Further Tests of Abortion and Crime

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Abstract

The inverse relationship between abortion and crime has spurred new research and much controversy. If the relationship is causal, then polices that increased abortion have generated enormous external benefits from reduced crime. In previous papers, I argued that evidence for a casual relationship is weak and incomplete. In this paper, I conduct a number of new analyses intended to address criticisms of my earlier papers. First, I examine closely the effects of changes in abortion rates between 1971 and 1974. Changes in abortion rates during this period were dramatic, varied widely by state, had a demonstrable effect on fertility, and were more plausibly exogenous than changes in the late 1970s and early 1980s. If abortion reduced crime, crime should have fallen sharply as these post-legalization cohorts reached their late teens and early 20s, the peak ages of criminal involvement. It did not. Second, I conduct separate estimates for whites and blacks because the effect of legalized abortion on crime should have been much larger for blacks than whites since the effect of legalization of abortion on the fertility rates of blacks was much larger. There was little race difference in the reduction in crime. Finally, I compare changes in homicide rates before and after legalization of abortion, within states, by single year of age. The analysis of older adults is compelling because they were largely unaffected by the crack-cocaine epidemic, which was a potentially important confounding factor in earlier estimates. These analyses provide little evidence that legalized abortion reduced crime.

"Counterfactuals are at the heart of any scientific study."

James Heckman, 1996.

I. Introduction

In a recent paper, Donohue and Levitt (2004) claim that further analyses strengthen their conclusion that abortion lowers crime. If their improved estimates are taken literally, then a one-standard deviation increase in the abortion rate lowers homicide rates by 31 percent and can explain upwards of 60 percent of the recent decline in murder.[1] Given the staggering costs associated with crime, a causal relationship between abortion and crime has enormous implications for social policy. Indeed, the implications go beyond crime. If abortion lowers homicide rates by 20 to 30 percent, then it's likely to have affected an entire spectrum of outcomes associated with well-being: health, child development, schooling, earnings and marital status. Thus, further assessment of the identifying assumptions and their robustness to alternative strategies is warranted.

In this paper, I extend my previous work on abortion and crime with additional tests that confront many of the objections raised by Donohue and Levitt (2004). For instance, Donohue and Levitt argue that changes in abortion rates are a more appropriate test of its relationship to crime than the mere act of legalization. Thus, I regress changes in homicide rates on changes in abortion rates for cohorts born just before and after *Roe.* The change in abortion rates between 1971 and 1974 were large, plausibly exogenous and had a demonstrable effect on fertility. If the relationship between abortion and crime is causal and as significant as Donohue and Levitt

[1] In Donohue and Levitt (2001), the coefficient on the effective abortion rate in the homicide regression is –0.121. Their new "best" estimate is –0.166, an increase of 37 percent. Replicating their calculations from the original article, homicide was 31.5 percent lower in 1997 than it would have been in the absence of abortion, up from their original estimate of 22.9 percent.

claim, then it should be evident among cohorts that experienced the largest and most sudden increases in abortion rates.

The second extension is a more detailed analysis by race. The legalization of abortion had a greater effect on the fertility rates of blacks than whites (Joyce and Mocan 1990; Levine et al. 1999; Angrist and Evan 1999). All else equal, we should observe a greater decline in black relative to white homicide rates among the cohorts exposed relative to those unexposed to legalized abortion. A limitation of the racial analysis, however, is that population data are only available in five-year age groups. Thus, in the third extension I analyze non-race-specific homicide rates by single year of age. The advantage is that I am able to construct within-state comparison groups that were exposed to the same period effects (i.e., the crack-cocaine epidemic) but unexposed to legalized abortion *in utero.* As a result, I am able to difference out hard to measures period effects that vary by age and within-state. Donohue and Levitt, by contrast, rely on cross-state comparisons of changes in crime. Problems of omitted variables arise, if the onset and diffusion of crack-cocaine use and its spillover effects varied by state, year and age.

All totaled, I find no consistent association between abortion and crime. Changes in homicide rates among cohorts born before and after *Roe* are uncorrelated with abortion rates between 1971 and 1974. I do find a negative association between abortion legalization and the homicide rates of black teens and young adults when I use a cross-state comparison group, but no association when I use within-state counterfactuals. Finally, I find no association among whites and adults, groups for whom the crack-cocaine epidemic appears less relevant. Thus, even if one finds Donohue and Levitt's identification strategy compelling, the lack of robustness to alternative identification strategies undermines a causal interpretation of their estimates.

The plan of the paper is as follows. In section II, I contrast the identification strategy of Donohue and Levitt with my own. In section III, I present results based on homicide rates by single year of age. In Section IV, I analyze national legalization by regressing changes in homicide rates on changes in abortion rates pre- and post-*Roe* . In Section V, I analyze differences in homicide rates by race and Section VI concludes.

II. Difference in Identification Strategies

Donohue and Levitt (2001) contend that abortion reduces crime by lowering the rate of unwanted childbearing among women whose children are at risk of criminal behavior. As a test, they regress state crime rates between 1985 and 1996 on a weighted average of state abortion rates approximately 15 to 30 years earlier. In another set of analyses, they regress arrests or homicides by single year of age on the abortion rate in the year before a cohort was born. Their basic regression can be written as follows:

$$\mathrm{LnC}_{ajt} = \beta \mathrm{A}_{jt-a-1} + \sum a_a \mathrm{Age}_a \sum \lambda_j + \sum \phi_t + e_{ajt} \qquad (1)$$

where C_{ajt} is the natural logarithm of arrests or homicides for age group a, in state j, and year t; A_{jt-a-1} is the state abortion rate in year t-a-1. Thus, arrests of 24-year olds in 1990 in state j are correlated with the abortion rate in state j in 1965 (t-24-1). The model also includes age (Age), state (λ_j) and year (ϕ_t) fixed effects. Additional specifications include interactions between age and state and age and year, none of which affects their basic estimates.

Identification of the effect of abortion on crime in equation (1) rests on two assumptions: first, that states with greater abortion rates have lower rates of unwanted childbearing; and second, that there are no unobserved within-state time-varying effects that explain crime but that are also correlated with lagged abortion rates. Donohue and Levitt never demonstrate an inverse relationship between state abortion rates and state fertility rates. In fact, they argue that such an association is unnecessary. "…As long as the number of unwanted births falls, even if total births do not decline at all, one would expect to see better life outcomes on average for the resulting cohort" (Donohue and Levitt (2004, p. 33). But they never explain how to identify such changes. How, for example, does one distinguish variation in state abortion rates due to changes in sexual activity and contraception from variation in state abortion rates associated with unwanted births averted. For instance, an increase in contraception or a decline in sexual activity could lower both the abortion rate as well as the rate of unwanted childbearing. Indeed, the abortion rate in the US fell 19 percent between 1992 and 2000, yet evidence suggests that the proportion of births from unintended pregnancies was unchanged (Lipscomb et al. 1998). The upshot is that without a demonstrable inverse association between state abortion rates and state fertility rates, there is no way to distinguish state variation in abortion that is causally related to lower rates of unintended childbearing from variation in abortion due to changes in sexual activity and contraception.

A more credible source of identification, and one used in several recent analyses of abortion and well-being, is to compare changes in outcomes among cohorts exposed and unexposed *in utero* in the period just before and after legalization (Gruber, Levine Staiger 1999; Angrist and Evans 1999; Klick and Stratmann 2003; Charles and Stephens 2002; Joyce 2001; 2004). The advantage of a quasi-experimental design is twofold: first, the legalization of abortion had a substantial effect on fertility, and by extension unwanted childbearing, in the period immediately after reform (Sklar and Berkov 1974; Joyce and Mocan 1990; Levine et al. 1999; Angrist and Evans 1999). And second, the change in the abortion rate during this period is large and more plausibly exogenous than changes in later years.

Donohue and Levitt (2004), however, are critical of designs that rely on passage of a state law or court decision to identify changes in unwanted childbearing. Such pre-post analyses do not account for state variation in abortion and by extension, unwanted childbearing. This criticism is easily addressed. The Centers for Disease Control and Prevention (1972,1973) published data on resident abortions in 1971 and 1972 in the 45 states in which access to abortion remained restrictive prior to *Roe* in 1973 . With these data, and data from the Alan Guttmacher Institute, I construct changes in resident state abortion rates between 1971 and 1973 and between 1972 and 1974 in the 45 states in which abortion became legal following *Roe*. I regress the change in crime among cohorts born in 1972 and 1974 and 1973 and 1975 on the respective changes in abortion rates. As I demonstrate below, the average change in abortion pre and post *Roe* is the largest ever observed. More importantly, I stay within the quasi-experimental design by using variation in abortion that is more plausibly exogenous than changes 8 to 10 years later. Finally, there is evidence that the decrease in fertility rates during this period was directly related to the legalization of abortion (Levine et al. 1999).

The second major difference between Donohue and Levitt's identification strategy and mine is the use of cross-state versus within-state comparisons. With state and year fixed effects, the coefficient on abortion (β) in equation (1) reflects a comparison of cross-state changes in crime and abortion (Moffitt 1998). Put differently, if the abortion variable in equation (1) were a dichotomous indicator of abortion legalization, then β would estimate the cross-state difference-in-differences ($DD_{\text{cross-state}}$). Contrast this to a within-state difference-in-differences estimate ($DD_{\text{within-state}}$) in which changes in crime among those exposed to abortion *in utero* are compared with changes in crime among groups in the same state, but who were unexposed to abortion.

The distinction between cross-state and within-state counterfactuals becomes significant in the presence of strong period effects that vary by state and year. For instance, youth homicide rates rose dramatically in the mid-to-late 1980s. The rise has been attributed to the development of illegal markets in crack cocaine, their control by gangs, and the proliferation of handguns (Blumstein 1995; Blumstein, Rivara and Rosenfeld (2000). To illustrate, Figure 1 shows drug-related homicide rates by age in the 45 states in which abortion became legal following *Roe*. Figure 2 shows the same in the six states that legalized abortion prior to *Roe*.[2] I refer to the latter as the repeal states. Data are from the FBI's Supplemental Homicide Reports (SHR)[Fox 2000a].[3] Figures 3 and 4 show homicide rates also by age and state in which guns were used. As is apparent in Figure 1, the homicide rates of teens and young adults begin to rise rapidly around 1987. The change in drug-related homicides is much more muted among adults 25 to 34 years of age. In the repeal states (Figure 2) we observe a roughly similar pattern but with an important distinction: the increase in homicide rates begins earlier, between 1983 and 1985, and peaks sooner, around 1989-1990. The pattern among gun-related homicide rates in Figures 3 and 4 mirrors that of drug-related homicides. The important point is that whatever shock caused homicide rates to rise rapidly, it varied by state, year and age.

The nature of these shocks and the lack of data with which to quantify them makes it difficult to identify cohort from period affects with available methodologies. For instance, Donohue and Levitt's specification (equation 1) will yield spurious estimates if lagged abortion rates are correlated with the rise and fall in crack.[4] An instrumental variable approach will also

[2] I treat Washington D.C. as an early legalizing state given the decision in *United States v. Vuitch* and the fact that the abortion rate in Washington D.C. exceeded that of every state in the US, including New York and California in 1971 (Centers for Disease Control 1972). However, because Washington D.C has such a small population, none of my conclusions are sensitive to this decision (Joyce 2004).
[3] Unless indicated, I use the un-imputed counts of homicide in which age and race were reported.
[4] One solution is to include state-specific trend terms. Such regressions yield nonsensical results because the trend terms wipe out the variation in abortion rates.

fail if the instrument—a state/year-of-birth interaction—is correlated with state-year changes in crack markets as Figures 1-4 suggest. Similarly, a difference-in-differences strategy will be compromised if the comparison group is differentially affected by exposure to crack. In sum, there is no way to prove that one identification strategy is superior to another. All that can be done is to articulate the underlying assumptions and to present supporting evidence in a transparent manner. This is the approach I take below.

