INFERENCE ON TREATMENT EFFECTS AFTER SELECTION AMONGST HIGH-DIMENSIONAL CONTROLS: FURTHER DISCUSSION OF EMPIRICAL EXAMPLE

A. BELLONI, V. CHERNOZHUKOV, AND C. HANSEN

1. Empirical Example: Estimating the Effect of Abortion on Crime: Results in Levels

In this note, we expand on the discussion of the empirical section in the main paper by considering estimation of the effect of abortion on crime in levels. We consider both the original model of Donohue III and Levitt (2001) as well as the model from Donohue III and Levitt (2008) which responds to a criticism raised in Foote and Goetz (2008) which is similar to the conclusion we draw in the original data. The results using variable selection show that the results in Donohue III and Levitt (2008) also become imprecise once one considers a broad set of controls and selects among them using our variable selection technique.

Donohue III and Levitt (2001) discuss two key arguments for a causal channel relating abortion to crime. The first is simply that more abortion among a cohort results in an otherwise smaller cohort and so crime 15 to 25 years later, when this cohort is in the period when its members are most at risk for committing crimes, will be otherwise lower given the smaller cohort size. The second argument is that abortion gives women more control over the timing of their fertility allowing them to more easily ensure that childbirth occurs at a time when a more favorable environment is available during a child's life. For example, access to abortion may make it easier to ensure that a child is born at a time when the family environment is stable, the mother is more well-educated, or household income is stable. This second channel would mean that more access to abortion could lead to lower crime rates even if fertility rates remained constant.

The basic problem in estimating the causal impact of abortion on crime is that state-level abortion rates are not randomly assigned, and it seems likely that there will be factors that are associated to both abortion rates and crime rates. It is clear that any association between the current abortion rate and the current crime rate is spurious. However, even if one looks at say the relationship between the abortion rate 18 years in the past and the crime rate among current 18 year olds, the lack of random assignment makes establishing a causal link difficult without adequate controls. An obvious confounding factor is the existence of persistent state-to-state differences in policies, attitudes, and demographics that are likely related to the overall state level abortion and crime rates. It is also important to control flexibly for aggregate trends. For example, it could be the case that national crime rates were falling over this period while national abortion rates were rising but that these trends were driven by completely different factors. Without controlling for these trends, one would mistakenly associate the reduction in crime to the increase in abortion. In addition to these overall differences across states and times, there are other time varying characteristics such as state-level income, policing, or drug-use to name a few that could be associated with current crime and past abortion.

To address these confounds, Donohue III and Levitt (2001) estimate a model for state-level crime rates running from 1985 to 1997 in which they condition on a number of these factors. Their basic specification is

$$y_{cit} = \alpha_c a_{cit} + w_{it}'\beta_c + \delta_{c,i} + \gamma_{c,t} + \varepsilon_{cit} \quad (1.1)$$

where i indexes states, t indexes times, $c \in \{\text{violent, property, murder}\}$ indexes type of crime, $\delta_{c,i}$ are state-specific effects that control for any time-invariant state-specific characteristics, $\gamma_{c,t}$ are time-specific effects that control flexibly for any aggregate trends, w_{it} are a set of control variables to control for time-varying confounding state-level factors, a_{cit} is a measure of the abortion rate relevant for type of crime c,[1] and y_{cit} is the crime-rate for crime type c. Throughout the remainder of this section, we drop the c subscript for convenience but note that separate models are estimated for each crime type and thus all coefficients are allowed to freely vary across crime type. Donohue III and Levitt (2001) use the log of lagged prisoners per capita, the log of lagged police per capita, the unemployment rate, per-capita income, the poverty rate, AFDC generosity at time $t-15$, a dummy for a state having a concealed weapons law, and beer consumption per capita for w_{it}, the set of time-varying state-specific controls. Tables IV and V in Donohue III and Levitt (2001) present baseline estimation results based on (1.1) as well as results from different models which vary the sample and set of controls to show that the baseline estimates are robust to small deviations from (1.1). We refer the reader to the original paper for additional details, data definitions, and institutional background.

