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The Impact of Legalized Abortion on Crime: Comment

by

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Abstract. This comment makes three observations about Donohue and Levitt's [2001] paper on abortion and crime. First, there is a coding mistake in the concluding regressions, which identify abortion's effect on crime by comparing the experiences of different age cohorts within the same state and year. Second, correcting this error and using a more appropriate per capita specification for the crime variable generates much weaker results. Third, earlier tests in the paper, which exploit cross-state rather than within-state variation, are not robust to allowing differential state trends based on statewide crime rates that pre-date the period when abortion could have had a causal effect on crime.

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1. Introduction

This comment revisits a seminal 2001 paper by Donohue and Levitt (henceforth DL) that linked the startling and unexpected decline in crime during the 1990s to the legalization of abortion some 20 years earlier. DL theorize that abortion reduces crime for two reasons. First, holding the number of pregnancies constant, a higher abortion rate today reduces the number of young people in the future. Because younger people commit more crimes than older people, this "cohort-size" effect should reduce crime if the share of young people in the population declines. Second, because a mother can abort a pregnancy more easily when abortion is legal, a child born after legalization is more likely to be wanted than a child born before legalization. If children who are wanted grow up to commit fewer crimes than unwanted children do, then abortion will bring about an additional, "selection" effect that further reduces crime.

The strongest evidence in favor of DL's hypothesis comes from comparing changes in crime rates across U.S. states. The prevalence of abortion differed markedly across states in the years following abortion's legalization. In the District of Columbia, New York, and California, more than one-third of pregnancies ended in abortion, on average, from 1970-1984. In North Dakota, Idaho, and Utah, however, abortion was used in less than 10 percent of pregnancies over the same period. In the 1990s, high-abortion states experienced bigger declines in crime than low-abortion states, suggesting that abortion reduces crime. Using regressions that are identified by cross-state comparisons of declines in crime, DL attribute about half of the 1990s crime decline to legalized abortion.

Yet statewide crime rates are influenced by other factors besides abortion. Crime in New York is determined by different factors than crime in Utah, so it should not be surprising that crime in the two states diverges over some period. The best way to isolate the true effect of abortion on crime is to use within-state rather than cross-state comparisons. This

is done by comparing cohorts of young people who live in the same state in the same year, but whose mothers had different probabilities of aborting an unwanted pregnancy. In other words, the best way to determine if abortion has a causal effect on crime is to compare two people who are in a similar environment today, but who had differing probabilities of being wanted at birth. The most compelling regressions in DL [2001] were the ones that concluded their paper, because these regressions were designed to implement exactly this type of within-state comparison, for cohorts defined on the state-year-age level. In this comment, we offer two reasons why these regressions were implemented incorrectly.

The first flaw in DL's concluding regressions is that they are missing a key set of regressors because of a computer coding error. The missing regressors would have absorbed variation in arrests on the state-year level, insuring that the abortion coefficient was identified using within-state comparisons only. Second, unlike the other tests in their paper, the concluding regressions do not model arrests in *per capita* terms. Instead, the dependent variable is the *total* number of arrests attributed to a particular cohort of young persons. Only by using per capita arrest data, however, can we test whether abortion has a selection effect on crime. In Section II of this comment, we run the concluding regressions on a per capita basis with the appropriate regressors, and we find that compelling evidence for a selection effect of abortion on crime vanishes. A reader may ask whether the concluding regressions at least show that abortion reduces crime by reducing the number of young people (the cohort-size channel, as opposed to the selection channel).[1] However, we argue below that the concluding regressions do not even provide this partial kind of evidence, owing to the way in which the abortion variable is defined.

At this point, the corrected concluding regressions appear to contradict the other tests in DL's paper, as the concluding regressions no longer suggest that abortion affects crime, while the other tests do. This brings us back to the reason that the concluding regressions are crucial for DL's argument. These regressions are the only formal tests in the paper that cannot be contaminated by time-varying state-level factors that affect both crime and abortion, such as changing aspects of a state's economic circumstances or social and cultural environment. However, it is reasonable to assume that state-specific factors jointly determine both abortion and crime. In Section III of this comment, we show that this is indeed the case. First, we show that state-level abortion and crime rates were strongly correlated before 1985, when it was impossible for abortion to have had a causal effect on

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[1]   Indeed, this was our first interpretation of these regressions, as discussed in Foote and Goetz [2005]. The same interpretation is lent to the total-arrests regressions by DL [2006]. But Joyce [2006, footnote 12], questions whether the total-arrests regressions are really estimating a cohort-size effect. His arguments were important in developing the line of reasoning we explore in the next section.

crime. We then show that accounting for this correlation has damaging consequences for the abortion coefficient in the cross-state regressions that DL use to quantify abortion's effect on crime. In fact, the abortion coefficients in these cross-state regressions are no longer significantly different from zero when a potential proxy for omitted state-year factors is added. Finally, Section IV concludes with a test that is robust to many of the econometric issues we discuss throughout this comment. This test also provides no evidence that abortion reduces crime.

2. Correcting DL's Concluding Regressions

In DL [2001], the concluding regressions presented in Table VII are defined on the state-year-age level:

$$ln(ARRESTS_{sta}) = \beta ABORT_{sb} + \gamma_{sa} + \lambda_{at} + \theta_{st} + \epsilon_{sta}, \tag{1}$$

where s, t, b, and a denote state, year, birth-year, and single year of age, respectively. The $ABORT$ variable is the ratio of abortions per 1,000 live births that is relevant for a given cohort of young people aged 15 through 24, as calculated by the Alan Guttmacher Institute (AGI).[2] The three sets of interactions prevent potentially confounding variation from contaminating the estimate of β, and thereby ensure the cleanest possible estimate of abortion's effect on crime. The state-age fixed effects (γ_{sa}) allow each state to have a different age profile for arrests. The age-year fixed effects (λ_{at}) control for nationwide fluctuations in criminal activity for persons of given ages. Finally, the crucial state-year fixed effects (θ_{st}) absorb all state-level variation in both the time-series and cross-sectional dimensions. As a result, including θ_{st} insures that β is identified solely by within-state comparisons of arrests by age group. That is, the effect of abortion is estimated by comparing the criminal propensities of two individuals living in the same state in the same year. These individuals differ only in age, and the regression controls for the usual effect of age on criminality. Therefore, these two individuals differ only in their risk of abortion before birth, and therefore in their risk of having been unwanted children.

DL's coding error was to omit the state-year interactions (θ_{st}) from these regressions. These regressors are especially important because earlier tests in the paper are identified solely by cross-state variation of changes in statewide crime rates. Omission of θ_{st} leaves

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[2] For example, because 15-year-olds in 1995 were generally conceived in 1979 (= 1995-15-1), the $ABORT$ variable for Massachusetts 15-year-olds in 1995 is abortions per 1,000 births in Massachusetts in 1979. In order to line up abortions with future births that are conceived at the same time, AGI measures the number of abortions in a calendar year, divided by the total number of births from July 1 of that year to June 30 of the following year.

the concluding regressions vulnerable to the same type of state-level omitted variables bias as the paper's earlier tests.

