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A REVIEW

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### **ABSTRACT**

Ten years have passed since John Donohue and Steven Levitt initially proposed that legalized abortion played a major role in the dramatic decline in crime during the 1990s. Criminologists largely dismiss the association because simple plots of age-specific crime rates are inconsistent with a large cohort affect following the legalization of abortion. Economists, on the other hand, have corrected mistakes in the original analyses, added new data, offered alternative tests and tried to replicate the association in other countries. Donohue and Levitt have responded to each challenge with more data and additional regressions. Making sense of the dueling econometrics has proven difficult for even the most seasoned empiricists. In this paper I review the evidence. I argue that the most straightforward test given available data involves age-specific arrest and homicide rates regressed on lagged abortion rates in the 1970s or indicators of abortion legalization in 1970 and 1973. Such models provide little support for the Donohue and Levitt hypothesis in either the US or the United Kingdom.

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*“Unwantedness leads to high crime; abortion leads to less unwantedness; abortion leads to less crime.”* Steven Levitt<sup>1</sup>

*“The academic debate will surely rage on for years, as studies and counterstudies are issued and enterprising young professors tie their hopes for tenure to either proving or disproving such a link.”* John W. Whitehead The Washington Times, June 28, 2001

Ten years have passed since the *Chicago Tribune* published its front-page story on the relationship between legalized abortion and crime.<sup>2</sup> In a then unpublished manuscript Steven Levitt and John Donohue III contended that the legalization of abortion in the 1970s could explain as much as 50 percent of the dramatic decline in crime in the 1990s. Almost two years later the manuscript was the lead article in the *Quarterly Journal of Economics*. A flurry of press reports followed, but it was not until the publication of Steven Levitt’s and Steven Dubner’s phenomenally successful book, *Freakonomics*, that the association between legalized abortion and crime became well-known among non-academics.<sup>3</sup>

The widespread familiarity of the abortion and crime hypothesis derives from several factors. First, the hypothesis is exceedingly controversial. Individually, abortion and crime generate impassioned responses, but linking them proved incendiary, which heightened interest in the media. Second, the conceptual hypothesis is readily accessible and easily expressed without equations or jargon. The notion that fewer unwanted children result in fewer psychologically injured and crime-prone adults strikes many as common sense. Third, the evidence, as presented, is similarly transparent. *Roe v. Wade* was decided in 1973. The number

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<sup>1</sup> Stephen J. Dubner, “The Probability that a Real-Estate Agent is Cheating You (and Other Riddles of Modern Life)” *New York Times Magazine*, August 3, 2003.

<sup>2</sup> Karen Brandon, “Abortion, Reduced Crime Linked.” *The Chicago Tribune* August 8, 1999, p. 1.

<sup>3</sup> Levitt, Steven and Steven Dubner. 2005. Freakonomics: A Rogue Economist Explores the Hidden Side of Everything. New York: HarperCollins.

of legal abortions grew so rapidly that by 1980 there were over 1.5 million abortions as compared to 3.6 million births. Since abortion is more prevalent among poor, unmarried and teen mothers, its legalization decreased large numbers of births to families at greater risk of raising potential criminals. The result is a major decline in crime 15 to 25 years after *Roe* as these more “wanted” cohorts reached the peak crime ages.

Among academics, however, the notion that legalized abortion had a major impact on crime has been met with greater skepticism. Criminologists describe the association as novel or interesting but are dismissive. They make the first-order observation that the timing between changes in age-specific crime rates and exposure to legalized abortion is inconsistent with the Donohue and Levitt story. Economists on the other hand have gone further. They have re-estimated Donohue and Levitt’s regressions, altered the specifications, questioned the identification strategy and tested the association in other countries. Donohue and Levitt have responded with more data and further analyses. Making sense of the competing regressions can be difficult even for the most experienced economists.<sup>4</sup> But what is often obscured in the statistical detail is the audacity of the claim: a causal relationship between abortion and crime based on an association between state aggregates separated by 20 years. Despite the simplicity of the empirics, many have found Donohue and Levitt’s evidence to be compelling.

The association between abortion and crime, however, is more than an academic debate. The popularity of *Freakonomics* insures its notoriety, but a causal relationship between abortion and crime has far-reaching implications for policy. First is the sheer numbers of lives saved. Homicides fell from 24,703 in 1991 to 15,586 in 2000. If the legalization of abortion is

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<sup>4</sup> Daniel Hammermesh, a well-known and highly regarded labor economist said to Stephen Dubner, “ I’ve gone over this paper in draft, in its printed version, at great length, and for the life of me I can’t see anything wrong with it. On the other hand, I don’t believe a word of it” (Dubner 2003)

responsible for half this decline, as Donohue and Levitt suggest, then over 21,000 murders were averted over the decade. But the potential welfare gains go beyond crime. If abortion increases the “wantedness” of cohorts, which in turn lowers crime, then we also should observe improvements across a panoply of outcomes: teen pregnancy, school achievement, drug addiction, depression, divorce, etc. Moreover, abortion represents just one form of fertility control. If the driving force is unwanted births averted, then we should be able to achieve many of these gains by improving family planning generally. Thus, increases in the use of contraception or the promotion of abstinence among teens should have similarly positive effects. Put simply, the policy implications of a causal relationship between abortion and crime are so far-reaching and yet so controversial, that anything less than a thorough vetting borders on social science negligence.

Literally thousands of regressions relating crime to abortion have been estimated in the course of the debate.<sup>5</sup> The statistical work, however, may have done more to obfuscate what appears obvious from time-series of age-specific crime rates: cohorts exposed to legalized abortion committed crimes at roughly the same rate as those who were unexposed. Donohue and Levitt never present this first-order evidence. They contend that national time-series of age-specific crime rates are uninformative or that the violence associated with the development and spread of crack cocaine markets obscures the positive effect of abortion. And yet, they argue that the effect of abortion is large—so large in fact that it can explain half of the dramatic decline in homicide rates between 1991 and 1997. They never address why effects this large are not more evident in the age-specific crime rates.

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<sup>5</sup> Lott and Whitley (2007) write, “Of the over 6,000 regressions that we estimated here, only one implied even a small reduction in the murder rate” (p. 323).

I begin this review by describing the conceptual foundation of the Donohue and Levitt (hereafter, DL) hypothesis. I assess the evidence that links unwanted childbearing and adverse outcomes. I next present DL's empirical model and results. I then describe the published criticisms of DL's work and DL's responses. In the final section I examine the association between abortion and crime in other countries. I conclude that the most credible tests of abortion and crime in both the US and abroad do not support the DL hypothesis. In the end, the simple time-series plots of age-specific arrest and homicide rates tell the story: the crime rates of cohorts born just before abortion was legalized follow the same time path as the crime rates of those born just afterwards. There is no discontinuity in crime rates associated with the early legalization of abortion in New York or California, nor is there a break in crime rates in the rest of the states after *Roe*. Donohue and Levitt's presentation of innumerable regression estimates divert attention from, rather than account for, the absence of this most basic evidence.

### **I. Conceptual Framework and Supporting Evidence**

A simple place to start is with the following identity:

$$(1) \quad CR = k * CR_k + (1 - k) * CR_{(1-k)}$$

$CR_t$  is the total crime rate. It is a weighted average of crime rates for high risk ( $k$ ) and low risk ( $1-k$ ) groups. Examples of high risk groups would be individuals raised in poverty or in single-parent households. Abortion can affect the crime rate in two ways. First, abortion may lower  $k$ , if, for example, teens or unmarried women have fewer children after abortion is legalized.

Abortion may also affect crime by lowering the criminogenic propensity of individuals--a fall in  $CR_k$  or  $CR_{(1-k)}$  or both. Children who are more wanted may receive more emotional support and greater investments in child health and human capital than their less wanted counterparts.

Donohue and Levitt refer to these pathways as selection effects. A third way in which abortion may lower crime is by reducing the size of a cohort. As these cohorts reach their teens and early twenties, there will be fewer young males in the peak crime ages and less crime. DL refer to this pathway as the cohort-size effect.<sup>6</sup>

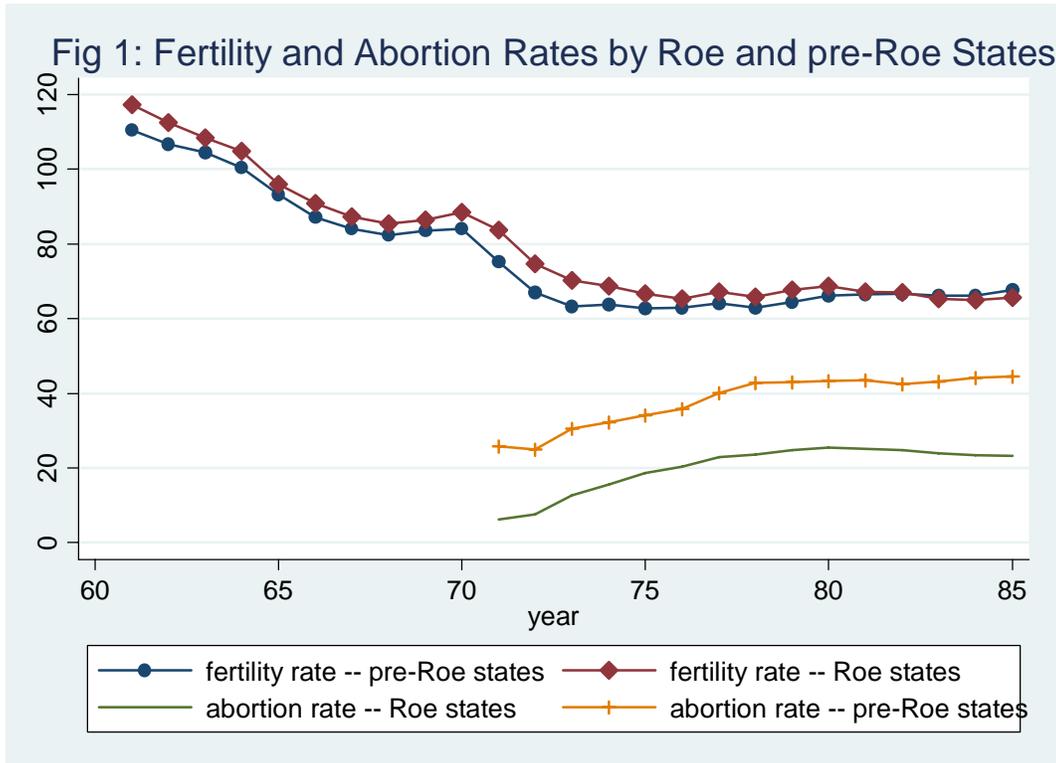
The conceptual association between abortion and crime relies on two relationships. First, an increase in the abortion rate reduces the proportion of mistimed and unwanted births.<sup>7</sup> Second, individuals from pregnancies that were mistimed or unwanted at birth have a greater propensity to commit crime as adults. Donohue and Levitt provide no direct evidence of either association, but rely on published studies. For instance, research in the US demonstrates that the early legalization of abortion around 1970 in Alaska, California, Hawaii, New York and Washington is associated with a 5 percent decrease in fertility rates relative to states that legalized abortion with Roe in 1973 (Levine et al. 1999; Angrist and Evans 1999). There is also evidence that the decline in fertility was greater among teens, unmarried and nonwhite women (Sklar and Berkov 1974; Joyce and Mocan 1990; Levine et al. 1999; Angrist and Evans 1999). Most analysts have interpreted the relative decline in fertility rates in the five pre-Roe states as a decrease in unwanted childbearing. However, it is difficult to show that state abortion rates are inversely

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<sup>6</sup> Surprisingly, Donohue and Levitt (2001) make no attempt to distinguish selection from cohort effects in their state-year regressions of total crime rates. Changes in cohort size would eventually affect the age structure, which is typically measured by including the proportion of the population in various age groups as a right-hand-side determinant of the crime rate. Donohue and Levitt (2001) never include the age structure in the model. However, in response to Foote and Goetz (2008) Donohue and Levitt (2008) regress the *number* of age-specific crimes and arrests (in logs) on the abortion rate. They argue that the coefficient on the abortion rate in these regressions captures both the cohort size effect as well as selection effects. They then estimate regressions of per capita arrests and contend that the coefficient on the abortion rates captures only selection effects. The difference in the abortion coefficient between these two regressions is the cohort size effect. Critics argue that a specification of log arrests on the abortion rates lacks justification and leads to spurious results (Foote and Goetz 2008; Joyce 2009). I discuss this point in more detail below.

<sup>7</sup> Demographers divide unintended birth into two categories: those from pregnancies that were mistimed as compared to pregnancies that were unwanted. The distinction is based on surveys generally conducted among women who have recently given birth. Women are asked if they had wanted to become pregnant and if not, had the pregnancy been mistimed or had they not wanted to become pregnant ever again.

related to state fertility rates in the period after Roe. Consider the fertility and abortion rates in Figure 1. Each series is stratified by whether states legalized abortion before (pre-Roe states) or with *Roe* (Roe states).



There is a large difference in abortion rates between pre-Roe and Roe states that persists well after 1973 with no corresponding difference in fertility rates. One would expect higher abortion rates to be associated with lower fertility rates if, in fact, abortion reduced the rate of unwanted childbearing. One explanation is that greater acceptance of abortion and greater availability of abortion services in the pre-Roe states lead to more sex and more pregnancies, but not necessarily fewer unwanted births (Klick and Stratmann 2003).<sup>8</sup> The lack of an association

<sup>8</sup> Another noteworthy detail from Figure 1 is that fertility rates had been trending downwards through much of the 1960s. Indeed, the absolute decline in fertility rates between 1960 and 1969 exceeds the decline from 1970 to 1979. The possible increase in the “wantedness” of birth cohorts in the 1960s is never addressed by DL, even though the contraceptive pill and illegal abortion are likely to have had important effects on fertility. Marvell and Moody

between abortion and fertility is relevant to the abortion and crime debate because DL only use data on abortions from 1973 to 1983. If there is no association between state abortion and state fertility rates, then abortion improves well-being by lowering the prevalence of mistimed as opposed to unwanted births. The association between mistimed births and adverse child development, however, is much weaker than the association between unwanted births and child well-being (Joyce, Kaestner, and Korenman 2000).

Some have argued that legalized abortion actually worsened the situation of some women. Akerlof et al. (1996) develop a model in which the legalization of abortion leads to a rise in out-of-wedlock childbearing, since it frees men from having to legitimize a pre-marital conception. The model fits the basic time-series well. The ratio of births to unmarried women rose from 7.7 percent of births to 17.8 percent between 1965 and 1980 (Ventura et al. 1995). Lott and Whitley (2007) use this argument to explain why abortion is positively related to homicide rates in direct contradiction to DL. In other words, the notion that states with greater abortion rates have lower rates of unwanted childbearing is more ambiguous than DL suggest.

