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Further Evidence that Legalized Abortion Lowered Crime

A Reply to Joyce

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ABSTRACT

Donohue and Levitt (2001) suggest there is a causal link between legalized abortion and reductions in crime almost two decades later when the cohorts exposed to legalized abortion reach their peak crime years. Joyce (2003) examines crime committed in the period 1985–90 for the cohorts born immediately before and after abortion legalization. He finds little impact of legalized abortion. In this paper, we demonstrate that Joyce's failure to uncover a negative relationship between abortion and crime is a consequence of his decision to focus almost exclusively on one nonrepresentative six-year period during the peak of the crack epidemic. We provide empirical evidence that the crack-cocaine epidemic hit the high-abortion early-legalizing states earlier and more severely than other states. When we simply replicate his analyses, but extend the sample to cover the entire lives of these exact same cohorts, abortion is just as negatively related to crime as in our original analysis. Joyce's results appear to be purely an artifact of omitted variable bias due to focusing on the peak crack years without including adequate controls for crack.

I. Introduction

In Donohue and Levitt (2001), we advanced the hypothesis that the legalization of abortion in the 1970s is causally related to the decline in crime experienced in the United States in the 1990s. We are delighted to have researchers probe the robustness of our initial findings, and a number of researchers have confirmed the link between abortion legalization and crime/deviant behavior that we first identified (Charles and Stephens 2002; Leigh and Wolfers 2000; Pop-Eleches 2003; Reyes

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2002; Sen 2002; Sorenson, Wiebe, and Berk 2002). In contrast to these papers, Joyce (2003) reports findings that he interprets as evidence against a causal link between abortion and crime.

In this reply, we demonstrate that none of the primary arguments that Joyce makes in his paper poses a serious challenge to our hypothesis. First, his claim that legal abortions simply replaced illegal abortions is shown to be directly at odds with the existing evidence. Second, although Joyce is critical of the shortcomings in our abortion data, we demonstrate both theoretically and empirically that the biases due to measurement error in the abortion proxy unambiguously lead our reported coefficients to *understate* the true impact of abortion; Joyce makes a basic econometric error in arguing to the contrary. Third, most of his empirical findings that contradict our original results are revealed to simply be an artifact of his decision to focus his analysis on the small subset of the data that coincides with the peak of the crack epidemic (without including controls for crack in the regressions). We present evidence that crack hit early-legalizing, high-abortion states earlier and harder than the rest of the country. We then demonstrate that if one simply takes Joyce's identification strategy regarding the early-legalizing states, but follows these same cohorts over their entire lifetime rather than just the six-year window 1985–90, abortion exposure is in fact associated with lower criminal involvement. Finally, we show that Joyce's claims about the sensitivity of our results to using particular subsets of the data are inaccurate.

The remainder of the paper is structured as follows. Section II briefly summarizes the basic findings from our earlier work that led us to advance the hypothesis that abortion legalization can lower crime, and underscores that Joyce has not directly challenged any of those findings. Section III then lays out the five primary pieces of evidence that Joyce (2003) presents as evidence against an impact of legalized abortion on crime. In responding to these points, we demonstrate that in each case the inference drawn by Joyce is likely to be false and that these new and different sources of identification that Joyce highlights yield further support to the initial hypothesis of Donohue and Levitt (2001). Section IV offers some concluding comments.

II. The Link between Abortion Legalization and Crime

Donohue and Levitt (2001) present five pieces of evidence consistent with the hypothesis that cohorts born after the legalization of abortion have lower crime rates because legalized abortion reduces the number of unwanted children, who are at higher risk of engaging in criminal conduct when they grow up. First, the five states that legalized abortion in roughly 1970 (as opposed to the national legalization resulting from the January 1973 U.S. Supreme Court decision in *Roe v. Wade*) experienced a somewhat earlier drop in crime. Second, higher abortion states (based on the rates of legal abortion in the 1970s) showed much greater drops in crime during the 1985–97 period. In contrast, the crime trends in high and low abortion states were similar over the period from 1973–85, when the children born after legalization were too young to be influencing crime rates. Third, this relation-

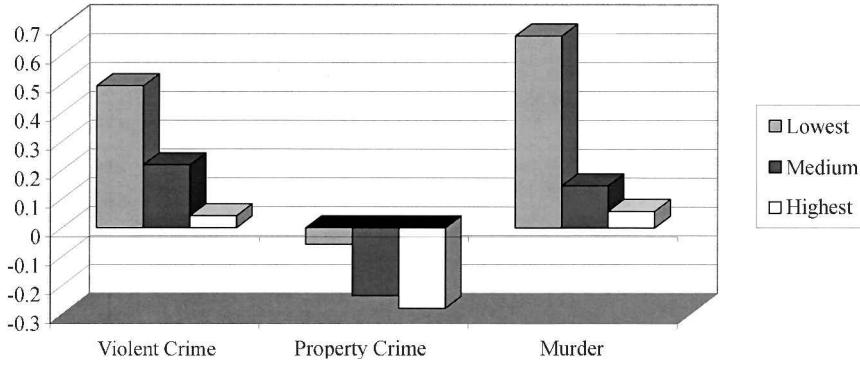

Figure 1
Change in Arrest Rates for Offenders under 25, 1985–1998
States Ranked into Three Categories by the Effective Abortion Rate (abortions by state of residence of birth mother according to AGI)

ship between more legal abortions in the 1970s and lower crime over the period 1985–97 persisted in panel data regression models that controlled for prisoners and police per capita, state economic conditions, and state and year fixed effects. This result was also robust to controls for cross-state mobility, the effects of immigration, and various deletions of potentially idiosyncratic states (including New York and California). Fourth, the link between abortion and crime was only present for those born after legalization (roughly those younger than age 25 when our arrest data ends in 1998), and not for those older than 25 as of 1998 (and therefore born prior to *Roe v. Wade*). Figure 1 shows that, for the period 1985–98, arrest rates for those younger than 25 fell more (or rose less) as one moves from the states with the lowest abortion rates to those with the middle level of abortion rates to those with the highest level of abortions. Figure 2 reveals, however, that the link between higher rates of abortion and lower rates of crime is not observed in these groupings if one limits the focus to those older than age 25, who were overwhelmingly born at a time when abortion was illegal. In this second figure, there is no discernible relationship between the rate of abortion and changes in arrests. Fifth, the pattern of lower rates of crime in states with higher rates of abortion held true when we linked the abortion rates in a particular state in a particular year with the crime committed by the cohort born in that year, even controlling for state-year specific interactions.