For our analysis, we follow Donohue III and Levitt (2001) and rely on the argument that the abortion rates defined above may be taken as exogenous relative to crime rates conditional upon

[1]This variable is constructed as a weighted average of abortion rates where weights are determined by the fraction of the type of crime committed by various age groups. For example, if 60% of violent crime were committed by 18 year olds and 40% were committed by 19 year olds in state i, the abortion rate for violent crime at time t in state i would be constructed as .6 times the abortion rate in state i at time $t-18$ plus .4 times the abortion rate in state i at time $t-19$. See Donohue III and Levitt (2001) for further detail and exact construction methods.

a set of factors. Unlike Donohue III and Levitt (2001), we do not assume that the identity of these factors is known and allow for smooth, flexible trends to account for unobservable factors that may influence both abortion and crime but smoothly trend over time. Given the seemingly obvious importance of controlling for state and time effects, we account for these in all models we estimate by including a full set of state and time dummies. Thus, we estimate models of the form

$$y_{it} = \alpha a_{it} + w_{it}'\beta_y + \delta_{y,i} + \gamma_{y,t} + g(z_{it}, t) + \zeta_{it} \tag{1.2}$$

$$a_{it} = w_{it}'\beta_a + \delta_{a,i} + \gamma_{a,t} + m(z_{it}, t) + v_{it} \tag{1.3}$$

where $g(z,t)$ and $m(z,t)$ are smooth functions of observed variables z_{it} which includes w_{it}, time-invariant characteristics of $\{y_{it}, a_{it}, w_{it}\}_{t=1}^{T}$ such as initial conditions or state-level averages, and time. We use the same state-level data as Donohue III and Levitt (2001) but delete Alaska, Hawaii, and Washington, D.C. which gives a sample with 48 cross-sectional observations and 13 time series observations for a total of 624 observations. With these deletions, our baseline estimates using the same controls as in (1.1) are quite similar to those reported in Donohue III and Levitt (2001). Baseline estimates from Table IV of Donohue III and Levitt (2001) and our baseline estimates of (1.1) are given in the first and second row of Panel A of Table 2.[2]

Note that interpreting estimates of the effect of abortion from model (1.1) as causal relies on the belief that there are no higher-order terms of the control variables, no interaction terms, and no additional excluded variables that are associated both to crime rates and the associated abortion rate. Allowing for such variables is important in that one might believe that there may be some feature of a state that is associated both with its growth rate in abortion and its growth rate in crime. For example, having an initially high-level of abortion could be associated with having high-growth rates in abortion and low growth rates in crime. Failure to control for this factor could then lead to misattributing the effect of this initial factor, perhaps driven by policy or state-level demographics, to the effect of abortion. In practice, it is common to account for this possibility by allowing state-specific trends (e.g. by specifying $g(z_{it}, t) = \kappa_{g,i}t$) in addition to state-specific intercepts. Results from estimating (1.1) with state-specific trends are given in the third row in Table 2 Panel A. In this example, the inclusion of state-specific linear trends renders the results very imprecise. Of course, one might argue that including state-specific linear trends is too aggressive in a sample with only 13 time series observations. The linear trend specification is also very restrictive in imposing that any unobserved factors that relate to both abortion and crime exhibit constant growth over the 13 year time period.

[2]Our estimates differ for three reasons. First, we delete Alaska, Hawaii, and Washington, D.C. Second, Donohue III and Levitt (2001) use population weighted estimates. Third, Donohue III and Levitt (2001) use an FGLS estimator based on an AR(1) model in the errors where the errors across states share the same AR coefficient.

The assumption of constant growth becomes even more problematic when one expands the time period as in Foote and Goetz (2008) and Donohue III and Levitt (2008) discussed below.

