NBER WORKING PAPER SERIES

FURTHER TESTS OF ABORTION AND CRIME

Ted Joyce

Working Paper 10564
http://www.nber.org/papers/w10564

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
June 2004

Further Tests of Abortion and Crime
Ted Joyce
NBER Working Paper No. 10564
June 2004, Revised September 2006
JEL No. K4

ABSTRACT

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

Ted Joyce
NBER
365 Fifth Avenue, 5th Floor
New York, NY 10016-4309
and Baruch College, City University of New York
ted_joyce@baruch.cuny.edu

I. Introduction

The debate as to whether legalized abortion lowers crime has 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 co-author John Donohue, which shows that a one-standard deviation increase in the abortion rate lowers homicide rates by 31 percent and can explain upwards of 60 percent of the recent decline in murder.[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 percent, then it's 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, 2004b; Lott and Whitely forthcoming; Foote and Goetz 2005; Cook and Laub 2003; Dinardo, forthcoming; Miron and Dills 2006). In published replies to Joyce (2004a) and Foote and Goetz (2005), Donohue and Levitt (2004, 2006) contend that their

[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

results become stronger when they address key criticisms of their original work. They show that the association between abortion and crime is strengthened if they use abortions by state of residence and not state of occurrence and if they instrument abortions for measurement error. They argue that if one looks at the association between abortion and crime over the life-cycle of criminals and not simply at a point in time their results hold. Finally, they concede that the association between age-specific arrests and abortion rates declines when they adjust for population, but that the estimates remain statistically significant and are of a meaningful magnitude.

In the analyses that follow, I show that the magnitude of the association between legalized abortion and age-specific crime rates is modest if appropriately scaled and statistically insignificant if corrected for serial correlation. I show that their heavily parameterized models are fragile. The coefficient on the abortion rate flips signs but remains statistically significant with minor changes in the specification. I contend that Donohue and Levitt's attempt to instrument the abortion rate against measurement error is not credible because the instrument is endogenous by construction. In the second half of this paper, I provide alternative strategies for identifying the effect of *Roe v Wade* on crime. First, I limit the analyses to the crime and arrest rates of cohorts born immediately before and after *Roe v Wade*. This is the only period during which the change in fertility rates represents an identifiable and plausibly exogenous decrease in unwanted childbearing. I then regress age-specific crime rates on abortion rates between 1971 and 1974 in the 45 states that legalized abortion after *Roe*. I lessen the measurement error by only using cohorts for which data on abortion exist. I gain greater statistical precision from a continuous proxy of unwanted childbearing; and I preserve the quasi-experimental design, since

–0.121. Their new "best" estimate is –0.166, an increase of 37 percent (Donohue and Levitt 2004). Based on the new estimates, homicide was 31.5 percent lower in 1997 than it would have been in the absence of abortion, up the identifying variation in abortion rates is driven primarily by legalization. All totaled, I find no consistent association between abortion and crime among cohorts born in the years just before and after abortion became legal. Simple time-series plots offer little evidence of substantial cohort effects. Regression analyses underscore what seems apparent from the time-series plots: the association between legalized abortion and crime rates is weak and inconsequential. I conclude that the lack of robustness to alternative specifications and identification strategies undermines a causal interpretation of Donohue and Levitt's findings.

II. Data, Empirical Model and Limitations: Donohue and Levitt (2001, 2004, 2006)

Data

I focus on the regressions of age-specific crimes and crime rates as analyzed by Donohue and Levitt (2001, 2004, 2006).[3] The age-specific analyses provide the most direct test of abortion and crime because the state-specific crime rate of each cohort is directly associated with the state-specific abortion rate that existed roughly one year prior to the cohort's year of birth. The data consist of arrests by state, year and single year of age for persons 15 to 24 years of age. These are from the FBI's Uniform Crime Reports and are used extensively by criminologists.

---

from their original estimate of 22.9 percent.

[3] Donohue and Levitt (2001) also present results from regressions of total crime rates on an average measure of abortion exposure for all ages, what they term the "effective abortion rate." The notion, however, that one could test for cohort effects without being able to identify cohorts seems strained. For instance, there is no obvious way to distinguish cohort from period effects without age-specific crime rates (Joyce 2004a). Second, these regressions include no controls for state-year shocks which Joyce (20004b), Foote and Goetz (2005) and Donohue and Levitt (2006) show to be important. Third, the effective abortion rate is a poorly measured and highly aggregated proxy of exposure to abortion. For instance, Donohue and Levitt (2001) only have data on abortion from 1973. They assume that the abortion rate is zero for cohorts born between 1960 and 1972 or they backcast from 1973 to 1970 for the five early-legalizing states. As a result, the number of cohorts with any measured exposure to abortion is extremely limited in the first half of their sample, 1985-1991. In addition, they use fixed weights from 1985 to assign exposure in the construction of the effective abortion rates. In that year teens accounted for 16 percent of all arrests for murder; by 1991 they accounted for 26 percent of arrests for murder. Thus, the effective abortion rate underestimates the exposure to abortion among teens during a period in which the dramatic rise in the total homicide rate between 1985 and 1992 rate was driven almost entirely by individuals less than 25 years of age (see also Lott and Whitley forthcoming; Blumstein, Rivara and Rosenfeld 2000).

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 arrest rates, however, all ages are identified. The SHR have reports on approximately 90 percent of all known homicides, but the proportion of reports of offenders with complete information on age and race is approximately 68 percent (Fox and Zawitz 2004). The FBI uses known reports of victims to impute age and race of offenders in cases where offenders are not known.[4]

I use abortions by state of residence as estimated by the Alan Guttmacher Institute from 1973 to 1987. I augment these data with the number of abortions as collected by the Centers for Disease Control and Prevention in 1971 and 1972. The CDC initiated its abortion surveillance in 1969. 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. The CDC reported 480,259 legal abortions in 1971 and 586,760 in 1972, a total of 1,067,019 in the two years prior to *Roe*. Of these 477,525 were to residents of Alaska, California, the District of Columbia, Hawaii, New York and Washington, the states in which abortion was *de facto* or d*e jure* legal. Thus, there were over 589,000 abortions to women from states that did not fully legalize abortion until *Roe* (Centers for Disease Control 1972, 1973). Donohue and Levitt do not count these abortions in their analyses.

[4] I use the SHR series with the imputed homicides in my replication of Donohue and Levitt (2004), but I use the non-imputed homicide series in subsequent analyses. Inferences are not sensitive to the choice of imputed and non-imputed series.

Empirical model

The basic model of arrests and homicides by single year of age as used by Donohue and Levitt (2001, 2004, 2006) is as follows:

$$LnC_{ajt} = \beta A_{jt-a-1} + Age_a + \lambda_j + \phi_t + \chi_c + e_{ajt} \qquad (1)$$

where C_{ajt} is the natural logarithm of arrests or homicides for age group a, in state j, and year t and cohort c; A_{jt-a-1} is the state abortion rate in year t-a-1.[5] Thus, arrests of 24-year olds in 1990 in state j are correlated with the abortion rate in state j in 1965 (t-24-1). Other versions of the model include interactions of age (Age), state (λ_j), and year (ϕ_t) fixed effects.

Limitations

1. <u>Underestimation of the standard errors</u>

Donohue and Levitt (2004) include arrests and homicides by single-year of age for 15 to 24 year olds from 1983 to 1998. There are potentially 7,140 cells (10 ages * 14 years * 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 includes have a unique abortion rate and over 60 percent of the state-age-year cells are assigned an abortion rate of zero. In their estimation of equation (1), 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.[6] However, such clustering imposes unrealistic restrictions on the correlation of residuals within states and across cohorts. Consider the regression of homicides by single year of age. Donohue and Levitt (2001, 2004, 2006) assume that the residual

[5] Donohue and Levitt (2001,2004,2006) refer to the ratio of abortions to births as the abortion rate. Demographers refer to the abortion rate as the ratio of abortions to women 15 to 44 years of age. For consistency with Donohue and Levitt, I will refer to the ratio of abortions to births as the abortion rate unless specifically noted otherwise.

associated with homicides of 18-year olds in New York in 1990, for example, is uncorrelated with the residual of homicides of 19-year olds in New York in 1990 or with the residual of 18-year olds in New York in 1989. However, a simple analysis of residuals is not consistent with such restrictions.[7] As a result, Donohue and Levitt (2001, 2004, 2006) underestimate the standard errors on the abortion rate by neglecting to account for this more general pattern of residual correlation (Bertrand, Duflo and Mullainathan 2004).

