A Question of Life and Death: Analyzing the Short-Term Impact of Capital Punishment Executions on Violent Offenses

Kabir Jain

December 13th, 2020

Abstract

Capital punishment has long been a hotly debated policy in the United States and is primarily argued to have a deterrent effect on the willingness of people to commit violent crimes. Through the use of difference-in-differences estimation and regression discontinuity estimation, I find that executions carried out between 2000 and 2009 have no significant short-term effect on violent crime rates.

Keywords: Crime, Justice, Legal Institutions

JEL Codes: D63, K30, K42

Section I: Introduction

Capital punishment, or the ‘death penalty’, continues to be amongst the most controversial policy decisions governments across the world have had to make, and it brings up the age-old question of whether a justice system should act as an agent of retribution or rehabilitation. The effect of capital punishment on violent crime is puzzling: it defies easy explanation, and research has failed to provide evidence on any kind of continuous deterrent effect on violent crime. The intuition behind this policy is that upon recognizing the possibility of being sentenced to death, people will be less willing to commit the types of crime that might put them on death row.

Based on the fact that capital punishment is still practiced in 54 countries, and 28 U.S. States, one might expect that the deterrent effect of capital punishment would be a well-established and thoroughly researched phenomenon. Unfortunately, that is only partly true; although there exists a large body of literature on the effect of the death penalty on homicide rates in the U.S (over 100 published papers), the effects found vary significantly, and there has been no convincing evidence of capital punishment increasing, decreasing, or having no effect on homicide rates.

Pioneering psychological experiments in the 1970’s, led Albert Bandura, show that observing someone being punished for aggression deters observers from aggression (Bandura, 1977). There is also limited anecdotal evidence that there exist criminals who may have been deterred from more serious crimes by the possibility of being sentenced to death (Great Britain Royal Commission on Capital Punishment). Isaac Ehrlich, in the mid 1970’s, considered U.S. murder and execution statistics for the period 1933-69 and found that executions significantly decrease homicide rates (Ehrlich, 1975). Thereafter, a great number of studies about the deterrent effect of capital punishment have been published about America. To summarize these, the National Research Council conducted two reviews, once in 1978 and once more in 2012. The 1978 NRC review found that the deterrent effect was “not a settled matter” (National Research Council, 1978, pp.359), and that the existing literature did not provide sufficient evidence on any effect of capital punishment, be it upwards, downwards, or none at all. The 2012 NRC review echoed the first review, concluding that “research to date on the effect of capital punishment on homicide is not informative about whether capital punishment decreases, increases, or has no effect on homicide rates” (National Research Council, 2012, pp.2).

Other, more recent reviews of the question reveal pessimistic conclusions. In 2005, Berk claims that the deterrent effect is a “statistical artifact” of death penalties in a small number of “anomalous” jurisdictions, especially in Texas (Berk, 2005). Kovandzic et al. use annual U.S. state panel data from 1977 to 2006 and claim that their study “provide(s) no empirical support for the argument that the existence or application of the death penalty deters prospective offenders from committing homicide” (Kovandzic et al, 2009). Muramatsu et al. use monthly homicide data from Japan’s National Police Agency to examine whether Japan’s death penalty deters homicide or robbery-homicide and conclude that neither death sentences nor executions deter these behaviors (Muramatsu et al., 2018, pp 434).

So, while there exists a significant body of literature analyzing the long-term, or continuous, effect of capital punishment on violent crime, there is little focus on the short-term effects of the same. Furthermore, existing research has consistently failed to find a significant long-term effect. This paper seeks to fill that void by analyzing whether there exists any short-term effect of capital punishment executions on violent crime rates in American States. It is possible that there is a short-term effect, but that it gradually dies out as violent crime levels return to a higher baseline, which might be why a long-term effect does not exist.

