A SIMPLE TEST OF ABORTION AND CRIME

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Abstract—I first replicate Donohue and Levitt's results for violent and property crime arrest rates. I apply their data and specification to an analysis of age-specific homicide rates and murder arrest rates. The coefficients on the abortion rate have the wrong sign for two of the four measures of crime and none is statistically significant at conventional levels. I then use the legalization of abortion in 1973 to exploit two sources of variation: between-state changes in abortion rates before and after *Roe,* and cross-cohort differences in exposure to legalized abortion. I find no meaningful association between abortion and age-specific crime rates.

I. Introduction

THE debate as to whether legalized abortion lowers crime leaped from academic journals to mainstream discourse with the huge success of *Freakonomics.*[1] In the chapter titled, "Where Have All the Criminals Gone?" Levitt and Dubner summarize academic work by Levitt and coauthor Donohue, which shows that a 1 standard deviation increase in the abortion rate lowers homicide rates by 31% and can explain upwards of 60% of the recent decline in murder.[2] If one accepts these estimates, then legalized abortion has saved more than 51,000 lives between 1991 and 2001, at a total savings of $105 billion. But the policy implications go beyond crime. If abortion lowers homicide rates by 20% to 30%, then it is likely to have affected an entire spectrum of outcomes associated with well-being: infant health, child development, schooling, earnings, and marital status. Similarly, the policy implications are broader than abortion. Other interventions that affect fertility control and that lead to fewer unwanted births—contraception or sexual abstinence—have huge potential payoffs. In short, a causal relationship between legalized abortion and crime has such significant ramifications for social policy and at the same time is so controversial, that further assessment of the identifying assumptions and their robustness to alternative strategies is warranted.

Several researchers have challenged Donohue and Levitt's results and others have voiced skepticism (Joyce 2001, 2004a, 2006; Lott & Whitley, 2007; Foote & Goetz, 2005, 2008; Cook & Laub, 2002; DiNardo, 2007; Dills & Miron, 2006). In replies to Joyce (2004a) and Foote and Goetz (2005, 2008), Donohue and Levitt (2004, 2008) contend that their results stand up even when they address key criticisms of their original work. In this paper, I argue that Donohue and Levitt's efforts to defend their earlier analyses have raised important issues regarding the robustness of their results. To illustrate, I first replicate Donohue and Levitt's most recent estimates that associate abortion with violent and property crime arrest rates. I focus on regressions of crime and arrest rates as opposed to regressions of the level of arrests on the abortion rate. As Foote and Goetz (2008) show, the abortion rate is mechanically related to the level of arrests even in the absence of any cohort-size or selection effect associated with the abortion rate.[3] Based on the replication, I argue that the magnitude of the association between age-specific arrest rates and the abortion rate is small, if appropriately scaled, and I show that the coefficients on the abortion rate are statistically insignificant if corrected for serial correlation. I demonstrate that there is no association between abortion and age-specific homicide rates or age-specific arrest rates for murder. I contend that Donohue and Levitt's attempt to instrument the abortion rate against measurement error is of questionable value because the instrument is likely correlated with the error term by construction. Finally, I argue that the abortion rate is endogenous (Joyce, 2004a; Ananat et al., 2006). States with greater abortion rates are assumed to have lower rates of unwanted births. Yet the availability of legalized abortion affects the decision to have sex, to use contraception, and to carry to term if pregnant. 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.[4]

To address these issues, I use national legalization of abortion after the 1973 decision in *Roe v. Wade* to estimate

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[1] See *Freakonomics: A Rogue Economist Explores the Hidden Side of Everything* by Steven D. Levitt and Stephen J. Dubner (New York: William Morrow, 2005).

[2] 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% (Donohue & Levitt, 2004). Based on the new estimates, homicide was 31.5% lower in 1997 than it would have been in the absence of abortion, up from their original estimate of 22.9%.

[3] Joyce (2006) argues that a specification of log arrests on the abortion rate is difficult to derive and interpret.

[4] Consider, for instance, the experience of U.S. teens over the past decade. Teen abortion rates fell 40% between 1990 and 2000. Teen birth rates, however, *fell* 21% over the same period (Alan Guttmacher Institute, 2004). Demographers estimate that 53% of the decline in teen pregnancies can be attributed to decreased sexual activity and more effective contraception (Santelli et al., 2004). Thus, decreases in state abortion rates may not be associated with increases in unwanted births given simultaneous changes in sexual activity and contraception.

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the reduced-form relationship between abortion and crime.[5] I exploit two sources of variation: between-state changes in abortion rates before and after *Roe* and cross-cohort differences in exposure to legalized abortion. The identification strategy has several attractive features. First, abortion rates increased more from 1971 to 1974 than in any subsequent three-year period in the 45 states that legalized abortion with *Roe*. Moreover, the increases induced by legalization were more plausibly exogenous than were succeeding changes in state abortion rates. Second, slightly older cohorts who were unexposed to legalized abortion *in utero* provide a transparent counterfactual. Finally, *Roe* had a demonstrable effect on fertility and thus, a credible decline in unwanted childbearing. Donohue and Levitt (2004) never demonstrate an inverse relationship between state abortion rates and state fertility rates after 1975 and even argue that such an association is unnecessary.[6] And yet, without an inverse relationship between abortion and birth rates, there can be no cohort-size effect associated with legalized abortion.

I find no consistent relationship between abortion and crime among cohorts born in the years just before and after abortion became legal. Regression analyses confirm what seems apparent from the time series plots: the association between legalized abortion and crime rates is weak and inconsequential. The lack of decline in crime rates associated with cohorts that experience a large and exogenous change in abortion undermines a causal interpretation of Donohue and Levitt's findings.

II. Limitations and Replication of Donohue and Levitt (2008)

A. *Underestimation of the Standard Errors*

Donohue and Levitt (2008) include arrests and homicides by single year of age for 15- to 24-year-olds from 1985 to 1998. There are potentially 7,140 cells (10 ages $\times$ 14 years $\times$ 51 states), and synthetic cohorts range from 1961 to 1983. Abortion rates, however, only vary by state and year beginning in 1973. Consequently, only 510 cells in their analysis have a unique abortion rate and over 60% of the state-age-year cells are assigned an abortion rate of 0. In their estimation of age-specific arrest rates, Donohue and Levitt correct the standard errors for clustering within year of birth and state. This adjusts for the repeated use of the same abortion rate as each cohort ages. However, such clustering does not allow for the most common form of first-order serial correlation.[7] As a result, Donohue and Levitt (2008) underestimate the standard error on the abortion rate.

