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Execution and deterrence: a quasi-controlled group experiment

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Using portfolio analysis in a type of controlled group experiment, this study develops an empirical model of homicide changes in Texas over a period of a 'normal' number of executions. The empirically derived model then estimates the changes in the number of homicides in Texas (1) over a period of near zero executions and; (2) over an immediate subsequent period of double the 'normal' number of executions. The actual changes in Texas homicides over the first period is less than estimated by the model and greater (or no different) than estimated by the model in the second period. Because changes in the number of homicides in Texas and throughout the United States were negative over both periods, these empirical results are consistent with the deterrence hypothesis. That is, there were a greater than predicted number of homicides in the first period and fewer than predicted number in the second period.

I. INTRODUCTION

Using a quasi-controlled group experiment, this research seeks to augment the existing econometric literature that finds empirical evidence consistent with the theory that executions deter future homicides. The literature includes, but is not limited to, Erhlich (1975, 1977), Cloninger (1977, 1991, 1992), Layson (1983, 1985), and Yunker (1977).

Ehrlich (1975) uses two stage least squares regression analysis to develop a logarithmic supply of homicides function that employs time series data from 1933 to 1967. Cloninger (1977) addresses the considerable criticism levied against Ehrlich's use of time series data that span several decades and his use of the logarithmic form by employing cross-sectional state data from 1965–1970 in a linear (arithmetic) model. Cloninger finds significant evidence consistent with the deterrent effect of capital punishment. In a subsequent paper, Ehrlich (1977) also responds to the major criticisms of his earlier work and finds, like Cloninger, significant evidence consistent with his earlier finding as does Layson (1983).

Cloninger (1991) investigates the response of various types of criminal activities to the risk of being killed by enforcement officers in the latter's line of duty (lethal response). He finds empirical support for general deterrence with respect to all non-homicide violent crime (including burglary). He finds that lethal response and homicide are, however, significantly positively correlated.

Cloninger (1992) uses a portfolio approach similar to the present study to compare data in those states and years when there was at least one execution with those states and years when there were no executions. In a series of statistical tests, he finds statistically significant evidence consistent with the deterrent effect of capital punishment. Cameron (1994) provides a detailed critical review of the econometric literature on the deterrence effect of capital punishment.

II. METHODOLOGY

The stay granted *Davis* (1996) by the Texas Court of Criminal Appeals (TCCA) on 2 January 1996 and the subsequent delay of other executions provides a unique opportunity to apply a type of control group experiment to the issue of the deterrence effect of capital punishment. The

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appeals court lifted its stay almost one year later on 18 December 1996. During the interim, the Texas Department of Corrections (TDC) executed three prisoners, *vis-à-vis*, an average of 17 for the immediate preceding three years (US Department of Commerce, 1998). From 10 February 1997 to 31 December 1997 Texas executed 37 inmates. Thus, in back-to-back years, the Texas Department of Corrections decreased its number of executions (from the expected number) by more than 80% and then, in the following year, more than doubled the expected number of executions. The change in actual executions from 1996 to 1997 was twelve-fold. This dramatic double reversal (normal to near zero to double the norm) in the number of executions provides an opportunity to compare, back-to-back in the same population, the impact of less than and greater than the expected number of executions.

Using a model first developed in Cloninger (1992) and subsequently elaborated in Cloninger and Marchesini (1995a,b), a linear relationship is developed between seasonally adjusted monthly changes in the number of homicides in the USA and seasonally adjusted monthly changes in the number of homicides in Texas for the six year period 1990–1995 inclusive. The changes are the 12 month percentage differences between corresponding months in successive calendar years. At the request of the authors, the Uniform Crime Reporting Section of the Federal Bureau of Investigation (1989–1997) provided the monthly variations for the years 1989–1997 inclusive. FBI data contain potentially serious reporting bias. The FBI omits from the national index states that have known omissions or discrepancies. The FBI delayed the release of 1997 monthly variations by eight months to further audit the data submitted by reporting agencies.

