Execution Moratoriums, Commutations and Deterrence: The Case of Illinois

By

Dale O. Cloninger, Professor of Finance & Economics*
(cloninger@uhcl.edu)

and

Roberto Marchesini, Professor of Finance
University of Houston-Clear Lake
Houston, TX 77058

ABSTRACT

In an earlier work we examined the impact of an execution moratorium in Texas on the monthly returns (first differences) of homicides. That moratorium was judicially imposed pending the appeal of a death sentence that could have had wide spread consequences. We apply similar methodology to the state of Illinois. In January 2000, the Governor of Illinois declared a moratorium on executions pending a review of the judicial process that condemned certain murderers to the death penalty. In January 2003 just prior to leaving office, the Governor commuted the death sentences of all of those who then occupied death row. We find that these actions are coincident with the increased risk of homicide incurred by the residents of Illinois over the 48-month post event period for which data were available. The increased risk is associated with an estimated 150 additional homicides during the post-event period.

June 2005

Execution Moratoriums, Commutations and Deterrence: The Case of Illinois

In our earlier work (Cloninger & Marchesini, 2001) we examined the impact of an execution moratorium in Texas on the monthly returns (first differences) of homicides. That moratorium was judicially imposed pending the appeal of a death sentence that could have had wide spread consequences. The moratorium remained in effect for approximately 12 months. During this period the number of executions in Texas decreased from a three-year average of 17 to three. In the immediate 12-month post moratorium period 33 executions occurred. Statistical tests found that during the moratorium period actual homicides exceeded those predicted by the empirically derived model, a finding consistent with the deterrence hypothesis.

We apply similar methodology in the present study to the state of Illinois. In January 2000, the Governor of Illinois declared a moratorium on executions pending a review of the judicial process that condemned certain murderers to the death penalty. In January 2003 just prior to leaving office, the Governor commuted the death sentences of all of those who then occupied death row.

This study treats the two executive decisions as separate 'events' and applies an event type methodology (Brown & Warner, 1985) to determine if the returns to homicides are significantly altered. We extend the analysis to include a comparison of pre- and post event homicide betas. It is hypothesized that in the presence of a deterrence effect, the post event residuals will be found, in the main, significantly positive and the absolute size of the homicide beta will significantly increase. The former may be interpreted as evidence in support of the deterrence hypothesis while the latter could be similarly interpreted as well as viewed as an increase in the systematic risk of homicide. A larger beta coefficient reflects greater variation in the returns to homicide.

Relevant Literature

Since 2001 a number of other research papers have been published finding evidence in support of the deterrence hypothesis with respect to criminal executions. These studies include, but may not be limited to, Dezhbakhsh, Rubin & Shepherd (2003); Dezbakhsh & Shepherd (2004); Liu (2004); Mocan & Gittings (2003); Shepherd (2004, 2004a); and Zimmerman (2004, 2004a). All of these studies employ some form of regression analysis. Mocan & Gittings explore issues similar to the present study, that is, executive interruptions to the execution process.

Specifically, Mocan & Gittings find significant evidence that three pardons contribute from one to one and a half additional homicides and marginally significant evidence that each execution deters 5-6 homicides. Berk (2005) criticizes Mocan & Gittings because their data appear to contain a small number of observations that dominate the statistical results. Berk finds that for states that have 5 or fewer executions per year, the relationship between executions and homicides is unpredictable or even positive. (Also see Shepard 2004a.) Only when the few states that execute significantly more than 5 year are added to the data set does any evidence in support of the deterrence hypothesis appear. Berk specifically lists Texas as one state whose inclusion in the data set provides evidence consistent with the deterrence hypothesis. Yet, he states that does not imply that deterrence is at work in Texas. Our 2001 paper suggests otherwise.

Goertzel (2002) critiques the above studies for employing one form or another of regression analysis with its attendant inherent potential problems with specification errors, multi-colinearity and autocorrelation problems. Ehrlich (1975) has been subject to similar criticisms to which he offered a substantial rebuttal and further empirical analysis, Ehrlich (1977 & 1977a). Despite the criticisms of regression analysis, it remains a powerful research tool providing valuable insight into the human condition in all the social sciences.

