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Measurement Error, Legalized Abortion, and the Decline in Crime A Response to Foote and Goetz

John J. Donohue

MEASUREMENT ERROR, LEGALIZED ABORTION, AND THE DECLINE IN CRIME: A RESPONSE TO FOOTE AND GOETZ*

JOHN J. DONOHUE III AND STEVEN D. LEVITT

We are grateful to Foote and Goetz for noting that the final table of Donohue and Levitt (*Quarterly Journal of Economics*, 116 (2001), 379–420) inadvertently omitted state-year interactions. Correcting our mistake does not alter the sign or statistical significance of our estimates, although it does reduce their magnitude. Using a more carefully constructed measure of abortion that better links birth cohorts to abortion exposure (by using abortion data by state of residence rather than of occurrence, by adjusting for cross-state mobility, and by more precisely estimating birth years from age of arrest data), we present new evidence that abortion legalization reduces crime through both a cohort-size and a selection effect.

I. INTRODUCTION

Donohue and Levitt (2001) put forth the hypothesis that the legalization of abortion in the United States in the 1970s played an important role in explaining the observed decline in crime approximately two decades later. In that paper, we presented six different types of evidence in support of the argument that legalized abortion reduced crime through a combination of smaller cohort sizes and lower criminal propensities among those born after legalization.

Foote and Goetz (2008) offer three challenges to our earlier results. First, they correctly note, and we regretfully acknowledge, that the text of Donohue and Levitt (2001) reports that state-year interactions are included in some of the columns in Table VII of the paper, but that the published version of the paper inadvertently omitted these interactions.[1] Second, they argue that it would be more appropriate to run the specifications in our Table VII in per

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1. Although any mistake is embarrassing, we are at least glad to have facilitated the catching of this error by posting our data and do files on the Web at http://islandia.law.yale.edu/donohue/pubsdata.htm. Elizabeth Oltmans Ananat, Jonathan Gruber, Phillip Levine, and Douglas Staiger were actually the first to discover this programming error in our code as part of their work on a larger analysis of the impact of abortion. See Ananat et al. (2006).

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capita terms. Third, they claim that the observed link between abortion and crime is spuriously driven by omitted variable bias.

In this paper, we analyze these three claims in turn. Section II illustrates that although state-year interactions were omitted from four of the eight regressions in the published version of Table VII of our original paper, this mistake has a relatively minor impact on the results.[2] The parameter estimates shrink, but remain economically large and statistically significant.

Section III demonstrates that the results presented in our original Table VII are likely to have *understated* the true magnitude of the impact of abortion. This is because of substantial measurement error in the crude abortion proxy used in our original paper. In this paper, we deal with measurement error in two ways. First, we construct an abortion measure that better corresponds to the actual month and year of birth of the individual, incorporates cross-state mobility between birth and adolescence, and reflects the state of residence of those having abortions (as opposed to the state in which the abortion is performed). Second, we instrument for our abortion proxy (based on Alan Guttmacher Institute data) using another independently generated estimate of the abortion rate (from the Centers for Disease Control).

Section IV shows that once we correct for measurement error in these ways, the point estimates we obtain even in specifications that capture only the selection effect channel are larger than the original results we reported.

In Section V, we address Foote and Goetz's claim that our analysis of crime rates by state and year, which shows a link between abortion and crime, is spuriously driven by omitted variable bias. As evidence for this claim, they run selected regressions that include interactions between lagged crime rates and a trend variable, finding that the abortion coefficients shrink. A more careful analysis demonstrates that their findings are quite sensitive to their particular (and restrictive) choice of functional form, including division-year interactions, and the presence of Washington, DC (which is an extreme outlier with respect to abortion rates and whose abortion data show a sharp, unexplained jump suggesting data problems). Indeed, when one runs specifications allowing more general functional forms (i.e., controlling for state-specific trends) that nest the one used by Foote and

2. The same omission of the state-year interactions is also repeated in the bottom two rows of Table 1 of Donohue and Levitt (2004), which used the same code as the original paper.