The analysis minimizes the reporting bias inherent in the FBI data in two ways: (1) because of the nature of the crime, the reporting bias for homicides is inherently less than for any other type of violent crime, and (2) by employing running 12 month percentage changes, the methodology converts the data to a type of first differences. Using first differences also has the added property of adjusting for possible serial correlation. This methodology does not require zero reporting bias, it requires only that the reporting bias be consistent over the sample period.

The inherent linear relationship of this methodology is given by,

$$\delta H_{\mathrm{tx}} = a + b(\delta H_{\mathrm{us}}) + e \tag{1}$$

where, δH_{tx} is the 12 month percentage change in the number of homicides in Texas, δH_{us} is the corresponding 12 month percentage change in the number of homicides in the USA, a and b are empirically derived intercept and slope parameters, and e is the error term. The slope term b, or beta, is a unique parameter that captures those forces that influence how homicide changes in Texas are associated with the national norm, i.e. the changes in the number of homicides in the United States.

This approach adopts the standard event study methodology developed by Brown and Warner (1980) and used extensively by financial economists to analyse the behaviour of returns on individual financial securities in literally thousands of event studies. In that framework, the returns of an individual security are regressed against a broad financial market index such as the Standard and Poor 500 Index, the New York Exchange Index or the Wilshire 5000 Index. A security's beta is a measure of the volatility of that security relative to the chosen index. For instance, a beta of 2 indicates that the security in question is twice as volatile as the selected index and, hence, is twice as risky. In this sense, beta is a measure of the security's systematic risk – risk that cannot be diversified away, that is, the risk endemic to the system in which the security resides.

Event studies construct a linear model of the type described above to measure the impact of a given event on the security's returns. The model normally uses daily returns for a period of about 200 days prior to the designated event. The sample period normally ends approximately 30 days before the event to allow for any leakage of news immediately prior to the event. The differences between the actual and estimated returns over a 20 day period surrounding the event measures the impact of the event on the security's returns. Good (bad) news is usually the presence of positive (negative) and statistically significant post-event abnormal returns. For a more thorough discussion and statistical analysis of the evolution of crime betas see Cloninger and Marchesini (1995a) and (1995b).

The 'events' identified in the present study are the back-to-back significant changes in the *incidence* of executions in Texas. Even though the Texas Court of Criminal Appeals issued the stay in *Davis* on 2 January 1996 and vacated that stay on 18 December 1996, the first 'event' did not occur until April 1996 when scheduled executions decreased to near zero. The second 'event' occurred in April 1997 when six executions took place followed by eight each in the succeeding two months. The derived model is used to estimate the changes in the number of Texas homicides for each month over the 24 month period of January 1996 to December 1997. These estimated or expected changes are then compared with the actual changes bearing in mind that the number of executions was three (one-sixth of the norm) from 1 March 1996 to 31 March 1997 and 35 (double the expected or normal annual rate) from 1 April 1997 to 31 December 31 1997 or a 12 fold difference between the two adjoining periods.

The specific hypothesis tested is the null hypothesis that changes in the incidence of homicides in Texas are not significantly associated with corresponding changes in the incidence of executions in Texas. If no deterrence effect of executions exists, the differences between actual and expected changes in Texas homicides should be random and insignificant over the two periods. However, if a deterrence effect does exist, then during the period of significantly fewer executions (when changes in the incidence of homicides in both the USA and Texas were *negative*) the actual changes in Texas homicides would be significantly less than the expected changes. During the period of significantly more executions the actual changes in Texas homicides would be significantly greater (more negative) than the expected changes or, not significantly different.