A second problem is the specification of the $ARRESTS$ variable. To test whether abortion has a selection effect, one needs to know whether a person exposed to a high abortion risk *in utero* is less likely to commit a crime. By "less likely," we mean a lower probability, but the only way to measure a probability is to divide the number of crimes by the number of people who could commit them. In other words, $ARRESTS$ must be in per capita terms. DL [2001], however, defines $ARRESTS$ as the total number of arrests for the cohort, "because of the absence of reliable measures of state population by single year of age" (p. 411). In fact, the Census Bureau constructs these population measures for each year beginning in 1980.

While no population estimates are perfect, we believe that estimating the arrests regressions in per capita form is vital, because it is the only way to test for the controversial selection effect of abortion on crime. In fact, it is hard to know what one is estimating when the $ARRESTS$ variable is not in per capita form. Recall that $ABORT_{sb}$ for a cohort that is a years old is simply the number of abortions over births in its birth year b. Ignoring the various fixed effects from Equation (1) and noting that $b = t - a$, we can write

$$ln(ARRESTS_{sta}) = \beta\left(\frac{Abortions_{s,t-a}}{Births_{s,t-a}}\right) + \epsilon_{sta} \, .$$

It is easy to see how β could be negative in this regression, even if abortion has neither a selection nor a cohort-size effect on crime. Yearly fluctuations in births are caused by many factors, with the perceived costs of abortion being only one example. Variation in the other factors determining births will generate movements in the abortion-births ratio that are negatively related to total arrests in a mechanical way. Specifically, abstracting from migration and deaths, an increase in births a years ago shows up as an increase in the number of people that are a years old this year. But more people in a cohort is likely to mean more arrests, simply because the cohort is larger. So an increase in births *reduces* the abortion-births ratio, while it *increases* the total number of arrests, via an increase in population. Therefore, arrests and the abortion-births ratio should be negatively related in a total-arrests regression, even if no selection or cohort-size effects exist. A true test of the cohort-size effect would regress the number of births on a discrete indicator of the availability of abortion, then trace out the implications of any decline in birth rates for the nation's per capita crime rate. It would not regress total arrest counts on abortions *divided by* births.

Results using original abortion data

Table I revisits the regressions from Table VII in DL [2001]. The first four columns use the same data DL used, over the same sample period (1985-1996). Panel A presents the results for property crime (the most common type of crime) and Panel B presents results for violent crime. Each of the regressions include state-age and age-year interactions. Before discussing our main results, we must say a word about the standard errors. We report two sets of standard errors, distinguished by how they are "clustered," or the extent to which individual residuals are assumed to be independent of one another. DL's original paper employs standard errors that are clustered by year-of-birth and state, because the same groups of people are observed at different ages in different years.[3] Since DL published their 2001 paper, however, applied econometricians have begun to worry more about residual-independence assumptions. In this case, as stressed by Joyce [2006], there may be a correlation between the error for, say, 17-year-olds in one year and other age groups (besides 18-year-olds) in the following year, even after entering all the fixed effects. The standard fix for this problem is to cluster the standard errors more widely (Bertrand, Duflo, and Mullainathan [2004]). In the second set of standard errors below, we cluster the standard errors by state.

Consider first the parameter estimates in Column 1, which mimics DL's original specification exactly. We are able to replicate their results for both the abortion coefficients (–.025 for property crime and –.028 for violent crime) and the original standard errors (0.003 and 0.004). The state-clustered standard errors in Column 1 are larger than the original ones, suggesting that this specification leaves a great deal of within-state correlation in the residuals. Column 2 corrects DL's computing error by adding the state-year fixed effects (θ_{st}). Both abortion coefficients drop by more than half. Column 3 adds the population data to the analysis by entering the log of the cohort size as a right-hand-side variable.[4] The log of population enters significantly in both regressions, but the estimated coefficient is less than one, which suggests that arrests and population do not vary proportionately. One possible explanation for this finding is that youths from large cohorts are generally better behaved than youths from small cohorts. A far more likely explanation is that (as DL pointed out) population is measured with error. If so, then well known

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[3] Ignoring migration and deaths, the persons making up the observation for Massachusetts 16-year-olds in 1990 also make up the observation for Massachusetts 17-year-olds in 1991. Residuals from these two observations will not be independent, because they will share any unobserved factors conducive to crime.

[4] We use modified population data constructed by the National Cancer Institute, which is available from 1969 to 2002. Using the unadjusted Census data gave essentially the same results. See SEER [2005] and Ingram et al. [2003] for a discussion of the population data we use.

econometric results predict that the population coefficient will be biased ("attenuated") towards zero.

Rather than omit the population data, a better choice is to eliminate the attenuation bias by moving the offending variable to the left-hand-side of the regression, transforming the dependent variable from total arrests into arrests per capita. Econometric theory suggests that classical measurement error does not cause bias if it appears on the left-hand-side of the regression. Of course, in our case, the move has the added benefit of permitting us to test if abortion has a selection effect on crime. Column 4 shows that the absolute values of the abortion coefficients fall to essentially zero when this is done.

Results using adjusted abortion data: DL [2006]

After the original version of this comment was released in 2005, DL responded that the corrected regressions do not argue strongly against an abortion-crime link [DL 2006]. Their main concern is measurement error in the abortion data, which arises from three sources. First, the original AGI data measured abortions by the place of occurrence, not the woman's state of residence. Second, because we do not know the due date of the fetus nor the day of the year on which the abortion occurred, we do not know the precise year in which an aborted fetus would have been born. Third, interstate migration means that the abortion exposure relevant for a young person may be a lagged abortion rate in some other state, where he was born. As with the population data, measurement error in the abortion data will bias the abortion coefficients toward zero. DL [2006] re-runs the concluding regressions with abortion data that has been adjusted to address these three issues. That paper's abstract states that "[w]hen one uses a more carefully constructed measure of abortion (e.g., one that takes into account cross-state mobility, or doing a better job of matching dates of birth to abortion exposure), ... the evidence in support of the abortion-crime hypothesis is as strong or stronger than suggested in our original work."

