the incident, and a range of information on both the offender and the victim. This data codes the relationship of the victim to the murderer, where known. We would like to be able to look exclusively at murders that may be affected by unilateral divorce. Therefore we are looking for cases in which the perpetrator maybe motivated by the laws pertaining to divorce – intimate homicides. A useful definition of the treatment group should include intimates such as spouses, ex-spouses, other partners, and other family members. However, relationships such as common-law spouse, boyfriend/girlfriend, and even ex-spouse are not consistently coded through our sample. While time-fixed effects will effectively difference out inconsistencies that are common across states, we are worried that family law reform may have changed the common meanings of certain definitions of the treatment group. (For instance the distinction between marriage, common-law marriage and live-in partners has changed with the social meaning of these terms, which has in turn been affected by family law.[30]) As such we can think of a bias/efficiency tradeoff. The most efficient strategy is based on a narrow definition of intimate homicide that includes only spouses, but runs the risk that this category is not well-defined through the sample period. At the other extreme, perhaps the safest identification strategy is to assume that the legal regime did not affect murder between strangers, but did affect murder where the victim is known to the murderer – in any capacity.

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

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

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

**Alternative definitions of the "Treatment Group"**

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

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

# Chapter 3

## THE IMPACT OF DIVORCE LAWS ON MARRIAGE-SPECIFIC CAPITAL

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 a useful quasi-experiment with which to examine the effects of this change. Data from the 1970 and 1980 censuses is analyzed to estimate the effect that marrying and/or living under a unilateral divorce regime has on investment in marriage-specific capital. Home ownership rates are used to examine the impact on joint investment in public goods within the marriage. I find a statistically significant decrease in home ownership rates of 2 to 3 percentage points. Investment in children is examined at the quantity margin. Newlyweds are found to be 7-8 percent less likely to have at least one child. A decrease in the probability of having more than three children is found for couples married 15 years or less. Investment in home-making skills has apparently decreased as hours in the labor force increases for women and decreases for men. In sum, the divorce regime appears to have real, significant effects on investment within marriage.

### 1. Introduction

During the past thirty years legislative reform to divorce law occurred in which most states stopped intervening in the divorce decision and began 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] This period also saw substantial increases in state divorce rates, prompting politicians and the public to blame the changed divorce regulations. However, some economists invoked the Coase Theorem, arguing that if one person wants to either end or preserve a marriage he or she can do so by sufficiently compensating his or

[1] Weitzman (1985)

her spouse. As such, a divorce will only occur when it is jointly optimal, regardless of the divorce law.[2]

Even if unilateral divorce does allow some marriages to dissolve that may not otherwise, it still may be good public policy. Advocates of tighter restrictions on divorce claim that divorced people are worse off along a number of dimensions than their married (or even never-married) counterparts, however that reasoning does not imply that the *marginal* divorces allowed by unilateral divorce should be prevented. Furthermore, it may be that the probability of divorce is unaffected but that bargaining within marriage changes in a way that affects outcomes. Stevenson and Wolfers (2000) analyze the effects of adult well-being when states switch to unilateral divorce. Their research finds a lower incidence of domestic violence and less female suicide in states with unilateral divorce. However, even if unilateral divorce benefits adults, it need not be unambiguously good for their children. Gruber (2000) examines adult outcomes for children who grew up in states in which unilateral divorce was legal finding that such children have lower educational attainment and lower family incomes and, on average, married earlier 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 may be the result of other changes. For example, both papers cite the importance of potential changes in marital bargaining. A further explanation might be that the presence of unilateral divorce affects marriage-specific investment and that one such investment is having children and dedicating time to their upbringing. Thus, a further explanation for the finding that adults exposed to unilateral divorce as children have lower educational attainment is that their parents simply invested less in them, specifically in their education.

[2] As stated best in Becker, Landes, Michael (1977) "compensation of a spouse to induce acquiescence is an excellent illustration of the "Coase Theorem" that the allocation of property rights or legal liability does not

The reason that parents may be investing less in their children is that investment in children is a form of marriage-specific capital. 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, when the marriage ends, the husband appropriates this human capital. Although the value of the investment is unchanged, the wife's ability to earn a return on that investment falls. Alternatively, some investments are public goods so that when the marriage dissolves there is a reduction in that only one person will get to enjoy the public good. With easily reproduced property, such as a television, division and compensation are easy. One person can get the television and the other person can be compensated for his or her loss. However, some property and investments are inherently difficult to divide or reproduce. Children are a prime example here; they may be enjoyed by both parents without the diminished utility of either as long as they are married. Once parents are divorced, however, children cannot be enjoyed jointly and compensation is hard to calculate.