2. Estimating the magnitude of effects

Donohue and Levitt's most widely cited estimates are based on a regression of total crime rates on the effective abortion rate (Donohue and Levitt 2001, Table 4; 2004, Table 1). Their preferred estimates indicate that an increase of 100 abortions per 1000 live births in the effective abortion rate, roughly one standard deviation, is associated with a 16.6 percent decrease in total homicides.[8] However, since only half the population of criminals was exposed to legalized abortion by 1997, the full impact of legalized abortion will not be felt until roughly 2017 (Donohue and Levitt 2001, p.415). In other words, the impact of abortion on any one cohort is, on average, 33.2 percent (16.6/.50).[9] Donohue and Levitt confirm this expectation when they introduce the analysis of age-specific arrest rates. They write, "On average, about half of those

[6] For instance, arrest rates of 15-year olds in 1991, arrest rates of 16-year olds in 1992, arrest rates of 17-year olds in 1993, etc., are all associated with the abortion rate in 1973 since they were all assumed to be born in 1974.

[7] I estimated the first-order auto-regressive coefficient from the residuals of a regression of the natural logarithm of arrests for 18-year olds on the abortion rate with state and year fixed effects. The coefficient on the AR1 regressor was 0.579 (n=775) for violent arrests, 0.616 (n-775) for property crime and 0.221 for murder (n=631). I obtained the similar estimates for each of the other age groups. I also estimated residual regressions with higher order lags that suggest a more complicated pattern of serial correlation.

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

[9] In that same paragraph Donohue and Levitt (2001, p. 415), make the prediction that as each new cohort is exposed to legalized, 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 percent, then the homicide rate of this newly exposed cohort fell by 35% (1% times 35 age groups).

arrested are under the age of 25. Thus, to generate the crime reduction in Table IV *(the 16.6 percent above)* requires coefficients on young arrests that are twice as large as the coefficients on overall crime" (Donohue and Levitt 2001, p. 410; author's insertion in italics).

These are huge effects and much greater than the estimated effect of the actual abortion rate on age-specific arrest rates. For instance, an increase of 100 abortions per 1000 live births is estimated to lower violent and property crime arrests by between 1.5 and 4.0 percent (Donohue and Levitt 2001, Table VII). However, in their presentation of the results, Donohue and Levitt (2001) multiply the coefficient on the abortion rate (β in equation (1)) by 350, which suggest that the abortion rate lowered arrest rates by 5 to 14 percent.[10] Such scaling is unjustified since no cohort between 1972 and 1986 was ever exposed to an increase in abortion of that magnitude. Indeed, the standard deviation of the abortion rate in their sample is 134 per 1000 live births. Alternatively, the weighted mean of the abortion rate increased from 135 in 1971 to 295 in 1974, a change of 160 during a period of its most rapid growth. Thus, a more appropriate scaling of effects suggests that arrests fell between 1.9 and 5.4 percent.[11] If I use their most recent estimates (Donohue and Levitt 2006), a one standard deviation increase in the abortion rate lowers violent and property crime arrest rates by 0 to 3.6 percent, changes that are statistically insignificant when adjusted for serial correlation.

3. Crimes versus crime rates

In their most recent paper, Donohue and Levitt (2006, Table 5) show that the analysis of arrest rates as opposed to arrests reduces the coefficient on the abortion rate by over 60 percent

---

[10] The 350 represents the difference in the abortion rate in the top versus the bottom tertile of states (Donohue and Levitt (2001, p. 412). They do not indicate the year in which this difference existed.

[11] The coefficients from Table VII of Donohue and Levitt (2001) range from -0.15 to -0.040. Multiply each by 1.35 which is standard deviation in the abortion rate divided by 100.

for violent crime arrests (-0.046 to -0.021) and eliminates the associations with property crime arrests altogether (-0.024 versus 0.001). This is not surprising they argue, since one mechanism by which abortion reduces crime is through cohort size. But it is not clear that a regression of the level of arrests on the abortion rate captures the effect of both cohort size and selection.[12] Moreover, it is difficult to demonstrate an association between abortion rates and fertility rates beyond the immediate period of legalization. Fertility rates in the early and later legalizing states converge in 1976, yet the abortion rate in the early legalizing states is almost double that of later legalizing states (Joyce 2001). In other words, there is no evidence that much higher abortion rates in the early legalizing states after 1975 are associated with lower fertility rates. Thus, there is no cohort size effect after 1975. Donohue and Levitt (2004) acknowledge as much and argue that abortion improves the timing of births and not completed fertility.[13]

4. 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

[12] Let a linear modal of total crimes to age group a be as follows: $C_{ijt}=\alpha_0+\alpha_1 A_{ijt-a}$ where C_{ijt} and Ai_{jt-a} are crimes and abortions to individual i in state j and years t and years t-a, respectively. Summing both sides of the equation within each state we obtain, $C_{jt}=\alpha_0 N_{jt}+\alpha_1 A_{jt-a}$ where C_{jt} ,A_{jt-a} and N_{jt} are the total number of crimes, abortions and population respectively in state j and year t for age group a. The aggregate specification, therefore, is a regression of C_{jt} on N_{jt} and A_{jt-a}. In this specification it could be argued that α_0 captures the effect of cohort size on crime and α_1 the effect of selection. But this specification remains problematic because N_{jt} measures not only cohort size but includes many in the population who were not born in state j and thus unaffected by abortions in year t-a. In other words, Texas may have more crime not because there were fewer abortions in year t-a, but because there were large inflows of individuals not born in the state. Thus, it seems extremely difficult to identify cohort size effects. Dividing C_{jt} by N_{jt} in the equation above, one obtains a regression of crime per capita on the abortions per birth appropriately lagged:, $C_{jt}/N_{jt} = b_0+ b_1 A_{jt-a} /N_{jt-a.}$ where N_{jt-a} is births in year t-a, a proxy for population N_{jt}. Donohue and Levitt (2001,2004, 2006), however, regress C_{jt} on A_{jt-a} /N_{jt-a}. They claim that the coefficient on the abortion rate in this specification captures the effect of both cohort size and selection, but they do not explain why.

[13] Ananat, Gruber and Levine (2004) find that the effect of early legalization of abortion was concentrated among women whose completed fertility fell from one child to no children. It is less likely that these women represent mothers who would have given birth to children at high-risk for criminality, which suggests that the impact of smaller cohorts on crime, even during the period of legalization, is probably minimal at best.

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 percent, 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 non-residents as reported by the CDC. The AGI researchers then use another algorithm by which to assign the total number of non-resident abortions in the state of occurrence to a state of residence (Henshaw and 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.[14]

Donohue and Levitt (2001) use 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 and Levitt 2004, 2006). 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. The identifying assumption is that the measurement errors in the two estimates are uncorrelated. But this is clearly questionable given that the AGI estimate of abortions by state of residence relies on CDC data for its construction. In a footnote Donohue and Levitt couch the assumption of uncorrelated measurement errors. They argue that if the

[14] For instance, the CDC may under or overestimate the proportion of abortions to non-residents in state A There is also error in how AGI researchers assign non-resident abortions that occur in state A to individual states B, C, D,

measurement errors in the two series are positively correlated, then instrumental variables estimates of abortion on crime will be biased downwards in absolute value. But this assumption is also speculative because there are multiple sources of error and no gold standard with which to assess them. Indeed, instrumenting the resident abortion rate by the occurrence rate may put back the error that the resident rates was estimated to fix. [15] In short, the instrumented estimates of abortion on crime in Donohue and Levitt (2006) lack credibility.

---

etc, based only on the proportion of all non-resident abortions obtained in state A.

[15] To see this consider the following:

Let R_i = abortions to residents of state j regardless of the state in which they occur.
Let R_{jj} = abortions to residents of state j that occur in state j
Let R_{jk} = abortions to residents of state j that occur in state k
Let O_j = abortions that occur in state j regardless of state in which women reside
Let O_{jj} = abortions to women that reside in state j and that occur in state j
Let O_{kj} = abortions to women that reside in state k but which occur in state j
Note that Rjj = Ojj.
Donohue and Levitt's first stage is :
$R_j = \beta_0 + \beta_1 O_j$ which can be rewritten as :
$R_{jj} + R_{jk} = \beta_0 + \beta_1(O_{jj} + O_{kj})$

Thus, the correlation between Rj and Oj will be driven by Rjj and Ojj. In words, the predicted Rj measures the 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 is likely lost by instrumenting. One ends up with a "resident - occurrence" rate, which largely defeats the purpose of using Rj to begin with.
A tangential point is that Rjk and Okj are likely to be negatively correlated. States in which many residents leave for an abortion (R_{jk}) are likely to be states in which relatively few non - residents obtain abortions (O_{kj}). Abortions performed outside the state of residence are probably a major sourceof measurement error, which would appear to be negatively and not positively correlated as suggested by Donohue and Levitt (2006).

