Estimating the Deterrent Effect of Incarceration using Sentencing Enhancements

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Web Appendix A. Robustness Checks

To check the robustness of the finding of a deterrent effect of add-on gun laws, a number of other specifications were tested. I discuss potential confounds and how they were addressed.

1. Linear Specification

Table A1 presents coefficients from a number of robustness checks. The basic specification in the table is to report the effect of add-on gun laws on log gun robberies per capita within the first 3 years of the effective date. In all specifications presented thus far, the log crime rate has been used as the outcome. The choice of log was discussed in part A of Section IV, but there may be reasons why the simple crime rate would be the preferred outcome. If for example, one preferred the assumption that equal changes in crime rates should be treated equally, regardless of initial level of crime, then crime rate is the preferred measure.

In the first row of Table A1, the coefficients from the linear specification are presented (and thus the outcome is gun robberies per capita). While a number of the coefficients are insignificant at the 5% level, most are significant at the 10%, and they are all negative and of a magnitude that is consistent with the coefficients found using log crime rates.

2. Restricted comparison group: only states ever passing add-on laws

Another potential concern is that the comparison group for the basic specification uses all states, regardless of whether they ever passed an add-on gun law. If there is a secular difference in the time series between states adopting add-on laws and those not adopting them (not already captured by controls) this could impact the results. In regressions restricted to those states that ever pass an add-on gun law (Table A1, row 2), I find very similar coefficients to those presented in Table 3.

3. State Level Data

Since the laws of interest in this study are at the state level, it is useful to compare the results to those obtained using aggregate state-level data sets. State-level data has the advantage of being substantially less noisy than agency data, and incorporates a considerably larger fraction of the U.S. population. However, as noted before, it has the disadvantage of representing a widely varying population. I find that the impact of add-on gun laws on gun robbery rates using state level data is similar to that found using agency level data (Table A1, row 3).

4. Population and Weighting

Population data provided in the UCR was used both to calculate crime rates and to weight data appropriately, and thus all reported results are sensitive to population data. Several specification checks were performed to ensure that the results are not due to spurious population numbers. They include running the regressions unweighted by population (Table A1, row 4), using number of incidents as the dependent variable (rather than per capita- reported in Table A1, row 5), and not allowing agency populations to vary over time (Table A1, row 6). All of the specification checks yielded a negative impact of the add-on gun laws on gun robberies, although the first two were statistically insignificant.

5. Higher order time trends

State legislatures may respond not simply to trends in crime, but to an acceleration in crime rates increases, or to short term spikes that are not easily captured using linear trends. Not including higher order time trends in the regressions allows for the possibility that some of the nonlinearity observable in the pre-add-on periods in Figure 4 is due to this phenomenon. I addressed this concern by adding a cubic function of time to the basic regressions (Table A1, row 7), with the central findings unchanged.

6. Trend Breaks

Thus far most of the specifications have focused on the coefficient on $addon_{st}$, i.e. the difference in means before and after add-on gun laws. This choice has been made because a shift in mean crime rate is what the economic theory of crime predicts as the response to an increase in sanctions. However, one could certainly incorporate non-instantaneous information transmission which would lead to both a change in mean of crime rates and a change in time trends relative to a change in sanctions, represented by Equation A1.

$$y_{at} = \beta Addon_{st} + \theta Addon_{st} * relyr + \lambda_s + \gamma_t + \omega_s t + x_{st} + mm_{st} + \varepsilon_{at} \qquad \text{(A1)}$$

In fact, it is possible that the response to this policy change will not be instantaneous, and a more accurate representation would include a higher order terms of relative time to allow for adjustment to the new regime. To estimate this type of model one would simply need to modify Equation A1 by adding a polynomial in time relative to the add-on gun law effective date.

Results from this estimation are reported in Table A1, row 8. I do not find evidence for a significant shift in slope using this specification. This is likely due to the fact that the mean shift captures most of the pre-post add-on shift in crime. However, the addition of higher order terms of relative time, motivated by a more detailed theory of dissemination of information on sanctions to potential criminals could be a better fit to the data.