If divorce rates did indeed rise as a result of unilateral divorce then there are strong implications for marriage-specific investments. Becker, Landes, and Michael (1977) state clearly that "couples are reluctant to invest in skills or commodities "specific" to their marriage if they anticipate dissolution."[3] Therefore, if unilateral divorce caused an increase in the expected probability of divorce then we should see a reduction in marriage-specific investment. Even if divorce rates did not rise, however, there may be important distributional changes that change an individual's investments in marriage-specific capital. In bargaining models of marriage, each spouse's threat point is, in part, a function of his or her best option outside the marriage. The ability to divorce without consent changes the circumstances under which a spouse can exercise

influence resource allocation when the parties involved can bargain with each other at little cost."

[3] p. 1142

this outside option. For instance, in a regime in which divorces are granted only for cause or by mutual consent, a husband who has a good outside option (a good job, a new girlfriend) may be unable to make very much use of this option to strengthen this threat point. 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 take a larger share of goods inside the marriage. Because he cannot commit *not* to take a larger share of goods, his wife is less likely to make marriage-specific investments because she receives a smaller share of the proceeds. Thus, even with no change in the couple's actual divorce behavior, marriage-specific investment may fall.

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 and 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 house ownership is easily and accurately measured. For a couple with a given income, it is probably strongly affected by 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 exclusively in non-market skills (homemaking) and less willing to invest in their husband's market skills. Men married to such women will be forced 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 looking at 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 (more children) and quality (more time with each child, more investment in each) margins. As a compromise between these margins, I examine the probability that a couple has three or more children. This measure does not reward a couple unduly for having many children rapidly. For newlywed couples, I examine the probability that they have at least one child.

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. Investment in children suggests that couples in unilateral divorce states reduce family size as there is a 3-7 percentage point decrease in the probability that a couple has three or more children. Evidence on newlyweds suggest that they are less likely to have any children, with a point estimate indicating a 7-8 percentage point decrease in the probability of having at least one child for those 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. Unilateral Divorce and the Divorce Rate

The most obvious way in which unilateral divorce may affect investment in marriage-specific capital is through its effect on the divorce rate. By definition, marriage-specific capital is capital that depreciates upon dissolution of the marriage to which it is specific. Therefore, if a couple anticipates a higher probability of divorce in a state that allows unilateral divorce, then they will invest less in capital that is specific to their marriage then they would if they lived in a state without unilateral divorce. Thus, to understand the potential effect of changes in divorce rates on marriage-specific investment, one must first assess whether or not couples anticipate a higher divorce probability when they live in a unilateral divorce state. The effect of unilateral divorce on divorce rates has been a hotly contested debate with arguments and empirical work going in both directions.[4] Recently, Gruber (2000) argued, using census data, that the stock of divorced people rose significantly in unilateral divorce states. However, research by Wolfers (2001) 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. In other words, if both Gruber and Wolfers are correct, a person living in a unilateral divorce state is more likely to be currently living as a divorcé, but has a similar lifetime probability of being divorced.

One interpretation of these results is that unilateral divorce results in earlier divorce and less remarriage. The implication of this interpretation is that unilateral divorce may affect the expected duration of a marriage without affecting the probability of dissolution. If the probability of divorce rises in the first few years of marriage and falls in later years then the marriage-specific investment should still decrease. The reason is that the value of marriage-specific investment is the present discounted value of a stream of future returns. When the horizon over which the returns are collected is shorter, so is the value of the investment. To see this consider

[4] Peters (1986), Peters (1992), and Wolfers (2001) all find that divorce rates did not increase as a result of unilateral divorce. Allen (1992) and Friedberg (1998) find that they did.

an investment in marriage-specific capital that yields a constant stream of payments, r, in any period in which the marriage is intact. The present discounted value of the investment at the time it is made is given by:

$$V_i = \left[ \sum_{\tau=\tau_o}^{T} \frac{r_i}{(1+\delta)_{(\tau-\tau_o)}} \pi_\tau \right] - C \text{ and } \pi_\tau = \sum_{\tau=0}^{T} (1-\rho_\tau)^\tau$$

where $C$ is the upfront cost, $\pi_\tau$ is the probability that the couple is still married in period $\tau$, and $\rho_\tau$ is the probability of divorce in period $\tau$. If unilateral divorce causes a rise in the probability of divorce in the first $\kappa$ periods such that $\rho_\tau^{\text{Unilateral}} > \rho_\tau^{\text{Mutual Consent}}$ for the periods $\tau=\tau_o, \tau_o+1, \tau_o+2, \ldots \tau_o+\kappa$ and a fall in the divorce rate occurs in period $\tau+\kappa+1$ such that $\pi_\tau^{\text{Unilateral}}$ is the same as $\pi_\tau^{\text{Mutual Consent}}$ for the periods $\tau=\tau_o+\kappa+1, \tau_o+\kappa+2, \ldots T$ then clearly $V_i$ decreases since

$$\frac{r_i}{(1+\delta)_{(\tau-\tau_o)}} \pi_\tau^{\text{Unilateral}} < \frac{r_i}{(1+\delta)_{(\tau-\tau_o)}} \pi_\tau^{\text{Mutual Consent}}$$ for the periods $\tau=\tau_o, \tau_o+1, \tau_o+2, \ldots \tau_o+\kappa$.

