

The Impact of Unilateral Divorce on Crime^{*}

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Abstract

Using data from the FBI's Uniform Crime Report program and differences in the timing in the introduction of the reform, we find that unilateral divorce has a positive impact on violent crime rates. Arrest data shows an average impact of 30% for murder and 39% for aggravated assault arrest rates over the period 1965-1997. Our results suggest that the impact comes principally from those who were children at the time of the reform. We confirm our findings using the age at the time of the reform as an additional source of variation and controlling for all confounding factors that may operate at the state-year, state-age or age-year level.

Keywords: unilateral divorce, crime rates, arrest rates

JEL classification: J12, J13

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1. Introduction

Family as institution has undergone a “complete make-over” in the U.S. and in the Western Hemisphere during the last fifty years. Institutional and technological changes such as abortion and contraceptive methods have not only changed gender roles (Goldin and Katz, 2002) but also the family structure faced by the new generations. Children are now more likely to grow up in a one parent or a blended family, and in all families mothers are more likely to work. Among the most important institutional changes was the reform in divorce legislation to the extent that it has been called the “*Divorce Revolution*”. Specifically, unilateral divorce, the right of one spouse to ask for a divorce without the consent of the other, is the aspect of the reform that has captured the greatest attention in the literature during the last twenty years.¹

Initially, the scholarly debate was focused on the impact of unilateral divorce on divorce rates (Peters, 1986; Friedberg, 1998; Gruber, 2004). There is growing consensus in the literature of a short-term increase in divorce following the reform (Wolfers, 2006). Scholars suggest a greater selection into and out of marriage in adopting states, and therefore an increase in the average match-quality of new and surviving marriages.² Despite the direct effects of unilateral divorce on divorce

¹ Nevertheless, the process began before 1950 in a number of states, by removing fault grounds, for example adultery, desertion or physical abuse, in order for spouses to ask for a divorce (Gruber, 2004). In the early 1970's some states started introducing not only no-fault grounds in the legislation but also allowing one spouse to ask for a divorce without the consent of the other spouse, which has been called “Unilateral divorce”. An additional aspect of the reform is related to the division of property and assets in case of divorce. By the end of 1970s, the majority of the states had moved to a regime where property was more equally divided. In the same period, many states eliminated the consideration of fault regarding asset division and spousal support settlement. Nevertheless, not all states adopting unilateral divorce introduced these reforms simultaneously so we can sort out the impact of unilateral divorce from the others. For a careful review of the characteristics of the reform, see Mechoulan (2005).

² This interpretation gains support from recent evidence on the lower divorce rate among couples married under unilateral divorce, compared with those married under mutual consent (Mechoulan, 2006). Additionally, evidence

rates, recent research has focused on the role of the reform in several other aspects of individual behavior. Some examples are studies on family formation (Drewianka, 2004; Rasul, 2004; Alesina and Giuliano, 2007), marriage-specific investments (Stevenson 2007) or female labor supply (Gray, 1998; Chiappori, Fortin and Lacroix, 2002; Stevenson 2007b). The evidence in these studies also points towards changes in behavior in those marriages formed under the new legislation.

Stevenson and Wolfers (2006) provide a link between unilateral divorce and crime, specifically domestic violence and spousal homicide.³ They show that in states that introduced unilateral divorce there is a sizable decline in domestic violence and in the number of women murdered by their partner. Given the nature of the outcomes (use of force), and biological differences in terms of physical strength between genders, this analysis captures mostly the “benefit” for individuals (women) who were locked into a bad marriage and who, as a consequence of unilateral divorce, profited from an easier divorce setting. It also points toward better selection into marriage.⁴

In this paper we evaluate whether unilateral divorce has any impact on aggregate crime, despite the current evidence of a reduction in crime and violence within marriage. The contribution of this paper is twofold. First, based on the literature that has already shown that unilateral divorce affects (at least in the short-run), both family structure and outcomes related with family investments, we study the effects of the divorce reform on aggregate crime. Second, we try to identify precisely the groups in

supports a reduction in the average duration of marriages that end in divorce (Matouschek and Rasul, 2006) and a decrease in marriage rates (Rasul, 2004).

³ In addition to these two outcomes, they find that unilateral divorce produces an 8–16 percent decline in female suicide.

⁴ Using data similar to Stevenson and Wolfers, Dee (2003) finds that unilateral divorce significantly increased the number of husbands killed by their wives. Stevenson and Wolfers do not find an effect on husbands killed. One way to reconcile these results, given Dee’s shorter sample period (1968-1978), is that his results may come from marriages formed under mutual consent (and where husbands were willing to divorce under the new legislation). If unilateral divorce implied selection into marriage, those effects may have disappeared once new marriages formed under unilateral divorce were taken into account.

the population that were particularly affected, which suggests the potential channels.⁵ To do so, we exploit two sources of variation and we use three different types of data. The first source of variation comes from differences in the timing of divorce law reforms across the United States. Using crime rates from the FBI's Uniform Crime Report program (UCR) for the period 1965-1998, we find that unilateral divorce has a positive impact on violent crime rates, with an 8% to 12% average increase for the period under consideration. Then, using UCR Arrest data, we not only confirm our previous findings but we are also able to identify the most affected groups because of the greater level of disaggregation of the data. For the overall period under analysis, we find an average increase of 30 percent in the murder arrest rate, and a 39 percent increase in the case of aggravated assault arrest rates. We also show that the impact is concentrated on the most relevant age group for this type of offenses (15 to 24 year-olds).⁶ In particular, the results for 15-19 year-olds reveal an average impact over the period under analysis of 40% for murder and 36% for aggravated assault arrest rates. Across the different specifications, we find that the impact is concentrated mostly on the middle to long-term. Therefore, the pattern of the results suggests that the impact comes principally from those individuals who faced the reform as children. Disaggregating total arrest rates by race, we find that the long-run effects on the aggregate rates are driven by the Black sub-sample, while for Whites we only see an impact in the first four years after the law was enacted.

