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Econometric Estimates of Deterrence of the Death Penalty: Facts or Ideology

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Econometric Estimates of Deterrence of the Death Penalty:

Facts or Ideology[1]

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Abstract

In 2007, the Wall Street Journal published an article claiming that each execution saves more than 70 lives. This example is used to show how easy it is, using simple or advanced econometric techniques, to produce results that do or do not support the deterrence hypothesis. Moreover, we also point to some puzzles which have not been satisfactorily solved so far. We then present a critical survey of the papers published in the last ten years. It is shown how simple changes can produce quite different results using the same data. Finally, we draw some conclusions about the usefulness of statistical arguments in policy debates, but also on the moral questions involved in this particular debate.

Keywords

Death Penalty, Deterrence, Econometric Evidence, Ideology.

JEL Classification

K14; K42

1 Introduction

[1] On November 2, 2007, R.D. ADLER and M. SUMMERS published an article in the *Wall Street Journal* with the title "Capital Punishment Works".[1] With reference to the diagram in *Figure 1* below, they state that over the period considered, from 1979 to 2004, "each execution seems to be associated with 71 fewer murders in the year the execution took place", and that this association was significant at the 0.05 percent level. Acknowledging that there might be a problem of causality because some murders took place in the years of observation before the executions, they lagged the executions' variable and got an even stronger result: "Each execution was associated with 74 fewer murders the following year", statistically significant even at the 0.03 percent level. Their defence of the death penalty ended in the following (rhetorical) question: "Do we save this particular life at the cost of the lives of dozens of future murder victims?"

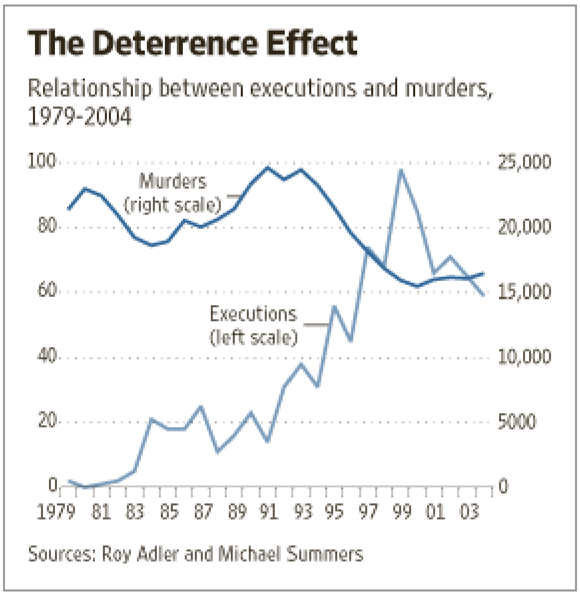

Figure 1: Executions and homicides, U.S.A., 1979 – 2004

[2] Aside from this figure, the two authors do not provide any information about how they derive these results. As will be shown below, these are results of OLS regressions that are insufficient according to any standard of conventional econometric techniques. This is the more astonishing as, during recent years, there has been a rather intense debate on this issue, and questions of the appropriate econometric methodology played a major part in this debate.[2] Neither of these two authors participated in it. Moreover, the second author, M. SUM-

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1. http://online.wsj.com/article_email/SB119397079767680173-lMyQjAxMDE3OTAzMjkwNzIwWj.html (10/03/09).

2. See, for example, H. DEZHBAKHSH, P. RUBIN and J.M. SHEPHERD (2003), J.J. DONOHUE and J. WOLFERS (2005, 2009), L. KATZ, S.D. LEVITT and E. SHUSTOROVICH (2003), H.N. MOCAN and R.K. GITTINGS (2003), J.M. SHEPHERD (2004, 2005), P.R. ZIMMERMANN (2004, 2006, 2009), as well as the discussion in *Economists' Voice*, issue 3/5 (April 2006) between J.J. DONOHUE and J. WOLFERS (2006, 2006a) and P.H. RUBIN (2006, 2006a).

MERS, is said to be a professor of quantitative methods.[3] But without taking any methodological problems into account and neglecting all that has been written before, publishing such a paper in the *Wall Street Journal* might be a better way to get a large audience and corresponding response from the general public than having scientific scruples.

[3] The 'economic' discussion about the possible deterrent effect of the death penalty started with the seminal paper by I. EHRLICH (1975), which itself was a response to a famous book by T. SELLIN (1959). Both used quite different statistical approaches.[4] And while T. SELLIN (1959) did not find a significant effect, I. EHRLICH (1975), using a time series of annual data from 1933 to 1967, found a "pure deterrent effect" and concluded that "on average the trade-off between the execution of an offender and the lives of potential victims it might have saved was of the order of magnitude of 1 for 8" (p. 398) for this observation period.

[4] This paper was the starting point for a huge debate in the late seventies and eighties. S. CAMERON (1994, p. 197f.) speaks of two generations of papers, the first one published between 1975 and 1978, i.e. around the time when the moratorium on capital punishment was lifted in the United States, and the second one after 1982, covering the time after the moratorium. The data used were partly time series, and partly cross section data, and all but a few studies, which looked at Canada or the United Kingdom, used U.S. data.[5] The results are rather mixed. In particular, they proved the fragility of the results of I. EHRLICH (1975) with respect to the specification of the test equation as well as the time period employed. Thus, while the overall results do not support his conclusion, they also do not give strong support to the opposite hypothesis. All that can be said is that the empirical evidence provided in this research does not lead to any strong conclusion.[6]

[5] In recent years, a third generation of papers has been published on this topic.[7] The main difference to the first two waves is that now panel data are used, mostly for the U.S. states, but partly also for U.S. counties.[8] This allows to distinguish between those states with and those without the death penalty, i.e. it avoids an aggregation bias when using time series data resulting from mixing these two categories, which might bias the estimated coefficients (and t-statistics) downwards. It can also take into account unobserved heterogeneity between the states which is not allowed for when using cross-section data. The use of state (or county) fixed effects to take account of this unobserved heterogeneity might, on the other hand, also create a bias.

3. Actually, he is Professor of Management Science at Pepperdine University, but is also teaching statistics and quantitative methods.

4. For a comparison of the two approaches see, for example, D.C. BALDUS and J.W. COLE (1975).

5. Studies for Canada are, for example, K.L. AVIO (1979) or S. LAYSON (1983), studies for the United Kingdom are K.I. WOLPIN (1978, 1978a).

6. For an overview of these results see, for example, the two surveys by S. CAMERON (1988, 1994).

7. See the literature mentioned in Footnote 2 above and discussed in *Section 3* of this paper.

8. In recent years, there are only few studies using cross-section of time series data. See, for example, P.K. NARAYAN and R. SMYTH (2006) or R. HJALMARSSON (2009).

[6] The results of this recent debate are, again, rather mixed. The old divide between the faculties which already characterised the earlier debates remains; while most (or perhaps nearly all) (U.S.) economists believe in the validity of the deterrence hypothesis and present corresponding results, most, but not all, scientists from other faculties, in particular law, have serious doubts on its validity and demonstrate the fragility of the results presented by economists. However, contrary to earlier discussions, the econometric methods used are the same: Today, non-economists are able to use the same advanced statistical techniques equally well as economists. Correspondingly, some of the discussions are about methodological problems, especially on the reliability of the data and the quality of the instruments used in instrumental variables estimations.

