Does Capital Punishment have a "Local" Deterrent Effect on Homicides?

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The vast majority of death penalty studies use geographically or temporally aggregated data. Such aggregation can make it virtually impossible to identify small amounts of variation in homicides due to executions. Therefore, this study uses data that are disaggregated down to daily and city levels to test whether executions have a short-term deterrent effect. Little consistent evidence is found that Texas executions deter Dallas, San Antonio, and Houston homicides from 1999 to 2004. The analysis also does not consistently support the hypotheses that the deterrent effect should be more evident for local executions or executions that received local media coverage. (*JEL* K14, K42)

1. Introduction

Whether or not the death penalty has a deterrent effect is a hotly debated topic in the popular press, political forums, and academic literature. High-profile executions, such as that of the Crips gang cofounder and Nobel Peace Prize nominee Tookie Williams on December 13, 2005, bring the death penalty and its potential deterrent effects to the forefront in the national media. Political debate is active as a number of states consider

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moratoria on executions. The academic debate took off in the 1970s with both Ehrlich's (1975) claim that each execution averted eight murders and the National Academy of Sciences' (Blumstein, Cohen, and Nagin, 1978) critical response. For the next twenty years, researchers attempted to replicate Ehrlich's findings using different data sets, methodologies, and sample periods, yielding mixed and often contradictory findings.[1]

A relatively recent spate of the literature, however, purports to find evidence of a deterrent effect.[2] However, Berk (2005) shows that many of these findings of deterrence are driven by just a few states and years, which have more than five executions. Similarly, Donohue and Wolfers (2006) demonstrate that such evidence of deterrence is not robust and is especially sensitive to specification characteristics, such as sample period.[3] In addition, Donohue and Wolfers conclude that "the death penalty . . . is applied so rarely that the number of homicides it can plausibly have caused or deterred cannot be reliably disentangled from the large year-to-year changes in the homicide rate caused by other factors."

In reading Donohue and Wolfers' extensive analysis and critique of the literature, it becomes apparent that a variety of methodologies have been used to test the deterrence hypothesis, including time series, panel data, and instrumental variable approaches. However, virtually all of these methods generally have one thing in common: they use aggregated data. Time series studies typically use annually aggregated national data.[4] Data are often annually aggregated to the state level in panel data and instrumental variable

1. Cameron (1994) provides a review of much of the post-Ehrlich debate and literature.

2. According to Fagan (2006), claims of a strong deterrent effect have been made in more than a dozen studies published in the past decade. See, for example, Mocan and Gittings (2003); Mocan and Gittings (2006); Shepherd (2004); Liu (2004); Dezhbakhsh, Rubin, and Shepherd (2003); Zimmerman (2004); Cloninger and Marchesini (2001); and Cloninger and Marchesini (2005).

3. Cohen-Cole *et al.* (2007) attempt to adjudicate the disparate results found across much of the literature by explicitly taking into account the presence of model uncertainty. In particular, they focus on the results presented by Dezhbakhsh, Rubin, and Shepherd (2003), which were reexamined by Donohue and Wolfers (2006). Cohen-Cole *et al.* (2007) conclude that there is some evidence of a deterrence effect, but that it is relatively weak.

4. For example, Ehrlich (1975); Passell and Taylor (1977), and Cover and Thistle (1988) use US time-series data while Avio (1979) uses Canadian data.

analyses.[5] One of the key contributions of this paper, therefore, is to test for a short-term deterrence effect with geographically and temporally disaggregated data. Specifically, I use daily homicide and execution data from Houston, Dallas, and San Antonio.

There are just a few studies, especially in the economics literature, which use disaggregated data. While the criminology literature tends to use more disaggregated data than economists, there are still few papers that use daily- or city-level data and I am unaware of any studies that use data disaggregated at both the daily and city levels.[6] Cloninger and Marchesini (2001, 2005) find evidence consistent with deterrence using monthly data and event study techniques to test for deterrence surrounding an eighteen-month stay on Texas executions and a moratorium on Illinois executions. However, Donohue and Wolfers (2006) show that this evidence disappears when using the homicide level rather than the homicide growth rate in the analysis. Grogger (1990) analyzes daily homicide counts in California, finding no support for a short-term deterrence hypothesis. Similarly, Stolzenberg and D'Alessio (2004) use monthly data from Houston and find that both the frequency of execution and its surrounding newspaper publicity have no discernible deterrent effect. In contrast, Phillips (1980) finds evidence of deterrence using weekly London homicide data from 1858 to 1921. However, given the location and time period, this is unlikely to be generalizable to the United States today. For instance, until 1863, London executions were not only publicized but they were also public events.[7,8]

There are a number of potential advantages of using homicide and execution data that are disaggregated down to daily and city levels. Primarily,

5. Katz, Levitt, and Shustorovich (2003) and Mocan and Gittings (2003) use panel data methods. Dezhbakhsh, Rubin, and Shepherd (2003) use instrumental variables and data disaggregated to the county level, though the execution data is still aggregated to the state level. Zimmerman (2004) uses both panel data and instrumental variable methods.

6. Chan and Oxley (2004) describe seventy-four death penalty papers published between 1952 and 2003; only three of these papers use city-level data.

7. More information about historical executions in London can be found at www.oldbaileyonline.org.

8. Other criminology papers that use relatively disaggregated data include (i) Cochran, Chamlin and Seth (1994), who use weekly data from Oklahoma; (ii) Cochran and Chamlin (2000), who use weekly data from California; and (iii) Sorenson *et al.* (1999), who use monthly data from Texas.

variation in the homicide rate due to executions may be too small to identify when geographically and temporally aggregating homicide data. For instance, it may be the case that homicide variation only occurs on the days immediately surrounding an execution. Alternatively, intertemporal substitution could occur, so homicide rates are deterred in the short-term but increase in the longer-term. Neither of these scenarios could be observed upon aggregating the data. Likewise, changes in the homicide rate may only occur in cities where the executed offender is convicted or where the execution is publicized; again, aggregating across all cities could hide these potentially small variations.