7. Triple Differences

If sentences of larger magnitude have a greater deterrent effect, one would expect to see a larger drop in gun robberies in those states with a larger add-on prison term. This dimension, add-on sentence term, can be interacted with the previous difference in difference to yield the following triple difference specification:

$$y_{at} = \phi Addon_{st} * Term_s + \beta Addon_{st} + \theta Term_s + \lambda_s + \gamma_t + x_{st} + mm_{st} + \varepsilon_{st} \qquad \text{(A2)}$$

The addition of a third dimension can be used to address the confound of contemporaneous policy changes as long as one does not expect a correlation between add-on magnitude and contemporaneous policy changes.

One empirical difficulty with estimating the triple difference is that data on the add-on sentence term is quite noisy. A number of states have fairly large ranges for their add-on sentence lengths, and thus the coding of this variable is difficult.[1] Perhaps due to this fact, the results from the triple difference regressions (Table A2) are largely insignificant. While insignificant, the coefficients are almost all negative providing weak evidence against the contemporaneous policy change possibility.

[1] When states have a range of add-on sentence length I used the minimum add-on term.

8. Lagged Dependent Variables

Thus far all models presented have made the assumption that crime is determined by contemporaneous variables, or lags of regional characteristics, such as prison population. It certainly seems plausible, however, that current levels of crime could be impacted by previous levels of crime. For example, a high level of crime in period t-1 could lead to a change in police vigilance, a quantity that is not readily quantifiable. This in turn could lead to a decrease in crime in period t. Another story which also leads to this sort of structure would be one where previous levels of crime are informative to prospective criminals in a way that is not fully accounted for in the control variables. Higher levels of crime in period t-1 could indicate greater likelihood of success, and thus a higher level of crime in period t. We can express this model with a lagged dependant variable as follows:

$$y_{st} = \rho y_{st-1} + \beta Addon_{st} + x_{st} + mm_{st} + \lambda_s + \gamma_t + \varepsilon_{st} \tag{A3}$$

The addition of the lagged dependant variable complicates the estimation procedure, relative to the models previously discussed. In particular, the fixed effect estimator thus far employed will be biased in the presence of a lagged dependant variable. This intuitively must be so since by the definition of a fixed effect, the lagged endogenous variable would be correlated with the error term. Given this difficulty, we follow the estimation procedure outlined in Arellano and Bond (1991).

First we may reinterpret all variables as deviations from the period means. This eliminates γ_t. Next, take the first difference of Equation A3 (first aggregating all variables in state s at time t into D_{st}):

$$y_{st} - y_{st-1} = \rho(y_{st-1} - y_{st-2}) + \beta(D_{st} - D_{st-1}) + (\varepsilon_{st} - \varepsilon_{st-1}) \tag{A4}$$

Ordinary least squares estimation will be inconsistent since the lagged dependant variable will be correlated with the error term through common period t-1 terms. Thus an instrumental variables approach is necessary to produce consistent estimates. Arellano and Bond propose using lagged values of the dependant variable and the other regressors as the instruments for the first differences. Their use requires the identifying assumption that a k^{th} lag may be used as an instrument only if there is no k^{th} order serial correlation. As in Arellano and Bond (1991) and Blundell and Bond (1998), I make use of the GMM procedure to optimally take advantage of this identifying assumption.[2] I use all lags of at least two years in per capita guns robberies, along with differences of the control variables to instrument for the lagged dependent variable, with the results shown in Table A3.[3]

Since the validity of the GMM procedure crucially hinges on the identifying assumptions, they must be tested. An Arellano-Bond test for autocorrelation in panel data is used to test the assumption of serial correlation for different orders. Further, a test of overidentifying restrictions that is robust to heteroskedasticity is also performed.

The Arrelano-Bond test for autocorrelation in panel data shows strong evidence for rejecting the assumption of no first order autocorrelation ($p<.002$), but cannot reject the assumption of no higher order autocorrelation (2nd through 5th order autocorrelation was tested). The Hansen J statistic was calculated for the overidentifying restrictions, and could not reject the hypothesis that the instruments were jointly exogenous. The specification yields an insignificant estimate that there was a 6% drop in the rate of gun robberies within three years of the add-on gun law effective date, using the preferred specification. Thus it appears that any bias caused by omission of the lagged crime rate is not substantial, and this finding bolsters the main results.