[7] The debate about the deterrence effect is, however, just one of three such debates in the United States, where mainly ideology seems to drive the empirical results. The second one is about gun prevalence and homicides,[9] a discussion that is also relevant to Switzerland.[10] The third one is whether legalised abortion reduced crime.[11] All three debates are highly politicised, and one could get the impression that in all three debates the authors mainly try to find scientific arguments to support their political convictions. This is not illegitimate, but it can easily lead to selective perceptions of the reality.

[8] One can really gain the impression that the economics of crime and the question whether death penalty deters murders or not is just a romping place for ideologists. However, that prior beliefs might have an impact on the reported results on the deterrent effect of death penalty has already been discussed by W.S. MCMANUS (1985). This does not imply that authors are necessarily shirking; it might simply be the result of selective perceptions: if contradictory results can be derived, authors choose those in the validity of which they are convinced a priori and are looking for strong arguments in order to support these results.

[9] In the following, we first take the data and methodology used by R.D. ADLER and M. SUMMERS mentioned above and show how easy it is to produce contradictory results (*Section 2*). From a methodological point of view, this goes even beyond what I. EHRLICH (1975) did when starting the economic discussion about this topic. However, due to the publication in the *Wall Street Journal*, the results of these two authors did get a far wider audience in the general public than those of all other authors, and the simplicity of this approach makes it very easy to demonstrate the possibility of manipulations that are equally possible if we would employ more advanced techniques. Moreover, in this section we also point to some puzzles in

9. See, for example, I. AYRES and J.J. DONOHUE (2003, 2003a, 2009, 2009a), P.J. COOK and J. LUDWIG (2006), M. DUGGAN (2001), T. KOVANDZIC, M.E. SCHAFFER and G. KLECK (2008), F. PLASSMANN and J. WHITLEY (2003), C. MOODY and TH.B. MARVELL (2008, 2009) or L. STOLZENBERG and S.J. D'ALESSIO (2000).

10. In Switzerland, people serving in the army usually have their military guns or handguns at home even after they have definitely finished their military service. However, a considerable proportion of homicides and suicides are performed by using these weapons, and – in international comparison – Switzerland has a relatively high rate of gun suicides and homicides. Nevertheless, in February 2011, a popular initiative to severely restrict having military guns at home failed.

11. See, for example, J.J. DONOHUE and S. LEVITT (2001, 2004, 2008), C.L. FOOT and C.F. GOETZ (2008), T. JOYCE (2004, 2009, 2009a) and J.R. LOTT and J. WHITLEY (2007).

the data which have not been satisfactorily solved so far. *Section 3* presents a critical survey of the third round papers including some summary statistics. It is shown how simple changes can produce quite opposite results using the same data. In the final *Section 4* we draw some conclusions about the usefulness of statistical arguments in policy debates, but also on the moral questions involved in this particular debate. The reason for the latter discussion is that, in contrast to all earlier papers where those authors defending the deterrent effect of capital punishment are keen to mention that this does not imply that this kind of punishment is justified,[12)] some authors now claim that due to its deterrent effect capital punishment might even be morally required.[13)]

2 Some Stylised Facts and Easy Estimates

[10] The results of R.D. ADLER and M. SUMMERS are derived from a simple OLS regression.[14)] Using contemporaneous execution data we get for the period from 1979 to 2004:[15)]

(1)
$$HOM_t = \underset{(33.90)}{22614} - \underset{(-4.98)}{71.880}\, EXEC_t + u_t$$

$$\bar{R}^2 = 0.487,\ D.\text{-}W. = 0.338,\ Q(4) = 37.926^{***},\ AIC = 18.215,$$

with

HOM homicides per 100'000 inhabitants,

EXEC number of executions.

The values of the Durbin-Watson test as well as of the Q-statistic indicate that the estimated residuals are highly correlated. Further examination of the residuals indicates second order autocorrelation. Thus, even if the true relation would be a simple bivariate one, the significance of the estimated parameters would be highly dubious. The easiest way to take this into account is to perform a second order Cochrane-Orcutt transformation. This leads to the following estimates:

(2)
$$HOM_t = \underset{(14.11)}{22246} - \underset{(-1.27)}{15.378}\, EXEC_t + u_t$$

$$u_t = \underset{(8.21)}{1.459}\, u_{t-1} - \underset{(-3.32)}{0.605}\, u_{t-2} + \varepsilon_t,$$

12. See, for example, I. EHRLICH (1975, p. 416), but also J. SHEPHERD (2009)

13. See the discussion between C.S. SUNSTEIN and A. VERMEULE (2005, 2005a), C.S. STREIKER (2005) and D.R. WILLIAMS (2006) mentioned in *Section 4* below.

14. Similar regressions are performed by H. DEZHBAKHSH and J.M. SHEPHERD (2006) for the period from 1960 to 2000 and the two sub-periods from 1960 to 1976 and from 1977 to 2000 to get “exploratory evidence” (p. 518).

15. The numbers in parentheses are the estimated t-statistics. D.-W. is the result of the Durbin-Watson test for autocorrelation of the residuals, Q(k) the Box-Ljung Q-statistic with k degrees of freedom, AIC the value of the Akaike criterion. '***', '**', '*' or ('*') denote significance at the 0.1, 1. 5, or 10 percent levels, respectively. The results are derived with EViews, Version 5.1. The sources of the data are given in the *Appendix*.

$$\bar{R}^2 = 0.886,\ \text{D.-W.} = 1.957,\ Q(2) = 1.762,\ \text{AIC} = 16.782.$$

According to all statistical criteria, this seems to be a much more reliable equation. However, the estimated coefficient of the number of executions is much smaller and no longer statistically significant at any conventional significance level.

[11] Another possibility to take the autocorrelation into account is to include lagged dependent variables. This leads to the following result:

(3)
$$\underset{}{HOM_t} = \underset{(2.71)}{5040} + \underset{(6.63)}{1.262}\, HOM_{t-1} - \underset{(-2.79)}{0.484}\, HOM_{t-2} - \underset{(-2.08)}{17.930}\, EXEC_t + u_t$$

$$\bar{R}^2 = 0.897,\ \text{D.-W.} = 1.878,\ Q(2) = 0.789,\ \text{AIC} = 16.674,$$

with the following long-run solution:

(3a)
$$HOM = \underset{(15.85)}{22685} - \underset{(-2.37)}{80.707}\, EXEC .$$

Again, according to the statistical criteria, this is a much more reliable result than equation (1). It is even slightly better than equation (2), though the differences are very small and would never prove to be statistically significant if a formal test were to be applied. On the other hand, the estimated long-run effect of the execution variable is now even slightly higher than in equation (1), and both coefficients are significantly different from zero at the five percent level. Thus, it is possible to get very different results for two plausible specifications for the autocorrelation of the error term in model (1) that are very close together according to conventional statistical criteria.