III. EMPIRICAL RESULTS

Computing 12 month percentage changes (on a month by month basis) in both the incidence of US and Texas homicides produced 72 seasonally adjusted monthly observations spanning from 1 January 1990 to 31 December 1995. Regressing Texas homicide changes on US homicide changes (in decimal form) produces the following linear equation:

$$\underset{}{\delta H_{\text{tx}}} = \underset{(-1.5)}{-0.0249} + \underset{(7.30)}{1.419}\delta H_{\text{us}} + e \qquad (2)$$

where the number in parentheses represent the t values of the respective coefficients. The coefficient of determination (r^2) is 0.432 and the standard error of the estimate is 0.141. The beta coefficient and the t value are highly significant with the probability of making a type I error less than 0.00001. Not only is the beta coefficient significantly different from zero, it is also significantly different from 1.0 ($t = 2.16$). A beta value of 0 would indicate that homicide changes in Texas are independent of US homicide changes. A beta of 1.0 would indicate that homicide changes in Texas vary in the same direction and amount as the US homicide changes. In the sample period, the derived beta coefficient indicates that homicide changes in Texas vary 1.42 times the corresponding US changes. In other words, the changes in homicide in Texas are 1.42 times greater than those for the US and move in the same direction.

The fact that the changes in US homicides include the changes in Texas homicides contributes to the correlation between US and Texas changes in homicide and could bias the parameters. Texas homicides account for just over 10% of the US total number of homicides. A separate regression using a US homicide index that *excludes* Texas yields parameters that are not significantly different (−0.0264 and 1.09 respectively) from those including Texas although, as expected, the coefficient of determination drops to 0.25 and the standard error of the regression rises to 0.1625. Both regression coefficients remain significant.

Table 1 presents the estimates of δH_{tx} (column 4) by substituting actual values for δH_{us} into Equation (2) during the 24 month period January 1996 to December 1997. The differences between actual Texas homicide changes and those estimated by the model (column 5) are divided by the standard error of the estimate to yield the respective t values (column 6). The number of actual monthly executions in Texas appears in column 7.

Table 1. *Actual versus estimated changes in Texas homicides – full sample, no adjustments*

Month	USHo	AcTXHo	EsTXHo	Act-Est	t stat	TXExe
01/96	−0.1604	−0.1088	−0.2526	0.1437	1.022	0
02/96	−0.0854	−0.1367	−0.1461	0.0094	0.0667	2
03/96	−0.2038	−0.2273	−0.3142	0.0870	0.6184	0
04/96	−0.2857	−0.3796	−0.4305	0.0509	0.3620	0
05/96	−0.164	0.1880	−0.2577	0.4457	**3.170**	0
06/96	−0.1356	−0.1569	−0.2174	0.0576	0.4099	0
07/96	−0.2002	−0.2635	−0.3091	0.0457	0.3247	0
08/96	−0.2604	−0.1273	−0.3946	0.2673	**1.901**	0
09/96	−0.2388	−0.0859	−0.3638	0.2780	**1.976**	1
10/96	−0.2258	−0.0076	−0.3454	0.3378	**2.402**	0
11/96	−0.1911	−0.2047	−0.2962	0.0914	0.6502	0
12/96	−0.1721	0.0672	−0.2692	0.3364	**2.392**	0
01/97	−0.0552	−0.2137	−0.1033	−0.1105	−0.7855	0
02/97	−0.1125	−0.1917	−0.1846	−0.0071	−0.0503	1
03/97	0.061	0.1961	0.0617	0.1344	0.9558	1
04/97	−0.0339	0.2	−0.073	0.2730	**1.942**	6
05/97	−0.0863	−0.3291	−0.1475	−0.1817	−1.292	8
06/97	−0.07	−0.0496	−0.1242	0.0746	0.5306	8
07/97	−0.0352	−0.0407	−0.0749	0.0343	0.2437	1
08/97	−0.0914	−0.0278	−0.1547	0.1269	0.9025	0
09/97	−0.051	−0.0171	−0.0972	0.0801	0.5699	4
10/97	−0.0435	−0.313	−0.0867	−0.2263	**−1.610**	3
11/97	−0.0429	0	−0.0858	0.0858	0.6099	4
12/97	−0.0846	−0.1748	−0.145	−0.0298	−0.2121	1

As can be seen in Table 1 all 12 months during 1996 have positive differences indicating that the actual negative changes in Texas homicides were less than the changes predicted by the empirical model. Recall that the changes in US homicides were negative during the entire 24 month period with the single exception of March 1997. In the 1 March 1996 to 31 March 1997 period six monthly differences are positive and statistically significant with five clearly falling into the period of reduced executions. The sixth (April 1997) falls just outside this period and could be interpreted as the end of a cumulative effect of reduced incidence of executions. The difference for April 1997 is the only significant difference in 1997 in this data set. The differences appear to revert to a normal pattern (insignificant differences with zero mean) after April 1997.