III. Joyce's Five Primary Arguments

Joyce does not challenge any of these findings directly, and indeed, confirmed a number of them in previously circulated drafts of his paper (Joyce 2001).

Rather, Joyce presents five different arguments as to why he believes the link between legalized abortion and crime is not causal:

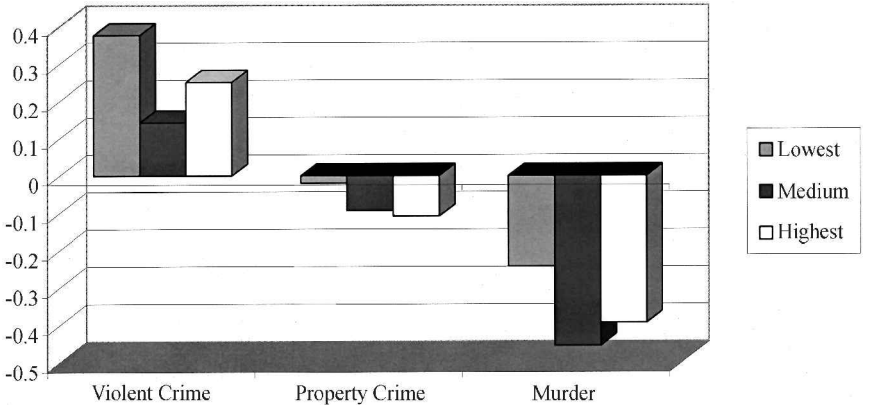

Figure 2
Change in Arrest Rates for Offenders under 25, 1985–1998
States Ranked into Three Categories by 1998 Effective Abortion Rate (abortions by state of residence of birth mother according to AGI)

(1) He claims that demographers have concluded that most legal abortions in the early 1970s replaced illegal abortions, so there should be no impact of legalized abortion. He further argues that measurement error in our abortion proxy may cause our original estimates to overstate the true impact of abortion on crime rates.

(2) For the six-year period 1985–90, there is no measurable impact of state abortion rates on state crime rates; only after 1991 does the strong negative relationship between abortion and crime emerge (Tables 1 and 3 of Joyce).

(3) A comparison of cohorts born just before or after the early legalizations in 1970 in legalizing and nonlegalizing states does not yield negative impacts of abortion on crime for the years 1985–90 (Table 4 of Joyce).

(4) In states where abortion only became legal in 1973 with the passage of *Roe v. Wade,* those born after legalization do not have systematically lower crime rates than those born before (Table 5 of Joyce).

(5) He finds that states that legalized abortion in 1970 experienced initial reductions in crime consistent with a causal impact of legalized abortion, but the fact that these early legalizers continued to experience greater reductions in crime even after abortion became legal nationwide argues against causality (Table 2 of Joyce).

We address these five points in turn.

Joyce Claim 1:

Demographers have concluded that most legal abortions in the early 1970s replaced illegal abortions, so there should be no impact of legalized abortion.

Measurement error in our abortion proxy may cause our original estimates to overstate the true impact of abortion on crime rates.

It is true, and for obvious reasons, that no one has good data on the number of illegal abortions. Both theory and evidence, however, strongly suggest that the prevalence of abortion rose sharply after legalization. From a theoretical perspective, there is no question that the dollar cost, the medical risk, and the taint associated with engaging in illegal conduct fell after legalization, all of which would lead to higher rates of abortion. With respect to the dollar cost, Kaplan (1988, p. 164) notes that "an illegal abortion before *Roe v. Wade* cost $400 to $500, while today, thirteen years after the decision, the now legal procedure can be procured for as little as $80." Kahane (2000) documents that the number of abortions is quite responsive to the price, so on that basis alone one would predict substantial jumps in the number of abortions after legalization. Empirically, if legal abortions simply replaced illegal ones, it is hard to understand why it took seven years after *Roe* for the numberof *legal* abortions to reach a steady state. The number of legal abortions morethan doubled between 1973 and 1980. Michael (1999) using self-reported data on pregnancy outcome histories finds abortion rates to be roughly an order of magnitude higher after legalization. Consistent with this finding is a dramatic decline in the number of children put up for adoption after abortion became legal. According to Stolley (1993), almost 9 percent of premarital births were placed for adoption before 1973; that number fell to 4 percent for births occurring between 1973 and 1981.

The increase in abortions, however, does not generate a one-for-one decline in fertility since the availability of legalized abortion is likely to lead to an increase in unintended pregnancies. This moral hazard effect, as well as the replacement effect of illegal abortions, are both factors that would cause the number of legal abortions to overstate the reduction in unwanted births caused by legalization. Of course, the ability to improve the timing and circumstances of births means that abortion improves birth and life outcomes even when it does not lower the total number of births. A woman who has an abortion after being raped, and who subsequently gives birth following a later, wanted pregnancy is improving the life chances of her child. As long as the number of unwanted births falls, even if total births do not decline at all, one would expect to see better life outcomes on average for the resulting cohorts.

In our initial paper, we acknowledged that our abortion data were imperfect for a variety of reasons. One shortcoming was that our original Alan Guttmacher Institute data computed abortion rates by the state of occurrence, when we would have preferred to have abortion data by the residence of the mother. After our initial paper was published, Stanley Henshaw of the Alan Guttmacher Institute provided these data by residence of the mother. In Table 1, we explore the impact of this and other issues related to the appropriate measure of abortions. Each row of the table represents a different dependent variable, specification, or sample. The first column of Table 1 provides estimates based on the original specifications presented in Donohue and Levitt (2001) using our original place of occurrence data (and some modestly updated crime data), while Column 2 uses the better state of residence abortion data. Comparing Columns 1 and 2, in every case the coefficients become more negative

Table 1
Estimates of the Impact of Abortion on Crime with Different Abortion Rate Measures