[12] We get quite similar results if we follow the suggestion of R.D. ADLER and M. SUMMERS to use lagged values of the execution variable in order to take account of the time delay between cause and effect. Using again OLS we get:

(1')
$$HOM_t = \underset{(36.42)}{22479} - \underset{(-5.26)}{72.722}\, EXEC_{t-1} + u_t$$

$$\bar{R}^2 = 0.517,\ \text{D.-W.} = 0.516,\ Q(4) = 28.687^{***},\ \text{AIC} = 18.158.$$

The coefficient of the execution variable and its significance are now somewhat higher, but there is still considerable autocorrelation in the estimated residuals. Performing a second order Cochrane-Orcutt transformation we get:

(2')
$$HOM_t = \underset{(10.534)}{19161} + \underset{(0.89)}{10.490}\, EXEC_{t-1} + u_t$$

$$u_t = \underset{(9.12)}{1.533}\, u_{t-1} - \underset{(-3.75)}{0.650}\, u_{t-2} + \varepsilon_t ,$$

$$\bar{R}^2 = 0.881,\ \text{D.-W.} = 1.922,\ Q(2) = 0.579,\ \text{AIC} = 16.821.$$

Again, according to all statistical criteria, this seems to be a much more reliable equation. But the estimated coefficient of the number of execution is now even positive but, of course, not statistically significant at any conventional significance level. Nevertheless, this estimate would rather support the brutalisation than the deterrence hypothesis.

[13] As before, we get quite different results if we include lagged dependent variables. Then we get:

$$\text{(3')} \qquad HOM_t = \underset{(2.36)}{4960} + \underset{(7.03)}{1.329}\, HOM_{t-1} - \underset{(-3.24)}{0.555}\, HOM_{t-2} - \underset{(-1.61)}{14.728}\, EXEC_{t-1} + u_t$$

$$\bar{R}^2 = 0.890,\ \text{D.-W.} = 2.039,\ Q(2) = 0.942,\ AIC = 16.742,$$

with the following long-run solution:

$$\text{(3a')} \qquad HOM = \underset{(16.374)}{22017} - \underset{(-2.09)}{65.372}\, EXEC\ .$$

In this estimate, the coefficient of the execution variable has still its negative sign but its significance is below the 10 percent level. In the long-run equation, it is, however, still significant at the five percent level. Thus, we find again the situation that, taking the autocorrelation of the residuals into account, we get two rather different results with equally plausible specifications which are hardly distinguishable according to statistical criteria.

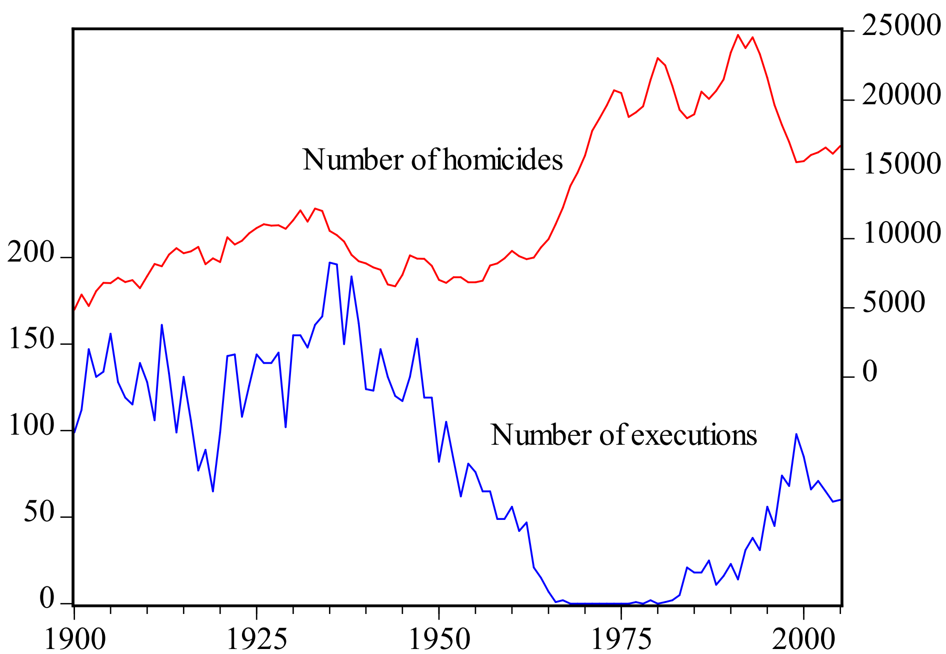

Figure 2: Executions and homicides, U.S.A., 1900 – 2008

[14] The results depend, however, also on the time period used. As can be seen from *Figure 2*, before 1940, the overall correlation was positive (ρ = 0.321).[16] After 1940, the overall correlation is negative (ρ = -0.635). But there are, nevertheless, considerable sub-periods with a positive correlation. As the following example for the 25 years from 1971 to 1995 shows, it is even possible to get significant positive results when taking the autocorrelation of the residuals into account:

(2")
$$HOM_t = \underset{(31.23)}{20314} + \underset{(2.16)}{53.595}\, EXEC_{t-1} + u_t$$
$$u_t = \underset{(6.67)}{1.208}\, u_{t-1} - \underset{(-3.33)}{0.528}\, u_{t-2}$$
$$\bar{R}^2 = 0.783,\ D.\text{-}W. = 2.035,\ Q(2) = 0.857,\ AIC = 16.637.$$

Taken literally, this equation would imply that every execution leads to 54 additional homicides. If we include lagged dependent variables we get similar results, though the statistical criteria indicate that this estimation is slightly inferior and the execution variable significant only at the 10 percent level and only in the long-run equation:

(3")
$$HOM_t = \underset{(3.29)}{6949} + \underset{(6.06)}{1.152}\, HOM_{t-1} - \underset{(-2.88)}{0.4.98}\, HOM_{t-2} + \underset{(1.61)}{28.468}\, EXEC_{t-1} + u_t$$
$$\bar{R}^2 = 0.763,\ D.\text{-}W. = 1.951,\ Q(2) = 1.752,\ AIC = 16.726,$$

with the long-run solution:

(3a")
$$HOM = \underset{(25.71)}{20046} + \underset{(1.74)}{82.128}\, EXEC\ ,$$

which implies that every execution leads to 82 additional murders in the long-run. Thus, even if, given the data from 1940 onwards, most sub-samples show a negative correlation between homicides and executions, it is possible to find sub-periods showing the opposite picture. Everybody who wants to claim that this negative correlation represents a causal relation has to provide a convincing explanation for the existence of sub-periods showing the opposite (causal) relation.

[15] *Figure 3* shows the t-statistics of the execution variable if we perform rolling regressions over 25 year-periods from 1900 to 2008. If we perform simple OLS estimations, depending on the sub-period we chose, we can get rather negative values, but also high positive ones. If we model the residual process as AR(2), we hardly get any significant results. If we include the lagged endogenous variable up to the second order, we find some significantly negative results. The estimated coefficients vary even more dramatically: between -225 and 132 in the case of the simple OLS regression, between -20 and 54 if we apply the Cochrane-Orcutt procedure, and between -4241 and 4092 if we include the two lagged endogenous vari-

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16. Because the official homicide data before 1920 is highly problematic to estimate the number of homicides we use the corrected homicide rates given in D.L. ECKBERG (1995) for the beginning of the 20th century.

ables.[17] Thus, one can find any result demanded; this simple model does not allow for robust results.

