Did Legalized Abortion Lower Crime?

Ted Joyce

ABSTRACT

In this paper I compare changes in homicide and arrest rates among cohorts born before and after the legalization of abortion to changes in crime in the same years among similar cohorts who were unexposed to legalized abortion. I find little consistent evidence that the legalization of abortion in selected states around 1970, and then in the remaining states following Roe v. Wade, had an effect on recent crime rates. I conclude that the dramatic association as reported in a recent study is most likely the result of unmeasured period effects such as changes in crack cocaine use.

I. Introduction

In a recent and controversial article, Donohue and Levitt (2001) present evidence that the legalization of abortion in 1973 explains over half of the recent decline in crime across the United States. A 50 percent increase in the mean abortion ratio is associated with an 11 percent decrease in violent crime, an 8 percent decrease in property crime and a 12 percent decrease in murder. These effects are generally larger and more precisely estimated than the effects of incarceration and police manpower. Moreover, they conclude that the full impact on crime of *Roe v. Wade* will not be felt for another 20 years. To quote, "Our results suggest that all else equal,

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legalized abortion will account for persistent declines of 1 percent a year in crime over the next two decades" (p. 415). Given the social costs associated with crime and the controversy surrounding abortion, a causal link between abortion and crime has profound implications for social policy.

The purpose of this paper is to analyze the association between legal abortion and crime. The primary difference between my analysis of abortion and crime and that of Donohue and Levitt is the identification strategy. Donohue and Levitt regress crime rates between 1985 and 1996 on abortion ratios lagged 15 to 25 years adjusted for state and year fixed effects. However, the study period coincides with the rise and decline of the crack cocaine epidemic, which many observers link to the spread of guns and the unprecedented increase in youth violence (Cook and Laub 1998; Blumstein 1995; Blumstein, Rivara, and Rosenfeld 2000). Moreover, data from police surveys, emergency rooms, and from urine samples of arrestees in major metropolitan areas suggests that the timing of the arrival, diffusion, and decline in crack use varied significantly by city (Golub and Johnson 1997; Cork 1999; Grogger and Willis 2000). Thus, even in models with state and year fixed effects, the relationship between abortion and crime may be biased by differences in within-state growth in cocaine markets over time, a classic problem of omitted variables. A crude solution is to include controls for state-specific linear or quadratic trends. However, this is not possible in the context of Donohue and Levitt's model, because the trend terms remove all variation in the abortion ratio.

I take a different approach to the identification of an abortion-crime nexus. I use the early legalization of abortion in selected states prior to *Roe v. Wade* and then national legalization after *Roe* in the remaining states to identify exogenous shifts in unintended childbearing. Specifically, I estimate a reduced-form equation in which changes in arrest and homicide rates among cohorts before and after exposure to legalized abortion are compared to changes among cohorts that are unexposed.[1] This is similar to Donohue and Levitt's fixed effect specification, since identification comes from changes in crime and abortion across states. However, I show that these estimates are sensitive to the years that are analyzed, which I interpret as an omitted variable problem related to unobserved, state-specific period effects. I then use a difference-in-difference estimator based on a *within-state* comparison group to net out changes in crime associated with hard-to-measure factors that vary by state and year, such as the spread of crack cocaine and its spillover effects. In these analyses I find no effect of abortion legalization on crime regardless of the years analyzed.

The difference-in-difference strategy has two other advantages in an analysis of abortion and crime. First, Donohue and Levitt use the ratio of abortions to births as an inverse proxy for unwanted births. However, abortion is endogenous to sexual activity, contraception and childbearing. A rise in abortion may have relatively little effect on unwanted childbearing. It is noteworthy, that the abortion rate rose from 16.3 abortions per 1,000 women ages 15 to 44 in 1973 to 29.3 in 1980, an increase of 79 percent. Over the same period, however, the number of births per 1,000 women

1. See Levine et al. (1999), Gruber, Levine, and Staiger (1999), Angrist and Evans (1999) for a similar approach applied to fertility, child well-being, and teen pregnancy, respectively. A recent manuscript by Lott and Whitley (2001) also focuses on a comparison of cohorts exposed and unexposed to legalized abortion. They report a *positive* but relatively small association between legalized abortion and murder rates.

ages 15 to 44 was essentially unchanged, from 69.2 to 68.4. By contrast, there is substantial evidence that the early legalization of abortion in selected states induced a significant decline in fertility between 1971 and 1973 (Sklar and Berkov 1974; Joyce and Mocan 1990; Levine et al. 1999; Angrist and Evans 1999). This change in fertility is a more plausible source of exogenous variation with which to identify a decline in unwanted births than within-state changes in reported legal abortions between 1973 and 1985.

The other advantage of the difference-in-difference approach is that it obviates the need to measure illegal or unreported abortion in the years before legalization. Donohue and Levitt use no data on abortion prior to 1973. Their analysis of arrests by single year of age, for instance, pertains to birth cohorts born between 1961 and 1981 where approximately 60 percent of the state/age/cohort cells are assigned an abortion ratio of zero. However, demographers have concluded that most legal abortions in the early 1970s replaced illegal abortions (Tietze 1973; Sklar and Berkov 1974). If the underreporting of abortion were random among states, their estimates would be biased downward. As I show below, however, the measurement error is negatively correlated with the true abortion rate in 1972 and thus the direction of the bias is unknown.

II. Conceptual and Empirical Issues

A. Abortion and Unintended Childbearing

As outlined by Donohue and Levitt, there are several ways in which legal abortion can affect crime. Cohort size is one. Fewer births mean fewer criminals in subsequent years. Second, legal abortion may also affect crime rates through a relative decrease in fertility rates among poor, young, and minority women. Since children from disadvantaged backgrounds are more likely to commit crimes as teens or adults, the result of a selective reduction in childbearing is a drop in crime rates approximately 15 to 25 years later. Third, even if the decline in fertility rates caused by legalized abortion were distributed equally among all women, a fall in unintended childbearing could bring about a fall in crime if those born from unintended pregnancies were more likely to commit crime than individuals from pregnancies that were intended.

Donohue and Levitt's identification strategy is to correlate crime rates and arrests to lagged abortion ratios adjusted for state and year fixed effects. Abortion ratios serve as an inverse proxy for unwanted childbearing. In their analysis of arrests of youths 15 to 24 years of age, they regress arrests by single year of age on the abortion ratio in the year before a cohort was born. Thus, arrests of 18-year-olds in 1988 in state j are correlated with the abortion ratio in state j in 1969 (t-18-1).

B. Period and Cohort Effects

The biggest challenge to identifying a cohort effect associated with legalized abortion is the potential confounding from strong period effects such as the spread of crack cocaine. There was an unprecedented rise in youth homicide between 1985 and 1993. The rise among blacks greatly exceeded that of whites and almost all the growth in homicide involved handguns (Blumstein 1995; 2000; Cook and Laub 1998). Criminologists have largely attributed the growth in youth homicide to the violent development of crack cocaine markets in poor urban centers (Blumstein, Rivara, and Rosenfeld 2000). The lack of consistent data on the extent of cocaine use or the spread of illegal handguns, however, has limited empirical work.

Despite the lack of data, several sources suggest that the introduction of crack occurred in the mid-to-late 1980s (Cork 1999; Grogger and Willis 2000; Caulkins 2001). Grogger and Willis (2000) surveyed police departments in 27 metropolitan areas as to the year in which crack was first noted and compared responses from the survey with changes in indications of drug use from emergency room incidents as collected by the Drug Abuse Warning Network (DAWN). Arrival dates tended to be earliest in East and West Coast cities and later for cites in the Midwest. Cork (1999) used data on drug arrests and gun homicides to associate changes in crack market activity and youth murder rates. He also found that clusters of drug arrests began first in the West and Northeast before moving inland.

The peak in crack use and its decline followed a similar pattern. Analyses of urine among arrestees from the Drug Use Forecasting (DUF) program suggest that crack use began to fall around 1989 in New York, Philadelphia, and Los Angeles but later and more slowly in Cleveland, Chicago, and Indianapolis (Golub and Johnson 1997). For example, the proportion of arrestees that tested positive for crack/cocaine in 1989 exceeded 70 percent in New York and Philadelphia, 60 percent in Washington D.C. and 56 percent in Los Angeles. In Cleveland, Chicago, Dallas, Denver, Houston, Indianapolis, Kansas City, San Antonio, and St. Louis, the prevalence of crack/cocaine among arrestees ranged from approximately 20 to 55 percent in 1989 and in several cities actually rose in the early 1990s.

Several points from this discussion are relevant. First, data on crack use by state and year are too incomplete to apply empirically. Second, what is known suggests that crack markets developed in different cities at different times and thus represent a state-year period effect that is not captured by national trends. Third, the data also suggest that New York City and Los Angeles were early sites of crack markets. Not only are these the largest cities in the two largest states, but abortion became legal in both states roughly three years before *Roe*. Thus, 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.