9. Placebo Laws

Bertrand et al. (2004) point out that standard errors in difference-in-difference regressions are often misestimated. This problem is particularly apparent in studies like the present one where the independent variable of interest is a dummy for a one-time law change, and therefore has substantial autocorrelation. I address this potential difficulty by adopting both of the remedies suggested in the paper: clustering standard errors, and using placebo laws[4] to generate standard errors.

All errors reported in the tables are clustered by state; in Table A4, I report standard errors generated by a Monte Carlo simulation. For each iteration of the simulation, a set of placebo laws was generated, by choosing with replacement from the effective dates for the actual add-on laws. A new effective date (or none) is assigned to each state. A series of regressions are

[2] There is one further assumption that is made, namely the standard assumption about the exogeneity of the other control variables being used for instruments.
[3] In order to use the techniques outlined in the above papers, I aggregated data to the state level.
[4] Helland and Tabarrok (2004) provide an excellent example of the importance of using placebo laws. They show that some of the most significant results found by Lott and Mustard in their 1997 paper become insignificant when using standard errors generated by placebo laws.

run using the placebo laws, and the point estimates recorded. The simulation is iterated 500 times, and the resulting standard errors reported in square brackets in Table A4.

The specification in this table is slightly different from those discussed previously in two ways. The data used is at the state-month level and the window of time used is symmetric around the add-on law effective date. For example, the two year impact uses data from two years prior to until two years after the add-on effective date.

The results here are consistent with those found previously using agency-level data. There seems to be a substantial deterrent effect of the add-on laws, which increases over the first three years. The coefficients in these regressions are not directly comparable to those from the annual ones, although they do seem to indicate a somewhat larger magnitude of impact of the add-on laws.

Importantly, the coefficients are still significant in a number of the specifications even when using the confidence intervals generated using the placebo laws. The standard errors generated by the simulation are larger than those resulting from asymptotic assumptions, although the clustering already makes a substantial correction to the standard errors. The placebo laws provide a strong test of the validity of the preceding analysis, which appears robust.

Table A1: Addressing Confounds

	Various outcomes - see notes							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) Outcome is gun robberies per capita (not log)	-0.71	-15.39	-20.32	-17.28*	-3.92	-17.35	-16.90	-11.52
	(12.08)	(10.99)	(11.38)	(7.81)	(10.51)	(12.34)	(9.43)	(6.40)
(2) Only states ever passing add-on laws	-0.03	-0.13**	-0.11*	-0.10**	-0.05	-0.12*	-0.10*	-0.06*
	(0.07)	(0.04)	(0.04)	(0.02)	(0.06)	(0.05)	(0.03)	(0.02)
(3) State Level Data	-0.15	-0.20**	-0.15**	-0.09*	-0.16	-0.21**	-0.14**	-0.08
	(0.10)	(0.06)	(0.04)	(0.03)	(0.11)	(0.06)	(0.04)	(0.04)
(4) Unweighted	-0.09	-0.15**	-0.12*	-0.11*	-0.08	-0.13*	-0.11*	-0.06
	(0.07)	(0.06)	(0.05)	(0.04)	(0.07)	(0.05)	(0.05)	(0.03)
(5) Reported crimes (not per capita)	-0.13	-0.17**	-0.14**	-0.08**	-0.17	-0.17**	-0.12**	-0.04
	(0.09)	(0.06)	(0.04)	(0.03)	(0.10)	(0.06)	(0.03)	(0.03)
(6) Initial Populations	-0.18*	-0.17**	-0.14**	-0.10**	-0.20*	-0.18**	-0.13**	-0.07*
	(0.07)	(0.05)	(0.04)	(0.03)	(0.08)	(0.06)	(0.03)	(0.03)
(7) Polynomial Time	-0.14	-0.17**	-0.13**	-0.09**	-0.17	-0.18**	-0.11**	-0.05*
	(0.08)	(0.05)	(0.04)	(0.02)	(0.09)	(0.06)	(0.03)	(0.02)
(8) Trend Breaks (coefficient reported is trend break)	-0.06**	-0.04	-0.00	0.00	-0.06**	-0.02	0.01	0.01
	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)
State-specific time trends	n	y	n	y	n	y	n	y
Balanced Panel	n	n	y	y	n	n	y	y
Restrict to Post-1974	n	n	n	n	y	y	y	y