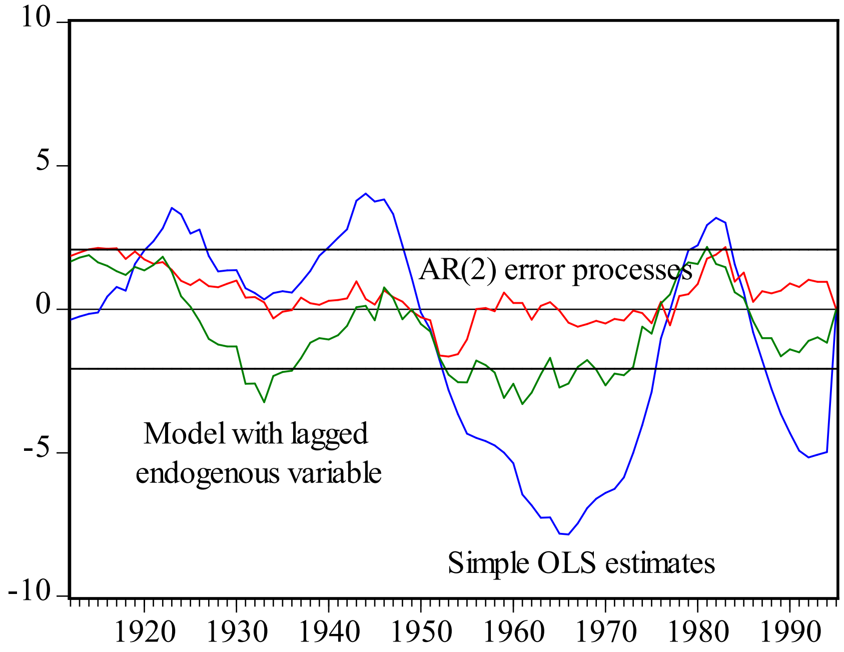

Figure 3: t-statistics of rolling regressions

[16] That most sub-periods of the recent decades show a negative correlation (but some sub-periods a positive one) between homicides and executions is just one particular stylised fact of the relation between these two variables. A second one, shown in *Figure 4* for the period between 1977 and 2007, i.e. after the suspension by the Supreme Court ended in 1976, is that those states that do not use the death penalty have consistently lower homicide rates than those which execute people. This should be disturbing for anybody defending the deterrence hypothesis. It might, however, be at least partly due to the fact that reverse causality exists: those states with higher homicide rates might seem to be more induced to use the death penalty than those with lower rates. Moreover, both series are very highly correlated, with a correlation coefficient of 0.918. In any case, however, if executions have a deterrent effect on murders, this effect should be larger in states with than in states without the death penalty.

[17] To test this, we used a principal component analysis. The first component represents the common movement of both series, while the second one takes up the difference. If we regress the two series on the first component (PC1), we get the following estimates for the homicide rates in states with (HOMRW) executions for the period from 1977 to 2007, i.e. after the moratorium ended:[18]

17. This is the coefficient in the long-run equation corresponding to relation (3a").

18. If we regress the homicide rate in states without executions on the first component, we get a similar picture, with identical values for the R^2 and the t-statistic of the principal component as well as the Durbin-Watson and Q-statistics. To take account of the autocorrelation in the residuals we use for all following models heteroscedasticity and autocorrelation corrected standard errors to calculate the t-statistics.

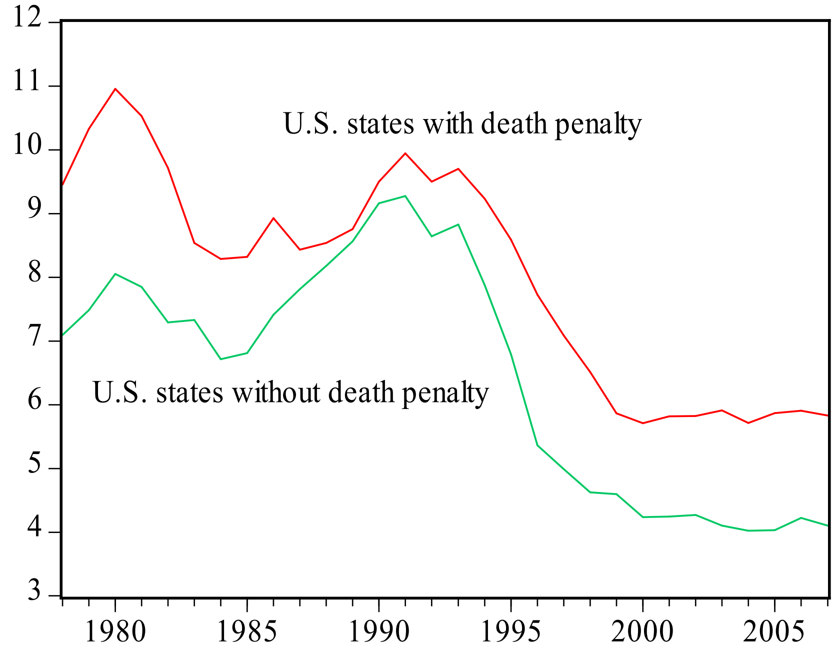

Figure 4: Homicides rates in U.S. States with and without death penalty, 1977 – 2007

(4a) $HOMRW_t = \underset{(176.71)}{8.034} + \underset{(48.25)}{1.202}\, PC1_t + u_t$

$\bar{R}^2 = 0.959$, D.-W. $= 0.305$, $Q(4) = 36.923^{***}$, AIC $= 0.812$.

If executions make a difference between murder rates in the states with and without the death penalty, the second principal component (PC2) that takes account of the differences between the two series should be influenced by the number of executions. Performing the corresponding regression we get:

(4b) $PC2_t = \underset{(-0.11)}{-0.066} + \underset{(0.98)}{0.002}\, EXEC_{t-1} + u_t$

$\bar{R}^2 = -0.003$, D.-W. $= 0.302$, $Q(4) = 38.550^{***}$, AIC $= 0.413$.

From these estimates it is obvious that the number of executions has no impact whatsoever on the difference in the developments of homicide rates of states with and without executions.

[18] We get a corresponding result if we regress the difference in the homicide rate between states with and without death penalty (DIFF) on the (lagged) number of executions:

(5) $Diff_t = \underset{(6.60)}{1.645} - \underset{(-0.40)}{0.002}\, EXEC_{t-1} + u_t$

$\bar{R}^2 = -0.030$, D.-W. $= 0.292$, $Q(4) = 45.410^{***}$, AIC $= 2.277$.