### 3. Unilateral Divorce and Asymmetric Investment

Unilateral divorce may also affect marriage specific investment because it changes bargaining power and the distribution of goods within marriage. As the right to dissolve the marriage shifts - from the partner who is reluctant to part to his or her spouse – bargaining power, and hence distribution within the marriage changes. With mutual consent divorce, if one spouse wants out of the marriage badly enough he or she must compensate the other spouse sufficiently in order to get that privilege. However, with unilateral divorce laws the party who wants to keep the contract intact must compensate the person who wants to end it. What this means in terms of utility is that one's minimum guaranteed utility changes from that which would be received if married to that which would be received if divorced.[5]

[5]As long as there is not asymmetric information between the spouses regarding the probability of divorce, it is irrelevant whether divorce is modeled as occurring because of uncertainty at the time of marriage (that is

Consider a simple Nash bargaining model, often used to evaluate bargaining within marriage. In these models household allocation is determined through Nash bargaining relying on threat points so that the return on an investment in marriage-specific capital for the wife (husband) now depends on the share allocated to the wife (husband). Suppose that a spouse's threat point is a function of his/her best alternative to marriage times his/her expected probability of being able to exercise this alternative if it becomes preferable. For instance, consider the following reasonable functions:

$$Utility\ if\ Divorced = f(Custody,\ Number\ of\ kids,\ Income,\ New\ Relationship)$$

$$Probability\ Divorce\ is\ Feasible = f(fault, mutual, prevent)$$

where the utility if divorce depends positively on the probability of getting custody of the children (if any), the number of children for which custody is given, potential income from labor force participation if divorced, and the probability that another relationship can be successfully established. Under mutual consent/fault laws the probability that divorce is an option depends positively on the probability that one's spouse commits a marital "fault" that can be verified by a court and on the probability that one's spouse will also want a divorce. It depends negatively on the probability that one's spouse engages in behavior to actively prevent a divorce (such as violence, etc). With unilateral divorce laws the probability that divorce is feasible is equal to one (with the exception that one's spouse can still attempt to prevent the divorce by means of violence). Therefore, the utility outside of marriage is higher under unilateral divorce because the probability of being able to exercise the outside alternative if it becomes preferable goes up. The utility for the spouses is symmetric.

If the threat points are symmetric then the distribution within the household is equal. If the one spouse has a stronger (weaker) threat point then they receive a higher (lower) allocation.

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resolved such that the gains from the marriage turn out to be less than expected) or because it is a fully

The split of goods varies with the threat points. Let $\alpha$ be the wife's share within the household then the value to her of an investment in marriage-specific capital that returns r in every period is:

$$V_i = \left[ \sum_{\tau=\tau_o}^{T} \frac{r_i \alpha}{(1+\delta)_{(\tau-\tau_o)}} \pi_\tau \right] - C$$

and the value to her husband is

$$V_i = \left[ \sum_{\tau=\tau_o}^{T} \frac{r_i (1-\alpha)}{(1+\delta)_{(\tau-\tau_o)}} \pi_\tau \right] - C$$

Clearly, if the threat points are not symmetric then a change to unilateral divorce will lower $\alpha$ reducing the value of marriage-specific investment for the person in a weaker position.

In models relying on external threat points, marriage-specific investment that requires a disproportionate sacrifice by one spouse reduces the value outside of the marriage to that spouse, relative to that inside the marriage. However symmetric investment reduces the exit threat equally for each spouse. As such, if couples want to credibly commit to staying married then they may increase their investment in symmetric marriage-specific capital as divorce becomes easier. Although most investments are not symmetric, e.g. wives typically get children and houses upon divorce and husbands typically get the value of their human capital, multiple investments can be made to balance out these asymmetries.

## 4. Types of Investment

So far marriage-specific investment has been discussed abstractly, ignoring exactly what such investments are. Roughly, marriage-specific investment can be divided into three categories: specialization, public goods, and collective goods. Public goods are those such that consumption per person does not decline proportionately with the number of consumers and are

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expected, rational life-cycle plan.

most likely to involve a joint investment decision. The clearest example is the shared home, which can be either rented or purchased. Clearly the latter choice represents greater investment in marriage specific capital.

Similarly children are a collective good within marriage, requiring the cooperative efforts of their parents in their production. Furthermore, expenditure on children increases the utility of both parents within a marriage. However, outside of the marriage the utility from children decreases for the non-custodial parent because of decreased contact, and potentially decreases for the custodial parent by reducing his or her potential utility in future marriages and with future children. Furthermore, the utility derived from income spent on non-custodial children may decrease because of monitoring problems.

Finally, specialization requires investment in a particular set of skills. For example, men tend to do more market work when married because they are substituting away from non-market work. Women tend to do the opposite, specializing in non-market production. Such specialization is a form of marriage-specific investment because women acquire human capital that is only useful inside of a marriage and allow their labor market human capital to deteriorate.