In this study, daily homicide data from the Dallas, San Antonio, and Houston police departments were merged with daily execution data as well as data describing the coverage of each execution in three city newspapers (*Dallas Morning News* (DMN), *Houston Chronicle* (HC), and *San Antonio Express-News* (SAE)) and on an NBC affiliate news station in Dallas. Analysis of the news data indicates two interesting phenomena. First, both city newspapers and television news stations are most likely to cover executions that were sentenced in their own counties. Second, a discontinuity exists in how the DMN covers Texas executions that coincides with a change in management; prior to June 2001, ninety-eight percent of executions are covered while just thirty-four percent are covered after June 2001. These data sets allow me to test a number of hypotheses implied by the assumptions underlying the economic model of crime, including whether there is evidence of deterrence in the days immediately surrounding an execution and whether more evidence exists for executions with the maximum media coverage or that are locally sentenced and convicted.

Estimating Poisson regression models of daily homicide counts on execution leads and lags, I find little consistent evidence that Texas executions have a short-term deterrence effect on homicides. Some evidence of deterrence is seen when considering the impact of executions on Houston capital murders, i.e., those crimes actually eligible for the death penalty; however, this effect is inconsistent in that it is only observed when considering all executions and not when focusing on local or locally covered executions. In addition, the results do not support the hypothesis that the deterrent effect should be more evident for local executions or for executions covered by the local media. Overall, there appears to be minimal evidence that a potential offender's fear of executions increases in a way that decreases homicides when an additional execution occurs.

2. Criteria for the Death Penalty to Have a Deterrent Effect

Becker's (1968) economic model of crime was extended to the death penalty and homicide by Ehrlich (1975). The underlying premise is that an increase in the probability of receiving a death sentence or being executed will increase the expected price of murder, and in theory, reduce the demand for murder or the number of homicides. However, a number of assumptions must be satisfied for this argument to hold. I will discuss each of these criteria in turn, often in the context of the Texas criminal justice system.

First, potential offenders must consider execution to be a more severe punishment than life imprisonment. On the one hand, individuals fight for years through the appeals process to get off death row; the average amount of time on death row in Texas from 1999 to 2004 is approximately eleven years. In addition, until 2005, potential punishments for capital murderers in Texas only included a death sentence and life imprisonment *with* the possibility of parole.[9] Thus, it is certainly plausible that execution is worse than life imprisonment in Texas. On the other hand, Katz, Levitt, and Shustorovich (2003) argue that life imprisonment could be worse given the extremely poor prison conditions and quality of life.[10]

Second, both Donohue and Wolfers (2006) and Katz, Levitt, and Shustorovich (2003) argue that the probabilities of receiving a death sentence and being executed are so low that they may only have a negligible effect on the homicide rate. For instance, Donohue and Wolfers (2006) state that only 8.7 death sentences were handed down in 2003 per thousand homicides. In addition, just 1.9 percent of the 3374 individuals on death row at the beginning of the year were executed. In Texas, however, there were 15.2 death sentences per thousand homicides from 1974 to 1995 and fifteen percent of the death sentences from this period were carried out. Thus, while concern

9. This is true of just one of the thirty-seven other death penalty states. However, in 2005, a bill passed giving Texas jurors the ability to sentence capital offenders to life without parole.

10. Katz, Levitt, and Shustorovich (2003) actually find a negative correlation between crime rates and the death rate among prisoners, a proxy for prison conditions.

is valid over whether the probability of execution is sufficiently large to have an effect, this criterion is more likely to hold in Texas than nationally.

Lastly, for the death penalty to have a deterrent effect, potential offenders must be aware of the chances of being executed. Of course, individuals do not know the objective probability of being executed; rather, an individual will be deterred when an event increases his perceived probability of execution. What kind of event would have such an impact on an individual's beliefs? One possibility is a change in the laws, e.g., making more crimes eligible for the death penalty or placing a moratorium on executions. This paper considers the event of an execution itself. An individual may update his beliefs about the death penalty on days when an execution occurs. One could argue that a potential offender living in Texas has no reason to update his beliefs because he already believes the probability of execution to be relatively high. However, this phenomenon is observed in other contexts. For example, Redelmeier, Tibshirani, and Evans (2003) find that Canadians who receive a ticket for speeding are thirty-five percent less likely to be involved in a fatal crash in the following month than in a month not preceded by a ticket. This effect was dampened out within two months from the time of receiving the ticket and eliminated within 3–4 months.[11] Thus, it is not out of the question, at least in the short term, that potential offenders are more fearful of the death penalty when an execution occurs.

It must also be the case that the individual actually knows that an execution occurred. Thus, if an execution is not publicized and the potential offender does not have personal knowledge of the executed individual, it seems highly unlikely that the execution can have a deterrent effect. An analysis of media coverage, discussed later in the paper, indicates that the strongest predictor of coverage is the location (i.e., county) of the conviction.

The above discussion illustrates a number of hypotheses that can be directly tested in the daily city-level data. First, evidence of deterrence, if any, should be seen on the days immediately surrounding the execution. Second, deterrent effects are greater for executions with the most media coverage and for local executions. Findings consistent with these hypotheses would provide evidence that an additional execution raises a potential offender's fear of execution.