The potential confounding from time-varying period effects is illustrated by the time-series of age- and race-specific homicide rates. Figure 1a shows homicide rates for white teens (ages 15 to 19) and young adults (ages 20 to 24) in repeal and nonrepeal states from 1985 to 1997; Figure 1b presents the corresponding series for blacks. Repeal states are those that legalized abortion between 1969 and 1970: Alaska, California, Hawaii, New York, and Washington. I also include Washington D.C. among the early legalizers.[2] Abortion became legal in the nonrepeal states in 1973 with the Supreme Court decision in *Roe v. Wade*.

2. Washington D.C. has not been treated as an "early legalizer" in previous analyses. However, the 1969 decision in *United States v. Vuitch* rendered the District's abortion law unconstitutional. As a result, writes Lader, "Washington's abortion facilities soon ranked among the busiest in the country, with 20,000 patients in 1971" (Lader 1974, p. 115). Data on abortion in 1971 from the Center for Disease Control (1972) support Lader's observation. The *resident* abortion ratio (abortions per 1,000 live births) in D.C. in 1971

Two points are noteworthy. First, homicide rates rise earlier in repeal than in nonrepeal states consistent with the earlier arrival of crack in New York and California. Second, the curvilinear trend in homicide rates is similar among teens and young adults within repeal and nonrepeal states and is inconsistent with a strong cohort affect associated with legalized abortion. Most teens in 1985 were born before 1970 and thus were unexposed to legalized abortion in utero. By 1990, however, teens in repeal states had been born after 1970 and were thus exposed. Put differently, teens in repeal states in 1990 represent the first cohort of "more wanted" births. Thus, evidence of a cohort effect associated with the pre-*Roe* legalization of abortion would be a relative decrease in teen homicide rates in repeal states beginning around 1988, followed five years later by a similar decline among young adults. There is no evidence of such a pattern among either blacks or whites. In fact, the coincident movement in homicide rates by teens and young adults is more consistent with strong period effects. In order to isolate a cohort effect associated with the legalization of abortion, researchers must adjust for these dramatic trends in crime *within-states*.

C. Mismeasurement and Endogeneity of Abortion

Another drawback to Donohue and Levitt's empirical strategy is the mismeasurement of abortion and its endogeneity in the years after legalization. Demographers estimate that approximately two-thirds of all legal abortions replaced illegal ones in the first year after legalization. Estimates are based on the change in births between 1970 and 1971 compared to the number of reported abortions in 1971 (Sklar and Berkov 1974; Tietze 1973). As noted above, Donohue and Levitt have no data on abortion for cohorts born before 1974 and thus assume a zero abortion ratio for more than half their observations. A facile argument is to assume that any error is likely random and estimates are biased downward. But this assumption is decisively contradicted by the data. As a simple example, Kansas had an abortion ratio of 414 per 1,000 live births in 1973. Donohue and Levitt assume the abortion ratio in Kansas is zero in 1972. However, data collected by the Centers for Disease Control (CDC) (Centers for Disease Control 1974) indicate that Kansas had an *observed* abortion ratio of 369 per 1,000 live births in 1972! Going further, I estimated the resident abortion rate in 1972 using published CDC data and the algorithm used by AGI for assigning abortions by state of residence in 1973. The correlation between resident abortion rates or ratios in 1972 and 1973 is 0.95. In other words, states with the greatest abortion ratios in 1973 had the greatest abortion ratios in 1972. By assuming the abortion ratio was zero in the 45 nonrepeal states and Washington, D.C., Donohue and Levitt build in an error that is negatively correlated with the true abortion rate. As a result, the direction of the bias is unknown.[3]

was 793, more than double that of New York or California. Thus, I include Washington, D.C. in all analyses as a repeal state. However, my results are not sensitive to its inclusion as a repeal state.

3. To illustrate, Let A_{72} be the observed abortion ratio in 1972, a_{72} the actual abortion ratio and u_{72} the error. Thus

$$A_{72} = a_{72} + u_{72}$$

Recall that $A_{72} = 0$ in their analysis; thus, $a_{72} > 0$ and $u_{72} < 0$ and the true abortion ratio and the error are negatively correlated; moreover, given the strong positive correlation between the observed abortion ratios in 1972 and 1973 noted above, the correlation between a_{72} and u_{72} is undoubtedly robust. Now let

The other difficulty with the abortion ratio as a measure of unwanted childbearing is that abortion is endogenous to sexual activity, contraception and fertility. Some pregnancies that were aborted in the mid- to late 1970s may not have been conceived had abortion remained illegal. This weakens the link between abortion and unwanted childbearing. In addition, Donohue and Levitt use the abortion ratio (abortions/births) and refer to it as the abortion rate (abortions/women). This exacerbates the endogeneity problem and makes the abortion ratio a less clear proxy for unwanted births. The growth in AFDC and Medicaid in the 1970s, for instance, changed the price of a birth for many poor women. Thus, the abortion ratio may vary for reasons unrelated to unwanted childbearing.

D. Selected replication of Donohue and Levitt's findings

To illustrate some of the difficulties with Donohue and Levitt's identification strategy, I have replicated their key findings and presented them in Table 1. Their primary evidence of an association between abortion and crime comes from two sets of regressions. In the first, rates of violent crime, property crime, and murder by state and year are regressed on what the authors term, the effective abortion rate. The latter is an average of state abortion ratios from 1970 to 1985 weighted by the proportion of arrestees "exposed" to legalized abortion.[4] In the second set of regressions, the logarithm of arrests for violent and property crime by single year of age is regressed on the state abortion ratio the year before the cohort was born. Arrests pertain to teens and young adults 15 to 24 years of age between 1985 and 1996, which correspond to birth cohorts from 1961 to 1981. Donohue and Levitt assume that the abortion ratio is zero for cohorts born before 1974.

Row 1 of Table 1 replicates the key index crime regressions from Donohue and Levitt (2001, Table 4). Only the coefficient on the effective abortion rate is shown. As Donohue and Levitt note, an increase of one standard deviation in the effective abortion ratio, an increase of approximately 100 abortions per 1,000 live births, lowers crime between 9 and 13 percent. As Donohue and Levitt demonstrate, these estimates are quite robust to changes in the set of included variables.[5] However, the estimates are very sensitive to the period analyzed, as shown in Rows 2 and 3. Specifically, if the same specification as in Row 1 is estimated for the years 1985 to

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C be the crime rate and following Maddala (1992) write the simple relationship between crime and the observed abortion ratio as used by Donohue and Levitt as follows:

(2) $C = \beta A + e \quad \beta < 0$

Substitute $(a + u)$ for A in Equation 2. It is straightforward to show that

$$\text{plim } b = \beta(\sigma_{aa} + \sigma_{au})/(\sigma_{aa} + 2\sigma_{au} + \sigma_{uu})$$

where σ_{ij} is the relevant covariance. Because σ_{aa} and σ_{uu} are both positive and σ_{au} is negative, the effect of the systematic error on the plim of b is unknown in this simple context.

4. In 45 states plus the District of Columbia they assume the abortion ratio was zero between 1961 and 1972. For the other five states they estimate abortions for 1970–72 by backcasting linearly from 1973 totals and then assume a zero abortion ratio from 1961 to 1969.

5. The important exception is when they include a state-specific trend term (Donohue and Levitt 2001, Table 5).

Table 1
The Relationship between Abortion and Crime: Regressions of Total Index Crime Rates and Log Arrests by Single Year of Age for 15- to 24-Year-Olds

	Panel A: Index Crime Rates on Effective Abortion Ratio		
	Violent Crime	Property Crime	Murder
Row/period			
1. 1985–97	−0.129	−0.091	−0.121
	(0.024)	(0.018)	(0.047)
2. 1985–90	0.017	−0.033	0.276
	(0.045)	(0.018)	(0.066)
3. 1991–97	−0.209	−0.186	−0.338
	(0.035)	(0.034)	(0.053)
	Panel B: Log Arrests on Lagged Abortion Ratio		
	Violent Crime Arrest	Property Crime Arrest	Murder Arrest
4. 1985–96	−0.015	−0.040	−0.028
	(0.003)	(0.004)	(0.006)
5. 1985–90	0.020	−0.028	0.041
	(0.006)	(0.006)	(0.013)
6. 1991–96	−0.011	−0.041	−0.013
	(0.007)	(0.006)	(0.007)
7. Birth cohorts 1974–81	−0.009	0.011	0.009
	(0.008)	(0.008)	(0.022)

Figures (standard errors) are the coefficients on the effective abortion ratio (Panel A) or the lagged abortion ratio (Panel B). Rows 1 and 4 replicate the regressions from Tables 4 and 7 in Donohue and Levitt (2001). Rows 2, 3, 5, and 6 estimate the same specifications but for the designated subperiods. Row 7 limits the regressions of log arrests to cohorts for which abortion data are available. This sample includes arrests of individuals 15 to 22 years of age and years 1989 to 1996. Following Donohue and Levitt, the abortion ratio has been multiplied by 100 in all regressions.