The estimated coefficient is far from any significance level. Because there is high autocorrelation in the estimated residuals, we also employed the two methods to correct for it with the following outcome:

$$(6) \qquad Diff_t = \underset{(2.37)}{1.356} - \underset{(0.78)}{0.002}\, EXEC_{t-1} + u_t, \quad u_t = \underset{(9.03)}{0.860}\, u_{t-1} + \varepsilon_t ,$$

$$\bar{R}^2 = 0.711, \; D.\text{-}W. = 1.484, \; Q(3) = 5.109, \; AIC = 1.034,$$

or:

$$(7) \qquad Diff_t = \underset{(1.07)}{0.213} + \underset{(8.57)}{0.852}\, DFF_{t-1} + \underset{(0.23)}{0.001}\, EXEC_{t-1} + u_t$$

$$\bar{R}^2 = 0.705, \; D.\text{-}W. = 1.546, \; Q(3) = 4.570, \; AIC = 1.054$$

with its long-run solution:

$$(7a) \qquad Diff = \underset{(1.90)}{1.440} + \underset{(0.23)}{0.004}\, EXEC .$$

We get the same picture again and again: The number of executions cannot explain the difference in the developments between the two groups of states.

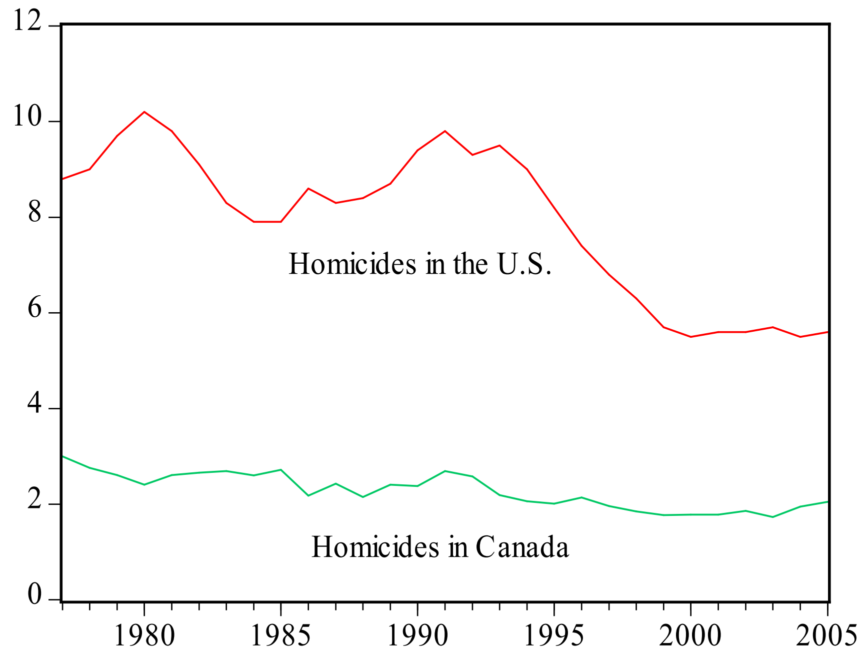

Figure 5: Homicides rates in the U.S. and in Canada, 1977 – 2005

[19] A third stylised fact is that homicide rates are consistently lower in Canada compared with the United States.[19)] *Figure 5* shows this for the period from 1977 to 2005. While the last execution was in 1962, death penalty was officially abolished in 1976. Thus, *Figure 4* shows the period after Canada abolished the death penalty.

[20] The correlation between both series is much lower than the one between the different U.S. states. It is, however, still 0.758, indicating that there is considerable common movement

19. For the development in Canada, but also in the U.S., see also A.K. DILLS, J. MIRON and G. SUMMERS (2010).

between the developments in the two countries. Thus, we can perform the same test as before. Estimating principal components and regressing the two homicide series on the first component we get for the U.S. homicide rate (HOMRUS):

(8a) $$\underset{}{HOMRUS_t} = \underset{(76.09)}{7.917} + \underset{(16.79)}{1.099}\, PC1_t + u_t$$

$$\bar{R}^2 = 0.875,\ D.\text{-}W. = 0.935,\ Q(4) = 7.669,\ AIC = 1.746.$$

[21] The very high value of the adjusted multiple correlation coefficient indicates that there is a rather strong common development of the homicide rates of the two countries, despite the different position with regard to death penalty. The regression of the second principal component that represents the differences between the two countries on the number of executions in the United States leads to

(8b) $$PC2_t = \underset{(-0.11)}{-0.018} + \underset{(0.20)}{0.0005}\, EXEC_{t-1} + u_t$$

$$\bar{R}^2 = -0.036,\ D.\text{-}W. = 0.937,\ Q(4) = 7.737,\ AIC = 1.556.$$

Thus, similar to the results for the U.S. states, we find that, taking into account the common movements between the two countries, the number of executions in the United States does not have any impact at all on the difference of the development of the murder rates between the two countries.

[22] Thus, proponents of the death penalty in the United States do have to give convincing answers to four questions:

(i) Why is the homicide rate in those U.S. states which do not apply the death penalty consistently lower compared to those states applying it?

(ii) Why does the number of executions not have an impact on the difference in the development of the homicide rates between those states which do not apply the death penalty compared to those states applying it?

(iii) Why is the homicide rate in Canada consistently lower than in the United States despite the fact that Canada does not apply the death penalty but the United States does?

(iv) Why does the number of executions not have an impact on the difference in the development of the homicide rates between Canada and the United States despite the fact that Canada does not apply the death penalty but the United States does?

So far, no convincing answers have been given to these four questions.

3 A Critical Survey of the Recent Literature

[23] The main participants in the new debate are three groups of authors.[20] H. DEZHBAKHSH, P. RUBIN and J.M. SHEPHERD (2003)[21] as well as H.N. MOCAN and R.K. GITTINGS (2003),

20. An overview of the different studies is given in *Table A1* in the Appendix.

defend the deterrence hypothesis. J.J. DONOHUE and J. WOLFERS (2005, 2009) as well as L. KATZ, S.D. LEVITT and E. SHUSTOROVICH (2003), J. FAGAN (2005, 2006), J. FAGAN, F.E. ZIMRING and A. GELLER (2006) question it.[22] Finally, J.M. SHEPHERD (2005) presents results in favour of the deterrence as well as the brutalisation effects and tries to explain why these different results occur. When discussing these papers, to make the results comparable we concentrate on the mean of the estimated t-statistics of the execution variable.[23]

[24] H. DEZHBAKHSH, P. RUBIN and J.M. SHEPHERD (2003) use county level data from 3054 US counties over the period from 1977 to 1996. They use a linear model for the murder rate with county fixed effects and different specifications of the deterrence variable.[24] They include the aggravated assault rate as well as the robbery rate as explanatory variables and use Two Stage Least Squares (TSLS) to instrument these two variables. They present altogether 48 estimates of the effect of the conditional probability of execution on the murder rate with a mean t-statistic of -5.47 and a standard deviation of 4.83.[25] Their conclusion from their results is “that each execution has resulted, on average, in eighteen fewer murders” (p. 369). These results were criticised by J.J. DONOHUE and J. WOLFERS (2005). Their main arguments are about the instruments used and about the influence of Texas and California on the results; the two states which have by far the highest number of executions. Their re-estimation leads to quite different results: the mean t-statistic is -1.43 with a standard deviation of 11.17.[26]

[25] A second paper from this group of authors is by J.M. SHEPHERD (2004). She uses monthly state-level data for the period from 1977 to 1999 and, including state fixed effects, estimates linear least squares models as well as negative binomial regressions for the murder rate. The mean of their 32 estimated t-statistics is 3.17 with a standard deviation of 2.08. The linear model tells her that in 1999 each execution prevented about three homicides, and additional 4.5 homicides have been prevented in this year by each death penalty sentence.[27]

[26] Similar evidence is presented by H. DEZHBAKHSH and J.M. SHEPHERD (2006). Aside from some ‘exploratory’ time series regressions for the period from 1960 to 2000 they use

21. See also H. DEZHBAKHSH, and J.M. SHEPHERD (2006), H. DEZHBAKHSH and P. RUBIN (2007), P.R. ZIMMERMANN (2004, 2006) as well as J.M. SHEPHERD (2004).