The tentative conclusions that can be drawn from Table 1 are:

(1) First, the period 1996–1997 is characterized by persistently declining incidence of homicide both in the USA and Texas.
(2) Second, assuming the period of reduced incidence of executions is 1 March 1996 to 31 March 1997, five of the intervening months show a significant positive difference between actual and expected changes in the incidence of homicides in Texas. If allowance is made for a carryover effect of reduced executions, the month of April 1997 can be added to that list.
(3) Third, subsequent to April 1997, the differences appear to revert to a normal pattern of positive and negative insignificant values.

The above results stem from the full sample of 72 seasonally adjusted monthly observations. There exists, however, two potential statistical problems with the full unaltered sample. First, in the Texas data there are two notable outliers – observations that are more than three standard deviations from the mean. These outliers cause the Texas changes to violate the normality presumption required for nonbiased linear correlations. Eliminating these two observations in both the US and Texas series produces variables whose probability distributions do not vary significantly from a normal distribution at any conventional level of significance.

The differences in the statistical results between the two data sets are subtle but potentially profound. The regression equation using the reduced sample (70 observations) is,

$$\delta H_{\mathrm{tx}} = -0.03565 + 1.158(\delta H_{\mathrm{us}}) + e$$
$$(-2.441) \qquad (6.515)$$

and the standard error of the regression is 0.1221 while the coefficient of determination is 0.384. In this case, the constant term is significant as is the slope and the coefficient of determination. The slope term is not significantly different from 1.0 or 1.419 (the corresponding beta value in the full sample). Table 2 provides the same analysis as Table 1.

Table 2. *Actual versus estimated changes in Texas homicides – reduced sample*

Month	USHo	AcTXHo	EsTXHo	Act-Est	t stat	TXExe
01/96	−0.1604	−0.1088	−0.2214	0.1126	0.9219	0
02/96	−0.0854	−0.1367	−0.1238	−0.0129	0.1059	2
03/96	−0.2038	−0.2273	−0.2610	0.0337	0.2758	0
04/96	−0.2857	−0.3796	−0.3558	−0.0238	−0.1948	0
05/96	−0.164	0.1880	−0.2149	0.4028	**3.299**	0
06/96	−0.1356	−0.1569	−0.1819	0.0222	0.1817	0
07/96	−0.2002	−0.2635	−0.2568	−0.0067	−0.0548	0
08/96	−0.2604	−0.1273	−0.3265	0.1992	**1.632**	0
09/96	−0.2388	−0.0859	−0.3014	0.2155	**1.765**	1
10/96	−0.2258	−0.0076	−0.2864	0.2788	**2.283**	0
11/96	−0.1911	−0.2047	−0.2462	0.0415	0.3397	0
12/96	−0.1721	0.0672	−0.2242	0.2914	**2.386**	0
01/97	−0.0552	−0.2137	−0.0888	−0.1249	−1.023	0
02/97	−0.1125	−0.1917	−0.1552	−0.0365	−0.299	1
03/97	0.061	0.1961	0.0457	0.1503	1.231	1
04/97	−0.0339	0.2	−0.0642	0.2642	**2.164**	6
05/97	−0.0863	−0.3291	−0.1249	−0.2042	**−1.673**	8
06/97	−0.07	−0.0496	−0.1059	0.0563	0.4613	8
07/97	−0.0352	−0.0407	−0.0657	0.0250	0.2052	1
08/97	−0.0914	−0.0278	−0.1308	0.1030	0.8436	0
09/97	−0.051	−0.0171	−0.0839	0.0668	0.5472	4
10/97	−0.0435	−0.313	−0.0753	−0.2377	**−1.947**	3
11/97	−0.0429	0	−0.0746	0.0746	0.6106	4
12/97	−0.0846	−0.1748	−0.1223	−0.0519	−0.4254	1