We follow the Chamberlain (1985) type approach and approximate $g(z_{it}, t)$ and $m(z_{it}, t)$ by a large number of controls. We approximate these functions by forming 27 factors to include in z_{it},

$$z_{it} = (a_{i0},\ \frac{1}{T}\sum_t a_{it},\ y_{i0},\ w'_{i0},\ \frac{1}{T}\sum_t w'_{it},\ w'_{it})',$$

forming nine smooth function of time,

$$f_t = (t,\ t^2,\ t^3,\ \sin(\pi\frac{t}{T}),\ \sin(2\pi\frac{t}{T}),\ \sin(3\pi\frac{t}{T}),\ \cos(\pi\frac{t}{T}),\ \cos(2\pi\frac{t}{T}),\ \cos(3\pi\frac{t}{T}))',$$

and then supposing that

$$g(z_{it}, t) \approx \sum_{r=1}^{27}\sum_{s=1}^{9} \beta_{g,r,s} z_{it,r} f_{t,s} = h'_{it}\beta_g \qquad \text{and}$$

$$m(z_{it}, t) \approx \sum_{r=1}^{27}\sum_{s=1}^{9} \beta_{m,r,s} z_{it,r} f_{t,s} = h'_{it}\beta_m$$

where h_{it} is the vector containing all the interactions, and β_g and β_m are the vectors of coefficients for each equation. That is, we add an additional 243 control variables to the model and use the methods developed in this paper to search among these 243 additional control variables to see if there are potentially important factors that are missed in equation (1.1).[3] With this set of controls, the models we estimate are all more general than (1.1) and are neither more nor less general than a model with state-specific trends in that we allow for nonlinearity in trends but do not allow for arbitrarily different state-specific coefficients. Rather, we restrict these coefficients to differ depending on values of observable covariates.

Controlling for a large set of variables as described above is desirable from the standpoint of making the belief underlying the causal interpretation of the abortion coefficient, that the abortion rate defined above may be taken as being as good as randomly assigned once the set of variables considered is controlled for, more plausible. As with the inclusion of state-specific trends, the downside is that controlling for many variables lessens our ability to identify the effect of interest and thus tends to make estimates far less precise. For example, the estimated abortion effects conditioning on the full set of 68 variables in (1.1) plus the 243 approximating functions (for a total of 311 control variables) are given in the fourth row of Table 2 Panel A. As expected, all coefficients are estimated very imprecisely. Of course, very few researchers would consider using 311 controls with only 624 observations due to exactly this issue.

[3] To allow time effects, state effects, and w_{it} to enter each equation without shrinkage, we use our methods based on $\tilde{y}_{it}$, $\tilde{a}_{it}$ and $\tilde{h}_{it}$ where $\tilde{y}_{it}$ is the residual from the regression of y_{it} on w_{it} and a full set of state and time dummies and $\tilde{a}_{it}$ and $\tilde{h}_{it}$ are defined similarly.

We are faced with a trade-off between the precision of the estimate and the plausibility of the conditional exogeneity assumption. By including additional controls in the specification, we make the conditional exogeneity assumption more plausible. At the same time, we potentially reduce the precision of our estimate. The double selection method proposed in this paper offers one rigorous approach to achieving a balance. Thus, the approach complements the usual careful specification analysis by providing a researcher a simple-to-implement, data-driven way to search for a set of influential confounds from among a sensibly chosen broader set of potential confounding variables.

In the abortion example, we use the post-double-Lasso estimator defined in Section **??** for each of our dependent variables. For violent crime, a total of 15 variables are selected: eight in the abortion equation[4] and seven in the crime equation.[5] For property crime, 16 variables are selected: ten in the abortion equation[6] and seven in the crime equation[7] with one occurring in both. For murder, ten variables are selected: eight in the abortion equation[8] and two in the crime equation.[9] It is interesting in looking at the selected variables that in all cases initial or average levels of abortion interacted with nonlinear trend terms and initial levels of crime interacted with nonlinear trend terms are selected. This selection illustrates the potential importance of allowing for nonlinear trends and also the potential that there may be omitted factors that are related to both abortion and crime.

Estimates of the causal effect of abortion on crime obtained by searching for confounding factors among our set of 243 potential controls are given in the fifth row of Panel A of Table 2. Each of these estimates is obtained from the least squares regression of the crime rate on the abortion rate, a full set of state dummies, a full set of time dummies, the initial eight controls that vary across states and time from (1.1) and the 15, 16, and ten controls selected by the post-double-Lasso procedure for violent crime, property crime, and murder respectively.