III. Replication and Re-estimation of Donohue and Levitt (2004, 2006)

In this section I replicate Donohue and Levitt's estimates of the effect of abortion on age-specific arrest rates. I scale the effects by showing the impact of a one standard deviation increase in the abortion rate. I then adjust the standard errors for serial correlation A major limitation of arrests is that they reflect both underlying violence as well as deterrence. Homicides, by contrast, are a more straightforward and relatively well-reported measure of crime. Thus, I apply Donohue Levitt's specification to an analysis of age-specific murder arrests and homicide rates. Finally, I show the sensitivity of Donohue and Levitt's specification to various interactions of age, state and year fixed effects.

1. Serial correlation, scaling of effects and murder

The specification in column (1) of Table 1 replicates Donohue and Levitt's results for violent crime and property crime arrest rates (Donohue and Levitt 2006, Table 5). 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 abortion.[16] Donohue and Levitt (2004) argue that the assumption of a zero abortion rate for the 1961-1972 cohorts should bias their estimates towards the null. If correct,

[16] Joyce (2004a) presented results from a similar regression of log arrests for cohorts born between 1974 and 1981. He found no association between arrests and abortion rates (See Joyce 2004a, Table 1, row 7). In their reply to Joyce, Donohue and Levitt indicate that when they use abortions by state of residence and not occurrence and when they extend the data to 1998, "two of the three coefficients become negative and statistically significant" (Donohue and Levitt (2004), footnote 1, p. 36). Donohue and Levitt never report these results. I have estimated those regressions using arrest rates and not arrests and adjusted them for serial correlation. The coefficients on the abortion rate in regressions of violent crime, property crime and murder arrest rates is small and statistically insignificant or has the wrong sign (available upon request). The results in column (3) of Table 1 above are from a

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, the estimate is no longer significant. Moreover, the effect of a one-standard deviation increase in the abortion rate is modest, reducing arrest rates by only 3.5 percent. If I limit the sample to cohorts born between 1974 and 1981, the effect of a one-standard deviation increase in the abortion rate rises to -4.6 percent, but is not statistically significant at the 0.05 level. The remaining results provide no evidence that abortion is associated with crime. Seven of the remaining 9 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.[17]

2. Lack of robustness

The specification used by Donohue and Levitt in their most recent work is highly parameterized and the estimates are sensitive to the set of fixed effects that are included. Recall that there are 10 age groups, 51 states and 14 years. With no missing data or cells with zero events, their model has 7,140 potential observations and 1,291 parameters, which is between two and six times more parameters than were included in their original specifications (Donohue and

similar specification but with the inclusion of state-year fixed effects. This precluded the inclusion of 15-year olds born in 1983. We also dropped 15 and 16 year olds born in 1982.

[17] 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 literally thousands of observations for the 1960-1972 cohorts in which the association the between abortion rate and crime cannot even be estimated. However, by appending those cohorts to the analysis, Donohue and Levitt shrink the standard errors without adding any information on the key variable of interest. I would contend, therefore, that the standard errors in column (3) provide a more realistic estimate of the level of precision with which the association between the abortion rate and age-specific crime rates can be estimated.

Levitt 2001, 2004). In their original paper and in their reply to Joyce, Donohue and Levitt included state, age and year fixed effects (Donohue and Levitt 2001, 2004). In some specifications they added state and age interactions and in others they added cohort fixed effects. Their estimates were relatively stable across specifications. However, Donohue and Levitt did not include state and year interactions as claimed, which was noted by Foote and Goetz (2005). In response to Foote and Goetz (2005), Donohue and Levitt re-estimated their models with state and year interactions. Their estimates fell by half.

In Table 2, I show the fragility of their estimates as one builds from their original specifications to their most recent. Estimates in columns (1) and (2) include the same set of fixed effects as Donohue and Levitt used in their earlier papers. Note that the dependent variable in each case is the age-specific arrest or homicide rate.[18] Consider the estimates for violent crime arrest rates in the top row. The coefficient in column (1) indicates that the abortion rate is *positively* associated with the violent crime arrest rate (β=0.018; t-ratio =3.0); addition of state-age interactions in column (2) leads to the opposite inference: abortion rates lower violent crime arrest rates (β= -0.031; t-ratio = -4.4); inclusion of state-year interactions (column 3) causes the coefficient to flip sign again but it remains highly statistically significant ((β= 0.065; t-ratio= 6.5). Finally with the addition of year-age interaction, we obtain the result reported in Donohue and Levitt (2006; Table 5) as shown in column (5). The pattern of coefficient instability is characteristic of the other three outcomes. Moreover, the vast majority of estimates is

[18] Donohue and Levitt (2001, 2004) used arrests not arrest rates in their earlier work, although they frequently referred to them as arrest rates. For instance, in their original paper they introduce the section of arrests by single year of age as follows: "As a further test of our hypothesis, we analyze arrest rates by single year of age "(Donohue and Levitt, 2001, p. 311). In Table 1 of Donohue and Levitt (2004) the sub-title for the last two rows is "ln(arrest rates by single year of age)" and yet the results are from regressions of log arrests and not log arrest rates. The title in Table 2 of Donohue and Levitt (2004) also refers to the homicide rate when in fact their outcome is homicides. The distinction is more than semantic. Donohue and Levitt (2004) contrast their specification based on homicides to Joyce's results which were based on homicide rates (Joytce, 2004a). Importantly, when Joyce (2004b) estimated

statistically significant but frequently have the “wrong” sign. Thus, the conclusion one reaches as to whether abortion increases or decreases crime depends critically on the set of fixed effects.

The fragility of the estimates reflects the lack of variation in the abortion rate. Despite the large number of potential observations (7,140), the abortion rate varies only by state and year and only for a subset of years (n=510). Although it is econometrically possible to include state-year interactions, the practical consequence is that there remains too little variation in the abortion rate with which to identify a robust effect on crime.

IV. An Alternative Identification Strategy

The sensitivity of the association between abortion and crime to population controls, fixed effects and corrections for serial correlation presents a serious challenge to the Donohue and Levitt hypothesis. However, a more fundamental problem is whether state abortion rates are a useful proxy for unwanted childbearing. The availability of abortion affects the decision to have sex, to use contraception, to carry to term if pregnant, to marry or to have a child outside of wedlock. Stated differently, it is unclear whether states with greater abortion rates have lower rates of unwanted childbearing. For instance, an increase in contraception or a decline in sexual activity could lower both the abortion rate as well as the rate of unwanted childbearing. Indeed, this seems to be the experience of U.S. teens over the past decade. The teen birth rate fell 21 percent between 1990 and 2000 while the teen abortion rate fell 40 percent over the same period (Alan Guttmacher Institute 2004). Demographers estimate that 53 percent of the decline in teen pregnancies can be attributed to decreased sexual activity and more effective contraception (Santelli et al. 2004). The upshot is that without a demonstrable inverse association between state

---

the exact same model as Donohue and Levitt (2004, Table 2) with homicide rates and not homicides, the average decline in homicide rate is small and statistically insignificant.

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.[19]

And yet, Donohue and Levitt never demonstrate a negative relationship between state abortion rates and state fertility rates. In fact, they argue that such an association is unnecessary. "…As long as the number of unwanted births falls, even if total births do not decline at all, one would expect to see better life outcomes on average for the resulting cohort" (Donohue and Levitt (2004, p. 33). But they never explain how to identify such changes. How, for example, does one distinguish variation in state abortion rates due to changes in sexual activity and contraception from variation in state abortion rates associated with unwanted births averted.

For these reasons, my identification strategy has focused around the period of legalization (Joyce 2004a, 2004b). It is only during this period that the price shock induced by legalization leads to a significant increase in abortion and a documented fall in fertility (Sklar and Berkov

---

[19] Another way to demonstrate the difficulty of identifying the effect of abortion on crime is to sketch out the underlying structural model.

$$(1f)\ \ C_t = \alpha_0 + \alpha_1 UW_{t-a}$$
$$(2f)\ \ UW_{t-a} = \beta_o + \beta_1 AR_{t-a-1}$$
$$(3f)\ \ AR_{t-a-1} = \lambda_0 + \lambda_1 P_{t-a-1}$$
$$(4f)\ \ C_t = \gamma_0 + \gamma_1 P_{t-a-1_t}$$
$$(5f)\ \ \mathrm{P}_{\text{t-a-1}} = b_0 + b_1 Q_{t-a-1}$$
$$(6f)\ \ \mathrm{Q}_{\text{t-a-1}} = a_0 + a_1 P_{t-a-1}$$

Let C_t be the crime rate in year t of age group a; let UW_{t-a} be the rate of unwanted childbearing in year t-a; Let Ar_{t-a-1} be the abortion rate in year t-a-1, let P_{t-a-1} be the price of abortion in year t-a-1 and let Q_{t-a-1} be the supply of abortion providers. This is clearly a simple specification. A more complete structural model of abortion and crime would include equations for contraception, sexual activity and pregnancy resolution. It would also contain the price of a birth which would include the availability of welfare benefits, Medicaid and Head Start. The point I want to emphasize is that Donohue and Levitt regress C_t on AR_{t-a-1} as if it were a reduced form. Certainly, abortion is pre-determined, but the decision to abort may be made simultaneously with the decision to have sex, use contraception and bear children. I estimate a version of equation (4f) above, but even this is problematic unless P_{t-a-1} varies exogenously. This is because the demand and supply of abortion services is also endogenous. Soon after legalization, markets for abortion services developed. Abortion providers located where demand was strongest.