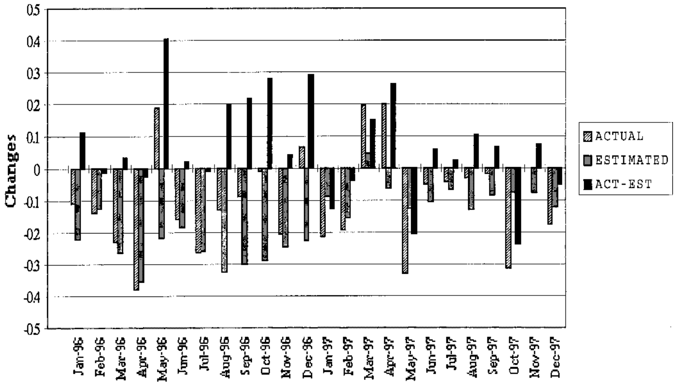

Fig. 1. *Actual versus estimated changes in Texas homicides: reduced sample (includes Texas in the index)*

Figure 1 graphically depicts the data provided in Table 2. Tables 1 and 2 yield similar results with the following important exceptions:

(1) as the number of executions changes from near zero to six and eight for the March to May 1997 time frame, the differences between actual and expected changes in Texas homicides go from significantly positive for April 1997 to significantly negative in May 1997; and
(2) the months of May and October 1997 exhibit significantly negative differences.

When this analysis is repeated using a US homicide index that excludes Texas, the results are similar. During the period of reduced executions six monthly differences are positive and significant at the 0.05 level. The monthly differences for the period of higher than normal executions appear random with two monthly differences negative and significant. As with the reduced sample (inclusive of Texas) the monthly differences go from significantly positive in April 1997 to significantly negative in May 1997 with another significantly negative difference in October 1997. The other post April 1997 differences are insignificant with varying signs.

The second potential statistical problem with the original full, unaltered sample is possible autocorrelation of the residuals. The Durbin–Watson statistic (1.25) in all derived models reveals the presence of positive autocorrelation of the residuals. Because the data are in the form of first differences, an additional correction for autocorrelation is provided by including on the right hand side of the equation lags of the dependent variable for the three periods evincing significant lagged correlations. This regression is also highly significant with a coefficient of determination of 0.508 and a Durbin–Watson statistic of 1.84 (the null hypothesis of no autocorrelation cannot be rejected). Table 3 repeats the format of Tables 1 and 2 and employs the full sample regression model (with corrections for autocorrelation) to estimate changes in Texas homicides.

Correcting for positive autocorrelation increases the significance of the differences between actual and estimated changes in Texas homicides. As can be seen in Table 3, there are seven positive monthly differences that are significant in the period of reduced executions and two negative monthly differences in the period of increased executions

Table 3. *Actual versus estimated Texas homicides – full sample, adjusted for autocorrelation*

Month	USHo	AcTXHo	EsTXHo	Act-Est	t stat	TXExe
01/96	−0.1604	−0.1088	−0.2596	0.1507	1.341	0
02/96	−0.0854	−0.1367	−0.1535	0.0168	0.1492	2
03/96	−0.2038	−0.2273	−0.2741	0.0468	0.4162	0
04/96	−0.2857	−0.3796	−0.3837	0.0042	0.0371	0
05/96	−0.164	0.1880	−0.2974	0.4854	**4.318**	0
06/96	−0.1356	−0.1569	−0.2029	0.0432	0.3840	0
07/96	−0.2002	−0.2635	−0.2576	−0.0059	−0.0523	0
08/96	−0.2604	−0.1273	−0.3404	0.2131	**1.896**	0
09/96	−0.2388	−0.0859	−0.3411	0.2552	**2.270**	1
10/96	−0.2258	−0.0076	−0.3092	0.3016	**2.683**	0
11/96	−0.1911	−0.2047	−0.2412	0.0365	−0.3244	0
12/96	−0.1721	0.0672	−0.2389	0.3061	**2.723**	0
01/97	−0.0552	−0.2137	−0.0929	−0.1209	−1.075	0
02/97	−0.1125	−0.1917	−0.1770	−0.0146	−0.1303	1
03/97	0.061	0.1961	−0.0089	0.2049	**1.823**	1
04/97	−0.0339	0.2	−0.0651	0.2651	**2.358**	6
05/97	−0.0863	−0.3291	−0.0593	−0.2698	−**2.400**	8
06/97	−0.07	−0.0496	−0.0961	0.0466	0.4141	8
07/97	−0.0352	−0.0407	−0.0918	0.0511	0.4550	1
08/97	−0.0914	−0.0278	−0.1543	0.1265	1.125	0
09/97	−0.051	−0.0171	−0.0834	0.0663	0.5900	4
10/97	−0.0435	−0.313	−0.0710	−0.2419	−**2.152**	3
11/97	−0.0429	0	−0.1174	0.1174	1.045	4
12/97	−0.0846	−0.1748	−0.1522	−0.0226	−0.2011	1