To evaluate this claim, Column 5 of Table I uses the adjusted abortion data from DL [2006]. The sample period is extended by two years, to 1998. As DL also found, the point estimate for abortion's effect on property crime becomes slightly *positive* (.001), though it remains insignificant. The coefficient from the violent-crime regressions moves to -.021, with its statistical significance dependent on the way in which the standard errors are calculated. DL [2006] calculates these errors in the same way as DL [2001], clustering by year-of-birth and state. Using what we would argue is a more appropriate method increases the standard error by about 75 percent, resulting in a t-statistic of only 1.5.[5]

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[5] DL [2006] also claims that evidence for a selection effect can be resurrected by using a separate measure

3. Reconciling Within-State and Cross-State Results

The results of Equation (1) do not provide evidence for a link between abortion and crime. However, as noted in the introduction, DL [2001] contains other tests besides the concluding regressions. We now illustrate why we should be skeptical of these other tests, which are based on cross-state comparisons, not within-state comparisons.

Consider the regression used in DL [2001] to quantify the effect of abortion on crime. That regression uses observations defined on the state-year level:

$$Crime_{st} = \delta EAR_{st} + \text{other variables} + \phi_s + \phi_t + \varepsilon_{st}. \tag{2}$$

Here, $Crime_{st}$ is the log per capita crime rate of state s in year t, where crime is defined by the number of crimes reported to police, not actual arrests. The ϕ_s and ϕ_t controls are state and year fixed effects, and EAR_{st} is the state's "effective abortion rate." This rate is constructed by weighting abortion rates $a - 1$ years ago by the fraction of crimes typically committed by persons of age a.[6] The "other variables" are time-varying, state-level factors such as incarceration rates, per-capita income levels, and gun laws. These controls are potentially important, because many factors affect state-level crime rates besides past abortions. Unlike the concluding regressions, which are designed to *eliminate* cross-state variation via the (inadvertently omitted) θ_{st} terms, the regression above is *identified* by cross-state comparisons of changes in crime rates. Of course, we are worried about state-year level factors that are omitted from the equation, not the state-year variables that are included. (The included variables turn out to have little effect on the abortion coefficient.)

In these regressions, the estimate of δ in Equation (2) will commingle both cohort-size effects and selection effects of abortion. In their regressions, DL [2001] estimated that

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of abortion, provided by the Centers for Disease Control (CDC). This measure of abortion is an occurrence-based indicator, but DL correct it for interstate migration using the same correction as that used for the residence-based AGI data. This transformed version of the CDC data is then used as an instrument for the AGI measure. Joyce [2006] provides a detailed argument for why this IV is not appropriate. In any case, it has a small effect on the estimates, moving the property-crime coefficient from 0.01 to -.013 and the violent-crime coefficient from -.021 to -.023. Neither IV estimate is statistically significant, no matter how the standard errors are clustered.

[6] Formally, the EAR is

$$\text{EAR}_{st} = \sum_a Abortion\ Ratio_{t-a-1}\left(\frac{Arrests_a}{Arrests_{total}}\right),$$

The abortion ratio is abortions over births, as in the state-year-age regressions. The extra "-1" in the subscript for the abortion ratio indicates that the relevant abortion rate of a child born in a given year is the previous year's abortion rate, because pregnancies last for most of one year.

increasing the abortion ratio by 100 abortions per thousand births reduces per capita crime in a state by about 10 percent. Based on these estimates, DL [2001] surmised that abortion's selection effect is large, calculating that "those on the margin for being aborted are roughly four times more criminal" than the average 18-24 year old (p. 405).

One potential reason why the cross-state regressions imply evidence for a selection effect — while the concluding within-state regressions do not — is that omitted variables bias remains a problem in the cross-state regressions, despite DL's attempts to control for it. A good way to determine whether omitted variables bias is possible is to look for a correlation between state-level abortion and crime rates before the mid-1980s, when the first cohorts affected by legalized abortion reached adolescence. DL [2001] recognize the potential for concern, stating: "There should be no effect of abortion on crime between 1973-1985. To the extent that high and low abortion states systematically differ in the earlier period, questions about the exogeneity of the abortion rate are raised" (p. 401).

We looked for a pre-1985 relationship between abortion and crime by calculating average abortion and crime rates for each state in the 1970-84 period, then regressing the crime averages on the abortion averages. To get a visual sense of these data, consider the top two panels of Figure 1. These two panels graph state-level, 1970-84 averages of per capita property crime (left panel) and violent crime (right panel) against average abortion ratios, with pre-legalization abortion ratios set to zero. These top panels indicate that states with high abortion ratios also had high crime rates during this early period. Coefficients from formal regressions of abortion averages on crime averages are positive and highly significant, with large amounts of the variation in crime "explained" by abortion. For example, a population-weighted regression of average property-crime rates on average abortion ratios gives an R^2 of .37 and a p-value for the abortion coefficient of 0.0012. The results for violent crime are even stronger: the R^2 from this regression is .62 and the abortion p-value is zero to four decimal points. When the data are unweighted, the R^2's for property crime and violent crime are .41 and .67, respectively.[7]

DL also examine the relationship between abortion and crime before 1985, but come to the opposite conclusion: "It is reassuring that the data reveal no clear differences in crime rates across states between 1973 and 1985 as a function of the abortion rate" (p. 401). Our interpretations differ because DL look for a uniform pattern in pre-1985 *changes* in crime rates as a function of the abortion rate, while we focus on the average *levels* of crime and abortion in the early period.[8]

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[7] Dropping DC from the unweighted regression generates R^2's of .36 and .42. The full set of regression statistics appears in Appendix Table I.

[8] DL find sharp differences between changes in crime across states with high and low abortion rates,

We believe that DL misread the data by focusing on changes rather than levels, because state-level factors that drive crime and abortion may not have constant effects over time. For example, there may be some reason that New York had both a higher crime rate and a higher abortion rate than did Utah before 1985. Perhaps New York's urban density, its wealth, its demographic structure or some other aspects of its culture offers New Yorkers more chances for interpersonal connections that lead to more crimes and to more unwanted pregnancies. Now consider what would happen if the influence that these factors had on crime were to diminish over time. States with high abortion rates in the past would see their crime rates fall the most, because their high abortion rates would proxy for those factors that are losing their significance in driving crime. If so, DL's regressions would detect the steeper decline in crime in high-abortion states and erroneously assign the credit for this drop to past abortions, when changes in other, unobserved factors are in fact responsible.

The problem becomes even more pernicious when we note that the standard fix for unobserved factors in state-level regressions can make the problem worse. In a crucial robustness check, DL enter interactions between the yearly dummies and dummies for the country's nine Census divisions. These division-year interactions will allow an unbiased estimate of the abortion coefficient as long as all the potentially confounding factors are determined on a geographic basis, operating *between* and not *within* Census divisions. But the confounding factors that jointly determine abortion and crime turn out to be even more important in driving crime when we compare states within the same Census division. We discovered this by first regressing the early state-level abortion and crime averages on a slate of Census division dummies, then regressing the resulting crime residuals on the resulting abortion residuals. These residuals are graphed in the bottom two panels of Figure I. In these panels, the positive relationship between early-period abortion and crime appears even stronger than in the unadjusted data in the top two panels. This impression is confirmed by population-weighted regressions that imply abortion "explains" 60 percent of the within-division variation in property crime and an astonishing 72 percent of within-division variation in violent crime in the early period.[9]

All in all, omitted variables bias is likely to be a serious problem in DL's cross-state regressions. The early correlations between abortion and crime suggest that these variables are driven by common factors, because abortion can have no causal effect on crime before

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but they dismiss their importance because these changes are not uniform across different types of crime.