⁵ A closely related work in terms of the outcomes studied here is Donohue and Levitt (2001). Donohue and Levitt link the legalization of abortion in the early 1970's with the fall in the crime rate in the 1990's. Even though here we focus on a different question, we try to take into account the several issues raised in the empirical debate developed over Donohue and Levitt's original work. (Joyce, 2004 and 2006; Foote and Goetz, 2005; Donohue and Levitt, 2004 and 2006).

⁶ The group of 15-24 year-olds is particularly important for violent crime. Over the period 1965-1997, this age group had a participation of 48% in total murder arrests, 41% in total aggravated assault arrests and 64% in total robbery arrests.

In order to identify more precisely the groups of the population affected by the reform and to check the robustness of our results, we introduce a second specification that uses the age at which a given cohort was first exposed to the reform as an additional source of variation. We apply this second source of variation to age-specific arrest rates and homicide rates (Supplemental Homicide Report). Since each observation in our data set is now defined on the state-year-age level, by using this second source of variation we are able to control for potential confounding factors at the state-year, state-age or year-age level that could contaminate our previous results⁷. Here we not only confirm the positive impact on the different components of violent crime, but we also find a positive impact on property arrest rates. The results for murder rates indicate that individuals who were four years old or older are those who face an increase in the probability of engaging in crime. We also find a greater impact in murder arrests for individuals who were 20 to 24 years old at the time of the reform, which might be linked to their own marriage disruption rather than those effects suffered as children. However, our findings on aggravated assault arrest rates are greater for the youngest cohorts exposed to unilateral divorce, consistent with our long-term findings in aggregate rates.

Even though most of the recent empirical research supports the idea that unilateral divorce has benefited married adults and specifically women, the current evidence suggests that the reform may have had some negative effects on children. For example, Gruber (2004), using a sample of adults (25 to 50 years old) from the US Census data for the period 1960-1990, studies the long-term effects of unilateral divorce on children. He finds that those adults who were exposed to the reform as children have lower educational attainments and lower family incomes, marry earlier but separate

⁷ One example of these factors are the temporary state-specific crime waves such as the introduction of new, illicit drugs, such as crack cocaine (Donohue and Levitt, 2001, 2004, 2006; Joyce, 2004, Foote and Goetz, 2005). For a detailed description of the crack epidemic, see Fryer et al. (2005).

more often, and have higher odds of adult suicide.⁸ More recently, Cáceres-Delpiano and Giolito (2008) go further down the causal chain and study the impact of unilateral divorce on outcomes of children together with those of their mothers, using U.S. Census data for the years 1960-1980. They find that mothers in adopting states whose eldest child was 5 years old or older at the time of the reform are approximately 16% more likely to be divorced and 16% more likely to be below the poverty line. Their children also face a 4% to 6% decrease in family income, and they are 16% to 24% less likely to be enrolled in a private school. The results for child outcomes show that children of pre-school age at the time of the reform (age 0-4) are more likely to repeat a grade. The analysis for the same cohorts of children ten years later (using the 1970-1990 Census), shows an increase in the probability of living in an institution (men), or falling below the poverty line (women), consistent with Gruber's (2004) negative impacts on adults.

Independently of the channels behind the relationship between unilateral divorce and negative family and child outcomes, the previous findings can be easily linked with some factors that the sociology and criminology literature consider determinant in defining the start and length of a criminal career. Some examples of these studies are those that link crime with family structure (Matsueda and Heimer, 1987; Sampson, 1987; Sampson, Laub and Wimer, 2006), poverty and inequality (Blau and Blau, 1982; Wilson, 1987) or school completion (Rand, 1987).⁹

⁸ Johnson and Mazingo (2000) using 1990 US Census data examine the amount of time individuals were exposed to unilateral divorce laws as children, finding results consistent with Gruber (2004).

The paper is organized as follows. In Section 2, we briefly review the related literature. Section 3 describes our empirical strategy and the data sources used in the analysis. In Sections 4 and 5 we present our results for the two specifications described above, respectively. Section 6 concludes.

2. Unilateral Divorce, Family Disruption and Crime

The current evidence points toward a better marriage selection as a consequence of unilateral divorce. However, since divorce legislation acts as the dissolution clause of a marriage contract, the unilateral reform can be seen as a retroactive change in this dissolution clause for those marriage contracts already in place at the time of the reform. Therefore, the change in legislation may have produced different effects on those individuals who were married, had children or made marriage investments decisions based on mutual consent divorce rules. Therefore, it is particularly important to identify whether the effects come from those individuals who faced the reform either as adults or as children.