B. *Estimating the Magnitude of Effects*

Donohue and Levitt's most widely cited estimates are based on regressions of overall crime rates at the state-year level on the effective abortion rate (Donohue & Levitt, 2001, table 4; 2004, table 1). Their preferred estimates indicate that an increase of 100 abortions per 1,000 live births in the effective abortion rate, roughly 1 standard deviation, is associated with a 16.6% decrease in the total homicide rate, a 15.3% fall in rate of violent crime, and 11.1% decline in property crime.[8] However, only half the population of criminals was exposed to legalized abortion by 1997, and thus, the full impact of legalized abortion will not be felt until roughly 2017 (Donohue & Levitt, 2001, p. 415). In other words, the 16.6% decrease in the total homicide rate that is attributable to legalized abortion is driven by criminals roughly 25 years of age or less. Thus, the impact of abortion on the homicide rate of any newly

[5] This paper extends the work of Joyce (2004a) in several key ways. First, I limit the analysis to the 45 states in which abortion became legal following *Roe*. Joyce (2004a) uses legalization in 5 states prior to *Roe* as the source of identifying variation. Second, I have added data on state abortion rates in 1971 and 1972 from the Centers for Disease Control and Prevention. Donohue and Levitt (2001, 2004, 2008) assumed that there were no abortions in these states prior to 1973. Thus, I can correlate changes in the abortion rate before and after legalization with changes in crime. This is not possible with the states that legalized abortion in 1970. Third, I use both a qualitative as well as a continuous measure of abortion. Abortion surveillance systems were new in the early 1970s and may not have recorded many legal and obviously illegal abortions. Thus, an indicator of abortion legalization is arguably more exogenous and less vulnerable to measurement error than state abortion rates. Fourth, national legalization is less contaminated by policy endogeneity at the state level, if the decision to legalize abortion was in response to the high rate of illegal abortion and unwanted childbearing. Lastly, I incorporate Donohue and Levitt's suggestion that analysts examine changes in crime over a longer period of a cohort's life in order to minimize the effect of period shocks such as the crack cocaine epidemic.

[6] "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 & Levitt, 2004, p. 33). In other words, abortion may improve the timing of births. This is a possible mechanism although the association between mistimed births and adverse outcomes is weak (see Joyce, Kaestner, & Korenman, 2000). More importantly, if there is no cohort-size effect, then the more appropriate specification is a regression of age-specific crime rates, and not age-specific crimes, on the abortion rate.

[7] Donohue and Levitt (2008) cluster the residuals by state and year of birth in order to allow for correlations across age groups within the same cohort and state. For instance, the residuals for state *j* associated with the arrest rate of 15-year-olds in 1991, the arrest rate of 16-year-olds in 1992, the arrest rate of 17-year-olds in 1993, and so on, are all associated with the abortion rate in 1973 in state *j,* since they were all born in roughly 1974. However, clustering by state and year of birth does not allow for even first-order serial correlation between, say, the residual associated with the arrest rate of 15-year-olds in 1991 with the residual of the arrest rate of 15-year-olds in 1990 within the same state. Econometricians have argued that researchers should allow for clustering by state and not by state and year, which permits a more general pattern of correlations across cohorts and age groups over time. They also show that if the right-side variable of interest is positively serially correlated, then the unadjusted standard error will underestimate the true standard error (see Bertrand, Duflo, & Mullainathan, 2004). The abortion rate is positively serially correlated. It takes on a value of 0 for cohorts born between 1961 and 1973 and then rises thereafter for cohorts born between 1974 and 1983.

[8] See table 1, column 2, top panel of Donohue and Levitt (2004). These are their preferred estimates because they use abortions by state of residence and not state of occurrence, as in the original paper, to construct the effective abortion.

TABLE 1.—ESTIMATED EFFECTS OF ABORTION RATE ON AGE-SPECIFIC ARREST AND HOMICIDE RATES (IN LOGS): REPLICATION OF DONOHUE AND LEVITT (2008)

	(1)	(2)	(3)
Ln violent crime arrest rate	−0.021*	−0.021	−0.039*
	(0.008)	(0.013)	(0.019)
Effect of 1 SD Δ	[−0.035]	[−0.035]	[−0.046]
Ln property crime arrest rate	0.001	0.001	0.005
	(0.005)	(0.008)	(0.012)
Effect of 1 SD Δ	[0.002]	[0.002]	[0.006]
Ln murder arrest rate	−0.004	−0.004	0.003
	(0.016)	(0.023)	(0.078)
Effect of 1 SD Δ	[−0.007]	[−0.007]	[0.004]
Ln homicide rate	0.006	0.006	0.048
	(0.017)	(0.018)	(0.066)
Effect of 1 SD Δ	[0.010]	[0.010]	[0.057]
Cluster by state	No	Yes	Yes
Cohort	1961–83	1961–83	1974–81

Figures in column 1 replicate results from Donohue and Levitt (2008, table II) for violent and property crime arrest rates. The results for murder arrests and homicide rates use the same specification. The unit of observation is the state-year-age cell. The sample in columns 1 and 2 covers the years 1985–1998, ages 15–24, and cohorts 1961–83. There are 6,724 observations for violent crime arrests, 6,730 for property crime arrests, 5,715 for murder arrests, and 5,851 for the homicide rate. The sample in column 3 is limited to cohorts for which abortion data exists in Donohue and Levitt (2008). Standard errors in parentheses are adjusted for clustering within state and year of birth in column 1, but are clustered by state in columns 2 and 3. The weighted mean and standard deviation of the abortion ratio for the samples in columns 1 and 2 are 1.35 and 1.65, respectively, and 3.02 and 1.19 for the samples in column 3. $*p < .05$

exposed cohort is, on average, 33.2% (16.6/0.50). Donohue and Levitt confirm this expectation when they introduce the analysis of age-specific arrest rates. They write, "On average, about half of those arrested are under the age of 25. Thus, to generate the crime reduction in table IV (*the 16.6% above*) requires coefficients on young arrests that are twice as large as the coefficients on overall crime" (Donohue & Levitt, 2001, p. 410; author's insertion in italics).[9] Cohort-specific effects this large should be evident in the time series of age-specific crime and arrest rates even during years of rapidly rising crime.

C. *Correcting for Measurement Error in Abortion*

There are two sources of state-level data on abortion. The Centers for Disease Control and Prevention (CDC) compiles data on abortion as recorded by state health agencies. In most states, abortion providers are required by law to report induced terminations as part of the vital registration system. In six states reporting to the local health agency is voluntary and in three other states the requirement to report is not a state statute but a regulation promulgated by the state health agency (Saul, 1998). The Alan Guttmacher Institute (AGI) is the other source of data on abortion. The AGI researchers survey abortion providers periodically as to the number of abortions performed at the facility. The number of abortions as measured by AGI exceeds estimates from the CDC by about 15%, but differences vary widely by state. Importantly, both the CDC and AGI measure abortions by state of occurrence. Researchers at AGI then *estimate* abortions by state of residence based on the proportion of abortions in a state to nonresidents as reported by the CDC. In the second step, AGI researchers use data from individual state health departments when available to distribute the total number of nonresident abortions in the state of occurrence to a particular state of residence (Henshaw & Van Vort, 1992). In other words, there are several sources of possible error in AGI's estimates of resident abortion rates that are linked to the CDC reporting errors in potentially complicated ways.[10]

Donohue and Levitt (2001) used abortions by state of occurrence as collected by AGI in their original analysis. In subsequent work they use AGI's estimates of abortions by state of residence (Donohue & Levitt, 2004). However, in the most recent analysis they use abortions by state of occurrence as published by the CDC as an instrument for AGI's abortions by state of residence (Donohue & Levitt, 2008). The identifying assumption is that the measurement errors in the two abortion series are uncorrelated. But this is questionable given that the AGI estimate of abortions by state of residence relies on CDC data for its construction. Indeed, instrumenting the resident abortion rate by the occurrence rate may put back the error that the resident rate was estimated to fix.[11] In short, the instrumented estimates of abortion on crime in Donohue and Levitt (2008) are based on dubious identifying restrictions.