11. I wish to thank John Donohue for raising this point.

3. Description of the Data

3.1. Homicide Data

Incident-level crime data were obtained through Open Records Requests to three Texas city police departments: Dallas, San Antonio, and Houston. The Houston data contain all incidents between January 1, 1999 and December 31, 2004; San Antonio and Dallas data were available through June and September of 2004, respectively. For each city, I can identify each murder incident and the date on which it occurred. In Dallas, it is also possible to determine a number of characteristics of the offense, such as the race of the victim. Most notably in Houston, I can separate out capital murders, i.e., those murders that are eligible for the death penalty.[12] If an offender is truly rational, then one's subjective probability of execution should only affect his decision to commit a crime eligible for that punishment. Yet, most death penalty researches consider all homicides; a recent, notable exception is Fagan, Zimring, and Geller (2006).

Table 1 displays the annual number of homicides in each city; over the six-year period, there were 1289 homicides in Dallas, five hundred in San Antonio, and 1552 in Houston. More than twenty percent of the Houston homicides can be classified as capital murders. Over this period, there has been a slight increasing trend in homicide levels in both Dallas and Houston and a fairly constant level in San Antonio. The last column of table 1 presents statistics on the number of murders throughout Texas; homicides

12. In Texas, a capital murder can be broadly characterized as one in which (i) the victim is a peace officer or fire fighter acting in his official capacity; (ii) the murder is committed intentionally during an aggravated sexual assault, arson, burglary, kidnapping, obstruction, retaliation, or robbery; (iii) the murder is for pay; (iv) the offender paid another person to commit murder; (v) murder occurred while escaping from prison; (vi) murder occurred while incarcerated; (vii) there were multiple victims; and (viii) the victim was a child under the age of six. Of course, it is important to recognize that the data only indicate those homicides that can be classified as capital cases according to this statutory definition. It does not identify those capital eligible cases that will not be prosecuted as such. For instance, the prosecutor may weigh mitigating and aggravating circumstances as well as the track record of the defense attorney when evaluating his chances of winning a capital conviction or even use the eligibility of a case for capital punishment as a bargaining chip when negotiating a plea. In addition, not all of these cases will even be cleared by an arrest. Though not all of these capital eligible cases will actually be tried as such, this does not change the fact that these are the only subset of homicides in Houston that are actually eligible for capital punishment.

Table 1. Annual City-Level Homicides: 1999–2004

	Dallas	Houston–All	Houston–Capital Murder	San Antonio	All Texas Murders
1999	184	242	49	94	1218
2000	227	227	39	82	1236
2001	223	266	61	99	1331
2002	235	261	53	99	1305
2003	233	277	67	85	1417
2004	187[a]	279	51	41[a]	1359
Total	1289	1552	320	500	7866

[a]San Antonio data were only available through June 2004 at the time it was collected and Dallas through September 2004. In Texas, a capital murder can be broadly characterized as one in which (i) the victim is a peace officer or fire fighter acting in his official capacity; (ii) the murder is committed intentionally during an aggravated sexual assault, arson, burglary, kidnapping, obstruction, retaliation, or robbery; (iii) the murder is for pay; (iv) the offender paid another to commit murder; (v) murder occurred while escaping from prison; (vi) murder occurred while incarcerated; (vii) there were multiple victims; and (viii) the victim was a child under the age of six.

in Dallas, Houston, and San Antonio account for more than forty percent of the statewide homicides. Overall, homicides increased by twelve percent in Texas from 1999 to 2004. In Dallas, almost sixty percent of the days have zero homicides and close to thirty percent have one. Approximately eighty percent of the days in San Antonio have zero homicides. In Houston, about thirty and ten percent of the days, respectively, have one and two homicides.

3.2. Execution Data

There were one hundred and seventy-two executions in Texas between January 1, 1999 and December 31, 2004. Information about the execution, offender, and victim were collected from the Texas Department of Criminal Justice's website.[13] There is a fair amount of annual variation in the number of Texas executions, ranging from forty in 2000 to seventeen in 2001. In addition, almost sixty percent of the weeks from 1999 to 2004 have zero executions while close to thirty percent have one execution.

Table 2 provides summary statistics of the execution data. Executions only occur on weekdays, and primarily on Tuesday through Thursday. In addition, ten percent of the executed offenders were convicted in Dallas, thirteen percent in Harris County (which contains Houston), and five percent in Bexar County (which contains San Antonio). In terms of the offender

13. See http://www.tdcj.state.tx.us/stat/executedoffenders.htm for a list of all executed offenders and links to offender information.

Table 2. Descriptive Statistics: Execution, Offender, and Victim Characteristics

Variable	Observations	Mean	Standard Deviation
Execution Characteristics			
Execution on Monday	172	0.023	0.15
Execution on Tuesday	172	0.28	0.45
Execution on Wednesday	172	0.42	0.50
Execution on Thursday	172	0.25	0.43
Execution on Friday	172	0.017	0.13
Execution on Saturday	172	0	0
Execution on Sunday	172	0	0
Sentenced in Dallas	172	0.10	0.31
Sentenced in Houston	172	0.13	0.34
Sentenced in San Antonio	172	0.05	0.21
December or January	172	0.23	0.42
Offender Characteristics			
Age at execution	172	40.12	9.56
Age at offense	171	27.94	8.77
Years on death row	170	11.11	4.73
Number of codefendants	167	0.61	0.87
18 or younger at offense	171	0.058	0.24
60 or older at execution	172	0.047	0.21
Black	172	0.37	0.48
Hispanic	172	0.14	0.35
White	172	0.49	0.50
From Texas	169	0.64	0.48
Dallas native	164	0.067	0.25
Houston native	164	0.13	0.34
San Antonio native	164	0.037	0.19
GED	165	0.17	0.38
Education $\leq$ 9 years	165	0.25	0.44
Education $\geq$ 12 or GED	165	0.47	0.50
Some college	165	0.097	0.30
Past prison record	168	0.51	0.50
Victim Characteristics			
In law enforcement	171	0.053	0.22
12 years old or younger	165	0.13	0.33
18 years old or younger	159	0.22	0.42
Related to victim	171	0.076	0.27
At least one victim Black	142	0.11	0.31
At least one victim White	142	0.77	0.42
At least one victim Hispanic	142	0.13	0.34
At least one victim male	170	0.56	0.50
Number of victims	171	1.40	0.84