On the one hand, those who faced the reform as adults can be affected either through their own marriage disruption or through a more difficult setting for individuals with risky backgrounds in order to access the benefits of marriage, due to the increase in marriage selection fueled by unilateral divorce. Specifically, criminology literature has linked family structure as a key factor that defines the start and length of a crime career. Sampson, Laub and Wimer (2006) describe three channels, among others, which explain the relationship between marriage and desistance from crime. First, marriage creates a social bond that defines obligations, mutual support and self-discipline which increases the cost of criminal activities. Second, marriage defines obligations that reduce leisure activities outside of the family (Osgood and Lee, 1993) and therefore changes an individual's routines and patterns of association with deviant peers (Warr, 1998). Third, marriage increases male

crime desistance because of the direct monitoring exerted by their spouses. Empirical research regarding the relationship between family structure and crime is not new in criminology or in sociology literature.¹⁰ For example, Rand (1987), examining data for 106 male offenders from a follow-up study of the 1945 birth cohorts in Philadelphia, finds that transitional life events such as marriage, completing school, and receiving vocational training in the military, were positively related to crime (as well as crime characteristics). Horney, Osgood and Marshall (1995) analyze the history of current offenders (incarcerated men), finding that particular life events affect their criminal behavior, at least temporarily. They find that moving in with one's wife doubles the odds of stopping a man from offending (compared with moving away), and moving away from one's wife doubles the odds of starting to offend (compared to moving-in). Sampson, Laub and Wimer (2006), expanding crime history for 500 high-risk boys from the original data of Glueck and Glueck (1950), find an average reduction of approximately 35 percent in the odds of crime associated with being married.

On the other hand, the impact of the unilateral reform may become permanent for children, specifically for those who belong to families "trapped" in the transition from mutual consent to unilateral divorce. Wolfers (2006) shows that unilateral divorce only affected divorce rates in the short-run (around 8-10 years). Because of this, we should expect only transitional effects on crime if an individual engages in (or resumes) criminal activities because of their own divorce. However, those effects might become long lasting if the increase in crime comes from children who were negatively affected by the law, especially through parental divorce, (Gruber, 2004).

¹⁰ A more in depth discussion can be found in Laub and Sampson (1990 and 2001) and Sampson, Laub and Wimer (2006).

There is an extensive literature that has linked divorce with factors related to an early start in crime. For example, Garfinkel and McLanahan (1986) find that approximately one in two single mothers lives below the poverty line. Furthermore, the disruption of the family implies a reduction of income for both spouses. Duncan and Hoffman (1985) estimate that the income of mothers and their children after divorce is 65% of their pre-divorce income, whereas the income of divorced men is about 90% of their pre-divorce income. Recently, Page and Stevens (2004) have found that, in the year following a divorce, family income falls by 41 percent and family food consumption falls by 18 percent. Six or more years later, the family income of the average child whose parent remains unmarried is 45 percent lower than it would have been if the divorce had not occurred. In addition, residential mobility is more likely in one parent (mother only) households, which implies an adjustment not only to a new neighborhood and new living conditions but also the loss of social networks (McLanahan and Booth, 1989). Specifically referring to unilateral divorce, Cáceres-Delpiano and Giolito (2008) find, using 1960-1980 Census data, that mothers of children aged 6-15 face a 4% to 6% decrease in family income because of the reform, independently of whether they divorce or not.

It is a well-known fact that single-headed households, and especially those of black young mothers, are concentrated in disadvantaged neighborhoods with higher crime rates and poverty, low rates of employment and poor educational facilities (Wilson, 1987), with all these factors being positively related to the engagement in a criminal career. Additionally, children who have gone through a divorce are more likely to live in a household where their mother is working, and therefore have less supervision. Sampson (1987) examines race-specific rates of robbery and homicide by juveniles and adults in over 150 U.S. cities in 1980, finding that black family disruption substantially increases the rates of black murder and robbery, especially by juveniles, although the effects are similar to those

of white family disruption on white violence. His main hypothesis is that variations in rates of black family disruption are positively related to rates of black criminal behavior, independent of those factors (e.g., poverty) associated with families headed by females and frequently hypothesized as providing motivation for crime. To the extent that the disruption of families are linked primarily to the social control of juveniles and their peer groups, the effect of family structure on crime should be strongest for juveniles.

3. Empirical Strategy and Data

We follow Friedberg's (1998) coding of state divorce regimes and the dates of divorce reforms.¹¹ There are five states that have not yet adopted any form of unilateral divorce: Arkansas, Delaware, Mississippi, New York and Tennessee. Of the forty-six states that currently have unilateral divorce regimes, eight had adopted some variant of unilateral divorce before the no-fault revolution during the early 1970s.¹² In our analysis, we consider as "adopting states" those 38 states (including the District of Columbia) that adopted unilateral divorce in 1968 or later; while the remaining 13 states are considered "control states".¹³

In the first part of our analysis, 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 aggregate

¹¹ However, our results are robust to alternative divorce coding such as that from Gruber (2004). The most important difference with Gruber (2004) coding comes from the fact that he considers as "non-adopting" those states that have unilateral divorce as well as separation requirements: Illinois, New Jersey, Ohio, Pennsylvania, South Carolina and the District of Columbia. In our analysis, we define an additional dummy variable that captures whether a state has separation requirements. See Table 1 for details.

¹² They are Louisiana, Maryland, North Carolina, Oklahoma, Utah, Vermont, Virginia and West Virginia. These states are coded as "Pre-1968" in Column 1 of Table 1.

¹³ This approach is similar to the one taken by Stevenson and Wolfers (2006).

crime and arrest rates. Consequently, we use state-based panel estimation, including state and time fixed effects as well as state-specific trends. We opted for a dynamic specification, allowing the impact to vary by time since the reform was introduced, in order to differentiate short run from long run impacts of the reform (Wolfers, 2006).

In the second part of our study, we use age-specific arrest and murder rates. Here we take advantage of an additional source of variation that comes from the age that a cohort faced the unilateral reform. This second source of variation allows us, first, to identify differential effects between individuals that have faced the reform at different points of their life. Since the source of variation is at the state-age-year level, we are able to control for confounding factors that might operate at the state-year, state-age or age-year level (Foote and Goetz, 2005; Donohue and Levitt, 2006).