[4]The selected variables are average abortion times t, average abortion times $\cos(\pi \frac{t}{T})$, initial crime times t^2, initial crime times $\cos(2\pi \frac{t}{T})$, average income times t^3, average income times $\sin(\pi \frac{t}{T})$, average income times $\cos(2\pi \frac{t}{T})$, and initial poverty times $\cos(2\pi \frac{t}{T})$.

[5]The selected variables are average abortion times t^3, initial abortion times t^3, initial abortion times $\sin(\pi \frac{t}{T})$, initial poverty times $\sin(2\pi \frac{t}{T})$, initial poverty times $\cos(\pi \frac{t}{T})$, police_{it} times t^3, and beer_{it} times $\sin(3\pi \frac{t}{T})$.

[6]The selected variables are average abortion times $\cos(\pi \frac{t}{T})$, initial abortion times $\sin(3\pi \frac{t}{T})$, initial crime times $\cos(\pi \frac{t}{T})$, average income times t, average income times $\cos(\pi \frac{t}{T})$, initial poverty times $\cos(2\pi \frac{t}{T})$, initial beer times $\cos(2\pi \frac{t}{T})$, prison_{it} times $\cos(\pi \frac{t}{T})$, income_{it} times $\cos(\pi \frac{t}{T})$, and AFDC_{it} times $\cos(2\pi \frac{t}{T})$.

[7]The selected variables are average abortion times t^3, initial crime times $\sin(2\pi \frac{t}{T})$, initial crime times $\cos(\pi \frac{t}{T})$, average police times $\cos(2\pi \frac{t}{T})$, average AFDC times t, initial AFDC times t, and initial AFDC times t^2.

[8]The selected variables are average abortion times t^2, average abortion times $\cos(\pi \frac{t}{T})$, initial crime times t^3, initial crime times $\cos(2\pi \frac{t}{T})$, average income times t^3, average income times $\sin(\pi \frac{t}{T})$, average income times $\cos(2\pi \frac{t}{T})$, and average income times $\cos(3\pi \frac{t}{T})$.

[9]The variables selected are average abortion times $\sin(\pi \frac{t}{T})$ and initial abortion times $\sin(\pi \frac{t}{T})$.

The estimates for the effect of abortion on violent crime and the effect of abortion on murder are quite imprecise, producing 95% confidence intervals that encompass large positive and negative values. The estimated effect for property crime is roughly in line with the previous estimates though it is no longer significant and has a 95% confidence interval that includes negative as well as modest positive effects. For a quick benchmark relative to the simulation examples, we note that the R^2 obtained by regressing the crime rate on the selected variables are .2522, .3533, and .0554 for violent crime, property crime, and the murder rate respectively and that the R^2's from regressing the abortion rate on the selected variables are .9906, .9039, and .9863 for violent crime, property crime, and the murder rate respectively. These values correspond to regions of the R^2 space considered in the simulation where the post-double-selection procedure performed quite well, while the standard post-single-selection procedures performed quite poorly.

While the inclusion of trigonometric terms in our approximations allows for capturing some types of cyclicality, some researchers may feel more comfortable restricting attention to simpler trend specifications. To allow for this, we also present results in which the trigonometric functions are dropped from f_t, so that

$$f_t = (t,\ t^2,\ t^3).$$

That is, we approximate the functions as $g(z_{it}, t) \approx \sum_{r=1}^{27} \sum_{s=1}^{3} \beta_{g,r,s} z_{it,r} f_{t,s} = h_{it}' \beta_g$ and $m(z_{it}, t) \approx \sum_{r=1}^{27} \sum_{s=1}^{3} \beta_{m,r,s} z_{it,r} f_{t,s} = h_{it}' \beta_m$ which allows only cubic polynomial trends interacted with state-level characteristics. In this case, only 81 terms are considered in addition to the 68 controls from the original specification. Results using all 149 controls are given in the row "Polynomial Trends" in Table 2 Panel A, and results based on Lasso selection among the 81 added controls are given in the row "Post-Double-Selection, Polynomial Trends." Looking at these results we see that we would draw the same qualitative conclusion using this restricted specification as we would when allowing for trigonometric terms as well. Specifically, the estimated abortion effects become quite imprecise after allowing only for the polynomial terms in time.[10]