## 5. Empirical Strategy

To identify the effects of unilateral divorce on marriage-specific investment, I use variation in the timing of states' divorce regimes. Table 3.1 shows the year that unilateral divorce was passed in each state. Currently 34 states allow for unrestricted unilateral divorce, of these 29 changed their law between 1967 and 1978.[6] Nine other states passed unilateral divorce laws with separation requirements during this period.[7]

The empirical strategy employed in the following regressions is to 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. To be precise, 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 reflect 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. The first measure emphasized 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

[6] Utah and South Dakota adopted unrestricted unilateral divorce in the mid-1980s. The other three states had pre-existing unrestricted unilateral divorce.

[7] 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.

1980 censuses on the age of first marriage can be used to calculate the year of marriage for individuals currently in their first marriage.[8] The analysis in this paper only considers individuals in first marriages to avoid potential selection problems, however there are obviously bias issues that arise from this restriction.

A problem with the empirical strategy is that it examines the pool of married people, something that itself will be changed by the presence of unilateral divorce. Put another way, the treatment (unilateral divorce) changes the nature of selection into marriage. It is not obvious how this change in selection biases the estimated effects on unilateral divorce on marriage-specific investment. Bad marriages will end earlier after unilateral divorce, so there will be more "bad marriages" prior to unilateral divorce. For example, a bad marriage may dissolve in its fourth year without unilateral divorce, but in its second year with unilateral divorce. Thus, a badly married couple will be in the pool of third-year marriages prior to unilateral divorce, but will not be recorded as married under 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 marriage investment will show an *increase* in marriage-specific investment. On the other hand, couples may be more likely to get married when they know that they can exit the marriage more easily.[9] In other words, match quality may go down because the cost of a bad match falls. Marriages that are a worse match have a higher expectation of divorce and, therefore, should have less marriage specific investment. This effect on selection into marriage will make regression results appear to show 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

[8] Whether an individual is in a first or subsequent marriage is identified by the census.

two sub-populations that each serve to minimize one of the forms of selection bias. First, I consider individuals married two years or less. These newlyweds have been married such a short time that selection *out of marriage* is unlikely to have taken place.[10] 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. Of the 29 law changes, 26 occurred between 1970 and 1978, therefore regressions using the population of newlyweds will largely be comparing the differences of the differences between the 1970 and 1980 populations between those who changed their law and those who did not. Of the 25 non-changing states, 6 had unilateral diovrce.

The second sub-population that I consider are those who are married under a fault-divorce regime, but then spend most of their married lives under a unilateral divorce regime.[11] Such marriages are not contaminated by the effects that unilateral divorce may have on selection *into marriage*. Therefore, regressions based on them should not contain bias from the formation of weakly committed relationships. To create this sub-population individuals married 1-2 years before the legal change occurs 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 is compared with the differences in the non-treatment states. Since the treatment here is having the divorce law change quickly after the marriage occurs only the non-reform, non-unilateral states are used as controls.[12] For example, Massachusetts changed to a unilateral divorce regime in 1975, so those married in 1973 or 1974 would be considered the population

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[9] 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.
[10] 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.
[11] The actual state in which the marriage took place is unknown. Only the year of marriage and the current state of residence is known.
[12] In this context a marriage cohort are all couples with the same number of years married.

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 a divorce law change 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 10 percent intervals of being exposed to unilateral divorce.

Table 3.2 shows summary statistics for each of the relevant sample groups.

## 6. The Effects of Unilateral Divorce on Investment in a Home

The home of a married couple typically represents their largest public good. Home ownership is 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. Most divorces occur within the first few years of marriage so an increase in divorce expectations or a decrease in marriage duration will increase the probability of having to sell the property before the fixed costs have been recouped.

The census identifies whether an individual 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. The first 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.[13] The regression run is:

$$(1) \qquad Own\ home_{i,s,t} = \alpha + \beta\, unilateral_{s,t} + \lambda Census_t + \sum_s \eta_s state + X_{i,s,t}\varphi + \varepsilon_{s,t}$$

where standard errors are clustered at the level of state*year of census cells. I control for individual characteristics that are not likely to be affected by unilateral divorce: race, region, metropolitan status, and state effects. I also include year fixed effects. I do not control for variables that might be affect 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 3.3a. Including only states with unrestricted unilateral divorce in the treatment group, the estimated coefficients, while correctly signed, are not statistically significant. In columns 4-6 the treatment group is expanded to include unilateral divorce with a one year separation requirement.[14] Using this definition of unilateral divorce, the estimated coefficient on home ownership increases slightly, enough to make it statistically significant at the 10 percent level. However there is not a convincing, large decrease in home ownership rates among newlyweds.