The second relationship that links abortion to crime is the association between unwanted childbearing and criminogenic adults. Again the literature is far from definitive. The most widely cited studies are from Europe in which the outcomes of children from pregnancies in which the women were denied abortions are compared to children from pregnancies that were wanted (David et al. 1988). The general finding is that children from unwanted pregnancies do worse in school, have less stable employment, and more mental health problems as teens and young adults than their counterparts who were wanted from conceptions. The internal validity of

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(1991) show that the proportion of the population 15 to 24 years of age fell every year from 1977-1990, the result of declining fertility rates in 1960s. Certainly this would qualify as a “cohort effect” as defined by DL. However, the time-series relationship between the age structure and crime rates over this period is ambiguous, which suggest that the “cohort effect” may not be important.

these studies rests critically on the comparison group. Children who were unwanted are compared to children from families in which the child was wanted. Comparisons between children from different families are vulnerable to confounding from hard to measure differences in family environment. It is noteworthy that the negative effects of “unwantedness” in the Prague cohort—the most widely cited of this type—were largely eliminated when the authors used sibling differences to net out family fixed effects (Kubicka et al. 1995).

In the US, measures of unintended childbearing are based largely on surveys of women who give birth (Brown and Eisenberg 1995). Women are asked after the birth if they had wanted to become pregnant and if not, had the pregnancy been mistimed or had they not wanted to become pregnant ever again. The retrospective aspect of the question is problematic since attitudes towards pregnancy and birth can change between conception and the postpartum interview. This ambiguity has led economists to rely on abortion as the most clear-cut indicator of an unwanted pregnancy. The difficulty is that there can be no direct association between the aborted fetus and subsequent outcomes. This creates a profound problem of missing data (DiNardo 2007). Economists, as one might suspect, have not been deterred. Grossman and Joyce (1990), for example, treat births as a censored sample of pregnancies. They use a Heckman-correction procedure to estimate the expected birth weight of pregnancies that were aborted had they not been terminated. They find that the mean birth weight of black births in New York City would have been 60 grams lower had all pregnancies to black women been carried to term. The analysis was a novel application of a censored regression, but the identification restrictions were largely *ad hoc* and would not be credible today.

Gruber, Levine and Staiger (1999) also estimate the effect of abortion on the child not born by associating marginal changes in child outcomes with marginal changes in births. They

use abortion legalization as an instrument for birth rates so as to isolate variation associated with the child not born (the “marginal child”). Their estimates suggest that children who were not born because abortion became legal were more likely to be low birth weight, experience poverty and be on welfare. In a follow-up study that uses the 2000 census, the authors report inconsistent associations between abortion and adult well-being. They find that the marginal child as an adult is more likely to be on welfare and less likely to graduate from college, but she is no more likely to live in poverty, to drop out of high school, to be unemployed or to have been incarcerated (Ananat et al. 2009).

Angrist and Evans (1999) also exploit the exogenous variation induced by the legalization of abortion in the pre-Roe states. They find significant declines in black fertility rates and reduced-form effects of legalization on the schooling of black women. They find small effects on teen fertility among whites and no effects on schooling. Angrist and Evans (1999) also find no effect of national legalization of abortion in 1973 on the fertility rates of either white or black teens. They conclude that changes in downstream outcomes may be too small to measure statistically, even with relatively large changes in black teen fertility rates associated the pre-Roe legalization of abortion.

Charles and Stephens (2006) reach different conclusions. They find that the early legalization of abortion is associated with very large decreases in the illicit substance use. Among the exposed cohorts, they find that illicit drug use would have been 35 to 57 percentage points higher among what Gruber, Levine and Staiger (1999) term the “marginal child” or the fetus not born. These are huge effects given a mean rate of use of 10 percent—an order of magnitude greater than the changes in child well-being found by Gruber, Levine and Staiger (1999). Moreover, Charles and Stephens only have a single age group. They can’t exploit age

differences to estimate the triple difference strategy of Joyce (2004, 2009) or the inclusion of state-year effects as used by Donohue and Levitt (2008) and Foote and Goetz (2008). Instead they add linear state-specific trends to control for period effects, which reduce their estimates by half. But trends in illicit drug use are clearly curvilinear and thus, more complete controls for state-effects might have reduced their estimates even further.<sup>9</sup> Finally, Charles and Stephens regress illicit drug use on lagged abortion rates, a specification more consistent with DL. Although the coefficient on the abortion rate is negative, the t-ratio is less than one.

Hotz, McElroy and Sanders (2005) use spontaneous abortion among teens to generate the counterfactual for teens that give birth. They compare the outcomes of four groups of teens: those who give birth; those who became pregnant but who spontaneously abort; teens who terminate their pregnancies voluntarily; and teens who don't become pregnant. They find that many of the consequences of teen childbearing are short lived and gone by age 28. There are no differences in completed schooling, earnings, or the number of children by age 28 between teen mothers and teens who were pregnant but who delayed birth until after 18 years of age because of a miscarriage. Moreover, the characteristics of teens who abort are more similar to those who never become pregnant than those who are pregnant and either carry to term or spontaneously aborted. Hotz et al. (2005) conclude that consequences of teen childbearing, or the gains to delayed teen childbearing, among latent teen mothers have been greatly overestimated.

There is little debate that children who are abused, children from unstable families and children in poverty are at greater risk of committing crimes as teens and adults than children from more nurturing settings. Donohue and Levitt's argument is that abortion reduces the likelihood that children grow up in such circumstances. The selected review of the literature

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<sup>9</sup> See Figure 1 in Charles and Stephens (2006).

just presented is mixed. There is convincing evidence that early legalization of abortion lead to a decrease in fertility. There is some evidence that the circumstances and subsequent outcomes of cohorts exposed the early legalization of abortion improved. There is less evidence that the widespread diffusion of abortion services had a similar impact. Indeed, the model by Akerlof, Yellen and Katz (1996) offers a compelling argument that abortion legalization contributed to the rise in out-of-wedlock childbearing, which may have worsened circumstances of children born to women unwilling to abort.

### **Outline of a Structural Model**

The focus on “wantedness” and child well-being, however, has obscured the larger issue that abortion is but one of a series of decisions related to sexual activity, contraception, marriage and labor supply. Donohue and Levitt treat the lagged abortion rate as an exogenously determined proxy for unwanted births averted. But unintended childbearing is imbedded in a complex structural model.<sup>10</sup> To illustrate, consider the stylized model below.

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<sup>10</sup> Levitt acknowledges the complexity in a posting on his blog. He writes, “The total number of unwanted births is equal to the number of unwanted conceptions times the fraction of women who don’t abort those unwanted pregnancies. Let’s assume that a woman who has conceived an unwanted pregnancy is equally likely to get an abortion in the 1990s or the 1970s, but that because of fear of AIDS, people use condoms and have fewer unwanted conceptions. With fewer unwanted conceptions, there will be both fewer abortions and fewer unwanted births. In that case, lower 1990s abortions are a harbinger of future crime reductions, not future crime increases” (Posted June 15, 2006 at <http://freakonomics.blogs.nytimes.com/2006/06/15/steve-sailer-asks-an-excellent-question/>). The argument goes both ways, of course. Assume the 1970s were a time of changing mores about sex which lead to increased sexual activity. According to Levitt’s statement, the demand for abortion shifts to the right resulting in more abortions and more unwanted births, consistent with Akerlof et al. (1996) and Lott and Whitley (2007).

- (2)  $C_t = \alpha_0 + \alpha_1 UW_{t-a}$
- (3)  $UW_{t-a} = \beta_0 + \beta_1 AR_{t-a-1} + \beta_2 Pill_{t-a-1} + \beta_3 S_{t-a-1}$
- (4)  $AR_{t-a-1} = \lambda_0 + \lambda_1 Pill_{t-a-1} + \lambda_2 S_{t-a-1}$
- (5)  $Pill_{t-a-1} = \phi_0 + \phi_1 AR_{t-a-1} + \phi_2 S_{t-a-1}$
- (6)  $S_{t-a-1} = \kappa_0 + \kappa_1 AR_{t-a-1} + \kappa_2 Pill$
- (7)  $C_t = \gamma_0 + \gamma_1 PA_{t-a-1} + PP_{t-a-1}$
- (8)  $AR_{t-a-1} = c_0 + c_1 PA_{t-a-1} + PP_{t-a-1}$

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  $t-a$ . For simplification, ignore all other determinants of crime except unwanted childbearing. Let  $AR_{t-a-1}$  be the abortion rate in year  $t-a-1$ , let  $Pill_{t-a-1}$  be the rate of pill use at time  $t-a-1$ ; let  $S_{t-a-1}$  be the rate of sexual activity; let  $PA_{t-a-1}$  be the price of abortion in year  $t-a-1$ , let  $PP$  be the price of the pill. Note that I have ignored equations for marriage and labor force participation as well prices associated with birthing and work. 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. A more realistic reduced form is probably equation (7), but even this is problematic unless the price of abortion,  $PA_{t-a-1}$ , varies exogenously and  $PP_{t-a-1}$  is assumed to remain unchanged. However, sexual mores and access to the pill among unmarried women were changing rapidly in the early 1970s, which further complicates the model. Another complication is that the availability of abortion services is also endogenous. Soon after legalization, markets for abortion services developed. Abortion providers located where demand was strongest and attitudes most hospitable.

The upshot is that designing a credible test of whether legalized abortion lowered crime is extremely challenging. Variation in abortion over time may not be a good estimate of changes in unwanted childbearing. Indeed, Levitt argues that only movements along the demand curve for abortion correctly identify changes in unwanted childbearing. Shifts in the demand curve for abortion can increase or decrease the proportion of unwanted births (see footnote 10). Thus, any model that purports to link abortion to crime must identify changes in abortion induced by shifts in the supply curve of abortion services only. Donohue and Levitt's identification strategy never addresses this issue. As I show in the next section, they assume that all variation in abortion between 1973 and 1983 represents movement along the demand curve.

## **II. Donohue and Levitt's empirical strategy**

Donohue and Levitt present five pieces of evidence consistent with an association between legalized abortion and crime (DL 2001; 2004).<sup>11</sup> Two involve national time series. The first shows that total crime rates, not age-specific crime rates, fell earlier in the pre-Roe states relative to states that legalized abortion with Roe. The second demonstrates that crime rates fell further in states with greater abortion rates in 1970s. The next three pieces of evidence involve regression analysis, but their main results involve regressions of total crime rates at the state-year level from 1985-1997 on a proxy for cohort exposure to abortion also at the state-year

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<sup>11</sup> Steven Levitt has made extensive use of the internet to promote his book and share his thoughts. In a three-part video presentation which is part of his audio book, Levitt describes how they came to the hypothesis that abortion lowered crime and why he believes the association is causal. Levitt acknowledges that no one piece of evidence relating abortion and crime is "completely convincing." Donohue and he rely on a "collage" of evidence that together convinces them that abortion did indeed lower crime (<http://freakonomics.blogs.nytimes.com/2007/09/13/more-video-on-abortioncrime-a-collage-of-evidence/>).

level. The latter they term the “effective abortion rate” (EAR).<sup>12</sup> It is critical to emphasize that DL cannot identify cohorts with these state-year regressions because the crime rate is not age-specific. The EAR is an attempt to overcome this limitation. To illustrate, consider equation (9)

$$(9) \quad EAR_{st} = \sum_a Abortion_{st-sa} * (Arrests_a / Arrests_{total})$$

Subscript “a” indexes age, “s” state and “t” year. The abortion rate in 1985 in state “s,” for example, is the sum of state abortion rates “a” years ago where “a” varies from 10 to 29. Thus, the EAR for murder in 1985 is the number of abortions per birth (the abortion rate) in 1975 weighted by the proportion of arrests for murder of 10-year olds plus the abortion rate in 1974 weighted by proportion of arrests for murder of 11-year olds plus the abortion rate in 1973 weighted by the proportion of arrests for murder of 12-year olds and so on up to age 29. The last term of this sum for the EAR in 1985 is the abortion rate in 1956 (1985-29) weighted by the proportion of arrests for murder to 29-year olds. The arrest weights are based on national counts in 1985 and are fixed for all years. Donohue and Levitt use no data on abortions prior to 1973 and assume the abortion rate to be zero.<sup>13</sup> Thus, the EAR is zero or near zero for period 1985-1990 because either the abortion rate is zero for most cohorts or the proportion of arrests to a particular age group is inconsequential.<sup>14</sup> With the EAR in hand, DL estimate in the following:

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<sup>12</sup> Donohue and Levitt (2001, 2004, 2008) 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.

<sup>13</sup> Actually, they backcast abortions in 1973 to 1970 for the 5 states that legalized abortion prior to Roe. The other 45 states are assumed to have zero abortion rates. As Joyce (2004, 2009) and Lott and Whitley (2007) show abortion was not zero in the other 45 states prior to Roe. Moreover, the rate of illegal abortion was also not zero prior to Roe. Their assumption that there were no abortions prior to 1973, whether legal or illegal, implies that there was no variation in unwanted childbearing prior to 1973, an assumption clearly at odds with the decline in fertility rates in 1960s (see Figure 1).

<sup>14</sup> The abortion rate was never zero prior to legalization. Tietze (1973), for instance, estimates that two-thirds of all illegal abortions in New York City were replaced by legal abortions in the first year of legalization.

$$(10) \quad CR_{st} = \beta * EAR_{st} + \mathbf{X}\boldsymbol{\alpha} + \lambda_s + \tau_t$$

in which CR is the state-year crime rate,  $\mathbf{X}$  is a vector of other determinants,  $\lambda$  and  $\tau$  capture state and year fixed effects. Donohue and Levitt (2004) estimate the coefficient on  $\beta$  to be  $-0.166$  (t-ratio = 3.01) when the dependent variable is the murder rate.<sup>15</sup> An increase in the EAR of 100, approximately one standard deviation in the EAR in 1997, is associated with a 16.6 percent decline in the murder rate. Since the murder rate fell by 31 percent between 1991 and 1997, legalized abortion can account for over half the decline. Regressions of the violent crime rate and the property crime rate on the respective EARs yield roughly similar results. The coefficients of the EAR are statistically significant and insensitive to the inclusion of the other determinants ( $\mathbf{X}$ ) (see Tables IV and V in DL 2001 and Table 1 in DL 2004). These are the results that are used by Donohue and Levitt when describing their findings to other academics and the general public.