III. DDDs by single year of age

In this first set of analyses, I compare homicide rates by single year of age before and after the legalization of abortion in the repeal states in 1970. The period-cohort diagram in Table 1 illustrates the design. The groups exposed to legalized abortion were born in 1969 and 1971. The comparison groups were born in 1967 and 1969. Consider, for example 19 and 20 year olds born in 1969 and 1971. The difference-in-differences estimate in repeal states (DD_{repeal}) contrasts changes in homicide rates among 19 and 20 year olds to changes among 21 and 22 year olds born in 1967 and 1969. Both groups are exposed to the same period effects (1988-1991), but only the younger ages are exposed to legalized abortion *in utero.* I compute the same estimate in non-repeal states and subtract the $DD_{nonrepeal}$ from DD_{repeal} to arrive at the difference-in-difference-in-differences (DDD) estimate. I obtain similar estimates for the full set of teen and adult comparisons up to 28 years of age.

The key to the analysis in Table 1 is the credibility of the counterfactual. Threats to internal validity can be lessened if the level of crime and the pre-intervention trend are similar between exposed and comparison groups (Meyer 1995). If not, then estimates may be sensitive to functional form and choice of counterfactual. Donohue and Levitt (2004) provide a good example of the pitfall. In one analysis they compute a series of difference-in-differences-in-differences (DDD) estimates for various age groups. Specifically, they compare changes in the natural log of homicides among those exposed and unexposed to legalized abortion in repeal states to similar changes in non-repeal states. They report that the average of the DDDs is negative and statistically significant from which they conclude that their original findings are robust to within-state and cross-state comparison groups.

I have replicated their results from Table 2 of Donohue and Levitt (2004) and reported them in column (4) of Table 2. Donohue and Levitt contend that my focus on 15 to 19 year olds is confounded by crack and that the positive DDD for that group is not representative of the full set of possible experiments. Indeed, the same exercises for 17 to 21 year olds and 18 to 22 year olds yield negative and statistically significant estimates. Moreover, the average of all the DDDs indicates that homicides fell nine percent more among those exposed as compared to those unexposed to legalized abortion (last row, $p<.01$).

Their analysis, however, has two major limitations. First, they compare relative changes in the *number* of homicides in 6 states to relative changes in the *number* of homicides in 45 states. The baseline discrepancy in homicides between repeal and non-repeal states differs by a factor of two and thus, small absolute changes in homicides in repeal states represent large relative changes. To illustrate, I re-estimate the exact same regressions using the natural log of homicide rates instead of the log of homicides. The results are displayed in column (5) of Table 2. Every negative coefficient either switches sign or becomes smaller in absolute value when compared to their estimates in column (4). The average for all estimates is now positive and statistically insignificant (last row).

The second limitation in Donohue and Levitt's DDD analysis is that trends in homicides among age groups exposed to legalized abortion are often quite different from trends among the comparison groups. Figures 5 and 6 illustrate.[5] Each figure shows the number of homicides in repeal and non-repeal states from 1980 to 1999. Consider the experiment involving 18 to 22 year olds, groups for which they found large negative effects (Figure 5). Donohue and Levitt compare the relative change in homicides among 18 to 22 year olds to relative changes among adults 23 to 27 years of age between 1988 and 1993. What is immediately apparent is that by 1988 the number of homicides among 18 to 22 years olds in both repeal and non-repeal states has begun to diverge dramatically from the slightly downward trend in homicides among the comparison group. As a result, the DDD as estimated by Donohue and Levitt is largely reduced to a cross-state DD among 18 to 22 years olds, since homicides vary little among the within-state comparison groups. But cross-state comparisons are problematic given the apparent differences in the timing of the crack epidemic between repeal and non-repeal states as well as the large difference in the number of homicides between the two groups of states.

Return, therefore, to the analysis that I have outlined in Table 1. The associated regression model is of the following form:

$$\begin{aligned}\mathrm{LnH_{ajt}} &= B_0 + B_1\mathrm{Exposed} + B_2(\mathrm{Exposed*Repeal}) + B_3(\mathrm{Exposed*After}) + B_4(\mathrm{Repeal*After}) \\ &+ B_5(\mathrm{Exposed*Repeal*After}) + \sum_t \tau_t + \sum_j \lambda_j + e_{ajt} \qquad (2)\end{aligned}$$

where H_{ajt} is the homicide rate of age group a, in state j, and year t; *Exposed* is one for cohorts born in 1969 and 1971; *Repeal* is one if homicides occurred in repeal states; and *After* is a dichotomous indicator of the post-legalization period in calendar years. The last two terms are for year and state effects, respectively. The DDD is estimated by β_5 and the DD_{repeal} by $(\beta_3+\beta_5)$.

[5] For consistency with Donohue and Levitt (2004), I use the imputed estimate of homicides from the SHR.

There are several advantages to this design. First, I use a within-state comparison group with roughly similar levels and trends in homicide rates in years prior to exposure. Figures 7 and 8, for instance, show homicide rates by single year of age for those 17 to 20 in repeal and non-repeal states, respectively. Homicide rates are similar in 1980, rise steeply through the late1980s and by 1999 have fallen to or below their 1980 levels. Importantly, homicide rates are between 50 and 100 percent greater in repeal relative to non-repeal states, which provides further support for a within-state comparison group. Second, the pre-post period is narrow, approximately three years, which serves to lessen the impact of diverging trends.[6] Third, fertility rates in repeal states fell more between 1969-1971 than in subsequent years, which provides a relatively large and plausibly exogenous decrease in unwanted childbearing (Sklar and Berkov 1974; Joyce 2001, Table 6). Fourth, I am also able to estimate what can be referred to as the "cross-state" DD in which I compare changes in homicide rates for the exposed age group in repeal states to changes in the same age group in non-repeal states. This is the standard fixed-effects estimate that I have argued against because there is no adjustment for time-varying shocks within states, such as the rise and diffusion of crack markets. Contrasts between the within-state and cross-state DDs provide insight as to the sensitivity of estimates to the respective comparison groups.

Lastly, the repeated comparisons over many years provides periods that are largely uncontaminated the crack-cocaine epidemic. Consider, for instance, the homicide rates of adults 27 to 30 years old in Figures 9 and 10. The vertical lines mark the years before and after 27 and 28 year olds in repeal states are exposed to legalized abortion *in utero*. The experiment is compelling because there is no rapid rise in homicide rates in the 1980s as among teens and

[6] One limitation of a tight pre-post-exposure period is the two-year window for the year of birth when assigned from year of crime. An 18-year-old who commits a homicide in 1989, for example, could have been born in January of 1970, if he were 18 years and 12 months in January of 1989. Similarly, an 18-year-old who just turned 18 in

young adults. Indeed, the crack epidemic was largely over by 1996, the beginning of the experiment. In addition, and more importantly for a quasi-experimental design, the level of homicide rates in repeal and non-repeal states are more similar among the older as compared to younger age groups and they follow a similar trend over the sixteen years leading up to exposure.

Table 3 shows estimates of the DDD, DD_{repeal} and $DD_{cross\text{-}state}$. Each row represents a separate regression. The younger age group in repeal states moves from unexposed to exposed over the designated years while the within-state comparison, or older age group, remains unexposed. The majority of the DDD and DD_{repeal} estimates are positive and the averages are small and statistically insignificant (last row, columns 4 and 5). In cases involving 19-22 year olds, 20-23 year olds and 21-24 year olds, however, the estimated DDD is negative and large in magnitude. But the negative estimates appear related to rapidly diverging trends among 19- and 20-year olds as Figures 11 and 12 illustrate. The homicide rate for 20-year olds, especially in repeal states (Figure 11), diverges significantly from the other three series in 1987. The rapid increase in homicide rates in the late 1980s among 19- and 20-year olds makes it difficult to find an appropriate counterfactual among older age groups (Figures 7 and 8).

The apparent confounding from crack among youth homicide rates makes the repeal-state DDs for older adults more compelling. As shown in Table 3 there is no statistically significant association between legalized abortion and crime for any age group 22 years of age and older. The DD_{repeal} and DDD are mostly positive and although the $DD_{cross\text{-}state}$ estimates are negative they are generally small with t-ratios less than one.

The other noteworthy result is that estimates of DD_{repeal} differ substantially from those of $DD_{cross\text{-}state}$ (column 5 vs. 6). Only five of the 15 DD_{repeal} estimates are negative and none are

December of 1989 was born in December of 1971. For this reason, I use comparison groups that are two years older instead of one to lessen such misclassification (see Table 2).

statistically significant (column 5). By contrast, 12 of the 15 $DD_{cross\text{-}state}$ are negative (column 6). Thus, the conclusion one draws from this exercise depends on which counterfactuals appear more appropriate. However, the very sensitivity of the results to this choice weakens a causal interpretation.

In sum, DDD estimates in Table 3 are an improvement over previous tests because they narrow the difference in ages between the exposed and unexposed groups and they provide tests during a period in which confounding from crack appears less relevant. They also underscore the choice of counterfactual. Donohue and Levitt (2001, 2004) rely almost exclusively on a comparison of changes in homicides across states, which is the standard fixed-effects estimator. As I demonstrate, however, both the level and trend in homicide rates vary substantially between repeal and non-repeal states, which makes cross-state comparisons problematic. To be fair, however, Donohue and Levitt's (2004) preferred specification uses changes in the abortion rate as a measure of unwanted childbearing, and not simply an indicator of legalization. Thus, in the next set of analyses, I associate changes in homicide rates with changes in the abortion rate before and after national legalization. The important difference between these regressions and those of Donohue and Levitt (2001, 2004) is that I limit the analysis to the years immediately before and after *Roe*, a period during which abortion had a demonstrable effect on fertility.

IV. National legalization following *Roe*

One limitation of using early legalization in the repeal states is the potential endogeneity of the legislation. Although the legalization of abortion in New York was a shock to many, including the leadership of the Catholic Church, New York and California were clearly liberal states in which sentiment towards abortion was arguably more permissive (Lader 1974; Garrow

1998). Tietze (1973), for instance, estimates that two-thirds of legal abortions in New York City between July, 1970 and June,1971 replaced illegal abortions from a year earlier. The legalization of abortion following *Roe*, therefore, offers a second experiment that may be less contaminated by policy endogeneity at the state level. The difficulty, as Donohue and Levitt (2004) point out, is that a before and after analysis based on a national change assumes a uniform effect in all states and is susceptible to confounding from broad trends.

To overcome the lack of variation in exposure to legalized abortion at the state level, I use the change in resident state abortion rates pre- and post-*Roe* as my measure of the treatment.[7] Specifically, I associate two sets of changes: log homicide rates for cohorts born in 1972 and 1974 with the change in abortion rates between 1971 and 1973; and log homicide rates for cohorts born in 1973 and 1975 with the change in abortion rates between 1972 and 1974. The exercise has several advantages as a test of abortion and crime. First, the change in abortion immediately after *Roe* is more plausibly exogenous than the changes that occurred in the late 1970s and early 1980s. Second, *Roe* had a demonstrable effect on fertility in states furthest from repeal states (Levine et al. 1999). And third, the change in abortion rates is often large and varies widely by state. To illustrate the latter point, I display the change in resident abortion rates by state between 1971 and 1973 ranked by the magnitude of the change in Table 4. In quartile one, for example, the mean abortion rate increased by 0.77 abortions per 1000 women 15 to 44 years of age.[8] In the fourth quartile, abortion rates increased by 11 per 1000 women of reproductive age, an increase more than 10 times greater than the change observed in quartile one. The average change for all 45 states is 5.5 between 1971 and 1973 and 5.3 between 1972

[7] Resident abortions in 1971 are from the CDC (1972, Table 5). Resident abortions in 1973 are from Forrest, Sullivan and Tietze (1979)
[8] Even if I remove New Mexico, the average change is only 1.6 abortions.

and 1974; both are the largest recorded two-year changes in abortion rates in the 45 states since *Roe*.[9]

The regression model is characterized by equation (3) below. The are 45 states, 2 sets of differences and 5 age groups. The dependent variable is the change in the log of homicide rates by age, state and year.

$$\Delta \ln H_{ajt} = \alpha_0 + \alpha_1 \Delta\text{Abrate} + \alpha_2 \text{C1} + \alpha_3 \text{C2} + \alpha_4 \text{C3} + \alpha_5 \text{C4} + \alpha_6 (\Delta\text{Abrate*C1}) + \alpha_7 (\Delta\text{Abrate} * \text{C2}) + \alpha_8 (\Delta\text{Abrate} * \text{C3}) + \alpha_9 (\Delta\text{Abrate} * \text{C4}) \quad (3)$$

To make the discussion concrete, consider the homicide rates of 17 year olds in 1989 and 18 year olds in 1990. I assume that both were born in 1972. Similarly, I assume that the 17 and 18 year olds in 1991 and 1992, respectively, were born in 1974. Thus, I regress the change in homicide rates among 17 year olds between 1989 and 1991 and 18 year olds between 1990 and 1992 on the change in the abortion rate between 1971 and 1973 *(ΔAbrate*). As a check against spurious associations, I analyze the change in homicide rates over the same years to older age groups who were born in or before 1972, and thus could not have been affected by the change in abortion. The variables *C1–C4* are the indicator variables for these older age groups and $\Delta Abrate{*}C_k$ the interactions with the change in the abortion rate between 1971-1973 or 1972-1974. Thus, α_1 shows the relative change in homicide rates given a change in the abortion rate for cohorts exposed to abortion in the wake of *Roe*. If abortion lowers crime, then α_1 should be negative.