A similar conclusion was reached by Foote and Goetz (2008) who, without doing formal variable selection, found that inclusion of a linear trend interacted with the average crime rate from a period before the abortion rate should have been able to have an effect on the crime rate substantially attenuated the estimated effects from Donohue III and Levitt (2001) and

[10]In addition to the 68 original variables, the double-selection procedure selects ten total additional variables for the violent crime regression, eight additional variables for the property crime regression, and five additional variables for the murder regression. In each case, the mean of the abortion rate times t is selected and this variable accounts for most of the explanatory power among the selected additional regressors.

also rendered them imprecise. It is interesting that we reach a similar conclusion through the use of formal variable selection procedures motivated by the desire to allow for allow flexible, yet parsimonious trends in an effort to make the exogeneity assumption conditional on controls more plausible.

In a response to Foote and Goetz (2008), Donohue III and Levitt (2008) note that one problem with allowing flexible trends is that the short time series renders estimates of the treatment effect imprecise once flexible trends are allowed. Specifically, estimated treatment effects are imprecise in their preferred specification

$$y_{it} = \alpha a_{it} + \delta_i + \gamma_{d,t} + \kappa_i t + \varepsilon_{it} \tag{1.4}$$

where δ_i is a state-specific effect, κ_i is a state-specific coefficient on a linear trend, and $\gamma_{d,t}$ is Census division × time effect. To address this issue, Donohue III and Levitt (2008) extend the sample period to 1960-2003 to allow more precise estimates of the trends and thus more reliable estimates of the treatment effect. They find that the results in this longer sample with the full set of division times time interactions and state-specific trends are similar to the initial results in the shorter panel. Results from this analysis in Donohue III and Levitt (2008) are provided in the first row of Panel B of Table 2. In the second row of Table 2, Panel B, we report results from our estimates of the abortion effect using data from 1960-2003 using exactly the same methodology as Donohue III and Levitt (2008), and we report results from simple OLS regression of (1.4) in the third row.[11]

While (1.4) is certainly more general than (1.1), state-specific linear trends are still quite restrictive, especially over a time period of 40 years. Specifically, it is a strong assumption that unobserved factors that are correlated to both state level abortion and crime rates exhibited constant growth over such a long time period. To allow for smooth, but flexible trends, we once again consider variable selection in a more general model

$$y_{it} = \alpha a_{it} + \delta_{y,i} + \gamma_{y,d,t} + \kappa_{y,i} t + g(z_{it}, t) + \zeta_{it} \tag{1.5}$$

$$a_{it} = \delta_{a,i} + \gamma_{a,d,t} + \kappa_{d,i} t + m(z_{it}, t) + v_{it} \tag{1.6}$$

where $g(z,t)$ and $m(z,t)$ are smooth functions of observed variables z_{it} which includes time-invariant characteristics of $\{y_{it}, a_{it}, w_{it}\}_{t=1}^{T}$ such as initial conditions or state-level averages and

[11]Our results differ due to the exclusion of Alaska, Hawaii, and Washington, D.C. We also completed the data on abortion before 1985 by filling in 0 for all abortion rates before 1985.

time. For this longer time period, we approximate g and m by setting

$$z_{it} = (a_{i1985},\ \frac{1}{44}\sum_{t=1960}^{2003} a_{it},\ y_{i1960},\ y_{i1961},\ w'_{i1985},\ \frac{1}{13}\sum_{t=1985}^{1997} w'_{it})',$$

$$f_t = (t^2,\ t^3,\ t^4,\ t^5,\ \sin(\pi\frac{t}{T}),\ \sin(2\pi\frac{t}{T}),\ \sin(3\pi\frac{t}{T}),\ \sin(4\pi\frac{t}{T}),$$
$$\cos(\pi\frac{t}{T}),\ \cos(2\pi\frac{t}{T}),\ \cos(3\pi\frac{t}{T}),\ \cos(4\pi\frac{t}{T}))',$$

and then supposing

$$g(z_{it},t) \approx \sum_{r=1}^{20}\sum_{s=1}^{12} \beta_{g,r,s} z_{it,r} f_{t,s} = h'_{it}\beta_g \quad \text{and}$$
$$m(z_{it},t) \approx \sum_{r=1}^{20}\sum_{s=1}^{12} \beta_{m,r,s} z_{it,r} f_{t,s} = h'_{it}\beta_m,$$

where h_{it} is the vector containing all the interactions, and β_g and β_m are the vectors of coefficients for each equation. Thus, we add an additional 240 control variables to (1.4).[12]