D. *Replication*

I replicate the results for violent and property crime arrest rates from Donohue and Levitt (2008, table II) in column 1 of table 1. I also show results for age-specific murder arrest rates and age-specific homicide rates from the same specification. The estimates in column 2 are from the identical model as in column 1 but the standard errors are adjusted for clustering within state. In column 3 I limit the sample to cohorts born between 1974 and 1981. These are the only cohorts for which Donohue and Levitt have actual data on

[9] In that same paragraph Donohue and Levitt (2001, p. 415) make the prediction that as each new cohort is exposed to legalized abortion, the total homicide rate would be expected to fall 1% per year. Assume all homicides are committed by those 15 to 50 years of age. If each new cohort exposed to legalized abortion reduced the total homicide rate by 1%, then the homicide rate of this newly exposed cohort fell by 35% (all else constant), since they are only 1/35th of total perpetrators.

[10] For instance, the CDC may under- or overestimate the proportion of abortions to nonresidents in state A. There is also an error in how AGI researchers assign nonresident abortions that occur in state A to individual states B, C, D, and so on, based only on the proportion of all nonresident abortions obtained in state A.

[11] To see this, consider the following: Let Rj = abortions to residents of state j regardless of the state in which the abortions occur. But $Rj = Rjj + Rjk$ where Rjj = abortions to residents of state j that occur in state j and Rjk = abortions to residents of state j that occur in state k. Similarly, let Oj = abortions that occur in state j regardless of the state in which women reside. Note that $Oj = Ojj + Okj$ where Ojj = abortions to women that reside in state j and which occur in state j and Okj = abortions to women that reside in state k but that occur in state j. Note also that $Ojj = Rjj$. The first stage of Donohue and Levitt's instrumental variables correction is as follows (the error terms are suppressed): $Rj = \beta_0 + \beta_1 Oj$, which can be rewritten as $Rjj + Rjk = \beta_0 + \beta_1 (Ojj + Okj)$.

Thus, the correlation between Rj and Oj is driven primarily by Rjj and Ojj. In words, the predicted Rj measures variation in Rj that overlaps with Oj, which is simply Rjj. But the reason one uses Rj instead of Oj is to capture abortions Rjk. But this variation may be lost by instrumenting. One ends up with a "resident-occurrence" rate, which largely defeats the purpose of using Rj to begin with.

abortion. Donohue and Levitt (2004) argue that the assumption of a zero abortion rate for the 1961–1972 cohorts should bias their estimates toward the null. If correct, then the estimates in column 3 should provide the strongest association between abortion and crime.

Several results in table 1 are noteworthy. First, only the abortion coefficient associated with violent crime arrest rates has the correct sign and is statistically significant (column 1). However, when I adjust the standard errors for a general form of serial correlation within states the estimate is no longer significant. Moreover, the effect of a 1 standard deviation increase in the abortion rate is modest, reducing arrest rates by only 3.5%. This estimate represents 25% of the effect obtained from the regression of total violent crime on the effective abortion rate (Donohue & Levitt, 2004, table 1), a result strongly counter to Donohue and Levitt's expectation. If I limit the sample to cohorts born between 1974 and 1981, the effect of a 1 standard deviation increase in the abortion rate rises to −4.6%. There is no association between the abortion rate and the other three measures of crime. Seven of the nine coefficients have the wrong sign. Even when I limit the sample to years in which Donohue and Levitt have abortion data, there is no evidence of an association (column 3).

The other point is that the standard errors of the coefficients in column 3 are more than 2.5 times greater than those in column 1. Donohue and Levitt include thousands of observations over a period in which the abortion rate is assumed to be 0 and thus provide little information regarding its association with crime. The standard errors in column 3 arguably provide a more realistic estimate of the precision with which the association between the abortion rate and age-specific crime rates can be estimated.

Finally, the lack of an association between abortion rates and age-specific homicide rates is at odds with the robust association between total homicide rates and the effective abortion rate as reported in Donohue and Levitt (2001, 2004). Not only is homicide relatively well measured, but it's the only direct measure of crime that is age-specific. Age-specific arrest rates for violent crime and property crime conflate variation in both crime and policing. Estimates from these regressions may be less comparable to regressions of total violent and property crime rates on the effective abortion rate.

In the next section, I offer a test of abortion and crime that tries to mitigate problems associated with the endogeneity of abortion and measurement error. I focus on the period just before and after *Roe* in order to exploit a plausibly exogenous source of variation in the price of abortion. I also add data on state abortion rates for the two years prior to *Roe* in order to improve the measurement of abortion.

III. An Empirical Model

I estimate two models. The first uses a difference-in-difference-in-differences (DDD) estimator. I compare changes in crime rates among cohorts exposed versus cohorts unexposed to legalized abortion in states with above- and below-median changes in the abortion rate between 1971 and 1974. This specification is similar to the reduced-form models used by Angrist and Evans (1999) and Gruber, Levine, and Stagier (1999) to analyze the effect of abortion legalization on fertility and child well-being. The second model follows Donohue and Levitt (2004, 2008) in which arrests and homicides rates by single year of age are regressed on lagged abortion rates. However, I limit the sample to cohorts born between 1972 and 1975, unlike Donohue and Levitt who use cohorts from 1961 to 1983. As I show, below, both models yield similar conclusions.

There is substantial variation in abortion rates across states and over time in the period just before and after *Roe*. In table A1 of the appendix I display abortion rates in 1971 and 1974 in the 45 states that legalized abortion with *Roe*. The average increase (weighted) for all 45 states is 152 abortions per 1,000 live births.[12] However, abortion rates increased by only 74 abortions per 1,000 live births for the 23 states at or below the median and by 188 abortions per 1,000 live births in states above the median. In addition to differences in changes across states, I also exploit differences in exposure to legalized abortion across cohorts. Table 2 illustrates the DDD for the case of 15- and 17-year-olds. The crime rate of 15-year-olds in 1987 pertains to cohorts born approximately in 1972, most of whom were *in utero* in 1971. The crime rate of 17-year-olds in 1987 pertains to cohorts born in 1970, most of whom were *in utero* in 1969. By 1989, 15-year-olds have been exposed to legalized abortion *in utero* whereas 17-year-olds have not. Thus, the difference-in-differences (DD_{am}) for states with above-median changes in abortion [(A − B) − (C − D) in table 2] captures the effect of exposure to legalized abortion net of ongoing trends in crime in these states that affect both groups of teens. However, crime varies by age, and thus, I use the DD in states with below-median change in abortion rates (DD_{bm}) to net out differences in crime rates due to age. If Donohue and Levitt are correct, then crime rates should fall more in states with substantially larger changes in abortion induced by legalization.