At first, the EAR may seem a clever way to reduce what should be an age-period-cohort analysis to a pooled time-series, cross-section of state aggregates. Yet, it is impossible to credibly identify a cohort effect without age-specific crime rates. Thus, the value of these regressions as a causal test of abortion and crime is questionable (DiNardo 2007; Foote and Goetz 2008; Joyce 2009). Indeed, it is not clear that DL's conclusions would have been as well received in the research community if the statistical association between abortion and crime had been limited to regressions of total crime rates on the EAR. Thus, DL bolster their argument

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<sup>15</sup> See Table 1 in Donohue and Levitt (2004). This coefficient is from a regression in which the EAR uses abortions by state of residence instead of abortion by state of occurrence. Joyce (2004) had criticized DL for measuring abortion by state of occurrence.

with an analysis of age-specific arrests as a function of lagged abortion rates.<sup>16</sup> The basic specification can be written as follows:

$$(11) \quad \text{Ln}C_{ajt} = \beta A_{jt-a-1} + A_a + \chi_j + \lambda_t + e_{ajt}$$

where  $C_{ajt}$  is the natural logarithm of arrests or homicides for age group  $a$ , in state  $j$ , and year  $t$ ;  $A_{jt-a-1}$  is the state abortion rate in year  $t-a-1$ . There are fixed effects for Age ( $A$ ), states ( $\chi$ ) and year ( $\lambda_t$ ). Thus, arrests of 24-year olds in 1990 in state  $j$ , for example, are correlated with the abortion rate in state  $j$  in 1965 ( $t-24-1$ ).<sup>17</sup> The strength of the analysis is that each birth cohort is associated with the abortion rate that existed roughly the year in which the cohort was *in utero*. This represents a major improvement over regressions of the total crime rate on the EAR, since the link between the “exposure” (legalized abortion) and the outcome (the crime rate) is more direct.

Data on arrests by single year of age are available for individuals 15 to 24 years of age from FBI’s Uniform Crime Reports. Donohue and Levitt (2001, 2004, 2008) analyze arrests for violent crime (murder and non-negligent homicide, forcible rape, aggravated assault and robbery) and property crime (burglary, larceny-theft, motor vehicle theft and arson) by single year age for the years 1985-1996. There are potentially 6120 cells by age, state and year. Results from these regressions are consistent with the DL hypothesis (Donohue and Levitt 2001, Table 7). Specifically, an increase in the abortion rate of 100 per 1000 live births would lower arrests for violent crime by 2.8 percent (t-ratio -7.0) and arrests for property crime by 4.0 percent (t-ratio = -10.0). In an effort to scale the results, DL multiply the coefficients by 350, which

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<sup>16</sup> Donohue and Levitt’s 1999 manuscript on abortion and crime that was featured in the **Chicago Tribune** did not include an analysis of age-specific arrest rates. The latter was added to the article that was published in the *Quarterly Journal of Economics*.

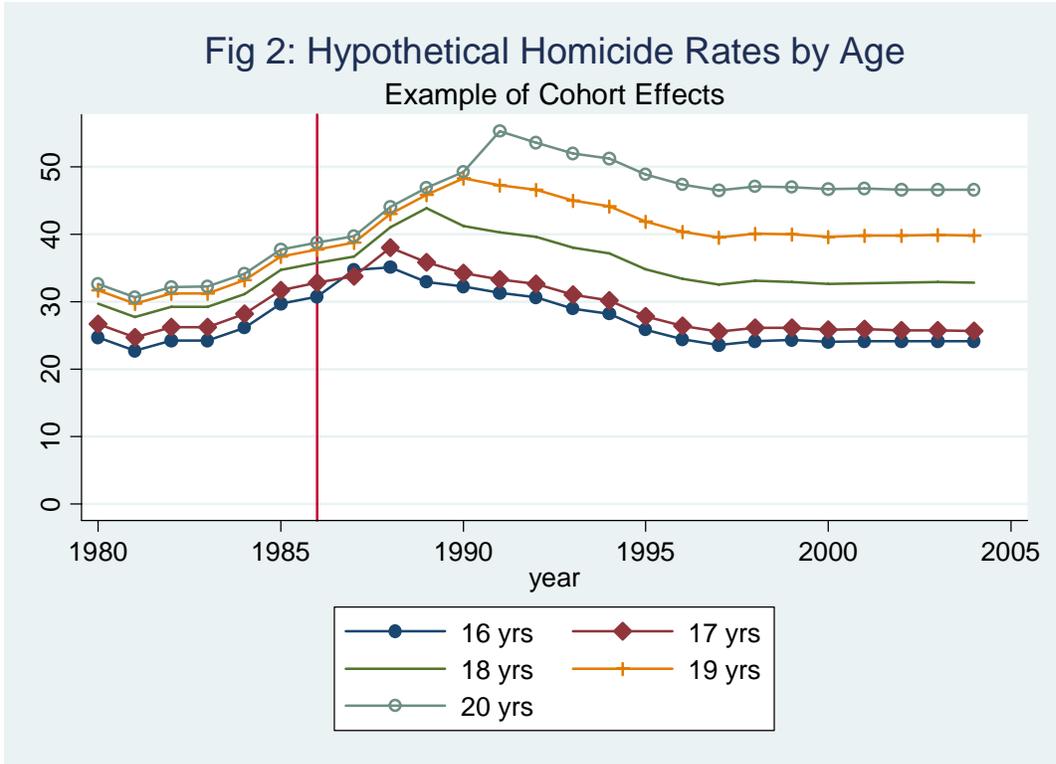
<sup>17</sup> The lag structure of the abortion rate is modified slightly in Donohue and Levitt (2008).

represents the difference in the abortion rate between states in the upper and lower tercile of abortion frequency (Donohue and Levitt 2001, p. 412). As such, abortion is associated with a decline of between 10 and 14 percent in age-specific arrest rates.<sup>18</sup>

The results from age-specific arrests provided important support for the state-year regressions of total crime rates. However, a more transparent presentation of the data should have begun with simple time-series plots of age-specific arrest and crime rates. Such figures are at odds with the timing of legalization and the downturn in crime. Critics of the DL have raised this point repeatedly, but it's worth expanding upon (Fox 2000; Joyce 2004, 2009; Lott and Whitley 2007; Cook and Laub 2002; Rosenfeld 2004; Zimring 2006). Donohue and Levitt's are trying to identify cohort effects associated with legalized abortion amidst profound period and age effects that were evident in the 1980s and 1990s. Fortunately, strong cohort effects present in a distinct manner. To illustrate, consider a hypothetical series of murder rates by single-year of age for individuals 16 to 20 years (Figure 2). Assume the rates pertain to the states that legalized abortion in 1970, three years prior to *Roe*. The vertical line represents the point at

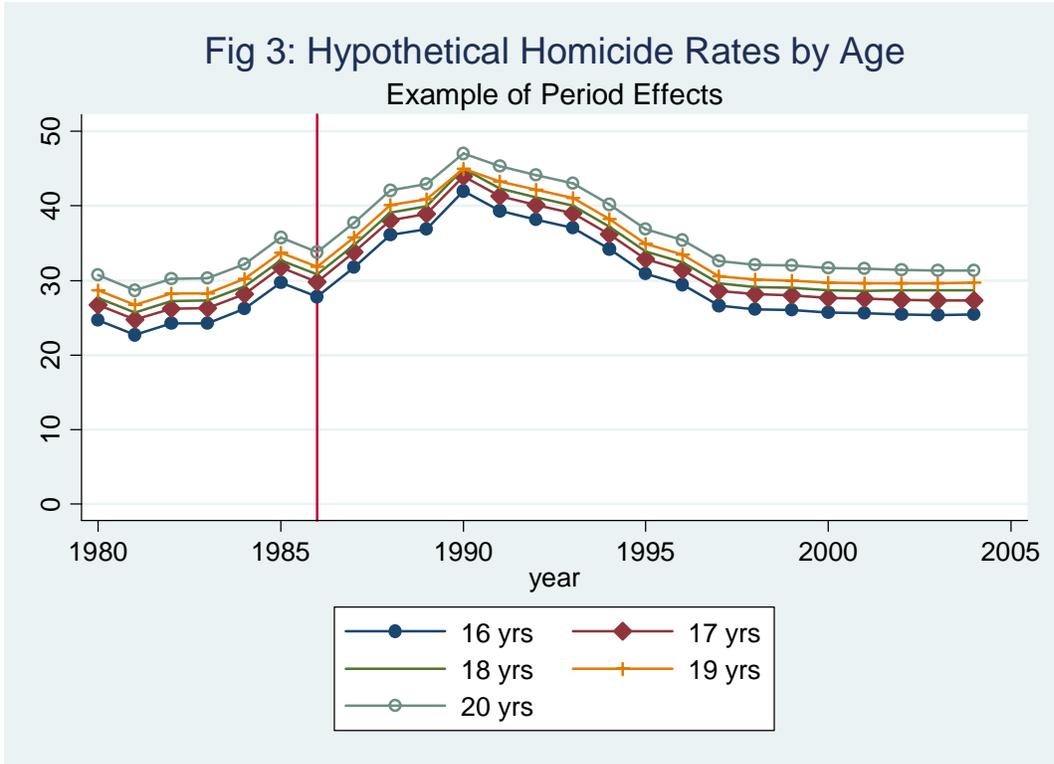
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<sup>18</sup> The scaling is overly generous. No cohort was ever exposed to a change in abortion rate of 350 per 1000 live births. The estimates from regressions of age-specific arrests are based on within-state changes in abortion and arrests. As Joyce (2009) shows, the largest increase in abortion rates occurred between 1972 and 1974. Joyce estimates the change at approximately 150 abortions per 1000 live births in the states that legalized abortion with *Roe*. Cohorts born later experienced smaller changes. If I use 150 as a generous scaling, then abortion is associated with decreases of 4 to 6 percent in arrests.



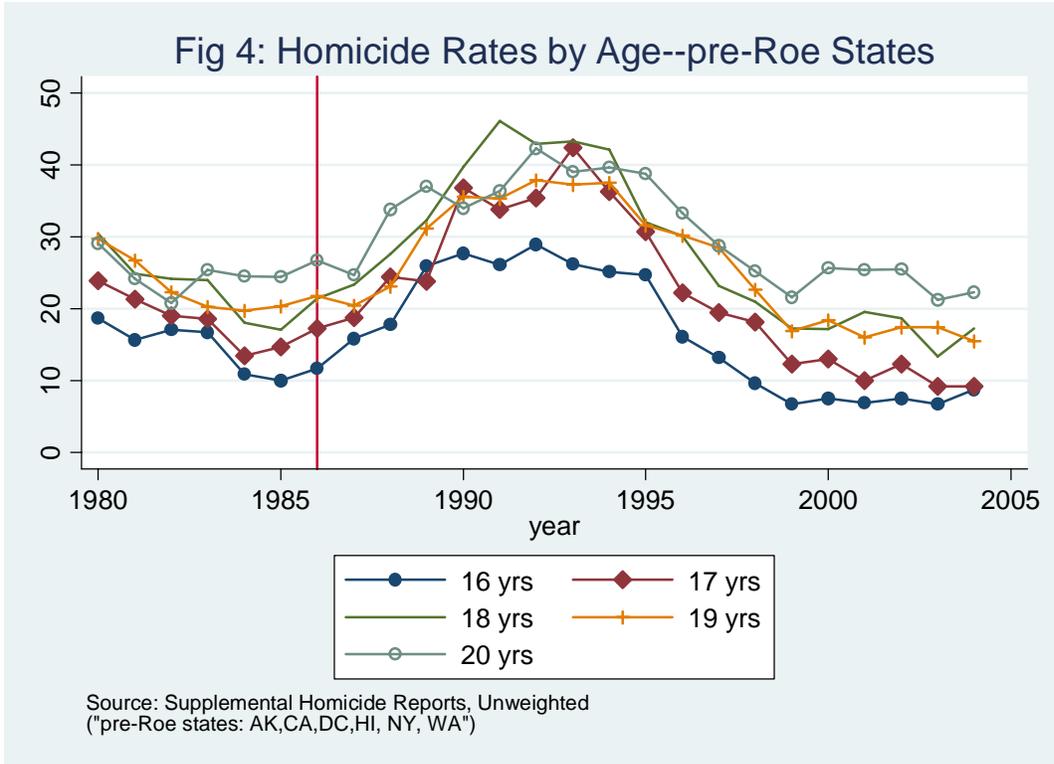
which the first cohort of 16-year olds that were exposed to legalized abortion in the pre-Roe states (the 1971 birth cohort). In this simulated example, the murder rate of 16-year olds begins to flatten after 1987 while the murder rate of the other age groups continues to rise. The change in trajectory among 17-year olds occurs one year later as the 1971 birth cohort ages, followed by a change in the homicide rate of 18 year olds, and so on.<sup>19</sup> Note that cohort effects remain evident even in the presence of strong age and period effects. In Figure 3, I show the same set of simulated murder rates with clear age and period effects but absent any cohort effect.

<sup>19</sup> Donohue and Levitt are clearly aware of this pattern. They write, “ 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).