The second regression considers the percent of a marriage spent in a unilateral divorce state. Dummy variables are constructed indicating 1 to 10 percent, 10 to 20 percent, 20 to 30 percent, 30 to 40 percent, 40-50 percent, 50-60 percent, 60-70 percent, 70-80 percent, 80-89 percent, and 100 percent spent in a state allowing for unilateral divorce.[15] Thus the following regression is run:

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

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

[15] There are actually no marriages in the 90-99 percent category.

(2) $Own\ home_{i,s,t} = \alpha + \sum_p \beta_p\ unilateral_{s,t} + \lambda Census_t + \sum_s \eta_s state + \sum_t \gamma_t Years\ Married\ + X_{i,s,t}\varphi + \varepsilon_{s,t}$

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 3.3b, subsequent columns report the results for couples married 15 years or less, 12 years or less, and 7 years or less. When the sample includes all couples there is a statistically significant decrease in home ownership rates of about one percentage point for all levels of exposure. Considering all couples will have significant bias resulting from selection out of marriage. Looking at couples married 15 years or less reduces the bias and as such the estimated coefficients should increase. Indeed for most levels of exposure there is a decrease of 2-3 percentage points. Interpreting between the different exposure levels is problematic because of the small number of couples falling into the categories comprising 10-50 percent exposure. Given homeownership rates for those married 15 years or less in the 1980s census of 70%, the estimated coefficients suggest that in unilateral divorce states such couples are about 4% less likely to own a home.

The third regression attempts to eliminate the bias results from selection into marriage by considering individuals who were married in the two years prior to unilateral divorce becoming law and the corresponding comparison cohort from the 1970 census. 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 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:

$$(3)\ Own\ home_{i,s,t} = \alpha + \beta unilateral_{s,t} + \lambda Census_t + \sum_s \eta_s state + \sum_t \gamma_t Years\ Married\ + X_{i,s,t}\varphi + \varepsilon_{s,t}$$

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 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 (Table 3.3a). When the sample is narrowed to consider to those married 12 years or less 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 and 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 less the number of treatment states is too small and the standard errors blow up too much for the estimated coefficients to be useful.

## 7. Investment in the Labor Force

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. The person who specializes in non-market work, typically the woman, allows her non-marriage-specific capital to deteriorate and invests in marriage-specific human capital. Although many homemaking skills may be transferable to another marriage, if she is unable, or unwilling, to remarry, the value of these skills decreases tremendously. Because the opportunity cost of acquiring skills in the non-market sector is development market sector skills, a homemaker expects lower wages upon re-entry to the labor force. Therefore, an increase in the expectation of divorce should lead to more two-earner couples (more equitable investment in market skills).

As with home ownership, I run three regressions to determine the effect of unilateral divorce on hours worked. The first regression examines newlyweds – the group least likely to be contaminated by selection out of marriage. Looking at hours worked separately for men and women I run:

$$HoursWorked_{i,s,t} = \alpha + \beta unilateral_{s,t} + \lambda Census_t + \sum_s \eta_s state + X_{i,s,t}\varphi + \varepsilon_{s,t}$$

The effect on male hours is negligible and statistically insignificant. The result for women suggests a decrease of a half-hour of work per week for women newly married in unilateral divorce states.

The second group considered are those married just before unilateral divorce becomes law. Looking at this group for those married 15 or less or 12 or less 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 less 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. Using this measure we see that women who have spent some of their marriage in unilateral divorce states tend to work more hours than those who have spent none of their marriage in unilateral divorce states. Similarly, men who have been exposed to unilateral divorce work less hours than their counterparts in non-unilateral states.

## 8. Investment in Children

According to Becker "the most obvious and dominant example of marriage-specific investment is children"[16]. 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 they give their parents. The ability to extract these returns are diminished 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 less 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 use three regressions to assess the impact of unilateral divorce on the quantity of children. The first starts by minimizing the selection bias arising from selection out of marriage, considering only the population of newlyweds. For this group the marriage duration is too short to make it sensible to look beyond the first child, as such the dependent variable considered is an indicator of whether or not the couple has at least one child. The independent variable of interest is an indicator of whether or not unilateral divorce prevailed at the time of the marriage. The regression run is:

(1) $$Prob\ of\ one\ child_{i,s,t} = \alpha + \beta unilateral_{s,t} + \lambda Year_t + \sum_s \eta_s state + X_{i,s,t}\varphi + \varepsilon_{s,t}$$

where standard errors are clustered at the level of state*year of census cells. I control for individual characteristics that are not likely to be affected by unilateral divorce: race, region,

[16] Becker, 1974. p. S23

metropolitan status, and state effects. I also control for year fixed effects. I estimate this regression separately for women and men and report the results in the first two columns of the first row of Table 3.5a. (The second row reports the same regression run for women where the probability of having at least one child is constructed using the measure of children ever born to illustrate the equivalence of the two measures). Including only states with unrestricted unilateral divorce in the treatment group, the estimated coefficients show a statistically significant decrease of 7.6 percentage points in the probability of at least one child for women and an equivalent finding of a 6.5 percentage point decrease for men. This finding is robust to including states with a one year separation requirement. Although the decrease measured 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.