1974; Joyce and Mocan, 1990; Gruber, Levine Staiger 1999; Angrist and Evans 1999; Charles and Stephens 2006; Joyce 2001; 2004a). Most researchers have interpreted the fall in fertility at this time as a decline in unwanted childbearing.

The other 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. 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 legalization (Meyer 1995). Cohort effects present in a specific manner and should be noticeably distinct from period effects (O'Malley, Bachman and Johnston 1984). As I show in the next section, simple time-series plots of age-specific crime rates offer little if any evidence of any meaningful cohort effect.

The Impact of Roe v. Wade on Crime

In previous work I have used abortion legalization prior to *Roe* in a sub-sample of states to test for an association between abortion and crime (Joyce 2004a). The national legalization of abortion following *Roe* offers a second experiment that may be less contaminated by policy endogeneity at the state level.[20] The difficulty, as Donohue and Levitt (2004) point out, is that a before and after analysis based on a national change assumes a uniform effect in all states and is

---

Thus, I emphasize results that are identified off of the change in the legalization of abortion, which I interpret as an exogenous change in the price of abortion.

[20] Although the legalization of abortion in New York was a shock to many, including the leadership of the Catholic Church, New York and California were clearly liberal states in which sentiment towards abortion was arguably more permissive even prior to legalization (Lader 1974; Garrow 1998). Tietze (1973), for instance, estimates that two-thirds of legal abortions in New York City between July, 1970 and June,1971 replaced illegal abortions from a year earlier.

susceptible to confounding from broad trends. However, there was wide variation in legal abortion rates in the 45 non-repeal states prior to *Roe* and marked differences in the change in abortion rates following legalization. I take advantage of this variation in two ways. First, I stratify states between those in which the change in the abortion rate between 1971 and 1973 was above or below the median. I then use a DDD estimator for cohorts born from 1970 to 1975. In the next set of analyses I follow Donohue and Levitt (2001, 2004, 2006) and estimate a version of equation (1) but only for the cohorts born in 1972-1975. These two analyses address key limitations of Donohue and Levitt's identification strategy. First, changes in abortion immediately after *Roe* are large and more plausibly exogenous than are changes that occurred in the late 1970s and early 1980s. Second, *Roe* had a demonstrable effect on fertility and thus on unwanted childbearing in states furthest from California, New York and Washington (Levine et al. 1999). Third, I use cohorts born just prior to legalization to net out period effects that may vary differentially between states above and below the median. And fourth, I use both an indicator of abortion legalization as well as a continuous proxy for unwanted childbearing as favored by Donohue and Levitt (2004).[21]

As evidence of the state-variation in abortion and its growth in the early 1970s, I show resident abortion rates in 1971 and 1973 in the 45 states in which abortion became legal following *Roe* in Table A1 of the Appendix. The average increase (weighted) in the abortion rate was 41 abortions per 1000 live births between 1971 and 1973 in states at or below the median and 142 in states above the median. I exploit this differential change by comparing variation in crime from before to after *Roe* in states above and below the median. The

[21] …"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 and Levitt 2004, p. 46-47.)

differential change in abortion among states above and below the median is 101 abortions per 1000 live births and is directly comparable to the scaling used by Donohue and Levitt (2001). The relevant regression is as follows:

$$LnH_{ajt} = \beta_0 + \beta_1 Exposed + \beta_2 (Exposed * Above\_med) + \beta_3 (Exposed * After) + \beta_4 (Above\_med * After)$$
$$+ \beta_5 (Exposed * Above\_med * After) + \sum_t \tau_t + \sum_j \lambda_j + e_{ajt} \qquad (2)$$

The dependent variable, LnH_{ajt} is the natural logarithm of the homicide 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 and who go from unexposed to exposed to legalized abortion *in utero*. The omitted category is the slightly older age groups who are born between 1970-1973 and thus, were unexposed to legalized abortion *in utero. After* refers to the birth years 1974-75 and *Above_med* are states in which the change in abortion from 1971 to 1973 was above the median. The model includes state and year fixed effects. The coefficient, β_5, estimates the difference-in-differences-in-differences (DDD). For a concrete example, refer to the age-period-cohort diagram in Table 3. Consider the homicide rates of 15- year olds in 1987 and 1988. They were born primarily in 1972-1973 and thus were in *in utero* prior to *Roe.* Fifteen-year olds in 1989 and 1990 are from the 1974-1975 birth cohorts and thus were exposed to legalized abortion *in utero*. If legalized abortion lowered crime, then the homicide rates of 15-year olds should decline from 1987-98 to 1989-90. However, strong period effects could confound the impact of abortion. Thus, I use the change in homicide rates among 17-year olds over the same period, 1987-88 to 1989-90, as the counterfactual. Although exposed to the same period effects as the 15-year olds, the 17-year olds were born prior to 1974 and thus unexposed to legalized abortion *in utero*.

V. Results

Time-series evidence

Homicide rates and arrest rates for three categories of crime are shown in Figures 1-4. The series are stratified by states in which the change in abortion rates between 1971 and 1973 was above the median versus states at or below the median (see Appendix Table 1A). 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. [22]

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 1973 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 factor driving down arrest rates among 21-year olds is having a similar effect on 23-year olds, which undermines a causal link between abortion and crime. Figure 2 displays homicide rates for 17- and 19-year olds also stratified by states with above and

[22] 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 and Levitt 2001, p. 411). Evidence of period effects would be the coincident rise, peak and fall in homicide rates of different cohorts.

below median changes in abortion. Again, there is little 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 more consistent with strong period effects than with any meaningful cohort effect. Figures 3 and 4 show time-series of homicide and property crime arrest rates. Again, there is little evidence of any substantive cohort effect.

Regression results

Regression results from the estimation of equation (1) are shown in Table 4. 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 (2)] and each estimate is from a separate regression. The bottom row is the average DDD for the 1972-1975 cohorts between the ages of 13 and 26.[23] I find no inverse association between the legalization of abortion and crime. Only 6 of the 19 estimates arenegative and none are statistically significant. The average of the DDDs within each crime category is positive. For instance, homicide rates increased 5.8 percent more among the 1972-1975 cohorts relative to the 1970-1973 cohorts in states at or above the median relative to states below the median change in abortion rates between 1971-1973.

Although the specification in equation (2) exploits the natural experiment afforded by *Roe*, it does not fully take advantage of the state-by-state change 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 use the specification favored by Donohue and Levitt (2001, 2004) as shown in equation (1) above.[24]

I show the coefficients on the abortion rate from the estimation of equation (1) in Table 5. For each outcome, I show estimates without and then with state linear and quadratic trend terms. The trend terms serve a similar role as the within-state comparison groups in the DDDs. 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 re-weighted to approximate the state in which the mother conceived.[25] 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 six out of eight specifications, but relatively small in magnitude and never statistically significant. For instance, the abortion rate increased by 150 abortions per 1000 live births between 1971 and 1974 and the standard deviation was 88 abortions per 1000 live births adjusted for cross-state movements. This is associated with a decline of 5.4 percent ($p<0.21$) in violent crime arrest rates and 3.2 percent ($p<0.38$) percent in property crime arrest rates. However, the association becomes positive in the case of violent crime and essentially zero in the case of property crime when I adjust for state-specific trends. There is no association with murder arrest rates or homicide offending rates in any specification.

---

[23] Donohue and Levitt argue that one should sum the coefficients in the 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 and Levitt 2004, p. 42)

[24] There are several important differences between their analysis and mine. First, I limit the sample to cohorts born in 1972-1975 in the 45 states that legalized abortion with *Roe* in January, 1973.[24] Each cohort is associated with the resident abortion rate lagged approximately one year. The identifying assumption is that the dramatic decrease in the "price" of an abortion induced by *Roe* caused an exogenous increase in abortion rates. The second difference is that I use only cohorts for which actual abortion rates exist. Donohue and Levitt (2004) include birth cohorts from 1968-1976 and they assume the abortion rate is zero for a large proportion of cells (Donohue and Levitt 2004, Table 3). Finally, I adjust for curvilinear trends in crime rates within each state in selected specifications. I risk over-fitting since the inclusion of state and year fixed effects as well as state-specific trend terms absorbs most of the variation in the abortion rate. Nevertheless, if legalization induced sharp discontinuities in crime and abortion rates, then there may be sufficient variation to detect an association between abortion and crime.