All our regressions are state population weighted.¹⁴ Finally, to control for serial correlation, we correct the standard errors by clustering by state, following Bertrand, Duflo, and Mullainathan (2004).

The crime data in our analysis comes from the FBI's Uniform Crime Report program (UCR) and from the Supplemental Homicide Report. The UCR data consists of information at the state level for the eight types of crimes that are considered the most important because of their nature or volume among all offenses (Part I offenses). These felonies are classified into two groups: Violent Crimes¹⁵ and Property Crime. Violent crime includes murder and non-negligent manslaughter, forcible rape,

¹⁴ We get similar conclusions for unweighted regressions.

¹⁵ The Uniform Crime Reporting (UCR) Program defines as violent crimes those that involve force or threat of force. The classification of these offenses is based on police investigation as opposed to the determination of a court, medical examiner, coroner, jury, or other judicial body. For more details, see the Uniform Crime Reporting codebook at <http://www.fbi.gov/ucr/handbook/ucrhandbook04.pdf>.

robbery, and aggravated assault. Property Crime includes burglary, larceny-theft, motor vehicle theft, and arson.

Because the FBI data rely on police reporting, there are often problems of underreporting or downgrading of crimes. However, the use of aggregate information at different levels (state-year, state-year-age or state-year-race), as well as analyzing different types of offenses allow us to draw conclusions based on results that are less sensitive to measurement errors.¹⁶

In this paper we use, first, the crime rates reported at state-year level for the period 1965-1998.¹⁷ We also use data on the number of arrests by type of offense. The number of arrests reported to the FBI UCR Program each year by police agencies in metropolitan statistical areas in the United States is disaggregated by age, sex, and race.¹⁸ The age detail is at single age for 15 to 24 year olds and grouped for the other ages. This level of detail is useful since it allows us to identify not only the population affected by the reform, but also the potentials channels. The third source of data consists of homicide offenders from the FBI's Supplemental Homicide Reports (SHR). These data are also available by state, year and single-year of age for the period 1976-1999; unlike the data on FBI UCR arrests, all ages are identified. The SHR account for approximately 92 percent of all known homicides, and just 4 percent of the cases lack information on age. The most significant problem in

¹⁶ Stevenson and Wolfers (2006) check the FBI counts of total murders each year by state against murder counts gathered by the National Center for Health Statistics (NCHS). They find that these two data sources provide murder counts that are consistent with each other.

¹⁷ The UCR data has information for the whole period 1960-2005 with the exception of NY for which the information is available as of 1965. Therefore, we restrict the analysis to the period 1965-1998, because for other covariates the information is not available beyond the selected period. Nevertheless, we checked the robustness by estimating a model for the whole period without controls; the results do not change qualitatively.

¹⁸ However, this level of the disaggregation is either by age, sex or race; that is, we are unable to identify rates at age-race cell level.

using SHR data to analyze offender characteristics, however, is the sizable and growing number of unsolved homicides contained in the data file. In this paper we used the imputation method created by Fox and Zawitz (2004).¹⁹

In order to construct the arrest and homicide (SHR) rates, we use three sources of population counts. The state population by year is from Donohue and Wolfers (2005). We also use population counts disaggregated by state and age from US Intercensal County Population Data, which is available for all years starting in 1970. We finally use population counts disaggregated by state, year and race from the National Cancer Institute (SEER Population estimates), available since 1969. (see data Appendix).

Figure 1 shows the comparative evolution between Adopting and Non-Adopting States for raw Violent and Property Crime rates, respectively. For each of the panels we introduce two vertical lines signaling the years 1970 and 1975, which indicate the period that most states adopted the unilateral divorce law (see Table 1). We see, first, that adopting states have a lower incidence of violent crimes than non-adopting states. On the other hand, however, adopting states have higher incidence of property crime for the period under analysis. Second, after (and not before) the unilateral reform started there is monotonic reduction in the gap in violent crime rates between adopting and non-adopting with almost no observable difference in the 1990's. On the other hand, the gap between adopting and non-adopting states in terms of property crime rates seems stable during the period, with a marginal tendency to increase after 1985.

¹⁹ With this methodology, offender profiles for unsolved crimes are estimated based on the offender profiles in solved cases matched by victim age, sex, and race as well as year and state. However, our results are robust to the use of non-imputed offender data.

Figure 2 shows the evolution of violent crime by its different components. In order to stress the evolution during the period under analysis, we index the different crime rates by using 1965 as base year. Three elements are worth noting. First, the evolution of violent crimes goes hand in hand with the evolution of aggravated assault, which is the most significant type of violent crime in terms of volume. Second, adopting states did not begin catching up in terms of violent crime rates until 1975, which is, at least at a descriptive level, consistent with non-preexistent trends driving this profile during this period. Third, the faster relative growth of violent crime rates among adopting states in relation to non-adopting states occurs for the periods 1975-1980 and 1985-1990. In fact, after 1990 both adopting and non-adopting states follow a similar profile for all violent transgressions, that is, a well-known and documented drop in crime rates after 1995. Finally, for “Murder” and “Forcible Rape” offenses, there is a mid-run relative increase among adopting states, which at the end of the period, however, is not perceptible.