Estimates of the abortion effect using the full set of 713 controls consisting of the 473 controls in (1.4) augmented with the 240 additional controls for smooth nonlinear trends are given in the fourth row of Table 2 Panel B. As expected, the estimated abortion effects are extremely imprecise given this large set of controls.

To pare down the number of controls, we employ the Double-Selection procedure developed in this paper to search for a smaller set of relevant controls among the 240 potential additions. Based on this exercise, we select a total of 31 additional variables for the violence equation, 30 for the abortion equation, and 27 for the murder equation. R^2's from the regression of crime rates on the controls are .2806, .3451, and .0422 for violent crime, property crime, and the murder rate respectively; and the R^2's from regressing the abortion rate on the selected variables are .9618, .9461, and .9775 for violent crime, property crime, and the murder rate respectively. Estimates of the treatment effect controlling for the variables in Donohue III and Levitt (2008) and those selected by Double-Selection are given in the "Post-Double-Selection" row of Table 2, Panel B. As in the original data, we find that estimates of the abortion effect are relatively imprecise once parsimonious nonlinear trends are allowed for.

As in the previous specification, we report results using only interactions with the polynomial trend terms, i.e.

$$f_t = (\ t^2,\ t^3,\ t^4,\ t^5)',$$

[12] To allow for all the effects in (1.4) to enter each equation without shrinkage, we use our methods based on $\tilde{y}_{it}$, $\tilde{a}_{it}$ and $\tilde{h}_{it}$ where $\tilde{y}_{it}$ is the residual from the regression of y_{it} on a full set of state dummies, a full set of Census division cross time dummies, and a full set of state-specific trends and $\tilde{a}_{it}$ and $\tilde{h}_{it}$ are defined similarly.

in the final two rows of Panel B of Table 2. [13] Using only the interactions with the polynomial terms adds 80 potential regressors to the 473 included in the original Donohue III and Levitt (2008) specification. Results using the full set of 553 regressors are reported in the row "Polynomial Trends" in Table 2 Panel B and show that once again using this broad set of regressors results in imprecise estimates of the regression coefficients. The lack of precision in the estimated abortion effect is qualitatively unchanged after using the double-selection procedure to select controls from among this restricted set, again illustrating that the baseline result is not driven by the inclusion of trigonometric terms in the set of approximating functions.[14]

We believe that the example in this section illustrates how one may use modern variable selection techniques to complement causal analysis in economics. In the abortion example, we are able to search among a large set of controls and transformations of variables when trying to estimate the effect of abortion on crime. Considering a large set of controls makes the underlying assumption of exogeneity of the abortion rate conditional on observables more plausible, while the methods we develop allow us to produce an end-model which is of manageable dimension. In this example, we see that inference about the treatment effects using the variable selection method differs substantively from inference drawn using the original set of controls. This statement is true whether one considers the data and model from Donohue III and Levitt (2001) or Donohue III and Levitt (2008). This difference is driven by the variable selection method's selecting different variables than are usually considered. Thus, it appears that the usual interpretation of there being a substantive causal effect of abortion on crime hinges on strong prior beliefs about the types of trends that may appear in the structural equation. In particular, inclusion of a modest number of smooth nonlinear trends interacted with time-invariant state-level characteristics substantively increases the variance of the estimated treatment effects.

References

Chamberlain, G. (1985): "Longitudinal Analysis of Labor Market Data," in *Heterogeneity, Omitted Variable Bias, and Duration Dependence*, ed. by J. J. Heckman, and B. Singer. Cambridge University Press.

Donohue III, J. J., and S. D. Levitt (2001): "The Impact of Legalized Abortion on Crime," *Quarterly Journal of Economics*, 116(2), 379–420.