One final concern is the small number of homicides or murder arrests in some age-state-year cells. The 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 weighted by state population lessen the impact of these observations. Nevertheless, I also estimate models in which I treat homicides and homicide arrests as counts. I use a negative binomial model to allow for over-dispersion 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.

VI. Discussion

I have tried to make two points in this paper. First, even if one accepts Donohue and Levitt's identification strategy, their estimates are small and statistically insignificant. The coefficient on the abortion rate in a regression of age-specific homicide or arrest rates has either the wrong sign or is small in magnitude and statistically insignificant when adjusted for serial correlation. Efforts to instrument for measurement error are flawed and estimates are highly sensitive to set of fixed effects and their interactions. The second point is that Donohue and Levitt's results are not robust to an alternative identification strategy. I have argued that a convincing test of a link between abortion and crime should be based on an exogenous change in abortion that had a demonstrable effect on fertility. Thus, I use the legalization of abortion following *Roe* to identify effects of abortion on crime in the 45 states affected by the ruling. I use within-state comparison groups to net out hard to measure period effects. I also follow Donohue and Levitt (2004) and average the effects of legalization on crime over 10 to 20 years of the life of a cohort. I find little support for their hypothesis.

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

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

Finally, I have never argued that unanticipated changes in unwanted childbearing would have no effect on family and/or child well-being. The more relevant question is whether such effects are large enough and our research designs credible enough to uncover such impacts 15 to 25 years later. Studies, for instance, that look at changes in outcomes in the years just after legalization may be better able to detect immediate impacts and may be less confounded by hard to measure period effects (Gruber, Levine and Staiger 1999; Klick and Stratmann 2003). Results from studies that have examined more long-term consequences are mixed. Angrist and Evans (1999) find significant effects of abortion legalization on teen fertility in the period right after legalization, but uncover no effect on the subsequent earnings of cohorts exposed. They speculate that longer-term outcomes may be "too far down the causal chain" . Charles and Stephens (2006), by contrast, find evidence that cohorts exposed *in utero* are less likely to use illicit drugs in high school. The decline in youth drug use, however, may be related to supply – side diseconomies rather than decreases in demand by "more wanted" youth (Jacobson 2004).

Studies of the effects of legalized abortion on well-being are more credible when the change in fertility is more dramatic. A recent analysis of the Romanian ban on abortion in 1966 found that cohorts exposed to the ban, adjusted for compositional changes, committed more crime than cohorts born just before (Pop-Eleches 2005). The author cautions, however, that omitted period effects and not the cohort effect associated with the ban may explain differences in crime among those born in the 1970s. Nevertheless, the Romanian intervention is unique since total fertility rates *doubled* as a result of the ban, an exogenous change in fertility roughly 20 times greater than the change that occurred after early legalization in the United States (Levine et al. 1999). Sen (2003), on the other hand, argues that variation in teen abortion rates explains 50 percent of the decline in *total* violent crime in Canada in the 1990s. However, the teen abortion rate can explain only 1 percent of the 33 percent decline in teen fertility rates between 1974 and 1988.[26] Moreover, teen fertility rates and pregnancy rates *rise* in the year after abortion is fully legalized, a result more consistent with moral hazard than fetal selection (http://www.statcan.ca/english/kits/preg/preg3g.htm).

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

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

Acknowledgements

I thank Christopher Foote, Andrew Racine, Steven Levitt and Robert Kaestner for helpful comments as well as seminar participants at the NBER Health Economics Program, Carnegie Mellon's Heinz School of Public Policy, American Enterprise Institute, University of Rochester, and University of Illinois, Chicago. Silvie Colman provided terrific research assistance. I am responsible for all errors.

References

Angrist, Joshua, and William N. Evans. 1999. "Schooling and Labor Market Consequences of the 1970 State Abortion Reforms." In Research in Labor Economics, ed. Ronald Ehrenberg, pp. 75-111. Westport, CT: JAI Press.

Alan Guttmacher Institute. 2004. "U.S. Teenage Pregnancy Statistics." http://www.agi-usa.org/pubs/state_pregnancy_trends.pdf Accessed October 4, 2005.

Ananat, Elizabeth O., Gruber, Jonathan and Phillip Levine. 2004. " Abortion Legalization and Lifecycle Fertility." National Bureau of Economic Research Working Paper 10705,

Bertrand, Marianne, Duflo, Esther and Sendhil Mullainathan. 2004. "How Much Should We Trust Differences-in-Differences Estimates?" Quarterly Journal of Economics. 117: 249-275.

Blumstein, Alfred. 2000. "Disaggregating the Violence Trends." In The Crime Drop in America, ed. Alfred Blumstein and Joel Wolman, pp: 13-44. New York: Cambridge University Press.

Blumstein, Alfred. 1995. "Youth Violence, Guns, and the Illicit Drug Industry." The Journal of Criminal Law and Criminology 86(1):10-36.

Blumstein, Alfred, Frederick Rivara, and Richard Rosenfeld. 2000. "The Rise and Decline of Homicide--and Why." Annual Review of Public Health 21:505-541.

Centers for Disease Control. 1972. Abortion Surveillance Report--Legal Abortions, United States Annual Summary, 1971, Atlanta, Georgia: Centers for Disease Control.

Centers for Disease Control. 1973. Abortion Surveillance Report--Legal Abortions, United States Annual Summary, 1972, Atlanta, Georgia: Centers for Disease Control.

Charles, Kerwin Kofi and Melvin Stephens Jr. 2002. "Abortion Legalization and Adolescent Substance Use." National Bureau of Economic Research Working Paper No. 9193.

Cook, Philip, and John H. Laub. 1998. "The Unprecedented Epidemic in Youth Violence." In Youth Violence, ed. Michael Tonry and Mark H. Moore, pp. 27-64. Chicago: University of Chicago Press.

Cook, Philip and John H. Laub. 2002. "After the Epidemic: Recent Trends in Youth Violence in the United States." Crime and Justice: A Review of Research Volume 29. Chicago: University of Chicago Press.

Dinardo, John. "A Review of Freakonomics." Journal of Economics Literature. Forthcoming.

Donohue, John, and Steven Levitt. 2001. "The Impact of Legalized Abortion on Crime." Quarterly Journal of Economics 116(2): 379-420.

Donohue, John, and Steven Levitt. 2004. "Further Evidence that Legalized Abortion Lowered Crime: A Reply to Joyce." Journal of Human Resources. 39(1):29-49.

Donohue, John, and Steven Levitt. 2006. "Measurement Error, Legalized Abortion, and the Decline in Crime: A Response to Foote and Goetz (2005)." National Bureau of Economic Research Working Paper 11987.

Foote, Christopher, and Christopher Goetz. "Testing Economic Hypotheses with State-Level Data: A Comment on Donohue and Levitt." Federal Reserve Bank of Boston Working Paper 10-15, November 22, 2005.

Fox, James Alan. 2000b. "Demographic and U.S. Homicide." In The Crime Drop in America, ed. Alfred Blumstein and Joel Wolman, pp: 288-317. New York: Cambridge University Press.

Fox, James Alan. 2004. "Uniform Crime Reports [United States]: Supplemental Homicide Reports, 1976-1998." [Computer File]. Inter-University Consortium for Political and Social Research versions. Boston, MA: Northeastern University, College of Criminal Justice [producer]; Ann Arbor: Michigan: Inter-University Consortium for Political and Social Research [distributor].

Fox, James Alan and Marianne W. Zawitz. 2004. "Weighting and Imputation Procedures for 1976-2002 Cumulative Data File." (http://www.ojp.usdoj.gov/bjs/homicide/imputationandweighting.htm)

Gruber, Jonathan, Phillip Levine, and Douglas Staiger. 1999. "Legalized Abortion and Child Living Circumstances: Who is the Marginal Child." Quarterly Journal of Economics 114(1):263-291.

Henshaw, Stanley and Jennifer Van Vort. 1992 Abortion Factbook. New York: Alan Guttmacher Institute.

Jacobson, Mireille. 2004. "Baby Booms and Drug Busts: Trends in Youth Drug Use in the United States, 1975-2000. Quarterly Journal of Economics. 1481-1512.

Joyce, Ted. 2001. "Did Legalized Abortion Lower Crime?" National Bureau of Economic Research Working Paper No. 8319, June.

Joyce, Ted. 2004a. "Did Legalized Abortion Lower Crime?" Journal of Human Resources. 39(1):1-28.

Joyce, Ted. 2004b. "Further Tests of Abortion and Crime." National Bureau of Economic Research Working Paper No. 10564, June.

Joyce, Ted and Naci Mocan. 1990. "The Impact of Legalized Abortion on Adolescent Childbearing in New York City." American Journal of Public Health 80(3):273-278.

Klick, Jonathan and Thomas Stratmann. 2003. "The Effect of Abortion Legalization on Sexual Behavior: Evidence from Sexually Transmitted Diseases." The Journal of Legal Studies 32(2):407-433.