22. Similar results which are, however, not discussed by the authors are presented in J.R. LOTT and J. WHITLEY (2007).

23. Because statistical significance does not always imply economic significance as well, an obvious alternative would be to ask how many lives are saved by each execution. However, these data are only provided in some of the studies, and we would not be able to distinguish between statistically significant and insignificant effects.

24. They report that the results are robust with respect to changes of the functional form.

25. Given the number of observations, a t-test whether this mean is significantly different from zero would lead to a highly significant result. Such a test assumes, however, that the observations are independent. This assumption is, of course, strongly violated in this situation. Thus, such a test cannot be applied here. In order to perform a meaningful test, an assumption about the correlation between the estimated t-values would be necessary.

26. In calculating the average t-statistics, those estimates exactly reproducing the results of other authors are always excluded.

27. See J.M. SHEPHERD (2004, p. 308).

state level data from 1960 to 2000. They use a linear model with the current and lagged number of executions as deterrence variable and include state fixed effects. The mean t-statistic of their 9 time series and 17 panel estimates is -5.81 with a standard deviation of 2.46. According to these estimates, each execution saves about eight lives. These results are also criticised by J.J. DONOHUE and J. WOLFERS (2005). Using the same data set, they show that using other measures of the executions' risk the deterrence variable is no longer significant. The mean t-statistic reported is -0.767 with a standard deviation of 1.01. This criticism is contradicted by H. DEZHBAKHSH and P. RUBIN (2007). Taking up the suggestions of J.J. DONOHUE and J. WOLFERS (2005) they present 47 different estimates with a mean t-statistic of -3.62 and a standard deviation of 2.94. They accuse J.J. DONOHUE and J. WOLFERS (2005) of data mining and only emphasising those few results which do not indicate a significant deterrence effect.

[27] Another paper is by H.N. MOCAN and R.K. GITTINGS (2003). They use state data from the years from 1977 to 1999 and get an average t-statistic of their 11 coefficients of -1.98 with a standard deviation of 0.24. Their conclusion is that every execution prevents about five homicides. This paper is again criticised by J.J. DONOHUE and J. WOLFERS (2005). Aside from correcting some programming errors, the main dispute between the two groups is about the construction of the deterrence variable. To construct the murder ratios, H.N. MOCAN and R.K. GITTINGS (2003) construct the probability of execution as the relation between the number of last year's executions and the number of death sentences seven years ago. The justification they provide for this procedure is that the average time length on the death row is six years. J.J. DONOHUE and J. WOLFERS (2005) question this because it is highly implausible that a potential offender is informed about the number of death sentences seven years ago in order to perform this calculation. Therefore, citing the argument of P.R. ZIMMERMANN (2003, p. 170) "that any truly meaningful assessment a potential murderer makes" on the probability of an execution "is likely to be based on the most recent information available to him/her", they use the relation of last year's executions to last year's death sentences. Applying this measure, their re-estimation leads to a mean t-statistic of -0.64 with a standard deviation of 1.21. These results are more or less confirmed by H.N. MOCAN and R.K. GITTINGS (2006) who replicate many models using the specification of J.J. DONOHUE and J. WOLFERS (2005) and find insignificant results in most cases. However, they insist that their measure is correct and performing another 195 regressions they show that their measure is insofar robust as a delay of four or five years does not alter the results qualitatively; excluding the specifications of J.J. DONOHUE and J. WOLFERS (2005) their mean t-statistic is -1.96 with a standard deviation of 0.83. The argument for a four or five years lag is, however, hardly more convincing than the one for a six years lag; it is still implausible that potential offenders use this information for their calculations.[28)]

[28] The data of H.N. MOCAN and R.K. GITTINGS (2003) are also used by J. FAGAN (2006). However, he estimates the model also for other specifications concerning the deterrence variable, data sources, or eliminating Texas from the data. This results in 13 estimates with a mean of -1.16 and a standard deviation of 1.17. In his paper he criticises the same studies that

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28. See also A.K. DILLS, J.A. MIRON and G. SUMMERS (2008, p. 10, FN 13): "their case for the assumed lag is not persuasive."

are also discussed in the current paper and concludes that "this cohort of studies and researchers, like Ehrlich before them, has created unjustified confidence in the minds of legislators, death penalty advocates, and a small group of legal scholars about the capacity of death sentences and executions to deter murder" (p. 319).

[29] A further critique of the studies postulating a deterrence effect is provided in J. FAGAN, F.E. ZIMRING and A. GELLER (2006). They criticise the data that have been used so far, because they do not distinguish between those homicides that are punishable by death and the other ones. Using state-level panel data from 1978 to 2000, restricting the murder rates on those homicides that are punishable by death, and estimating linear as well as Poisson regressions, they do not find a significant effect of the existence of a death penalty statute or of the number of executions lagged one or two years on felony homicide rates. The mean of their 24 estimated t-statistics is -0.31 with a standard deviation of 1.24.

[30] Another author who finds, however, a significant deterrent effect is P.R. ZIMMERMANN (2004, 2006). In his 2004 paper he employs a panel of state-level data from 1978 to 1997. He takes into account the potential effect of murders on the probability of execution and compares the results of the t-statistics of four OLS with two TSLS estimates. While the mean of the OLS estimates is -1.31 with a standard deviation of 0.24, the mean of the TSLS estimates is -2.59. According to J.J. DONOHUE and J. WOLFERS (2005, p. 835), P.R. ZIMMERMANN's (2004) most preferred specification implies that every execution saves 19 lives, with a confidence interval from 7 to 31 lives. Re-estimating this equation and clustering the standard errors according to states in order to take account of autocorrelation of the residuals, J.J. DONOHUE and J. WOLFERS (2005) derive, however, an interval from saving 54 lives to causing 23 additional homicides per execution, i.e. the deterrence effect is no longer statistically significant. P.R. ZIMMERMANN (2009), on the other hand, suspects that clustering might not be the appropriate measure to cope with the autocorrelation of the residuals and, using alternative methods, finds again significant results of the deterrent effect, but only as long as the deterrence effect is measured by the probability of execution given conviction. If it is measured by the probability of conviction given arrest, the estimated coefficients are still negative but no longer significantly different from zero.