Methodology

The present study does not attempt to construct a supply of criminal offenses function and thereby avoids the usual criticisms associated with multiple regression analysis in any of its forms. Rather, we construct a model of homicide returns in a particular state based on how those returns are correlated with a national index of all homicide returns. That model is expressed given by,

$$R_{jt} = \alpha + \beta H_t + \mu \qquad (1)$$

Where R_{jt} is the return on homicides in a particular state (j) at time (t), H_t is the return on homicides as represented by a national index. The Greek letter α is the intercept term and β is the slope coefficient. The latter term can be interpreted as that unique factor that relates homicide returns in a given state to those in the nation as a whole. Beta[i] includes all those forces that impinge upon changes in the number of homicides in a particular state with those of a broad national index.

Should the empirical form of this model prove significant over some control period, it may be used to estimate changes in homicides over an experimental period. The latter represents a period over which some unanticipated events occur that could significantly alter the relationship established over the control period. These estimates could then be compared with actual observations and the differences (residuals) noted.

In our Texas study there were two events that defined the experimental period; the imposition of a moratorium on executions and the subsequent lifting of that moratorium approximately 12 months later. We found a series of significant monthly residuals during the experimental period that were consistent with the deterrence hypothesis. In addition, around the date of the lifting of the moratorium (~60 days) we found back to back significant monthly residuals; the first producing a greater than expected number of homicides followed immediately by a smaller than expected number of homicides. After that reaction, the differences between expected and actual homicides resumed the random pattern found in the control period.

We did not test for a shift in beta in our 2001 study as the experimental period was too short (~12 months). However, in the present study, 36 to 48 monthly observations exist in the experimental period, a sufficiently large data set to warrant testing for a beta shift. As stated earlier, any significant increase in beta would be consistent with the deterrence hypothesis. This evidence may also be interpreted somewhat differently *vis-à-vis* differences between expected and actual homicides.

A greater absolute beta during the experimental period would indicate greater variability in the returns to homicide in a particular state such as Illinois. Greater variability in homicide returns translates into greater systematic risk of homicide. While the higher beta indicates smaller expected homicides returns during periods of declining homicide returns nationally, it likewise indicates greater than expected homicides returns during periods of increasing national homicide returns. This variability is systematic because it is directly linked to national homicide returns. Any remaining variability is considered random or nonsystematic risk. The total risk of homicide is simply the sum of the systematic and nonsystematic risk.

Empirical Results

Replicating the procedure adopted in our 2001 paper, we defined the 60 months preceding the Governor's declared moratorium as the control period. We seasonally adjusted the monthly data (provided upon request by the Uniform Crime Reporting Section of the FBI) by calculating the homicide returns based on the 12-month interval from the corresponding month in the previous year. This methodology requires data for 72 months to generate 60 observations. We then regressed the Illinois returns on the returns for the nation.[ii] The resultant empirical model is,

$$R_{jt} = -.04 + .39H_t + e. \qquad (2)$$

Because the standard error of the national index (H_t) is .31, its coefficient is not significantly different from zero. That is, the empirical model along with the distribution of its residuals suggests that fluctuations of homicide returns in Illinois are randomly distributed about a mean of - .07. These results will be compared to the post-event measures in a subsequent section.

(Insert Table 1 here.)

Turning to an analysis of the homicide beta in Illinois, we computed the corresponding beta over the 36-month period following the first event (the declaration of a moratorium on executions in January 2000) but prior to the second event (commutation of all death row sentences in January 2003). The resultant empirical beta is .81 with a standard error of .5. This beta is marginally significantly different from zero (t = 1.63) when employing a one-tail test. This evidence suggests that the systematic risk of homicide significantly increased during the moratorium period *vis-à-vis* the control period.

(Insert Table 2 here.)

In January 2003, a second event occurred when the outgoing Governor commuted all remaining death row inmates' sentences. Adding the available 12 months of post second event data, we repeated the estimate of the post event beta. The resultant beta is .97 with a standard error of .44. Thus, both the size and significance (t = 2.2) of the post first event beta increased. This evidence suggests that the second event increased the systematic risk of homicide over that associated with the first event. Because the resultant beta coefficient is not significantly different from one, it may be said that homicides returns in Illinois now vary directly and proportionally with homicide returns nationally. Recall, in the control period the Illinois returns were randomly distributed.

(Insert Table 3 here.)