Lader, Lawrence. 1974. Abortion II: Making the Revolution. New York: Beacon.

Levine, Phillip et al. 1999. " *Roe v. Wade* and American Fertility." American Journal of Public Health 89(2):199-203.

Levitt, Steven. 2004. "Understanding Why Crime Fell in the 1990s: Four Factors that Explain the Decline and Six that Do Not." Journal of Economic Perspectives. 18(1):163-190.

Lott, John and John Whitley. Forthcoming. "Abortion and Crime: Unwanted Children and Out-of-Wedlock Births." Economic Inquiry.

Meyer, Bruce. 1995. "Natural and Quasi-Experiments in Economics." Journal of Business and Economic Statistics. 13(2): 151-161.

O'Malley, Patrick M., Bachman, Jerald G. and Lloyd Johnston. 1984. "Period, Age, and Cohort Effects on Substance Use Among American Youth, 1976-82." American Journal of Public Health 74(7):682-688.

Pop-Eleches, C. 2005. "The Impact of an Abortion Ban on Socio-Economic Outcomes of Children: Evidence from Romania." Unpublished manuscript, Department of Economics, Columbia University

Saul, Rebekah. 1998. "Abortion Reporting in the United States: An Examination of the Federal-State Partnership." Family Planning Perspectives. 30(5):244-247.

Santelli JS, Abma J, Ventura S, Lindberg L, Morrow B, Anderson JE, Lyss S, Hamilton BE 2004. "Can changes in sexual behaviors among high school students explain the decline in teen pregnancy rates in the 1990s?" Journal of Adolescent Health 35:80-90.

Sen, Anindya. 2003. "Does Increased Abortion Lead to Lower Crime? Evaluating the Relationship between Crime, Abortion and Fertility." Manuscript. Department of Economics, University of Waterloo, Canada.

Sklar, June, and Beth Berkov. 1974. "Abortion, Illegitimacy, and the American Birth Rate." Science185 (September).

Tietze, Christopher. 1973. "Two Years Experience with a Liberal Abortion Law: Its Impact on Fertility Trends in New York City." Family Planning Perspectives 5(1): 36-41.

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 from 1971-1973*

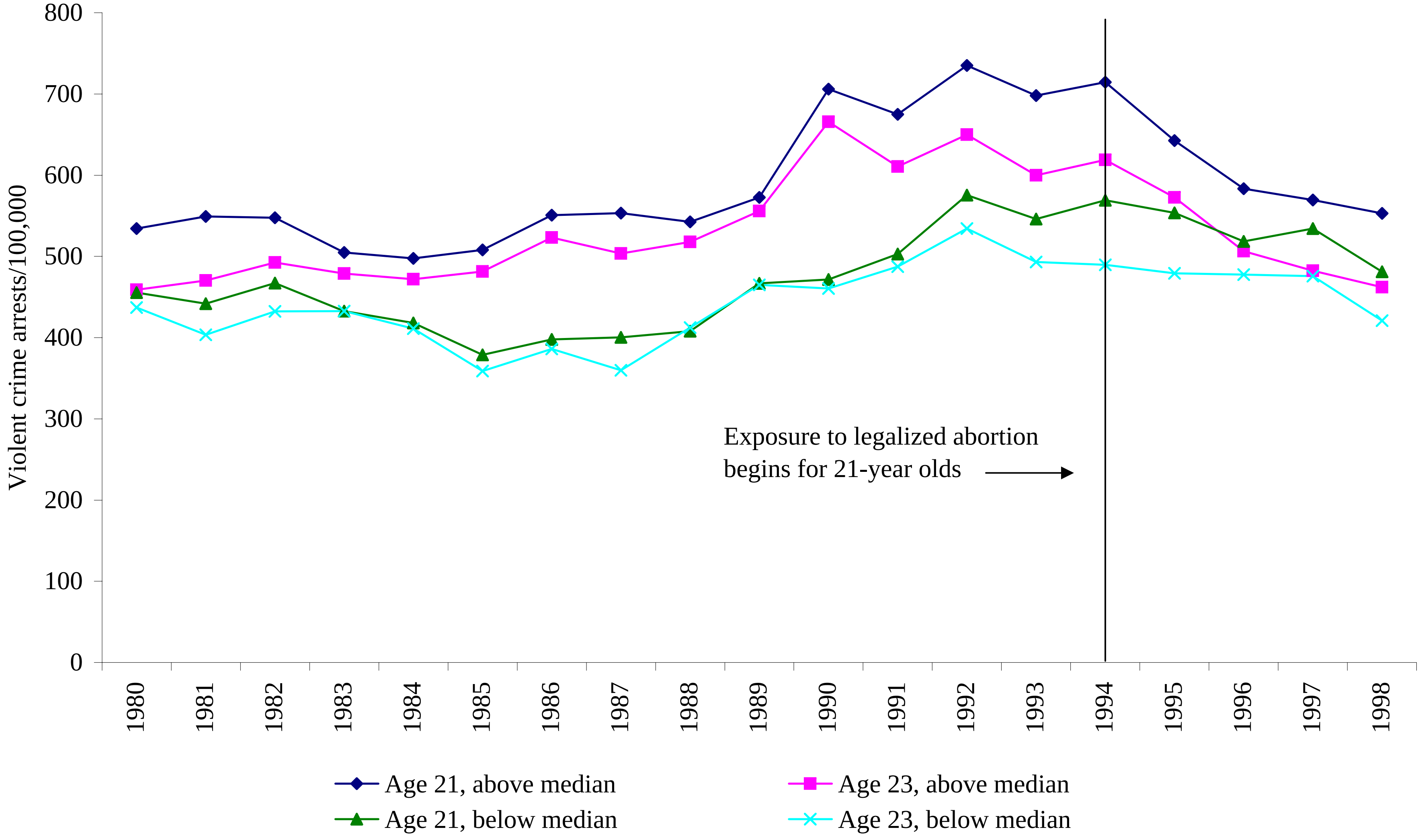

Figure 2. Homicide Rates among 17 and 19-Year Olds in 45 States that Legalized Abortion after Roe, Stratified by States with Above and Below Median Increases in the Abortion Rate from 1971-1973*