Goetz, omits division-year interactions, or excludes Washington, DC, estimates similar to our original ones are obtained.[3]

II. The Mistake in the Original Table VII of Donohue and Levitt (2001)

Table VII of Donohue and Levitt (2001) analyzes arrest rates by state of residence, year, and single year of age and estimates the following model:

$$(1) \qquad \ln(\text{ARRESTS}_{stb}) = \beta_1 \text{ABORT}_{sb} + \gamma_{sa} + \lambda_{ta} + \theta_{st} + \varepsilon_{stb}$$

where s, t, a, and b correspond to state of residence, year, age, and birth cohort, respectively. The variable ARRESTS is the raw number of arrests for a given crime category. The abortion proxy used is the abortion rate in the current state of residence in the calendar year most likely to have preceded an arrestee's birth given the year in which he or she was arrested and age. Included in the specification are state-age interactions, year-age interactions, and state-year interactions.

Due to a mistake on our part, the version of Table VII that was published did not include state-year interactions, despite what was written in the text of the article. Donohue and Levitt (2006), the working paper version of this reply, contains a Table I that reproduces the incorrect version originally published as Table VII of Donohue and Levitt (2001), along with the corrected columns as they should have appeared. As Foote and Goetz also confirm in their own Table I, Panel B, columns (1) and (2), adding the state-year interactions reduces the abortion coefficient for violent crime from –.028 to –.013, but the estimate remains statistically significant at the .01 level, even when clustering by state, which may exaggerate the standard errors (Hansen forthcoming). The abortion coefficient with state-age and state-year interactions is about the same in magnitude as when only state-fixed effects are included (–.015 v. –.013). When arrest rates are broken down by single year of age, all of the estimates remain negative, although almost always smaller. Again, as Foote and Goetz show in their Table I, Panel A, columns (1) and (2), adding

3. Foote and Goetz also add a brief national time series analysis in the conclusion of their paper. In light of the strong evidence for the presence of shocks such as crack cocaine (Fryer et al. 2005) that vary both with age and over time, the time series approach is unlikely to yield meaningful insights. See Donohue and Levitt (2004) for a further discussion of these issues.

state-year interactions to the property crime regressions induces a similar pattern of coefficient changes. In other words, the inadvertent error changes neither the sign nor significance of our results.

III. Measurement Error in the Abortion Proxy Explains the Smaller Estimates Obtained When State-Year Interactions Are Included

There are two possible explanations for why the coefficient on the abortion variable shrinks when state-year interactions are included. The first explanation is the presence of omitted factors that vary by state and year, which are positively correlated with current crime rates and negatively correlated with the abortion rate two decades earlier. The second possibility is that there is substantial measurement error in the abortion proxy. As more controls are included in the regression, the remaining variation in the abortion measure may become dominated by noise. The shrinkage of the abortion coefficient, in this scenario, is due to attenuation bias.

An analysis of the correlation between the two independently collected abortion proxies (one collected by the Alan Guttmacher Institute [AGI] and the other by the Centers for Disease Control [CDC]) highlights the problem of measurement error and how it is exacerbated by including state-year interactions.[4] In the raw data, these two proxies do indeed track each other closely with a raw correlation of .849. In the regression analysis, however, it is not the measurement error in the raw data that matters, but rather the measurement error after partialing out the set of included controls. As Table II of Donohue and Levitt (2005) demonstrates, the more controls are included, the lower the correlation between the two abortion measures becomes, suggesting that the signal-to-noise ratio is substantially reduced in the portion of the abortion measure that is actually being used to identify the parameter estimate. In the most heavily saturated models that include state-year interactions, the correlation is only .396. What remains of the variation in these two proxies is only relatively weakly correlated,

4. Although the CDC currently collects data on abortion rates by state of residence and by place of occurrence, as far as we can determine, only the data by place of occurrence are available over the entire period of interest. Therefore, when we use the CDC abortion measure as an instrument below, we use this occurrence variable.

implying that there is substantial measurement error in at least one, and likely both of the indicators.