The rest of this article proceeds as follows. Section II describes data sources, structures, and provides important baseline comparative statistics for this study. Section III introduces the empirical methodology used to test whether a deterrent effect visibly exists and strengthens the reasoning behind the use of the specific identification strategies. Section IV introduces and analyzes the results I get from the empirical methodology implemented in Section III, specifically that when we look at data at a daily level, three weeks before and after an execution, there is no pattern or evidence that consolidates the hypothesis that the death penalty has an effect on violent crime that is statistically distinguishable from zero. Finally, Section V summarizes the findings of the paper, discusses their significance, and makes recommendations for future studies. Of course, further research is needed, but at present, there is no convincing evidence to continue to claim that executing criminals serves the purpose of deterring others from committing violent crime.

Section II: Data

I obtained state-level crime data from the National Incident-Based Reporting System (NIBRS) for the years 2000 through 2009. NIBRS data include detailed information on each reported crime, including the date and time of occurrence and the type of committed offense. Detailed data on executions carried out between 2000 and 2009 were obtained from the Death Penalty Information Center. This data includes the State within which the subject was executed, the date of the execution, and the method of execution.

Our primary focus is on whether an execution on a particular date leads to lower violent crime levels in the state for up to three weeks after the date of execution. As a result, the above data were utilized to create a new dataset where the observational unit is offenses by type of crime by state. For each execution we know of, there is such data three weeks before and three weeks after the date of execution.

Though NIBRS reporting has gradually expanded over time, there are some States that did not report the required data during our time period. Executions, too, limit the States we can study, since capital punishment was abolished before 2000 in many American States, and, as mentioned before, some that had not yet abolished it were not actively executing criminals. After accounting for both of these data limitations, I was left with 16 States that could be studied, namely: Alabama, Arizona, Arkansas, Connecticut, Delaware, Georgia, Kentucky, Missouri, Montana, Ohio, Oklahoma, South Carolina, South Dakota, Tennessee, Texas, and Virginia. Table 1 tabulates summary statistics to convey a sense of what the data look like.

Table 1: Pre and Post-Execution Comparison of Nonviolent and Violent Crimes in U.S. States, 2000-2010

	NonViolent_Pre mean	Nonviolent_Post mean	Violent_Pre mean	Violent_Post mean
Alabama	10.677	11.319	2.527	2.586
Arizona	21.636	19.286	3.227	3.667
Arkansas	120.127	126.158	32.464	30.132
Connecticut	200.545	184.905	46.636	43.000
Delaware	191.621	178.111	59.197	59.413
Georgia	3.141	3.072	0.766	0.818
Kentucky	37.409	37.476	7.909	7.429
Missouri	3.545	3.857	1.545	1.143
Montana	150.136	138.905	28.318	29.429
Ohio	1035.270	1032.411	323.477	317.909
Oklahoma	57.900	56.371	16.500	17.295
South Carolina	894.667	887.669	292.409	288.333
South Dakota	75.591	77.714	18.273	17.048
Tennessee	1134.455	1133.571	392.121	393.706
Texas	488.359	483.656	112.573	112.180
Virginia	1055.142	1065.156	271.803	272.173
Total	568.414	565.057	147.349	146.339

Table 1

This table shows summarizing statistics for our data, separated by time period (columns 1 and 3 represent pre-treatment and columns 2 and 4 represent post treatment, States (rows), and Type of Offense (columns 1 and 2 represent Non-Violent Offenses whereas columns 3 and 4 represent Violent Offenses.

Section III: Empirical Methodology

The relevant policy question here is the effect of an execution on violent criminal activity in the weeks thereafter. The first method of studying this is by comparing trends between non-violent and violent offenses over six weeks, with an execution that takes place at the central point of the three-week mark. The second method of studying this is by analyzing the same effect on violent offenses and non-violent offenses separately and seeing whether there are comparable, if any, discrete breaks in the pattern exhibited by our data on daily criminal activity right after an execution takes place.