* significant at 5%; ** significant at 1%

Note - See text in Appendix A for more details. The data consists of agency-year level observations (except for the third row, which is state-year level). Standard errors reported in parentheses are clustered at the state level to allow for intra-state correlation in error structure. State and year effects are included in all specifications. Controls include mandatory minimum law dummy, poverty rate, unemployment rate, racial composition, age composition, lagged police population share, and lagged imprisoned population share. All point estimates are for the impact of add-on gun laws within three years of the effective date. Unless otherwise noted, dependant variable is log gun robberies per 100,000 residents, and independant variable is a dummy that is one within 3 years after the add-on gun laws effective date and zero otherwise. Exceptions: Outcome in row 1 is gun robberies per 100,000 residents. Outcome in row 5 is log gun robberies. Coefficients for row 8 (trend breaks) are on post-add-on*relative time interaction.

Table A2: Triple Difference: Impact of Add-on Term

	Annual Reported Gun Robberies per 100,000 residents							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Within one year post add-on law effective date*add-on term	-0.04*	-0.03	-0.00	-0.07*	-0.03*	-0.03	-0.05	-0.05
	(0.02)	(0.02)	(0.03)	(0.03)	(0.01)	(0.01)	(0.02)	(0.03)
Within two years post add-on law effective date*add-on term	-0.04*	-0.03	0.01	-0.06	-0.03*	-0.02	-0.03	-0.04
	(0.02)	(0.02)	(0.03)	(0.03)	(0.01)	(0.01)	(0.03)	(0.04)
Within three years post add-on law effective date*add-on term	-0.04	-0.02	0.03	-0.05	-0.03*	-0.02	-0.02	-0.03
	(0.02)	(0.02)	(0.04)	(0.04)	(0.01)	(0.01)	(0.03)	(0.04)
State-specific time trends	n	y	n	y	n	y	n	y
Balanced Panel	n	n	y	y	n	n	y	y
Restrict to Post-1974	n	n	n	n	y	y	y	y
Observations	7839	7839	1944	1944	6542	6542	1520	1520
R-squared	0.19	0.19	0.24	0.25	0.18	0.19	0.22	0.23

* significant at 5%; ** significant at 1%

Note - The data consists of agency-year level observations. Standard errors reported in parentheses are clustered at the state level to allow for intra-state correlation in error structure. State and year effects are included in all specifications. Controls include mandatory minimum law dummy, poverty rate, unemployment rate, racial composition, age composition, lagged police population share, and lagged imprisoned population share.

Table A3: Lagged Dependant Variable Specification

	Annual Reported Gun Robberies per 100,000 residents			
	(1)	(2)	(3)	(4)
Within one year post add-on law effective date	-0.15	-0.05	-0.01	0.01
	(0.09)	(0.04)	(0.19)	(0.05)
Within two years post add-on law effective date	-0.19**	-0.10**	-0.13	-0.10*
	(0.06)	(0.03)	(0.13)	(0.04)
Within three years post add-on law effective date	-0.12	-0.08	-0.09	-0.06
	(0.06)	(0.04)	(0.11)	(0.06)
Balanced Panel	n	y	n	y
Restrict to Post-1974	n	n	y	y
Observations	1405	290	1107	185
Arellano-Bond test for AR(1)	0	.001	0	.016
Arellano-Bond test for AR(2)	.436	.979	.583	.629
Hansen over-ID test	1.00	1.00	1.00	1.00

* significant at 5%; ** significant at 1%

Note - The data consists of state-year level observations. Arellano-Bond dynamic panel estimates, one-step difference GMM results are reported. All available lagged differences of log gun robberies were used. The following were used as exogenous instruments: mandatory minimum law dummy, poverty rate, unemployment rate, racial composition, age composition, lagged police population share, and lagged imprisoned population share. Robust standard errors in parentheses. Hansen J statistic for overidentifying restrictions, and Arellano-Bond tests for autocorrelation are reported.