Testing for a significant difference between the mean return of the control sample (-.0702) and that of the experimental sample (.0055) yields a 't' value equal to -2.03 or significance that exceeds the 95 percent confidence level. Likewise, the F test of the two corresponding variances yields significant results reflecting the fact that the size of the experimental variance exceeds that of the control sample by more than a factor of two.

Thus, both the average return and variance are significantly greater in the post (first) event period. The analysis yields no significant autocorrelation in any series.

(Insert Tables 4 & 5 here.)

Because the mean return in the pre-event period is significantly less than the mean return in the post-event period, it can be reasonably concluded that the number of homicides in the post-event period is significantly greater than what would have occurred in the absence of the shift in beta. That is, had the random pattern of the pre-event period continued about its mean, the incidence of homicide would have resulted in approximately 150 fewer homicides in the 48 months following the first event (January 2000). Table 6 shows the calculation of the additional homicides during the 48-month post-event period.

(Insert Table 6 here.)

Conclusions

The inability of the empirical model (2) to provide reliable expected changes in homicide over the control period does not speak to the issue of deterrence. Thus, the lack of this type of evidence may be attributed to the random nature of changes in homicides during periods in which the state of Illinois permitted executions albeit at a rate rarely exceeding one per year. Illinois is perhaps one of those states identified by Berk whose execution rate is so low as to prohibit any reliable conclusions about the relationship between executions and the homicides. Illinois clearly falls below the threshold suggested by Shepherd. The above statements do not suggest the absence of a deterrence effect of in the state of Illinois.

Whereas the pre-event empirical model does not lend itself to reliably predicting homicides, the significant increase in the mean return during the post-event period does allow an estimate of the expected number of homicides. Comparing these estimates with the actual reported homicides suggests that approximately 150 additional homicides occurred in Illinois in the 48-month post-event period.

The evidence also suggests that the citizens of Illinois experienced increased systematic risk of homicide as a result of the executive actions that imposed a moratorium and subsequently commuted all death row sentences. Systematic risk of homicide in Illinois now parallels that of the nation as a whole. The total risk of homicide in Illinois, as measured by the pre and post (first) event variances in homicide returns, also increased significantly.

The increased risk was cumulative, that is, a result of the two executively imposed events. Illinois has not, to date, abandoned the death penalty. Given the above results it may be reasonably concluded that should abolition occur the risk of homicide to the citizens of Illinois would again increase. It may also be reasonably concluded that a resumption of executions in Illinois would thereby reduce the risk of homicide.

Miscellanea

Based on nearly three decades of study and research into the deterrence hypothesis with respect to executions, we offer the following lessons gleaned:

There is no such thing as a perfect research study. The present study is no exception. While not subject to the same criticisms as other recent studies that employ some form of multiple regression analysis (with or without panel data), the present study remains sensitive to data anominalies such as the presence of a small number of executions. Conceivably, a suspension of executions in a state that executes no more than one or two per year could have a differential impact *vis-à-vis* a state, with approximately the same population and homicide rate that executes, say, 10 or more per year. Berk's analysis implies such an eventuality. Shepherd (2004a) sheds additional light on these assertions by suggesting that there is a threshold (nine executions per year) below which deterrence is not found.

There is no such thing as a perfect critique. Just because some scholars publish critiques of other scholar's work does not mean the critiques are appropriate, justified or correct. Ehrlich's response (1977 & 1977a) to his critics appears to address most if not all of the specific criticisms levied. However, many subsequent critiques of his and others' works ignore Ehrlich's rebuttals while still recognizing the original criticisms of his work. In our experience, criticisms by other scholars have been so riddled with errors that editors have declined to publish them. *Corollary: The mere publication of a critique does not necessarily mean that the original study is 'discredited'.*

Evidence in support of the deterrence hypothesis has come, in the main, from economists. Studies that find no evidence to support the deterrence hypothesis have, in the main, come from sociologists and the like. One possible explanation is that the deterrence hypothesis presumes rational behavior (as defined by economists). Perhaps, sociologists, criminologists and anthropologists still have lingering reservations with this presumption. Are crimes of passion, anger and impulse subject to risk and return? Economists such as Shepherd (2004) find evidence that supports, at least in part, a positive answer to that question. Social scientists in general appear skeptical. Economists have little to support their hypotheses other than empirical evidence and the presumption of rationality. Therein may lie a root cause of the difference between economists and other social scientists with respect to the deterrence issue. There are, undoubtedly, other causes as well.