[31] In P.R. ZIMMERMANN (2006) it is investigated whether the method by which the death penalty is effected has an effect on the deterrence. Using a panel of state-level data from 1978 to 2000, only electrocution has a deterrent effect; none of the other four methods, neither lethal injection, nor gas chamber asphyxiation, nor hanging, nor firing squad has a statistically significant effect. Thus, his average t-statistic is -1.05 with a standard deviation of 1.19. Correspondingly, he argues against the change from electrocution to lethal injection that took place in several death penalty states if one hopes that capital punishment has a deterrent effect on potential murderers.

[32] Similar, but even more astonishing results are presented by R.B. EKELUND et al. (2006), for the period from 1995 to 1999. While the existence of the death penalty raises the murder rate, executions and, in particular electrocutions diminish it. J.J. DONOHUE and J. WOLFERS (2009, p. 271f.) comment this result by noticing that the sheer effect of the existence of the death penalty is rather large and the estimated effect even of the executions by electrocution is

so small, that many more murderers would have had to be executed than even in Texas in the years considered in order to render the total effect negative, i.e. to get a deterrent effect of the death penalty. A second result of R.B. EKELUND et al. (2006) is that multiple murders are not deterred at all: all coefficients, not only those of the existence of death penalty, but also those of the executions have the ‘wrong’ negative sign, and 15 out of the 16 estimated coefficients are highly significant. While this is compatible with a strong brutalisation effect, the authors explain their results by the fact that there is no deterrence currently for murders after the first one, and, referring to the torture practices of the Middle Ages, they demand that one should think of establishing forms of marginal deterrence in the application of capital punishment.

[33] While these studies (at least implicitly) assume that, aside from what is reflected by the state fixed effects, the effects of capital punishment are identical in all states, J. SHEPHERD (2005) finds different results for different states. She strongly believes in the deterrence effect, but she also takes brutalisation into account. And she also looks for an explanation for the often contradictory results that have been produced. Employing the same data of 3054 counties over the time period from 1977 to 1996 as in H. DEZHBAKHSH, P. RUBIN and J.M. SHEPHERD (2003), she is able to perform separate panel estimates for the different states. According to these results, in six states executions have a deterrent effect, where the number of saved lives per execution runs from 6 in Nevada to 61 in South Carolina. In contrast to this, 13 states show a brutalisation effect, from 3 additional murders in Oklahoma to 175 in Utah and Oregon. In the remaining eight states implementing the death penalty, there is no significant effect. She gets similar results using monthly state-level data from 1977 to 1999; there are six states with a deterrent effect and eight states with a brutalisation effect, while the remaining 13 states do not show any significant effect. Using state-level annual data the number of states with a significant effect is even smaller; there are five states with a significant deterrence and another five states with a significant brutalisation effect.

[34] The solution she suggests for this puzzle is a threshold effect: brutalisation as well as deterrence are effective. Deterrence is, however, nonlinear; a certain number of executions is necessary for it to become relevant, because the marginal deterrence effect increases with the number of executions. If there are only rather few executions in a state, the brutalization effect dominates, if there are some more, both effects cancel each other, and if there were more than about 9 executions in the observation period, deterrence dominates. Actually, she shows that those states with a significant deterrence effect had, at the 90 percent confidence level, significantly more executions than the remaining states implementing the death penalty.

[35] Another differentiation was undertaken by M. FRAKES and M. HARDING (2009). They ask for the effect of extending the death penalty eligibility to murders of youth victims and find a significant deterrent effect. They find a similar, but much less significant effect for the eligibility of multiple victim murders, and insignificant, but numerically large effects with the ‘wrong’ sign for narcotic related murders and those with victims over 70 years of age. They do not, however, find a general deterrent effect of capital punishment. Thus, their results are rather mixed.

[36] Aside from the critique in J.J. DONOHUE and J. WOLFERS (2005) and the papers by J. FAGAN and his co-authors, there are also other papers which do not find a significant deter-

rence effect. Using state-level data, L. KATZ, S.D. LEVITT and E. SHUSTOROVICH (2003) estimate a linear model of the murder rate between 1950 and 1990. They present 22 t-statistics for their deterrence variable with a mean of -0.20 and a standard deviation of 1.42. A similar result is presented by J.R. LOTT and J. WHITLEY (2007). Using state-level data from the period from 1976 to 1998 and applying Poisson and Negative Binomial regressions, their intention is to show that legalised abortion increased crime. A by-product of their attempt is, however, the result that the execution rate does not have a significant impact on the murder rate: the 5 corresponding t-statistics have a mean of -0.05 and a standard deviation of 0.24. Finally, T. KOVANDZIC, L.M. VIERAITIS and D.P. BOOTS (2009), using state level data from 1977 to 2006 and employing fourteen different statistical models and seven different indicators of the deterrence effect, present altogether 98 estimated coefficients out of which only four are statistically different from zero at the 5 percent level. The average t-statistic of their models is -0.558 with a standard deviation of 0.999.[29)]

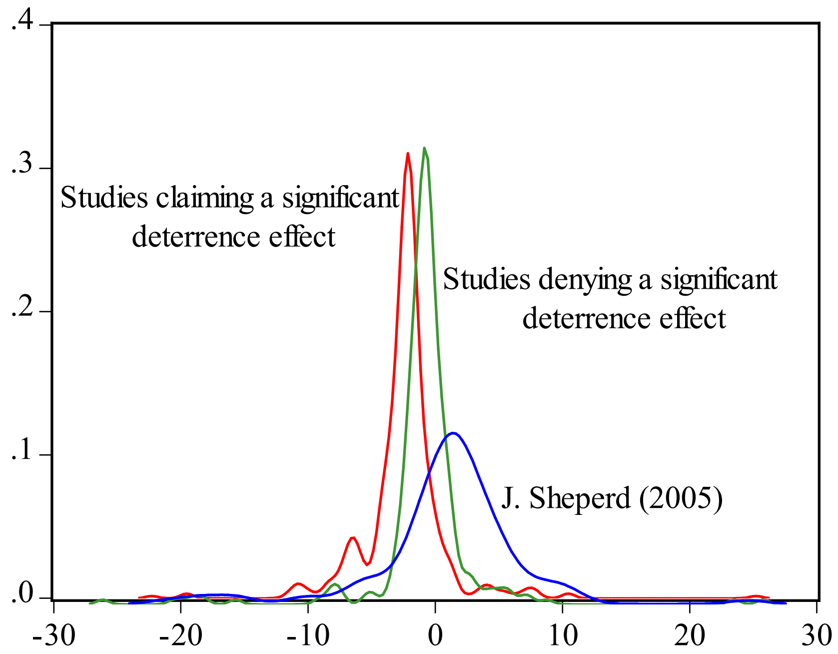

Figure 6: Distributions of the t-statistics

[37] *Figure 6* shows the distributions of the t-statistics of the three groups. The results differ widely, and it is rather difficult to draw firm conclusions. Taking all results together, the only – admittedly weak – indication for the existence of a deterrence effect of death penalty is the fact that the mean of the reported t-statistics is negative even in those papers coming to the conclusion that executions are non-deterring.[30)] The only papers with positive means are by J. SHEPHERD (2005) and, despite that they believe in the deterrence effect, by R.B. EKELUND et

29. R. HJALMARSSON (2009) who uses daily time-series data for 1999 to 2004 for three Texas cities, Houston, Dallas and San Antonio, does not find a 'local effect' of executions on homicides.