The relevant regression is as follows:

$$\begin{aligned} LnC_{ajt} = {} & \beta_0 + \beta_1 Exposed \\ & + \beta_2(Exposed \times Above\_med) \\ & + \beta_3(Exposed \times After) \\ & + \beta_4(Above\_med \times After) \\ & + \beta_5(Exposed \times Above\_med \times After) \\ & + \sum_t \tau_t + \sum_j \lambda_j + e_{ajt}. \end{aligned} \tag{1}$$

[12] This is the largest three-year increase in abortion rates post-legalization. Abortion rates in these 45 states increased by 78 per 1,000 live births from 1975 to 1978 and by 3 abortions per 1,000 live births between 1979 and 1982.

TABLE 2.—ILLUSTRATION OF THE DDD ESTIMATE

States with Abortion Rate Changes Above the Median			
Age (year of birth)	Crime Year 1987	Age (year of birth)	Crime Year 1989
15 (1972)	A	15 (1974)	B
17 (1970)	C	17 (1972)	D
		DD_{am}	(A − B) − (C − D)
States with Abortion Rate Changes At or Below the Median			
Age (year of birth)	Crime Year 1987	Age (year of birth)	Crime Year 1989
15 (1972)	E	15 (1974)	F
17 (1970)	G	17 (1972)	H
		DD_{bm}	(E − F) − (G − H)
		DDD	$DD_{am} - DD_{bm}$

The DDD exploits two sources of variation: between-state differences in the change in abortion rates from before to after *Roe* and cross-cohort differences in exposure to legalized abortion while *in utero*. The letters A–H represent the crime rates for specific age, cohort, and state groupings. The abortion rate increased by 188 per 1,000 live births between 1971 and 1974 in the above-median states and by 74 abortions per 1,000 live births in the states at or below median (see appendix table A1). I treat the former as the "treatment" states and the latter as the comparison states. In the example above, 15-year-olds born in 1972 and 1974 are the exposed group since they experience a change in legal status of abortion while *in utero;* 17-year-olds born in 1970 and 1972 are unexposed to legalized abortion but experience the same period effects as the 15-year-olds. They serve as the comparison group.

The dependent variable, LnC_{ajt}, is the natural logarithm of the crime rate of age group *a,* in state *j* and year *t*. *Exposed* is an indicator of age groups who were born between 1972 and 1975; these groups go from being unexposed to being exposed to legalized abortion *in utero*. The omitted category is the slightly older age groups who were born between 1970 and 1973 and thus, were unexposed to legalized abortion *in utero*. *After* refers to the crime years that map to the birth years 1974–1975 for the cohorts who were exposed to legalized abortion, but to the birth years 1972–1973 for the older cohorts.[13] *Above_med* are states in which the change in the abortion rate between 1971 and 1974 was above the median. The model includes state and year fixed effects. The coefficient β_5 estimates the difference-in-differences-in-differences (DDD).

Donohue and Levitt (2004) argue that a comparison of changes in crime rates among cohorts before and after legalization neglects important variation in abortion rates.[14] Thus, in the next set of regressions I relate crime rates to lagged abortion rates following Donohue and Levitt (2001, 2004, 2008). However, I limit the sample to cohorts born between 1972 and 1975 in the 45 states that legalized abortion with *Roe*. The benefit of analyzing only the states that legalized abortion with *Roe* is that there exist data on abortion before and after the Supreme Court decision.

The advantage of a quasi-experimental design is that the change in the abortion rate during this period is large and more plausibly exogenous than changes in later years (see Joyce, 2006; Ananat et al., 2006). Moreover, a difference-in-differences strategy based on abortion legalization provides a transparent check as to the plausibility of the identifying assumptions. Cohorts exposed and unexposed to legalized abortion should have similar levels and trends in crime prior to the point at which the younger cohorts are exposed to legalized abortion (Meyer, 1995). Moreover, cohort effects present in a specific manner and should be noticeably distinct from period effects (O'Malley, Bachman, & Johnston, 1984). Finally, abortion surveillance systems were newly developed in the early 1970s and many abortions, both legal and illegal, may not have been counted. Thus, the DDD analysis is less vulnerable to measurement error in the abortion rate.

A. Data

I use the same data on age-specific arrest and homicide rates as do Donohue and Levitt (2001, 2004, 2008). Arrests are from the FBI's Uniform Crime Reports and are used extensively by criminologists. They are available by state, year, and single year of age for persons 15 to 24 years of age. The second source of data consists of homicide offenders from the FBI's Supplemental Homicide Reports (SHR). These are also available by state, year, and single year of age; unlike the data on arrests, however, all ages are identified. The SHR account for approximately 90% of all known homicides, and just under 4% of cases lack information on age (Fox & Zawitz, 2004).[15]

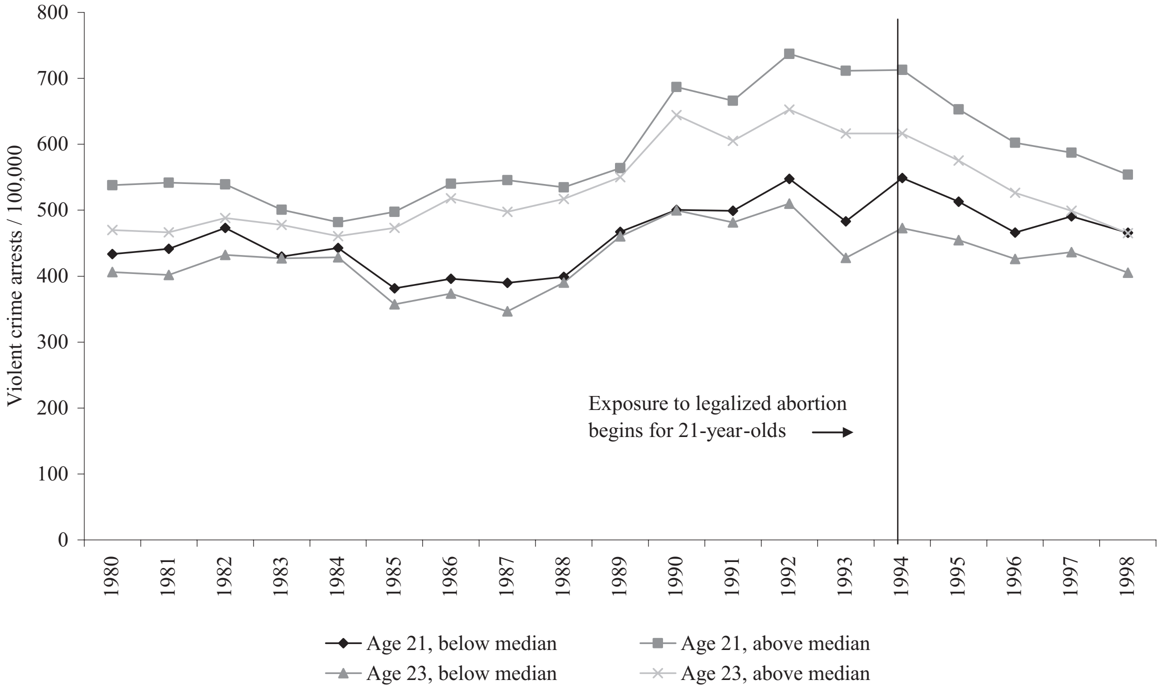
[13] Consider the homicide rates of 15- and 17-year-olds in the years 1987–1990 in the 45 states ($n = 2 \times 4 \times 45 = 360$). The variable, *After,* equals 1 in 1989 and 1990. For 15-year-olds 1989–1990 refers to the birth years 1974–1975, but for 17-year-olds, 1989–1990 refers to birth years 1972 and 1973. I assume those born in 1973 were mostly *in utero* in 1972 and thus unexposed to legalized abortion.