Deterrence is neither served by executing an innocent person nor by failing to execute a guilty one. The presumed stimulus for the Governor of Illinois to impose a moratorium and later to commute all death row sentences was the concern over the possibility that the legal and judicial process in Illinois may have been convicting potentially innocent defendants[iii]. There existed sufficient questions in a sufficient number of death row cases to cause the Governor pause. A necessary prerequisite for deterrence is a common and universal perception that the judicial process is unbiased, fair and accurate. Otherwise, executions may engender more violence not less. Likewise, for the threat of execution to generate a deterrent effect, citizenry must be convinced that executions will occur when warranted. The mere presence of a death penalty does not in and of itself provide a measurable deterrence effect. Potential murders must be convinced that the threat of execution is very real. Executing condemned death row inmates, when appropriate, provides convincing evidence of a real threat.

Empirical tests that compare homicide rates in death penalty states to those in non death penalty states are unlikely to detect a deterrence effect even if one in fact exists. Potential murders are likely to perceive states with no death penalty no differently than states with a death penalty but rarely, if ever, employ it. To these individuals, it is not so much what the law says as what the law does. These types of studies possess only face validity.

The presence of higher homicide rates in death penalty states than in non death penalty states does not imply either an absence of a deterrent effect or suggest the presence of a brutalization effect. The implied causation may be reversed. Instead of executions engendering a greater amount of homicides, low homicide rates may allow jurisdictions to effectively eliminate the death penalty. That is, perhaps the best method to abolish the death penalty is to lower the homicide rate in jurisdictions that do execute offenders to levels consistent with those of jurisdictions that do not. When the risk of homicide is sufficiently low, citizens may feel imposing the death penalty is unwarranted. In economic lexicon, the marginal deterrent effect of executions in such jurisdictions is very low or negligible, that is, the perceived marginal moral cost of executions exceeds their perceived marginal moral benefit. In this manner, states with low homicide rates would be expected to abolish or use the death penalty sparingly while high homicide states would be expected to rely on executions more liberally. It would, therefore, seem logical that low executions states would either increase the rate of executions or abolish the death penalty. But then, logic does not seem to dominate the debate over the use of the death penalty.

APPENDIX

States not included in sample due to lack of consistent data over the sample period

1. Alabama
2. Alaska
3. Arizona
4. Colorado
5. D.C.
6. Florida
7. Idaho
8. Iowa
9. Louisiana
10. Maryland
11. Minnesota
12. Montana
13. New York
14. Ohio
15. Oregon
16. Rhode Island
17. Vermont

Table 1

Illinois and US Homicide: Monthly Changes 1995 through 1999

Regression Statistics

	Coefficients	*Standard Error*	*t Stat*	*P-value*
Intercept	-0.04021	0.03101	-1.29676	0.19985
Index	0.39138	0.31344	1.24867	0.21680

Table 2

Illinois and US Homicide: Monthly Changes 2000 through 2002

Regression Statistics

	Coefficients	*Standard Error*	*t Stat*	*P-value*
Intercept	0.00409	0.03720	0.10986	0.91317
Index	0.81149	0.49763	1.63070	0.11218

Table 3

Illinois and US Homicide: Monthly Changes 2000 through 2003

Regression Statistics				
	Coefficients	*Standard Error*	*t Stat*	*P-value*
Intercept	-0.01434	0.03182	-0.45067	0.65434
Index	0.96807	0.44032	2.19858	0.03298

Table 4

Comparison of Pre and Post Event Returns, Illinois

	Pre-Event	*Post-Event*
Mean	-0.07021	0.005569
Variance	0.023296	0.048296
Observations	60	48
Hypothesized Mean Difference	0	
Df	81	
t Stat	-2.02925	
P(T<=t) one-tail	0.022859	
t Critical one-tail	1.663884	
P(T<=t) two-tail	0.045717	
t Critical two-tail	1.989688	

Table 5

Comparison of Pre- and Post Event Variances

F-Test Two-Sample for Variances

	Post-Event	*Pre-Event*
Mean	0.005569	-0.07021
Variance	0.048296	0.023296
Observations	48	60
Df	47	59
F	2.07315	
P(F<=f) one-tail	0.00412	
F Critical one-tail	1.571618	