4. **Aggregate Crime and Arrest Rates**

The following expression represents the first specification of interest,

$$\log(y_{st}) = \delta_s + \eta_t + \beta Z_{st} + \pi_s trend + \sum_c \gamma^c U_{st}^c + \varepsilon_{st}, \quad (1)$$

with y_{st} representing one specific crime or arrest rate for state s at time t , δ_s and η_t , represent state and year fixed effects, respectively. Finally, Z_{st} stands for time-varying aggregate and policy state variables. Among these time-varying covariates we distinguish three groups of variables: Demographic variables, State Aggregate and Policy variables, and finally variables that have been associated with crime by previous studies. Demographic covariates include the fraction of Blacks, the state poverty rate, fraction of foreign-born population, the fraction of people living in

metropolitan areas, the state-year age structure of the population and the interaction of the fraction of Blacks with the poverty rate, and fraction in a metropolitan area. Policy variables are the log of per capita income, the unemployment rate, and dummies for the requirement of fault for property division, separation requirements, and equitable division of property, and covariates for welfare benefits²⁰. Other variables are dummies for abortion accessibility by state and year and for the five states that legalized abortion prior to *Roe vs. Wade*,²¹ a dummy variable for the existence of the death penalty, the log of the lagged incarcerated population, and a dummy for the crack introduction in the mid-late 80's.²²

The variable of interest is U^c_{st} , which represents a series of dummy variables that take a value of one for those states that have adopted the unilateral reform after “ c ” years. To estimate the impact of the reform, γ^c , we rely on the standard source of identification that is usual in the literature on unilateral divorce, a Differences-in-Differences approach; not all states moved to the unilateral regime and those that adopted these new divorce laws did not move simultaneously to the new regime. We also include a state-specific trend, π_s . By doing so, we try to prevent the estimated impact of the unilateral reform from just capturing pre-existing trends in the selected outcomes.

²⁰ Welfare benefits variables include a dummy indicating the existence of the Aid to Families with Dependent Children Unemployed Parent Program (AFDC-UP), the food stamp guarantee for a family of four with no other income and the maximum AFDC rate for a family of four.

²¹ Abortion was nationally legalized in 1973 with *Roe vs. Wade*. Nevertheless, five states (Alaska, California, Hawaii, New York, and Washington) had legalized or quasi-legalized abortion around 1970. Donohue and Levitt (2001) show that the five states that allowed abortion in 1970 experienced declines in crime rates earlier than the rest of the nation, which legalized in 1973 with *Roe v. Wade*. However, Joyce (2004) argues that Donohue and Levitt's evidence that crime fell earlier and faster in the early legalizing states may be spurious, a result of the differential timing in the evolution of crack markets.

²² According to Fryer et al. (2005), crack cocaine emerged in the mid-1980s, peaking in the early-1990s, before falling slowly thereafter.

Table 2 presents estimates of γ^c in equation (1) with the natural log of violent crime as a dependent variable.²³ Each column corresponds to a different specification with the later columns controlling for an increasing number of covariates. The first column presents the basic model (“Basic”) including dummies by year and state just as explanatory variables. The second column includes demographic variables; Column 3, state aggregate variables; Column 4, variables that have been related to crime in the literature and Column 5, a linear trend whose impact varies by state.

The results in Table 2 are robust across specifications. First, for all specifications we find that the unilateral reform is associated with an increase in the average violent crime rate for the period under analysis.²⁴ The magnitude of this impact varies from 8.4 to 12.2 percent for the complete period (35 to 51 violent crimes for every 100,000 people), depending on the specification, which is reported at the bottom of each column. Second, the impact monotonically increases during the years under analysis. While we cannot distinguish a statistically significant impact during the first 10 years following the reform, we do see the impact unfolding over the mid to long term. For all five specifications, we find a positive and statistically significant impact between 7 and 20 years after the introduction of unilateral divorce. However, for our preferred specification (Column 5) the coefficients are not significant 18 years after the reform was passed.

In Table 3, we break violent crime down into its respective sub-definitions and present the results for the log of property crime using our preferred specification from Table 1 (including state-specific time trends). The results are in line with Figure 2: the impact on violent crime is driven by the

²³ The same analysis was done for the log of property crime, finding no significant impact for any specification.

²⁴ The average impact over the 20 years under analysis is just a linear combination of the coefficients in the regression. That is $(\sum \hat{\gamma} * t)/20$, where t is the length of the period (in years) associated to the dummy variable U^c_{st} .

impact on aggravated assault, while we do not find a statistically significant effect on property crime rates. Notice that the impact on aggravated assault crime rates, as well as the effect that we find for violent crimes, is statistically significant starting seven years after the introduction of unilateral divorce with an average impact of approximately 11 percent on the period under analysis. That is, those states that adopted unilateral divorce faced an increase of approximately 28 offenses per 100,000 people that qualify as aggravated assault in relationship to non-adopting states. Finally, for some years we also find a significant impact on murder rates.

We check the robustness of the results by analyzing the timing of the changes and the sensitivity of the selection of the control group. A causal interpretation of the previous findings would be weakened if we found that crime rates were falling or increasing in adopting states prior to the enactment of unilateral divorce, compared with non-adopting states. In order to examine these issues, we add a series of leads to equation (1), coding dummies for whether the unilateral divorce law would be passed in 1 year, 2 years, and so on. Figure 3 shows the results for this modified specification for the three outcomes for which we find an impact: murder, aggravated assault and violent crime rates. A vertical line indicates the first year under unilateral regime. First, for the three crime rates, the coefficients of the dummies indicating the periods prior to the divorce law reform in no case are they (individually or jointly) statistically distinguishable from zero. Second, looking at the coefficients to the left of the vertical line (before the reform), we find no indication of a pre-existing trend in adopting states. Third, we obtain the same pattern if we include only state and year fixed effects in the regression, which suggests that our findings are not driven by other covariates included in the model. Finally, to check the sensitivity of the results with respect to the control

group, Table 4 replicates Table 2 just keeping adopting states²⁵. The results confirm our previous findings; unilateral divorce had a long run positive impact on violent crime rates, specifically on aggravated assault rates and, for some years on murder rates.