	Abortion Proxy				
Specification and dependent variable	Abortions per 1,000 live births, by state where procedure performed (noisier proxy, used in original Donohue and Levitt paper)	Abortions per live birth by state of residence of birth mother (better, newly available proxy)	Identical to Column 2, but backcast prelegalization abortion rates for all states	Abortions per 1,000 live births, using CDC data rather than AGI abortion data	Defining abortion rate as abortions per 1,000 women aged 15-44, rather than as abortions per 1,000 live births
ln (state crime rates) (includes criminals of all ages)					
Violent crime	−0.129	−0.153	−0.158	−0.125	−2.803
	(0.024)	(0.030)	(0.029)	(0.039)	(0.563)
Property crime	−0.091	−0.111	−0.117	−0.075	−2.213
	(0.018)	(0.018)	(0.019)	(0.025)	(0.308)
Homicide	−0.121	−0.166	−0.168	−0.108	−2.875
	(0.047)	(0.055)	(0.055)	(0.061)	(0.929)

ln (arrest rates younger than age 25) minus ln (arrest rates older than age 25)					
Violent crime	−0.062	−0.107	−0.117	−0.098	−1.713
	(0.034)	(0.041)	(0.039)	(0.050)	(0.757)
Property crime	−0.063	−0.092	−0.093	−0.112	−1.394
	(0.019)	(0.024)	(0.024)	(0.030)	(0.435)
Homicide	−0.137	−0.179	−0.180	−0.122	−3.088
	(0.046)	(0.070)	(0.075)	(0.078)	(1.198)
ln (arrest rates by single year of age)					
Violent crime	−0.027	−0.040	−0.052	−0.037	−0.598
	(0.004)	(0.006)	(0.007)	(0.007)	(0.116)
Property crime	−0.024	−0.032	−0.038	−0.033	−0.529
	(0.003)	(0.004)	(0.004)	(0.004)	(0.064)

Notes: All entries in the table represent coefficients estimated from separate regressions. The regressions underlying Column 1 use abortions per 1,000 live births by the state in which the procedure was performed. This cruder proxy was the only data available at the time of Donohue and Levitt (2001). Note that some numbers in Column 1 are slightly different than those presented in Donohue and Levitt (2001) simply because of minor UCR crime data updates. The regressions underlying Column 2 use abortions per 1,000 live births by the state of residence of the birth mother, data newly made available by the Alan Guttmacher Institute (AGI). The regressions underlying Column 3 estimate pre-1973 abortion rates for all states by backcasting from the 1973 figure. The regressions underling Column 4 use abortions per 1,000 live births by the state of residence of the birth mother from the CDC. The regressions underlying Column 5 use abortions per 1,000 women between the ages of 15 and 44. The abortion numbers are by the state of residence of the birth mother from the AGI. Results in the top, middle, and bottom panel of the table correspond to specifications in Tables 4, 6, and 7 of Donohue and Levitt (2001). While our earlier paper provides a more complete description of the specifications used, essentially the top panel tests the impact of abortion legalization on overall crime across states, the middle panel narrows the focus to separate out the influence on the younger-than-25 age cohorts that were born after abortion legalization, and the bottom panel links the abortion rates in a state at the time a particular cohort was born with the crime rate of that cohort over time. In all cases, the full set of covariates used in our earlier paper is included in the regressions (but not shown here). Standard errors, corrected for first-order serial correlation, are in parentheses. The top panel covers the period 1985–97, while the second and third use the period 1985–96.

when the better proxy is used. Moreover, the coefficients in Column 2 are 40 percent larger in magnitude on average than the coefficients in Column 1.[1]

The remaining columns of Table 1 demonstrate how the use of other improved abortion proxies also strengthens our original results. Admittedly, abortion rates are a relatively crude proxy for unwantedness, both because of poor data on prelegalization abortions (both illegal and whatever abortions occurred legally prior to 1970 in repeal states and prior to 1973 in nonrepeal states) and because abortions do not map one-for-one into unwanted births averted. As is typically the case, one expects the presence of measurement error to result in attenuation of any estimates obtained. This is particularly true in the current setting in which abortion rates are highly positively serially correlated over time. To the extent that prelegalization abortions were being performed and at greater rates in states that would have high abortion rates after legalization (as Joyce himself argues quite convincingly on page 8 of his article), our assumption of no prelegalization abortions unambiguously leads us to understate the true impact of abortion on crime in our analysis. The intuition for the attenuation is simple. By assuming that no prelegalization abortions were occurring, we systematically overstate the increase in abortions associated with legalization, especially in the states with the highest legal abortion rates (because these same states likely had the highest illegal abortion rates). The measured change in crime, on the other hand, is unaffected by the exaggerated estimate of the change in abortion. Thus, the abortion coefficients are biased towards zero: the regression believes that the change in crime was induced by a larger shock to abortion than actually occurred and thus attributes a smaller impact per abortion than is actually the case. Joyce's argument to the contrary is simply faulty econometrics.[2]

For instance, in the simplest case with two periods corresponding to pre- and post-legalization, the fixed-effects model reduces to a first-difference model: $\text{Crime}_{\text{post,s}} - Crime_{pre,s} = \beta(Abortion_{post,s} - Abortion_{pre,s}) + \varepsilon_{post,s} - \varepsilon_{pre,s}$, where s indexes states. In our initial work, we did not observe $Abortion_{pre,s}$, instead treating it as if it were equal to zero. Thus, the specification we estimated was $Crime_{post,s} - Crime_{pre,s} = \beta Abortion_{post,s} + \varepsilon_{post,s} - (\varepsilon_{post,s} + \beta Abortion_{pre,s})$, with the abortions from the pre period in the error term. The coefficient on our abortion measure will converge to $\beta * [1 - \text{cov}(Abortion_{post}, Abortion_{pre})/\text{var}(Abortion_{post})]$. All that is required for attenuation bias in our fixed effects model is that there is a positive correlation across states in abortion rates before and after legalization, which Joyce argues is the case.[3]

1. Using the better abortion proxy also improves the results of the specifications used by Joyce, and indeed, many of his null findings regarding abortion disappear when the better data are used. For instance, with respect to the cohorts born after 1973 in Joyce's Table 1b, he finds no impact of abortion on arrest rates in this subsample, but when we run those precise regressions using the better abortion data, we obtain negative coefficients on all three crime categories. Furthermore, when we extend the arrest rate data through 1998, two of the three coefficients become negative and statistically significant.