The second regression used here examines couples who were married one to two years prior to unilateral divorce becoming legal in their state. 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 considered are an indicator of whether or not the couple has at least one child, an indicator for at least two children, and an indicator of at least three children. I use these measures rather than sheer quantity of children (such as number or children), because sheer quantity unduly rewards parents who sacrifice child quality for quantity. In addition, sheer quantity of children unduly rewards couples who get married when they discover an unplanned pregnancy. (Indeed, this phenomenon may become more prevalent under unilateral divorce as couples will be able to reverse the decision should it not work out.)

I focus on families having two or more children and those with three or more children because the average women in a first marriage has between 2 and 3 children. By looking at the probability that a couple has two children and the probability that they have three or more children, I attempt to evaluate their distance from the norm. Thus the regressions run are:

$$Probability\ of\ x\ children = \alpha + \beta Unilateral + \varphi Cenus + \sum_{s} \eta_s State + \sum_{t} \lambda_t Years\ Married + X_{i,s,t}\delta + \varepsilon_{i,s,t}$$

The estimated regression coefficients are shown in Table 3.5a. The results for the various measures of number of children clearly reflect that potential selection bias problems. Approximately 35 percent of newlywed couples have at least one child in the home. Given that the treatment group considered here are 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 less (Table 3.5a), 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 less, 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 less 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 less 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 less. Despite the selection biases arising from these estimates there is clear evidence that some couples reduced their family size as a result of the change in the legal parameters surrounding divorce.

The third set of regressions considers all marriages, using indicator variables for the amount of the marriage that was spent under a unilateral divorce regime. As with the previous sample three regression are run using three different dependent variables: probability of at least one child, two children, and three children. The results from these regressions are reported in Table 3.5b. The selection issues here are even more complicated than those which were discussed above. For the category of 100 percent of the marriage spent in a unilateral divorce state there is no selection into marriage, but there is selection out of marriage. Furthermore, couples in this category make all of their child-baring decisions in the presence of unilateral divorce laws. The coefficient on the probability of having one child for couples who were married under unilateral divorce (100 percent) are reported in the last cell of the first column of table 3.5 b . Considering couples married 15 years or less, the estimated coefficient for both men and women whose entire marriage was spent under unilateral divorce (100 percent), there is a statistically significant decrease in the probability of having at least one child of about one percentage point. Looking at the other categories where there is some exposure to unilateral divorce, but they were not married under it, there is a statistically significant increase in the probability of having at least one child. The reason for this finding is as in the previous paragraph, these couples are likely to have already had their first child by the time unilateral divorce passes and may have different tendencies to select out of marriage following unilateral divorce than couples without children.

Examining the effect on two and three children we again see some evidence that unilateral divorce is causing couples to reduce family size. For couples married prior to unilateral divorce and subsequently spending some fraction of their marriage in unilateral divorce states there is a statistically significant decrease of three to seven percentage points in the probability of having three or more children. 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.

## 9. Conclusion

Clearly there is more to divorce regulation than simply the affect on those who choose to divorce. Evidence was shown in chapter 2 that unilateral divorce led to less suicide among women and less domestic violence. This chapter has demonstrated that investment in marriage-specific capital is affected by the legal regime in place. People invest in their marriage to the extent that they expect it to stay intact. Weakening the marriage contract by making it easier for someone to exit the marriage changes the incentive to invest the marriage. There is evidence that couples are less likely to invest in a major public good – their own home. 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 3.1: Year of Introduction of Unilateral Divorce Laws, by State**

| | Unilateral | | Unilateral with Separation Requirement | | | |
|---|---|---|---|---|---|---|
| | | | One Year | 18 Months to Two Years | Three Years | Five or More Years |
| Alabama | 1971 | | | | | |
| Alaska | 1935 | | | | | |
| Arizona | 1973 | | | | | 1931 |
| Arkansas | | * | | 1991 | 1937 | |
| California | 1970 | | | | | |
| Colorado | 1972 | * | | | | |
| Connecticut | 1973 | | | | | |
| Delaware | 1968 | * | | | | |
| District of Columbia | | | 1977 | | | |
| Florida | 1971 | | | | | |
| Georgia | 1973 | | | | | |
| Hawaii | 1972 | * | | 1965 | | |
| Idaho | 1971 | | | | | 1945 |
| Illinois | | | | 1984 | | |
| Indiana | 1973 | | | | | |
| Iowa | 1970 | | | | | |
| Kansas | 1969 | | | | | |
| Kentucky | 1972 | | | | | 1916 |
| Louisiana | | | 1979 | 1938 | | |
| Maine | 1973 | | | | | |
| Maryland | | * | | 1983 | 1973 | 1969 |
| Massachusetts | 1975 | | | | | |
| Michigan | 1972 | | | | | |
| Minnesota | 1974 | | | | | 1933 |
| Mississippi | | | | | | |
| Missouri | | * | | 1974 | | |
| Montana | 1973 | * | | | | |
| Nebraska | 1972 | | | | | |
| Nevada | 1967 | * | | | 1939 | 1931 |