Results from the estimation of equation (2) are reported in Table 5. Column (3) shows the estimate of α_1 from equation (3) for each exposed group; columns (4)–(7) show estimates of $\alpha_1 + \alpha_k$ (k=6,..9), the effect of changes in the abortion rate on changes in homicide rates of the

[9]Inchoate surveillance systems in many states may have resulted in an under or even overestimation of abortions by state of residence. Thus, I also use the resident abortion rate in 1973 to proxy the change in abortion before and after *Roe.* This is consistent with Donohue and Levitt (2001), who assume that the abortion rate in the non-repeal states was zero prior to 1973. The estimates do not change meaningfully.

unexposed groups. There is little evidence that changes in abortion rate pre-and post *Roe* are associated with homicide rates. The average of the coefficients for the exposed groups is small (-0.008) and statistically insignificant. For instance, a change in the abortion rate of 5.5 per 1000 women, the average increase between 1971 and 1973, would be expected to lower homicide rates 4.4 percent (column 3, last row). In addition, only one of the 6 coefficients among the exposed groups is negative and statistically different from zero (23-24 years olds). Further, changes in crime among the comparison groups are sometimes negative, statistically significant and larger in absolute value than the changes among the exposed group. Consider, for example, 21 and 22 years olds as the exposed group. The coefficient on the change in the abortion rate, α_1, is -0.007. However, changes in homicide rates among 27-28 and 29-30 year olds are larger in absolute value (-0.017 and –0.033, respectively) and statistically significant.

The simple analysis presented in Table 5 is quite damaging to Donohue and Levitt's argument that abortion lowers crime. In no other period did measured abortion rates increase more than between 1971 and 1974. Moreover, the change appears as the direct consequence of national legalization. Further, I found large spurious effects among cohorts born years before legalization. I turn, therefore, to the last set of analyses based on race-specific homicides rates, a dimension that Donohue and Levitt neglect to exploit.

V. Analyses by Race

Crime and abortion rates vary significantly by race. The national homicide rate is approximately 8 times greater among blacks than whites and the abortion rate is approximately two and a half times greater (http://www.ojp.usdoj.gov/bjs/homicide/hmrt.htm; Henshaw and Van Vort 1992). More importantly for this analysis, the legalization of abortion had a much greater impact on the fertility rates of blacks than whites and thus, its effect on crime among blacks should be greater (Angrist and Evans 1999; Levine et al. 1999; Joyce and Mocan 1990).

There are, however, two difficulties with an analysis by race. First, the crack epidemic appeared to have a much greater effect on the homicide rates of blacks than whites (Blumstein, Rivara and Rosenfeld 2000). The other limitation is that population by state, year, age and race is only available in five-year groupings. Given these restrictions I undertake a series of comparisons that again exploit early legalization of abortion in repeal states. Specifically, I compare the change in homicide rates among 15 to19 year olds between 1984 and 1989 to those of young adults 20 to 24 years of age. I repeat the analysis and compare the homicide rate of 20 to 24 year olds between 1989 and 1994 to that of 25 to 29 year olds over the same period. Finally, I compare changes in homicide rates among 25 to 29 year olds between 1994 and 1999 to those of 30 to 34 year olds. For each period, the younger age group goes from unexposed to exposed to legalized abortion *in utero* in repeal states only. The older age group remains largely unexposed. As before, I refer to this contrast as the difference-in-differences for repeal states (DD_{repeal}). I then subtract the DD for non-repeal states from DD_{repeal} to obtain the DDD. Lastly, I estimate the cross-state DD ($DD_{cross\text{-}state}$). The age-period cohort diagram in Table 6 illustrates the quasi-experiment for 25 to 29 years olds between 1994 and 1999.

Homicide rates by age, race and state are shown in Figures (13-16). The vertical lines again demarcate the beginning and end of the exposure period for the relevant age groups. The increase in homicide rates among teens and young adults is striking, but especially for blacks (Figures 14 and 16). There is also a substantial difference in the level of homicide rates between repeal and non-repeal states for these age groups. For older adults, however, homicide rates drift mostly downwards over the entire 18-year span in both repeal and non-repeal states. As before, the analysis of adults should provide the test least confounded by crack.

An important concern is that trends among the comparison groups, especially such aggregated age groups, may not provide an appropriate counterfactual for the exposed group. Thus, I also estimate a series of pseudo-quasi-experiments in which none of the groups are exposed to legalized abortion *in utero*.[10] This serves two purposes. First, it provides some check on the plausibility of the quasi-experimental estimates. Second, some of the age groups in non-repeal states are exposed to legalized abortion following *Roe*.[11] This will tend to bias the DDD estimates towards zero. Thus, I also estimate a DDD based only on the experience of repeal states by subtracting the DD_{repeal} from the pseudo-quasi-experiment from the DD_{repeal} from the quasi-experiment. Again, the purpose is to provide a different means of "detrending" the DD_{repeal} with the hope that results are consistent across comparisons.

The results are presented in Table 7. Estimates for blacks are displayed in the top panel, rows 1-5, and whites in the bottom panel, rows 6-10. As before, each row displays estimates from a separate regression. Within the row, the younger age group is exposed to legalized abortion and the older is not. Thus, the DD_{repeal} for 15 to 19 year olds indicates that homicide rates rose 47 percent more among teens relative to young adults in repeal states between 1984-89 (row 1, column 5). When I subtract from this the DD in non-repeal states (not shown), I obtain a statistically insignificant DDD of –0.087 (row 1, column 4). The same pattern is observed for blacks 20 to 24 years of age (row 2). For older adults both the DD_{repeal} and DDD are large and positive. The large, negative estimates of the $DD_{cross\text{-}state}$ for teens and young adults are clearly

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[10] See Table 6, the change among 25 to 29 year olds between 1988 and 1993 for an illustration of the pseudo-quasi-experiment.

[11] Refer to the age-period-cohort diagram in Table 6. In 1999, for example, 25 and 26 year olds were born in 1973 and 1974 and thus were exposed to legalized abortion following *Roe* in the non-repeal states.

at odds with the other estimates. Importantly, they are also discordant with the $DD_{cross\text{-}state}$ for adults 25-29 years of age (row 3, column 6). This points to omitted variable bias in cross-state comparisons among groups affected by crack-cocaine markets.

Results from the pseudo-quasi-experiments for blacks are very similar to those from the quasi-experiments. The DD_{repeal} is positive and DDD small and statistically insignificant (rows 4 and 5). In other words, the positive DD_{repeal} reflects differences in crime by age that are eliminated by the DDD. Lastly, I estimate the DDD within repeal states only (not shown).[12] I obtain a small and positive estimate for both 20 to 24 year olds (0.470-0.347 = 0.123) and 25 to 29 year olds (0.447-0.338= 0.109), estimates consistent with the DDD based on repeal and non-repeal states. The results for whites offer no evidence of an association between abortion and crime regardless of the estimate (row 6-8). Indeed, I obtain negative estimates only with the pseudo-quasi-experiment, an obviously spurious finding (rows 9 and 10).

Although an analysis by race offers little consistent evidence of an association between abortion and crime, it does provide insight into Donohue and Levitt's findings. The spread of crack appears to have affected the crime rates of black teens and young adults the most. If crack hit New York and California earlier and possibly more intensely than other states, then estimates that correlate changes in crime across states will be susceptible to spurious associations.[13] I re-estimated the $DD_{cross\text{-}state}$ for teens and young adults combining all races to approximate Donohue and Levitt's cross-states analyses. I find that legalization is associated with a 10 percent decline in homicide rates among teens in repeal relative to non-repeal states and an 11 percent decline

[12] As an example consider black teens. The estimate is obtained by subtracting row 4 column 5, from row 1 column 5.

[13] See Joyce (2004) for a review of the literature of the onset and diffusion of crack-cocaine.

among young adults, estimates very similar to what they report (see Donohue and Levitt 2001, Table 1; 2003, Table 3).[14]

VI. Discussion.

Contrary to Donohue and Levitt's (2004) assertion, differences in identification are at the core of our disagreement. I have argued that the most convincing test of a link between abortion and crime should be based on an exogenous change in abortion that had a demonstrable effect on fertility. Thus, I use the early legalization of abortion in 1970 or the increase in abortion rates before and after *Roe* to identify changes in unwanted fertility. Donohue and Levitt (2001, 2004) use state variation in abortion rates between 1961 and 1985 to proxy changes in unintended childbearing. However, they never demonstrate an association between state abortion rates and state fertility rates in the years after *Roe*, a seemingly essential relationship for the plausibility of their hypothesis.[15]

The other threat to identifying a causal association between abortion and crime is the omitted variable problem associated with the crack epidemic. I have used within-state comparison groups of similar age to net out state-specific period effects and then differences across states to net out age effects. I find the results for older adults and white youth compelling because they appear to have been much less affected by the crack epidemic than black teens.

[14] It is also noteworthy that Donohue and Levitt's fixed-effects estimates from regression of log arrests by single year of age on lagged abortion rates are limited to 15 to 24 year olds (see Donohue and Levitt 2001, Table VII; Donohue and Levitt 2004, Table 3). And although they like to emphasize that the models are robust to state-age interactions, identification still comes from the association of cross-state changes in crime and abortion.

[15] Fox (2000b) makes a similar point. The alternative story is that abortion affects the timing of births. If true, then states with greater abortion rates should have lower rates of mistimed births. Again, they never present evidence of such an association. But even if they could, the association between births from mistimed pregnancies and child development is much weaker than the association with births from unwanted pregnancies (Joyce, Kaestner and Korenman 2000).

The analysis of adults also answers a criticism by Donohue and Levitt (2004) that I ignore the effect of abortion on cohorts at different points in the life cycle.

I have also addressed the criticism by Donohue and Levitt (2004) that I use only a dichotomous indicator of legalization and not the change in abortion rates as the relevant treatment. With data on abortion from the CDC, I regress changes in homicide rates among cohorts born before and after *Roe* on changes in the abortion rates between 1971 and 1974. Changes in abortion rates during this period are much larger than the year-to-year changes observed in the late 1970s and early 1980s, are more plausibly exogenous, and are associated with decreases in fertility. Again, I find no association between abortion and crime.