[9] The R^2 from the within-division property- and violent-crime regressions without population weights are .59 and .78, respectively. Dropping DC from the unweighted regressions generates R^2's of .58 and .54. See Appendix Table I.

1985. Cross-state estimates of the effect of abortion on crime are invalid if the effect of these driving forces on crime changes over time. If we try to account for these complicating factors by using geographic interactions, we can turn a bad regression into a worse one, because the remaining within-division variation in abortion rates is even more tightly linked to remaining variation in past crime rates. Without some direct control for omitted factors that drive this correlation, entering division-year dummies in these regressions will exacerbate, not solve, the omitted variables problem.

Revisiting DL's cross-state regressions

To see if this criticism of DL's cross-state regressions is empirically relevant, Table II presents cross-state regressions with some new data and new specifications.[10] Column 1 uses DL's original specification and original abortion data.[11] Column 2 employs the same specification, but uses the new residence-based abortion data to construct the effective abortion rates. Column 3 updates the sample to end in 2003 rather than 1997. All of these regressions generate significantly negative abortion coefficients.

As an ostensible control for potentially omitted variables, we enter the division-year interactions in the regressions of Column 4. As DL found in their original paper, including these controls does not materially affect the abortion coefficients. Recall, however, that the effect of omitted variables may be worse when using within-division variation alone to identify abortion's effect. We cannot eliminate this bias without a model that identifies the omitted variables. Yet we can at least reduce the bias by entering an appropriate proxy. This proxy must be correlated with factors that caused crime in the past, but whose intensity changed after 1985.

Accordingly, in Column 5, we enter an interaction between the mean of the state's log per capita crime rate from 1970-84 and a linear trend. A negative coefficient on this variable indicates that states with relatively high crime rates from 1970 to 1984 experience relatively steeper crime declines after 1985. Two points of discussion about this variable

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[10] As in DL [2001], we use a Prais-Winsten method to account for first-order serial correlation in the residuals. Unlike DL's regressions, our regressions cluster the standard errors by state. We found that state-clustered standard errors were larger than simple White-type robust standard errors of DL [2001], probably because the AR(1) correction does not purge the regression of all serial correlation in the residuals (Bertrand, Duflo, and Mullainathan [2004]). See the appendix for more discussion of this point.

[11] The regressions in Column 1 include only the effective abortion rates and the state and year fixed effects, so they are comparable to the regressions in Columns 1, 3, and 5 of Table IV in DL [2001]. Our estimates are marginally different than those in DL [2001] for four reasons: We used a slightly different methodology for calculating the first-order autocorrelation parameter, we allowed the Prais-Winsten procedure to iterate on this parameter, we used constant population weights within each state, and we used updated estimates of crime and population. The corresponding point estimates in DL [2001, p. 404] are -.095 for property crime, -.137 for violent crime, and -.108 for murder.

are important. First, entering this crime-trend interaction requires an estimate of only one additional coefficient, so it is far more parsimonious than entering 51 unrestricted state–specific trends. (In a robustness check, DL show that there is not enough variation in the data to estimate separate state-specific trends.) Second, because this proxy is not perfect, it will not eliminate omitted variables bias completely. Nevertheless, the evidence provided by the cross-state regressions will be much less convincing if adding the proxy reduces the importance of the abortion coefficient.

Column 5 shows that the abortion coefficients weaken sharply when this proxy is added. The effective abortion coefficient drops by about 77 percent in absolute value in the property crime regression (from -.131 to -.030), by about 52 percent in the violent crime regression, and by about 42 percent in the murder regression. All of the abortion coefficients decline to statistical insignificance. By contrast, the coefficients on the new interaction terms are strongly significant for both property and violent crime. The coefficient for murder, while not significant, is about the same size as that for violent crime in general.[12] All told, our results suggest that the estimated abortion effect in cross-state regressions is sensitive to controls for omitted variables. It is therefore crucial to absorb potential omitted variables bias on the state-year level, using controls like the state-year interactions (θ_{st}) that can be included the concluding state-year-age regressions.[13]

4. Conclusion

DL [2001] suggests alternative ways of studying the abortion-crime relationship, but different methods give different answers. Their concluding state-year-age regressions, when run correctly, provide little evidence for a selection effect of abortion. Their cross-state regressions, by contrast, imply a large selection effect. Each method has it drawbacks: DL [2006] contend that measurement error plagues the concluding regressions, while we argue that the cross-state results are not robust to controls for omitted variables.

Fortunately, there is a way to test the abortion-crime hypothesis that simultaneously

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[12] Robustness checks for these regressions appear in the appendix.

[13] In addition to the cross-state regressions, DL [2001] includes two other cross-state tests. One test argues that five states that legalized abortion in 1970 saw crime decline sooner than the rest of the country, which legalized three years later. But these early-legalizers also tend to be high-crime states, so this test involves both the timing of crime declines and the amount by which crime fell in each state. As DL point out, the source of identification is not independent from the cross-state regressions. The other test uses arrests data to calculate a per capita arrest rate for those over 25 and under 25 in each state. Then the difference between the two rates is regressed on state-level EARs. This method would appear to be an improvement over the other cross-state regressions (and the implied effects of abortion are indeed smaller). Yet the data used are arrests, not crimes reported, so it is hard to see how this method is an improvement over linking arrests to population and abortion exposure by single year of age, as is done in the concluding regressions.

addresses DL's concerns about measurement error and our worries about omitted state-year factors. By collapsing the state-year-age data into nationwide age-year means, we can then regress each national cohort's per capita criminal propensity on the appropriate national abortion rate, along with age and year fixed effects. Using the same notation as before, the equation is:

$$ln(ARRESTS\ PER\ CAPITA_{ta}) = \beta ABORT_b + \phi_a + \phi_t + \epsilon_{ta}.$$

This age-year regression has a number of advantages. It gives a direct test of the controversial selection effect of abortion, because it is run with per capita data on well-defined age cohorts. The use of nationwide data also obviates DL's measurement-error concerns, which are caused by the difficulty of measuring arrests, abortion ratios, population, and migration on the state level. And because the identifying variation in this regression is national, it is not biased by the omission of any state-level factors.