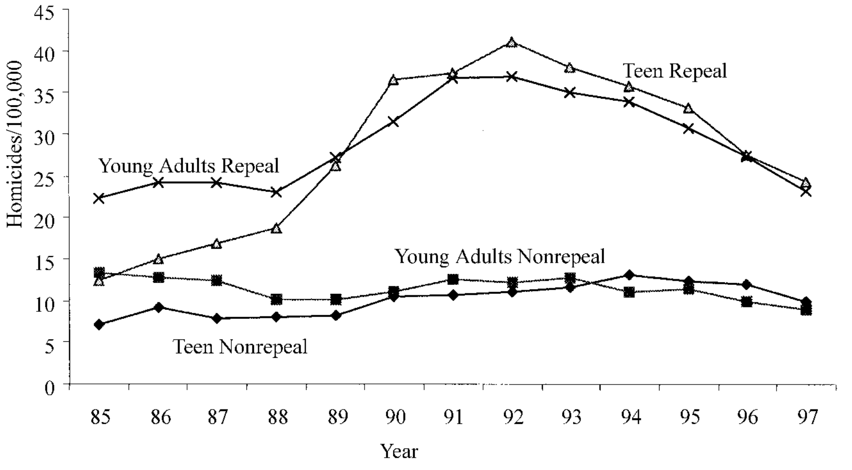

Figure 1a
White Homicide Rates for Teens (15–19) and Young Adults (20–24) by Repeal and Nonrepeal States, 1985–97*

1990, the coefficient on the effective abortion ratio becomes positive and statistically insignificant in the case of violent crime, negative but greatly reduced in the case of property crime ($p < .10$), and positive, very large, and statistically significant in the case of murder. When I estimate the model for the years 1991 to 1997, the results are largely reversed. For each crime, the coefficient on the effective abortion ratio is negative and statistically significant. Indeed, the change in the effective abortion ratio between 1991 and 1997 multiplied by its coefficient in the murder regression explains the entire fall in homicide between 1991 and 1997.[6]

Estimates in Panel B are from the same exercise as in Panel A but applied to age-specific arrests. In these regressions, the natural logarithm of arrests for 15- to 24-year-olds by single year of age are regressed on the abortion ratio in the year before each cohort was born. The unit of observation is the cohort/state/age cell. Estimates in Row 4 again replicate the results in Donohue and Levitt (2001, Table 7); estimates in Rows 5 and 6 are for the designated subperiods. The pattern observed with the index crimes in Panel A is repeated in Panel B: abortion is inversely related to arrests ($p < .01$) over the full period, but the association reverses sign for violent crime and murder arrests between 1985 and 1990, and is consistently negative when estimated for years 1991 and 1996.

The lack of temporal homogeneity in the abortion-crime association points to problems of omitted variables.[7] As shown in Figure 1, murder rates among teens

6. The murder rate fell from 9.8 to 6.8 per 100,000 between 1991 and 1997, a decline of 31 percent. The effective abortion rate for murder rose from 33 to 142 per 1,000 live births over the same period. Thus, the predicted change in the log murder rate based on the regression result for murder in Row 3 is $-0.00338*(142 - 33) = -0.368$ or 36.8 percent.

7. Donohue and Levitt (2003) argue that tests of abortion and total crime are weak between 1985 and 1990 because a relatively small proportion of all criminals were exposed to legalized abortion before 1990.

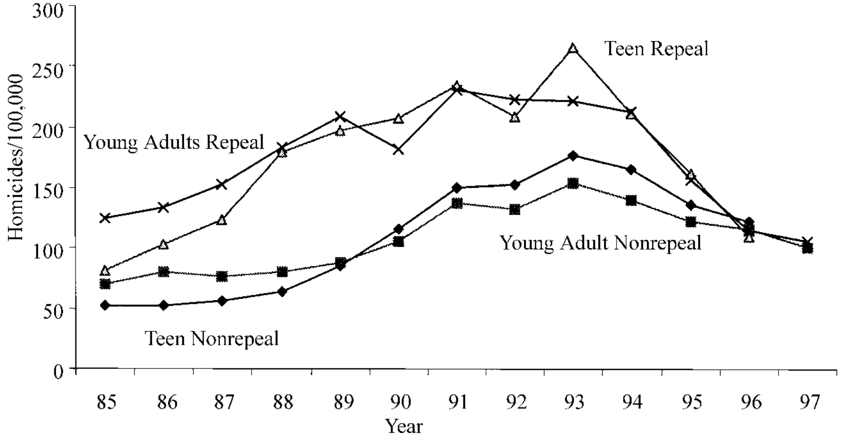

Figure 1b
Black Homicide Rates for Teens (15–19) and Young Adults (20–24) by Repeal and Nonrepeal States, 1985–97
*Repeal States: AK, CA, DC, HI, NY, WA
Source: FBI's Supplemental Homicide Reports

and young adults rise rapidly between 1985 and 1992 and then fall precipitously. Lagged abortion ratios are also rising during this time. Year fixed effects remove national trends in both abortion and crime, but they do not eliminate confounding from state-specific shocks associated with say, the diffusion of crack cocaine. One solution is to include controls for state-specific linear or quadratic trends but such terms remove all variation in the abortion ratio.[8]

The other notable result in Table 1 is the lack of any association between abortion and arrests when the analysis is limited to cohorts for which data on abortion exist (Table 1, Row 7). These regressions associate arrests between 1989 and 1996 to abortion between 1974 and 1981. This is a period of rapid growth in *reported* legal abortion and there is substantial variation both within and between states. Moreover, the

As evidence, they point to their relatively low effective abortion ratio over this period. However, the low figure results from their inappropriate assumption that there were no abortions prior to 1973 in the 45 nonrepeal states. Early surveillance by the CDC found that there were 175,508 *reported* abortions in 1970, 480,259 in 1971, and 586,760 in 1972 in the United States (Centers for Disease Control 1971, 1972, 1973). Moreover, the *resident* abortion ratio in the repeal states: Alaska, California, Washington D.C., Hawaii, New York, and Washington, was 340 in 1971 and 370 in 1972 (Author's calculations based on data from CDC (1972, Table 4) and CDC (1974, Table 5). According to CDC data, the abortion ratio for the entire US peaked in 1981 at 358 (Koonin et al. 1997). In other words, cohorts born in repeal states between 1971 and 1973 were exposed to a level of abortion that exceeded the maximum average exposure for the entire country at any time since abortion became legal.

8. The adjusted R-squared in a regression of the effective abortion ratio on state dummies, year dummies, and state-specific linear trends is over 0.99, which explains the sensitivity of Donohue and Levitt's estimates to the inclusion of state-specific linear trend terms (Donohue and Levitt 2001, Table 5). Moreover, quadratic trends are more appropriate given the curvilinear trajectory of crime rates, but their estimates become nonsensical when such terms are included.

matching of arrest rates by single year of age to the abortion ratio in the year the cohort was "in utero" is a more direct means of linking the exposure to the outcome than is the analysis of the total crime rate regressed on an "effective abortion ratio," a highly aggregated measure of exposure. The absence of a correlation between abortion and arrests in this subsample suggests that Donohue and Levitt's decision to code the abortion ratio as zero prior to legalization may be driving their results. Alternatively, the endogeneity of abortion may explain the lack of an association with arrests. States in which the cost of abortion is lower may have greater sexual activity, lower use of contraception and higher abortion rates than states in which the cost and stigma associated with abortion are greater. If true, then variation in abortion may be only weakly associated with differences in unintended childbearing.[9]

In the empirical analysis that follows, I attempt to address each of the identification issues just discussed. The advantage of the difference-in-differences strategy is that by staying close to the "experiment" made available by the legalization of abortion, I associate changes in crime with plausibly exogenous changes in unintended fertility. At the same time, I avoid problems with poorly measured abortion. What I lose is any dose-response effect associated with variation in unwanted childbearing. However, in some analyses I estimate models separately for states with abortion rates above and below the median abortion rate in 1973. If abortion rates were essentially zero in 1972 in the nonrepeal states, as Donohue and Levitt assume, then the effects should be more negative for the states with greater post-*Roe* abortion rates.

III. Empirical Specification and Results

A. *Comparison by Year of Birth in Repeal and Nonrepeal States*

Abortion laws in Alaska, California, Hawaii, New York, Washington, and the District of Columbia, what I have referred to as the "repeal states," changed dramatically between late 1969 and 1970. The result was de jure or de facto legalization in repeal states almost three years prior to national legalization in 1973. Thus, there are two major policy changes that I use to identify effects of abortion on crime: early legalization among cohorts from repeal states and national legalization following *Roe*. I limit the analysis to 15- to 24-years-olds because the Uniform Crime Reports record arrests by single year of age for this group only. These are the same data used by Donohue and Levitt. In addition, I analyze homicide offenses as recorded on the FBI's Supplemental Homicide Reports (SHR) [Fox 2000]. These are also available by single year of age.[10] I further limit this sample to cohorts born between 1967 and 1979.[11]

9. Joyce (2001) shows that the resident abortion rate in repeal states is almost double that of nonrepeal states between 1975 and 1985, but that the fertility rate is the same in both groups of states. The higher pregnancy rate but similar fertility rate in repeal states is consistent with greater sexual activity and/or less contraception induced, in part, by the protection against unwanted childbearing afforded by the relatively greater accessibility of abortion services.