But even this weak correlation is likely to dramatically overstate the amount of signal in the abortion proxy used by Donohue and Levitt (2001) for two reasons. First, given that both AGI and CDC collect their data through similar survey methods, it seems quite likely that the measurement errors in the two data series will actually be positively correlated, implying that some of the observed correlation is not due to true signal, but instead to correlated errors in the two proxies. Second, and probably far more important, the crude measure of abortion that was used has other obvious weaknesses.

In our original paper, the proxy for abortion exposure for individuals of age a in state s in year t is the AGI estimate of abortions performed in state s per live birth in state s in year t-a-1. This particular choice of proxy suffers from three important sources of measurement error, for which we now correct:[5]

1. *A nontrivial fraction of abortions performed in the United States, especially in the time around when legalization occurred, involved women crossing state lines to get abortions.* As a consequence, measuring abortions in terms of the state in which the abortion is performed, rather than the *state of residence* of the woman getting the abortion, induces further measurement error into the abortion proxy. We now have obtained and use AGI abortion data by state of residence of the pregnant woman (as opposed to the state where the abortion was performed).
2. *Cross-state mobility.* Based on census data, more than one-third of Americans aged 15–24 currently reside in a state other than the one in which they were born. For these individuals, one should not expect lagged abortion rates in their current states of residence to matter, but rather, the lagged abortion rates in the states in which they themselves were born. Our mobility correction uses a weighted average of the lagged abortion rates in the states of birth of those currently living in the state, with the weights determined by the percentage of a state's current residents born in each state.[6]

5. We provide the full details of these corrections in Donohue and Levitt (2006).

6. In Table 5 of Donohue and Levitt (2001), we carry out precisely this exercise in the sensitivity analysis of our primary identification strategy using *state-year*

3. *Using the year t-a-1 as the relevant timing of abortion exposure leads to the wrong year of birth for a nontrivial fraction of individuals.* The arrest data report how many 19-year-olds are arrested in a state and year for a particular crime. A 19-year-old arrested in 1993 might have been born as early as Jan. 2, 1973 (making him 19 if arrested on Jan. 1, 1993) or as late as Dec. 31, 1974 (if arrested on Dec. 31, 1993). Because of the way the arrest data are collected, there is a two-year window of birthdays that are relevant and not adequately captured by the simple abortion proxy.[7] To correct for this, our year of birth adjustment uses a weighted average of the abortion rates in years *t-a-2, t-a-1*, and *t-a*.

We also correct for imperfections in the way that abortion data are gathered by using instrumental variables estimation. If one has two noisy abortion measures but the measurement error in the two proxies is uncorrelated, instrumenting for one abortion proxy using the other will eliminate attenuation bias. In this setting, the CDC's independently generated measure of legalized abortions is likely to be an excellent instrument.[8]

Table I presents a comparison of the results before and after the corrections described above are made to deal with measurement error, now using data from 1985–1998 instead of our original sample of 1985–1996. The top row of the table reports the estimates using the uncorrected measure for violent crime arrests; the second row shows results when the three corrections noted above are made to the abortion proxy. The third row uses the corrected abortion measure, and in addition instruments for the AGI measure using the CDC measure. In all cases, the abortion coefficient jumps sharply when the original

crime data. As would be expected, the abortion coefficient became larger when this correction was made. The measurement error problem in that setting is likely to be much less severe than in the analysis of *arrests by single year of age*.

7. For example, roughly half of those individuals arrested in January 1993 at age nineteen were born in 1973 and the rest were born in 1972. Overall, we compute that 25% of nineteen-year-olds arrested in 1993 were born in a year other than 1973.