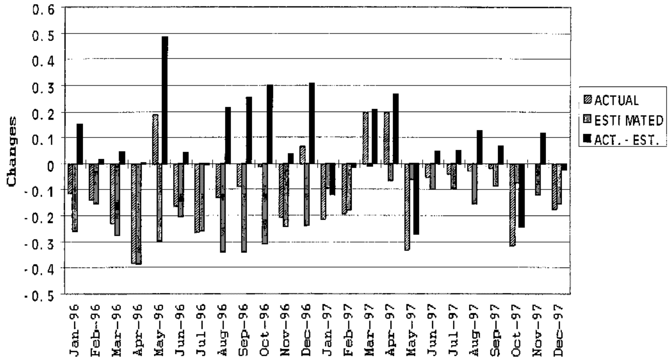

Fig. 2. *Actual versus estimated changes in Texas homicides (Texas included in index): adjusted for autocorrelation*

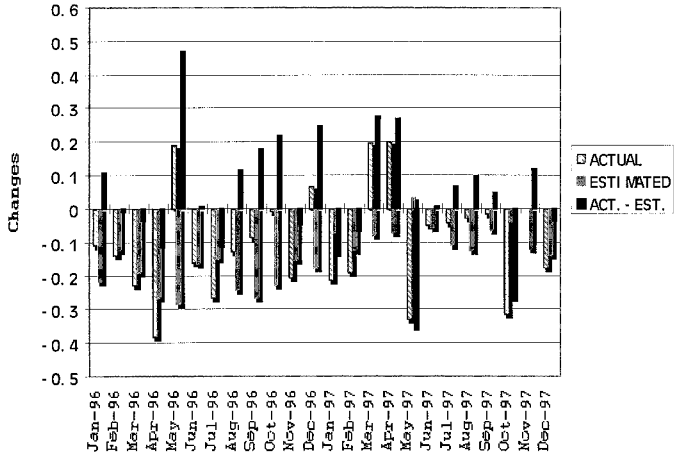

Fig. 3. *Actual versus estimated changes in Texas homicides (Texas excluded from index): adjusted for autocorrelation*

including one that immediately follows the two successive significantly positive differences in the March to May 1997 time period. These results parallel those of Tables 1 and 2. Figure 2 graphically displays the data provided in Table 3. Correcting for positive autocorrelation in the model that excludes Texas from the US index produces similar results as shown in Table 4 and graphically displayed in Fig. 3. It appears, therefore, that including (or excluding) Texas from the US index does not significantly alter the conclusions.

IV. ANALYSIS

The presence of a deterrence effect of executions would generate similar results to those outlined above although uniform positive significant differences during the reduced period of executions would more clearly make the case to reject the null hypothesis. The null hypothesis can be rejected with little likelihood of a Type I error for as many as seven of the 14 monthly results surrounding the period of reduced executions.