Finally, I have never argued that unanticipated changes in unwanted childbearing would have no effect on family and/or child well-being. The more relevant question is whether such effects are large enough and our research designs credible enough to uncover such impacts 15 to 25 years later. Studies, for instance, that look at changes in outcomes in the years just after legalization may be better able to detect immediate impacts and may be less confounded by hard to measure period effects (Gruber, Levine and Staiger 1999; Klick and Stratmann 2003). Results from studies that have examined more long-term consequences are mixed. Angrist and Evans (1999) find significant effects of abortion legalization on teen fertility in the period right after legalization, but uncover no effect on the subsequent earnings of cohorts exposed. They speculate that longer-term outcomes may be "…too far down the causal chain…:." Charles and Stephens (2002), by contrast, find evidence that cohorts exposed *in utero* are less likely to use illicit drugs in high school. However, the latter study, like Donohue and Levitt's (2001), suffers from an inability to control for within-state, time-varying changes associated with crack, a potentially damaging limitation given their focus on drug use.

Studies of the effects of legalized abortion on well-being are more credible when the change in fertility is more dramatic. A recent analysis of the Romanian ban on abortion in 1966 found that cohorts exposed to the ban, adjusted for compositional changes, committed more crime than cohorts born just before (Pop-Eleches 2002). The author cautions, however, that omitted period effects and not the cohort effect associated with the ban may explain differences in crime among those born in the 1970s. Nevertheless, the Romanian intervention is unique since total fertility rates *doubled* as a result of the ban, an exogenous change in fertility roughly 20 times greater than the change that occurred after early legalization in the United States (Levine et al. 1999). Sen (2003), on the other hand, argues that variation in teen abortion rates explains 50 percent of the decline in total violent crime in Canada in the 1990s. However, the teen abortion rate can explain only 1 percent of the 33 percent decline in teen fertility rates between 1974 and 1988.[16] Moreover, teen fertility rates and pregnancy rates *rise* in the year after abortion is fully legalized, a result more consistent with moral hazard than fetal selection (http://www.statcan.ca/english/kits/preg/preg3g.htm).

In summary, the legalization or de-legalization of abortion may generate a credible source of variation in unintended childbearing. However, identifying effects of such changes on longer-term outcomes such as crime depends on the magnitude of the change as well as the ability to net out hard to measure period effects. Regressions of current crime on lagged abortion, as favored by Donohue and Levitt, are not robust to alternative identification strategies, which at the very least calls into question a causal interpretation of their findings.

[16] There are other limitations with Sen's study. He finds no association between teen abortion rates and homicide rates, robbery rates or rates of property crime. The association with violent crime is limited to sexual and physical assault. Finally, he lacks age-specific crime rates and thus his tests are weaker than those of Donohue and Levitt (2001, 2004).

Acknowledgements

I thank Andrew Racine, Sanders Korenman, and Robert Kaestner for helpful comments. Silvie Colman provided terrific research assistance. I am responsible for all errors. The quote above the Introduction is from Heckman's comment on the article by Angrist, Imbens and Rubin (1996) in the Journal of American Statistical Association 91(434), 1996, p. 459.

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Figure 1. Drug-related Homicide Rates by Age in Non-Repeal States*

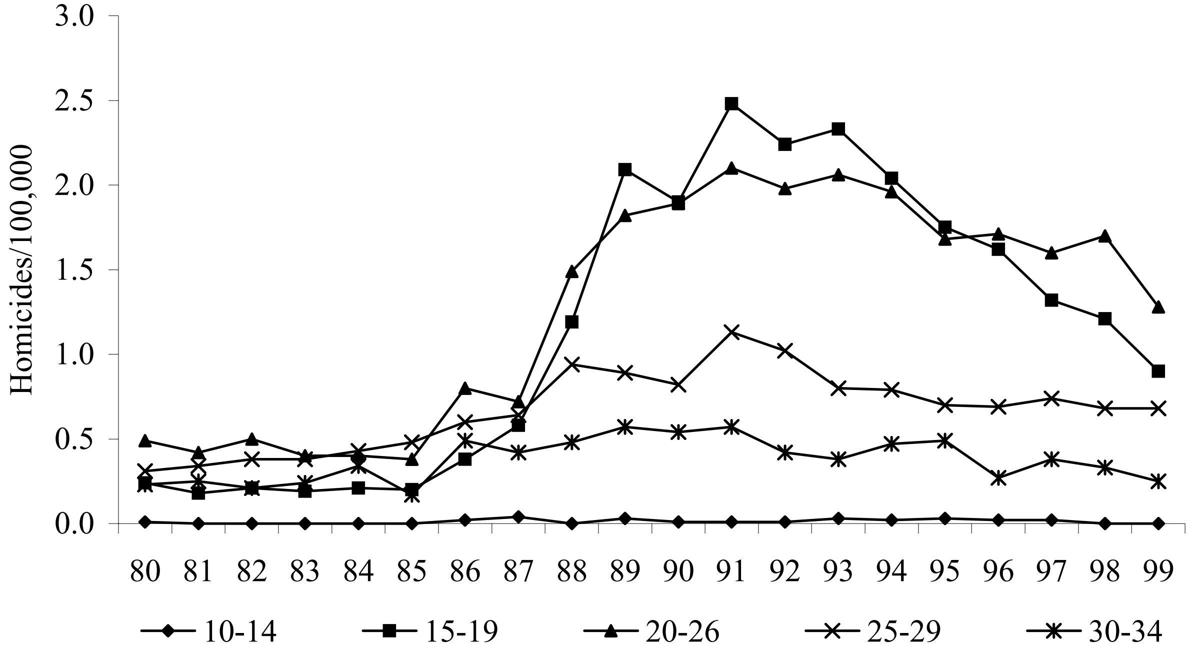
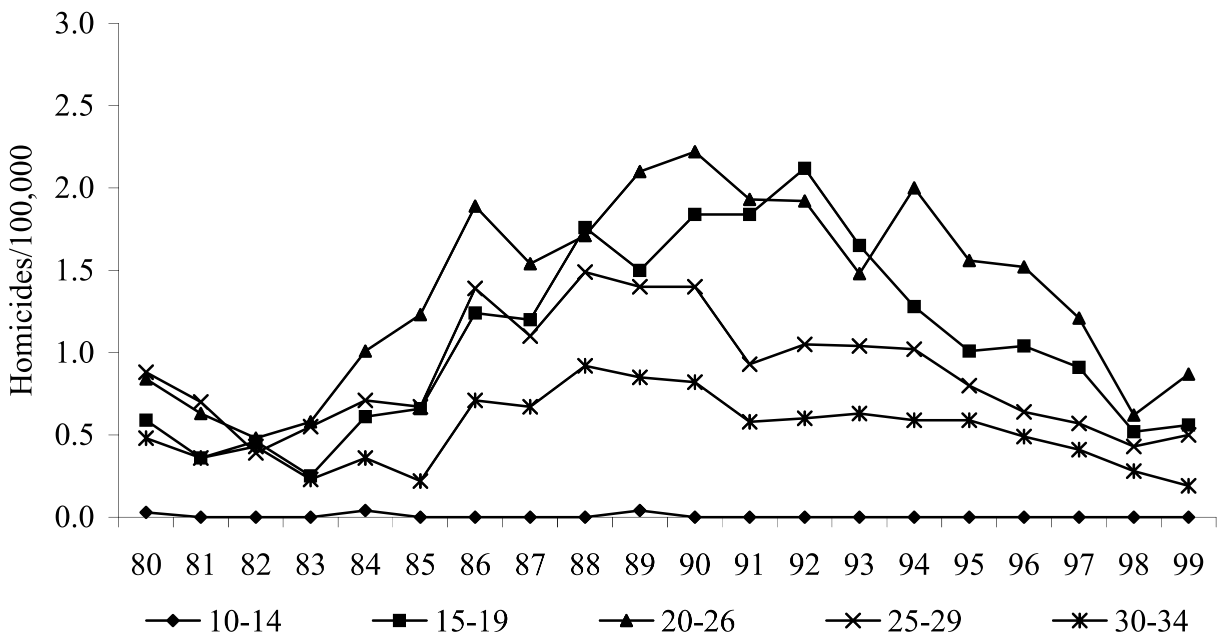


Figure 2. Drug-related Homicide Rates by Age in Repeal States*



*Repeal states include AK,CA,DC,HI,NY & WA; Non-repeal states include all others.

Figure 3. Gun-related Homicide Rates by Age in Non-Repeal States*

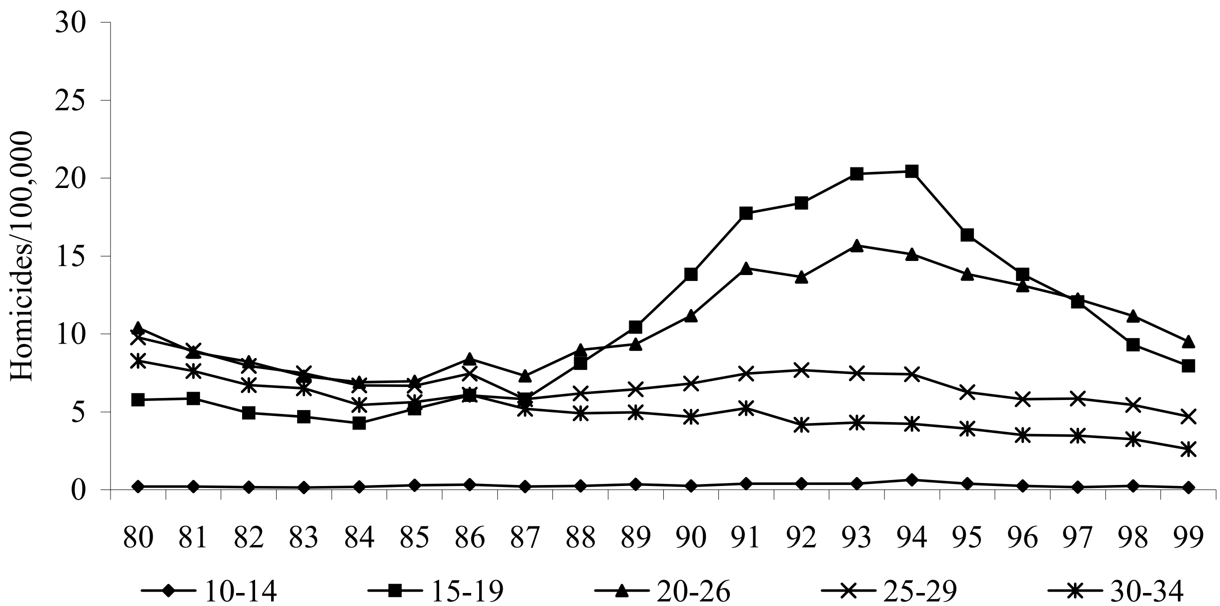
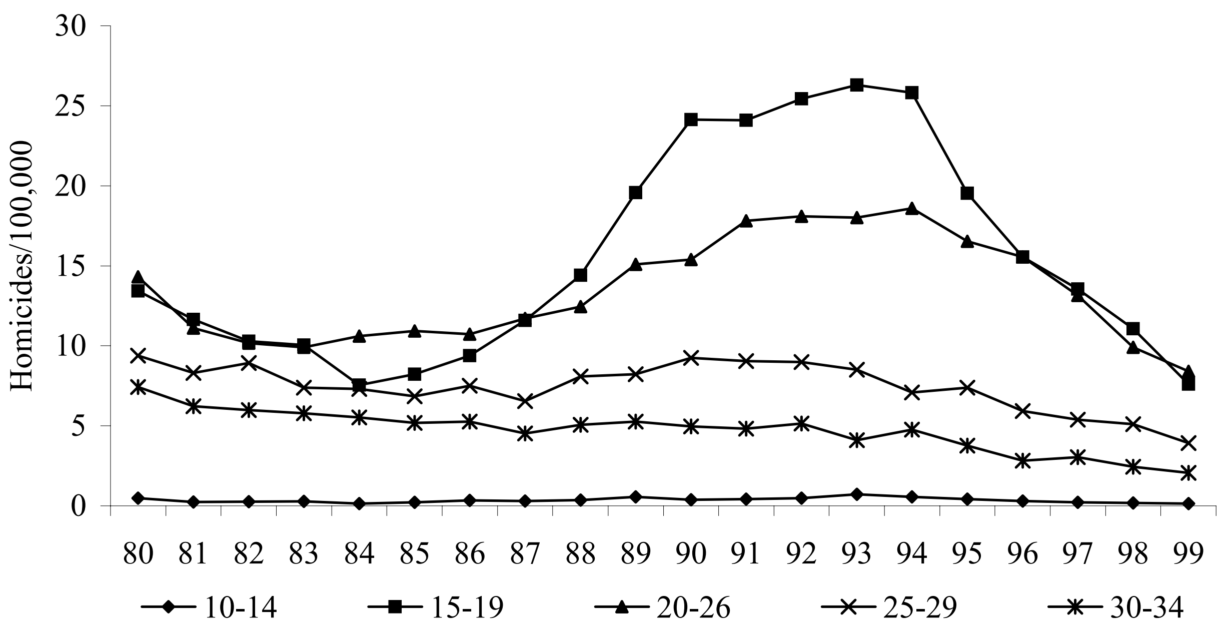


Figure 4. Gun-related Homicide Rates by Age in Repeal States*



*Repeal states include AK,CA,DC,HI,NY & WA; Non-repeal states include all others.