Arrest Rates: Heterogeneity by Race and Age Group

The introduction of unilateral divorce, in accordance with our previous findings, increases violent offenses; among them aggravated assault, and depending on the specification, murder rates. Now, using arrest data disaggregated by race and age, we analyze which groups are the most affected by the reform. Table 5 reports the estimates of model (1) for murder arrest rates (Columns 1 to 4), and for aggravated assault arrest rates (Columns 5 to 8). For each type of crime, we split the samples between the White and Black sub-samples. For aggregate murder arrest rates, Column 1 shows that the impact of the reform is non-monotonic. For the first four years the reform is associated with an approximately 21% increase in the murder arrest rates. Nevertheless for the fifth to the tenth year after the reform, we observe a reduction of the point estimates (and insignificant at 5%), which after the eleventh year following the reform start to rise and becoming significant again. Splitting the samples by race, we observe that this pattern is driven by the sample of Blacks (Column 3), while for the White sub-sample (Column 2) we only find effects for the four years following the reform. For aggravated assault rates, we also observe that the findings for the full sample are driven by the Black sample, but here we do not distinguish an immediate impact after the reform. For the overall period under analysis, our findings are equivalent to an average annual increase of approximately 30 percent in the murder arrest rate, and an approximately 39 percent increase in the case of aggravated

²⁵ Here the omitted category is the period preceding the reform.

assault arrest rates. Finally, in Columns 5 and 8 we repeat the analysis for the murder and aggravated assault arrest rates of Blacks using Gruber (2004) coding. The robustness of the results to this alternative coding is particularly important. Since Gruber classifies as “non-adopting” those states that, having unilateral divorce, also have separation requirements, the states of Illinois, New Jersey, Ohio, Pennsylvania, South Carolina and the District of Columbia are considered “control” states under this coding.²⁶

The above findings suggest, first, that most of the mid to long run impact appears to come from the sample of Blacks. Unfortunately, very little research has been done on the impact of the reform on African-Americans. However, Cáceres-Delpiano and Giolito (2008) find, using the 1970-1990 Census, that young black men aged 16-25 who were younger than age 4 at the time of the reform are more likely to be living in an institution than other black men from the same age group that were either not exposed to unilateral divorce or exposed at an older age.

A characteristic of these types of offenses is that they are concentrated among younger age groups. Therefore, given the characteristics of these offenses, we should see a higher relative impact among younger age groups. In order to check this, we split arrest rates by age of the arrestee. Tables 6 and 7 report the estimates of model (1) by five age groups for murder and aggravated assault arrest rates, respectively. Consistent with the types of crime being affected by the reform, we observe that the increase in arrest rates occurs among the younger age groups with non statistically significant impact for over-25 age groups. Specifically, the main impacts are concentrated among individuals between the ages of 15 and 19. Unlike previous results based on aggregated rates, the results for the group of

²⁶ In addition, Gruber (2004) considers Delaware as an adopting state, unlike Friedberg (1998). See Table 1.

15-19 year-olds are now significant for almost the whole period. This finding is consistent with an average annual increase of approximately 40 percent in the murder arrest rate (an increase of approximately 5 annual arrests per 100,000 people), and an approximately 35 percent increase in the average annual rate for aggravated assault (an annual increase of 60 arrests per 100,000 habitants). For the group of 20-24 year-olds we observe that the impact of unilateral divorce is only present for the first 4 years after the reform and for the period 13 to 14 years after its introduction in the case of murder arrest rates, with no significant impact for any period in the case of aggravated assault. This pattern is consistent with an earlier start in relatively less serious offenses like aggravated assault, and a later escalation to more serious offenses like murder. The lack of a significant impact beyond the fourth year after the reform for the murder arrest rate for the age groups 15-19 and 20-24 might be mechanical. An earlier arrest implies that some individuals are locked up for a time, preventing them from engaging in crime. For the period 1981-1995 the average time served for juveniles convicted for murder was approximately 112 months (approximately 9 years)²⁷, which almost matches the 8 years in which we do not observe an impact until 13 or 14 years after the introduction of unilateral divorce when younger cohorts arrive on the scene.

Figure 4 and Figure 5 provide a robustness check for our findings in Murder and Aggravated Assault arrest rates by age group. For both offenses, and specifically for the group of 15 to 24 years old, we find that arrest rates start to rise after the introduction of unilateral divorce.²⁸ Evidence in criminology is consistent with our findings. The types of crime affected by the reform and the timing of these impacts provide some explanation about the channels through which the reform is affecting

²⁷ Bureau of Justice Statistics, U.S. Department of Justice, <http://www.ojp.usdoj.gov/bjs/pub/html/cjusew96/ts.htm> .

²⁸ As well as with aggregated crime rates, the same profile is observed when we include just state and year fixed effects.

crime. Family structure is an element producing desistance from crime but its impact on crime depends on the point in the life cycle in which it occurs. Divorce not only increases the chances of a longer crime career but also increases the chances of more serious offenses (Laub and Sampson, 2001). Our findings of mid to long-term impact on violent-crimes are consistent with an effect on younger age groups who are negatively affected by the reform as children (Gruber, 2004; Cáceres-Delpiano and Giolito, 2008). In the next section, we further investigate this hypothesis by analyzing age-specific arrest and murder rates.