**Table 3.1 Continued**
**Year of Introduction of Unilateral Divorce Laws, by State**

| | | | | | | |
|---|---|---|---|---|---|---|
| New Hampshire | 1971 | | | | | |
| New Jersey | | | | 1971 | | 1907 |
| New Mexico | 1933 | * | | | | |
| New York | | | | | | |
| North Carolina | | | 1965 | 1933 | | |
| North Dakota | 1971 | | | | | |
| Ohio | | | 1982 | 1974 | | |
| Oklahoma | 1953 | | | | | |
| Oregon | 1971 | * | | | | |
| Pennsylvania | | | | 1988 | 1980 | |
| Rhode Island | 1975 | * | | | | 1910 |
| South Carolina | | | 1979 | | 1969 | |
| South Dakota | 1985 | | | | | |
| Tennessee | | * | | 1963 | | |
| Texas | 1970 | * | | | 1967 | 1941 |
| Utah | 1987 | | | | 1943 | |
| Vermont | | * | 1972 | 1971 | 1969 | |
| Virginia | | | 1975 | 1964 | 1960 | |
| Washington | 1973 | | | 1965 | | 1921 |
| West Virginia | | * | 1977 | 1969 | | |
| Wisconsin | 1978 | * | | | | |
| Wyoming | 1977 | | | | | |

Source: Gruber (2000)

* Indicates that the coding disagrees with Friedberg (1998)

**Table 3.2 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<br>(5.4) | 21.6<br>(5.1) | 24.4<br>(6.0) | 24.2<br>(5.6) |
| Number of years Married | 20.2<br>(14.2) | 21.2<br>(15.0) | 20.2<br>(14.2) | 21.2<br>(14.9) |
| Own Home | 70.6% | 78.2% | 70.8% | 78.3% |
| Hours Worked | 12.4<br>(18.0) | 15.6<br>(19.0) | 34.8<br>(20.1) | 33.7<br>(21.3) |
| Wages | 1,529<br>(2,533) | 4,351<br>(23,314) | 7,133<br>(6,324) | 14064<br>(20313) |
| Children ever born | 2.4<br>(2.0) | 2.3<br>(1.8) | | |
| With at least 2 children in house<br>(Nos. in brackets reflect children ever born) | 43.3%<br>(66.4%) | 38.7%<br>(65.7%) | 43.4% | 38.6% |
| With at least 3 children in house<br>(Nos. in brackets reflect children ever born) | 23.5%<br>(40.9%) | 16.4%<br>(37.5%) | 23.6% | 16.5% |
| **Newlyweds In First Marriages** | | | | |
| Age of Marriage | 21.4<br>(6.4) | 22.3<br>(5.6) | 23.8<br>(6.4) | 24.3<br>(6.2) |
| Own Home | 30.0% | 40.9% | 28.4% | 40.7% |
| Hours Worked | 16.7<br>(19.1) | 21.4<br>(19.6) | 32.3<br>(19.8) | 36.8<br>(18.2) |
| Wages | 2,260<br>(2,540) | 10,804<br>(74,960) | 5,312<br>(3,818) | 13418<br>(52459) |
| At least one child | 37.9% | 34% | 38.7% | 35.5% |
| At least one child ever born | 40.2% | 36% | | |

| Couples Married 1or 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; these numbers assume that the marriage occurred in the current state of residence.

**Table 3.3a**
**DIVORCE REGIME AND HOME OWNERSHIP**

$$(Own\ Home)_{i,s} = \alpha + \beta Unilateral + \sum_s \eta_s State_s + \sum_t \chi_t Year\ Married_t + X_{i,s}\lambda + \varepsilon_{s,t}$$