Figure 5. Number of Homicides to 18 to 22 and 23 to 27 Year Olds in Repeal and Non-Repeal States*

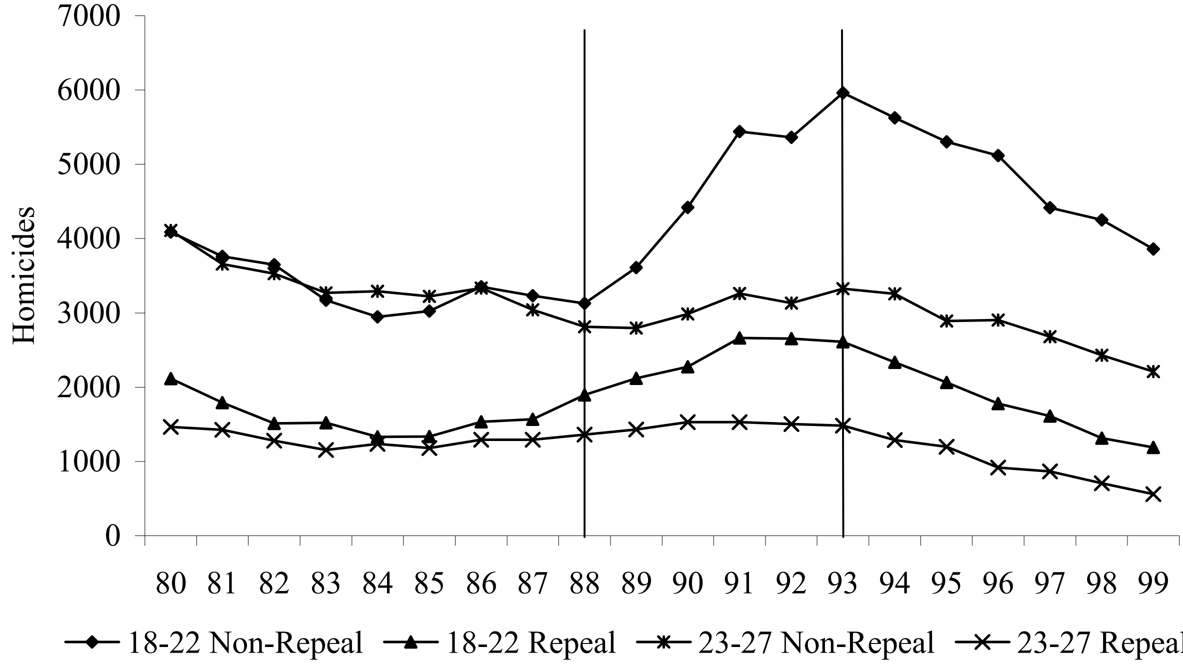
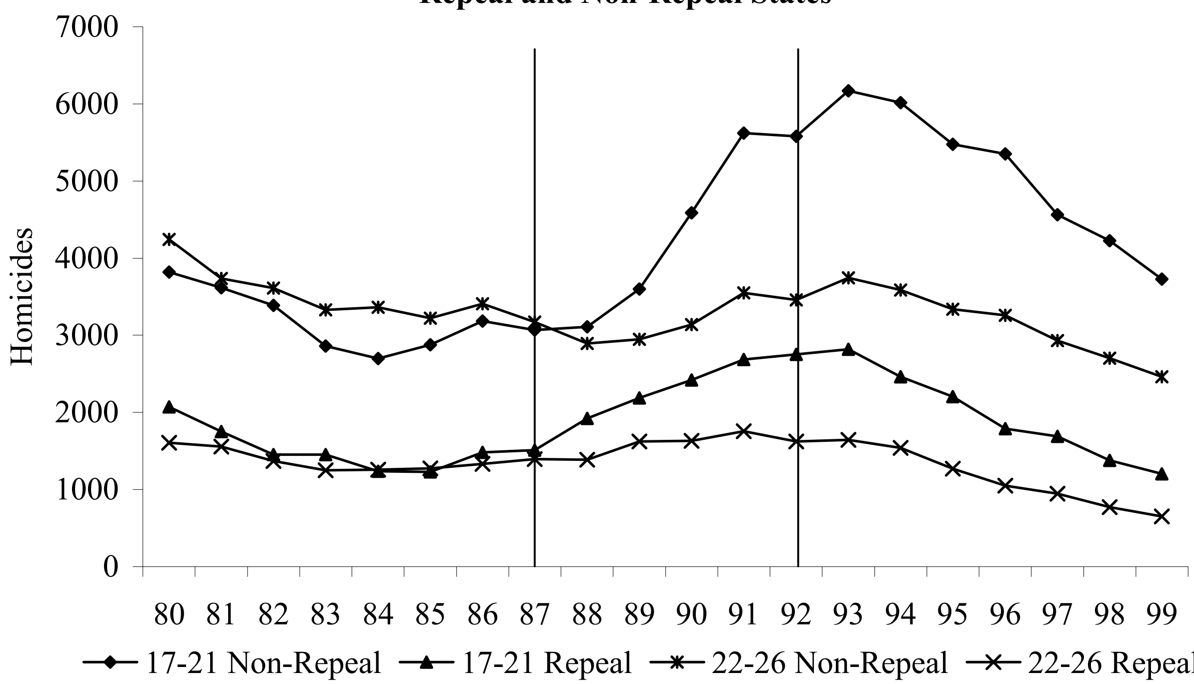


Figure 6. Number of Homicides to 17 to 21 and 23 to 27 Year Olds in Repeal and Non-Repeal States*



* Vertical lines show the first and last year of the quasi-experiment. Repeal states include AK,CA,DC,HI,NY, & WA. Non-repeal states include all others

Figure 7. Homicide Rates by Single-Year of age for Youth Ages 17 to 20 in Repeal States*

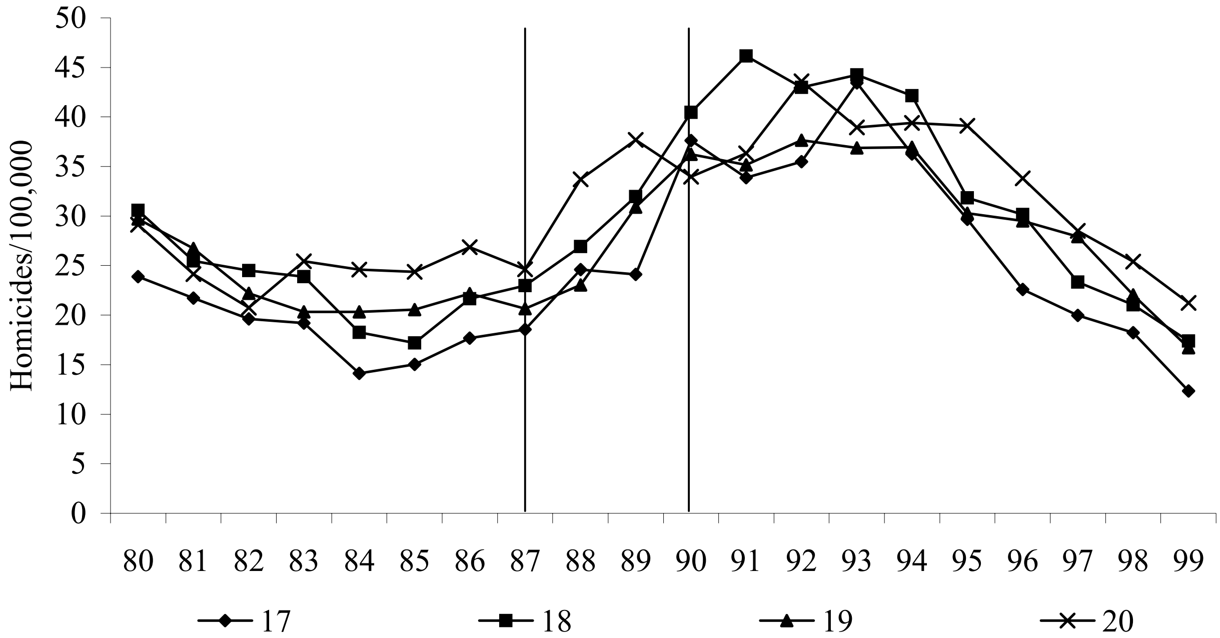
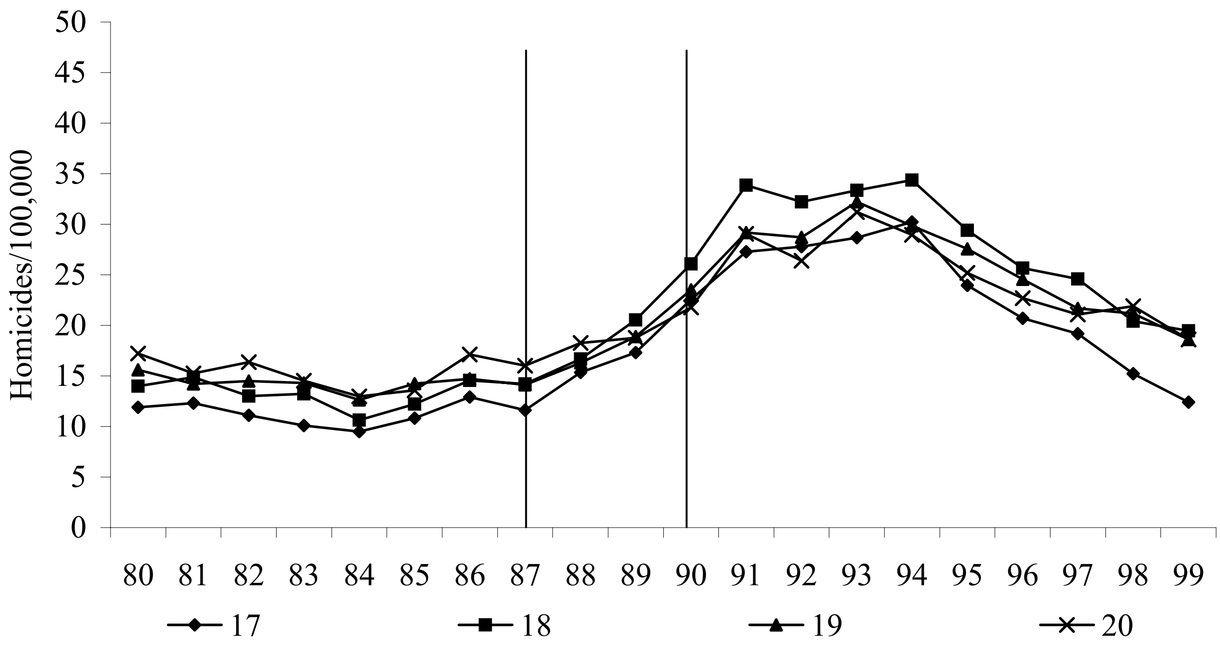


Figure 8. Homicide Rates by Single-Year of age for Youth Ages 17 to 20 in Non-Repeal States*