As pointed out by numerous previous authors [Sailer 1999; Lott and Whitley, forthcoming; Joyce 2004, 2006], this method does not support the abortion-crime hypothesis. These authors often use graphs like Figures 2a and 2b, which show that the criminal activity of different age cohorts does not appear to decline when these cohorts begin to be affected by legalized abortion.[14] Table III formalizes this point with regressions of national age-specific arrest rates on a nationwide version of the abortion ratio. Column 1 shows that for both property and violent crime, abortion exposure appears to raise criminality, not lower it.

DL [2001] points out a drawback to this approach. Just as the cross-state regressions can be contaminated by omitted state-year effects, the regressions in Table III are susceptible to omitted age-year effects. If there is some shock that raises criminality for cohorts with relatively high abortion exposures, then the age-year tests will be misleading. Footnote 21 of DL [2001] points out that the crack wave of the late 1980s and early 1990s may have delivered these shocks.[15] In Column 2 of Table III, we exclude data from the zenith of the crack wave (1985-1992). Contrary to what DL's theory would imply, the coefficients from age-year regressions become slightly more positive. Placing these results alongside those

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[14] For example, Figure 2a shows that the property-crime rate of 21-24 year-olds begins to decline around 1989, while that of 15-17 year-olds keeps rising. But if abortion were truly affecting crime rates, we would expect the crime rate of the younger cohort to fall relative to that of the older cohort, because the younger cohort begins to be affected by legalized abortion at about this time.

[15] For the crack wave to contaminate the age-year tests, however, it is not enough for crack to raise criminality for all 15-24 year-olds in some years, because the year dummies account for shared influences of this type. Crack must raise criminality for various years and ages in ways that coincidentally line up with cohort-specific abortion exposure and mask the large selection effects of abortion from showing up in age-specific arrests.

from the corrected concluding regressions and our expanded cross-state analysis, we find no compelling evidence that abortion has a selection effect on crime.

5. Appendix

Appendix Table I presents statistics for regressions of 1970-84 state-level crime averages on state-level abortion averages. These regressions are discussed in Section II, and Figure I presents scatterplots of the data. In Columns 1 and 2, the regression is:

$$\overline{Crime_s^{1970-84}} = \beta \overline{Abort_s^{1970-84}} + \epsilon_s,$$

where the dependent variable is the average per capita crime rate for state s from 1970 to 1984 inclusive, and the regressor is the average abortions-per-births ratio over the same period. These two columns show the strong positive relationship between average abortion and crime levels in the 1970-84 period, when abortion could not have had a direct, causal effect on crime. In Columns 3 and 4, the regression is

$$\overline{\widetilde{Crime_s^{1970-84}}} = \beta \overline{\widetilde{Abort_s^{1970-84}}} + \varepsilon_s,$$

where the variables of interest have been pre-whitened by regressing them against a slate of Census division dummies. The larger R^2's in the Columns 3 and 4 show that the positive relationship between abortion and crime in the raw averages becomes even stronger when we focus the comparison on states within the same Census division. For example, the R^2 for the population-weighted property-crime regression using the raw averages is .37 (Column 1 of first panel), while that for the corresponding "within-division" regression is .60 (Column 3 of first panel).

    Appendix Table II illustrates the robustness of our Table II to the exclusion of DC. Columns 1 and 2 of this table replicate Columns 4 and 5 of Table II. These columns show that the abortion coefficient declines to insignificance when both division-year interactions and our crime-trend interactions are included in the regressions. Columns 3 and 4 repeat this exercise after omitting DC. For example, in Column 3, the abortion coefficients in the property-crime and violent-crime regressions are negative and strongly significant, as they are in Column 1. However, in Column 4, where the crime-trend interactions are also included, these abortion coefficients decline to insignificance, as they do in Column 2. Additionally, the crime-trend interactions in the top two panels of Column 4 are highly significant, as they are in Column 2. The fact that Columns 3 and 4 replicate the pattern of Columns 1 and 2 illustrates the robustness of Table II to the exclusion of DC. The last two columns of the table repeat the exercise after omitting DC, NY, and CA, with similar results.

Appendix Table III illustrates the consequences of calculating the standard errors differently than we did in our Table II. Additionally, the table shows that geographic interactions interactions are useful in controlling for some omitted variables, as long as the crime-trend interactions are also included.[16]

The top standard error for each coefficient estimate in this table uses the Huber-White "robust" method, which is also employed in the state-year regressions of DL [2001, Table IV]. The bottom standard errors are clustered by state, as they are in our Table II. As we note in Footnote 10, the state-clustered standard errors are larger than Huber-White ones, even though the Prais-Winsten method used in all regressions is designed to purge serial correlation from the data. The consequences of incomplete purging of serial correlation are discussed at length in Bertrand, Duflo, and Mullainathan [2004]. We follow the suggestion of that paper to use the more conservative, state-clustered errors here.[17]

The importance of the geographic interactions is seen by comparing the first two columns of the table with the last two columns. In Column 1, we enter the abortion variable without any crime-trend interactions or geographic controls. All of the abortion coefficients are statistically significant in this specification, as we noted in Table II. Column 2 adds the crime-trend interaction, but continues to omit any geographic interactions. Inclusion of the crime-trend interaction reduces the absolute value of the abortion coefficients. For example, the abortion coefficient in the property-crime regression falls from -.133 to -.084, while that in the violent-crime regression falls from -.165 to -.113. Yet even though the new abortion coefficients are all smaller than in Column 1, each of them remains statistically significant, regardless of how the standard errors are calculated. The implication is that entering our crime-trend interactions alone, without any geographic interactions, does not eliminate the statistical significance of the abortion coefficients.

The next two columns add the geographic interactions. Column 3 uses the (more parsimonious) region-year interactions. All abortion coefficients decline to statistical insignificance when the standard errors are clustered by state. Similar results obtain using the (more flexible) division-year interactions in Column 4, which replicates the last column of our main Table II. This exercise shows that, in contrast to the findings of DL [2001], entering geographic interactions has a large effect on the abortion coefficients. However, the crime-trend interactions must also be included for these effects to be evident.

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[16] There are four Census region of the country: Northeast, South, Midwest, and West. These four regions are mutually exclusive groupings of the nine Census divisions. DL [2001] uses the term "region" when referring to the nine Census divisions.

[17] As we note in the text, we also state-cluster the standard errors in the state-year-age regressions of Table I, whereas Donohue and Levitt cluster by year-of-birth × state.