10. The biggest drawback to the SHR is their reporting deficiencies. Information on the age and race of the offender when missing is imputed based on the known distribution by age/race/sex of victims and offenders by state and year (Maltz 1999). Nevertheless, Supplemental Homicide Reports are widely used to track crime by age and race (Maltz 1999; Cook and Laub 1998; Fox and Zawitz 2000). Moreover, I use them in conjunction with murder arrest rates. Thus, a consistent relationship between abortion and crime across these two measures of homicide provides an important check of these data.

11. Cohort is equal to year minus age.

I structure the difference-in-difference (DD) analysis in two ways. In the first, I compare changes in crime by birth cohorts before and after exposure to legalized abortion. This is closest to what Donohue and Levitt do, but they use a continuous measure of abortion to proxy unwanted childbearing. The identifying variation is based on cross-state changes in crime among cohorts of the same age. In the other set of DDs, changes in crime among cohorts before and after exposure to legalized abortion in utero are compared to changes among older cohorts who are close in age, but who were unexposed to legalized abortion. The identifying variation comes from within-state changes in crime.

In the cross-state DDs exposure is based on state/year-of-birth interactions. Specifically, I define birth years 1967–69 as the pre-exposure years and 1971–73 as the post-exposure year in repeal states. I subtract changes in crime among cohorts born between 1967–69 and 1971–73 in nonrepeal states from changes observed for the same cohorts in repeal states. The identifying assumption is that changes in crime among cohorts in nonrepeal states are a good counterfactual for changes in repeal states. A potential problem with this strategy is that hard to measure period effects, such as the spread of crack, may affect crime in repeal and nonrepeal states at different times and with different intensity. If so, then nonrepeal states do not provide an adequate counterfactual (see Figures 1a and 1b). To improve the counterfactual, I estimate models limited to a subset of states in which there was evidence of crack/cocaine use in their major cities between 1984 and 1989 as reported by Grogger and Willis (2000). These include Colorado, Florida, Georgia, Illinois, Indiana, Louisiana, Maryland, Massachusetts, Michigan, Missouri, New Jersey, Ohio, Pennsylvania, Texas, and Virginia. I refer to these as the comparison states. The purpose is to pair repeal states to a subset of nonrepeal states that may have experienced similar period effects. The relevant regression is as follows:

$$(1) \quad \mathrm{Ln}\, C_{ajy} = \beta_0 + \beta_1(Repeal_j * Y70_y) + \beta_2(Repeal_j * Y7173_y) + \beta_3(Repeal_y * Y7476_y) + \beta_4(Repeal_y * Y7779_y) + U_{aj} + V_{ay} + \varepsilon_{ajy}$$

where $\mathrm{Ln}\, C_{ajy}$ is the natural logarithm of arrests or homicides for age group, a, in state, j, and year of birth, y. This is the same dependent variable used by Donohue and Levitt (2001). *Repeal* is a dummy variable that is one for repeal states; *Y70, Y7173, Y7476, and Y7779* are dummy variables for cohorts born in the designated years. The omitted category includes the birth years 1967–69. Equation 1 also includes fixed effects for age-state (U_{aj}) and age-year (V_{ay}) interactions. Thus β_2, the coefficient on the interaction of *Repeal* and *Y7173,* measures the proportionate change in crime between the 1971–73 and 1967–69 birth cohorts in repeal states relative to nonrepeal states.[12] The coefficient on the other interaction term, β_3, mea-

12. Significant court decisions in the fall of 1969 affected abortion laws in California and Washington, DC. Legalization occurred in Alaska in July of 1970, Hawaii in March of 1970, New York in July of 1970 and Washington in November of 1970. Given that the full impact of these reforms on unintended childbearing would not be evident until 1971, I treat *Repeal*1970* as a separate interaction in order to compare periods clearly pre and post the change in the legalization (see Sklar and Berkov 1974; Gruber, Levine and Staiger 1999). Including 1970 in the prelaw period does not affect my results.

sures the effect of national legalization. If abortion lowers crime, then *Roe v. Wade* should bring about a relative improvement in crime rates among nonrepeal states. As a result, β_3 should approach zero depending on the speed of adjustment; and β_4 should unambiguously equal zero as adjustment to national legalization is completed (Gruber, Levine, and Staiger 1999).

Results from the estimate of Equation 1 are shown in Table 2. I display only the coefficients on the interaction terms β_2, β_3 and β_4 in Equation 1. There are two specifications for each measure of crime. The first includes all states and contrasts changes in crime between repeal relative to nonrepeal states.[13] The second limits the sample to repeal and 15 comparison states.

I have also included estimates of the reduced-form regression using the natural log of state fertility rates as the dependent variable (Columns 1 and 2). These estimates are almost identical to those of Levine et al. (1999).[14] They show that early legalization in the repeal states was associated with approximately a 6 percent relative decline in fertility rates regardless of whether I use all 51 states (Column 1) or only repeal and comparison states (Column 2). National legalization following *Roe v. Wade* had no additional impact on fertility rates in repeal states.

Estimates in the first row of Table 2 indicate that arrests and homicides fell for cohorts born between 1971 and 1973 relative to those born between 1967 and 1969 in repeal relative to nonrepeal states. These estimates are largely consistent with results obtained by Donohue and Levitt (2001). Violent crime arrests, for instance, declined 5.0 percent more in repeal relative to nonrepeal states over this period (Column 3). This decline is similar in magnitude to the effect obtained by Donohue and Levitt with a continuous measure of abortion.[15] However, two other patterns emerge from these results that are less supportive of the Donohue and Levitt hypothesis. First, estimates based on the subsample of repeal and comparison states are relatively small in magnitude and statistically insignificant. The coefficient on violent crime arrests, for instance, is −0.026, half as large as when all states are included. A distinguishing characteristic of the comparison states is that they all have large urban centers with a sizeable African-American population. As such, the comparison states may provide a more credible counterfactual for changes in crime among the repeal states, which are dominated by California and New York. The other inconsistent

13. The unit of observation is the cohort/state/age cell. There are potentially 4,896 observations given 10 age groups, 51 states and various years. Three hundred and forty-one observations on arrests and 157 on homicide are missing because some states did not report arrests or homicides in selected years. There are 3 cells with zeros for violent arrests, 666 for murder arrests and 739 for homicides. The model includes dummy variables for all age and state interactions as well as age and year of birth interactions, as represented by the last two terms of Equation 1. The specification is identical to that of Donohue and Levitt (2001) with the important difference that I have included categorical variables to measure differential exposure to legalized abortion instead of the actual abortion ratio.

14. Unlike Levine et al. (1999), I include Washington, D.C. as a repeal state.

15. Donohue and Levitt multiply the coefficient on abortion by 350, which is the difference in abortion ratios between states in the top third versus bottom third of abortion ratios. Using the results in Row 4, Column 1 of Table 1, this yields an effect of −5.3 percent ($-0.015 * 350$). The precision of their estimates and mine differ because I allow for a more general covariance structure among states following Betrand, Duflo, and Mullainathan (2002). This is implemented in Stata by clustering on state. When I redo Donohue and Levitt's regressions of log arrests and allow for a more general covariance structure, the standard errors double. This is not surprising since 60 percent of their observations assume an abortion ratio of zero, which probably induces substantial serial autocorrelation.

Table 2
Reduced-Form Estimates of Fertility Rates and Log Arrests and Murders among 15- to 24-Year-Olds by Birth Cohort and Repeal and Nonrepeal States 1985–96

	Ln Fertility Rate Women 15–44		Ln Violent Crime Arrests		Ln Property Crime Arrests		Ln Murder Arrests		Ln Murders	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Repeal 71–73	−0.065*	−0.063*	−0.050	−0.026	−0.066+	−0.030	−0.108	−0.073	−0.060	−0.038
	(0.011)	(0.014)	(0.066)	(0.070)	(0.032)	(0.039)	(0.061)	(0.064)	(0.073)	(0.079)
Repeal 74–76	−0.015	−0.002	−0.021	0.027	−0.069	−0.014	−0.167*	−0.087	−0.169	−0.124
	(0.021)	(0.025)	(0.103)	(0.116)	(0.063)	(0.082)	(0.063)	(0.064)	(0.141)	(0.152)
Repeal 77–79	0.004	0.010	−0.066	0.022	−0.089	−0.010	−0.409*	−0.290^{+}	−0.356	−0.276
	(0.033)	(0.037)	(0.123)	(0.145)	(0.089)	(0.126)	(0.135)	(0.137)	(0.200)	(0.218)
Only repeal and comparison states?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	0.963	0.974	0.983	0.979	0.982	0.976	0.929	0.939	0.919	0.917
N	969	399	4,552	1,890	4,555	1,890	3,889	1,829	4,000	1,870