8. Under a standard set of assumptions, the estimates obtained will be purged of the attenuation bias that will be present due to measurement error. To the extent that there is a positive correlation in the measurement error in the two abortion proxies, the instrumental variables estimates will tend to understate the impact of abortion exposure on crime. Correlated measurement error will lead to an exaggeratedly large first-stage relationship between the two abortion proxies, which will result in the second-stage estimates being too small in absolute value.

TABLE I

ESTIMATED EFFECTS OF ABORTION ON CRIME WITH AND WITHOUT MEASUREMENT ERROR CORRECTION

	ln (Violent arrests)		
Abortion measures:			
Original	−0.018	−0.027	−0.009
	[0.003]**	[0.004]**	[0.003]**
	{0.008}*	{0.012}*	{0.004}*
With corrections	−0.045	−0.083	−0.046
	[0.007]**	[0.008]**	[0.008]**
	{0.020}*	{0.022}**	{0.014}**
IV using CDC	−0.045	−0.078	−0.055
	[0.007]**	[0.010]**	[0.013]**
	{0.016}**	{0.018}**	{0.017}**
Controls include:			
Fixed effects for state and age*year interactions	yes	yes	yes
State*age interactions	no	yes	yes
State*year interactions	no	no	yes
	ln (Property arrests)		
Abortion measures:			
Original	−0.040	−0.028	−0.010
	[0.004]**	[0.003]**	[0.002]**
	{0.007}**	{0.006}**	{0.003}**
With corrections	−0.084	−0.056	−0.024
	[0.008]**	[0.006]**	{0.005]**
	{0.023}**	{0.018}**	{0.007}**
IV using CDC	−0.085	−0.053	−0.044
	[0.010]**	[0.008]**	[0.010]**
	{0.019}**	{0.017}**	{0.014}**
Controls include:			
Fixed effects for state and age*year interactions	yes	yes	yes
State*age interactions	no	yes	yes
State*year interactions	no	no	yes

Notes. The unit of observation is a state by year by single year of age. The sample covers the period 1985–1998 for ages 15–24. If data were available for all states, years, and ages, the total number of observations would be 7,140. Due to missing arrest data and occasional zero values for arrests, the actual number of observations is somewhat smaller. The dependent variable for the top panel is ln(violent arrests) while the dependent variable for the bottom panel is ln(property arrests). The number of observations for the violent arrests regressions is 6,724 and for the property arrests regressions is 6,730. Estimation is weighted least squares. The rows labeled "original" use the abortion measure that was used in Donohue and Levitt (2001), but for the extended sample from 1985–1998. The rows labeled "with corrections" incorporates the changes described in the text (correcting for cross-state mobility, the appropriate year for the abortion rate, and using place of residence instead of place of occurrence of abortions). The rows labeled "IV using CDC" run weighted instrumental variables regressions where the Alan Guttmacher Institute measure of abortion is instrumented for the Center for Disease Control (CDC) measure of abortion. Two sets of standard errors are reported: square brackets indicate standard errors that account for clustering by year of birth×state, and curly brackets indicate standard errors that account for clustering by state. All reported coefficients are multiplied by 100. * implies statistical significance at the .05 level, and ** implies statistical significance at the .01 level.

measure is replaced with the more carefully constructed measure.[9] Even the estimates from the most saturated version of the model, which includes age-year, state-age, and state-year interactions, yields a coefficient of –.046, which is highly statistically significant. This estimate is larger than the coefficients reported in the original Donohue and Levitt (2001) paper, which *did not include state-year interactions*. Instrumenting with the CDC measure of abortion further increases the magnitude of the coefficient.[10]

The bottom panel of Table I is identical in structure to the top panel, except that the dependent variable in the regression is the natural log of the number of property crime arrests rather than violent crime arrests. Using the better proxy once again leads the estimated impact of the abortion rate to more than double in all three specifications. Instrumenting with the CDC abortion measure has a substantial impact in column (3), nearly doubling the estimate to –.044, which is larger than any of the estimates for property crime originally reported in the flawed Table VII of our 2001 paper.[11]