[14] "Our original hypothesis, however, was based on a view that the mere act of abortion legalization is not sufficient to equalize the costs (financial, social, and psychological) of abortion across time and place. Rather, our model argues that abortion rates as a fraction of live births are a better proxy for the impact of legal abortion than is the dichotomous indicator of legal status" (Donohue & Levitt, 2004, pp. 46–47).

[15] Fox and Zawitz (2004) create weights to impute missing homicides by state, but I use the nonimputed homicide series in these analyses.

[16] Lott and Whitley (2007) also use abortion data from the CDC. They emphasize results based on abortions by state of occurrence and I use abortions by state of residence. Nineteen seventy-one was the first year in which the CDC published abortions by state of residence for all 45 states in my sample.

FIGURE 1.—VIOLENT CRIME ARREST RATES AMONG 21- AND 23-YEAR-OLDS IN 45 STATES THAT LEGALIZED ABORTION AFTER *ROE*, STRATIFIED BY STATES WITH ABOVE- AND BELOW-MEDIAN INCREASES IN THE ABORTION RATE, 1971–1974



I use abortions by state of residence as estimated by the Alan Guttmacher Institute from 1973 to 1975. I augment these data with the state abortions in 1971 and 1972 as collected by the CDC. The CDC initiated its abortion surveillance in 1969.[16] However, 1971 is the first year in which there were annual data from all 50 states and the District of Columbia by state of residence. According to the CDC there were 589,494 reported legal abortions in 1971 and 1972 in the 45 states that legalized abortion with *Roe*. Donohue and Levitt (2001, 2004, 2008) did not use the CDC data; they assumed there were no abortions in 1971 and 1972 in these states. Addition of these abortions to the analysis may lessen the bias from measurement error.[17]

IV. Results

A. *Time Series Evidence*

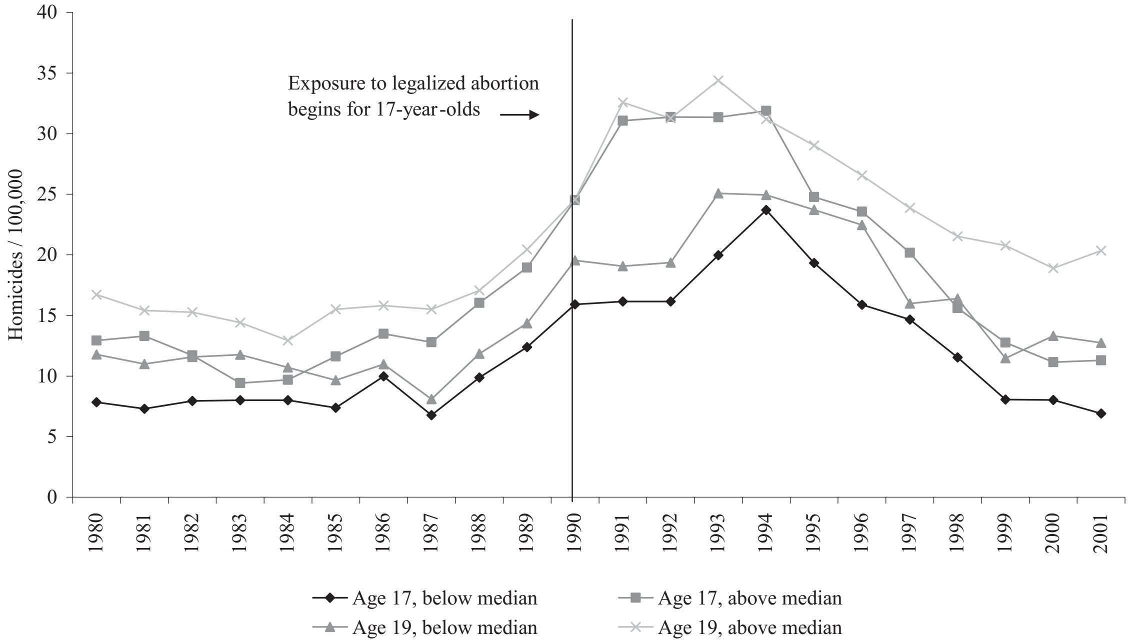
Homicide rates and arrest rates for three categories of crime are shown in figures 1–5. The series are stratified by states in which the change in the abortion rate between 1971 and 1974 was above the median versus at or below the median (see appendix table A1). In each figure, the vertical line is the first year after which exposure (*in utero*) to legalized abortion begins for the younger age group. If there is a strong cohort effect associated with legalized abortion, then we should observe a moderation or decline in the crime rates of the younger age groups in states with above-median changes in abortion relative to the crime rates of both the older age groups in the same states as well as the younger age groups in the below-median states.[18]

Figure 1 shows violent crime arrest rates of 21- and 23-year-olds in the 45 states that legalized abortion following *Roe*. There is a noticeable decline in violent crime arrests among 21-year-olds starting in 1994 in states in which the change in abortion rates was above the median between 1971 and 1974 and relatively little change in states with more modest growth in abortion rates (at or below the median). The magnitude of the decline and its timing are consistent with Donohue and Levitt's hypothesis. However, there is a similar decline in arrest rates among 23-year-olds in the above-median states two years before this older cohort is exposed to legalized abortion. The coincident decline in the two series is more consistent with period as opposed to cohort effects. Figure 2 displays homicide rates for 17- and 19-year-olds also stratified by states with above- and below-median changes in abortion. Again, there is little

[17] Using data on abortions in the period prior to *Roe* will eliminate the attenuation bias that results from assuming the abortions are 0 if the CDC's count of abortion is equal to the true count of abortion. If the CDC estimates are measured with error, as is likely, then whether they will lessen the attenuation bias depends on the ratio of the variance of the error to variance of the true number of abortions in the post-*Roe* period. This result is available upon request.

[18] Cohort effects present distinctly from period effects. As Donohue and Levitt note, "If abortion legalization reduces crime, then we should see the reduction begin with, say, fifteen-year-olds, about sixteen years after legalization, then extend to sixteen-year-olds a year later, and so on" (Donohue & Levitt, 2001, p. 411). Evidence of period effects would be the coincident rise, peak, and fall in crime rates of different cohorts.