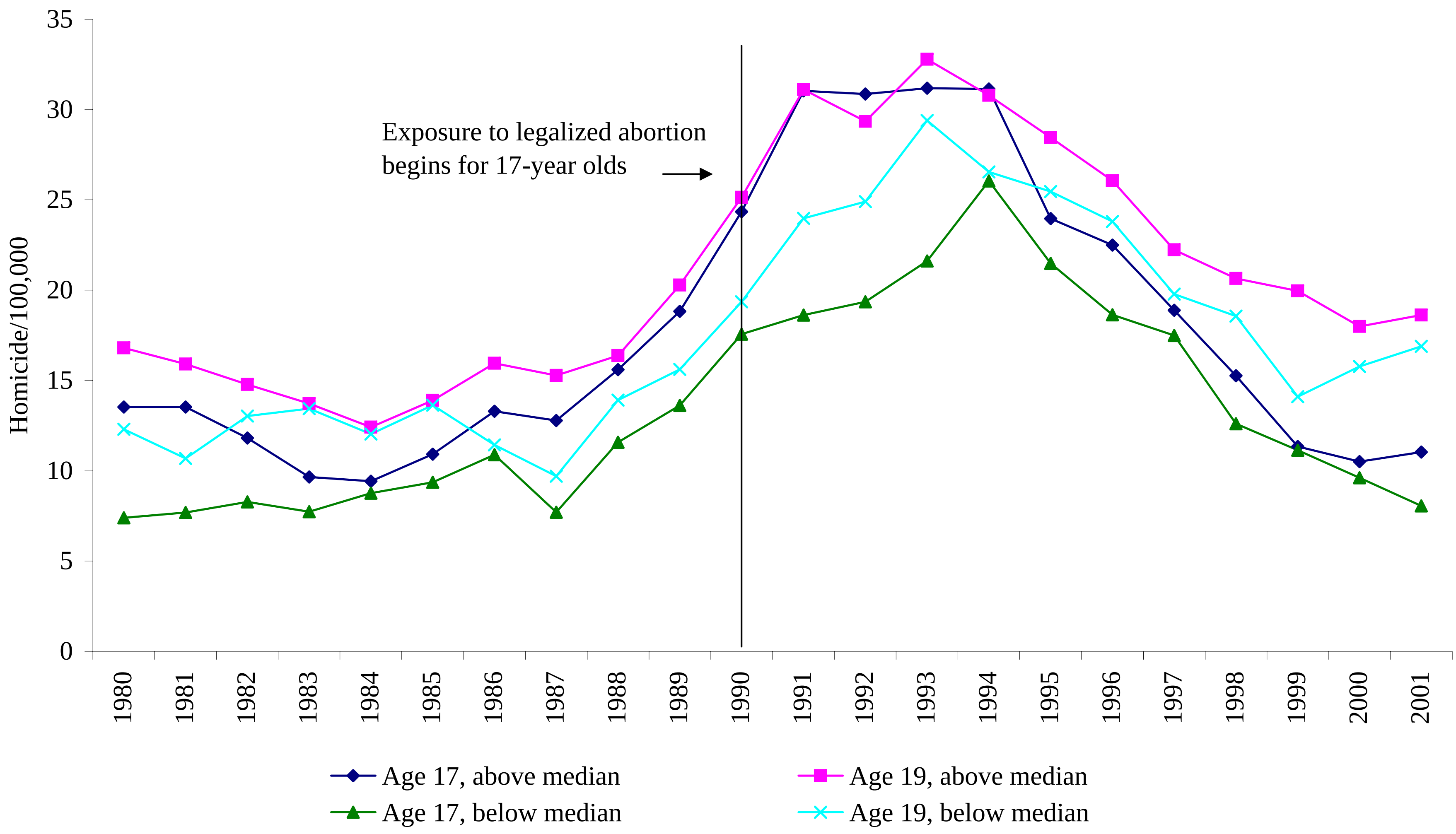

Figure 3. Murder Arrest Rates among 18 and 20-Year Olds in 45 States that Legalized Abortion after Roe, Stratified by States with Above and Below Median Increases in the Abortion Rate from 1971-1973*

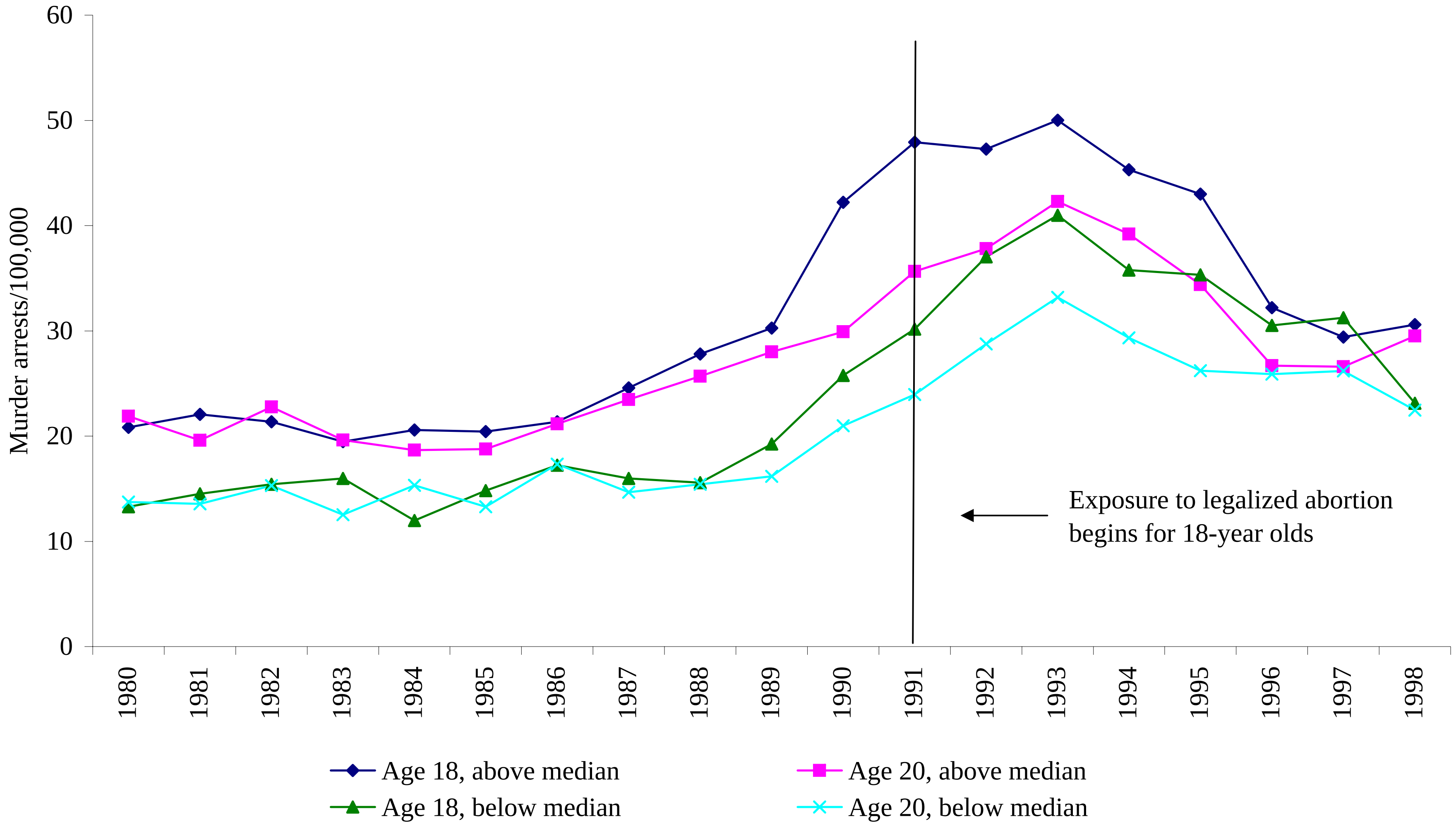

Figure 4. Property Crime Arrest Rates among 16 and 18-Year Olds in 45 States that Legalized Abortion after Roe, Stratified by States with Above and Below Median Increases in the Abortion Rate from 1971-1973*

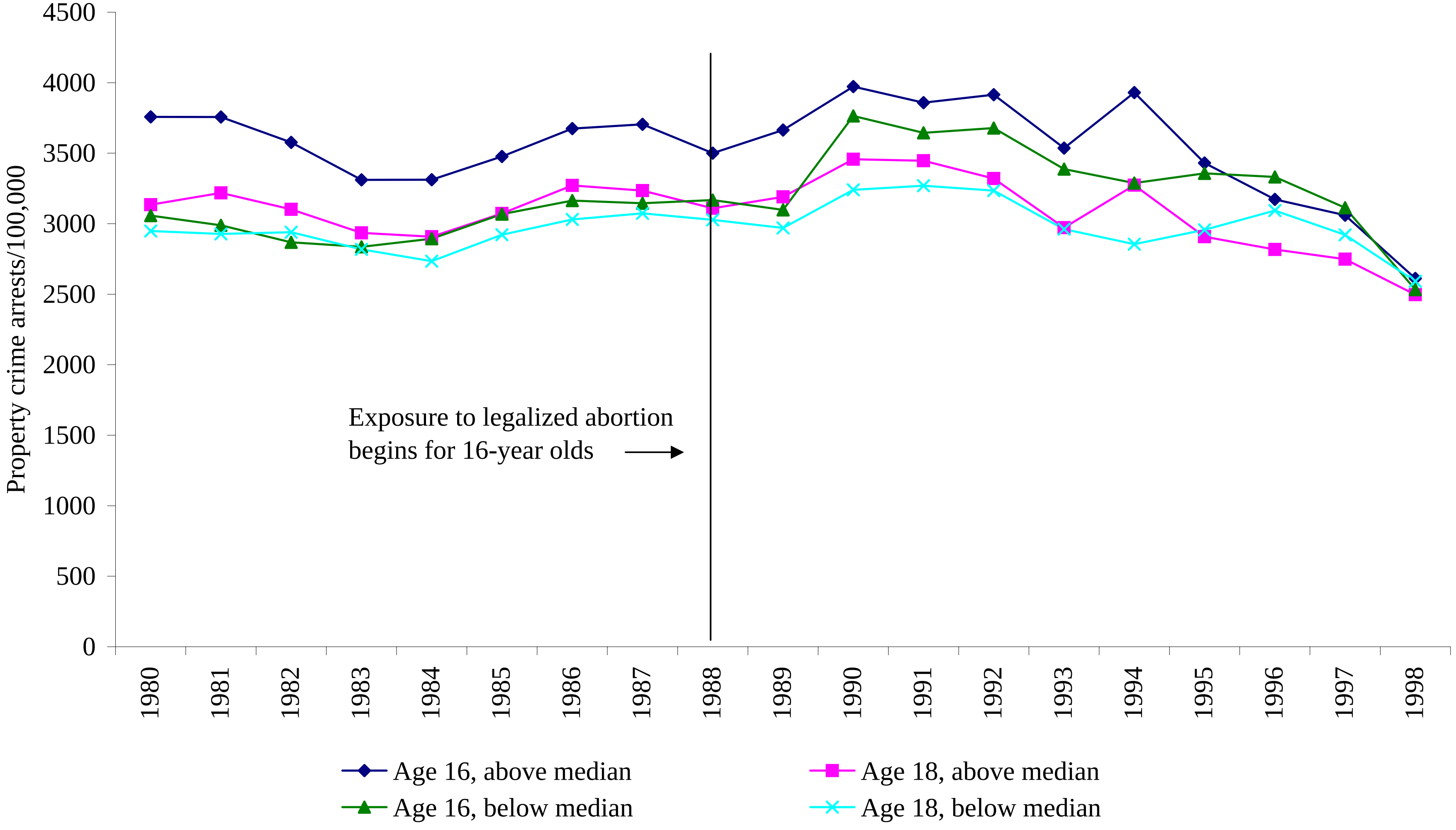

Table 1. Estimated Effects of Abortion Rate on Age-specific Arrest and Homicide Rates (in logs): Replication of Donohue and Levitt (2006)

	(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 (2006, Table 5) 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 6724 observations for violent crime arrests, 6730 for property crime arrests, 5715 for murder arrests and 5851 for the homicide rate. The sample in column (3) is limited to cohorts for which abortion data exist in Donohue and Levitt (2006). 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).