IV. Distinguishing between the Alternative Channels through Which Abortion Operates

The results in Table I suggest that, properly measured, a higher abortion rate when a cohort is *in utero* is associated with a statistically significant and substantively large reduction in later crime by that cohort. Abortion exposure can reduce aggregate

9. All three of the corrections we make to the abortion proxy contribute to the increased coefficient. Adjusting for state of residence as opposed to state of occurrence in the model with state-year interactions raises the coefficient from –.009 to –.021. Correcting for cross-state mobility in addition to the first fix raises the coefficient from –.021 to –.039. Getting the birth years right moves the coefficient from –.039 to –.046.

10. Instrumenting with the CDC abortion measure has a relatively small impact on the coefficients once the better abortion proxy is utilized, although the coefficient does rise by 20% in the regression with state-year interactions, which is likely to have the most measurement error. In some cases, the clustered standard errors reported by STATA are smaller in the IV specifications than in OLS. This result is a mathematical impossibility for nonclustered standard errors, which raises conceptual concerns about clustering, at least as implemented in STATA. In spite of these important concerns, we follow Foote and Goetz in reporting these clustered standard errors.

11. The evidence that when a more precise abortion measure is employed, the abortion-crime link becomes stronger casts doubt on Foote and Goetz's contention that the abortion rate is somehow proxying for an unknown and unspecified factor that happened to reduce crime. It would be a strange coincidence if employing more precise data uniformly strengthened a spurious result.

crime in at least two ways: (1) by shrinking the size of the cohort and (2) by lowering the average crime propensity of those who are born through positive selection.

Neither Table I in this paper nor Table VII of Donohue and Levitt (2001) distinguishes (or claims to distinguish) between those two competing hypotheses. Adding controls for cohort size to the regressions helps to discriminate between the two avenues for abortion reducing crime.

Foote and Goetz's Table I reports the results from two specifications, one in which the natural log of the state population by single year of age is included as a control in the regression, and another in which the dependent variable is the natural log of arrests per capita, rather than simply the natural log of arrests. The latter specification is equivalent to imposing a coefficient of one on the population variable in the first specification.[12] Once controls for population are included, any remaining impact of abortion is likely to be attributable to positive selection.[13]

Table II reports results from both of these specifications, along with regressions without controls for cohort size for purposes of comparison. In contrast to the null results obtained by Foote and Goetz for violent crime using the crude abortion proxy (the top row), with the better measure the abortion rate remains negative (and although imprecisely estimated, in many of the specifications statistically significant), even with the inclusion of population controls or measuring arrests per capita. It is not a surprise that controlling for population reduces the magnitude of the estimates, because a smaller cohort size is one channel through which legalized abortion plausibly reduces crime. These estimates suggest that at least 40% of the measured impact of abortion on arrests is operating through the selection channel.

12. Foote and Goetz provide an incomplete discussion of the risks and benefits of these two specifications. If there are no social interactions associated with cohort size, and any measurement error in the population variable is uncorrelated with the other right-hand-side variables, then theory suggests that the arrests per capita specification is preferable. In the presence of social interactions or population measurement errors that are correlated with the right-hand-side variables, the specification using the population control is probably more appropriate. See Jacobson (2004) for evidence of social interaction effects on youth crime rates associated with variation in the size of youth cohorts.

13. This discussion ignores any dynamic effects of abortion access on later cohorts. A woman who seeks an abortion today is likely to alter her future fertility decisions. See Ananat, Gruber, and Levine (2007) and Donohue, Grogger, and Levitt (2007) for an exploration of these issues.

The bottom panel of Table II mirrors the top panel, except that it corresponds to property crime arrests. The results for property crime are weaker. The better abortion proxy by itself does not yield statistically significant estimates once population controls are included. The instrumental variables regressions generate bigger parameter estimates but are only marginally statistically significant because of the large standard errors.