A. Difference-in-Differences Model

We begin with a difference-in-differences (DiD) design that utilizes the daily variation in violent offenses and in non-violent offenses. For this specification, the relevant regression is:

$$Y = \beta_0 + \beta_1(Post * Treat) + \alpha_{fe} + \epsilon$$

Where Y refers to the log of offenses per 1,000 population, Post refers to time period, which is 0 before an execution and 1 thereafter, and Treat is 0 for nonviolent offenses and 1 for violent offenses. The idea behind having nonviolent offenses as the control or reference group is that, if a deterrent effect exists, it is highly likely to be much more pronounced on violent offenses than on nonviolent offenses, simply because capital punishment is meted out only for especially heinous violent offenses and never for nonviolent offenses. Treat*Post is our explanatory variable of interest, varying between 1 for violent offenses after the execution, and 0 for all other observations. Included, as $\boldsymbol{\alpha_{fe}}$, are a host of fixed effects: month fixed effects to control for seasonal variation in criminal behavior, state fixed effects to control for static differences across states, year fixed effects to account for general time trends, execution-level fixed effects to control for different factors across the 368 executions (publicization, method), and finally, day-of-week interacted with treat fixed effects to absorb differences in crime levels that vary across our days of the week, separately for violent and non-violent crimes. Day-of-week fixed effects are important here because, upon a closer look at the data, I found that executions only happen on weekdays, and most often between Tuesday and Friday. Furthermore, there is likely to be a difference in criminal behavior on weekdays vs. on weekends. Table 2 presents evidence of the existence of different patterns of crime on Fridays, Saturdays, and Sundays.

Table 2: Mean of Violent Offenses by Day of Week

Day of Week	Mean Violent Offenses
Friday	150.3953
Monday	137.7877
Saturday	164.2236
Sunday	165.1727
Thursday	137.2765
Tuesday	137.6521
Wednesday	136.9618
Total	146.8555

Table 2
This table shows the mean number of violent offenses committed on each day of the week in our dataset. While not extremely different, Friday, Saturday, and Sunday have sufficient difference from the rest of the week that justify the inclusion of day-of-week fixed effects to account for this. Later in this paper, I also go over what happens we exclude these day-of-week fixed effects.

B. Regression Discontinuity

We move on to a regression discontinuity (RD) design, where the running variable is days relative to the execution (-21 to +21), 0 being the date of execution. I run two regression discontinuity models with the same specification, once with violent offenses as the outcome, and once with non-violent offenses as the outcome. The relevant regression here is:

$$Y = \beta_0 + \beta_1(Treat) + \beta_2(L\_running) + \beta_3(R\_running) + \alpha_{fe} + \epsilon$$

Where Y is once again our outcome variable, which is the log of offenses per 1,000 population, Treat is a binary variable, with 0 indicating an observation before the date of execution, and 1 indicating an observation after the date of execution. The variables L_running and R_running indicate the relative days to execution, with the former indicating those days before the execution and the latter, those after. By including these, we allow for varied linear slopes on either side of our threshold (day of execution).

Due to the use of a unit of time as our running variable, it is possible that some assumptions of the regression discontinuity identification strategy may be violated. Executions don't ever happen on weekends, and Saturdays and Sundays have different crime patterns than other days. As a potential adjustment, I include day-of-week fixed effects.

Section IV: Results

A. Difference-in-Differences

Table 3 below presents the main findings from various specifications of the difference-indifferences model.

Table 3: Comparison of DiD Specifications

	Base	Model 1	Model 2	Model 3
Post * Treat	-0.831***	-0.00281	-0.00438	-0.0173
	(0.0181)	(0.0204)	(0.00986)	(0.0113)
Day of Week x Treat F.E	No	Yes	Yes	Yes
Execution F.E	No	No	Yes	Yes
Month and Year F.E.	No	No	No	Yes

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3
This table presents results from our DiD estimation strategy, with the columns going from left to right in order of added fixed effects. Values shown here are regression coefficients from the model outlined in the Empirical Strategy section and represent the effect of our explanatory variables on the log of Offense rates.