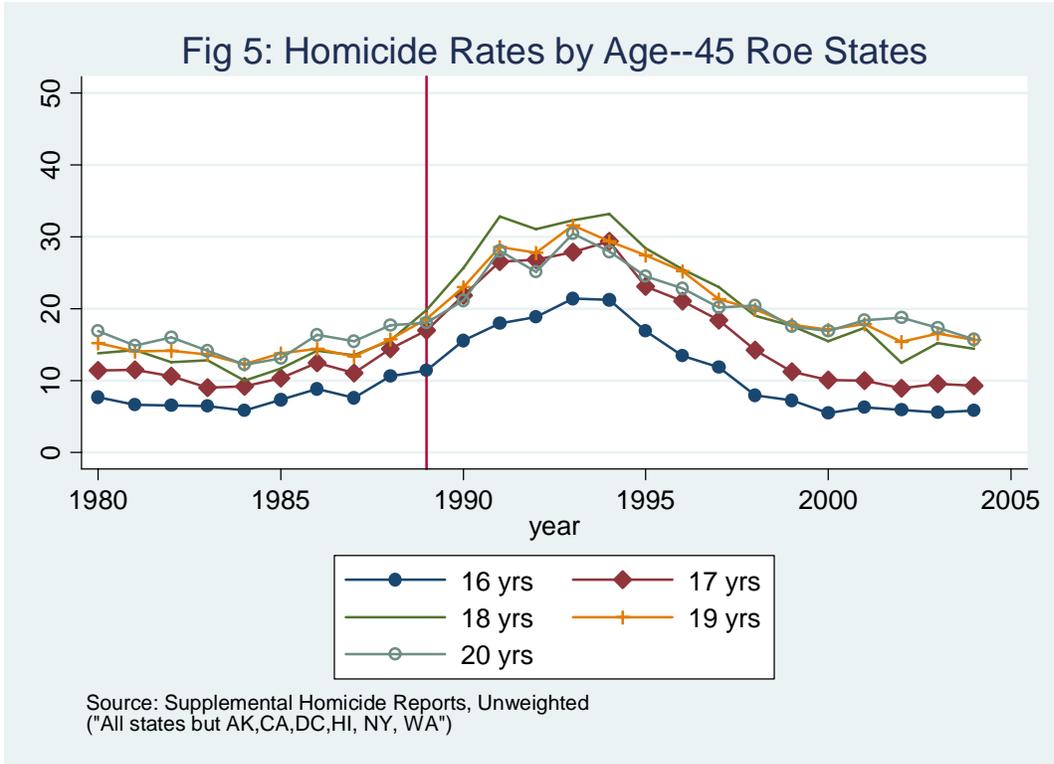


The distinguishing characteristic of dominant period effects is the coincident movement and turning point in the series. In this example, the homicide rate of each age group peaks around 1990 and then declines. Now consider actual homicide rates. Figure 4 shows the homicide offending rates in the pre-Roe states based on the FBI’s Supplemental Homicide Reports (SHR, Fox 2007).<sup>20</sup> As before, the vertical line shows the year after which the effect of legalized abortion should begin to show among 16-year olds.

<sup>20</sup> The SHR offending data have strengths and limitations. The SHR has homicides by age over a wider range than the arrest data used by Donohue and Levitt. However there are a substantial number of missing observations in addition to some states with obviously incomplete reports. Just over 26 percent of the records have no information about the perpetrator (Fox 2007). In addition, I dropped selected years due to questionable reports from DC, FL, IA, KS, KY, MT and ND. As a check on the SHR, I also plotted homicide victimization rates as collected by National Center for Health Statistics. The national mortality files are considered the most complete census of death to non-negligent homicide. The problem with mortality files is that victims are not perpetrators. Nevertheless as shown in the Appendix, the time-series pattern to the homicide victimization rates is very similar to the offending series.



Although the murder offending rates from the pre-Roe states are rather noisy, there is no evidence of any cohort effect. Indeed the homicide rate of 16-year olds climbs rapidly after 1987 as do the homicide rates of the other age groups. Figure 5 displays the actual homicide offending rates in the 45 states that legalized abortion with Roe. The series are smoother which makes the dramatic period effects more obvious. Again, there is no evidence of any cohort effect. Similar presentations for age-specific arrest rates are equally unresponsive of DLs' hypothesis.



Donohue and Levitt regard the age-specific crime rates as unpersuasive. They contend that the upsurge in violence associated with crack-cocaine markets obscures the positive effects of abortion. But their response is unjustified given the magnitude of their estimated effects. To illustrate, note that an increase of one standard deviation in the EAR in 1997—an increase of 100 abortions per 1000 births—would be expected to lower the total homicide rate by 16.6 percent based on their reported estimates (DL 2004, Table 1, column 2). Approximately 50 percent of all murders in 1997 were committed by individuals who were born before abortion became legal, and thus, the full impact of legalized abortion will not be felt until roughly 2017 (Donohue and Levitt 2001, p. 415). In other words, the 16.6 percent decrease in the total homicide rate that is attributable to legalized abortion is driven by criminals roughly 25 years of age or less. Accordingly, the impact of legalized abortion on the homicide rate of any newly exposed cohort

is, on average, 33.2 percent (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 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). Based on DL’s coefficients, I estimate that a one-standard deviation increase in the actual abortion in 1973 would be expected to reduce the homicide rate of 16-year olds in 1987 by 7.9 per 100,000 below its expected trajectory in the absence of legalization.<sup>21</sup> A decline this large would be unmistakable in the time-series plots.

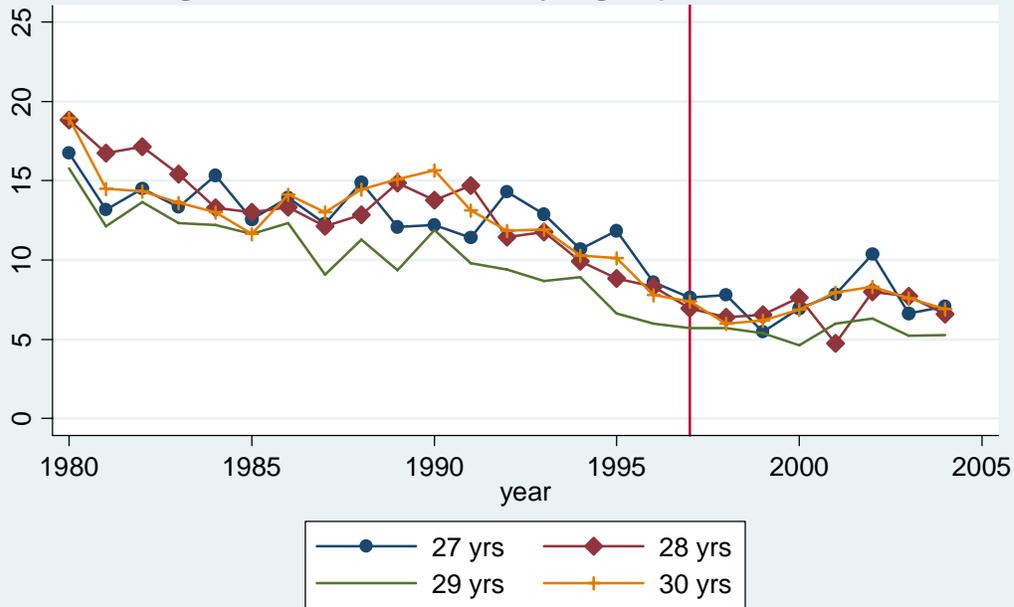
Donohue and Levitt also argue that plots of age-specific crime rates are not informative because one must follow cohorts over the life-cycle in order to capture the effect of abortion on crime (DL 2004).<sup>22</sup> This objection can be accommodated with plots of homicide offending rates of older age groups, those 27 to 30 years of age. Figure 6 shows the time-series for the pre-Roe states and Figure 7 the Roe states. In both figures, the vertical line marks the point at which the first cohort of 27-year olds (those born in 1971 in the pre-Roe States and 1974 in the Roe states) would have been exposed to legalized abortion *in utero*. One advantage of the older age

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<sup>21</sup> A one-standard deviation in the actual abortion rate between 1971 and 1974 is 150 abortions per 1000 live births (Joyce 2009). The homicide rate of 16-year olds in 1987 in the pre-Roe states is 15.8 per 100,000, up from 11.6 in 1986. If an increase in the abortion rate 100 per 1000 live births is expected to lower homicide for newly exposed cohort by 33.2 percent, then an increase in the abortion rate of 150 would be expected to lower homicide rates by 7.9 per 100,000 [ $.332 * 1.50 * 15.8 = 7.9$ ]. Since homicide rates were trending upward at approximately 4 per 100,000 per year prior to legalized abortions, the expected homicide rate of 16-year olds in the pre-Roe states in 1987 would be about 8.0, a change that clearly would be evident in Figure 4.

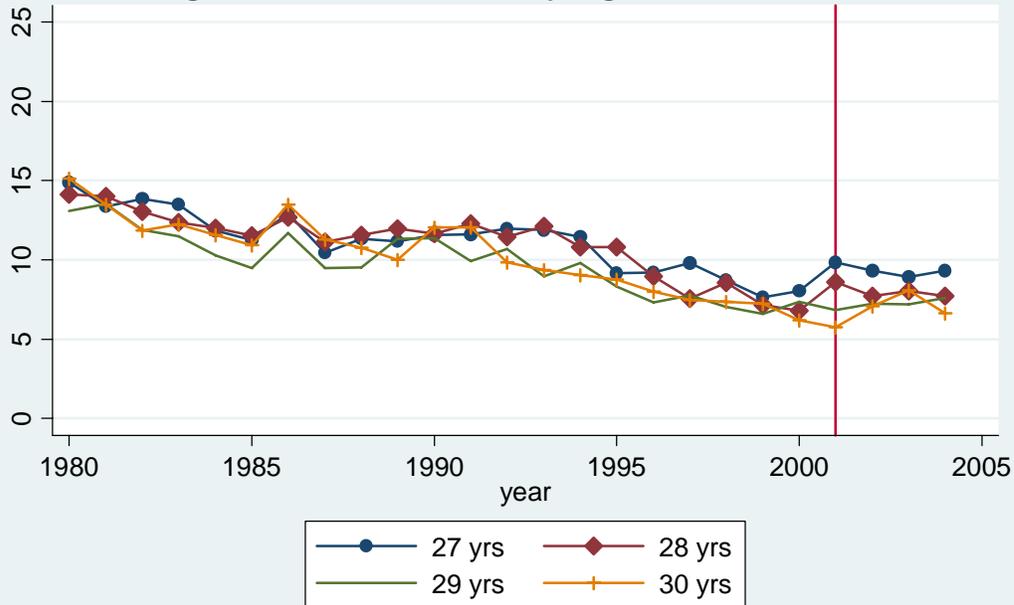
<sup>22</sup> Donohue and Levitt (2004, Table 2) made this argument in response to Joyce’s (2004) difference-in-differences-in-differences (DDD) estimates. The title of DL’s Table 2 refers to “Changes in the Log Homicide Rate for Cohorts 70-75...” But DL use log homicides and not log homicide rates. As I discuss below there is no justification for a regression in log levels. Foote and Goetz (2008) show that such models are likely to yield spurious results. Moreover, they are especially flawed in the context of a DDD estimator in which the baseline difference in the number of homicides between the 5 pre-Roe states and the 46 Roe states (including DC) is large. Indeed, when Joyce replicates DL’s Table 2 but with log homicide rates instead of log homicides, there is no effect of legalized abortion on the homicide rate (see Joyce 2004b, Table 2).

Fig 6: Homicide Rates by Age--pre-Roe States



Source: Supplemental Homicide Reports, Unweighted ("pre-Roe states: AK,CA,DC,HI, NY, WA")

Fig 7: Homicide Rates by Age--45 Roe States



Source: Supplemental Homicide Reports, Unweighted ("All states but AK,CA,DC,HI, NY, WA")

groups is that the strong period effects associated with crack cocaine are absent. In neither figure is their evidence of any cohort effect. Thus, regardless of whether I look at the 1971-1974 birth cohorts in their teens or as adults, there is no suggestion of any discontinuity in crime associated with legalized abortion.

Since publication of their seminal paper in 2001, Donohue and Levitt have responded to critics by revising the measure of the abortion rate, modifying the specification and estimating the regressions over different time periods. In each case, they conclude that their essential finding stands: the legalization of abortion in 1973 is a major reason why crime fell so substantially in 1990s. The critics believe otherwise.

### **III. Responses to the Donohue and Levitt Hypothesis**

The most detailed responses to DL have come from within the economics profession.<sup>23</sup> Joyce (2004, 2009), Lott and Whitley (2007) and Foote and Goetz (2008) each have replicated DL findings and then tested the sensitivity of the estimates to modifications. The debate often reduces to dueling regressions, which can be numbing to observers not enmeshed in the issues. I will try to avoid such a result in my review to the debate. I will also describe two studies that have attempted to replicate DL's findings in Canada (Sen 2007) and the United Kingdom

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<sup>23</sup> Criminologists have also weighed in on the debate, but their comments have not figured prominently in the exchange among economists. This is unfortunate since criminologists make the straightforward point that the downturn in age-specific crime rates does not line up with the cohort's exposure to legalized abortion (Fox 2000; Cook and Laub 2002; Rosenfeld 2005; Wilson 2005; Blumstein and Wallman 2006). DL dismiss this evidence which is ironic since DL present more aggregated time series on total crimes rates in support of their hypothesis (see Donohue and Levitt 2001, Figures 1 and 2). Zimring (2006) offers a broader criticism of the DL hypothesis. Zimring uses national trends in fertility by age, race and marital status to show that there has been a seemingly uninterrupted rise in the percent of births to unmarried women and teens between 1965 and 1980. The percent of births to African Americans has risen modestly. Based on these trends Zimring asks, "How can more babies born to single mothers aged under 19 generate fewer crimes when they grow up? How can a stable or larger group of poor children explain half a crime drop? It is here that the positive signs on the demographic arithmetic of risk categories are particularly troublesome" (Zimring 2006, p. 97).

(Kahane, Paton, Simmons 2007) as well work by Dills, Miron and (2008), who provide descriptive evidence of the timing of abortion legalization and crime from 20 countries. Lastly, I will discuss an analysis of an unprecedented national experiment in Romania in which a ban on abortion causes the birth rate to double within a year (Pop-Eleches 2006).

### **Joyce: identification and specification**

In two articles, Joyce (2004, 2009) argues that DL's empirical strategy is flawed along several dimensions.<sup>24</sup> First, DL never articulate an identification strategy that is capable of distinguishing between changes in "wantedness" due to variation in abortion from changes in wantedness due to shifts in sexual activity, contraception, marital status, etc. Donohue and Levitt use variation in state abortion rates as a proxy for variation in wantedness of a birth cohort. But as outlined above, the relationship between crime and "wantedness" is embedded in a complex structural model that includes equations for sexual activity, contraception, marriage, and birthing [see equations (2)-(8) above]. In other words abortion is endogenous. To give a simple example, teen fertility rates and teen abortion rates have both fallen substantially during the 1990s. Does the substantial fall in teen abortion rates portend an upsurge in crime? The answer, by Levitt's own admission, is no. In an internet exchange Levitt was asked whether decreases in the abortion rate in the 1990s will increase crime 20 years later. He responded that they would not because changes in abortion in the 1990s represented shifts in the demand for abortion as opposed to movements along the demand curve. Only the latter, he argues, will be associated with changes in unwanted childbearing (Posted June 15, 2006 at <http://freakonomics.blogs.nytimes.com/2006/06/15/steve-sailer-asks-an-excellent-question/> ). In other words, the rate of unintended childbearing may *fall* in a period of falling abortion rates or

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<sup>24</sup> The Joyce-DL exchange in the Journal of Human Resources did not allow for rejoinders. Joyce (2009) used a follow up article to respond to some of the issues raised by DL (2004, 2008).

even zero abortion rates, if the decline in abortion rates results from increases in contraception or less sexual activity. But DL never offer an empirical strategy that identifies shifts in the demand for abortion from movements along the demand curve. They simply regress age-specific crimes and arrests from 1985-1998 on abortion rates from 1960 to 1983. All variation in the abortion rate is assumed to represent movement along the demand curve for abortion and by assumption, changes in the “wantedness” of a cohort.