* Vertical lines show the first and last year of the quasi-experiment. Repeal states include AK,CA,DC,HI,NY, & WA. Non-repeal states include all others

Figure 9. Homicide Rates by Single-Year of age for Adults Ages 27 to 30 in Repeal States*

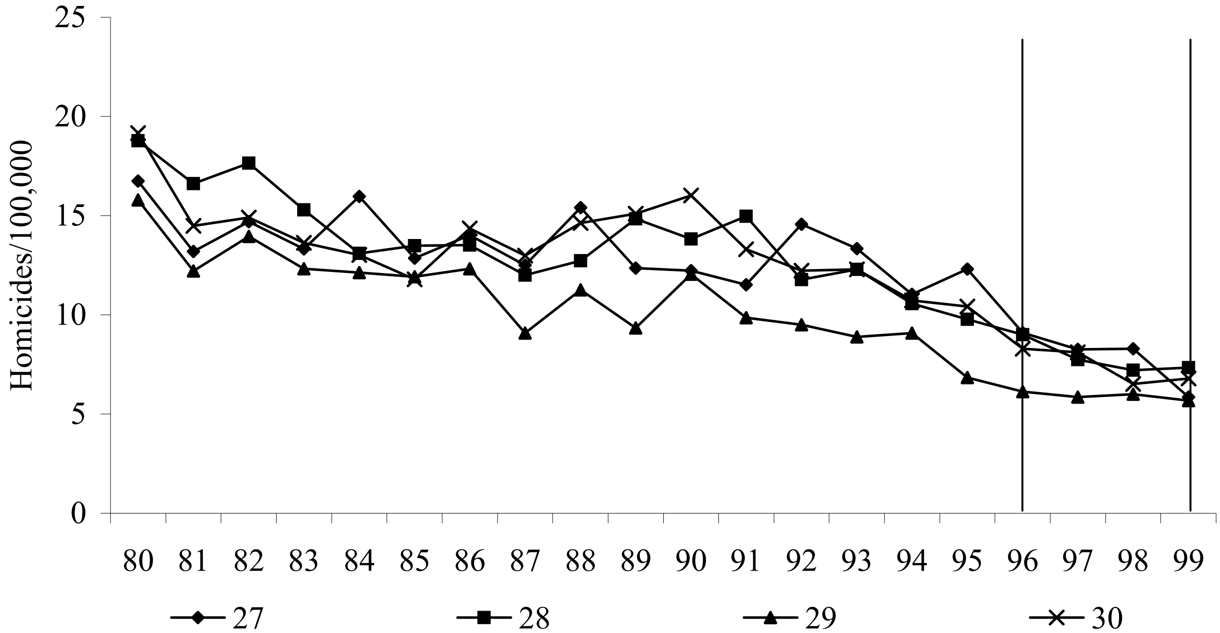
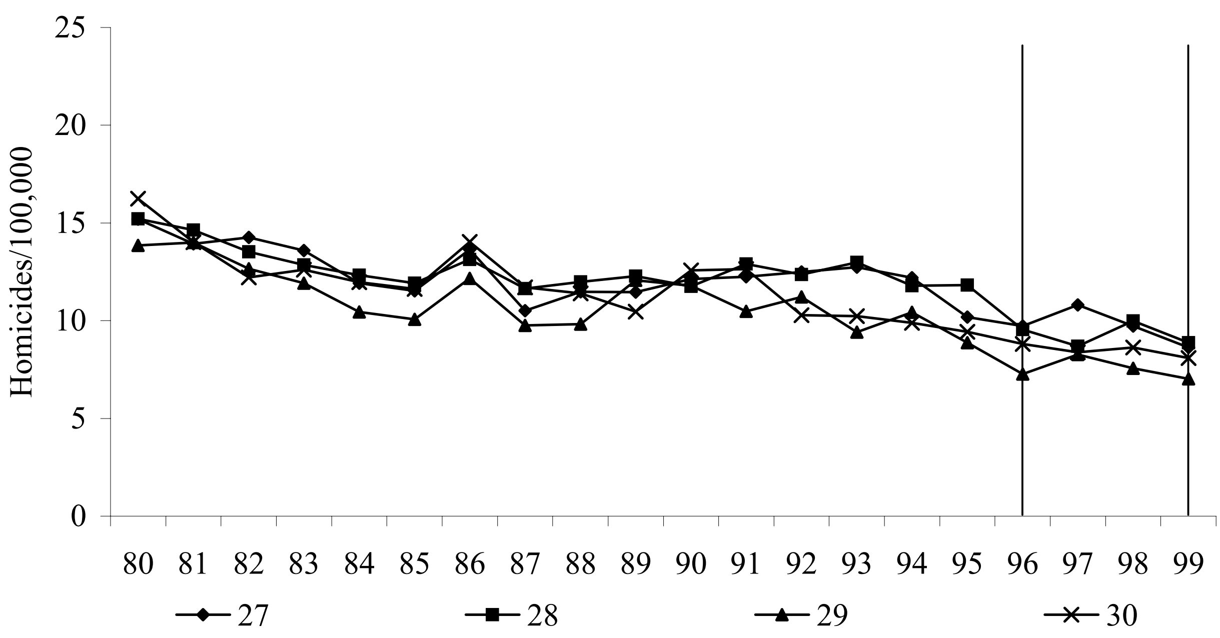


Figure 10. Homicide Rates by Single-Year of age for Adults Ages 27 to 30 in Non-Repeal States*



* Vertical lines show the first and last year of the quasi-experiment. Repeal states include AK,CA,DC,HI,NY, & WA. Non-repeal states include all others

Figure 11. Homicide Rates by Single-Year of age for Adults Ages 20 to 23 in Repeal States*

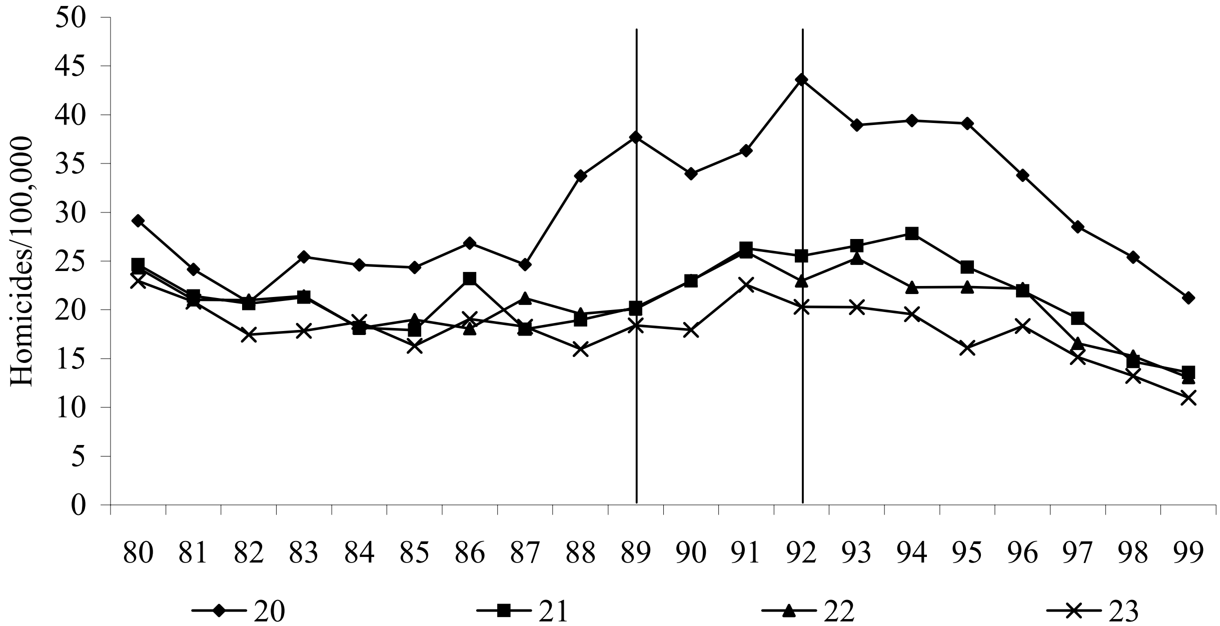
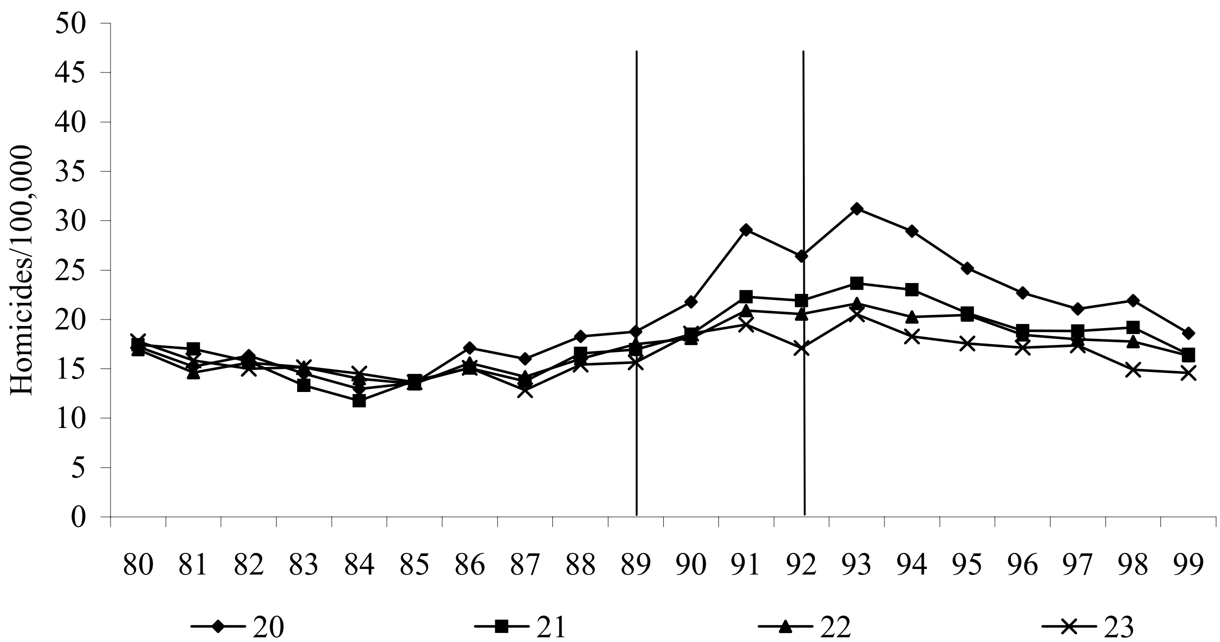


Figure 12. Homicide Rates by Single-Year of age for Adults Ages 20 to 23 in Non-Repeal States*



* Vertical lines show the first and last year of the quasi-experiment. Repeal states include AK,CA,DC,HI,NY, & WA. Non-repeal states include all others

Figure 13. Black Homicide Rates by Age for Adults 25 to 34 in Repeal and Non-Repeal States*