2. In his formal derivation in Footnote 3, Joyce ignores the fact that state-fixed effects are included in the regression.

3. Assessing the true selection effect is more complicated than this simple discussion suggests if the probability of unwanted conception and the probability of abortion change differentially among the two groups once legalization occurs. It is most likely, though, that this complication would only buttress our conclusion, because abortions were more likely available to the more educated prior to abortion legalization in their state of residence (suggesting that the change in the abortion rate post-legalization was smaller for the more educated group).

The empirical results in the third column of Table 1 reinforce the theoretical argument that our initial estimates are understated due to attenuation bias. One means of proxying for the prevalence of illegal abortions is simply to linearly backcast into the period prior to legalization using the post-legalization abortion rates. While Column 2 of Table 1 shows our results using the better state of residence abortion data while retaining our original assumption of no abortions prior to full legalization, Column 3 presents the results using linear backcasting. Just as theory would suggest, in each case using our original assumption leads to estimates that are *smaller* in absolute value due to attenuation bias.

Joyce also alludes to two other alleged defects in our abortion data. First, he suggests that the abortion data (by state of residence) from the Centers for Disease Control (CDC) might be preferable to the AGI data. But the results in Column 4 of Table 1 reveal that higher rates of abortion continue to correlate with reduced crime when using CDC data instead of AGI data.[4] Second, Joyce contends that our results may be spurious because we use abortions per live birth as a proxy for unwantedness, as opposed to his preferred measure—abortions per woman of childbearing age. Column 5 presents estimates using abortions per woman of childbearing age. Because this proxy is scaled differently than our abortion rate measure, one cannot directly compare the coefficient estimates. Whether one judges by the impact on crime of a one standard deviation change (one standard deviation is 214.99 for abortions per 1,000 live births and 13.12 for abortions per 1,000 women of childbearing age) or by the t-statistics of the estimates, the two proxies perform equally as well.

Finally, we were quite surprised by Joyce's arguments that abortion legalization had no impact on the composition of births given that his own previous research provides some of the most compelling evidence for the impact of abortion legalization on birth outcomes through precisely the channel we hypothesize. In Joyce (1985), a paper entitled "The Impact of Induced Abortion on Birth Outcomes in the U.S.," Joyce used state-level abortion rates after legalization as a covariate to explain health outcomes of babies, finding that "by reducing the number of unwanted births, abortion enhances the healthiness of newborns of a given weight and gestational age, as well as improving the distribution of births among high-risk groups." In other words, the legalization of abortion reduces unwanted births and improves the life prospects of cohorts born after legalization by increasing the chance that they are healthy and wanted by their mothers. Indeed, in a 1987 published paper, Joyce underscored this point:

> The findings from this study support the contention that by preventing unwanted births, abortion reduces the percentage of preterm and low-weight infants. Few would argue that averting unintended pregnancies is the most preferable strategy for reducing unwanted births. However, until family planning is more widely practiced, especially among women of low socioeconomic status, abortion will

4. One shortcoming of the CDC data is that it is only available from 1971–81. To obtain 1970 abortion figures, we backcast from the 1971 values. The last two rows in Table 1 do not require abortion data beyond 1981, but the first six rows are constructed using effective abortion rates that do extend beyond that date (see footnote 9 below for the definition of the effective abortion rate). Thus, our estimates in the first six rows of Column 4 using this CDC data are somewhat diminished because of this data shortcoming.

> remain an important option for many pregnant women. According to the estimates from the study, legislative attempts to ban abortion would have a negative impact on birth outcomes. (Joyce, 1987: 241)

Hence, if legalizing abortion has a positive effect on birth outcomes then 1) legal abortions cannot be simply replacing formerly illegal abortions, and 2) it should not be surprising that legalizing abortion would have a positive effect on life outcomes such as criminal activity as well.

Despite his previous writings, Joyce now surprisingly suggests that any selection effect caused by abortion should cut in the opposite direction of our finding because, in his words, "a careful examination of studies of pregnancy resolution reveals that women who abort are at *lower* risk of having children with criminal propensities than women of similar age, race and marital status who instead carried to term" (p.24, emphasis supplied). Joyce then cites studies that he claims show that women who abort as opposed to carry a pregnancy to term are more educated, more intelligent, and more affluent. But Joyce's statements are highly misleading. The studies that Joyce cites do not address the overall probability that a woman will have an abortion (which depends not only on what she will do if she has an unwanted conception but also the likelihood of having an unwanted conception) but only the conditional probability of an abortion by girls and women who are already pregnant. For example, Joyce cites Leibowitz, Eisen, and Chow (1986: 69), who note that their study "abstracts from the decisionmaking process that led the teenager to an out-of-wedlock conception and focuses on the decision of unmarried teenagers as to the outcome of their pregnancies."

While it may well be the case that highly intelligent, highly educated, and affluent teens *who become pregnant* are more likely to have an abortion than unintelligent, uneducated, poor teens (the finding Joyce stresses), it is also much more likely that members of the latter group will find themselves in the situation of having an unwanted pregnancy and therefore have a much higher overall likelihood of having an abortion. It is this overall probability of having an abortion that is most important for our purposes, as the following example illustrates. Assume that there are two, equal-sized groups of females—Groups 1 and 2, where those in Group 1 are more highly educated and intelligent. The evidence we cited in our original paper indicated that the overall probability of abortion, $P(Abortion)$, is lower for Group 1 than for Group 2 (that is, $P_1(Abortion) < P_2(\text{Abortion})$). But since $P(Abortion) = P(Unwanted\ Conception)*P(Abortion/Unwanted\ Conception)$, it could well be the case that the second probability could be higher for Group 1 even though $P(Abortion)$ is lower, as long as $P(Unwanted\ Conception)$ is substantially lower for Group 1 (as all of the available evidence suggests is the case). Thus, the evidence that Joyce cites on the conditional probability $P(Abortion/Unwanted\ Conception)$ is of little relevance to determining the overall selection effect as long as the unconditional probability $P(Abortion)$ is lower for the higher education group (as the evidence we cited in our original article indicated).