A related difficulty with their empirical approach is that approximately 60 percent of cohorts included in their analysis are assigned a zero abortion rate.<sup>25</sup> Donohue and Levitt (2004) dismiss the issue. They contend that zero abortions will only attenuate their estimates. But the assumption of zero abortions is more consequential for it implies that there was no variation in the wantedness of cohorts born between 1961 and 1973. This is certainly erroneous given the introduction of the pill, state variation in illegal abortions and the large drop in fertility rates in the 1960s (see Figure 1). A simple alternative is to limit the analysis to cohorts for which data on abortion exists. If DL are correct about the attenuation bias, then their results should be stronger in the more limited sample. As I show below, their findings largely disappear with this sample restriction.

A second major problem with DL’s empirical strategy relates to functional form. Donohue and Levitt (2001, 2004, 2008) use the log of arrests or the log of homicides as a dependent variable instead of the log of arrest or homicide rates. Foote and Goetz (2008) and Joyce (2009) demonstrate that DL’s estimates become smaller, often have the wrong sign and are

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<sup>25</sup> Donohue and Levitt assume that the abortion rate is zero for individuals born between 1960 and 1973 (individuals born in 1973 are assigned the abortion rate in 1972). They use resident abortion rates from the Guttmacher Institute from 1973-1984. In the 5 pre-Roe states they backcast the abortion rate from 1973 to 1970 and assume the abortion rate is zero from 1960-1969. In the 45 Roe states and Washington, DC they assume the abortion rate was zero from 1960-1972.

statistically insignificant if log arrest rates or log homicide rates are used instead of log counts. Donohue and Levitt (2008) concede the point, but contend that specifications with the log of age-specific arrests or crimes capture both cohort and selection effects, whereas specifications with arrest or crimes rates estimate only selection effects. They never explain how they arrive at such an assertion and it is difficult to derive a specification in which the level of crime is regressed on the abortion rate without a population offset. Consider, for instance, a linear relationship between crime and wanted births at the individual level.

$$(12) C_{ijt} = a + bW_{ijt-a}$$

Let  $C_{ijt}$  be the number of crimes committed by individual  $i$  of age  $a$ , in state  $j$  and year  $t$ . Let  $W_{ijt-a}$  be a dummy variable of whether this person was wanted at birth.<sup>26</sup> Now aggregate this model to the state level.

$$(13) C_{jt} = aN_{jt} + bW_{jt-a}$$

Let  $N_{jt}$  be the number of people in state  $j$  of age  $a$  in year  $t$ . Equation (13) represents a regression of the number of crimes in state  $j$  for a specific age group on the number of people in that age group and the number of individuals in that age group who were wanted at the time of birth. One might interpret the coefficient on  $N_{jt}$  as the cohort size effect and the coefficient on  $W_{ijt-a}$  as the selection effect. However, this would only be true if there were no deaths or migration into or out of the state. In other words,  $N_{jt}$  would have to be the number of births ( $B_{ijt-a}$ ) “ $a$ ” years ago in state “ $j$ ”. Moreover, this is a regression in which the number of crimes at the state level is

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<sup>26</sup> I use the demographer’s construct of intended and unintended births in which women are asked about their pregnancies after they give birth (see Institute of Medicine 1995). However, if all unwanted pregnancies are aborted, then there is a profound problem of missing data at the individual level as noted by (DiNardo 2007). This would still leave mistimed births, which are an important component of DL’s argument.

regressed on the state population and the *number* of unwanted births “a” years ago. To obtain a specification that uses the abortion rate, divide through by population,  $N_{jt}$  and treat  $N_{jt}$  and  $B_{jt-a}$  as perfect substitutes. Thus,

$$(14) \quad \frac{C_{jt}}{N_{jt}} = a + b \frac{W_{jt-a}}{B_{jt-a}} \quad \text{or}$$

$$(15) \quad CR_{jt} = a + bWR_{jt-a}$$

The results is a regression of the state crime rate ( $CR_{jt}$ ) on the rate of wanted childbearing ( $W_{jt-a}$ ). This is the specification estimated by Joyce (2004, 2009), Lott and Whitley (2007) and Foote and Goetz (2008). Donohue and Levitt (2001, 2004), by contrast, use the ratio of abortions to births as a proxy for WR but regress the *number* of crimes ( $C_{jt}$ ), in logs, on the abortion rate:

$$(16) \quad C_{jt} = a + b \frac{A_{jt-a}}{B_{jt-a}}$$

They offer no justification for the specification in equation (16); nor is it clear how to interpret,  $b$ , the coefficient on the abortion rate. Foote and Goetz (2008) make the additional point that the relationship between  $C_{jt}$  and  $A_{jt-a}/B_{jt-a}$  will be negative by construction. If  $B_{jt-a}$  is a proxy for the population in the state,  $N_{jt}$ , then increases in  $B_{jt-a}$  unrelated to abortion will decrease the abortion rate but increase  $C_{jt}$  simply because more people are mechanically related to more crime. In other words, the strong positive correlation between  $B_{jt-a}$  and  $N_{jt}$  creates a negative and spurious relationship between the level of crime and the abortion rate.

The third issue related to DL’s empirical approach involves the use of instrumental variables to ameliorate problems of measurement error in the abortion rate. In their most recent reply to Foote and Goetz (2008), Donohue and Levitt (2008) regress abortion rates by state of residence as estimated by the Guttmacher Institute on abortion rates by state of occurrence as

collected by the Centers for Disease Control and Prevention. They write, “If one has two noisy abortion measures but the measurement error in the two proxies is uncorrelated, instrumenting for one abortion proxy using the other will eliminate attenuation bias. In this setting, the CDC’s independently generated measure of legalized abortions is likely to be an excellent instrument” (DL 2008, p. 430). The strategy is flawed because researchers at the Guttmacher Institute *estimate* abortions by state of residence using the CDC’s estimate of abortions to out-of-state residents.<sup>27</sup> As Joyce (2009) details, errors in the CDC estimate of abortions to non-residents is built into the Guttmacher’s estimate of abortions by state of residence in potentially complicated ways. In short, the fundamental assumption of DL’s IV strategy, that the measurement errors in the two proxies are independent, is untenable.

Lastly, both Joyce (2009) and Foote and Goetz (2008) point out that DL (2001) did not adjust the standard errors for serial correlation within-states over time in the regressions of age-specific arrests or arrest rates. Although this may seem like a second-order issue, adjusting the standard errors for positive serial correlation renders key estimates statistically insignificant.

To illustrate the sensitivity of DL’s estimates to modifications in the specification, I replicate their published estimates and then alter the analysis as suggested by Joyce (2004, 2009) and Foote and Goetz (2008). The results are presented in Table 1. The figures are estimates of the coefficient on the abortion rate,  $\beta$ , from equation (11). Each estimate is from a separate regression. The dependent variable in columns (1) and (3) is the log of the *number* of crimes or

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<sup>27</sup> The following describes how researchers use the CDC data to estimate the Guttmacher Institute’s count of abortions by state of residence. “.....resident abortions in each state were derived from data obtained from the CDC and from AGI’s abortion survey. The first step was to calculate the percentage of each state’s abortions that occurred to residents of other states, using the CDC data. This proportion was applied to AGI’s count of abortion occurring in each state to estimate the number of abortions occurring in each state to out-of-state residents. The second step was to distribute the non-resident abortions to the appropriate states according to the percentage distribution of the state or residence of women obtaining abortions in each state for which data are reported to the CDC by the state health department (Henshaw and Van Vort 1992, p. 163).

arrests. I contrast these results with estimates based on the log of crime or arrest *rates* [columns (2) & (4)]. The standard errors are adjusted serial correlation within states using Stata's cluster procedure.

The coefficients in column (1) are supportive of the DL hypothesis. All four coefficients are negative and two are statistically significant. The estimates for violent and property crime arrests in column (1) replicate those from DL (2008, Table II). But as Foote and Goetz (2008) argue, regressions of log arrests or log crimes on the abortion rate are likely to yield negative and spurious estimates. The results in column (2) support this suspicion. When the dependent variable is expressed as a rate, two of the four coefficients become positive and none is statistically significant. Note that the estimates for violent and property crime arrests rates in column (2) also replicate those from DL (2008, Table II). Even if the coefficient on abortion in the regression of violent crime arrest rates is considered marginally significant, the decrease associated with one-standard deviation in the abortion rate is only 3.5 percent, about a one-fifth the size of the effect expected by DL (see the discussion on the magnitude effects on pages 21-22).

The estimates in columns (3) and (4) follow from Joyce's suggestion that DL limit the sample to cohorts for which data on abortion exist. Donohue and Levitt contend that the inclusion of observations in which the abortion rate is zero should bias their estimates toward zero. If true, then the negative association between abortion and crime should be stronger in columns (3) and (4). A comparison of estimates in columns (1) and (3) and columns (2) and (4) undermines this argument. Six of the eight coefficients in columns (3) and (4) are smaller than

**Table 1. Estimated Effects of Abortion Rate on Age-specific Counts or Rates of Log Arrests and Log Homicide (in logs): Replication of Donohue and Levitt (2008)**

	(1)	(2)	(3)	(4)
1. Violent crime arrests	-0.046*	-0.021	-0.051*	-0.039*
	(0.014)	(0.013)	(0.019)	(0.019)
Effect of 1 SD $\Delta$	[-0.076]	[-0.035]	[-0.061]	[-0.046]
2. Property crime arrests	-0.024*	0.001	-0.007	0.005
	(0.007)	(0.008)	(0.014)	(0.012)
Effect of 1 SD $\Delta$	[-0.040]	[0.002]	[-0.008]	[0.006]
3. Murder arrests	-0.028	-0.004	-0.009	0.003
	(0.025)	(0.023)	(0.077)	(0.078)
Effect of 1 SD $\Delta$	[-0.046]	[-0.007]	[-0.011]	[0.004]
4. Homicides	-0.022	0.006	0.037	0.048
	(0.018)	((0.018)	(0.066)	(0.066)
Effect of 1 SD $\Delta$	[-0.036]	[0.010]	[0.044]	[0.057]
Dependent var counts or rates	Counts	Rates	Counts	Rates
Cohorts	1961-83	1961-83	1974-81	1974-81

Figures in columns (1) and (2) and rows 1 and 2 replicate results from Donohue and Levitt (2008, Table 2) for violent and property crime arrests and arrest rates. The results for murder and homicide 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 columns (3) and (4) is limited to cohorts for whom abortion data exist in Donohue and Levitt (2008). Standard errors in parentheses are adjusted for clustering within state. Effects of a one-standard deviation increase in the abortion rates is shown in brackets. The weighted mean and standard deviation of the abortion rate 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

their counterparts in the full sample. All six are statistically insignificant and four of the six have the wrong sign.<sup>28</sup>

### **Footnote and Goetz: missing fixed effects**

Another major challenge to DL (2001) arose when two economists noticed an error in DL's computer code. In the description of their regression model, DL include state-year fixed effects, but omit these regressors in the actual estimation [see DL 2001, equation (3), p. 411].<sup>29</sup> Most readers of *Freakonomics* would not appreciate the significance of the omission. For applied economists, however, this was an important revelation. Many had been impressed that DL's estimates seemed robust to the inclusion of state-year fixed effects, which are often the only way to control for unobserved differences between states over time. There is widespread agreement among analysts, including DL, that the unprecedented rise in homicides among teens and young adults between 1984 and 1992 was largely attributable to the mix of crack, gangs and guns (see Cook and Laub 1999; Blumstein and Wallman 2006). Failure to adjust for these variables is likely to bias the association between abortion and crime. But there is no credible measure of crack markets available across states and time that can be added to the regressions. One way to overcome the lack of explicit data is to include state-year fixed effects in the

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<sup>28</sup>It is important to revisit Donohue and Levitt's (2004) reply to Joyce in the *Journal of Human Resources* given that estimates based on regressions of log crimes on the abortion rate may yield spurious results. For instance, in attempting to replicate Joyce's analysis, DL used the *number* of arrests or homicides whereas Joyce estimates all models with arrest and homicide rates. But the language Donohue and Levitt use to describe their results obscures the fact their regressions use log levels where as Joyce uses log rates as the dependent variable. For instance, the title of Table 2 in DL (2004) reads, "Changes in the Log Homicide Rate for Cohorts 70-75 Relative to Cohorts 65-70. Replication of Joyce Table 4. Using Multiple Available Six-Year Periods" (p. 43). The dependent variable, however, is the log of homicides and not the log of homicide rates. The title to Table VII in DL (2001) refers to arrest rates when again the actual dependent variable is arrests. The same is true in Table 1 of DL (2004). The bottom panel has the subtitle of arrest rates when again they are arrests not arrest rates. Joyce (2004b) re-estimated Table 2 from DL (2004) with log homicide rates instead of log homicides and finds that the sum of the DDDs is small and statistically insignificant (see also Joyce 2009). The results in Table 1 in the text above underscore the importance of the distinction between specifications with log levels versus log rates.

<sup>29</sup> To their credit, DL made their data and programs available on the internet.

regression. Specifically, researchers add a dummy variable for each state-year data point. If DL have 14 years of data across 51 states then they will add 650 dummy variables [13 years times 50 states] to the regression. The goal is to eliminate as potential confounders factors such as crack markets that can't be explicitly measured. As one might suspect, adding so many variables to the regression eliminates a great deal of variation from both crime and abortion. But it is important to recall that DL claim to have uncovered a robust cohort effect (legalized abortion) amidst profound period effects. If changes in cohort "wantedness" produce a noticeable break in crime, then state-year fixed effects should not obscure the finding. An alternative approach that eliminates confounding from strong period effects relies on an explicit comparison group within each state that was unexposed to legalized abortion *in utero* but was subject to the same period effects as those who were exposed to legalized abortion *in utero*. The "difference-in-differences" between these two groups is an estimate of the effect of abortion on crime.<sup>30</sup>

Donohue and Levitt (2008) acknowledge that they inadvertently omitted state-year fixed effects from their regressions of age-specific arrests. They also show that the omission has a substantial impact on their estimates. Their original estimates are reduced by at least 50 percent when state-year fixed effects are added to the model (DL 2008, Table I). For example, they originally report that an increase in the abortion rate of 100 abortions per 1000 live births is associated with a 1.8 percent reduction in arrests for violent crime (Donohue and Levitt 2001, Table VII). With state-year fixed effects, the same increase in the abortion rates is associated

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<sup>30</sup> For instance, consider the homicide rates in Figure 4. Joyce (2004, 2009) compares changes among 17 year olds between 1987 and 1989 to those among 19 year olds over the same period. Seventeen-year olds in 1987 were *in utero* in 1969 and thus unexposed to legalized abortion in the pre-Roe states whereas seventeen-year olds in 1989 were *in utero* in 1971 and thus exposed. Nineteen-year olds in 1987 and 1989 in the pre-Roe states were *in utero* in 1967 and 1969, respectively, and thus unexposed to legalized abortion. If the 1971 cohorts are more "wanted" as DL contend, then crime should fall or increase less than it does among 19-year olds. The key to the difference-in-differences is whether the comparison group is a credible counterfactual (see Meyer 1995).

with a 0.9 percent decrease in arrests. Although the coefficient is statistically significant, the magnitude of the reduction in arrests is not substantial.