Table 2. Sensitivity of Abortion Rate Coefficient to Interactions of Age, State and Year Fixed Effects in Regressions of Age-Specific Crime and Arrest Rates (in logs)

	(1)	(2)	(3)	(4)	(5)
Dependent variable					
Ln violent crime arrest rate	0.018**	-0.031***	0.065***	0.071***	-0.021**
N=6724	(0.006)	(0.007)	(0.009)	(0.004)	(0.008)
R-squared	0.846	0.901	0.912	0.964	0.970
Ln property crime arrest rate	-0.021**	-0.003	-0.035***	0.038***	0.001
N=6730	(0.006)	(0.006)	(0.008)	(0.003)	(0.005)
R-squared	0.861	0.908	0.931	0.979	0.983
Ln murder arrest rate	0.002	-0.034*	0.051***	0.119***	-0.004
N=5715	(0.013)	(0.016)	(0.014)	(0.009)	(0.016)
R-squared	0.735	0.760	0.813	0.835	0.843
Ln homicide rate	-0.024	-0.079***	0.068***	0.125***	0.006
N=5851	(0.015)	(0.019)	(0.014)	(0.009)	(0.017)
R-squared	0.668	0.683	0.789	0.797	0.810
State	X				
State*Age		X		X	X
Year*Age	X	X	X		X
State*Year			X	X	X
# of Parameters	191	641	841	1174	1291

Each coefficient is from a separate regression. Standard errors are in parentheses and are adjusted for clustering by state and year of birth following Donohue and Levitt (2004, 2006). There are potentially 7140 observations given 14 years (1985-1998), 10 age groups (15-24) and 51 states. Samples are less due cells with zero observations or non-reporting. The number of parameters is the maximum that would be included if there were no missing data. The specifications in columns (1) and (2) are used by Donohue and Levitt (2001,2004). The specification is column (5) if from Donohue and Levitt (2006).
*** p.<.01; ** p<.05; * p<.10

Table 3. Age-period-cohort diagram of arrest rates for 15 to 24 year olds

	Year of Crime													
Year of Birth	**85**	**86**	**87**	**88**	**89**	**90**	**91**	**92**	**93**	**94**	**95**	**96**	**97**	**98**
70	15	16	17	18	19	20	21	22	23	24				
71		15	16	17	18	19	20	21	22	23	24			
72			15	16	17	18	19	20	21	22	23	24		
73				15	16	17	18	19	20	21	22	23	24	
74					15	16	17	18	19	20	21	22	23	24
75						15	16	17	18	19	20	21	22	23

Table 4. DDD Estimates of Changes in Log Homicide Rates and Log Arrest Rates in 45 States Following *Roe vs. Wade* for Birth Cohorts 1972-1975

Years	Exposed (Age Groups)	Unexposed (Age Groups)	Homicide Rate		Arrest Rates: Murder		Arrest Rates: Violent		Arrest Rates: Property	
(1)	*(2)*	*(3)*	*(4)*		*(5)*		*(6)*		*(7)*	
85-89	**13-14**	15-16	0.054	(0.214)	---	---	---	---	---	---
87-91	**15-16**	17-18	0.019	(0.135)	0.068	(0.164)	0.019	(0.062)	0.011	(0.036)
89-93	**17-18**	19-20	0.180	(0.098)	0.027	(0.086)	0.009	(0.046)	-0.008	(0.025)
91-95	**19-20**	21-22	-0.026	(0.083)	0.177	(0.084)*	0.029	(0.035)	-0.011	(0.028)
93-97	**21-22**	23-24	0.134	(0.123)	0.091	(0.127)	-0.024	(0.031)	-0.013	(0.033)
95-99	**23-24**	25-26	-0.047	(0.127)	---	---	---	---	---	---
97-01	**25-26**	27-28	0.092	(0.168)	---	---	---	---	---	---
	Simple average		0.058		0.091		0.008		-0.005	
	Weighted average		0.054		0.098		0.003		-0.007	

Each DDD estimate is from a separate regression (β_5 from equation (3) in the text). The sample is limited to cohorts born in 1972-1975 in the 45 states that legalized abortion with *Roe*. The states in which changes in the abortion rate between 1971 and 1973 exceeded the median change are the "exposed" states and those at or below the median are the comparison states (see Table 1A in the Appendix). Arrests include the years 1987-1998 and homicides 1985-2001. Homicides are from the Supplemental Homicide Reports and have not been weighted to reflect state totals as reported in the Uniform Crime Reports. The weighted average of the coefficients is weight by the inverse of their variances. Reports *p<.05; ** p<.01

Table 5. 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*

			Arrest Rates					
	Homicide Rate		Murder		Violent Crime		Property Crime	
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
Abortion rate	0.015	0.033	0.019	0.023	-0.029	0.04	-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	2009		1355		1618		1621	

Coefficients pertain to β from equation (1) in the text. The sample for homicides includes cohorts 1972-75, years 1985-01 and ages 13-27. The sample for arrests includes cohorts 1972-1975, years 1987-98 and ages 15 to 24. The abortion rate is the number of resident abortions per 1000 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 1971-1974; thus, crime rates in year t are regressed on the lagged abortion rate (AR) weighted as follows: 0.125*AR(t-a)+0.75*AR(t-a-1)+0.125*AR(t-a-2) where "a" is age in year t (See Donohue and Levitt 2006). Thus, the homicide rate of 17-year olds in 1990 is regressed on $.125*AR_{1973}+.75*AR_{1972}+.125*AR_{1971}$

*The adjusted abortion rate attempts to approximate the abortion rate by mother's state of birth. I create a 51x51 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 1985-1998 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.

Appendix Table 1. Abortion Ratios by State of Residence in 1971 and 1973 Ranked by the Change in Abortion Ratios between the Two Years.*

State (ranked by difference in abortion ratio 73-71: lowest to highest)	**Abortion ratio 1971**	**Abortion ratio 1973**	**Difference 1973-1971**
New Mexico	224	146	-78
West Virginia	29	24	-5
Louisiana	16	20	4
North Dakota	25	40	15
Mississippi	7	27	20
Oregon	212	237	25
Montana	35	61	26
Indiana	52	81	29
Utah	2	36	34
Arkansas	29	68	39
Tennessee	39	78	39
Kentucky	37	81	44
Idaho	2	48	46
Oklahoma	33	81	48
Iowa	63	116	53
Kansas	112	165	53
Wisconsin	74	130	56
South Carolina	39	95	56
Alabama	22	81	59
Wyoming	32	100	68
Nebraska	42	113	71
Georgia	53	129	76
New Hampshire	96	173	77
Average for below median	55	93	37
Weighted Average for below median	51	92	41
Maine	75	159	84
South Dakota	14	103	89
North Carolina	64	160	96
Ohio	71	169	98
Minnesota	54	155	101
Missouri	59	160	101

Colorado	115	221	106
Texas	11	127	116
Delaware	167	286	119
Arizona	11	135	124
Pennsylvania	113	243	130
Virginia	84	222	138
Illinois	81	225	144
Florida	79	227	148
Vermont	96	250	154
Rhode Island	113	269	156
Connecticut	174	342	168
Michigan	88	266	178
Massachusetts	152	360	208
New Jersey	188	431	243
Maryland	154	418	264
Nevada	4	352	348
Average for above median	89	240	151
Weighted Average for above median	86	229	142
Average for 45 states	72	165	93
Weighted Average for 45 states	74	180	106

*Abortion rates are the ratio of abortions by state of residence per 1000 births. Data on resident abortions in 1971 are from Table 5 of the Centers for Disease Control (1972) and resident abortion rates in 1973 are from Stanley Henshaw of the Alan Guttmacher Institute.

Appendix Table 2. 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	0.06	0.022	-0.007
	(0.039)	(0.037)	[0.042]	[0.042]
State-trends	No	Yes	No	Yes
N	2559	2559	1621	1621

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 8 in the text.