TABLE II
DISTINGUISHING BETWEEN THE CHANNELS THROUGH WHICH ABORTION AFFECTS CRIME

	ln (Violent arrests)	ln (Violent arrests)	ln (Violent arrests per capita)
Abortion measures:			
Original	−0.009	−0.003	0.000
	[0.003]**	[0.003]	[0.003]
	{0.004}*	{0.004}	{0.004}
With corrections	−0.046	−0.031	−0.021
	[0.008]**	[0.008]**	[0.008]**
	{0.014}**	{0.012}*	{0.013}
IV using CDC	−0.055	−0.037	−0.023
	[0.013]**	[0.014]**	[0.013]
	{0.017}**	{0.015}*	{0.018}
Controls include:			
Fixed effects for state and age*year interactions	yes	yes	yes
State*age interactions	yes	yes	yes
State*year interactions	yes	yes	yes
ln(Population)	no	yes	no
	ln (Property arrests)	**ln (Property arrests)**	**ln (Property arrests per capita)**
Abortion measures:			
Original	−0.010	−0.004	0.000
	[0.002]**	[0.002]*	[0.002]
	{0.003}**	{0.003}	{0.003}
With corrections	−0.024	−0.009	0.001
	[0.005]**	[0.005]	[0.005]
	{0.007}**	{0.007}	{0.008}
IV using CDC	−0.044	−0.028	−0.013
	[0.010]**	[0.011]**	[0.010]
	{0.014}**	{0.016}	{0.019}

TABLE II
(CONTINUED)

	ln (Property arrests)	ln (Property arrests)	ln (Property arrests per capita)
Controls include:			
Fixed effects for state and age*year interactions	yes	yes	yes
State*age interactions	yes	yes	yes
State*year interactions	yes	yes	yes
ln(Population)	no	yes	no

Notes. The unit of observation is a state by year by single year of age. The sample covers the period 1985–1998 for ages 15–24. If data were available for all states, years, and ages, the total number of observations would be 7,140. Due to missing arrest data and occasional zero values for arrests, the actual number of observations is somewhat smaller. The number of observations for the violent arrests regressions is 6,724 and for the property arrests regressions is 6,730. Estimation is weighted least squares. The rows labeled "original" use the abortion measure that was used in Donohue and Levitt (2001), but for the extended sample from 1985–1998. The rows labeled "with corrections" incorporates the changes described in the text (correcting for cross-state mobility, the appropriate year for the abortion rate, and using place of residence instead of place of occurrence of abortions). The rows labeled "IV using CDC" run weighted instrumental variables regressions where the Alan Guttmacher Institute measure of abortion is instrumented for with the Center for Disease Control (CDC) measure of abortion. Two sets of standard errors are reported: square brackets indicate standard errors for clustering by year of birth×state while curly brackets indicate standard errors that account for clustering by state. All reported coefficients are multiplied by 100. * implies statistical significance at the .05 level, and ** implies statistical significance at the .01 level.

V. Foote and Goetz's Claim That Omitted Variable Bias Explains the Link between Abortion and Crime Is Unfounded

Abortion rates are not randomly distributed across states. Abortion rates tend to be higher in states that are highly urbanized, have larger minority populations, are politically liberal, and had higher preabortion crime rates. Because the analysis in Donohue and Levitt (2001) includes state-fixed effects, differences across states that remain constant over time are not used to identify our estimates. If, however, there are omitted variables across states that are changing over time, we might spuriously attribute the effects of these omitted variables to legalized abortion. Foote and Goetz argue, in particular, that because high abortion states also tended to be high crime states prior to legalization, some other factor that happened to correlate with abortion really caused crime to fall in these states.[14]

14. It should be noted that Foote and Goetz's "unidentified but posited explanation" for the drop in crime that happens to correlate with abortion legalization is not a regression-to-the-mean effect: crime in the high-abortion states had been persistently high for decades prior to the period of our analysis. Moreover, Foote