Joyce Claim 2:

For the six-year period 1985–90, there is no measurable impact of state abor-

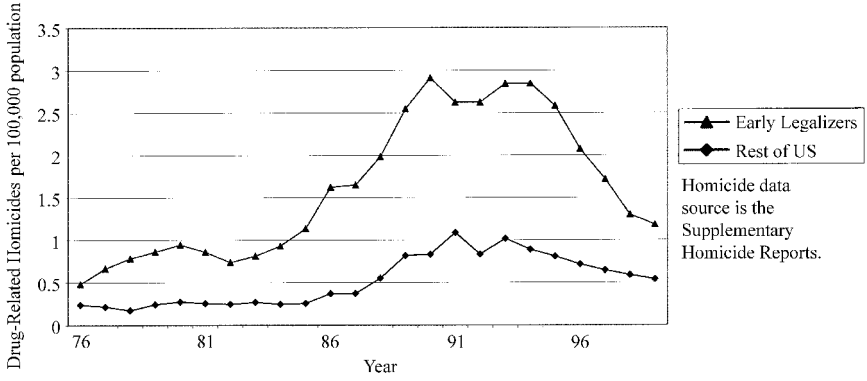

Figure 3
Drug-Related Homicide Rates for 5 States Legalizing Abortion Early and for the Rest of the United States (1976–1999)

tion rates on state crime rates; only after 1991 does the strong negative relationship between abortion and crime emerge.

Joyce Tables 1 and 3 examine the six-year period from 1985 through 1990, finding that the link between abortion rates and crime is not evident in this period. These years correspond almost exactly to a spike in violence typically attributed to crack cocaine. The data suggest that the impact of crack was extremely heterogeneous geographically and demographically, with the effects concentrated on youths (especially minorities) in large urban centers. Thus, the cohorts most directly affected by crack are those born immediately after abortion legalization. Because the early-legalizing states of California and New York have a disproportionate number of minorities residing in large cities, failure to control for crack is likely to induce a bias against finding a crime-reducing impact of legalized abortion. Unfortunately, the crack epidemic has not proven amenable to easy quantification. Without good data for each state, year, and cohort on the criminogenic influence of crack, any regression model will have a serious omitted variable problem during the major crack epidemic of the late 1980s that caused dramatic crime increases. Given the magnitude of the shock associated with crack (youth homicide rates more than doubled in a five year period), any effects of legalized abortion could be easily swamped. Thus, Joyce pins much of his argument against an effect of abortion legalization on crime by focusing on exactly the period when crime regression models will have the hardest time teasing out *any* effect on crime.[5]

Figure 3, which depicts the rate of drug-related homicides from 1976–99 in the

5. Indeed even a statistically significant impact of incarceration on violent crime and homicide—an extremely robust result in the literature—disappears in Joyce's regressions on the 1985–90 period. In contrast, for the full 1985–97 time period that Donohue and Levitt use, the estimated impact of incarceration on homicide and violent crime is highly statistically significant. A similar pattern emerges regarding the impact of police on crime. Over the whole sample, the coefficient on police is negative and statistically significant. For the period 1985-90, the police coefficient is positive.

early-legalizing states relative to the rest of the country, provides empirical evidence substantiating the conjectures of the previous paragraph.[6] Throughout the time period, the early-legalizing states have a higher rate of drug-related homicides. Over the period 1985–90, drug-related homicides increase everywhere, but the gap between the states that legalized abortion in 1970 and the rest of the country grows prominently. The year 1990 represents the point at which the gap in drug homicides between early-legalizers and the rest of the nation is greatest—more than four times the magnitude of the gap in the early 1980s and about three times the magnitude of the gap in the late 1990s.

If the crack epidemic is a temporary phenomenon in the late 80s and early 90s that hits the high-abortion, early-legalizing states more severely, then regression estimates that fail to adequately control for crack will be biased *against* finding a causal impact of legalized abortion for the years 1985–90 and will be biased *in favor* of legalized abortion in the latter period as the epidemic subsides. That story is completely consistent with the pattern of results in Joyce's Table 1. In order to make reasonable inferences about the link between abortion and crime, one needs to look at the entire period: before, during, and after the peak of the crack epidemic. To the extent that early legalizing states were affected more severely by crack, using data for the whole period will still be biased against finding an impact of legalized abortion on crime, but to a lesser degree than if one looks only at the peak crime years. Concerns about the confounding influence of crack led us to analyze long-difference models using only data from 1985 and 1997 in Table V of our original paper, obtaining estimates that are similar to our baseline model.[7]

It is widely believed that the impact of crack was concentrated on homicide and violent crime, and much smaller for property crime.[8] The results in Joyce's Table 1 show that legalized abortion was associated with drops in property crime, even during the 1985–90 period. This finding is once again consistent with Joyce's results being driven by crack as an omitted variable.

Finally, it should be emphasized that the effective abortion rate (the key explanatory variable in Joyce Table 1a) tends to be quite low during the period from 1985–90, particularly for violent crime and murder.[9] Further, Joyce acknowledges that the

6. As discussed in Footnote 10 below, we continue to adhere to our definition of the early-legalizing states and thus do not include the District of Columbia in that category in Figure 3.

7. This long difference estimation essentially enabled us to relate the abortion rate for all 50 states at roughly the time of legalization in the 1970s with the change in crime between 1985 and 1997. This approach was specifically designed to address the problem of the impact of the crack cocaine phenomenon by seeing how crime changed in the period before crack to a period when the criminogenic influence of crack had largely subsided. For all three crime categories, the abortion rate in a state was negatively correlated with the change in crime in that state over the 1985–97 period (and the estimates for violent and property crime were statistically significant at the 0.05 level).

8. Indeed, while the murder rate soared and the National Crime Victimization Survey reports substantial increases in violent crime during the "crack years," property crime actually fell over this period from a rate of 385 per 100,000 households in 1985 to 349 in 1990.

9. The "effective abortion rate" is a weighted average of the abortion rates for a particular state that would be expected to influence crime in that state in a particular year, where the weights are based on the proportion of crime committed by individuals of a particular age. The concept is needed to provide a single measure of how abortion legalization would be expected to influence crime in a certain state in a certain year. Thus, in computing the 1990 effective abortion rate for a state that legalized abortion in 1970, one would take the weighted average abortion rate for all abortions between 1970 and 1989 weighted by the

effective abortion rate for murder was only 8 per 1,000 live births between 1985 and 1990—roughly one-twentieth the level reached by 1997, and one-fortieth of the steady-state level once all potential criminals are born after legalized abortion. Thus, in addition to the other problems associated with the 1985–90 period, Joyce's estimates of the effect of abortion on crime are being identified off of very small amounts of variation relative to the latter part of the sample.