characteristics, table 2 indicates that the average age at the time of the offense is about twenty-eight years and that about six percent of the offenders were eighteen or younger. Five percent were sixty or older at the time of execution and thirty-seven percent of the executed offenders were Black; fourteen percent were Hispanic and forty-nine percent were White. Only sixty-four percent of the individuals were born in Texas; the remaining executed offenders were born in twenty-seven states and five countries besides Texas and the United States, respectively. Just forty-seven percent of the executed offenders had either received a GED or finished twelve or more years of school. About half of the sample had been incarcerated in the past. Information regarding the victims is also available. Approximately five percent of the victims were in law enforcement, thirteen percent were twelve years old or younger, and eight percent of offenders were related to at least one of their victims. Eleven percent of the offenders had killed at least one Black individual while seventy-seven and thirteen percent, respectively, had killed at least one White and at least one Hispanic individual. On average, there were 1.4 victims per executed offender.

3.3. Print and Television Media Coverage of Executions

Potential offenders must be aware of an execution in order to update their beliefs regarding the probability of being executed. There are a number of potential channels through which such information can be dispersed, including television coverage, newspaper coverage, and social networks. Newspaper data are most accessible, and therefore I focus my attention on three local daily newspapers: DMN, HC, and SAE.[14] I searched for news stories on each of the one hundred and seventy-two executions in all three newspapers and recorded the total number of stories about the execution as well as the dates of the first and last articles. The first thing to note from an analysis of the newspaper data is that many of the executions were covered prior to the execution date. For instance, this is true of more than half of the executions covered in the HC and more than a third of those covered in the DMN. Thus, this indicates that it is actually possible for there to be a deterrence effect on the days leading up to an execution.

14. The *San Antonio Express-News* is the major newspaper in Central and South Texas with circulation spreading from Austin to the Mexico border. The *Houston Chronicle* has the largest circulation of any newspaper in Texas. Searches of the *San Antonio Express-News* and the *Houston Chronicle* were conducted through Lexis-Nexis while searches of the *Dallas Morning News* were conducted through the paper's own website and archives.

In addition, I find that overall execution coverage varies across city papers. While the DMN reported on sixty-five percent of the executions from 1999 to 2004, the HC and SAE covered just thirty-five and thirty percent of executions, respectively. However, the DMN covered eighty-three percent of executed offenders who were sentenced in or native to Dallas; similarly, the HC and SAE covered seventy-six and one hundred percent of the Houston- and San Antonio-related executions, respectively. Regression analyses also emphasize the importance of the sentencing locale. For instance, a Dallas execution is thirty-one percent more likely to be covered in DMN than a non-Dallas execution. Few other offender and victim characteristics significantly determine coverage.

Figure 1 plots the proportion of monthly and annual executions covered in each of the city papers. Though the proportion of executions covered each month in Houston and San Antonio are fairly noisy, aggregating annually indicates an increasing trend in coverage in Houston and the reverse in San Antonio. In Dallas, however, a discontinuity in the coverage rate is observed in June 2001 (month 30 in the graph). Specifically, prior to June 15, 2001, ninety-eight percent of the executions were covered in the DMN compared to just thirty-four percent of the post-June 15, 2001 executions. This regime change in how executions were covered (i.e., from all executions to primarily local executions) appears to have resulted from a change in the DMN management team on June 15, 2001. This change in editorship at the DMN provides a natural experiment to test the hypothesis that deterrent effects are greater for executions covered by the media.

Of course, a legitimate concern is the possibility that potential offenders do not read the newspaper. Though television coverage of executions is much harder to come by, I have obtained the coverage information for the eighty-three most recent executions in the data from an NBC affiliate station in Dallas. Specifically, thirty of these eighty-three executions were covered at least once on the news (fifteen were also covered in the DMN). For those covered by NBC, the average amount of time devoted to the story was about fifty-five seconds. As in the print media case, a regression analysis (which is not included but available from the author) indicates that executions sentenced in Dallas receive the maximum media attention. Specifically, such an execution is about fifty percent more likely to be covered and is covered, on average, for forty-eight seconds more. Likewise, Houston and San Antonio sentences are significantly less likely to be covered. Thus, regardless of the media outlet considered, it appears that potential offenders are