Appendix Table IV replaces the region-year and division-year interactions with region-specific or division-specific trends. This is done for two reasons. First, interacting the region or division dummies with one (linear) or two (quadratic) trend terms is more parsimonious than interacting them with a full slate of yearly dummies. Second, we can construct F-tests for the exclusion of the trends even when the standard errors are clustered by state.[18] As in the previous appendix table, Column 1 enters the abortion variable by itself, while Column 2 enters both the abortion and crime-trend interactions. Columns 3 and 4 enters region-specific linear and quadratic trends, respectively. For both property crime and violent crime, the region-specific trends are highly significant and the abortion coefficients decline to statistical insignificance. In the murder regressions, the use of the trends reduces the absolute value of the abortion coefficient, but the inclusion of the regional trends is not supported by the significance tests. Columns 4 and 5 repeat this exercise using division-specific trends. The results for the property-crime and violent-crime regressions are similar to those in the previous two columns. In the murder regressions, the use of quadratic, division-specific trends (which resemble the division-year interactions most closely) is supported by the significance tests.

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[18] As is well known, the state-clustered covariance matrix is singular when state and year fixed effects are also included. Typically, this is not a concern, because the coefficients on the state and year fixed effects themselves are of little interest. However, since the regions and divisions are mutually exclusive groupings of states, it is impossible to perform F-tests on region-year or division-year interactions with a state-clustered covariance matrix.

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	(1)	(2)	(3)	(4)	(5)
Arrests as Per Capita?	No	No	No	Yes	Yes
State-Year Fixed Effects Included?	No	Yes	Yes	Yes	Yes
Abortion Data Used	Original	Original	Original	Original	Adjusted
Sample Period	85-96	85-96	85-96	85-96	85-98
Panel A: Log of Property Crime Arrests					
Abortion Ratio/100	-.025	-.010	-.004	-.001	.001
Std Err Clustered by:					
Birth Year × State	(.003)*	(.002)*	(.002)*	(.002)	(.005)
State	(.005)*	(.003)*	(.003)	(.004)	(.008)
Population Coefficient			.605		
Std Err Clustered by:					
Birth Year × State			(.062)*		
State			(.135)*		
N	5740	5740	5740	5740	6730
Panel B: Log of Violent Crime Arrests					
Abortion Ratio/100	-.028	-.013	-.007	-.004	-.021
Std Err Clustered by:					
Birth Year × State	(.004)*	(.004)*	(.003)*	(.004)	(.008)*
State	(.012)*	(.005)*	(.004)	(.005)	(.014)
Population Coefficient			.686		
Std Err Clustered by:					
Birth Year × State			(.086)*		
State			(.220)*		
N	5737	5737	5737	5737	6724

Notes: Each observation in the data set is a cohort of 15- to 24-year-olds defined by state, year and age (e.g., Massachusetts 17-year-olds in 1991). Results correspond to OLS regressions of the log of the cohort's arrests (or log per capita arrest rates in columns 4 and 5) on the cohort's *in utero* abortion exposure and various interactions. An asterisk denotes statistical significance at the 5 percent level. Age-year and state-age interactions are always included; state-year interactions are included in columns 2-5. In columns 1-4, abortions are measured by place of occurrence (as in DL [2001]), not by the state of residence of the mother. Column 5 uses the adjusted abortion data described in DL (2006), which is based on residence-based abortion data and further adjustments accounting for migration and statistical uncertainty about the timing of births and arrests within a calendar year. The sample period for columns 1-4 is 1985-1996 (as in DL [2001]), and the sample period for column 5 is 1985-1998. Not all states report arrest data for all years. The abortion ratio is divided by 100 in all regressions. State-level population weights are always used.
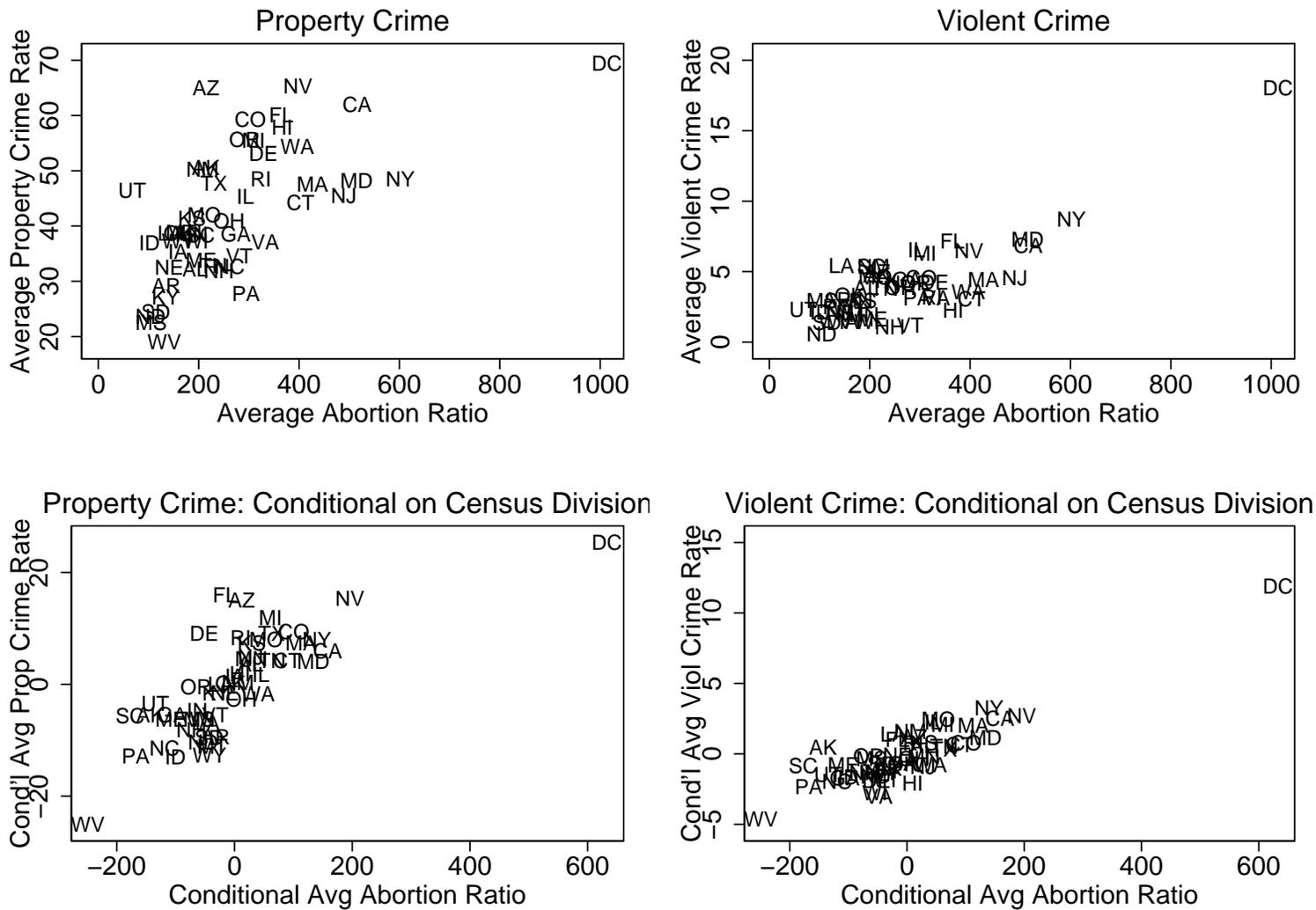
Figure 1: **Average Abortion Ratios and Per Capita Crime Rates, 1970-1984.** The abortion ratio is calculated by the Alan Guttmacher Institute as the number of abortions per 1,000 live births, and is based on the state of residence of the woman, not the state in which the abortion occurred. The crime rate is measured as incidents per 1,000 population and is not logged. The bottom row plots residuals from regressions of either abortion or crime averages on a slate of Census division dummies.