Joyce Claim 3:

A comparison of cohorts born just before and after 1970 (when the early-legalizing states legalized) in early-legalizing and nonlegalizing states does not yield negative impacts of abortion on crime for the years 1985–90.

In Table 4 of his paper, Joyce attempts to identify the impact of legalized abortion on crime by exploiting the fact that a subset of states legalized in 1970, three years before *Roe v. Wade*.[10] By comparing the crime rates of individuals born in the years immediately before and after 1970 in states that did or did not legalize early, one can generate a "differences-in-difference-in difference" estimate of the impact of legalization on crime.

The idea is a sensible one, modeled directly after the identification strategy of Gruber et al. (1999). In actually carrying out his analysis, however, Joyce makes a fateful choice: rather than looking at the lifetime criminal involvement of individuals born in the years surrounding 1970, he only looks at the crimes they commit in the years 1985–90. Because of the way in which Joyce assembles treatment and control groups in his triple-difference estimates, and the fact that he uses arrest data that does not disaggregrate by single year of age for those older than 24, he cannot look at crime for these cohorts at other ages. For the period 1985–90, he finds no evidence of a drop in crime for the treatment group exposed to legalized abortion.

proportion of crimes committed by each individual age from 1 to 20. (Obviously, since 20-year-olds commit a good deal of crime while 1-year-olds commit none, the contribution of the abortion rate in 1970 to the effective abortion rate is high, while that of the abortion rate in 1989 is zero.)

10. Unlike previous scholars, Joyce includes the District of Columbia as an early legalizer on the basis of a 1974 book by Lawrence Lader, entitled: "Abortion II: Making the Revolution." Lader does in fact claim that the District Court decision in *U.S. v. Vuitch* "made Washington the first area in the country where abortion was completely legalized." (P.111.) *U.S. v. Vuitch*, 305 F. Supp. 1032 (Dist. D.C. 1969) involved a doctor who was indicted for performing an abortion in violation of the D.C. statute that made it a felony to perform an abortion, unless it was done 1) for the preservation of the mother's life or health, and 2) under the direction of a competent licensed practitioner of medicine. The trial court in *Vuitch* held that the first exception was unconstitutionally vague as the term "health" was ambiguous, and the burden was on the doctor to show that an abortion was necessary for the mother's life or health. (It also held that the statute was severable and that the second exception remained valid despite the invalidity of the first exception.) While this was obviously a pro-legalization decision, it was a decision of a lower court and it was quickly overturned on appeal by the United States Supreme Court in the case of *U.S. v. Vuitch*, 402 U.S. 62 (U.S. 1971), which upheld the DC abortion law. Consequently, the most comprehensive assessment of the various changes in abortion law—Merz, Jackson, and Klerman (1995)—does not identify the *Vuitch* case as legalizing abortion in the District, and the other scholars who have written in this area have not included DC as an early legalizer. The Supreme Court's decision in *Vuitch* did give some comfort to those wishing to provide abortion services in the District, though, because it interpreted the DC abortion statute (which had been unchanged since 1901) as not establishing criminal liability if the doctor acted to preserve the psychological or physical health of the mother. The potential for criminal prosecution of DC abortion providers was not lifted until the decision in *Roe v. Wade* in 1973.

As we argued above, the years 1985–90 are precisely the period in which crack was differentially affecting the *early-legalizing* states, especially New York and California. The first cohort exposed to legalized abortion reached its teenage years at just the time that the crack epidemic got started. Table 2 demonstrates how radically different the conclusions are if one carries out the same exercise, looking not just at a six-year period in their teens, but at criminal involvement of these cohorts throughout their whole life. We report triple-difference estimates in Table 2 that mirror the estimates shown in Row 3 of Joyce's Table 4.[11] We replicate his specification with one important difference: We use the Supplemental Homicide Report data in place of arrest data so that we are not constrained to look at crimes that occur below the age of 25. Thus, we are able to look at crime over the entire lives of the cohorts in question, not just one well chosen point in time when these cohorts were in their late teens.[12] We use exactly the same birth cohorts and exactly the same definition of treatments and controls as Joyce; we just look at these cohorts at various points in their life.

For purposes of comparison to Joyce's Table 4, it is worth starting with the estimate of the impact of abortion legalization over the period 1985–90, which we have indicated with boldface in the table. This is the only time period for which Joyce reports estimates. Just as in Joyce's Table 4, we find a large positive association between exposure to legalized abortion and crime in that time period. Perusal of data over the whole lifecycle of these same cohorts, however, tells a dramatically different story. Of the 15 estimates we obtain, 12 carry a negative coefficient as predicted by our original hypothesis. Both before and after the crack epidemic, those cohorts exposed to legalized abortion commit fewer homicides. This finding is even more compelling when one realizes that Joyce's cohorts are not well-defined into "exposed" and "unexposed" groups and the regressions are, therefore, less likely to find any effect.[13] Overall, taking an average of the 15 coefficient estimates (as reported in the final row of the table) those born in *early-legalizing* states immediately after legalization have experienced *lower* lifetime crime involvement, even having come of age in the peak crack years in the states most drastically affected by crack.

The results in Table 2 are highly consistent with a scenario in which exposure to abortion systematically reduced criminal involvement of those born immediately after legalization in the early-legalizing states, but the effects of abortion were tempo-

11. Very similar results hold if one uses much simpler and more intuitive approaches in place of Joyce's triple-difference methodology. See, for instance, Table 3 of Donohue and Levitt (2003).

12. There are also three other minor differences between our specifications and Joyce's. First, Joyce weights his regressions by age-specific population data while we have simply used the entire state population. Second, Joyce includes a few control variables for which we could not get data for all of the Table 2 regressions (through 1999), so we omitted these in all the regressions to maintain consistency. Third, Joyce used arrest rates (arrests per person) while we have used the number of homicides, not homicide rates. Nonetheless, our results match his results closely in the period for which he reports estimates.