Except for Columns 1 and 2, coefficients (standard errors below) are relative changes in arrests and homicides in repeal relative to nonrepeal states for the indicated birth cohorts (1971–73, 1974–76, and 1977–79) relative to the 1967–69 birth cohorts. Columns 1 and 2 show the reduced-form regression for log fertility rates. For each outcome there are two specifications: Columns 1, 3, 5, 7, and 9 use all states; Columns 2, 4, 6, 8, and 10 use only repeal and comparison states (see text for a list of comparison states). All specifications for arrests and homicides include fixed effects for interactions of age and state as well as age and year of birth (see Equation 1 in the text). Standard errors have been adjusted for intra-class correlation within state by clustering on state with Stata's robust procedure. There are 4,896 possible cells in the full sample of arrest and homicides: 10 age groups, 51 states and a varied number of age/year cells since the sample is limited to cohorts born between 1967 and 1979. Cells are lost due to nonreporting by states and zero crimes (see Footnote 13 in text). All regressions have been weighted by the state population.
$^{+}p < .05$; $^{*}p < .01$

pattern is that estimates of β_2 and β_3 in Equation 1 exceed those of β_1 in absolute value for murder arrests and murders. As Gruber, Levine, and Staiger (1999) have argued, one would expect a relative decrease in adverse outcomes in nonrepeal states following national legalization, which should drive β_2 and β_3 to zero. Instead, I find that cohorts born between 1977 and 1979 in repeal states experience relative declines in murders and murder arrests of between 28 to 41 percent, much larger than the declines experienced among the 1971–73 birth cohorts. Importantly, there is no relative decrease in fertility for cohorts born between 1977 and 1979, which undermines a link between abortion and crime.[16]

Lastly, I divide the sample between 1985–90 and 1991–96 and reestimate Equation 1 separately for the two subperiods. The results for all states are shown in Table 3. The pattern is similar to what occurred when I split the sample in Table 1. If I restrict the sample to those arrested between 1985 and 1990, I find that exposure to legalized abortion in the repeal states is positively related to arrests and murders. By contrast, analyses of arrests and murders from 1991 to 1996 reveal the opposite. Moreover, the coefficients in each subperiod are large in absolute value and they are unexpectedly larger for cohorts born after 1973 relative to those born before 1973.

The temporal inconsistency calls into question the DD strategy based on changes in similar cohorts across states. Changes in crime in nonrepeal states will be an inappropriate counterfactual, if crack markets developed earlier and had a greater impact on state crime rates in repeal relative to nonrepeal states. I turn, therefore, to my alternative strategy of using a within-state comparison group to adjust for hard to measure period effects. I focus first on the 1985–90 period, which provides a broad comparison of aggregated crime and arrest rates of teen and young adults. Donohue and Levitt have criticized my use of this period, since I fail to use data from the 1990s. However, 1985–90 is a useful period because I can create a plausible within-state comparison group that was clearly affected by the upsurge in crime, but that was unexposed to legalized abortion. Second, I can analyze the same experiment by race given the availability of population data by state, year, and race for five-year age groups. This adds an important dimension to the test since the legalization of abortion had a much larger effect on black relative to white fertility (Levine et al. 1999; Angrist and Evans 1999). I then turn to a test of abortion and crime using arrest and homicide rates in 1990s. I have to narrow the age groups analyzed in order to isolate those exposed and unexposed to national legalization following *Roe*. However, I use some of the most crime-prone age groups and the narrow age bands have the advantage of minimizing differences in age-crime profiles between the exposed and comparison groups.

16. There is virtually no difference in fertility rates between repeal and nonrepeal states for the years 1977–79 despite the fact that the abortion rate is 76 percent greater in repeal states (see Figure 1 in Joyce 2001). For abortion to lower crime, therefore, it must be argued that abortion improved the timing of births, which in turn had an enormous effect on the well-being of the effected cohorts. Indeed, the effects of better-timed births on homicide have to be an order of magnitude greater than the effects associated with an actual decrease in fertility for this story to hold. This seems implausible in light of the recent literature on the effects of delayed childbearing among teens (Geronimous and Korenman 1992; Hotz, McElroy, and Sanders 1999).

Table 3
Split-Sample Reduced-form Estimates of Log Arrests and Murders among 15- to 24-Year-Olds by Birth Cohort and Repeal and Nonrepeal States: 1985–90 and 1991–96

	Ln Violent Crime Arrests		Ln Property Crime Arrests		Ln Murder Arrests		Ln Murders	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1985–90	1991–96	1985–90	1991–96	1985–90	1991–96	1985–90	1991–96
Repeal 71–73	0.054	−0.045	0.009	−0.097+	0.106	−0.121	0.175*	−0.228+
	(0.061)	(0.054)	(0.027)	(0.044)	(0.059)	(0.094)	(0.066)	(0.116)
Repeal 74–76	0.194	−0.012	0.055	−0.139	0.162	−0.124	0.138	−0.330
	(0.118)	(0.076)	(0.060)	(0.081)	(0.208)	(0.134)	(0.236)	(0.211)
Repeal 77–79	na	−0.054	na	−0.183	na	−0.352	na	−0.529
		(0.101)		(0.123)		(0.203)		(0.286)
R-squared	0.990	0.987	0.995	0.987	0.929	0.937	0.917	0.930
N	1,917	2,635	1,919	2,636	1,901	2,299	1,583	2,417

See note to Table 2.
"na," not applicable since 15-year-olds born in 1977 would be arrested after 1990.

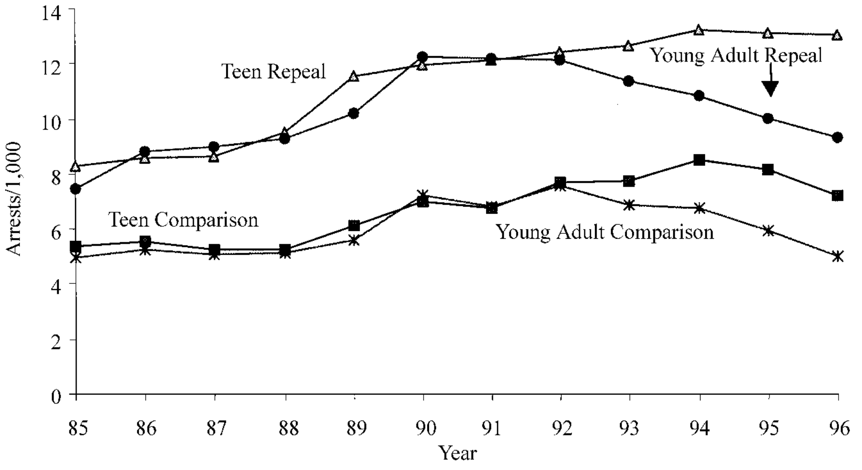

Figure 2a
*Violent Crime Arrest Rates for Teens and Young Adults by Repeal and Comparison States**

B. Comparisons by Year of Crime within Repeal and Nonrepeal States

In this analysis I compare the change in arrests and homicide rates among teens between 1985 and 1990 to the change among young adults. Teens and young adults in 1985 were born prior to 1971 and thus unexposed to legalized abortion *in utero*. By 1990, almost all teens had have been born after 1970 but few of the young adults. Thus, teens in repeal states go from unexposed to exposed between 1985 and 1990 and young adults remain essentially unexposed. A limitation of using a within-state comparison group is that the age-crime profile of teens and young adults may differ. Thus, I allow for a third set of differences (DDD) in which I subtract the DD in nonrepeal states from the DD in repeal states. Since few teens in the nonrepeal states were exposed to legalized abortion during this period, the DD in nonrepeal states measures age effects under the assumption of common period and cohort effects.

Figures 2 and 3 present time-series of arrests and homicide rates stratified by repeal and comparison states. A key observation is that the level and pattern of crime among teens and young adults is more similar *within* states than across. This provides visual support for the use of a within-state DD. To test for a cohort effect more formally, I estimate the following regression.

$$\text{(2)}\quad \mathrm{Ln}CR_{ajt} = \beta_0 + \beta_1 Teen_a + \beta_2(Teen_a * Repeal_j) + \beta_3(Repeal_j * Y8788_t) + \beta_4(Teen_a * Y8788_t) + \beta_5(Repeal_j * Y8990_t) + \beta_6(Teen_a * Y8990_t) + \beta_7(Teen * Repeal * Y8990) + X_{jt}\pi + U_j + V_t + e_{ajt}$$

where $\mathrm{Ln}CR_{ajt}$ is the natural logarithm of the arrest or homicide rate for age group a (teen or young adult), in state j, and year t. *Repeal* is a dummy variable that is

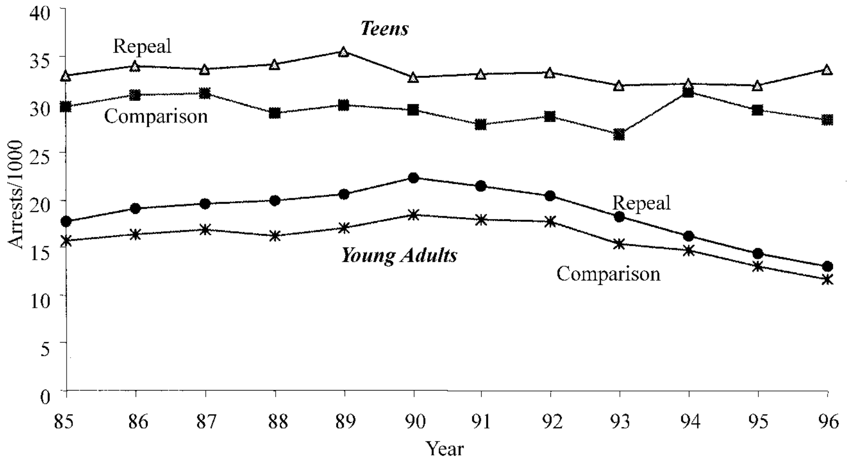

Figure 2b

*Property Crime Arrest Rates for Teens and Young Adults by Repeal and Comparison States**