Table II: Per Capita Crime Regressions on the State-Year Level

	(1)	(2)	(3)	(4)	(5)
Sample Period	85-97	85-97	85-03	85-03	85-03
N	663	663	969	969	969
Abortion Data: Occurrence or Residence?	Occ	Res	Res	Res	Res
Geographic Controls	None	None	None	Division × Year	Division × Year
Panel A: Log Per Capita Property Crime Rate					
Effective Abortion Ratio/100	-.096	-.114	-.133	-.131	-.030
	(.022)*	(.026)*	(.025)*	(.043)*	(.036)
1970-84 Log Per Capita Property Crime Average × Trend					-.034
					(.008)*
Panel B: Log Per Capita Violent Crime Rate					
Effective Abortion Ratio/100	-.137	-.159	-.165	-.182	-.087
	(.032)*	(.044)*	(.035)*	(.068)*	(.073)
1970-84 Log Per Capita Violent Crime Average × Trend					-.014
					(.004)*
Panel C: Log of Per Capita Murder Rate					
Effective Abortion Rate/100	-.115	-.139	-.121	-.139	-.081
	(.045)*	(.067)*	(.053)*	(.097)	(.105)
1970-84 Log Per Capita Murder Average × Trend					-.013
					(.007)

Notes: Each observation in the data set corresponds to a group of persons defined by state and year (e.g., all Massachusetts residents in 1991). Results correspond to Prais-Winsten regressions of the natural log of the state's per capita crime rate on the corresponding effective abortion ratio and state and year fixed effects. Crime is defined by crimes reported to police, not actual arrests. An asterisk denotes statistical significance at the 5 percent level. Interactions between the year fixed effects and dummies for Census division are included in columns 4 and 5. Column 5 also adds a trend that varies by state, constructed by multiplying the state's mean annual log per capita crime rate from 1970-1984 with a linear time trend. State and year fixed effects are always included, and constant state population weights are always included. Standard errors are clustered by state, to account for the serial correlation of residuals within each state that remains after the Prais-Winsten quasi-differencing procedure.

Table III: Per Capita Arrests Regressions on the Year-Age Level

	(1)	(2)
Sample Period	1985-2003	1993-2003
N	190	110
Panel A: Log Per Capita Property Crime Rate		
Abortion Ratio/100	.026	.030
	(.015)	(.020)
Panel B: Log of Per Capita Violent Crime Rate		
Abortion Ratio/100	.057	.062
	(.014)*	(.017)*

Notes: Each observation in the data set corresponds to a cohort of 15- to 24-year-olds in one calendar year (e.g., U.S. 17-year-olds in 1991). Results correspond to Prais-Winsten regressions of the natural log of the cohort's per capita arrest rate on its average abortion exposure and year and age fixed effects. An asterisk denotes statistical significance at the 5 percent level. The per capita arrest rates are calculated by dividing national age-specific arrest totals from various issues of *Crime in the United States* by population for the age-year cell. The abortion ratio for each birth cohort is constructed by averaging the appropriate residence-based abortion ratio by state, then making a further adjustment for statistical uncertainty about the date of arrests and births within a calendar year. Standard errors are clustered by year of birth.

Appendix Table I: Regressions of 1970-84 State-Level Crime Averages on 1970-84 State-Level Abortion Averages

	Raw Averages		Averages Conditional on Census Division	
	(1)	(2)	(3)	(4)
	Property Crime	Violent Crime	Property Crime	Violent Crime
Weighted by Population (N=51)				
Abortion Coefficient	.045	.011	.067	.017
Standard Error	(.013)	(.001)	(.009)	(.001)
P-Value	[.0012]	[.0000]	[.0000]	[.0000]
R-Squared	.37	.62	.60	.72
Unweighted, excluding DC (N=50)				
Abortion Coefficient	.056	.010	.077	.014
Standard Error	(.011)	(.002)	(.011)	(.002)
P-Value	[.0000]	[.0000]	[.0000]	[.0000]
R-Squared	.36	.42	.58	.54
Unweighted, all states (N=51)				
Abortion Coefficient	.047	.014	.056	.017
Standard Error	(.008)	(.002)	(.010)	(.002)
P-Value	[.0000]	[.0000]	[.0000]	[.0000]
R-Squared	.41	.67	.59	.78

Notes: Results corresponds to univariate regressions of $\overline{Crime}_s^{1970-84}$ on $\overline{Abort}_s^{1970-84}$, where the dependent variable is the average per capita crime rate for state s from 1970 to 1984 inclusive, and the regressor is the average abortions-per-births ratio over the same period. Before averaging, neither the abortion nor crime averages is logged, but the abortion ratio is divided by 100. Abortions in pre-legalization years are set to zero. Huber-White robust covariance matrices are used to generate standard errors and P-values. For the conditional results in Columns 3 and 4, both the crime and abortion averages are pre-whitened by regressing them on a slate of Census division dummies before running the univariate regressions reported. By the properties of OLS, the abortion coefficients in these columns are numerically identical to those from one-step regressions of crime averages on abortion averages and the Census division dummies. In Columns 3 and 4, standard errors and P-values are generated by the one-step regressions, so that the implied degrees of freedom for the model will be correct. R^2's from Columns 3 and 4 are those from the univariate regressions in the two-step procedure. See Figure I for scatterplots of the crime and abortion averages used in these regressions.