Table 3 shows us the effect of our (Post*Treat) estimate on the offense rate, with a plethora of fixed effects to control confounding sources of variation. Model 3 from Table 3 is the preferred specification here as it includes all important fixed effects. Model 3 indicates that when an execution takes place within a State, daily violent offense rates for the next three weeks fall by approximately 1.7 percentage points, which, due to a lack of statistical significance, is statistically indistinguishable from zero.

A graphical representation of the differential effect of executions on violent offenses and nonviolent offenses can be seen below, in Figure 1, which was created using the following regression equation:

$$Y = \sum_{n=-21}^{21} (Treat * RelativeDay_n) + \alpha_{fe}$$

Where Y is once again the log of offense rate per 1,000 population, and RelativeDay refers to the days since execution, which range from -21 (three weeks before) to 0 (day of execution) to 21 (three weeks after).

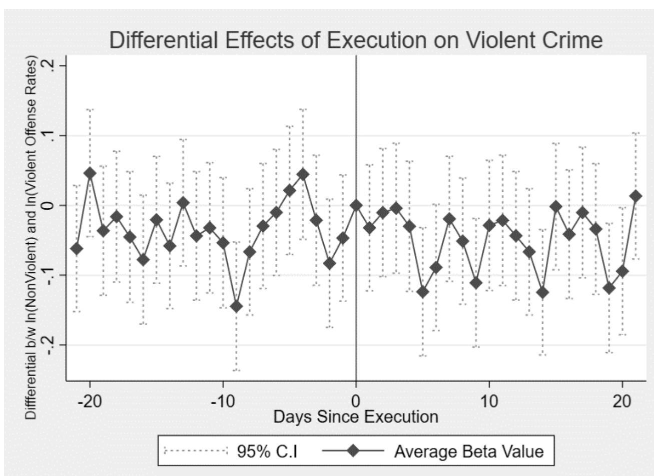

Figure 1
This figure plots difference in offense rates for violent offenses compared to non-violent offenses. The solid line indicates regression coefficients on the offense rate, and the dashed lines represent the 95% confidence intervals for each of these point estimates.

Figure 1 corroborates the story told by our regression results from Table 3; as we cross the treatment threshold (day of execution, x = 0), there is no discernible change in the violent offense rates observed. Both Table 3 and Figure 1 together indicate that there is no evidence of the existence of a statistically significant deterrent effect at the daily, State level.

B. Regression Discontinuity

The regression discontinuity results are tabulated below, in Table 4.

Table 4: Regression Coefficients for the Regression Discontinuity Model.

	Log(Non-Violent Offense Rate)	Log(Violent Offense Rate)
Treat	-0.00189	-0.0297
	(0.0401)	(0.0427)
L_Running	-0.000372	0.00165
	(0.00219)	(0.00233)
R_Running	-0.000228	0.000871
	(0.00235)	(0.00251)

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4
This table tabulates the regression coefficients obtained from running the model outlined in Part B of the Empirical Methodology section. These coefficients measure the effect of the corresponding explanatory variable on the log of Non-Violent offense rate (column 1) and the log of Violent Offense Rate (column 2).

Results from Table 4's first row indicate that an execution in a state has an effect on the nonviolent offense rate of reducing it by 0.189 percentage points, and on the violent offense rate of reducing it by 2.97 percentage points, both of which are statistically indistinguishable from zero, due to a lack of statistical significance. The values of L_Running and R_Running indicate the linear trend in the offense rates before and after the treatment respectively and can be seen in

Figures 2 and 3 below, of which Figure 2 represents the treatment effect for nonviolent offense rate and Figure 3 represents the same for the violent offense rate.