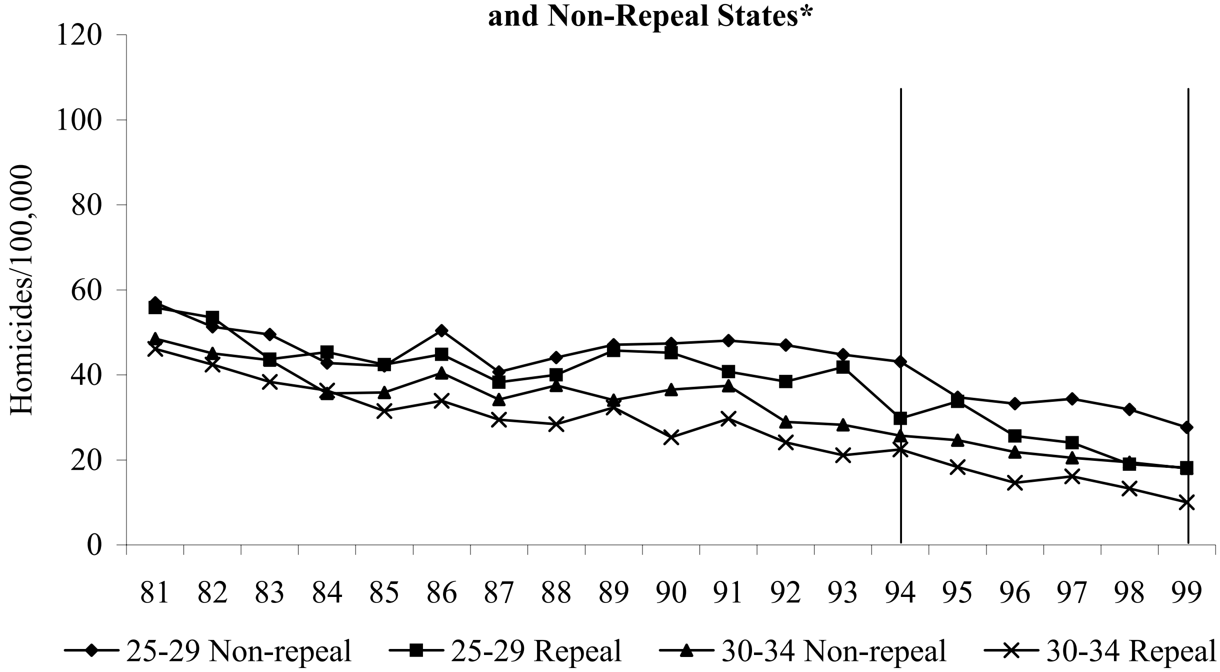
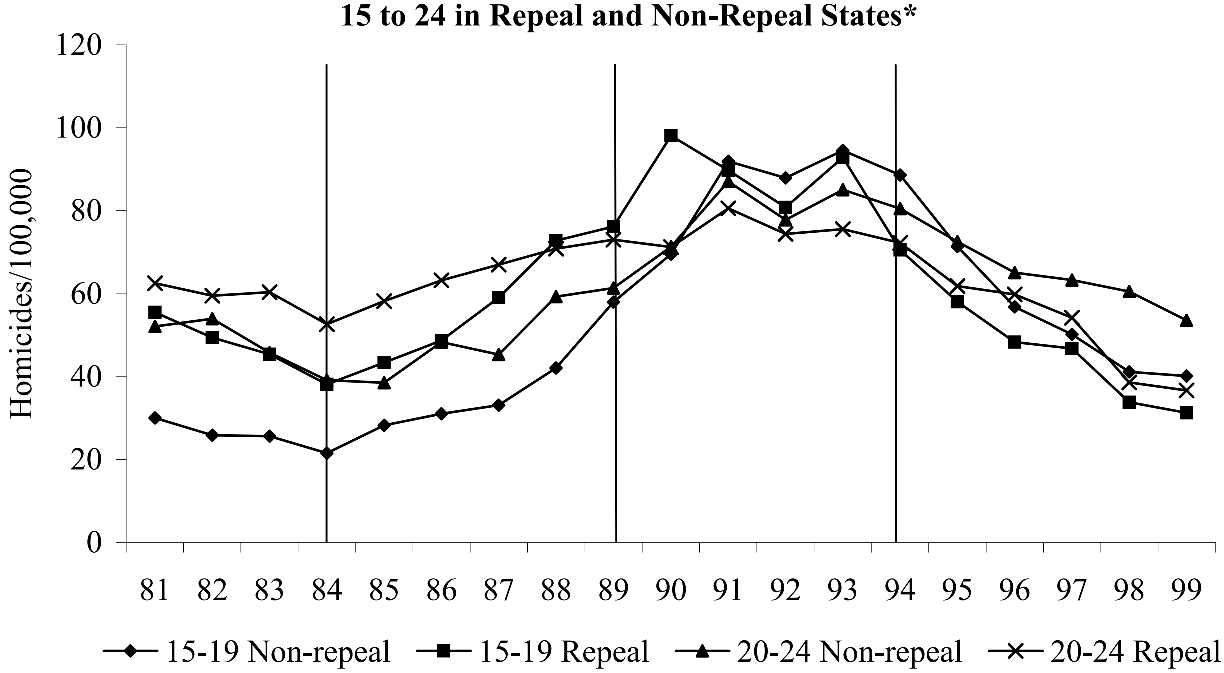


Figure 14. Black Homicide Rates by Age for Teens and Young Adults 15 to 24 in Repeal and Non-Repeal States*



* Vertical lines show the first and last year of the quasi-experiment for various age groups.

Figure 15. White Homicide Rates by Age for Adults 25 to 34 in Repeal and Non-Repeal States*

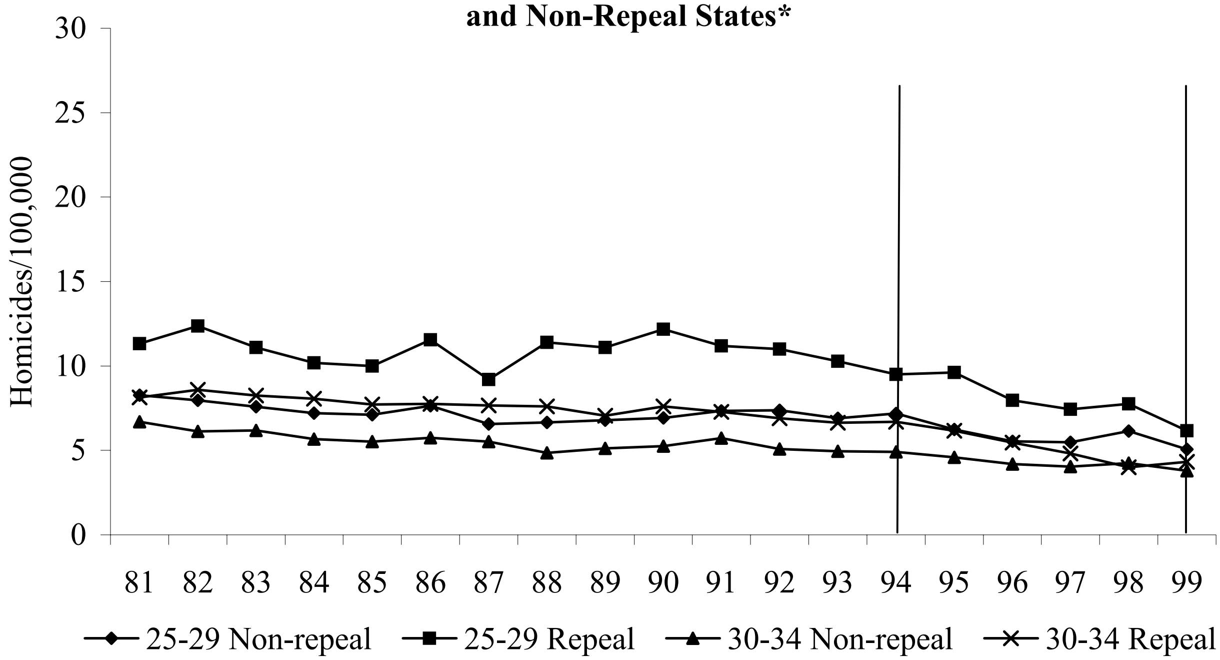
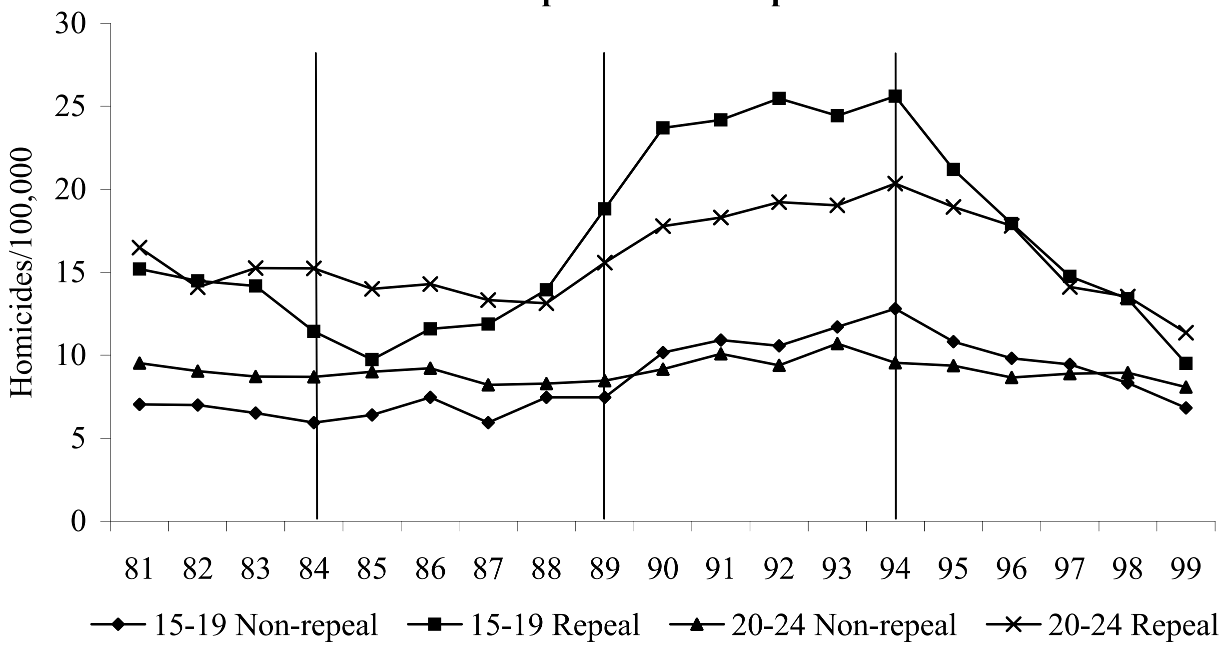


Figure 16. White Homicide Rates by Age for Teens and Young Adults 15 to 24 in Repeal and Non-Repeal States*



* Vertical lines show the first and last year of the quasi-experiment for various age groups.

Table 1: Age by Year of Birth and Calendar Year

	Calendar Year																	
Year of Birth	**82**	**83**	**84**	**85**	**86**	**87**	**88**	**89**	**90**	**91**	**92**	**93**	**94**	**95**	**96**	**97**	**98**	**99**
1965	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34
1966	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33
1967	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32
1968	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31
1969	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
1970		13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29
1971			13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28
1972				13	14	15	16	17	18	19	20	21	22	23	24	25	26	27
1973					13	14	15	16	17	18	19	20	21	22	23	24	25	26
1974						13	14	15	16	17	18	19	20	21	22	23	24	25

Abortion became legal in the repeal states in roughly1970. Age groups born in 1967 are always the comparison group whereas those in 1971 are always the exposed group.

Table 2. Difference-in-Difference-Differences Estimates of the Effect of Abortion Legalization Pre-*Roe* on Log Homicides and Log Homicide Rates

Years	Age Groups: Exposed	Comparison	Ln Homicides		Ln Homicide Rate	
(1)	*(2)*	*(3)*	*(4)*		*(5)*	
80-85	**10-14**	15-19	-0.296	(0.150)	0.098	(0.166)
81-86	**11-15**	16-20	-0.171	(0.106)	-0.015	(0.081)
82-87	**12-16**	17-21	-0.281	(0.115)	-0.124	(0.066)
83-88	**13-17**	18-22	-0.031	(0.097)	0.053	(0.099)
84-89	**14-18**	19-23	0.173	(0.135)	0.227	(0.097)
85-90	**15-19**	20-24	0.159	(0.098)	0.190	(0.087)
86-91	**16-20**	21-25	-0.090	(0.080)	-0.035	(0.063)
87-92	**17-21**	22-26	-0.185	(0.093)	-0.098	(0.079)
88-93	**18-22**	23-27	-0.165	(0.075)	-0.074	(0.068)
89-94	**19-23**	24-28	-0.020	(0.061)	0.064	(0.050)
90-95	**20-24**	25-29	0.029	(0.059)	0.099	(0.060)
91-96	**21-25**	26-30	-0.178	(0.115)	-0.087	(0.121)
92-97	**22-26**	27-31	-0.105	(0.178)	-0.035	(0.175)
93-98	**23-27**	28-32	-0.109	(0.106)	-0.051	(0.108)
94-99	**24-28**	29-33	-0.069	(0.072)	-0.046	(0.063)
	Average:		**-0.089**	**(0.029)**	**0.011**	**(0.026)**

Estimates in Column (4) are from Donohue and Levitt (2004, Table 2). Each row represents a separate regression based on the indicated years. All models include state and year fixed effects and standard errors are adjusted for clustering within states. The exposed groups are cohorts in the repeal states (AK, CA, DC, HA, NY and WA) who were *in utero* before and after abortion became legal.