FIGURE 2.—HOMICIDE RATES AMONG 17- AND 19-YEAR-OLDS IN 45 STATES THAT LEGALIZED ABORTION AFTER *ROE*, STRATIFIED BY STATES WITH ABOVE- AND BELOW-MEDIAN INCREASES IN THE ABORTION RATE, 1971–1974



evidence of a cohort effect. The homicide rate of 17-year-olds in the above-median states continues to rise after exposure to legalized abortion, as does the homicide rate of 19-year-olds. Indeed, the coincident peak in homicide rates in all four series is again more consistent with strong period effects than with any meaningful cohort effect.

The rise in homicide rates of teens in the late 1980s and early 1990s has been associated with the spread of crack cocaine (Blumstein, Rivara, & Rosenfeld, 2000). Such strong period effects may obscure the effect of legalized abortion, although cohort-specific decreases of 15% to 30% should be evident. As an alternative, figure 3 makes the same comparison between 24- and 26-year-olds, age groups who appear much less affected by the spread of crack. There is still little evidence of a break among 24-year-olds beginning in 1997. Figure 4 and 5 show time series of murder and property crime arrest rates. Again, there is little evidence of any substantive cohort effect.

FIGURE 3.—HOMICIDE RATES AMONG 24- AND 26-YEAR-OLDS IN 45 STATES THAT LEGALIZED ABORTION AFTER *ROE*, STRATIFIED BY STATES WITH ABOVE- AND BELOW-MEDIAN INCREASES IN THE ABORTION RATE, 1971–1974

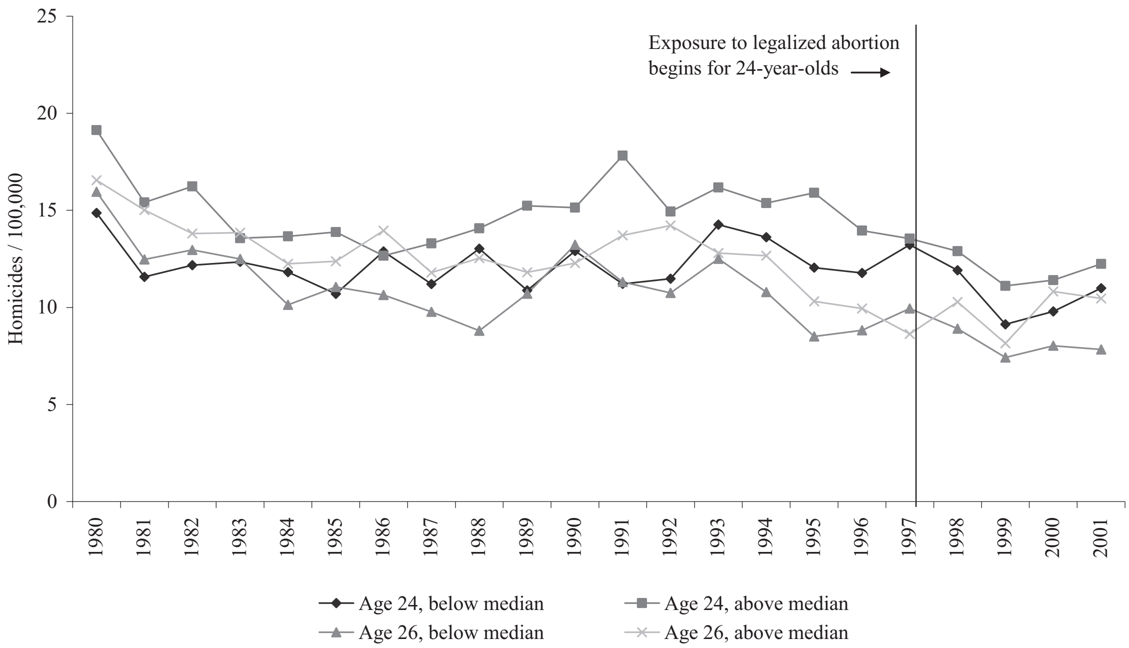


FIGURE 4.—MURDER ARREST RATES AMONG 18- AND 20-YEAR-OLDS IN 45 STATES THAT LEGALIZED ABORTION AFTER *ROE,* STRATIFIED BY STATES WITH ABOVE- AND BELOW-MEDIAN INCREASES IN THE ABORTION RATE, 1971–1974

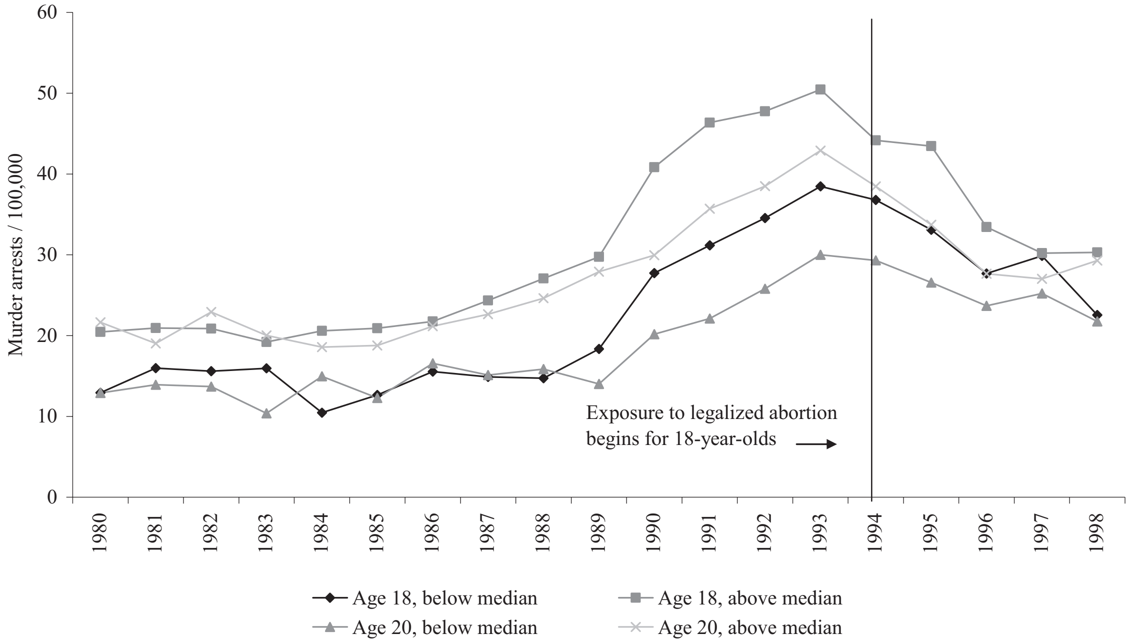
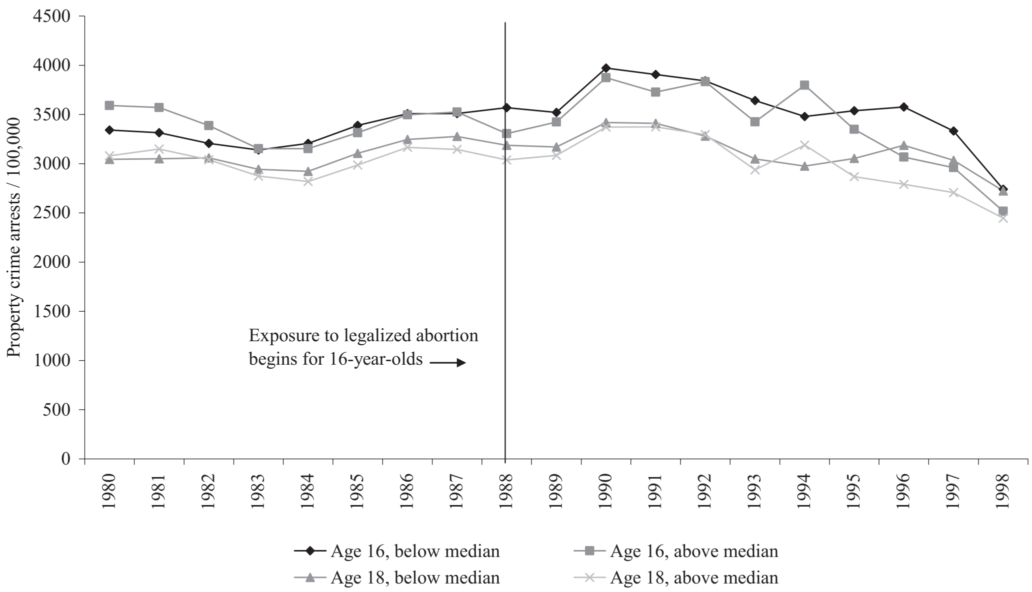