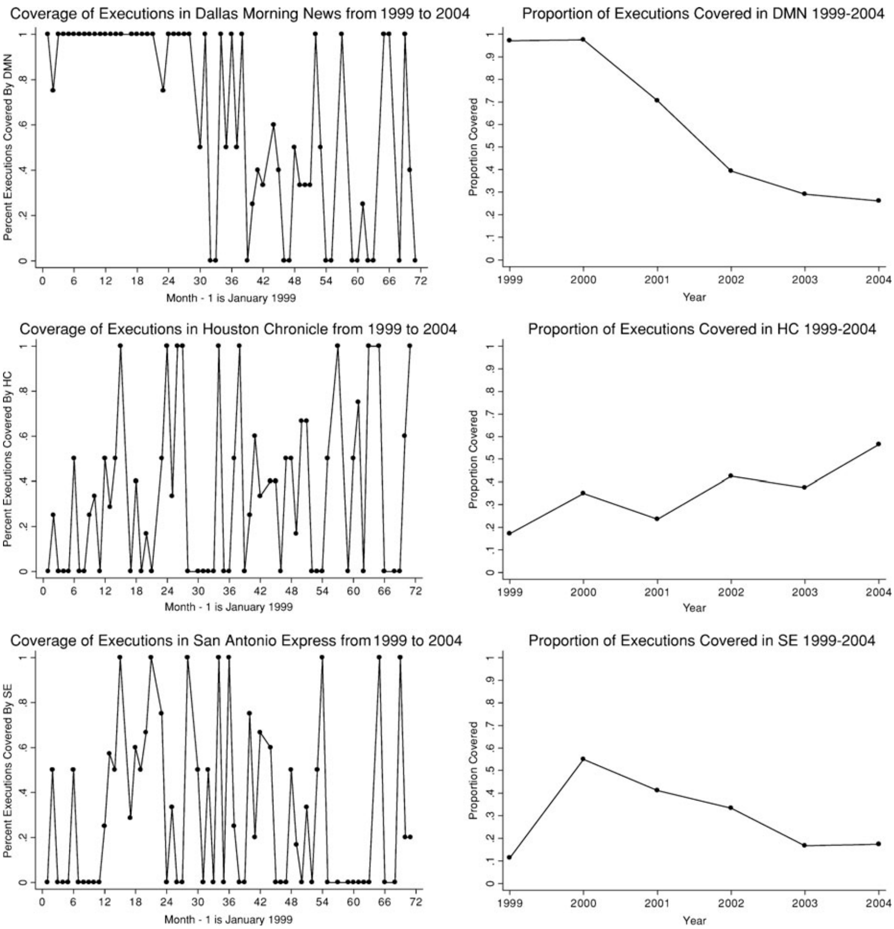

Figure 1 Proportion of Executions Covered in the Three City Newspapers.

significantly more likely to be exposed to information about local executions than nonlocal executions.[15]

15. Unfortunately, little is known about the newspaper reading and television news viewing habits of potential murderers. It is certainly possible that they do not read the newspaper or watch the evening news. However, one can potentially argue that even if this is the case, potential murderers may still be more likely to be aware of local executions by learning about them through social networks of criminals, which can serve as information markets. There is even evidence that criminal gangs and other crime networks may have productive and learning effects (Sarnecki, 2001; Warr, 2002). Though one cannot test this

4. Empirical Design

Since homicides peak on weekends and ninety-five percent of Texas executions occur midweek, one expects there to be fewer homicides on execution days than on the surrounding days. Plots of the data document this relationship. But, such a pattern does not necessarily imply a short-term deterrence effect. Rather, to identify such an effect, it is essential to control for day of the week and the seasonal nature of homicides. Thus, equation (1) presents the basic empirical specification, which is estimated separately for each city:

$$H_{tc} = \alpha + Any\_Exec_{t-3,t+3}\beta_1 + Any\_Exec_{t+4,t+10}\beta_2 + Any\_Exec_{t-4,t-10}\beta_3 + Day\_of\_week_t\delta + Week_t\lambda + Month_t\theta + Year_t\phi + \varepsilon_{tc} \quad (1)$$

The number of homicides on day t in city c is regressed on three execution variables: a dummy indicating whether there was an execution within three days of day t and dummies indicating whether there was an execution in the seven days before and after $t-3$ and $t+3$, respectively.[16] Controls for day of the week are included since almost forty percent of the Dallas, San Antonio, and Houston homicides occur on Saturday and Sunday while about twelve percent occur on each weekday. Graphs of the number of monthly and weekly homicides indicate that seasonality is relevant; thus, month and week of the year dummies are included. Year dummies are also included to account for possible general trends in homicide over time.[17] Lastly, ε_{tc} is assumed to be an unobserved, mean zero random variable that is uncorrelated with the observed covariates.

hypothesis directly, I will indirectly consider this possibility by looking at networks of "likes," e.g., race-based networks, in the analysis.

16. The results are qualitatively similar under a number of alternative specifications, including (i) panel data specifications; (ii) using the number of executions, rather than whether there were any executions; (iii) defining the dependent variable as any daily homicide rather than the number of daily homicides; and (iv) allowing additional leads and lags to be included.

17. The homicide trends observed in each of these cities are very similar to those observed in the counties surrounding these cities. In addition, the number of homicides in surrounding counties is fairly small relative to the homicide levels in the cities themselves.

Assuming that the error term is unrelated to observables is a strong assumption. Donohue and Wolfers (2006) highlight a number of reasons that may invalidate this assumption and typically imply the need for an instrument to identify a causal effect. For instance, they indicate that (i) potential offenders could be responding to other changes, such as longer prison sentences, which occur contemporaneously with an increased use of the death penalty; (ii) causation may run from homicides to executions; and (iii) there may simply be a large number of correlated unobservable factors changing over time. While these concerns are certainly valid in the current context, I believe that the high-frequency nature of the data goes a long way in mitigating the problem. For instance, it is highly unlikely that jail sentences become notably longer on the same day that an execution occurs.

Lastly, since the daily number of homicides is a count variable and always greater than or equal to zero, it is appropriate to estimate equation (1) using a count regression model. Thus, Poisson regression models are used throughout the paper, implying the assumption that the number of daily city homicides conditional on the covariates in the model has a Poisson distribution.[18, 19]

5. Results

5.1. Testing for Short-Term Deterrence with All Executions

Table 3 presents the results of estimating equation (1), displaying just those coefficients for the three variables indicating whether or not there was an execution in a particular 7-day period, regardless of whether that

18. As a specification check of the appropriateness of the Poisson model, I looked at the unconditional means and variances of the daily number of homicides in San Antonio, Dallas, and Houston as well as that for Houston capital murders. The means and variances are generally close together, but there is some evidence of overdispersion in the Dallas and Houston homicide sequences. Thus, a Poisson regression model is appropriate for the San Antonio homicide and Houston capital murder sequences, but a negative binomial model is better suited for the Dallas and Houston homicide sequences. However, as the negative binomial and Poisson specifications for Dallas and Houston were virtually identical, I present the results of Poisson models for each of the homicide series.