In the full sample model, the differences after April 1997 are not negative and significant indicating that doubling the number of executions did not produce any greater deterrent effect than the normal (expected) number of executions. That is, this data set provides no evidence that the marginal deterrence effect of an additional execu-

Table 4. *Actual versus estimated changes – excluding Texas and adjusting for autocorrelation*

Month	USHo	AcTXHo	EsTXHo	Act-Est	*t* stat	TXExe
01/96	−0.1656	−0.1088	−0.2160	0.1072	0.9568	0
02/96	−0.0793	−0.1367	−0.1238	−0.0128	−0.1147	2
03/96	−0.2015	−0.2273	−0.1895	−0.0378	−0.3373	0
04/96	−0.277	−0.3796	−0.2636	−0.1159	−1.035	0
05/96	−0.1977	0.1880	−0.2821	0.4701	**4.197**	0
06/96	−0.1331	−0.1569	−0.1651	0.0054	0.0482	0
07/96	−0.1934	−0.2635	−0.1479	−0.1156	−1.032	0
08/96	−0.2731	−0.1273	−0.2437	0.1165	1.039	0
09/96	−0.2516	−0.0859	−0.2650	0.1791	1.599	1
10/96	−0.2442	−0.0076	−0.2275	0.2199	**1.963**	0
11/96	−0.1899	−0.2047	−0.1544	−0.0504	−0.4496	0
12/96	−0.1932	0.0672	−0.1770	0.2441	**2.18**	0
01/97	−0.038	−0.2137	−0.0723	−0.1414	−1.263	0
02/97	−0.1037	−0.1917	−0.1239	−0.0677	−0.6047	1
03/97	0.048	0.1961	−0.0796	0.2757	**2.461**	1
04/97	−0.0526	0.2	−0.0694	0.2694	**2.405**	6
05/97	−0.052	−0.3291	0.0345	−0.3636	**−3.247**	8
06/97	−0.0719	−0.0496	−0.0556	0.0060	0.0538	8
07/97	−0.0347	−0.0407	−0.1072	0.0665	0.5941	1
08/97	−0.0987	−0.0278	−0.1249	0.0971	0.8672	0
09/97	−0.0544	−0.0171	−0.0653	0.0482	0.4303	4
10/97	−0.0136	−0.313	−0.0363	−0.2767	**−2.470**	3
11/97	−0.0467	0	−0.1185	0.1185	1.058	4
12/97	−0.0741	−0.1748	−0.1365	−0.0383	−0.3424	1

tion above the norm is anything other than zero. However, the reduced sample model produces two significantly negative differences in the post April 1997 period including one during the month (May) immediately following the sudden increase in the number of executions from near zero to six and then eight. This sudden coincident reversal of the sign and significance is in a direction and magnitude consistent with the deterrent hypothesis. More importantly, correcting for positive autocorrelation in all the models (including those that do not include Texas homicides in the US homicide series) strengthens the significance of the monthly differences.

Assuming that deterrence exists in Texas in a manner and amount suggested by this empirical evidence, it is possible to estimate the additional homicides that occurred as a result of the suspension of executions from March 1996 to April 1997. In Table 5, the differences given in column 5 of Table 3 (full sample model corrected for autocorrelation) are repeated in column 2 and are multiplied by the number of homicides in the corresponding (base) month 12 months earlier (column 3) to yield an estimate of the number of homicides (column 4) that occurred because the operative deterrent effect established from 1990 to 1995 was, for the most part, lost. Column 5 gives the cumulative number of such homicides over the respective number of months. The cumulative effect peaks at 249 in April 1997 and remains at 221 eight months after the sudden increase in executions.

In this fashion, the lost deterrent effect due to the significantly reduced incidence of capital punishment produces a public health issue in the form of a net preventable loss of human life. It could be argued that the additional homicides should be offset by the saving of human life through reduced executions. Recall, however, that the 14 lives saved by the dearth of executions from March 1996 to March 1997 are more than offset by the 20 'extra' executions between April to December 1997. Thus, there are no net lives spared by the temporary suspension of executions – only additional homicides created during the hiatus.