FIGURE 5.—PROPERTY CRIME ARREST RATES AMONG 16- AND 18-YEAR-OLDS IN 45 STATES THAT LEGALIZED ABORTION AFTER *ROE,* STRATIFIED BY STATES WITH ABOVE- AND BELOW-MEDIAN INCREASES IN THE ABORTION RATE, 1971–1974



B. *Regression Results*

Regression results from the estimation of equation (1) are shown in table 3. There are four outcomes: homicide rates as measured by the Supplemental Homicide Reports (SHR) and then arrest rates for murder, violent crime, and property crime. I display only estimates of the DDDs (β_5 in equation [1]), and each estimate is from a separate regression. The bottom row is the average of the DDDs for the 1972–1975 cohorts between the ages of 13 and 26.[19] I find no inverse association between the legalization of abortion and crime. Only seven of the nineteen estimates are negative and none is statistically significant. The average of the DDDs within

[19] Donohue and Levitt argue that one should sum the coefficients in a DDD analysis in order to capture the effect of crime "over the entire lives of the cohorts in question, not just one well chosen point in time when these cohorts were in their teens" (Donohue & Levitt, 2004, p. 42).

TABLE 3.—DDD ESTIMATES OF CHANGES IN LOG HOMICIDE RATES AND LOG ARREST RATES IN 45 STATES FOLLOWING *ROE V. WADE* FOR BIRTH COHORTS 1972–1975

	Age Groups			Arrest Rates		
Years *(1)*	Exposed *(2)*	Unexposed *(3)*	Homicide Rate *(4)*	Murder *(5)*	Violent *(6)*	Property *(7)*
85–89	13–14	15–16	0.284 (0.198)	— —	— —	— —
87–91	15–16	17–18	−0.234 (0.148)	−0.093 (0.199)	−0.040 (0.068)	0.010 (0.040)
89–93	17–18	19–20	0.181 (0.105)	0.037 (0.099)	−0.003 (0.046)	−0.019 (0.028)
91–95	19–20	21–22	−0.021 (0.095)	0.122 (0.081)	0.044 (0.032)	0.007 (0.026)
93–97	21–22	23–24	−0.269 (0.121)*	0.177 (0.141)	0.022 (0.030)	0.017 (0.035)
95–99	23–24	25–26	−0.073 (0.159)	— —	— —	— —
97–01	25–26	27–28	0.218 (0.163)	— —	— —	— —
Simple average:			0.089	0.061	0.006	0.004
Weighted average:			0.082	0.088	0.021	0.001

Each DDD estimate is from a separate regression (β_5 from equation [1] in the text). Standard errors are in parentheses. They have been adjusted for clustering within the state. The sample is limited to cohorts born in 1972–1975 in the 45 states that legalized abortion with *Roe*. Arrests include the years 1987–1998 and homicides, the years 1985–2001. Each DDD estimate has at most 360 observations (45 states × 4 age groups × 2 years). The younger age group in each row refers to cohorts that moved from unexposed to exposed to legalized abortion *in utero*. The older age group in each row is cohorts who were *in utero* before *Roe*. Thus, in row 1, the change in homicide rates of 13-year-olds in 1985 and 1987 and 14-year-olds in 1986 and 1988 are contrasted with the homicide rates of 15- and 16-year-olds over the same years. The weighted average of the coefficients is weighted by the inverse of their variances. $^*p < .05$; $^{**}p < .01$

TABLE 4.—ESTIMATE FROM REGRESSIONS OF CRIME AND ARREST RATES ON ABORTION RATES IN THE 45 STATES THAT LEGALIZED ABORTION AFTER *ROE* FOR BIRTH COHORTS 1972–1975*

	Homicide Rate		Arrest Rates: Murder		Arrest Rates: Violent Crime		Arrest Rates: Property Crime	
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
Abortion rate	0.015	0.033	0.019	0.023	−0.029	0.040	−0.022	−0.004
	(0.029)	(0.030)	(0.032)	(0.029)	(0.027)	(0.018)*	(0.025)	(0.017)
Effect of 1 SD Δ	[0.016]	[0.035]	[0.020]	[0.025]	[−0.031]	[0.043]	[−0.024]	[−0.004]
Abortion rate adjusted¶	0.039	0.064	0.038	0.017	−0.062	0.042	−0.037	0.000
	(0.051)	(0.052)	(0.052)	(0.052)	(0.048)	(0.039)	(0.045)	(0.028)
Effect of 1 SD Δ	[0.034]	[0.056]	[0.033]	[0.015]	[−0.054]	[0.037]	[−0.032]	[0.000]
State trends	No	Yes	No	Yes	No	Yes	No	Yes
N	2,009		1,355		1,618		1,621	

Figures pertain to the coefficient on the abortion rate. Each coefficient is from a separate regression. The sample for homicides includes cohorts 1972–75, crime years 1985–01, and ages 13–27. The sample for arrests includes cohorts 1972–75, crime years 1987–98, and ages 15–24. The abortion rate is the number of resident abortions per 1,000 live births and then divided by 100. The weighted mean and standard deviation of the abortion rate unadjusted for mother's place of birth is 1.529 and 1.071, respectively. The abortion rates pertain to the years 1971–74; thus, crime rates in year t are regressed on the lagged abortion rate (AR) weighted as follows: $0.125 \times AR(t\text{-}a) + 0.75 \times AR(t\text{-}a\text{-}1) + 0.125 \times AR(t\text{-}a\text{-}2)$ where a is age in year t (see Donohue & Levitt, 2008). Thus, the homicide rate of 17-year-olds in 1990 is regressed on $125 \times AR_{1973} + 0.75 \times AR_{1972} + 0.125 \times AR_{1971}$.