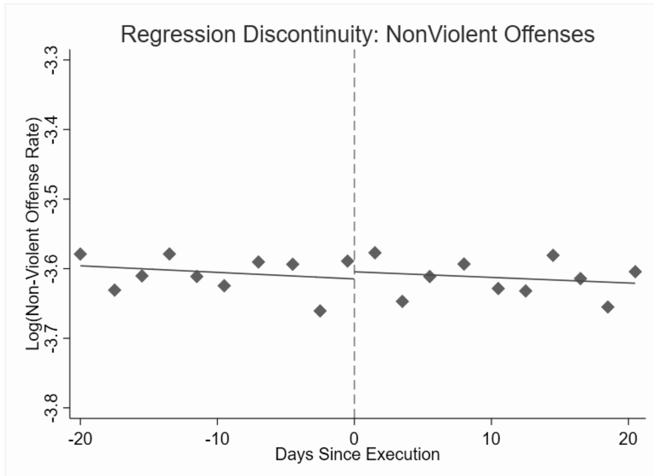

Figure 2

This figure represents the change in the log of Non-Violent Offense rate upon crossing the threshold of treatment (day of execution). On the Y-axis we have the log of non-violent offense rate, and, on the X-axis, we have the running variable – days since execution.

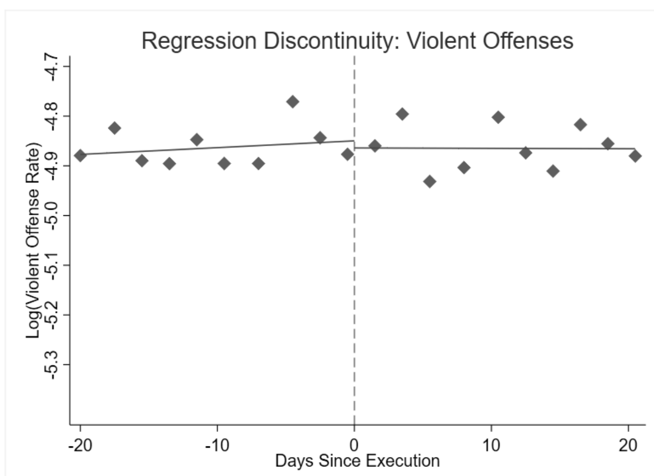

Figure 3

This figure represents the change in the log of Violent Offense rate upon crossing the threshold of treatment (day of execution). On the Y-axis we have the log of violent offense rate, and, on the X-axis, we have the running variable – days since execution.

Column 1 from Table 4 and Figure 2 together represent my analysis of the effect on non-violent offenses. It is clear that there is no significant discrete change in the log of non-violent offense rates as we move through time from the period before an execution to after it. Furthermore, the linear trends in the same statistic remain largely unchanged before and after an execution.

Column 2 from Table 4 and Figure 3 present my analysis of the effect on violent offenses. For violent offenses, too, there is no statistically significant discrete change in the log rate as we cross the threshold of treatment. There is also no change in linear trends upon crossing the same threshold.

Overall, the regression discontinuity results, too, fail to provide any evidence of the existence of a deterrent effect of capital punishment executions on violent offenses committed in the next three weeks.

Robustness Check on Regression Discontinuity

As a robustness check for my regression discontinuity model, I reconstructed my dataset to include six weeks before and six weeks after every date of execution, as opposed to the original bandwidth of three weeks on either side. With this adjusted bandwidth, I re-run my regression discontinuity model. The robustness check is presented below in Table 5.

Table 5 shows us that changing the bandwidth for my regression discontinuity model does not change much – the discrete effect of crossing the threshold of treatment remains extremely miniscule and maintains the same direction as earlier. It also remains statistically insignificant.

Table 5: Robustness Check: Regression Coefficients for the Regression Discontinuity Model.

	Log(Non-Violent Offense Rate)	Log(Violent Offense Rate)
Treat	-0.00360	-0.00398
	(0.0282)	(0.0301)
L_running	0.000226	0.000719
	(0.000797)	(0.000851)
R_running	-0.000267	-0.000736
	(0.000827)	(0.000883)

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Section V: Discussion and Conclusion

I present novel research on the short-term effect of capital punishment on violent offenses in the United States and find no evidence of the existence of a statistically significant deterrent effect. I analyzed this question using violent and non-violent offense rates from three weeks before and three weeks after 368 distinct executions that took place in 16 States across the country, between 2000 and 2009. I used two identification strategies; difference-in-differences estimation and regression discontinuity. Both fail to provide any evidence pointing towards the existence of any sort of deterrent effect.