In their response to Foote and Goetz (2008), Donohue and Levitt (2008) do more than simply insert the missing regressors. They revise their measure of abortion and re-estimate their models. In their original article they had used abortions by state of occurrence. This can be misleading, however, since many women travel out of state for an abortion especially in the early 1970s. The more appropriate measure is abortions by state of residence. Donohue and Levitt (2008) further tweak the abortion rate with an adjustment for cross-state mobility between a person's state of birth and their current state of residence. They also instrument it with abortions as reported by the CDC. The adjusted abortion measure increases the magnitude of the coefficients on the abortion rate substantially. An increase in the abortion rate of 100 per 1000 live births is expected to lower age-specific arrests by 5.5 percent in models with state-year fixed effects—an estimate six times greater than the coefficient with the original abortion measure.

Foote and Goetz (2008), however, also question the appropriateness of using log arrests instead of log arrest rates as the dependent variable. Foote and Goetz (2008) show that a specification of log arrests regressed on the abortion rate is likely to yield a negative but spurious association between abortion and crime. They further criticize DL for failure to adjust the standard errors for serial correlation. Foote and Goetz's (2008) preferred specification is a regression of log arrest rates on the abortion rate with state-year fixed effects and adjustments for serial correlation. When Donohue and Levitt (2008) estimate the regressions suggested by Foote and Goetz (2008), their estimates become small and statistically insignificant. As shown in Table 1 above (column (2), rows 1 and 2), the coefficient on the abortion rate in the violent

crime arrest rate regression is -0.021 (t-ratio is -1.62) and the coefficient in the property crime arrest rate model is 0.001 (t-ratio 0.20).

### **Lott and Whitley: Abortion raises crime**

In the third published challenge to Donohue and Levitt by economists, Lott and Whitley (2007) provide a detailed analysis of age-specific homicide offending rates to cohorts born between 1950 and 1988. The paper is notable because Lott and Whitley argue that legalized abortion is expected to raise not lower the murder rate. The theory is based on a paper by Akerlof, Yellen and Katz (1996) in which legalized abortion leads to fewer “shotgun” marriages and more out-of-wedlock births. The availability of safe, legal abortion allows men to insist that women terminate a pregnancy instead of offering marriage. Women unwilling to terminate their pregnancies are more likely to raise the child alone. The impoverishment of women reduces investment in their children’s human capital, which leads to later increases in crime. The theory is consistent with national time-series on shotgun weddings and out-of-wedlock childbearing, and thus, provides an alternative to the DL hypothesis.

The Lott and Whitley paper is notable for several other reasons. First, they present an extensive set of time-series plots of homicide offending rates by year and age or age and cohort stratified by states that legalized abortion before *Roe* or with *Roe*. As argued by Joyce (2004, 2009) and numerous criminologists (Cook and Laub 2002; Blumstein and Wallman 2006; Zimring 2006), the basic time-series are inconsistent with the DL story. Lott and Whitley make this point forcefully as they compare homicide rates of adults 26 to 30 years of age between 1980 and 1998. If DL are correct, there should be a noticeable change in homicide rates among 26-year olds approximately four years earlier than the change among 30-year olds. Comparison of the adult homicide rates offers a compelling response to Donohue and Levitt (2004) argument that the crack epidemic obscures the impact of legalized abortion on age-specific crime rates.

The impact of crack was concentrated among teens and young adults between 1985 and 1992. There is little indication that crack had a significant impact on the homicide rates of older adults (see Figures 6 and 7 above).

Lott and Whitley also challenge DL's assumption that abortion rates were zero before 1973 in states that legalized abortion with *Roe*. Lott and Whitley present abortion rates (abortions per 1000 live births) in selected states from 1969 to 1972 as collected by the Centers for Disease Control and Prevention, which they include in their regression analyses. The third notable characteristic of the Lott and Whitley analysis is the use of count data models in the analysis of homicide rates. Donohue and Levitt (2001) argue against an analysis of homicides because too many cells have zero observations.<sup>31</sup> The count data regressions overcome this objection (see also Joyce 2009).

Lott and Whitley (2007) specify a regression model with an extensive set of controls. The basic specification includes measures of the prison population, execution rate, arrest rate, right-to-carry laws, unemployment rate, poverty rate, per capita income, population density as well as state and year fixed effects. An increase in the abortion rate from zero to its 1980 level is associated with an approximately 25 percent increase in the homicide rate. They perform extensive sensitivity tests and report that coefficient on the abortion rate was negative only once in over 6000 regressions (Lott and Whitley 2007, p.323).

Lott and Whitley's regression analysis is the least appealing aspect of the paper. First, they greatly expand the number of age groups and cohorts. They analyze homicide offending rates from 1980 to 1998 by single year of age for those 10 to 30 years old as well adults 31 years and older as a single group. There are potentially 26,979 cells by age, state and year of birth as

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<sup>31</sup> However, DL use homicides (not homicide rates) to bolster their case against Joyce (2004).

compared to the 6120 potential cells in Donohue and Levitt (2001). But many of the additional cells added by Lott and Whitley have no information about abortion, and thus, tell us little about the abortion crime association. For example, the homicide rate in 1980 pertains to cohorts born from 1950 to 1970. Only 4 of the possible 1122 age-state-year-of-birth cells have an abortion rate paired with a 1980 homicide rate. In short, Lott and Whitley have only worsened the problem of zero cells that characterized DL's approach. The large number of cells also creates a false sense of precision as the standard errors are artificially reduced. Finally, Lott and Whitley should be applauded for collecting additional data on abortion from the CDC, but they make the mistake of using abortions by state of occurrence instead of abortions by state of residence. Consequently, they greatly overestimate abortions in some states and underestimate them in others. For instance, 60 percent of 255,000 abortions obtained in New York in 1971 were to non-residents of the state (CDC 1972).

### **Seeing the Forest from the Trees**

Donohue and Levitt (2004, 2008) provide seemingly convincing responses to their critics. In Table 3 of their response to Joyce, 8 of the 8 coefficients on the abortion rate are negative and six are statistically significant. In Table II in their response to Foote and Goetz, 15 of the 18 coefficients are negative and 11 are statistically significant. But many of DL's specifications are inappropriate and should not be considered. For instance, in their response to Joyce, DL analyze only the log of arrests and homicides and not arrest or homicide rates; nor do they include state-year fixed effects. In their response to Foote and Goetz, DL continue to show results based on the log of arrests and they have added estimates based on an instrumental variable of dubious validity. To those not immersed in such details, the exchange may appear to end in a "he said, she said" stalemate. However, the hypothesis is so provocative and the policy implications so

profound that it is important to distill the exchange further. Thus, it is useful to step back for a moment and ask whether there are features of an empirical test of abortion and crime that criminologists and economists might agree upon. I would offer the following:

1. The crime measure must be age-specific in order to identify cohorts.
2. The outcome should be a rate of crime and not a level.
3. The hypothesis should be consistent with the timing of abortion legalization and should be evident or not contradicted by basic time-series plots.
4. The abortion rate should be measured by state of residence.
5. The abortion rate should be inversely related to fertility rates.
6. Regressions of age-state-year crime rates should include state-year fixed effects
7. The number of observations with no measure of abortion should be minimized.
8. Statistical tests should take account of the auto-correlation in crime-rate residuals.

Many of these principles are achieved with Figures 4-7 and the regression results in Table 1.

Taken together, they provide almost no support for the Donohue and Levitt hypothesis. There is no evidence of a decline or moderation in homicide rates that is consistent with the timing of legalized abortion. The abortion rate in Table 1 is measured by state of residence and incorporates DL's suggested refinements in timing and migration. Specifications in columns (2) and (4) use age-specific arrests and homicide rates. The regression estimates in Table 1 have been adjusted for state-year fixed effects, which provide a powerful control for period effects. The standard errors are adjusted for a general form of auto-correlation within each state. The estimates in column (4) are based on a sub-sample of DL's data for which information on abortion rates exist. These estimates should be stronger than those in column (2), if the inclusion

of many cells with no abortion data attenuates the estimates. If we focus only on the results in columns (2) and (4) then five of the eight coefficients have the wrong sign, none is statistically significant at the 5 percent level, and the magnitude of the coefficients is relatively small.

#### **IV. International Evidence**

Evidence on the abortion crime link from other countries provides another means by which to move the debate forward. Two published studies offer detailed attempts to replicate Donohue and Levitt's work in Canada and the United Kingdom (Sen 2007; Kahane, Patton and Simmons 2007). Pop-Eleches (2006) analyses the effect of Romania's ban on abortion in 1967 on the education and labor market outcomes of cohorts born immediately following the ban. He also includes a limited analysis of the ban's impact on crime (Pop-Eleches 2006). Another "study" of abortion and crime in Australia is cited by Levitt and Dubner in Freakonomics. But the article is little more than speculation as to a possible association that includes little data and no analysis (Leigh and Wolfers 1999). Finally, Dills, Miron and Summers (2008) plot homicide rates 20 years after abortion was legalized in 11 countries. Collectively, the international evidence provides little convincing support for an association between legalized abortion and crime.

##### **United Kingdom**

Kahane, Patton and Simmons (2007) analyze the abortion crime link in England and Wales. Abortion was legalized in England, Scotland and Wales but not North Ireland in 1967 and went into effect in April of 1968. The number of abortions is assumed to be relatively complete since health authorities are required by law to report terminations. Data on crime is from 42 police force areas in England and Wales from 1984-2004 and are available by single

year of age.<sup>32</sup> The authors first present a series of time-series plots of total crime rates and then separately for violent and property crime rates. There is no evidence of any decline in crime associated with legalized abortion. In fact, rates of violent crime have risen steadily in England and Wales since 1984. The pattern for property crime and total crime differ significantly from violent crime. Total crime and property rise from the mid 1980s to the early 1990s and then turn sharply downwards. Unlike violent crime, total crime and property crime mimic the time-series pattern of US crime rates. However, the timing of the downturn argues against an association in the UK since abortion became legal five years earlier in the UK than in the US (1967 vs. 1973).

The regression results in Kahane, Patton and Simmons (2007) largely reflect the time-series patterns. The coefficient on the effective abortion rate in the violent crime regression is positive and statistically significant and thus, the exact opposite of the results obtained by DL in the US. However, the coefficient on the effective abortion rate in the in the property crime and total crime regressions is negative and statistically significant. But the negative association between total crime and the effective abortion rate does not stand up to robustness tests. The association, for example, becomes positive if London is dropped from the sample.

As I have argued, regressions of total crime rates on the effective abortion rate offer a weak test because cohorts cannot be identified. However, Kahane, Patton and Simmons (2007) also provide an age-specific analysis. They regress rates of cautions and guilty verdicts on the abortion rate that existed the year before the cohort was born. The authors have over 8000 age-area-year cells. They include year-area and age-area fixed effects. The coefficient on the

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<sup>32</sup> Crimes include total offenses officially recorded. A second category of crime available by age is termed cautions plus guilty verdicts. Cautions is a formal police warning as an alternative to court proceedings. Cautions do not include informal warnings for shoplifting or traffic violations (Kahane, Patton and Simmons 2007).

abortion rate is *positive* in all seven regressions and statistically significant in two, the opposite of what DL would have expected. Indeed, the estimates are more consistent with Lott and Whitley's (2007) hypothesis that legalized abortion increased crime than with Donohue and Levitt's story of more wanted cohorts committing less crime.

## **Canada**

The relationship between abortion and crime in Canada is of particular interest given its proximity and culture similarity to the US. In addition, Canada legalized abortion in 1969 and then expanded its availability again in 1988. Third, Canada was not affected as much as the US by crack, gangs and guns, and thus, an important source of confounding in the US data is probably absent in Canada. Sen (2007) has data on crime rates by year and province in Canada from 1983-1998 (n=144). Data on abortion begin in 1970 and are stratified by age (teens and adults). Thus, Sen can only replicate DL's state-year regressions because he cannot measure the crime rates of specific cohorts. Moreover, he has only nine provinces and thus limited geographic variation. One potential advantage of Sen's data is that the abortion rates are age-specific. In addition, Sen uses age-specific fertility rates as an alternative to abortion rates to measure variation in unwanted childbearing. He finds that modifications of DL's effective abortion rates are negatively associated with violent crime rates but not property crime rates in Canada. The growth in the effective teen abortion rate or general abortion rate for all women can explain approximately 2.5 percent of the 10 percent decline in violent crime in Canada from 1992 to 1998. Sen re-estimates the same models but uses weighted lags in teen fertility rates or general fertility rates instead of the teen abortion or general abortion rate. In these regressions only the lagged teen fertility rate is associated with an increase in violent crime and it can explain more than half the decline in violent crime in the 1990s.

Sen provides two robustness checks. First, he breaks total violent crime into its components: homicide, robbery, sexual assault and physical assault and re-estimates his models for each component separately. The teen fertility rate is positively associated with only physical assault and none of the other three crime rates whereas the teen abortion rate is negatively associated with sexual and physical assault. The lack of an association with homicide and all forms of property crime is inconsistent with DL. The second check is a regression of teen fertility rates on indicators of abortion legalization. The absence of an association between abortion legalization and teen fertility rates would undermine any association between abortion and crime. Sen presents estimates from his regressions of teen fertility on abortion legalization from which he concludes, “These results suggest that both periods of abortion legalization experienced significant declines in teen fertility, with the 1970-88 period coefficient unsurprisingly smaller in magnitude than the corresponding 1989-1990 estimate” (Sen 2007, p. 27).