As evidence for this claim, Foote and Goetz report specifications similar to Table IV of Donohue and Levitt (2001), with four changes: (1) a residence-based measure of abortion replaces the original measure, (2) the data set has been extended to include six additional years of data (1985–2003), (3) they add an interaction between the mean of a state's log per capita crime rate from 1970 to 1984 and a linear trend, and (4) they control for year effects within Census divisions. In Table III, we introduce these changes into our first three columns. In column (1) of Table III, we report point estimates using the original sample period (1985–1997) and specification, plus the residence-based abortion measure. Column (2) is identical to column (1), except that the sample has been extended to cover the period 1985–2003. Notably, the results obtained are just as strong after the addition of six years of new data, demonstrating that our model performed well out of sample.[15] Column (3) adds the Foote and Goetz interaction between the 1970–1984 mean log per capita crime rates and a linear time trend, as well as the division-year controls. As they previously demonstrated, the abortion coefficients shrink roughly in half from column (2) to column (3).

The Foote and Goetz findings, however, prove to be very sensitive to minor alterations in specification. Foote and Goetz's Table II, column (5) results include Census division-year interactions. Column (4) of our Table III shows that without the division-year interactions, but including the interaction of 1970–1984 mean log per capita crime rates and a linear crime trend, the effect of abortion on crime remains highly statistically significant for violent *and* property crime. Column (4) also shows that even the effect of abortion on murder is sizeable and close to statistical

and Goetz's conjectured "omitted" variable is not one that any other crime researcher has identified. In an interesting paper using Bayesian econometrics to identify plausible covariates in crime regressions, Strnad (2007) takes all of the variables from a number of studies of U.S. crime and systematically evaluates which permutations of explanatory variables best fit the state panel data on crime over the period from 1977 to 1999 for each of nine FBI crime categories. The approach assigns a posterior probability to each permutation for each crime category. Based on these probabilities, two variables stand out as crime predictors: the rate of incarceration and the effective abortion rate. According to Strnad, these two variables occur so frequently in high-probability permutations that across all nine FBI crime categories their overall probability of inclusion is virtually one.

15. In stark contrast, for instance, to the crime model that Lott and Mustard (1997) used to estimate the impact of state right to carry concealed gun laws, as demonstrated by Ayres and Donohue (2003).

TABLE III

DEMONSTRATING THE ROBUSTNESS OF THE ABORTION-CRIME LINK TO ALLOWING FOR CONVERGENCE IN STATE CRIME RATES

	Coefficient on abortion measure in regressions							
Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Violent crime	−0.159	−0.165	−0.087	−0.113	−0.13	−0.152	−0.543	−0.16
	[0.044]**	[0.035]**	[0.073]	[0.035]**	[0.079]	[0.133]	[0.227]*	[0.088]
Murder	−0.139	−0.121	−0.081	−0.102	−0.123	−0.115	−0.741	−0.248
	[0.067]*	[0.053]*	[0.105]	[0.054]	[0.119]	[0.200]	[0.401]	[0.100]*
Property crime	−0.114	−0.133	−0.03	−0.084	−0.063	0.008	−0.262	−0.062
	[0.026]**	[0.025]**	[0.036]	[0.032]*	[0.036]	[0.047]	[0.135]	[0.030]*
Exactly replicates which column in Foote/Goetz Table II?	(2)	(3)	(5)	—	—	—	—	—
Years included in the regression	1985–1997	1985–2003	1985–2003	1985–2003	1985–2003	1985–2003	1993–2003	1960–2003
Include division*year interactions?	No	No	Yes	No	Yes	Yes	Yes	Yes
Include lagged crime*time trend?	No	No	Yes	Yes	Yes	No	No	No
Include Washington, DC?	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Include state-specific trends?	No	No	No	No	No	Yes	Yes	Yes