A. Limitations of the Study

Of course, there are limitations to this study that undercut the results. One such limitation is the fact that I use state-level data. Although our data is granular at the time level (daily data), our data isn't granular at the geographic level, which is a drawback for accurately measuring the deterrent effect. It is possible that the deterrent effect is more pronounced in the county within which the execution takes place. Considering this, State level data may cause our analysis to average out the deterrent effects and claim there is none when there actually Is, but only in and around the county of execution. Another limitation of this study is the fact that my response variable is all violent offenses as opposed to homicides. Homicides are the type of violent offense that are most likely to receive the death penalty, and therefore we may be not seeing a deterrent effect on violent offenses as a whole when such an effect may exist on homicides. This limitation, however, is not that serious. Past research, while it has not focused on short-term effects as my paper does, does indeed focus on homicide deterrence, and consistently fails to find any such deterrent effect.

Another significant source of variation could be isolated if publicity data could be collected and incorporated. Executions of more infamous criminals undoubtedly receive greater publicity, which leads to more people knowing about it, and therefore expands the channel through which a deterrent effect might work. The opposite is also true: executions that are not widely publicized would be less likely to cause a deterrent effect, if it exists. Unfortunately, data on publicity of individual executions was not accessible by me as I could not find any reliable source for the same.

B. Policy Suggestions

The policy implications of this research are similar to those proposed by most of the hundred papers done before this on the same topic – the death penalty has no distinguishable deterrent effect on violent offenses, and if that is the main argument that keeps it instituted, the policy should be revoked. The revocation or abolition of capital punishment stands to benefit those who are wrongfully convicted. Even proponents of the system cannot claim that the system is perfect. It is, after all, "administered through men, and therefore may occasionally disclose the frailties of men. Perfection may not be demanded of law, but the capacity to correct errors of inevitable frailty is the mark of a civilized legal mechanism" (Justice Felix Frankfurter, 1962). Those who stand to face harm at the prospect of abolition of the death penalty do not exist, according to evidence from this paper and the many before it – there is no statistically significant reduction in violent crime after an execution.

The National Research Council, however, continues to reject all relevant findings, contemporary and historical, that pertain to the deterrent effect that we have discussed at length. The policy suggestion that this paper, along with the many before it would be to place the policy on a moratorium, as has been done in Pennsylvania, Oregon, and California, until more convincing evidence arises before the National Research Council.

References

Bandura, A. 1977. Social Learning Theory. Englewood Cliffs, N.J.: Prentice-Hall

Great Britain Royal Commission on Capital Punishment. 1953. Report. London: Her Majesty's Stationery Office.

Ehrlich, I. 1975a. "Deterrence: Evidence and Inference." Yale Law Journal 85:209-27. . 1975b. "The Deterrent Effect of Capital Punishment: A Question of Life and Death."

National Research Council (1978) Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates. Panel on Research on Deterrent and Muramatsu et al. 455 Incapacitative Effects, edited by Blumstein A, Cohen J, and Nagin DS. Washington, DC: The National Academies Press

National Research Council (2012) Deterrence and the Death Penalty. Committee on Deterrence and the Death Penalty, edited by Nagin DS and Pepper JV. Washington, DC: The National Academies Press

Berk R (2005) New claims about executions and general deterrence: De´ja` Vu all over again? Journal of Empirical Legal Studies

Kovandzic TV, Vieraitis LM and Boots DP (2009) Does the death penalty save lives? Criminology & Public Policy 8(4): 803–843.

Muramatsu K, Johnson DT, Yano K. The death penalty and homicide deterrence in Japan. *Punishment & Society*. 2018;20(4):432-457. doi:10.1177/1462474517706369