Table 3. Difference-in-Difference-Differences (DDD) Estimates of the Effect of Abortion Legalization Pre-*Roe* on Log Homicide Rates

Years	Age Groups: Exposed	Control	DDD		DD_{repeal}		$DD_{cross\text{-}state}$	
(1)	*(2)*	*(3)*	*(4)*		*(5)*		*(6)*	
82-85	**13-14**	15-16	-0.104	(0.285)	-0.248	(0.158)	-0.419	(0.197)*
83-86	**14-15**	16-17	0.259	(0.258)	0.275	(0.207)	-0.059	(0.223)
84-87	**15-16**	17-18	0.052	(0.136)	0.071	(0.050)	0.075	(0.188)
85-88	**16-17**	18-19	0.336	(0.127)*	0.243	(0.070)**	0.300	(0.121)*
86-89	**17-18**	19-20	0.071	(0.128)	0.144	(0.081)	0.048	(0.152)
87-90	**18-19**	20-21	-0.063	(0.090)	0.109	(0.059)	0.026	(0.104)
88-91	**19-20**	21-22	-0.220	(0.076)**	-0.019	(0.035)	-0.164	(0.088)
89-92	**20-21**	22-23	-0.422	(0.164)*	-0.194	(0.145)	-0.310	(0.142)*
90-93	**21-22**	23-24	-0.230	(0.132)	-0.088	(0.103)	-0.108	(0.129)
91-94	**22-23**	24-25	0.032	(0.227)	0.100	(0.208)	-0.097	(0.080)
92-95	**23-24**	25-26	0.032	(0.294)	0.208	(0.284)	-0.117	(0.098)
93-96	**24-25**	26-27	-0.066	(0.162)	0.058	(0.124)	-0.027	(0.086)
94-97	**25-26**	27-28	-0.111	(0.119)	-0.007	(0.079)	-0.024	(0.167)
95-98	**26-27**	28-29	-0.110	(0.208)	0.012	(0.185)	-0.088	(0.149)
96-99	**27-28**	29-30	0.062	(0.285)	0.094	(0.263)	-0.087	(0.095)
	Average:		**-0.032**	**(0.052)**	**0.051**	**(0.042)**	**-0.070**	**(0.038)**

Each row represents a separate regression based on the indicated years. All models include state and year fixed effects and standard errors are adjusted for clustering within states. The exposed groups are cohorts in repeal states (AK, CA, DC, HA, NY and WA) who were *in utero* before and after abortion became legal. The comparison groups were never exposed *in utero*. The DD_{repeal} is obtained from comparisons within the repeal states only. The $DD_{cross\text{-}state}$ compares the exposed group in repeal states to the same age group in non-repeal states. The DDD adjusts the DD_{repeal} for changes in homicide rates by age [see equation (2) in the text]. *p<.05; ** p<.01

Table 4. Abortion Rates by State of Residence in 1971 and 1973 Ranked by the Change in Abortion Rates between the Two Years.*

State (ranked by difference in abortion rate 73-71: lowest to highest)	Abortion rate 71	Abortion rate 73	Difference 73-71
New Mexico	21.76	13.26	-8.50
West Virginia	2.47	2.25	-0.22
Louisiana	1.44	1.67	0.22
North Dakota	2.05	3.09	1.04
Iowa	5.07	6.39	1.32
Indiana	4.52	5.90	1.38
Mississippi	0.74	2.33	1.58
Montana	2.94	4.72	1.78
Kansas	8.85	10.76	1.91
Wisconsin	5.86	8.63	2.77
Arkansas	2.72	5.54	2.81
Kentucky	3.32	6.47	3.15
Average for 1st quartile:	5.15	5.92	0.77
Oklahoma	2.83	6.08	3.25
Delaware	13.54	17.29	3.75
Utah	0.21	4.18	3.97
New Hampshire	7.95	11.96	4.01
South Carolina	3.49	7.59	4.10
Alabama	2.04	6.31	4.26
Idaho	0.19	4.62	4.43
Nebraska	3.63	8.36	4.73
Ohio	6.29	11.23	4.94
Maine	6.65	11.90	5.25
Tennessee	3.24	8.59	5.34
Average for 2nd quartile:	4.55	8.92	4.37
Georgia	4.82	10.23	5.42
Minnesota	4.28	10.15	5.88
Wyoming	2.62	8.57	5.95
Connecticut	12.35	18.31	5.96

Pennsylvania	8.56	14.61	6.05
North Carolina	5.40	11.58	6.19
Missouri	4.78	11.04	6.26
Rhode Island	8.49	15.25	6.77
Colorado	9.07	15.98	6.91
South Dakota	1.30	8.23	6.93
Virginia	6.69	13.91	7.22
Average for 3rd quartile:	6.21	12.53	6.32
Oregon	15.84	23.97	8.13
Vermont	8.14	16.38	8.24
Illinois	6.90	15.13	8.24
Maryland	11.35	20.26	8.91
Massachusetts	11.16	20.67	9.51
Florida	6.68	16.63	9.95
Texas	1.04	11.12	10.08
New Jersey	14.11	24.21	10.09
Michigan	7.58	18.16	10.58
Arizona	1.05	11.77	10.72
Nevada	0.36	27.10	26.74
Average for 4th quartile:	7.65	18.67	11.02
Average for the 45 states:	5.87	11.39	5.51

*Abortion rates are the ratio of abortions by state of residence to women 15 to 44 years of age in thousands. Data on resident abortions in 1971 are from Table 5 of the Centers for Disease Control (1972) and resident abortion rates in 1973 are from Forrest, Sullivan and Tietze (1979).

Table 5. Estimates of the Effect of a Change in State Abortion Rates on Changes in Log Homicide Rates for Cohort Exposed and Unexposed to Legalized Abortion Following *Roe.*

Crime Years	Abortion Years	Exposed group	Comparison group			
(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*
85-89		**13-14**	**15-16**	**17-18**	**19-20**	**21-22**
(N=354)	71-74:	0.010 0.020	0.020 0.017	0.027 0.013*	0.030 0.022	0.003 0.013
87-91		**15-16**	**17-18**	**19-20**	**21-22**	**23-24**
(N=374)	71-74:	-0.006 0.020	-0.013 0.015	-0.013 0.009	0.003 0.016	-0.016 0.011
89-93		**17-18**	**19-20**	**21-22**	**23-24**	**25-26**
(N=384)	71-74:	0.010 0.008	-0.002 0.011	-0.014 0.013	-0.004 0.013	0.028 0.011*
91-95		**19-20**	**21-22**	**23-24**	**25-26**	**27-28**
(N=375)	71-74:	-0.014 0.010	0.020 0.012	0.021 0.010*	-0.004 0.011	0.005 0.012
93-97		**21-22**	**23-24**	**25-26**	**27-28**	**29-30**
(N=354)	71-74:	-0.007 0.011	0.006 0.013	-0.004 0.015	-0.017 0.010*	-0.033 0.010**
95-99		**23-24**	**25-26**	**27-28**	**29-30**	**31-32**
(N=348)	71-74:	-0.040 0.012**	-0.013 0.012	0.005 0.009	0.006 0.016	0.011 0.015
Average:	**71-74:**	**-0.008 0.006**	**0.003 0.006**	**0.004 0.005**	**0.002 0.007**	0.000 0.005

Each row represents a separate regression based on the indicated years. There are 45 states, 2 difference periods, and 5 age groups for a possible sample of 450 observations. The exposed groups are cohorts who were *in utero* before and after *Roe* Changes in the resident abortion rates pertain to 1973-1971 and 1974-1972. Change in homicide rates among the exposed groups pertain to cohorts born between 1974-1972 and 1975-1973. Standard errors are adjusted for clustering within-state. *p<.10; **p<.05*

Table 6: Age by Year of Birth and Calendar Years

	Calendar Years												
Year of Birth	**87**	**88**	**89**	**90**	**91**	**92**	**93**	**94**	**95**	**96**	**97**	**98**	**99**
1954	33	34	35	36	37	38	39	40					
1955	32	33	*Pseudo-quasi-experiment*					39	40				
1956	31	32	33	34	35	36	37	38	39	40			
1957	30	31	32	33	34	35	36	37	38	39	40		
1958	29	30	31	32	33	34	35	36	37	38	39	40	
1959	28	29	30	31	32	33	34	35	36	37	38	39	40
1960	27	28	29	30	31	32	33	34		*Quasi-experiment*			
1961	26	27	28	29	30	31	32	33	34	35	36	37	38
1962	25	26	27	28	29	30	31	32	33	34	35	36	37
1963	24	25	26	27	28	29	30	31	32	33	34	35	36
1964	23	24	25	26	27	28	29	30	31	32	33	34	35
1965	22	23	24	25	26	27	28	29	30	31	32	33	34
1966	21	22	23	24	25	26	27	28	29	30	31	32	33
1967	20	21	22	23	24	25	26	27	28	29	30	31	32
1968		20	21	22	23	24	25	26	27	28	29	30	31
1969			20	21	22	23	24	25	26	27	28	29	30
1970				20	21	22	23	24	25	26	27	28	29
1971					20	21	22	23	24	25	26	27	28
1972						20	21	22	23	24	25	26	27
1973							20	21	22	23	24	25	26
1974								20	21	22	23	24	25

Table 7. Difference-in-Difference-Differences Estimates of the Effect of Abortion Legalization Pre-*Roe* on Log Homicide Rates of Adults 25 to 29 Years of Age by Race

	Years	Age Groups: Exposed	Comparison	DDD	DD_{repeal}	$DD_{cross\text{-}state}$	N	R^2
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*		
Blacks:								
1.	84-89	**15-19**	20-24	-0.087 (0.152)	0.466 (0.124)*	-0.347 (0.278)	148	0.78
2.	89-94	**20-24**	25-29	0.048 (0.177)	0.470 (0.160)*	-0.430 (0.167)*	153	0.83
3.	94-99	**25-29**	30-34	0.541 (0.306)	0.447 (0.278)	0.146 (0.383)	141	0.80
				Pseudo-quasi-experiments:				
4.	83-88	**20-24**	25-29	-0.036 (0.134)	0.347 (0.109)*	-0.094 (0.116)	160	0.77
5.	88-93	**25-29**	30-34	0.038 (0.104)	0.338 (0.026)*	-0.083 (0.132)	146	0.75
Whites:								
6.	84-89	**15-19**	20-24	0.175 (0.263)	0.418 (0.222)	0.093 (0.343)	193	0.75
7.	89-94	**20-24**	25-29	0.329 (0.129)*	0.412 (0.041)*	0.108 (0.204)	186	0.82
8.	94-99	**25-29**	30-34	0.175 (0.123)	0.023 (0.076)	0.035 (0.162)	181	0.80
				Pseudo-quasi-experiments:				
9.	83-88	**20-24**	25-29	-0.202 (0.168)	-0.153 (0.134)	-0.145 (0.111)	192	0.84
10.	88-93	**25-29**	30-34	0.033 (0.107)	0.006 (0.059)	-0.099 (0.111)	186	0.88

Each row represents a separate regression based on the indicated years. All models include state and year fixed effects and standard errors are adjusted for clustering within states. The younger age group in each row was exposed to legalized abortion *in utero,* in the quasi-experimental analyses. In the pseudo-quasi-experiments, none of the age groups were exposed. *p<.05.