13. Specifically, Joyce's DDD approach strives to compare how teens fare in 1985–6 versus 1989–90 in the two sets of states (relative to young adults) depending on whether the teens had been born prior to or after abortion legalization. But some 15- and 16-year-olds in repeal states in 1985–86 were born after abortion legalization, so the prelegalization teen category in the repeal states is muddy. Similarly, the post-legalization teen category in nonrepeal states in 1989–90 is muddy because some 15- and 16-year-olds at that time may have been born prior to legalization.

Table 2
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

Years of Data Included	Ages of "Exposed" Cohort	DDD Estimate of Impact of Exposure to Legalized Abortion
80–85	10–14	−0.296 (0.150)
81–86	11–15	−0.171 (0.106)
82–87	12–16	−0.281 (.115)
83–88	13–17	−0.031 (0.097)
84–89	14–18	0.173 (0.135)
85–90	**15–19**	**0.158 (0.098)**
86–91	16–20	−0.090 (0.080)
87–92	17–21	−0.185 (0.093)
88–93	18–22	−0.165 (0.075)
89–94	19–23	−0.020 (0.061)
90–95	20–24	0.030 (0.059)
91–96	21–25	−0.178 (0.115)
92–97	22–26	−0.105 (0.178)
93–98	23–27	−0.108 (0.106)
94–99	24–28	−0.069 (0.072)
Average over all years		−0.089 (0.028)

Notes: Each entry in the table reflects the coefficient on the DDD variable, which compares the difference in homicides offenses of exposed to unexposed groups in repeal states versus nonrepeal states. These regressions replicate Joyce's Table 4 with three exceptions noted in the text. The bolded row represents the only set of years that Joyce reported. The final row of the table is the unweighted average of the DDD coefficients, treating the different estimates as independent. State and year fixed effects included. Standard errors are in parentheses. The number of observations varies across crime categories because of observations in which homicide data are missing or equal to zero.

rarily masked by a disproportionately severe crack epidemic in these states in the late 1980s. Table 2 is not at all consistent, however, with a story in which exposure to crack, rather than abortion, is the primary difference between these cohorts. If that were the explanation, one would expect similar crime rates pre-crack, crime rates that were especially elevated in the late 1980s, and presumably similar or still elevated crime rates in the 1990s as crack weakens but does not disappear.

Joyce Claim 4:

In states where abortion only became legal in 1973 with the passage of Roe v. Wade, those born after legalization do not have systematically lower crime rates than those born before. (Note, however, that Joyce makes no distinction between states with high abortion rates versus those with low abortion rates. It is identified only from national time-series variation in crime rates.)

Joyce's fourth challenge to our initial hypothesis is presented in Table 5 of his paper. In that table, Joyce compares the crime rates of those born in the years immediately prior to *Roe* and the years immediately after. Early-legalizing states are excluded from the analysis. He finds that the number of crimes committed between the ages of 18 and 22 (as well as 20–24) was somewhat higher among those individuals born in 1974–76.

There are two important points to make about this analysis. First, these estimates in the table are identified from national time-series trends.[14] The comparison made in the table is simply between those born before and after legalization. Any factor (such as crack) which is correlated with abortion rates and differentially affected the propensity to commit crime across ages or cohorts during the time period he examines will bias his estimates. The analysis does not differentiate between states in which it was very easy to get abortions after legalization (for example, Kansas had 414 abortions per 1, 000 live births in 1973) and states where it remained quite difficult (for example, not a single legal abortion was performed in North Dakota or Louisiana in 1973 in spite of de jure legalization). Second, Joyce inexplicably limits his analysis to just a few years of the lives of these cohorts, rather than using all of the available data.

Given the dramatic aggregate fluctuations in youth crime rates that were observed during this time period and the plausible alternative explanation (crack), a more direct and believable approach to identifying the impact of legalized abortion on crime uses within-state or even within-state and within-age variation over time. Thus, we estimate specifications of the following form

14. The line of argument that the national time-series trends are not supportive of the abortion argument in this time period is not new, having been previously presented by both Sailer (1999) and Cook and Laub (2001). Indeed, we addressed this point in our initial article, stating that "this finding is not inconsistent with the central claim that abortion legalization contributed to lower crime rates, but merely shows that this dampening effect on crime can be outweighed in the short term by factors that can stimulate crime. Elevated youth homicide rates in this period appear to be clearly linked to the rise of crack and the easy availability of guns. That abortion is only one factor influencing crime in the late 1980s points out the caution required in drawing any conclusions regarding an abortion-crime link based on time-series evidence alone." Donohue and Levitt (2001: 395.) After seeing an earlier draft of our reply, Joyce did add a brief verbal description of the results he obtained when dividing states into those with abortion rates above and below the median.

$$\ln (Crime_{csy}) = \beta\, Abort_{cs} + \theta_c + \gamma_{sa} + \lambda_y + \varepsilon_{cst}$$

where *c, s, y,* and α, represent cohort of birth, state, year, and age respectively. The variable *Crime* captures either arrests in a particular crime category, or homicides from the Supplemental Homicide Reports; *Abort* is the number of legal abortions per 1000 live births that a particular cohort was exposed to in utero. Because of the richness of the data (the variation is at the level of state-cohort-year, rather than simply state-year), we are able to control for national cohort effects, national year effects, and (in some specifications) state-age interactions in crime rates.[15] Unlike the estimates in Joyce's Table 5, our estimate of β is identified off of within-state (and even within state and age) variation over time in abortion exposure.

The results of estimating Equation 1 for the same cohorts used in Joyce's Table 5 (those born 1968–76) are presented in Table 3. For each of the three arrest categories, as well as homicide offending rates (based on Supplemental Homicide Report data), we present the abortion coefficient from two different specifications: one with state and age fixed-effects and the other with state-age interactions. Only the coefficient on the abortion rate measure is reported in the table. In every specification, the abortion variable carries a negative coefficient, as predicted by our theory. The estimates are statistically different from zero in six of the eight columns. Indeed, the coefficients are *larger* in absolute magnitude than those obtained using the full-sample of all cohorts from all states presented in Table 7 of Donohue and Levitt (2001). Thus, there is no support for Joyce's claim that our results are sensitive to focusing on the set of cohorts born just before and after *Roe,* or that our results are dependent on including the early-legalizing states in the analysis (early-legalizing states are excluded in Table 3). Rather, the difference between our results and Joyce's Table 5 hinge critically on the fact that his identification is driven by national-time series variation in a period of sharply rising youth crime rates that coincides with the crack epidemic.