19. While it is common to use an offset measure, such as the population size, to account for the overall exposure in count models, this is not necessary (or even feasible) in these specifications. As the models are estimated using daily data for a single city over about four years, there is no available population measure. Any offset measure would be constant across observations and have no impact on the estimation.

Table 3. Regressions of City Homicide Counts on All Daily Executions

	(1)	**(2)**	**(3)**	**(4)**
	Dallas Homicides	**San Antonio Homicides**	**Houston Homicides**	**Houston Capital Murders**
EXlag4_lag10	0.041 (0.064)	−0.117 (0.104)	0.091 (0.058)	−0.177 (0.130)
EXlag3_lead3	−0.026 (0.065)	−0.032 (0.102)	0.004 (0.059)	−0.236* (0.130)
EXlead4_lead_10	0.035 (0.064)	−0.031 (0.101)	0.102* (0.058)	0.004 (0.127)
Observations	2100	2008	2192	2192

Standard errors in parentheses.

*Significant at 10%; **significant at 5%; ***significant at 1%. Each specification presents the execution variable coefficients resulting from the estimation of a Poisson regression model that also includes day of week, week of year, month, and year dummies. EXlag3_lead3 is a dummy variable equal to 1 if there is an execution within 3 days of day *t*. EXlag4_lag10 is a dummy variable equal to 1 if there is an execution in the fourth through tenth day lags, etc. In Texas, a capital murder can be broadly characterized as one in which (i) the victim is a peace officer or fire fighter acting in his official capacity; (ii) the murder is committed intentionally during an aggravated sexual assault, arson, burglary, kidnapping, obstruction, retaliation, or robbery; (iii) the murder is for pay; (iv) the offender paid another to commit murder; (v) murder occurred while escaping from prison; (vi) murder occurred while incarcerated; (vii) there were multiple victims; and (viii) the victim was a child under the age of 6.

execution was covered or was a local execution. The analysis begins by considering all executions for three reasons. First, this is what is done in many other death penalty studies that use aggregated data and is a natural starting point for comparison purposes. Second, it establishes a baseline to which the effects of local and covered executions can be compared. Third, potential offenders may learn of executions throughout the state through criminal networks; that is, noncapital offenders who are released from prison back to their communities may be better informed about executions than other individuals. Columns (1) through (3) present the results for Dallas, San Antonio, and Houston homicides, respectively. None of the execution coefficients in these columns are negative and significant; in fact, there is actually a positive and significant coefficient in Houston in the week leading up to an execution. However, a significant negative coefficient is seen for the contemporaneous week when considering just Houston capital murders; there are 23.6 percent fewer capital murders on days within 3 days of an execution.[20] There is also a sizable negative coefficient, though insignificant, in the following week.

5.2. Is a Deterrent Effect More Evident for Local Executions?

As discussed previously, the primary determinant of whether an execution receives media coverage is whether the offender was sentenced in the media outlet's own county. To test the resulting hypothesis that deterrence is more evident for local executions, equation (1) is estimated when considering only executions in the cities' home counties; table 4 presents the results. Once again, none of the execution coefficients are negative and significant. This is also the case for Houston capital murders, for which the greatest evidence of deterrence was seen when considering all executions. There is also a significant positive coefficient in the week leading up to the execution, though in this specification it is seen for Dallas whereas it was previously seen for Houston. Thus, the hypothesis of a greater deterrence effect for local

20. In general, the point estimates from nonlinear models do not capture the marginal effects (or the associated standard errors) of interest. In the context of the Poisson model, this implies that one cannot draw a conclusion from the estimated coefficients about a level change. However, the estimated coefficients in the Poisson model can be easily interpreted. The point estimates can be directly interpreted as a measure of the percentage change rather than as a level change. Thus, the coefficient of -0.236 on the contemporaneous week of executions for the Houston capital murder specification in table 3 implies that there are 23.6 percent fewer capital murders on days within three days of an execution.

Table 4. Regressions of City Homicide Counts on Local Executions

	(1)	(2)	(3)	(4)
	Dallas Homicides	**San Antonio Homicides**	**Houston Homicides**	**Houston Capital Murders**
EXlag4_lag10	0.051 (0.129)	0.300 (0.275)	−0.014 (0.118)	0.031 (0.263)
EXlag3_lead3	0.005 (0.130)	−0.454 (0.353)	−0.060 (0.117)	−0.028 (0.261)
EXlead4_lead_10	0.228* (0.121)	−0.012 (0.322)	0.167 (0.108)	−0.033 (0.255)
Observations	2100	2008	2192	2192

Standard errors in parentheses.
*Significant at 10%; **significant at 5%; ***significant at 1%. Each specification presents the execution variable coefficients resulting from the estimation of a Poisson regression model that also includes day of week, week of year, month, and year dummies. EXlag3_lead3 is a dummy variable equal to 1 if there is a "local" execution within 3 days of day *t*. EXlag4_ lag10 is a dummy variable equal to 1 if there is a "local" execution in the fourth through tenth day lags, etc. A local execution is one in which the executed offender was convicted and sentenced to the death penalty in the local county, i.e., Dallas County for Dallas, Bexar County for San Antonio, and Harris County for Houston. In Texas, a capital murder can be broadly characterized as one in which (i) the victim is a peace officer or fire fighter acting in his official capacity; (ii) the murder is committed intentionally during an aggravated sexual assault, arson, burglary, kidnapping, obstruction, retaliation, or robbery; (iii) the murder is for pay; (iv) the offender paid another to commit murder; (v) murder occurred while escaping from prison; (vi) murder occurred while incarcerated; (vii) there were multiple victims; and (viii) the victim was a child under the age of 6.

executions is not supported and, if anything, less evidence of deterrence is seen. It is important to note, however, that the local execution results will naturally be less precise than the results for all Texas executions due to the much smaller number of executions considered.