Table 6

Actual vs. Estimated Homicides: Post-Event

Period	Actual	Estimated	Difference
Jan-00	49	55	-6
	44	40	4
	46	43	3
	40	56	-16
	50	59	-9
	58	71	-13
	84	55	29
	70	72	-2
	64	52	12
	59	45	14
	60	55	5
	48	44	4
Jan-01	44	46	-2
	33	41	-8
	40	43	-3
	63	37	26
	46	46	0
	74	54	20
	78	78	0
	63	65	-2
	74	60	14
	80	55	25
	68	56	12
	55	45	10
Jan-02	51	41	10
	32	31	1
	44	37	7
	49	59	-10
	50	43	7
	70	69	1
	80	73	7
	83	59	24
	75	69	6
	64	74	-10
	46	63	-17
	71	51	20
Jan-03	41	47	-6
	32	30	2
	55	41	14
	62	46	16
	61	46	15

	53	65	-12
	76	74	2
	53	77	-24
	58	70	-12
	55	60	-5
	55	43	12
	47	66	-19
Total (net)	**2753**	**2602**	**151**
2000-2003			

REFERENCES

Brown, Stephen J. and Jerold B. Warner. (1985) Using Daily Stock Returns: The Case of Event Studies. *Journal of Financial Economics*, 14, 3-31.

Cloninger, Dale O. (1992) Capital Punishment and Deterrence: A Portfolio Approach, *Applied Economics,* 24, 645-655.

Cloninger, Dale O. and Roberto Marchesini, (1995) Crime Betas: A Portfolio Measure of Criminal Activity," *Social Science Quarterly*, 76, pp.634-647.

Cloninger, Dale O. and Roberto Marchesini, (1995a) Crime Betas: A Portfolio Measure of Criminal Activity: A Reply," *Social Science Quarterly*, 76, 916-18.

Cloninger, Dale O. and Roberto Marchesini, (2001) Execution and Deterrence: A Quasi-Controlled Group Experiment," *Applied Economics*, 33, 569-76.

Dezhbakhsh, H., Paul Rubin, and Joanna Shepherd (2003) Does Capital Punishment Have a Deterrent Effect? New Evidenced from Post-moratorium Panel Data, *American Law and Economics Review*, 5, 344-76.

Ehrlich, Issac (1975) The Deterrent Effect of Capital Punishment—A Question of Life and Death, *American Economic Review*, 65, 397-417.

Ehrlich, Issac (1977) The Deterrent Effect of Capital Punishment: Reply, *American Economic Review,* 67, 452-58.

Ehrlich, Issac (1977a) The Deterrent Effect of Capital Punishment: Some Further Thoughts and Additional Evidence, *Journal of Political Economy*, 85, 741-88.

Goertzel, Ted. (2002) Econometric Modeling as Junk Science, The Skeptical Inquirer, V. 26, No. 1, 19-23

Liu, A. (2004) Capital Punishment and the Deterrence Hypothesis: Some New Insights and Empirical Evidence, *Eastern Economic Journal*, 30, 237-58.

Shepherd, Joanna (2004) Murders of Passion, Execution Delays and the Deterrence of Capital Punishment, *Journal of Legal Studies* (forthcoming).

Shepherd, Joanna (2004a) Deterrence vs. Brutalization: Capital Punishment's Differing Impacts Among States (Emory Legal Scholarship Working Paper No. 1, 2004).

Zimmerman, P. (2004) Estimates of the Deterrent Effects of Alternative Execution Methods in the United States: 1978-2000, *American Journal of Economics and Sociology* (forthcoming).

Zimmerman, P. (2004a) State Executions, Deterrence and Incidence of Murder, *Applied Economics* (forthcoming).

[i] An extended discussion and statistical analysis of crime betas appears in our 1995 and 1995a papers.
[ii] States that did not consistently supply data to the Uniform Crime Division of the FBI over the entire sample period (1994-2003 inclusive) were excluded from the sample data set. There were 13 such states. Our 2001 paper employed a similar adjustment. See the Appendix for a complete listing of states excluded.
[iii] Innocent as used in this context does not necessarily imply factual innocence as much as it does legal innocence.