Notes. Values reported in the table are the coefficient on the effective abortion rate in regressions in which the dependent variable is either violent crime, murder, or property crime. Columns (1)–(3) replicate columns (2), (3), and (5) of Foote and Goetz (2008) Table II. Column (2) differs from column (1) in that six extra years of data have been added to the sample. Column (3) adds a division–year interaction term as well as an interaction between a national time trend and the logged average per capita crime rate in the state over the period 1970–1984. Column (4) is identical to column (3) except that division–year interactions are excluded. Column (5) is identical to column (3) except that Washington, DC is excluded from the sample. Columns (6)–(8) allow more general forms of convergence by including state-specific time trends; these specifications nest the particular form of convergence allowed for in columns (3) and (5). Column (7) restricts the sample to the period 1993–2003, in which crack is less important. Column (8) expands the sample to cover the period 1960–2003, which aids in pinning down state-specific trends. All reported coefficients are multiplied by 100. The numbers in brackets are standard errors that account for clustering at the state level. * implies statistical significance at the .05 level, and ** implies statistical significance at the .01 level.

significance at the .05 level, with an estimated effect of –.102 (standard error = .054).[16]

Second, the Foote and Goetz results depend critically on the inclusion of the District of Columbia, which is an extreme outlier on abortion rates (nearly four times the national average for the 1970–2000 period and far above any state in the union). Additionally, there is a sharp break in the AGI measured abortion rate in DC between 1976 and 1977, with the reported abortion rate almost doubling between those two years.[17] When we omit the District of Columbia, the coefficients on the abortion measure in the violent crime and murder regressions become nearly as large as in the original specification (compare column (5) to column (3) in our Table III). The estimate for property crime remains smaller than the original, but is twice as large as in column (3).

Third, the particular functional form for the interaction used by Foote and Goetz is restrictive. An alternative approach is to include state-specific trends, as we do in column (6). This specification nests the preferred Foote and Goetz model in column (3) but with even greater freedom in capturing omitted variables. Once again, the coefficients on violent crime and murder are nearly identical to the estimates that do not include Foote and Goetz's interaction term, except that the standard errors are much larger because so much of the variation has been removed from the data. The property crime coefficient, in contrast, is close to zero.

Fourth, the coincident timing of crack cocaine's arrival and the peak crime ages of the first cohorts exposed to legalized abortion confounds the measurement of the impact of abortion. To address this concern, column (7) restricts the sample to the period 1993–2003, when crack-related violence is less prevalent (Fryer et al. 2005). All of the abortion coefficients increase dramatically and are much larger than in any of the previous columns.

As a final robustness check, we expand the sample to cover the period 1960–2003, which provides greater precision in pinning down state specific crime trends. These results, shown in column

16. All three abortion estimates are stronger if DC is dropped from this regression, and all are statistically significant at the .05 level – including murder (with an estimated coefficient of –.116 and a standard error of .053). The next paragraph notes the problem with the DC abortion measure.

17. Other states do not experience this jump. The timing of this break corresponds to a time when the AGI changed the manner in which they collect abortion data. There is no concurrent media report or other evidence to suggest this reported jump in DC abortions represents anything other than an artificial change in the way AGI records abortions in DC.

(8), demonstrate that the abortion coefficients continue to be large and statistically significant.

VI. Conclusions

We are indebted to Foote and Goetz for identifying a mistake in one of the tables in our original paper and pointing out the opportunity to more directly test the competing hypotheses regarding the explanation for why exposure to legalized abortion is associated with lower future crime. We illustrate that using better data than we initially had available and a more thoughtfully constructed abortion proxy yields results that are in many cases stronger than those reported in our initial paper, even after the issues raised by Foote and Goetz are addressed. Our further analysis of their claims regarding omitted variable bias as an explanation for the link between legalized abortion shows that their results are extremely sensitive to minor alterations, including allowing for more general models that nest their original specification.

Yale Law School and National Bureau of Economic Research
University of Chicago

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