5.3. Is a Deterrent Effect More Evident for Locally Covered Executions?

This section tests the hypothesis that deterrent effects are more evident for covered executions. Table 5 presents the results of estimating equation (1) when the execution variables are defined for those executions covered by the specified local media outlet, i.e., the local newspaper. For Dallas, I also estimate a specification that considers whether the execution was covered by either the local NBC news affiliate or the DMN. It is important to point out that these coverage variables could be measured with error, either because my newspaper search did not find a particular execution or because the execution was covered in another media outlet; such measurement error would bias the results away from a finding of deterrence. No significant evidence of deterrence is found when looking at the Dallas, San Antonio, and Houston homicide series. The only evidence of deterrence is seen in the coefficient on the lagged week for Houston capital murders.[21] In addition, a positive and significant coefficient is seen in the leading week when considering Dallas executions covered by either the DMN or local NBC news affiliate.

As a second test of the media coverage hypothesis, I turn to the natural experiment provided by the change in management at the DMN. Prior to June 15, 2001, almost all Texas executions were covered by the DMN while after June 15, 2001, primarily just local executions were covered. Table 6 presents the results of restricting the analysis to pre- and post-June 15, 2001 data in Dallas and estimating equation (1) when the execution variables once again capture all Texas executions. If the hypothesis were true, then one would expect to find more evidence of deterrence prior to June 2001. Though none of the coefficients are significant, it is the case that a reasonably sized negative coefficient is seen prior to June 2001 for executions within three days of day *t* while the same coefficient is slightly positive post-June 2001. While this is consistent with the hypothesis, it is certainly not conclusive support, given the imprecision of these estimates seen in the standard errors.

21. I also consider the possibility that executions have separate effects from whether the executions were covered, but find no additional evidence of deterrence.

Table 5. Regressions of City Homicide Counts on Locally Covered Executions

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
	Dallas (DMN)	**Dallas (DMN or NBC)**	**San Antonio (SAE)**	**Houston (HC)**	**Houston Capital Murders (HC)**
EXlag4_lag10	0.080 (0.073)	0.109 (0.103)	0.053 (0.138)	0.016 (0.076)	−0.297* (0.179)
EXlag3_lead3	−0.083 (0.074)	0.048 (0.106)	−0.211 (0.147)	0.079 (0.074)	−0.166 (0.170)
EXlead4_lead_10	0.100 (0.071)	0.201** (0.100)	0.004 (0.141)	0.051 (0.074)	−0.151 (0.171)
Observations	2100	1065	2008	2192	2192

Standard errors in parentheses.
*Significant at 10%; **significant at 5%; ***significant at 1%. Each specification presents the execution variable coefficients resulting from the estimation of a Poisson regression model that also includes day of week, week of year, month, and year dummies. EXlag4_ lag10 is a dummy variable equal to 1 if there is a locally covered execution in the fourth through tenth day lags, etc. A locally covered execution is one in which the execution is reported in the relevant local newspaper: *Dallas Morning News* for Dallas, *Houston Chronicle* for Houston, and *San Antonio Express-News* for San Antonio. In Texas, a capital murder can be broadly characterized as one in which (i) the victim is a peace officer or fire fighter acting in his official capacity; (ii) the murder is committed intentionally during an aggravated sexual assault, arson, burglary, kidnapping, obstruction, retaliation, or robbery; (iii) the murder is for pay; (iv) the offender paid another to commit murder; (v) murder occurred while escaping from prison; (vi) murder occurred while incarcerated; (vii) there were multiple victims; and (viii) the victim was a child under the age of 6.

Table 6. Testing for Deterrence Before and After the Change in Dallas News Regime

	(1)	(2)	(3)
	All Dallas Data	**Pre-June 15, 2001**	**Post-June 15, 2001**
EXlag4_lag10	0.041 (0.064)	0.044 (0.109)	0.104 (0.087)
EXlag3_lead3	−0.026 (0.065)	−0.093 (0.108)	0.037 (0.087)
EXlead4_lead_10	0.035 (0.064)	0.042 (0.107)	0.088 (0.086)
Observations	2100	899	1201

Standard errors in parentheses.
*Significant at 10%; **significant at 5%; ***significant at 1%. Each specification presents the execution variable coefficients resulting from the estimation of a Poisson regression model that also includes day of week, week of year, month, and year dummies. EXlag3_lead3 is a dummy variable equal to 1 if there is an execution within 3 days of day *t*. EXlag4_ lag10 is a dummy variable equal to 1 if there is an execution in the fourth through tenth day lags, etc. The first column uses all of the data; the second column restricts the analysis to data prior to June 15, 2001, when almost all executions were covered; the third column restricts the analysis to data after June 15, 2001, when a more limited set of executions were covered in the *Dallas Morning News*.

Overall, the results presented in this section provide limited support of the hypothesis that deterrent effects are greater for locally covered executions.