——— (2008): "Measurement Error, Legalized Abortion, and the Decline in Crime: A Response to Foote and Goetz," *Quarterly Journal of Economics*, 123(1), 425–440.

[13] The results are qualitatively similar if one only allows up to a cubic term in the trends, i.e. if one considers $f(t) = (t^2,\ t^3)$.

[14] In addition to the 473 original variables, the double-selection procedure selects 12 total additional variables for the violent crime regression, 11 additional variables for the property crime regression, and 11 additional variables for the murder regression.

Foote, C. L., and C. F. Goetz (2008): "The Impact of Legalized Abortion on Crime: Comment," *Quarterly Journal of Economics*, 123(1), 407–423.

Table 2. Estimated Effects of Abortion on Crime Rates (Levels)

	Violent Crime		Property Crime		Murder	
	Effect	Std. Err.	Effect	Std. Err.	Effect	Std. Err.
	A. Donohue and Levitt (2001) Table IV					
DL (2001) Table IV	-0.129	0.024	-0.091	0.018	-0.121	0.047
Fixed Effects	-0.131	0.045	-0.091	0.016	-0.131	0.058
Fixed Effects + State Trends	-0.149	0.185	0.060	0.093	-0.383	0.207
All Controls	0.183	0.447	0.013	0.067	0.855	0.974
Post-Double-Selection	0.133	0.303	-0.053	0.044	-0.692	0.438
Polynomial Trend	0.321	0.349	-0.032	0.060	0.851	0.616
Post-Double-Selection, Polynomial Trend	0.013	0.251	-0.041	0.047	-0.178	0.276
	B. Donohue and Levitt (2008) Table III					
DL (2008) Table III	-0.160	0.088	-0.062	0.030	-0.248	0.100
DL (2008) Specification	-0.158	0.087	-0.057	0.026	-0.249	0.099
Fixed Effects	-0.186	0.063	-0.110	0.046	-0.061	0.078
All Controls	0.516	0.400	0.146	0.127	0.611	0.523
Post-Double-Selection	0.060	0.214	-0.025	0.086	0.460	0.322
Poynomial Trend	0.203	0.296	0.141	0.089	0.199	0.309
Post-Double-Selection, Polynomial Trend	-0.264	0.179	0.090	0.046	-0.088	0.192

Note: The table displays the estimated coefficient on the abortion rate, "Effect," and its estimated standard error. Numbers in the first row of Panel A are taken from Donohue III and Levitt (2001) Table IV, columns (2), (4), and (6). Numbers from the first row of Panel B are taken from Donohue III and Levitt (2008) Table III, column (8). The remaining rows are estimated by OLS of the crime rate on the abortion rate and different sets of controls described in the text and use standard errors clustered at the state-level. In Panel A, the row labeled "All Controls" uses 311 control variables as discussed in the text that include the 68 controls from the original specification of Donohue III and Levitt (2001) Table IV along with 243 variables meant to allow for flexible, smooth trends. The row labeled "Polynomial Trend" in Panel A restricts the set of controls added to allow for flexible trends to include only polynomial terms and uses only 149 total regressors, the 68 from the original specification and 81 added variables. In Panel B, the row labeled "All Controls" uses 713 control variables as discussed in the text that include the 473 controls from the original specification of Donohue III and Levitt (2008) Table III along with 240 variables meant to allow for flexible, smooth trends. The row labeled "Polynomial Trend" in Panel B restricts the set of controls added to allow for flexible trends to include only polynomial terms and uses only 553 total regressors, the 473 from the original specification and 80 added variables. The rows "Post-Double-Selection" report results from regressing the crime rates on the variables from the original Donohue III and Levitt (2001) and Donohue III and Levitt (2008) along with additional variables selected using the technique developed in this paper from among the set of variables considered in the corresponding "All Controls" row. The rows "Post-Double-Selection, Polynomial Trend" report results from regressing the crime rates on the variables from the original Donohue III and Levitt (2001) and Donohue III and Levitt (2008) along with additional variables selected using the technique developed in this paper from among the set of variables considered in the corresponding "Polynomial Trend" row. Further details are provided in the text.