¶The adjusted abortion rate attempts to approximate the abortion rate by mother's state of birth. I create a 51 × 51 weight matrix in which each element (a_{ij}) is the proportion of homicide victims that reside in state i but who were born in state j. I use all homicide victims ages 15 to 29 from the national mortality files for the year 1985–98 to create the weights. I then pre-multiply the resident abortion rate in each state and year by the weight matrix. The weighted mean and standard deviation of the adjusted abortion rate is 1.496 and 0.875, respectively.

each crime category is positive. For instance, homicide rates increased 8.9% more among the 1972–1975 cohorts relative to the 1970–1973 cohorts in states with relatively large changes in abortion induced by legalization, a result clearly at odds with Donohue and Levitt (2004).

Although the specification in equation (1) exploits the natural experiment afforded by *Roe,* it does not take advantage fully of the state-by-state variation in abortion rates from the period immediately before and after legalization. Indeed, Donohue and Levitt (2004) consider the abortion rate a better proxy for state-by-state variation in unwanted childbearing than the dichotomous indicator of legalization. Thus, in the next set of regressions I regress age-specific arrests and homicide rates on lagged abortion rates. I display the coefficients on the abortion rate in table 4. For each outcome, I show estimates first without and then with state linear and quadratic trend terms. In the top panel, I use the resident abortion rate as reported by the CDC and AGI. In the bottom panel, I use a resident abortion rate that I have reweighted to approximate the state in which the mother conceived.[20] For homicides and murder arrest rates, the coefficient on the abortion rate is positive but never statistically significant. The coefficients on violent and property crime arrest rates are negative in five out of eight specifications, but relatively small in magnitude and never statistically significant. For instance, a 1 standard deviation increase in the abortion rate is associated with a decline of 2.9% or 5.4% in violent crime arrest rates depending on whether the abortion rate is adjusted for cross-state migration (table 3, column 5). These estimates are very close to those obtained by Donohue and Levitt as shown in table 1. Neither change is statistically significant. Inclusion of state-specific trends changes the association from negative to positive (column 6). Results for property crime arrest rates are similar.

The lack of an association between abortion rates and homicide rates is particularly damaging to the abortion and crime hypothesis. As noted above, homicide is a measure of crime whereas arrests measure both crime and policing. In

[20] See Joyce (2004b, pp. 14–15) for a description of the weighting procedure.

addition, homicide is well reported, although the SHR underreports homicide offending rates by age. Thus, it is also reassuring that homicide rates and murder arrest rates yield the same result. One final concern with age-specific homicide rates is the small number of murders or murder arrests in some age-state-year cells. Observations with zero events are dropped in a logarithmic specification. This occurs primarily in the less-populated states and among the youngest age groups. Regressions, however, are weighted by state population, which lessens the effect of eliminating cells with zero counts. Nevertheless, I also estimate models in which I treat homicides and murder arrests as counts with age-specific population as a right-hand-side variable. I use a negative binomial model to allow for overdispersion and conditional maximum likelihood to adjust for state fixed effects. The results are presented in table A2 of the appendix. The coefficients on the abortion rate are positive and statistically insignificant.

V. Conclusion

In this paper I focus on the association between the abortion rate and age-specific crime rates. I replicate Donohue and Levitt's results for violent and property crime arrest rates. I then apply their data and specification to an analysis of age-specific homicide rates and murder arrest rates. The coefficient on the abortion rates has the wrong sign for two of the four measures of crime and none is statistically significant at conventional levels. In the second half of the paper, I present alternative tests of abortion and crime that attempt to mitigate problems of endogeneity and measurement error. I find no association between legalized abortion and age-specific crime rates among cohorts born just before and after *Roe*. My results are very similar to Donohue and Levitt's regression of age-specific arrest rates. I conclude that there is little evidence that legalized abortion lowers crime through a selection effect.

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APPENDIX

TABLE A1.—ABORTION RATES BY STATE OF RESIDENCE IN 1971 AND 1974 RANKED BY THE CHANGE IN ABORTION RATES BETWEEN THE TWO YEARS*

State (ranked by difference in abortion ratio 74−71: lowest to highest)	Abortion Rate 1971	Abortion Rate 1974	Difference 1974–1971
New Mexico	224	146	−78
New Hampshire	96	120	24
Mississippi	7	32	25
Arkansas	29	72	43
Louisiana	16	62	46
Oregon	212	263	51
West Virginia	29	83	54
Oklahoma	33	94	61
Idaho	2	64	62
Kentucky	37	104	67
Utah	2	73	71
Nebraska	42	117	75
Alabama	22	100	78
Maine	75	160	85
Montana	35	125	90
North Dakota	25	121	96
Iowa	63	162	99
Wisconsin	74	174	100
Wyoming	32	141	109
Indiana	52	164	112
South Dakota	14	127	113
Missouri	59	182	123
Delaware	167	292	125
Average for below median	59	129	71
Weighted average for below median	52	126	74
Ohio	71	199	128
Minnesota	54	185	131
Kansas	112	245	133
South Carolina	39	177	138
Arizona	11	153	142
North Carolina	64	210	146
Texas	11	163	152
Colorado	115	271	156
Rhode Island	113	274	161
Georgia	53	214	161
Michigan	88	251	163
Vermont	96	261	165
Pennsylvania	113	280	167
Tennessee	39	217	178
Connecticut	174	370	196
Florida	79	286	207
Illinois	81	307	226
Virginia	84	323	239
Massachusetts	152	427	275
Nevada	4	331	327
New Jersey	188	520	332
Maryland	154	549	395
Average for above median	86	282	196
Weighted average for above median	83	272	188
Average for 45 states	72	204	132
Weighted average for 45 states	74	226	152

*Abortion rates are the ratio of abortions by state of residence per 1,000 births. Data on resident abortions in 1971 are from table 5 of the Centers for Disease Control (1972) and resident abortion rates in 1974 are from Stanley Henshaw of the Alan Guttmacher Institute.

TABLE A2.—NEGATIVE BINOMIAL ESTIMATES OF THE ASSOCIATION BETWEEN HOMICIDES AND MURDER ARRESTS AND THE ABORTION RATES IN THE 45 STATES THAT LEGALIZED ABORTION AFTER *ROE* FOR BIRTH COHORTS 1972–1975*

	Homicide		Murder Arrest	
	(1)		*(2)*	
Abortion rate	0.013	0.045	0.013	0.005
	(0.025)	(0.024)	[0.027]	[0.026]
Abortion rate weighted¶	0.031	0.060	0.022	−0.007
	(0.039)	(0.037)	[0.042]	[0.042]
State trends	No	Yes	No	Yes
N	2,559	2,559	1,621	1,621

*Coefficients are from a negative binomial regression estimated by conditional (on state fixed effects) maximum likelihood with Stata 9.0. Age-specific population was used as offsets. Exponentiation of the coefficients is interpretable as the incident rate ratio.
¶See notes to table 4.