5.4. Do Likes Deter Likes?

It is also possible that potential offenders do not learn about executions from the local media, but rather have access to networks of other potential offenders who may have knowledge of the execution. Given the data, my ability to test whether offenders are deterred by executions that they may hear about in their network is limited. However, I can consider race-based networks and ask whether executing an offender who had at least one Black victim deters homicides of Blacks. Similarly, I can assess whether there are cross-race effects, i.e., whether homicides of Blacks are deterred when the executed offender has White victims. If such networks were really race based, then one would expect to only find a deterrence effect for the same race cases.[22] Table 7 presents the coefficients of the execution variables for these specifications; the homicide and execution variables vary across columns

22. Ideally, one would like to consider whether executing black offenders, for instance, deters potential black offenders rather than focusing on the characteristics of the offenders' offense. Though data on the executed offender's characteristics are available, the homicide data only include characteristics of the offense and not the offender. One should also note that the Dallas homicide data do not separately distinguish Hispanic victims from white victims.

Table 7. Testing for Deterrence With Victim-Specific Characteristics in Dallas

Dependent Variable is Number of Homicides of:	**(1) Victims of Any Race**	**(2) Black Victims**	**(3) White Victims**	**(4) White Victims**	**(5) Black Victims**
Executions of Offenders with:	**Any Victim**	**Black Victims**	**Black Victims**	**White Victims**	**White Victims**
EXlag4_lag10	0.041	0.406**	0.060	−0.026	−0.043
	(0.064)	(0.191)	(0.192)	(0.094)	(0.102)
EXlag3_lead3	−0.026	−0.028	−0.258	0.146	−0.114
	(0.065)	(0.224)	(0.210)	(0.092)	(0.102)
EXlead4_lead_10	0.035	0.134	−0.099	0.045	0.010
	(0.064)	(0.218)	(0.198)	(0.094)	(0.099)
Observations	2100	2100	2100	2100	2100

Standard errors in parentheses.
*Significant at 10%; **significant at 5%; ***significant at 1%. Each specification presents the execution variable coefficients resulting from the estimation of a Poisson regression model that also includes day of week, week of year, month, and year dummies. EXlag3_lead3 is a dummy variable equal to 1 if there is an execution within 3 days of day *t*. EXlag4_ lag10 is a dummy variable equal to 1 if there is an execution in the fourth through tenth day lags, etc. The execution variables included in this table are different for each column and correspond to whether there was an execution of an offender whose victims had the characteristics in the third rows of the table. Likewise, not all daily homicides are considered, but rather, just those of victims with the characteristics in the second row of the table. For instance, are there less Black homicide victims on days where the victim of the executed offender was Black? Or, are there less White homicide victims on days where the victim of the executed offender was Black?

and are denoted in the second and third rows of the table. No evidence of the short-term deterrence is seen, and there is no more evidence in the same race specifications than when considering cross-race effects.[23] One possible explanation of the lack of deterrence evidence is that information about executions is simply not dispersed in a manner efficient enough to result in potential offenders updating their perceived risks of execution; perhaps individuals who are released from prison are more likely to talk about the prison conditions than who was executed.

5.5. Aggregation versus Disaggregation

The use of disaggregated data distinguishes this paper from much of the recent death penalty research. To help put the results found here in context

23. Precision can certainly be a concern in these specifications given that only fifteen executed offenders had black victims; one hundred and eight executed offenders had white victims. Note, however, that homicides of at least one black occurred on about twenty-five percent of the days in Dallas; the same is true of homicides with white victims.

with the rest of this literature, I briefly discuss the potential impact of aggregation. Specifically, aggregating the data temporally or geographically should dilute the estimated deterrent effects (to the extent that any are found). Let us consider the case of temporal aggregation; a similar thought experiment can be done for geographical aggregation. Table 3 indicates that there are 23.6 percent fewer capital murders in Houston in the seven days surrounding an execution. Given that there is, on average, just one capital murder per week in Houston, about 0.25 capital murders are deterred in a week surrounding an execution. If there is only one execution and four capital murders in a particular month, then there should be a reduction of *only* around six percent in the monthly number of capital murders, i.e., the effect would be diluted and may even be too small to identify.

Empirical evidence of such dilution is seen when using monthly aggregated data and estimating a Poisson regression model of the number of Houston capital murders on the number of executions. Note that more than sixty percent of the months have at least two executions and very few have zero executions, making it necessary to consider the level of executions. Thus, this specification is similar but not completely comparable to the specifications presented throughout this paper. Using daily data allows one to assess whether an event that potentially shocks an individual’s beliefs has a short-term deterrent effect. Yet when temporally aggregating, one cannot consider such shocks but rather whether the level of executions has a deterrent effect. In addition, aggregation makes the analysis more susceptible to the endogeneity issues discussed earlier in the paper. Nevertheless, this specification does provide evidence of dilution. The significant evidence of deterrence previously seen for Houston capital murders is not observed and the point estimate indicates that increasing the monthly number of executions by one will decrease capital murders by about five percent.

6. Conclusion

Donohue and Wolfers’ (2006) analysis of the recent death penalty literature concludes, in part, that even if there is a deterrent effect, the variation in the homicide rate due to executions is simply too small to disentangle from the variation due to other determinants. While I do not disagree with this statement, I argue that it may be an artifact of the aggregated data commonly used in the recent death penalty literature. Using temporally aggregated data may make it impossible to identify variations in homicides that occur in the days immediately surrounding an execution when aggregating over the entire year. Likewise, geographically aggregated data can mask changes in homicides that may occur in just one jurisdiction rather than statewide. Therefore, this paper uses daily data to test for a short-term deterrent effect in three Texas cities.

Estimating Poisson regression models of daily homicide counts on daily executions as well as execution leads and lags, I find minimal evidence that executions have a short-term deterrent effect on homicides. If there is any evidence of deterrence, it is with respect to capital murders; however, this evidence is weakened by inconsistencies across specifications and/or cities. Thus, there is little indication that executions have any short-term impact on how much a potential offender fears executions.

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