| | 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 (1) | Men (2) | Combined (3) | Women (4) | Men (5) | Combined (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 3.3b
## 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 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 3.4b**
**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 <25ercent | .004<br>(.035) | -.515**<br>(.269) | .011<br>(.008) | -.621**<br>(.311) | -.021<br>(.094) | -.227<br>(.564) |
| 25 to < 50 percent | .029<br>(.029) | -.666***<br>(.258) | .071<br>(.007) | -1.22***<br>(.339) | -.034<br>(.056) | -2.01***<br>(.607) |
| 50 to <75 percent | .039**<br>(.019) | -.278<br>(.231) | .017<br>(.005) | -.370<br>(.270) | -.012<br>(.068) | -.699<br>(.524) |
| 75 to < 100 percent | .063***<br>(.020) | -.383<br>(.263) | .070<br>(.006) | -.383*<br>(.263) | -.159***<br>(.060) | -.675*<br>(.388) |
| 100 percent | .027*<br>(.016) | -.181<br>(.261) | -.002<br>(.005) | -.181<br>(.261) | .040*<br>(.022) | .294<br>(.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 3.4b**
**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 <25ercent | .005<br>(.005) | -.515**<br>(.269) | .006<br>(.008) | -.621**<br>(.311) | -.028<br>(.011) | -.227<br>(.564) |
| 25 to < 50 percent | .021***<br>(.006) | -.666***<br>(.258) | .009<br>(.007) | -1.22***<br>(.339) | -.061***<br>(.008) | -2.01***<br>(.607) |
| 50 to <75 percent | .022<br>(.003) | -.278<br>(.231) | .006<br>(.005) | -.370<br>(.270) | -.073<br>(.006) | -.699<br>(.524) |
| 75 to < 100 percent | .013<br>(.004) | -.383<br>(.263) | .008<br>(.006) | -.383*<br>(.263) | -.061***<br>(.006) | -.675*<br>(.388) |
| 100 percent | -.011<br>(.262) | -.181<br>(.261) | -.002<br>(.005) | -.181<br>(.261) | .018<br>(.005) | .294<br>(.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 3.5a
## DIVORCE REGIME AND QUANTITY 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*** | -.065** | -.085*** | -.071** |
| | (.031) | (.030) | (.031) | (.030) |
| At least one child ever born | -.075*** | | -.084*** | |
| | (.030) | | (.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** | .018*** | .015** | .018*** |
| | (.007) | (.007) | (.007) | (.007) |
| Two or more children | -.010 | -.011 | -.010 | -.011 |
| | (.009) | (.008) | (.009) | (.008) |
| Three or more children | -.043*** | -.040*** | -.043*** | -.040*** |
| | (.010) | (.009) | (.010) | (.009) |
| **Married 12 years or less** | | | | |
| At least one child in the household | .030*** | .032*** | .030*** | .032*** |
| | (.008) | (.007) | (.008) | (.007) |
| Two or more children | .005 | .003 | .005 | .003 |
| | (.010) | (.009) | (.010) | (.009) |
| Three or more children | -.052*** | -.050*** | -.052*** | -.050*** |
| | (.010) | (.010) | (.010) | (.010) |
| **Married 7 years or less** | | | | |
| At least one child in the household | .004 | .017* | .004 | .017* |
| | (.010) | (.008) | (.009) | (.008) |
| Two or more children | -.023 | -.019 | -.023 | -.019 |
| | (.025) | (.025) | (.025) | (.025) |
| Three or more children | -.034* | -.031 | -.034* | -.031 |
| | (.018) | (.020) | (.019) | (.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 3.5b
## DIVORCE REGIME AND NUMBER OF CHILDREN

Married 15 Years or less

| 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) |
| 1 to <25ercent | .005<br>(.005) | .007<br>(.005) | .006<br>(.008) | .009<br>(.008) | -.028***<br>(.011) | -.028***<br>(.010) |
| 25 to < 50 percent | .021***<br>(.006) | .015***<br>(.006) | .009<br>(.007) | .007<br>(.008) | -.061***<br>(.008) | -.060***<br>(.007) |
| 50 to <75 percent | .022***<br>(.003) | .021***<br>(.004) | .006<br>(.005) | .007<br>(.005) | -.073***<br>(.006) | -.070***<br>(.005) |
| 75 to < 100 percent | .013***<br>(.004) | .011***<br>(.004) | .008<br>(.006) | -.010*<br>(.005) | -.061***<br>(.006) | -.060***<br>(.005) |
| 100 percent | -.011***<br>(.003) | -.011***<br>(.003) | -.002<br>(.005) | -.004<br>(.004) | .018***<br>(.005) | .016***<br>(.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.

**Table 3.5c continued**
**DIVORCE REGIME AND NUMBER OF CHILDREN**

Married 12 Years or less

| Percent of Marriage Spent under Unilateral Divorce Regime | At Least One Child | | At Least Two Children | | At Least Three Children | |
|---|---|---|---|---|---|---|
| | Women | Men | Women | Men | Women | Men |
| 1 to <25ercent | -.001<br>(.007) | .001<br>(.006) | -.003<br>(.010) | -.000<br>(.010) | -.029**<br>(.014) | -.028**<br>(.012) |
| 25 to < 50 percent | .005<br>(.009) | -.008<br>(.008) | -.024**<br>(.010) | -.027***<br>(.010) | -.057***<br>(.014) | -.059***<br>(.012) |
| 50 to <75 percent | .018***<br>(.005) | .017***<br>(.005) | -.008<br>(.007) | -.004<br>(.007) | -.082***<br>(.008) | -.077***<br>(.007) |
| 75 to < 100 percent | .015***<br>(.005) | .012***<br>(.004) | -.009<br>(.006) | -.012**<br>(.006) | -.064***<br>(.006) | -.065***<br>(.005) |
| 100 percent | -.006**<br>(.003) | -.008**<br>(.003) | .003<br>(.005) | -.000<br>(.005) | .015***<br>(.005) | .011**<br>(.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.