*Repeal states include: AK,CA,DC,HI,NY,WA; Comparison states include: CO,FL,GA,IL,IN,LA,MD, MA,MI,MO,NJ,OH,PA,TX,VA

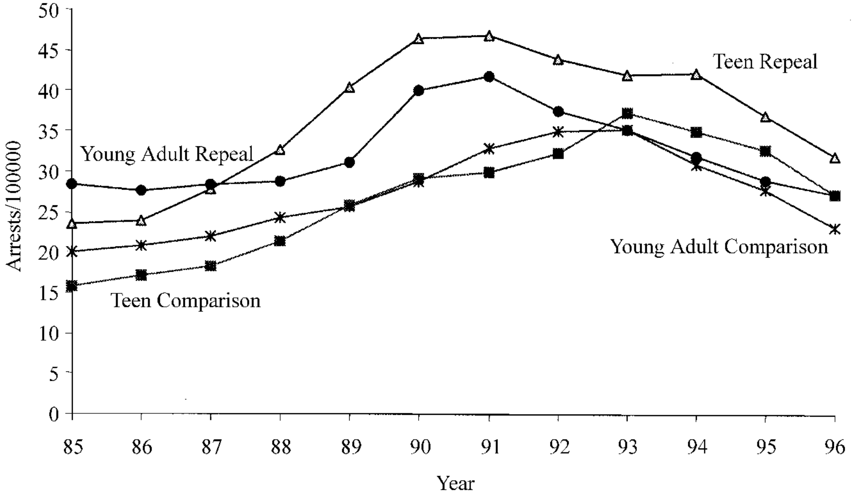

Figure 3a

*Murder Arrest Rates for Teens and Young Adults by Repeal and Comparison States**

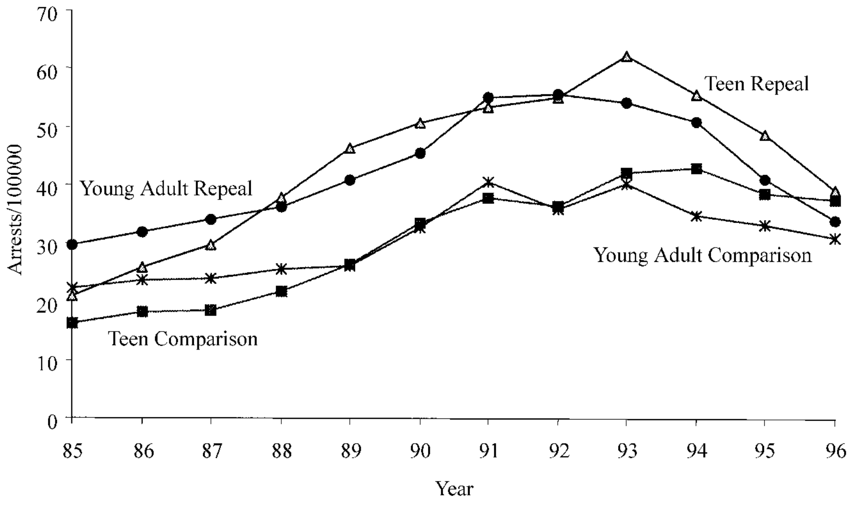

Figure 3b
*Murder Rates for Teens and Young Adults by Repeal and Comparison States**
*Repeal states include: AK,CA,DC,HI,NY,WA; Comparison states include: CO,FL,GA,IL,IN,LA,MD, MA,MI,MO,NJ,OH,PA,TX,VA

one for repeal states; *Y8788* and *Y8990* are dummy variables for the designated years and *Teen* is an indicator of those 15 to 19 years of age as compared to young adults ages 20 to 24. The omitted category includes the years 1985–86. State and year effects are represented by U_j and V_t, and $\boldsymbol{X}_{jt}$ is the matrix of control variables used by Donohue and Levitt (2001) in their regressions of index crime rates. The DDD estimate is β_7, which measures the proportionate change in arrest or homicide rates before and after exposure to legalized abortion (years 1985–86 versus 1989–90) among teens relative to young adults in repeal relative to nonrepeal states. If abortion lowers crime, then β_7 should be negative.[17]

Results are displayed in Table 4. The first three columns show estimates for arrest rates; the next three columns present estimates for homicide rates for all perpetrators, then separately for whites and blacks. The sample in Panel A includes all available states whereas Panel B is limited to repeal and comparison states only. The figures in Row 1 represent the difference-in-difference of arrest and homicide rates (in logs) between teens and young adults for the years 1989–90 and 1985–86 in repeal states.[18] Thus, the natural logarithm of violent crime arrest rates rose 0.02 or 2.0 percent

17. There are several differences between Equations 1 and 2 that merit note. In Equation 2 I analyze arrest and homicide rates, instead of levels; I also aggregate arrests and homicides by age for teens (ages 15 to 19) and young adults (ages 20 to 24). Aggregation also lessens the loss of cells due to zero homicides in a semi-logarithmic specification. In addition, the regressions are by year of arrest or homicide and not by year of birth explicitly. This makes the structure of the DDD more transparent. Finally, the analysis is limited to the years 1985–90.

18. The DD estimates are obtained from the regressions. Using the notation from Equation 2, the DD estimate for repeal states is $\beta_6 + \beta_7$.

Table 4
Changes in Log Arrest and Homicide Rates for Teens (15–19) Relative to Young Adults (20–24) in Repeal and Nonrepeal States by Exposure to Legalized Abortion, 1985–90

	Arrest Rate for			Homicide Offending Rates		
	Violent	Property	Murder	All	Whites	Blacks
Changes in Arrests and Homicide (90–89)–(86–85): Teens newly exposed, young adults unexposed	Panel A: All States					
1. DD, teens-adults, repeal states	0.020	−0.127*	0.379*	0.434*	0.561*	0.432*
	(0.031)	(0.011)	(0.050)	(0.079)	(0.075)	(0.148)
2. DD, teens-adults, nonrepeal states	−0.010	−0.098*	0.210*	0.260*	0.380*	0.396*
	(0.036)	(0.022)	(0.044)	(0.053)	(0.075)	(0.069)
3. DDD (Row 1–Row 2)	0.030	−0.029	0.169+	0.174	0.181	0.036
	(0.047)	(0.025)	(0.066)	(0.095)	(0.106)	(0.148)
R-squared	0.934	0.917	0.867	0.850	0.827	0.841
N	594	594	576	581	559	473
	Panel B: Repeal and Comparison States Only					
4. DD, teens-adults, repeal states	0.019	−0.126*	0.380*	0.435*	0.561*	0.432*
	(0.032)	(0.011)	(0.053)	(0.082)	(0.078)	(0.095)
5. DD, teens-adults, comparison states	−0.043	−0.127*	0.243*	0.302*	0.438*	0.423*
	(0.052)	(0.035)	(0.056)	(0.070)	(0.106)	(0.095)
6. DDD (Row 4–Row 5)	0.063	0.000	0.137	0.133	0.123	0.008
	(0.060)	(0.036)	(0.076)	(0.108)	(0.131)	(0.181)
R-squared	0.948	0.938	0.919	0.887	0.885	0.838
N	242	242	241	244	229	230

Difference-in-difference-in-difference (DDD) estimates show relative changes in arrest and homicide rates between those exposed and unexposed to legalized abortion in repeal and nonrepeal states [Equation 2 in the text]. There are 612 possible state/age/year cells in the full sample (51 states $\times$ 2 age groups $\times$ 6 years). Missing cells are due to a nonreporting by states and/or zero crimes. Standard errors are in parentheses. Models include controls for prisoners, police, income, poverty, AFDC generosity, concealed gun laws, and beer tax as in Donohue and Levitt (2001). All models include state and year fixed effects. Regressions are weighted by the population, or race-specific population, 15 to 24 years of age. $^{+}p < .05$; $*\ p < .01$.

more among teens relative to young adults in repeal states between 1989–90 and 1985–86. In nonrepeal states violent crime arrest rates fell 1.0 percent more among teens relative to young adults (Row 2). The DDD estimates in Row 3 indicate that violent crime arrest rates increased 3.0 percent more among teens in repeal states relative to teens in nonrepeal states adjusted for within-state trends in arrests.

The last three columns contrast total homicide rates and then separately for whites and blacks. When limited to repeal and comparison states only (Panel B), we see that white teen homicide rates rose 56 percent more than homicide rates of young adults in repeal states and 44 percent more than in comparison states. The corresponding changes among blacks are 43 and 42 percent respectively. Clearly, age and period effects are huge during this period. Not only is there a dramatic relative increase in teen homicide rates, but it occurs in both repeal and nonrepeal states. As a result, the DDD estimates provide no evidence that exposure to legalized abortion among teens in repeal states had any dampening effect on the rise in homicide. The lack of an effect on black homicide rates is particularly noteworthy, since legalized abortion had a greater impact on black relative to white fertility.