Sen’s conclusion that the legalization of abortion is negatively correlated with teen fertility rates in Canada is a critical piece of evidence for it links his work to US studies that show a significant fall in fertility rates after the legalization of abortion (Sklar and Berkov 1974; Joyce and Mocan 1990; Levine et al. 1999; Angrist and Evans 1999). Indeed, Donohue and Levitt repeatedly cite this US evidence to support their claim that legalized abortion improved the wantedness of cohorts.<sup>33</sup> However, the putative association between abortion legalization and teen fertility in Canada reported by Sen does not stand up to closer scrutiny. Sen estimates the following two equations:

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<sup>33</sup> Donohue and Levitt (2004) never show that abortion rates are negatively associated with fertility rates beyond the period immediately following legalization in the US. They fall back on the argument that abortion improves the timing of fertility, but they provide no evidence to support the claim.

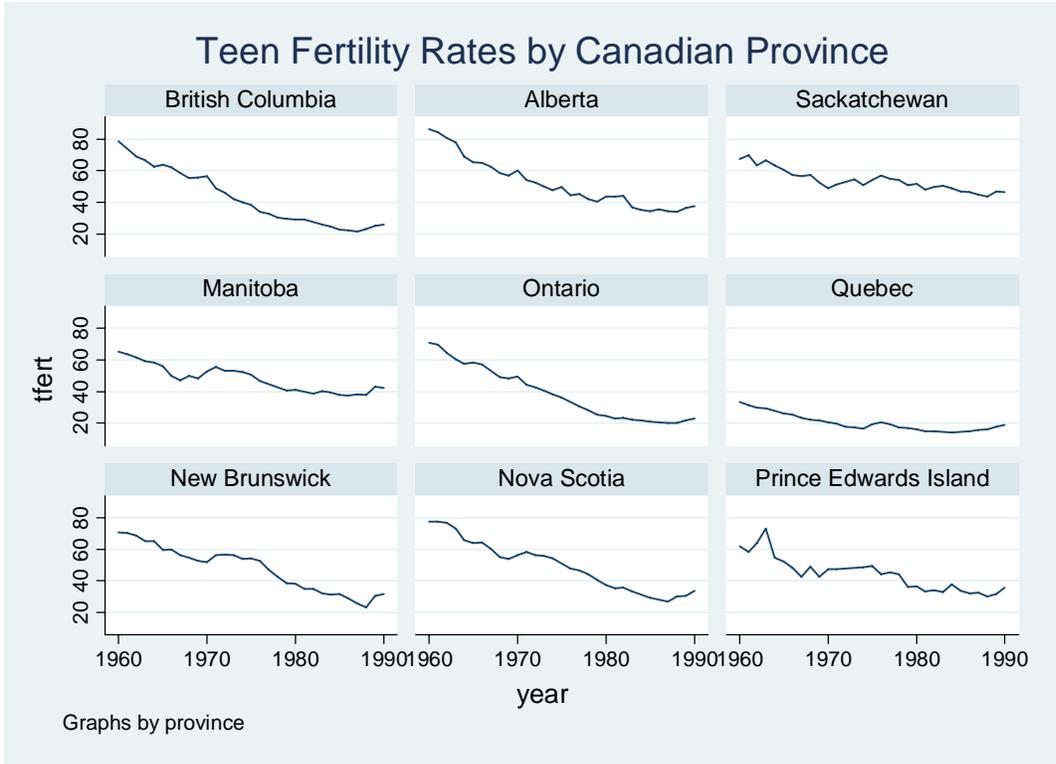
$$(17) \quad TFR_{jt} = \alpha_0 + \alpha_1 Leg_t + \alpha_2 Marriage_{jt-1} + \gamma_j + e_{jt}$$

$$(18) \quad TFR_{jt} = \alpha_0 + \beta_1 Spline1_t + \beta_2 Spline2_t + \alpha_2 Marriage_{jt-1} + \gamma_j + e_{jt}$$

In equation (19),  $TFR_{jt}$  is the teen fertility rate in province  $j$  ( $j=1,..9$ ) and year  $t$  ( $t=1960,..1990$ ).  $Leg_t$  is a dummy variable that represents the legalization of abortion nationally in Canada. Early legalization occurred in 1969 and a later liberalization occurred in 1988. However, Sen specifies  $Leg_t$  to be zero from 1960 to 1969, one in 1970 only, zero again from 1971 to 1988, one in 1989 only and zero again in 1990. He includes no trend term in the model. As specified,  $\alpha_1$  in equation (17) measures the level of teen fertility rates in 1970 and 1989 relative to teen fertility in 1960-1969, 1971-1988, and 1990. The specification makes little sense since the reference category includes both the pre- and the post-legalization years. Similarly the splines in equations (18) are simple linear trends terms and there is no legalization dummy. Given the strong downward trend in teen fertility rates, they will undoubtedly be negative. For instance, Figure 9 shows teen fertility rates by year and province in Canada.<sup>34</sup>

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<sup>34</sup> I thank Professor Sen for sharing the fertility and abortion data.



In each province, teen fertility rates fall almost continuously between 1960 and 1990 except for an up tic in the early 1970s and late 1980s. Ironically, the up tics occur just after abortion was legalized in 1969 and liberalized in 1988. To explore this further I use the same data as Sen (2007) and regress teen fertility rates on two dummy variables for legalization. The first indicator of legalization is one from 1970 to 1990 and zero in the years prior to 1970; the second indicator is one in 1989 and 1990 and zero in all other years. I also included a linear trend term, the lagged marriage rate, and province fixed effects. Standard errors are adjusted for clustering at the province level. Thus, I duplicate Sen’s regression but with more conventional indicators for legalization and a linear trend to control for secular movements in fertility. If legalization lowers teen fertility rates there should be a discontinuity at 1970 and then in 1988. The estimated equation and standard errors for the key variables are shown below.

$$(19) \quad TFR_{jt} = \alpha_0 + 0.85 * Leg70_t + 0.210 * Leg89 - 0.036 * Trend$$

(0.034)
(0.026)
(0.004)

The coefficients on both legalization dummies are positive and statistically significant. The trend term is negative and also statistically significant. These estimates suggest that the legalization of abortion increased teen fertility in Canada, a result more consistent with Akerlof et al. (1996). The exercise is limited because legalization was national and identification comes from within-province deviations from a national trend. Nevertheless, the coefficient on abortion legalization in equation (19) is the exact opposite of Sen's and contrary to his claim that legalized abortion lowered the rate of unwanted childbearing among teens.

The Sen article figures prominently in the debate, since Levitt and Dubner cite an earlier version of the manuscript in *Freakonomics* as evidence that abortion lowered crime in other countries. But Sen's analysis is actually a step backwards in the debate. There is no association between abortion legalization and teen fertility, a seemingly necessary condition for an association between abortion and crime.<sup>35</sup> There are only nine provinces in Canada and thus the analysis is even more vulnerable than DL's state-year regressions to uncontrolled state-year effects. The results for violent crime are only robust for sexual assault, an outcome less consistently measured than murder. Finally and most damaging, the study includes no analysis of age-specific crime rates. Consequently, Sen is unable to associate the crime rates of cohorts and the abortion rate that existed the year before their birth—a basic test of cohort effects.

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<sup>35</sup> Even if legalized abortion affected only the timing of births among teens, there still should be a negative association between legalized abortion and teen fertility rates. This would not necessarily be the case for 20-24 year olds, since teens who delay childbearing until their early 20s could offset the decrease in births among young adults. But since teens are the youngest group of mothers, a fall in fertility or an increase in delay fertility should be negatively associated with legalized abortion.

## Romania

The third prominent study with data on abortion and crime exploits one of the most dramatic changes in fertility ever recorded (Pop-Eleches 2006). In 1967, the Romanian dictator, Nicolae Ceausescu, banned all abortion in the country. Birth rates doubled that same year, since abortion had been the most common form of fertility control. Outcomes among children born immediately after the ban *improved*. Educational achievement increased and employment in high-skilled jobs rose among the cohort exposed to the ban. The unexpected result occurs because the ban increased the proportion of births to more educated urban households. Conditional on the parents' socioeconomic status, the ban on birth causes outcomes to worsen consistent with a decrease in "wantedness." The probability of finishing high school, for example, fell 1.7 percentage points relative to a mean of 52.1 percent and the probability of employment in a high-skilled job fell 0.7 percentage points, a decline of over 7 percent.

Motivated by the DL study in the US, Pop-Eleches compares the crime rates of cohorts born before and after the ban. However, unlike in the analysis of schooling and labor market outcomes, he was unable to control for the composition of births given limitations in the data. Accordingly, he finds that the crime rate of cohorts born in the first few years after the ban falls, the opposite of the DL hypothesis. As a crude adjustment for the composition of births, Pop-Eleches compared crime rates of cohorts born in 1970 to those born prior to the 1967 ban. He finds that the ban is associated with an increase in crime, a finding more consistent with DL's hypothesis. Pop-Eleches (2006), however, acknowledges the tenuousness of his findings with respect to crime. The treatment group consists of cohorts born more than three years after the ban. The strategy undermines the quasi-experimental design since the estimates based on cohorts so long after the ban are more vulnerable to confounding from period effects. Pop-

Eleches writes, “However, the present framework cannot control for other time-specific factors that might also have affected the criminal behavior of cohorts born after 1970. As an example, these results could also be explained by the increased criminal behavior of young people during the transition process” (Pop-Eleches 2006, p. 769). In an effort to control for period effects, Pop-Eleches includes age-specific trends terms. In these specifications the association between abortion and crime is weaker and no longer statistically significant.

### **Australia and other countries**

A review of the association between abortion and crime in other countries would not normally include Australia. However, Levitt and co-author Dubner contend that, “studies of Australia and Canada have since established a similar link between legalized abortion and crime” (Levitt and Dubner, 2005, p.141). The Canadian study is by Sen and discussed above. There is, however, no Australian study. Levitt and Dubner are referring to a three-page overview of whether the association between abortion and crime might be relevant to Australia. The authors of the Australian article provide no analysis, only speculation from a few stylized facts. They write, “Unlike Donohue and Levitt, we lack two sets of statistics. We have been unable to access any figures for the numbers of abortions performed during the 1970s and 1980s. Nor have we been able to obtain a full breakdown of homicide in Australia by age of perpetrator. If this data does exist, it would shed considerable light on whether there is a link between abortion and crime in Australia (Leigh and Wolfers 2000, p. 30).<sup>36</sup> Finally, in a review of the economics of crime, Dills, Miron and Summers (2008) include a graphical presentation of abortion legalization

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<sup>36</sup> The mention of Australia as corroborating evidence might be forgiven as popular writing, but as DiNardo (2006) argues, it is characteristic of Levitt’s rhetoric in *Freakonomics*. “...one disappointing aspect of *Freakonomics* was its discussion about research conducted by others. It would appear that the goal of characterizing other research accurately conflicted with other goals” (DiNardo 2006, p. 618).

and homicide rates in 20 countries. They look at homicide rates 20 years after abortion became legal. In the 11 countries with sufficient years of data since legalization they find "...little support for the hypothesis that legalizing abortion reduces crime" (p. 15).

### **Other evidence**

To this point I have focused on specific critiques of DL or studies cited by DL that directly support their hypothesis. Other studies have controlled for abortion in analyses of crime or have published work in journals not commonly read by economists and criminologists. For example, Reyes (2007) argues that reduction in childhood lead exposure in the 1970s led to lower crime rates in the 1990s. Her research design is similar to DL's state-year regressions. Total crime rates are regressed on lagged measures of lead exposure at the state level. Reyes includes the effective abortion rate as an additional regressor. Her results suggest that lead can explain 56 percent and abortion 29 percent of the decline in violent crime between 1992 and 2002. In other words, 85 percent of the recent drop in crime can be attributable to factors never considered by criminologists. These are huge effects which immediately raises skepticism. But more substantively, Reyes's identification strategy builds on the weakest aspects of DL's design. Without age-specific crime rates, there is no way to credibly test for cohort effects. Moreover, Reyes claims to have uncovered dramatic cohort effects during years characterized by unprecedented period effects that were specific to a particular age, race and geographical segments of the population. As I have argued above, distinguishing period from cohort effects cannot be accomplished convincingly without linking the intervention (or exposure) to specific cohorts

As noted previously, a recent study updated the work of Gruber, Levine and Stagier (1999) analysis of the "marginal child." Ananat et al. (2009) use the 2000 census to analyze the

longer-term association between abortion legalization and the “marginal child” (Ananat et al. 2009). In this paper the authors refer, but never show, regressions that attempt to replicate the work of DL (2001). They write, “...we estimate IV models where changes in the birth ratio and birth rate are instrumented using changes in abortion policy. With this approach we found a negative but not precisely estimated effect of reduced abortion costs on crime per capita....Therefore, our results align with the previous literature in that reduced abortion costs led to reduced crime, but largely through a reduction in cohort size (total crime) rather than through selection (crime per capita)” [Ananat et al. 2009, p. 135]. The authors refer readers to estimates from a National Bureau of Economic Research Working Paper (Ananat et al. 2006). However, an examination of the actual regression results reveals conflicting findings and puzzling estimates. As the authors state, the association between the natural logarithm of arrests for violent and property crime and the marginal birth rate are positive, suggesting that marginal children would have committed more crimes had they been born. But as I have argued previously, regressions of total crimes or arrests on the birth rate make little sense since they are mechanically related. An increase in the birth rate will lead to an increase in the population which leads to an increase in the *number* of crimes. The results are also puzzling in that the coefficients and *standard errors* obtained by ordinary least squares (OLS) are essentially the same as those estimated by two-stage least squares (TSLS). For instance, the coefficient on the birth rate in a regression of total violent crime arrests is 0.387 (0.112) when estimated by OLS and 0.310 (0.127) when estimated by TSLS (standard errors are in parentheses). The same occurs with total arrests for property crime: 0.249 (0.094) by OLS and 0.306 (0.099) by TSLS (see Table 6 in Ananat et al. 2006). It is extremely unlikely that the standard errors on the TSLS estimates would be same as those estimated by OLS. The other revealing finding is that

regressions of arrests per capita, a more appropriate outcome, on the natural logarithm of the birth rate are negatively related to arrest rates in both the OLS and IV regressions, a finding that contradicts DL. None of these are precisely estimated but neither do they support Ananat et al.'s (2009) statement of a direct association between crime per capita and the cost of abortion.