5. Age-Specific Arrest and Homicide Rates

The evidence so far points towards an impact of unilateral divorce on violent crime (murder and aggravated assault), principally in the mid to long term for the 15-24 age group. The analysis by race showed that the findings for the aggregate rates appear to follow closely from the ones for Blacks sub-sample. In this section, we introduce a second specification that allows us, first, to identify more precisely the group of the population most affected by the reform and, second, to introduce additional controls to check the robustness of previous results. We concentrate on age-specific arrests and homicide (SHR) rates of the population aged 15-24, since the Uniform Crime Reports record arrests by single year of age for this group only. In order to construct age-specific arrest and homicide rates we rely on the single-age population counts from the Census US Intercensal County Population Data, which is available for all years starting in 1970 (see data Appendix). Therefore, we restrict our analysis on age-specific arrest rates to the period starting in 1970.²⁹ Our second specification is the following:

²⁹ However, we obtain similar results if we construct rates back to 1965 by using linear interpolations of Census data.

$$y_{ast} = \alpha_s + \beta_t + \delta_a + \theta_{st} + \mu_{at} + \eta_{sa} + legal_{ast} + \sum_j \gamma^j U_{st} * 1\{a_j \leq (YB_{ast} - YUni_s) \leq b_j\} + \varepsilon_{ast}, \quad (2)$$

with y_{ast} now representing a crime rate for the population of age a , living in state s at year t .³⁰ Here $\alpha_s, \beta_t, \delta_a, \theta_{st}, \mu_{at}$ and η_{sa} represent state, year, age, state-year, age-year and state-age fixed effects, respectively. Furthermore, $legal_{ast}$ is a dummy variable representing whether abortion was already legalized in state s in the year of birth of population that is a years old at time t . In this case, U_{st} is a dummy variable that indicates that unilateral divorce was already legalized in state s in year t ; $1\{\}$ is an indicator function that takes a value of one when the logic statement $\{\}$ is true, and zero otherwise. YB_{ast} is the year of birth for individuals of age a at time t living in the state s ; $YUni_s$ is the year of adoption of unilateral divorce in the state s . That is, $(YB_{ast} - YUni_s)$ represents the age at the time of introduction of the reform. The parameter of interest, γ^j , can be interpreted as the *ceteris paribus* contribution of unilateral divorce of those cohorts of ages in the range $[a_j, b_j]$ at the time of the reform, compared to those individuals living in states that have not adopted unilateral divorce ($U_s = 0$). Seven age groups at the time of the introduction of unilateral divorce are defined; Born after the reform ($YB_{ast} - YUni_s > 0$), between 0 and 3 years old at the time of the reform, between 4 and 7, 8 and 11 and so on. In this last specification, three are the source of variation of the variable associated to the parameter γ^j . In addition to the differences in the timing of adoption of unilateral divorce and the fact that not all states have introduced it, we use in the specification the fact that the reform affected individuals at different points in their lives.

³⁰ Due to the high number of zeros in age-specific crime data, here we calculate elasticities evaluated at the sample mean instead of using logs, as we did in the previous section.

This specification also allows us to control for confounding factors that vary on the state-year level, that is, they affect everyone within a state in a particular year. One example of these factors are the temporary state-specific crime waves such as the introduction of new, illicit drugs, such as crack cocaine (Donohue and Levitt, 2001, 2004, 2006; Joyce, 2004, Foote and Goetz, 2005). Given that here the source of variation is by state-year-age, we are able to introduce state-year, state-age and age-year interactions (Foote and Goetz, 2005; Donohue and Levitt, 2006) and therefore remove any confounding effect operating at the state-year level, state-age level or year-age level.

Table 8 presents the results for murder, robbery, aggravated assault, violent and property arrest rates using our second specification. First, in contrast to our previous specifications, we now also find a significant impact for robbery and property arrest rates. Nevertheless, the relative impact is still more important for murder and aggravated assault arrest rates. Second, the coefficients for murder arrest rates are only significant for those individuals aged 4 years old or more at the time of the reform, with higher point estimates for those aged 20-24 when the law was passed. A similar pattern appears for robbery rates, significant for those who were older than 12. However, for aggravated assault the coefficients are increasing for the younger the individuals were at the time of the reform.

Homicide information, in general, is less likely than other types of offenses to suffer from measurement problems. The robustness for murder rates is shown in Table 9, which presents the estimates for murder arrest rates with each column representing a different specification. Observe in Column 5 that the results are virtually the same when we replace the variable “legal” by directly including the state abortion rates at the year of birth (Donohue and Levitt, 2001, 2004, 2006). We also check our results by considering two alternative unilateral divorce codings: Friedberg (1998)

without separation requirements³¹ and Gruber (2004) (Columns 6 and 7).³² In all cases, the point estimates for individuals who were 20-24 years old at the time of the reform are considerably higher than the coefficients for other age groups.

Finally, in Table 10, we also verify the robustness of our findings in murder arrest rates using homicide rates constructed from SHR data, available from 1976 to 1999. The results are very similar to those from arrest data, except that we do not observe the increase in the estimates for those individuals aged 20-24 at the time of the reform, probably because the period under analysis starts six years later.