C. Within-state Comparisons in Nonrepeal states: The Effect of Roe v. Wade on Crime

The next set of analyses takes advantage of the second "natural experiment," the national legalization of abortion following *Roe v Wade*. I drop repeal states and compare changes in crime pre- and post-*Roe* in the 45 nonrepeal states only. I limit the analysis to older teens and young adults for two reasons. First, I need a within-state comparison group that is close in age to the exposed group but born before *Roe*; at the same time, however, I need age groups that commit crimes in the 1990s in order to test the association between legalized abortion and crime during a period of declining criminality. The relevant regression is as follows:

$$(3)\quad \mathrm{Ln}\,CR_{ajt} = \beta_0 + \beta_1 Exposed + \beta_2(Post\_Roe * Exposed) + U_j + V_t + \varepsilon_{ajt}$$

Let $\mathrm{Ln}\,CR_{ajt}$ be the natural logarithm of the arrest or homicide rate for age group, a, in state, j, and year, t; *Exposed* is a dummy variable that is one for age groups that were exposed to legalized abortion *in utero* following *Roe*; the variable, *Post_Roe*Exposed,* is an interaction term for the years after *Roe* among age groups exposed to legalized abortion *in utero*. The next two terms are state and year fixed effects, respectively. The coefficient on the interaction term, β_2, provides an estimate of the DD: the change in crime among the exposed relative to the comparison group before and after exposure to legalized abortion.

I estimate two versions of Equation 3. In each I use only four years of data on arrests and homicides; there are 720 possible state/year/age cells.[19] In the first, I consider 18- and 19-year-olds as the exposed group and 21- and 22-year-olds as the comparison group. Figures 4a and 4b illustrate the experiment. The years 1990 and 1991 are the pre-*Roe* period and the years 1993 to 1994 are the post-*Roe* or exposure period. Eighteen and 19- year-olds in 1990 and 1991 were born primarily between 1971 and 1973 and thus largely unexposed to legalized abortion during pregnancy in the 45 nonrepeal

19. There are 720 cells given 4 years × 4 age groups × 45 states.

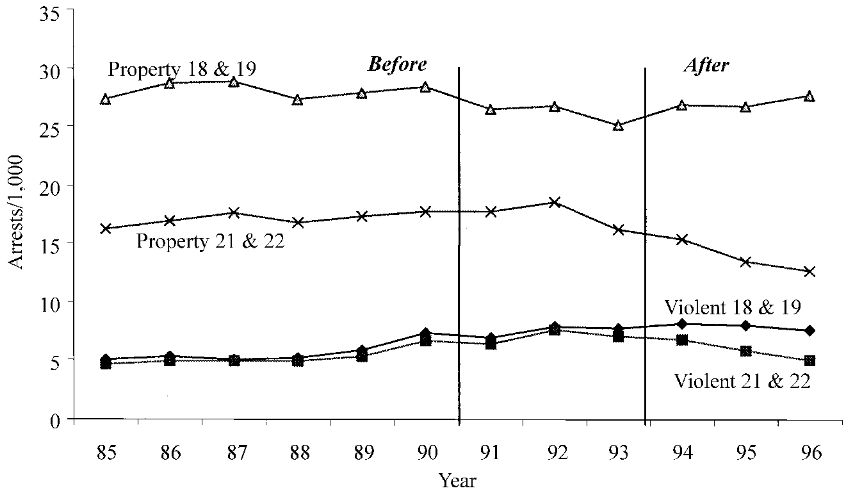

Figure 4a
Violent and Property Crime Arrest Rates for 18- and 19-year-olds vs. 21- and 22-year-olds Before and After Roe in Nonrepeal States

states. By 1993–94 the 18- and 19-year-olds had been born after *Roe* and thus had been exposed to legalized abortion. Twenty-one and 22-year-olds during this period were born primarily before *Roe* and thus unexposed to legalized abortion.[20]

If the legalization of abortion following *Roe* lowered crime, then I would expect to see a drop in arrest and homicide rates among the 18- and 19-year-olds relative to 21- and 22-year-olds from the before to after period. Figures 4a and 4, however, provide no evidence of a cohort effect. Rates of violent crime arrests are practically identical for the two age groups. What is particularly impressive is the similarity in the violent crime arrest rates prior to 1990, which supports the use of 21- and 22-year-olds as a plausible comparison group. The plot for murder and murder arrest rates point to significant period effects as all series begin to rise steeply around 1988 and peak between 1993 and 1994.

The second set of analyses compares changes in arrest and homicide rates among different groups of young adults. In this exercise 20- and 21-year-olds are the exposed group and 23- and 24-year-olds the comparison group. Figure 5 presents a

20. Several other decisions about the design of the analysis merit note. First, I compare pairs of age-groups in order to lessen the problem of small cell size. Second, there is not a direct one-to-one matching between age at arrest and year of birth. A 19-year-old arrested in January of 1991, for example, could have been born in January of 1971 if he were 19 years and 12 months when arrested. Similarly a 19-year-old arrested in December of 1991 could have been born in December of 1972 if he had just turned 19. In other words, there is roughly a two-year window to the birth year, which may contaminate exposure to the law. For instance, the 19-year-old arrested in 1993 could have been born either in 1973, and perhaps unexposed to legalized abortion during pregnancy, or in 1974 and thus exposed. I try to minimize the impact of such contamination by choosing years and age groups such that the pre-*Roe* years include primarily birth years 1971–73 with the greatest concentration in 1972. The same is true of the post-law period.

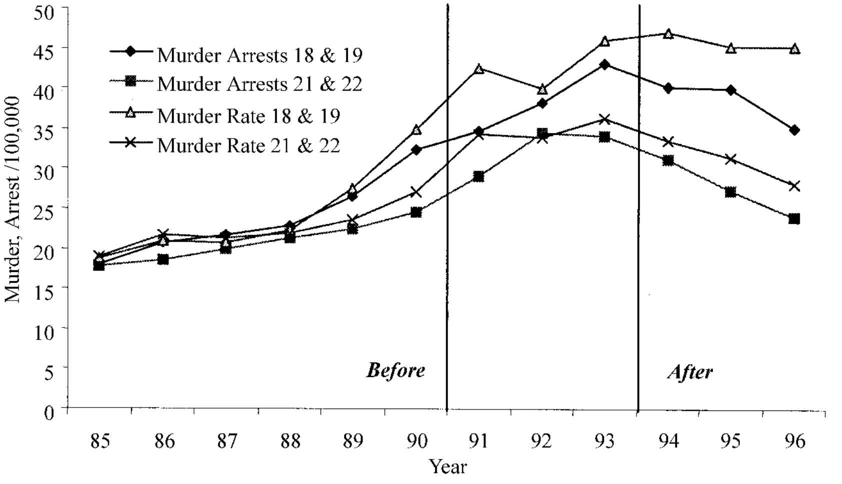

Figure 4b
Murder Arrest and Murder Offending Rates for 18- and 19-year-olds vs. 21- and 22-year-olds Before and After Roe in Nonrepeal States

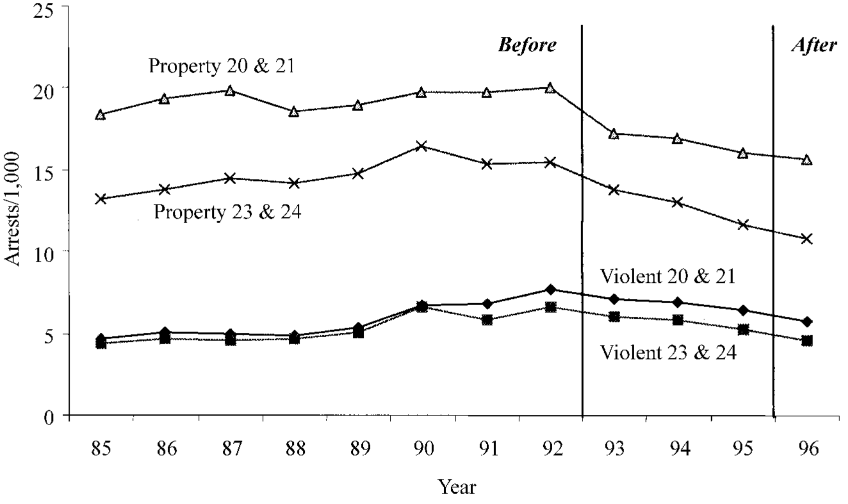

Figure 5a
Violent and Property Crime Arrest Rates for 20- and 21-year-olds vs. 23 and 24-year-olds Before and After Roe in Nonrepeal States