Another study uses an interrupted time-series analysis to test for the effect of *Roe v. Wade* on national number of homicide victim by age (15 to 24 year olds), race and sex (Berk, Sorenson, Wiebe and Upchurch 2003). The authors find that national legalization was associated with a gradual decline in homicide victims after 1988. The authors do not uncover any discontinuity in the time-series. Instead they report a change in slope associated with lagged exposure to legalized abortion. The results are not convincing. As shown in Figure 5, homicide rates for 15 to 20 years olds peak in roughly the same year, a pattern inconsistent with a cohort effect. Moreover, their reliance on various lags that tend to increase with age is ad hoc and vulnerable to confounding from unobserved period effects. The authors downplay the importance of crack markets arguing that that drug violence affected primarily blacks. But the white homicide rates in the SHR include Hispanics, many of whom were exposed to drug markets in major cities such as New York, Los Angeles and Chicago. Foote and Goetz (2008) also analyze national time series of arrest rates of 15 to 24 year olds by single year of age. They find no association with state abortion rates.

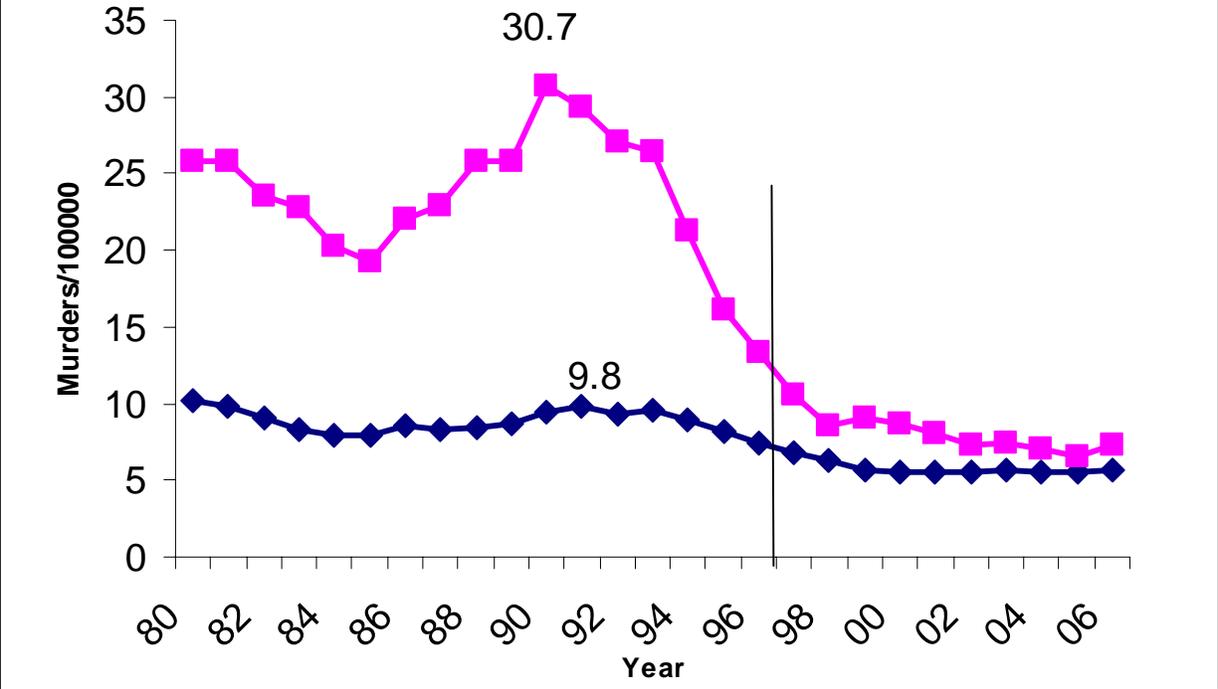
## **V. Conclusion**

Donohue and Levitt (2001) end their seminal piece with a remarkable prediction. Using 1997 as the point of reference they write,

Roughly half of the crimes committed in the United States are done by criminals born prior to the legalization of abortion. As these older cohorts age out of criminality and are replaced by younger offenders born after abortion became legal, we predict that crime rates will continue to fall. When a steady state is reached roughly twenty years from now, the impact of abortion will be roughly twice as great as the impact felt so far (p. 415)

We now have 10 years of data on homicide rates since 1997 with which to offer a preliminary assessment of their prediction. If DL are correct, then we would expect the decline in crime that had been realized up to 1997 to essentially double over the next twenty years. Figure 9 shows the homicide rate in the US and New York City from 1980 to 2006. Nineteen ninety-seven is marked by the vertical line. I include New York City because not only did New York legalize abortion prior to *Roe*, but the city's abortion rate is approximately triple the national rate. In addition, the city has experienced the largest decline in homicide since 1990 of any major city in the US. The time-series data in Figure 9 are not consistent with a major cohort effect that inexorably pushed crime down as more and more cohorts were exposed. The data instead

Figure 9: Murder Rates in the US and NYC



suggest some shock that caused early cohorts to experience rapidly rising homicide rates after 1985, followed by a dramatic reversal through much of the 1990s, ending with little change among the most recent cohorts. The leveling off after 2000 is clearly at odds with their prediction

The times series data in Figure 9 are too crude for conclusions. However, they underscore a point I have emphasize throughout this review. Donohue and Levitt's presentation of the evidence relied too much on highly-aggregated regressions, run over limited time periods, with questionable specifications and no acknowledgement of the endogeneity of abortion. If they had started with simple time-series of age-specific arrest and homicide rates pre and post the points of abortion legalization, they would have been confronted with the lack of discontinuities consistent with large cohort effects. Their responses to Joyce (2004) and Foote and Goetz (2008) were akin to doubling down on their regression bets.

Despite the remarkable public dissemination of the findings, the policy implications of their results have been largely ignored. If taken seriously, their findings imply that all forms of fertility control will reap major gains in well-being, reduced crime being only one. It would seem that both pro- and anti-abortion advocates could coalesce around the call for better family planning and fewer unwanted pregnancies. Instead, their results devolved into another battle to be waged in the politics of abortion. Few extended the implications for crime for other outcomes. Criminologists, according to DL, appeared to have missed the big story behind the great crime decline. But the straightforward approach to the evidence served this community of social scientists well. Criminologists have long debated the effect of cohort size on crime rates.

The notion that abortion may have selection effects beyond cohort size was a new wrinkle that at first peaked interest, but was eventually dismissed after an early examination of age-specific crime rates.

In the end, DL's story remains a best seller. It is bold, provocative, accessible and beautifully argued. Academics and graduate students will continue to explore the association between abortion and crime and its many offshoots for years to come.

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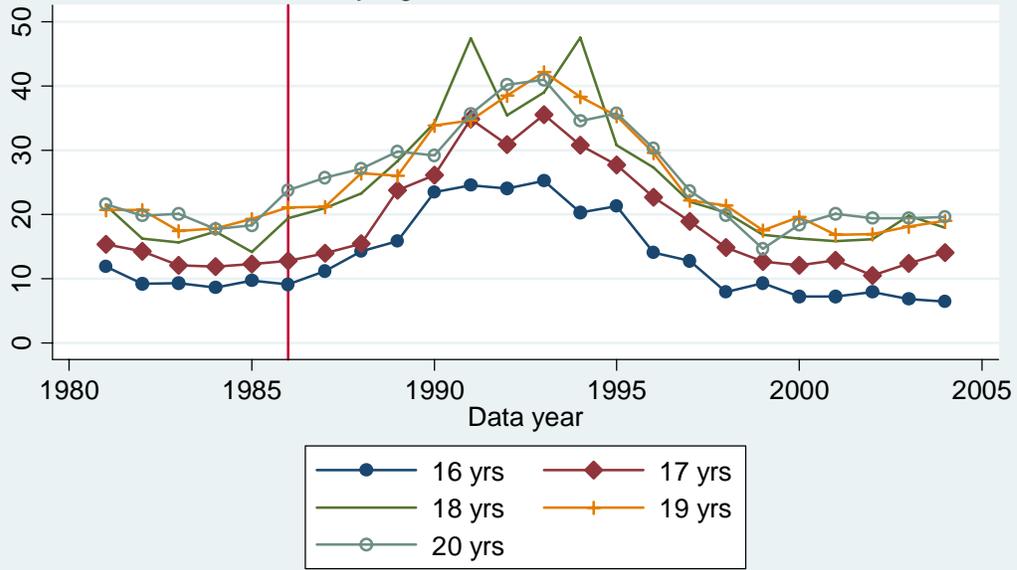
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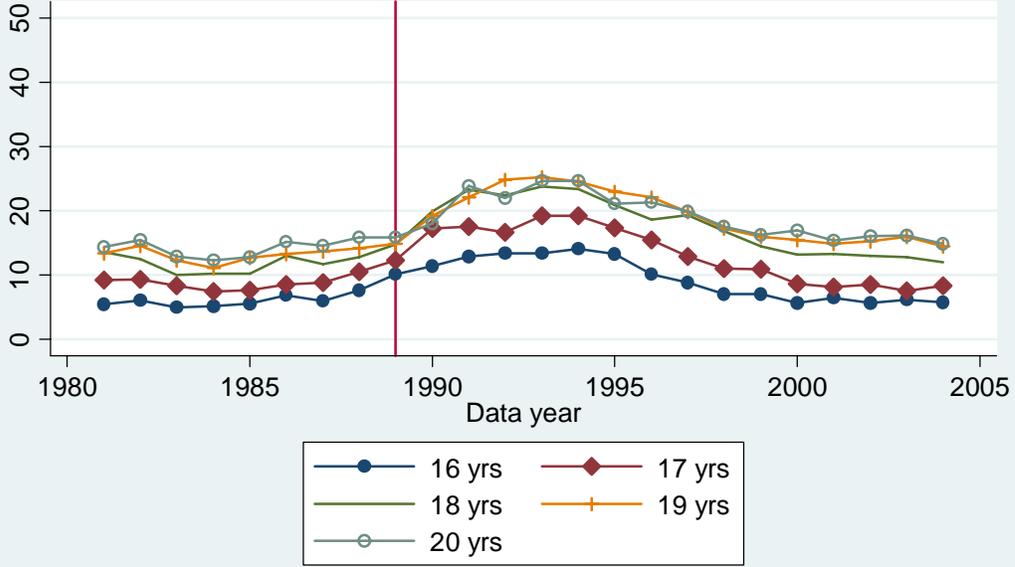
## Appendix

Fig A1: Homicide Victimization Rates--pre-Roe States  
By Age & State of Residence



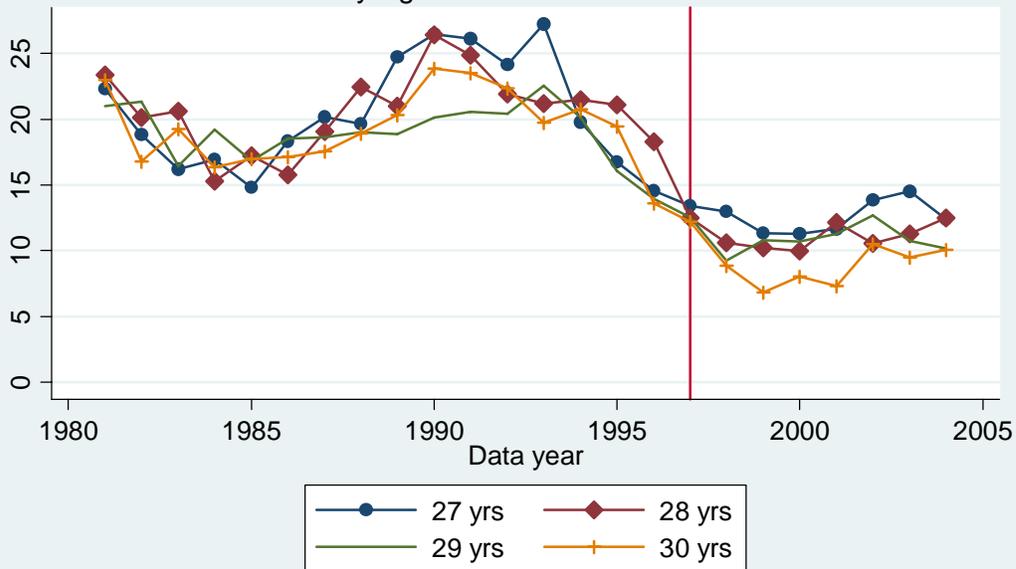
Source: NCHS Mortality Data  
("pre-Roe states: AK,CA,DC,HI, NY, WA")

Fig A2: Homicide Victimization Rates--45 Roe States  
By Age & State of Residence



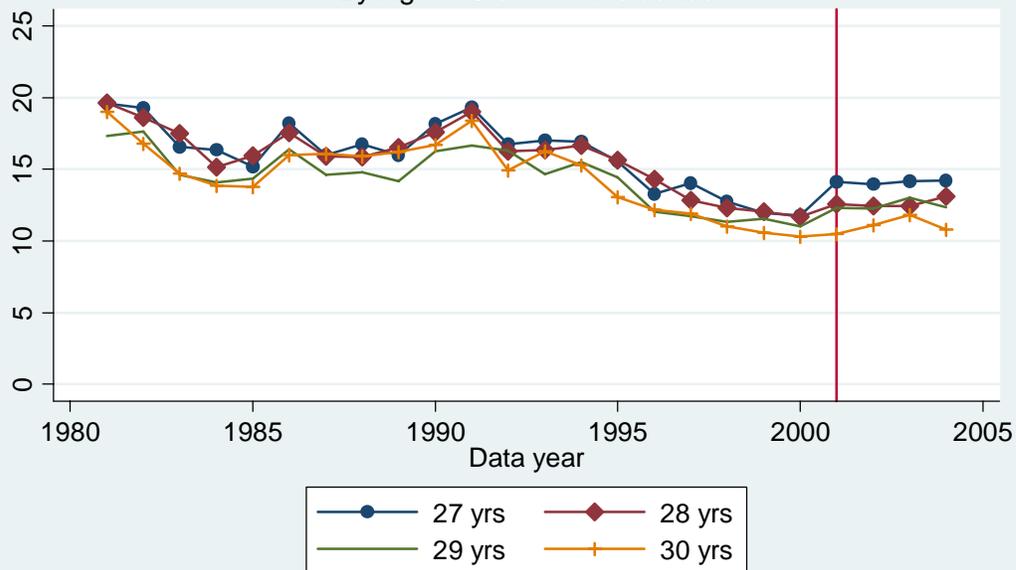
Source: NCHS Mortality Data  
("All states but AK,CA,DC,HI, NY, WA")

Fig A3 : Homicide Victimization Rates - - pre - Roe States  
By Age & State of Residence



Source: NCHS Mortality Data  
("pre-Roe states: AK,CA,DC,HI, NY, WA")

Fig A4: Homicide Victimization Rates--45 Roe States  
By Age & State of Residence



Source: NCHS Mortality Data  
("All states but AK,CA,DC,HI, NY, WA")