Joyce Claim 5:

States that legalized abortion in 1970 experienced initial reductions in crime consistent with a causal impact of legalized abortion, but the fact that these early legalizers continued to experience greater reductions in crime even after abortion becomes legal nationwide argues against causality.

The results presented in Joyce's Table 2 confirm that crime dropped relative to other states for cohorts born in early-legalizing states after legalization. His regression also suggests that the birth cohorts born in early-legalizing states in the years after 1973 seem to show even *greater* drops in crime, which Joyce takes as evidence against our hypothesis, but only because he fails to understand the hypothesis that we put forth.

Joyce's implicit assumption is that the sole determinant of the impact of legalized abortion is simply whether abortion is legal. Since after 1973 abortion is legal everywhere, Joyce posits that the post-1973 birth cohorts should experience similar effects on crime (if any) between the early-legalizing states and other states. Our original

15. This specification mirrors the one used in Donohue and Levitt (2001), Table 7.

Table 3
The Impact of Exposure to Legalized Abortion on Crime, Limiting the Sample to Cohorts Born 1968–76, Excluding Early-Legalizing States

	Dependent variable							
Variable	ln (violent crime arrests)		ln (property crime arrests)		ln (homicide arrests)		ln (homicide offenses) using supplemental homicide report data	
Abortions per 1,000 live births (× 100)	−0.060	−0.061	−0.050	−0.044	−0.028	−0.035	−0.040	−0.047
	(0.016)	(0.015)	(0.019)	(0.018)	(0.023)	(0.029)	(0.017)	(0.019)
State-age interactions included as controls?	No	Yes	No	Yes	No	Yes	No	Yes
Number of observations	3,738	3,738	3,743	3,743	3,124	3,124	3,221	3,221
R^2	0.94	0.97	0.94	0.98	0.87	0.91	0.87	0.90

Notes: The dependent variable is identified at the top of each column. Each entry in the table reflects the estimated coefficient on the variable abortions per 1,000 live births (by mother's state of residence) from a separate regression. All regressions include cohort, state, age, and year fixed effects. In addition, the even-numbered columns also control for state-age interactions. Standard errors, corrected for first-order serial-correlation, are in parentheses. The number of observations varies across crime categories because of state-cohort-year observations in which crime data are missing or equal to zero. The cohorts included in the regression correspond to those in Joyce (2003) Table 5. The differences between this table and Joyce's Table 5 are 1) we use the abortion rate instead of a dummy for whether abortion is legal or not, 2) we include cohort effects to absorb nationwide changes in criminality across cohorts, 3) we use all of the years of available data for these cohorts (1983–98), and 4) we allow for state-age interactions in some specifications.

Table 4
Effective Abortion Rates for Early-Legalizing States versus the Rest of the United States Selected Years

Effective abortion rate	1982	1988	1994	1997
Early legalizers	0.0	64.0	238.6	327.0
Rest of U.S.	0.0	10.4	87.7	141.0
Difference between early legalizers and rest of U.S.	0.0	53.6	150.9	186.0

Notes: Early legalizing states are Alaska, California, Hawaii, New York, and Washington. These five states legalized abortion in late 1969 or 1970. In the remaining states, abortion became legal in 1973 after *Roe v. Wade*. The row labeled "Difference" is the difference in the effective abortion rate between early legalizers and the rest of the United States. The effective abortion rate is a proxy measure for the average exposure to abortion among potential criminals in a state and year (see Footnote 9 of the text for further discussion). Because the age distribution of criminals differs across crimes, the effective abortion rate also varies by crime. Values in the table are effective abortion rates for violent crime. The calculation of the effective abortion rate follows Equation 1 in Donohue and Levitt (2001) and is based on the observed age distribution of national arrests for violent crime in 1985. Abortion data are from the Alan Guttmacher Institute.

hypothesis, however, was based on a view that the mere act of abortion legalization is not sufficient to equalize the costs (financial, social, and psychological) of abortion across time and place. Rather, our model argues that abortion rates as a fraction of live births are a better proxy for the impact of legal abortion than is the dichotomous indicator of legal status. And, in fact, in our earlier paper, we demonstrate that the gap in abortion rates between the early legalizers and other states *actually grows over time,* even though abortion becomes legal everywhere in 1973. Table 4 shows the effective abortion rates for violent crime for the early legalizers and other states at selected points in time. In 1988, the early legalizers had an effective abortion rate that was 54 (per 1000 live births) higher than in other states, but that this difference *grew* to 151 in 1994 and then to 186 in 1997.

Thus, when Joyce finds in his Table 2 that the gap in crime continues to widen between early-legalizing and all other states after 1973, he presents evidence that exactly coincides with the prediction of our original hypothesis. We take this as compelling evidence in support of our theory.

IV. Conclusion

Joyce's article has suggested a number of interesting identification strategies for testing a causal relationship between legalized abortion and crime. In our reply, we demonstrate that the differences between our earlier findings and Joyce's results are *not* due to his identification strategies (except when he relies on national time-series variation), but rather, to his exclusive focus on the six-year period in which the crack epidemic was most virulent. We present evidence supporting the claim that crack hit the states that legalized abortion in 1970 harder than the rest

of the country. Because of the influence of crack on crime for a particular time window, it is important to look at the whole period for which data are available (as opposed to arbitrarily limiting the sample to the period 1985–90). When we do just that, using the same sources of variation as Joyce, legalized abortion is once again strongly associated with reductions in crime. Finally, we demonstrate that Joyce's claim that our results do not hold for the cohorts born immediately before and after *Roe* is the consequence of his identification strategy of using national time-series variation in a period of sharply rising youth crime rates coinciding with the crack epidemic. Looking within states over time, the negative relationship between abortion exposure and crime emerges even *more* strongly in the cohorts born immediately before and after *Roe* than it does in the sample as a whole. Thus, we find nothing in Joyce's paper that offers a serious challenge to the original hypothesis proposed in Donohue and Levitt (2001). Indeed, the results we present in this reply represent some of the strongest evidence to date in favor of the hypothesis that abortion legalization has dampened crime.

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