This analysis demonstrates the ethical dilemma of capital punishment. If deterrence does exist, then electing to forgo capital punishment is not without potentially serious consequences – the increased incidence of homicide. This outcome produces a serious moral issue for consequential (teleological) based ethical systems, e.g. utilitarianism. The consequences (additional homicides) of not using capital punishment are potentially significantly greater than the corresponding loss of life by execution. That is, society, by employing capital punishment, experiences a net gain in the form of preventable loss of human life if a deterrent effect exists.

The consequences of not using capital punishment is irrelevant to nonconsequential (deontological) based moral systems such as that of Immanual Kant and most of the world's organized religions. In these systems the moral obligation is to respect and preserve human life whenever possible (such as by the cessation of capital pun-

Table 5. *Effect on Texas homicides of varying executions – adjusted for autocorrelation*

Month	Act-Exp	12 mth prior Hom	Additional Hom	Cum Hom
01/96	0.1507	147	—	—
02/96	0.0168	139	—	—
03/96	0.0468	132	—	—
04/96	0.0042	137	0.57	0.57
05/96	**0.4854**	133	64.55	65.13
06/96	0.0431	144	6.22	71.34
07/96	−0.0059	167	−0.98	70.36
08/96	**0.2131**	165	35.16	105.52
09/96	**0.2552**	128	32.66	138.18
10/96	**0.3016**	132	39.81	177.99
11/96	0.0365	127	4.63	182.62
12/96	**0.3061**	134	41.02	223.64
01/97	−0.1209	131	−15.83	207.81
02/97	−0.0146	120	−1.76	206.04
03/97	**0.2049**	102	20.90	226.95
04/97	**0.2651**	85	22.53	249.48
05/97	**−0.2698**	158	−42.63	206.86
06/97	0.0466	121	5.63	212.49
07/97	0.0511	123	6.29	218.78
08/97	0.1265	144	18.22	237.00
09/97	0.0663	117	7.75	244.76
10/97	**−0.2419**	131	−31.69	213.06
11/97	0.1174	101	11.86	224.93
12/97	−0.0226	143	−3.23	221.69

ishment) regardless of the consequences. Much of the debate over the morality of capital punishment reflects the differences between these two moral systems. For the most part, societies find it difficult to rely entirely on one or the other of these systems for all their moral decisions. Fortunately, on many issues the two systems agree. However, on a significant few, like capital punishment and abortion, they produce conflicting and contested standards.

V. FINAL REFLECTIONS

The empirical results suggest the following conclusions:

(1) Significant changes in the number of homicides appear associated with sudden changes in the number of executions in a manner consistent with the deterrence hypothesis.
(2) The changes in homicide corresponding to the changes in executions appear closely coincidental.
(3) The discernible deterrent effect appears to have a short memory, that is, it seems to be subject to the 'what have you done for me lately' syndrome. From an economic point of view, potential murderers appear to form their expectations of the risk of execution rationally rather than adaptively.
(4) Significantly more executions than expected appear to have a declining deterrent effect. That is, the deterrent effect may be subject to diminishing returns.
(5) Excluding Texas homicides from the US Homicide index, eliminating outliers, or adjusting for positive autocorrelation strengthens these conclusions.

Any single empirical study, including the present one, is subject to honest criticism. The perfect empirical study remains elusive to scholars of all disciplines. A morally contested issue like the deterrence effect of capital punishment attracts criticism that other less contested issues eludes. Moreover, studies such as the present one that rely on inductive statistical analysis cannot prove a given hypothesis correct. They can only find evidence that is or is not consistent with a given hypothesis. In this vein, the present study joins a growing list that finds evidence that is consistent with the deterrent hypothesis. However, the present study does not prove that deterrence exists. On the other hand, the failure of some studies to find evidence consistent with the deterrence hypothesis does not prove that deterrence is inoperative.

If this were the only study to find evidence of deterrence, then the scrutiny that it will undoubtedly attract could cast some doubt upon its conclusions. However, this study is but another on a growing list of empirical work that finds evidence consistent with the deterrence hypothesis. These studies as a whole provide robust evidence – evidence obtained from a variety of different models, data sets and methodologies that yield the same conclusion. It is the cumulative effect of these studies that causes any neutral observer pause.

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