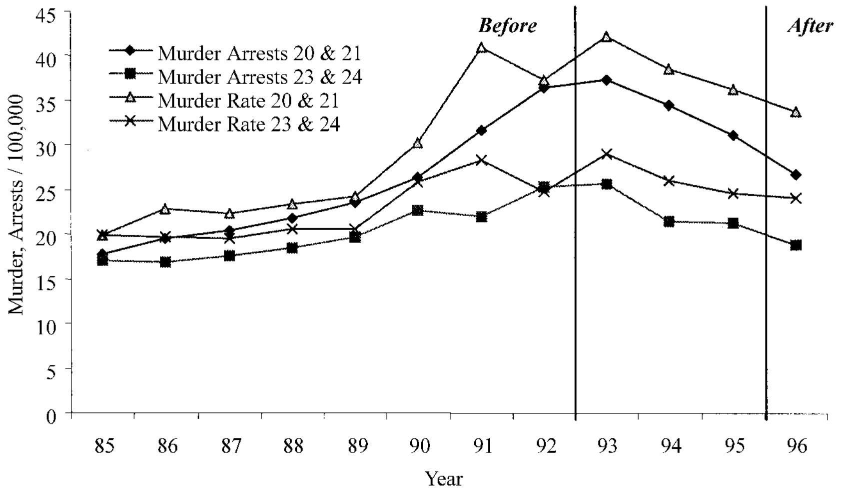

Figure 5b
Murder Arrest and Murder Offending Rates for 20- and 21-year-olds vs. 23-and 24-year-olds Before and After Roe in Nonrepeal States

visual comparison. Because the exposure group includes individuals 20 and 21 years of age, instead of 18 and 19 as in Figure 3, the pre-*Roe* period is now 1992–93 and the post-*Roe* or exposure period is 1995 and 1996. Again, with the exception of property crime, the pre-*Roe* levels and trends in arrest and homicide rates are similar. Moreover, there is little to suggest that arrests or homicide rates fell differentially for 20- and 21-year-olds relative to 23- and 24-year-olds before and after exposure to legalized abortion.

The regression estimates of β_2 in Equation 3 are displayed in Table 5. Given Figures 4 and 5, it is not surprising that I find that exposure to legalized abortion following *Roe v. Wade* has no effect on arrest or homicide rates of the two exposed groups. Consider arrest rates for violent crime in Panel A. The estimated coefficient, 0.064, indicates that violent crime arrests rose 6.4 percent more among teens 18 to 19 years of age relative to 21- and 22-year-olds. The remaining DDs indicate that *Roe* had a statistically insignificant and qualitatively unimportant impact on arrest and homicide rates.

Finally, a criticism of the difference-in-differences strategy is that it fails to exploit the variation in abortion, and by proxy unwanted childbearing, across states and over time. To address this point, I reestimate Equation 3 separately for states with abortion rates above and below the median abortion rate in 1973. Recall that Donohue and Levitt assume that the abortion ratio is zero in all 45 nonrepeal states in 1972. As noted above this assumption is extreme. Nevertheless, the absolute change in resident abortion rates between 1972 and 1973 in nonrepeal states is correlated with the level of the abortion rate in 1973.[21] The weighted mean abortion rate in 1973 for states

21. The unweighted correlation is 0.72 and the correlation weighted by population of women 15 to 44 years of age is 0.28.

Table 5

Difference-in-Difference Estimates of Log Arrest and Homicide Rates among Selected Age Groups Exposed and Unexposed to Legalized Abortion Before and After Roe: Nonrepeal States 1990–96

	Arrest Rate for			
	Violent (1)	Property (2)	Murder (3)	Rates of Homicide (4)
	Panel A: 18 and 19 year-olds vs. 21 and 22 year-olds			
Changes (1994–93) minus (1991–90):	0.064*	0.051^{+}	0.050	0.085
18 and 19 exposed, 21 and 22 comparison group	(0.022)	(0.017)	(0.051)	(0.065)
R-squared	0.908	0.909	0.793	0.747
N	656	656	566	608
	Panel B: 20 and 21 year-olds vs. 23 and 24 year-olds			
Changes (1996–95) minus (1993–92):	0.062*	0.095*	−0.014	−0.006
20 and 21 exposed, 23 and 24 comparison group	(0.018)	(0.018)	(0.056)	(0.056)
R-squared	0.875	0.853	0.740	0.649
N	660	660	576	612

In each panel, the younger age groups went from unexposed to exposed to legalized abortion during pregnancy. In Panel A, for instance, 18- and 19-year-olds in 1991–90 were born between 1971 and 1973 and unexposed to legalized abortion. These same aged teens in 1994–93 were born primarily between 1974 and 1976 and thus exposed. By contrast, 21–22 year-olds in 1994–93 were born primarily between 1971 and 1973 and thus were never exposed to legalized abortion in utero during the entire period. The comparisons in Panel B are structured similarly. There are 720 possible state/age/year cells (45 states × 4 years × 4 age groups). Missing cells are due to nonreporting by states and/or zero crimes. Models include controls for age, year and state. Standard errors (in parentheses) have been adjusted for clustering within state. Regressions are weighted by the state population. $^{+}p < .05$; $^{*}p < .01$.

below the median is 6.6 abortions per 1,000 women 15 to 44 as compared to 16.0 in states above the median. If there is a "dose-response" effect of abortion on crime, then the effect of *Roe v. Wade* on arrest and homicide rates should be greater in absolute value for the states with greater abortion rates in the year immediately following *Roe*. I find no evidence of such an effect. Estimates from the separate estimation of Equation 3 for high and low abortion states differ inconsequentially from the pooled results in Table 5 (results not shown but available upon request).

The significance of the findings in Table 5 is that the DDs pertain to years in which arrest and homicide rates were falling or beginning to fall. Moreover, they pertain only to nonrepeal states and provide a test that is unaffected by the heavily weighted influence of New York and California. Third, the narrower age difference between the exposed and unexposed enhances the credibility of the counterfactual as evidenced by the similarity in the level and trend in the arrest and homicide rates well before legalization. Fourth, they provide compelling evidence that the negative association between abortion and crime between 1991 and 1997 is spurious (Tables 1 and 3), the result of inadequate controls for hard to measure period effects that vary by state.

IV. Conclusion

Donohue and Levitt (2001) present an intriguing association between the growth in abortion and the decrease in crime in the 1990s. The evidence I have presented questions the magnitude of the association and its causal interpretation. The primary difference between Donohue and Levitt's approach and mine is one of identification. We all agree that the impact of crack and its spillover effects had a dramatic influence on crime and that its impact varied by state, year and age. The problem, therefore, is how to identify a cohort effect, such as the legalization of abortion, amidst strong age and period effects. What I have tried to show is that the comparison of changes in crime across states, the essence of the state fixed effects methodology, is flawed because the period effects vary by state and year. Thus, my preferred identification strategy uses a within-state comparison group to difference out changes in crime related to the unmeasured period effects. The weakness of this strategy is that my comparison groups differ in age from the groups exposed to legalized abortion. Since age-crime profiles may differ, the comparison groups may not provide the appropriate counterfactual. To minimize this problem I first used a difference-in-differences-in-differences (DDD) estimate based on a DD within repeal states to eliminate period effects and a second DD in nonrepeal states to net out age-crime differences. However, given the increasing exposure to legalized abortion over time, I could not repeat this strategy during the years of falling crime in the 1990s. I overcame this by focusing only on national legalization following *Roe* and by limiting the analysis to groups closer in age but still with different exposures to legalized abortion. What I found compelling was that the age-crime profiles for these cohorts in the years leading up to *Roe* were remarkably similar (see Figures 4 and 5), which suggests that the older cohorts provided a credible counterfactual.

In closing, however, it would be useful to pull back from issues of measurement and identification and ask more generally why a cohort effect associated with legalized abortion was not more evident in the data. I have two explanations. First, the actual number of unintended births averted, although significant, was an order of magnitude less than the number of reported legal abortions in the early 1970s. Many analysts, including Donohue and Levitt treat reported abortions as an appropriate counterfactual for unintended childbearing. I have questioned this strategy because the availability of legal abortion may figure into decisions regarding sex and contraception, which weakens the link between abortion and fertility. Second, analysts, I being one, have tended to overestimate the selection effects associated with abortion. A careful examination of studies of pregnancy resolution reveals that women who abort are at lower risk of having children with criminal propensities than women of similar age, race and marital status who instead carried to term. For instance, in an early study of teens in Ventura County, California between 1972 and 1974, researchers demonstrated that pregnant teens with better grades, more completed schooling, and not on public assistance were much more likely to abort than their poorer, less academically oriented counterparts (Leibowitz, Eisen, and Chow 1986). Studies based on data from the National Health and Social Life Survey (NHSLS) and the National Longitudinal Survey of Youth (NLSY) make the same point (Michael 2000; Hotz, McElroy, and Sanders 1999). Indeed, Hotz, McElroy, and Sanders (1999) found that teens who abort are similar along observed characteristics to teens that were never pregnant, both of whom differ significantly from pregnant teens that spontaneously abort or carry to term. Nor is favorable selection limited to teens. Unmarried women that abort have more completed schooling and higher AFQT scores than their counterparts that carry the pregnancy to term (Powell-Griner and Trent 1987; Currie, Nixon, and Cole 1995). In sum, legalized abortion has improved the lives of many women by allowing them to avoid an unwanted birth. I found little evidence to suggest, however, that the legalization of abortion had an appreciable effect on the criminality of subsequent cohorts.

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