Appendix Table II: Checking Robustness of State-Year Crime Regressions for Omission of Particular States

Omitted States	None (Table II Baseline)		DC		DC, NY & CA	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log Per Capita Property Crime Rate						
Effective Abortion Ratio/100	-.131	-.030	-.165	-.063	-.142	-.030
	(.043)*	(.036)	(.036)*	(.036)	(.041)*	(.041)
1970-84 Log Per Capita Property Crime Average × Trend		-.034 (.008)*		-.030 (.008)*		-.030 (.008)*
Panel B: Log Per Capita Violent Crime Rate						
Effective Abortion Ratio/100	-.182	-.087	-.221	-.130	-.187	-.110
	(.068)*	(.073)	(.065)*	(.079)	(.056)*	(.069)
1970-84 Log Per Capita Violent Crime Average × Trend		-.014 (.004)*		-.012 (.004)*		-.011 (.004)*
Panel C: Log of Per Capita Murder Rate						
Effective Abortion Ratio/100	-.139	-.081	-.181	-.123	-.099	-.057
	(.097)	(.105)	(.103)	(.119)	(.095)	(.098)
1970-84 Log Per Capita Murder Average × Trend		-.013 (.007)		-.012 (.008)		-.012 (.006)

Notes: As in Table II, each observation in the data corresponds to a group of persons defined by state and year. Results correspond to Prais-Winsten regressions of the natural log of the state's per capita crime rate on the corresponding effective abortion ratio (divided by 100), state fixed effects, and interactions between year fixed effects and Census division dummies. The sample period is 1985-2003, and all regressions have 969 observations (19 years × 51 states). Crime is defined by crimes reported to police, not actual arrests. An asterisk denotes statistical significance at the 5 percent level. Columns 2, 4, and 6 add a trend that varies by state, constructed by multiplying the state's mean annual log per capita crime rate from 1970-1984 with a linear time trend. Constant state population weights are used for all regressions. Standard errors are clustered by state, to account for the serial correlation of residuals within each state that remains after the Prais-Winsten quasi-differencing procedure.

Appendix Table III: Robustness Checks for State-Year Crime Regressions Using Region-Year and Division-Year Interactions and Alternative Standard Errors

	(1)	(2)	(3)	(4)
Geographic			Region	Division
Controls	None	None	× Year	× Year
Panel A: Log Per Capita Property Crime Rate				
Effective Abortion Ratio/100	-.133	-.084	-.008	-.030
	(.017)*	(.020)*	(.020)	(.024)
	(.025)*	(.032)*	(.030)	(.036)
1970-84 Log Per Capita Property		-.023	-.039	-.034
Crime Average × Trend		(.005)*	(.005)*	(.005)*
		(.005)*	(.007)*	(.008)*
Panel B: Log of Per Capita Violent Crime Rate				
Effective Abortion Ratio/100	-.165	-.113	-.064	-.087
	(.020)*	(.024)*	(.029)*	(.041)*
	(.035)*	(.035)*	(.044)	(.073)
1970-84 Log Per Capita Violent		-.011	-.015	-.014
Crime Average × Trend		(.003)*	(.003)*	(.003)*
		(.003)*	(.003)*	(.004)*
Panel C: Log of Per Capita Murder Rate				
Effective Abortion Ratio/100	-.121	-.102	-.082	-.081
	(.024)*	(.025)*	(.037)*	(.044)
	(.053)*	(.054)	(.080)	(.105)
1970-84 Log Per Capita Murder		-.011	-.010	-.013
Average × Trend		(.003)*	(.004)*	(.005)*
		(.006)	(.007)	(.007)

Notes: As in Table II, each observation in the data corresponds to a group of persons defined by state and year. The sample period is 1985-2003, and all regressions have 969 observations. Crime is defined by crimes reported to police, not actual arrests. Estimates are generated from Prais-Winsten regressions of the natural log of a state's per capita crime rate on the corresponding effective abortion ratio (divided by 100) and state and year fixed effects. The first standard error for each estimate is generated using the Huber-White robust method. The second standard errors are clustered by state, to account for serial correlation that remains after the Prais-Winsten quasi-differencing procedure. Columns 1 and 2 employ no additional geographic controls. Column 3 uses interactions between the year fixed effects and dummies for the four Census regions of the country, while Column 4 uses interactions between the year dummies and the nine Census divisions. Columns 2-4 also enter a crime-trend variable, constructed by multiplying the state's average annual log per capita crime rate from 1970-1984 with a linear time trend. Constant state population weights are used in all regressions. Asterisks denote statistical significance at the 5 percent level.

Appendix Table IV: Robustness Checks for State-Year Regressions using Region-Specific and Division-Specific Linear and Quadratic Trends

	(1)	(2)	(3) Linear Region	(4) Quadratic Region	(5) Linear Division	(6) Quadratic Division
Trends Included	None	None	Region	Region	Division	Division
Panel A: Log Per Capita Property Crime Rate						
Effective Abortion Ratio/100	-.133	-.084	-.013	-.011	-.010	-.022
	(.025)*	(.032)*	(.030)	(.028)	(.033)	(.030)
1970-84 Log Per Capita Property Crime Average × Trend		-.023	-.039	-.039	-.038	-.036
		(.005)*	(.007)*	(.007)*	(.008)*	(.008)*
P-values for Exclusion of All Trend Terms			[.0017]	[.0001]	[.0000]	[.0000]
Panel B: Log of Per Capita Violent Crime Rate						
Effective Abortion Ratio/100	-.165	-.113	-.070	-.070	-.088	-.097
	(.035)*	(.035)*	(.041)	(.041)	(.063)	(.065)
1970-84 Log Per Capita Violent Crime Average × Trend		-.011	-.015	-.015	-.014	-.013
		(.003)*	(.003)*	(.003)*	(.003)*	(.003)*
P-values for Exclusion of All Trend Terms			[.0219]	[.0157]	[.0235]	[.0000]
Panel C: Log of Per Capita Murder Rate						
Effective Abortion Ratio/100	-.121	-.102	-.088	-.088	-.097	-.100
	(.053)*	(.054)	(.079)	(.079)	(.097)	(.098)
1970-84 Log Per Capita Murder Average × Trend		-.011	-.009	-.009	-.011	-.011
		(.006)	(.007)	(.007)	(.006)	(.007)
P-values for Exclusion of All Trend Terms			[.5804]	[.8836]	[.3466]	[.0005]

Notes: As in Table II, each observation in the data set corresponds to a group of persons defined by state and year. The sample period is 1985-2003, and all regressions have 969 observations. Crime is defined by crimes reported to police, not actual arrests. Estimates are generated from Prais-Winsten regressions of the natural log of a state's per capita crime rate on the corresponding effective abortion ratio (divided by 100) and state and year fixed effects. Standard errors are clustered by state, to account for serial correlation that remains after the Prais-Winsten quasi-differencing procedure. Columns 1 and 2 employ no additional geographic controls. Columns 3 and 4 use interactions between the linear (Column 3) and quadratic (Column 4) trends that are specific to the four Census regions of the country. Columns 5 and 6 replicate the previous two columns using division-specific linear and quadratic trends. Columns 2-6 also enter a crime-trend variable, constructed by multiplying the state's average annual log per capita crime rate from 1970-1984 with a linear time trend. Constant state population weights are used in all regressions. Asterisks denote statistical significance at the 5 percent level.