Table A4: Monte Carlo Standard Error Calculation

Panel A: One year Impact

	Monthly Reported Gun Robberies per 100,000 residents					Monthly Log Reported Gun Robberies per 100,000 residents				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Post Add-on Law										
Effective Date	-1.68	-1.61	-1.19	-0.59	-1.28	-0.20	-0.19	-0.16	-0.09	-0.07
Robust standard errors	(0.52)**	(0.57)	(0.53)*	(0.25)*	(0.60)*	(0.06)**	(0.06)**	(0.06)*	(0.03)*	(0.07)
Placebo standard errors	[1.00]	[1.02]	[0.99]	[0.83]	[0.79]*	[0.10]	[0.10]	[0.10]	[0.10]*	[0.10]
State Fixed Effects	n	n	n	y	n	n	n	n	y	n
Time Trend	n	y	y	n	n	n	y	y	n	n
Post-74 Dummy	n	n	y	n	n	n	n	y	n	n
Year Dummies	n	n	n	n	y	n	n	n	n	y
Observations	576	576	576	576	576	573	573	573	573	573
R-squared	0.48	0.51	0.63	0.88	0.74	0.51	0.53	0.59	0.88	0.64

Panel B: Two year Impact

	Monthly Reported Gun Robberies per 100,000 residents					Monthly Log Reported Gun Robberies per 100,000 residents				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Post Add-on Law										
Effective Date	-2.72**	-2.53**	-1.86*	-0.97*	-1.50	-0.34**	-0.31**	-0.24**	-0.17**	-0.11
Robust standard errors	(0.64)**	(0.68)**	(0.67)*	(0.35)*	(0.83)	(0.09)**	(0.08)**	(0.08)**	(0.04)**	(0.08)
Placebo standard errors	[1.03]	[1.12]	[1.07]	[0.86]	[0.97]*	[0.12]*	[0.13]*	[0.12]*	[0.10]*	[0.14]
State Fixed Effects	n	n	n	y	n	n	n	n	y	n
Time Trend	n	y	y	n	n	n	y	y	n	n
Post-74 Dummy	n	n	y	n	n	n	n	y	n	n
Year Dummies	n	n	n	n	y	n	n	n	n	y
Observations	1152	1152	1152	1152	1152	1147	1147	1147	1147	1147
R-squared	0.49	0.52	0.61	0.85	0.69	0.49	0.52	0.58	0.86	0.66

Panel C: Three year Impact

	Monthly Reported Gun Robberies per 100,000 residents					Monthly Log Reported Gun Robberies per 100,000 residents				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Post Add-on Law										
Effective Date	-3.59	-3.20	-2.24	-1.35	-1.21	-0.45	-0.39	-0.30	-0.25	-0.12
Robust standard errors	(0.80)**	(0.77)**	(0.79)**	(0.44)**	(0.94)	(0.11)**	(0.08)**	(0.10)**	(0.06)**	(0.10)
Placebo standard errors	[1.27]	[1.29]*	[1.24]	[0.96]	[1.17]	[0.13]	[0.14]**	[0.14]*	[0.10]*	[0.16]
State Fixed Effects	n	n	n	y	n	n	n	n	y	n
Time Trend	n	y	y	n	n	n	y	y	n	n
Post-74 Dummy	n	n	y	n	n	n	n	y	n	n
Year Dummies	n	n	n	n	y	n	n	n	n	y
Observations	1722	1722	1722	1722	1722	1716	1716	1716	1716	1716
R-squared	0.45	0.50	0.57	0.78	0.65	0.45	0.51	0.55	0.82	0.65

* significant at 5%; ** significant at 1%

Note - The data consists of a balanced panel of state-month level observations. Standard errors reported in parentheses are clustered at the state level to allow for intra-state correlation in error structure. Controls include poverty rate, unemployment rate, racial composition, age composition, lagged police population share, and lagged imprisoned population share. The calculation method for bootstrap standard errors in brackets is described in the text.