When analyzing the murder arrest rates for the age group 15-24 by the timing since adoption of the law (Table 6, Columns 1 to 3), we find a significant increase in the first four years after the reform. The short-run increase for individuals aged 20-24 in Table 6 is equivalent to the result for those aged 20-24 at the time of the reform we showed in Table 9. This group is the least likely to be affected by parental family disruption because of the reform. In fact, some of these individuals could have already left the parents' household when unilateral divorce was introduced. The higher impact for this group might be linked to their own marriage disruption, or the greater selection into marriage caused by the law (Stevenson, 2007, Rasul, 2004) that may have prevented some marginal individuals from the benefits of marriage, a key factor in criminal desistance according to the criminology literature (Sampson, Laub and Wimer, 2006).

³¹ In this case, the states that require time of separation in order to grant a divorce are considered non-adopting.

³² Gruber (2004) considers unilateral divorce only in the case where there are no separation requirements.

It is also worth noticing that the point estimates for murder arrests and SHR homicide rates (Tables 9 and 10) do not show a significant impact for those groups born after the reform. This lack of significance raises some doubts about the long-term effects (more than 20 years after the reform) that are shown in Table 6 for the age group 15 to 19. Since we are now controlling for confounding factors at the state-year level, we may interpret that the negative effects fade for children born after the reform, consistent with a better selection into marriage and the decrease in the divorce rates 10 years after the reform. However, our findings in aggravated assault arrest rates, which appear to be increasing for the youngest cohorts exposed to unilateral divorce, are consistent with the long run increase in aggregate arrest rates (Table 5 and Column 2 of Table 7), and therefore striking.

6. Conclusion

In this paper, we study the impact of unilateral divorce on crime. Previous research has suggested that divorce laws affected marriage selection and produced some negative effects on individuals who faced the reform as children. Here we study whether those changes affected crime and arrest rates in states that passed unilateral divorce laws.

First, using data from the FBI's Uniform Crime Report program for the period 1965-1998 and differences in the timing in the introduction of the reform we find that unilateral divorce has a positive impact on violent crime rates, with an 8% to 12% average increase for the period under consideration. The analysis using arrest data not only confirms the findings of a positive impact on violent crime but also that these impacts are concentrated among those age groups (15 to 24) which are more likely to engage in these type of offenses. Specifically for the group of 15-19 year olds, we observe an average impact over the period under analysis of 40% and 36% for murder and aggravated assault arrest rates, respectively. When we disaggregate the arrest rates by race, we find

the results are driven by the Black sub-sample, while for the White sub-sample, we only find effects for the four years following the reform.

We finally analyze age-specific arrest and murder rates using the age at the reform as the second source of variation. We are not only able to confirm the positive impact on different types of violent crime but we also find a positive impact for property crimes. The results for murder arrest rates for the period 1970-1997 and for homicide rates for the period 1976-1999 (Supplemental Homicide Report) are robust with respect to specifications, specifically those that include year-state and year-age dummies. The results confirm, except for the case of aggravated assault, that individuals who were already born at the time of the reform (aged 4 and older for murder) are the ones who face the increase in the probability of engaging in crime. The magnitude goes from 15% to 40% depending on the specification and age at the time of the reform.

Data Appendix

Unilateral Divorce Coding

The coding for unilateral divorce and separation requirements comes from Friedberg (1998) and Gruber (2004). The coding for equitable division of property is from Rasul (2004). The coding for requirements of fault regarding property division is from Mechoulan (2005). See Table 1 for details.

Aggregate Crime and Arrest Data

Aggregate crime and arrest data used in the analysis are from FBI's Uniform Crime Report program (UCR) for the period 1965-1998. The UCR data has information for the whole period 1960-2005 with the exception of New York (since 1965). The data on crime rates is available from the Bureau of Justice Statistics web page:

<http://bjsdata.ojp.usdoj.gov/dataonline/Search/Crime/State/StatebyState.cfm>

Arrest data is from Chilton and Weber (2000), available from the Inter-University Consortium for Political and Social Research (ICPSR) web page: <http://www.icpsr.umich.edu>.

Supplemental Homicide Reports (SHR)

Data from SHR is from Fox (2004), available from the ICPSR web page:

<http://www.icpsr.umich.edu>.

Prisoners, and Death Penalty

Data on number of prisoners and the existence of death penalty are from Donohue and Wolfers (2005), available at Justin Wolfers' web page:

<http://bpp.wharton.upenn.edu/jwolfers/DeathPenalty.shtml>. The variable indicating the stock of incarcerated people have some states missing for the years 1965, 1968 and 1970-72.

Population

Population counts by year and state are taken from Donohue and Wolfers (2005). For counts at year-state-age level, we use the Census US Intercensal Population Data, which is available for all years starting in 1970. The data is available at the NBER website: <http://www.nber.org/data/census-intercensal-population/>

For population counts at the state-race-year level, we use data from the National Cancer Institute, Surveillance Epidemiology and End Results, available at <http://seer.cancer.gov/popdata/download.html>. For the period 1965-1969, we make a linear interpolation using IPUMS Census data for the years 1960 and 1970.

Abortion Rates

Data on abortion rates by state and year is from Donohue and Levitt (2006), available from John J. Donohue web page: <http://islandia.law.yale.edu/donohue/pubsdata.htm>.

Welfare Generosity, Unemployment rate, and Log of per capita Income

The data is from the “Moffitt Benefits File”, available from Robert Moffitt’s web page: http://www.econ.jhu.edu/people/moffitt/ben_data.txt.

Demographic Covariates

The share of Population living in Metropolitan areas, fraction of people below the poverty line, fraction of blacks and the age structure of the population (5-year cohorts) are constructed from March CPS data starting in 1977. Since for previous years some states are grouped in March CPS, we make a linear interpolation using IPUMS census data. The share of foreign-born population is also by linear interpolation of census data.

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