30. The mean of these t-statistics is -0.780 with a standard deviation of 5.710. The mean of the t-statistics in those studies claiming a significant deterrence effects is -2.879 with a standard deviation of 2.703. See also the summary of the t-statistics in *Table A1* of the *Appendix*.

al. (2006). If the true value would be zero, we should more often observe a (non-significant) positive mean, in particular in those studies denying a deterrent effect.

[38] Despite the fact that the econometric techniques employed in these studies are much more advanced than those in the first two waves, serious methodological problems remain. First of all, there are still serious data problems. The results are heavily dependent on measures of the deterrence variable as, for example, the rather different results employing the H.N. MOCAN and R.K. GITTINGS (2003) data show. Moreover, as J. FAGAN, F.E. ZIMRING and A. GELLER (2006) show, the correct specification of the dependent variable is also a major problem. The functional form of the equation does not, on the other hand, seem to be a major problem: those studies that employ log-linear instead of linear specifications do not produce consistently different results.

[39] There are, however, other serious econometric problems that have only been considered in some of the studies and at least were not always solved convincingly. One of the major problems is simultaneity. Potential offenders might choose from a portfolio of different criminal acts. Thus, a system of equations containing a separate equation for every punishable act would be appropriate. If only one equation, i.e. the one for murder rates, is estimated, the usual cure is to employ instrumental variable estimates. However, as several studies show, the question which variables are well suited for being an instrument is highly debatable, and the results are largely dependent on the instruments used.

[40] Another major problem in this respect is the possible instantaneous and/or feedback relation between murders and executions; executions might not only have an effect on murder rates but murder rates also on executions. In the time domain, this might not be considered as being an important problem because, if they have any impact, murder rates should have a positive effect on executions. Thus, not taking this into account might downward bias the estimated coefficients of the deterrence variables and their t-statistics. The considerable increase of the estimated parameters in the model of P.R. ZIMMERMANN (2006) by switching from OLS to TSLS might point in this direction.

[41] There is, however, another problem in this respect. All panel studies use state (or county) fixed effects. Thus, different average levels of murder rates between the states are not explained but represented by dummy variables. Thus, all that can be explained by these regressions are reactions over time. The more interesting question as to whether states with the death penalty have in the long-run higher or lower murder rates than those that do not execute cannot be answered by these models. Given the fact mentioned above that those states without the death penalty have consistently lower murder rates than those with, this is a serious problem. Because even if an increase of executions deters murderers in the short-run, there might be a long-run brutalisation effect that is not reflected in the models. Thus, we have again a serious simultaneity problem: states might have less need for the death penalty if their murder rate is lower, but fewer executions might also lead to less brutalisation.

[42] Attempts in this direction have been done by investigating the effect of the moratorium or its lifting in 1976, respectively. Several studies take this into account, partly by introducing dummy variables for the existence of a death penalty statute and partly by considering the re-

introduction of the death penalty in different states as natural experiments. However, the results of those studies are also far from being univocal. H. DEZHBAKHSH and J.M. SHEPHERD (2006) find that the abolition of the death penalty was associated with an increase while its re-introduction led to a (smaller) decrease of homicides, but J.J. DONOHUE and J. WOLFERS (2005) using a difference-in-difference approach do not find any evidence in this respect. Moreover, DEZHBAKHSH and J.M. SHEPHERD (2006) find a significant positive effect of the moratorium on murder rates in their state-panel date regression results, while the results of J. FAGAN, F.E. ZIMRING and A. GELLER (2006) mentioned above do not point in this direction. Finally, J.K. COCHRANE and M.B. CHAMLIN (2000) investigate the effects of re-introducing capital punishment in California in 1992. Using an ARIMA-approach they find "a significant decline in the level of non-stranger felony-murders and a significant increase of argument-based murders of strangers in the period following the [first] execution" (p. 685) on April 21 and, therefore, a deterrence as well as a brutalisation effect. But while the brutalisation effect remained permanent and was even increasing over time, the deterrent effect declined over time and appeared, therefore, "to be contingent on an additional, perhaps continuous application" (p. 701) of the death penalty. Nevertheless, the net effect of executions on homicide was zero.[31)]

4 Concluding Remarks

[43] A critical and cautious examination of these results leads to the conviction that we cannot draw any strong conclusions. While there is some evidence that a deterrent effect might exist, it is too fragile to be certain. Furthermore, the possible quantitative effect usually measured by the number of homicides prevented by each execution is so uncertain that it is difficult to conclude anything that would be relevant for policy purposes.[32)] Of course, defenders of the death penalty as G.S. BECKER (2006) or R.A. POSNER (2006) will still insist that the econometric evidence is strong enough to justify the belief in a considerable deterrence effect, even if they admit that the evidence is far from being perfect.[33)] Those who consider the death

31. A similar study is performed by W.C. BAILEY (1998) (with reference to J.K. COCHRANE, M.B. CHAMLIN and M. SETH (1994)) to investigate the effects of the re-introduction of capital punishment in Oklahoma in 1990. He presents strong evidence for a brutalisation but at best rather weak evidence for a deterrence effect.

32. See also E. COHEN-COLE, J. FAGEN and S. DURLAUF (2009, p. 335) who average the models of H. DEZHBAKHSH, P. RUBIN and J.M. SHEPHERD (2003) with the one of J.J. DONOHUE and J. WOLFERS (2005) and reach the conclusion that "evidence of deterrent effects appears, while not inexistent, weak."

33. See, for example, G.S. BECKER (2006, p. 1): "I support the use of capital punishment because, and only because, I believe it deters murders. … The available data are quite limited, however, so one should not base any conclusion solely on the econometric evidence. Still, I believe the preponderance of evidence does indicate that capital punishment deters, … Of course, public policy on punishments cannot wait until the evidence is perfect. Even with the limited quantitative evidence available, there are good reasons to believe that capital punishment deters murder. Most people, and murders in particular, fear death, especially when it follows swiftly and with considerable certainty following the commission of a murder." See also P.H. RUBIN (2009) who makes it very clear that there has to be a deterrent effect of the death penalty whatever econometric analyses show.

penalty to be unconstitutional point to the weaknesses of these studies.[34] As the examples of J. FAGAN (2005) and J. SHEPHERD (2004a) show, this can lead to contradictory statements in testimonies before parliamentary (and other) committees.

[44] That social scientists contradict each other in such situations is neither new nor unique: there are numerous other such examples.[35] What is more or less unique is the divide between economics and the other social sciences: While most (not all) economists believe in the deterrent effect of death penalty and defend the evidence presented, most other social scientists, including law professors, question the evidence and do not believe that it can justify death penalty. Moreover, this debate might be more heated than other ones, but, taking into account the issue at stake, “a question of live and death”, to cite I. EHRLICH (1975), this is not at all astonishing.

[45] The divide between the social sciences is also not surprising. Economists believe in incentives more than other social scientists do.[36] They usually believe in the deterrent effect of punishment and, therefore, also of capital punishment. The only relevant question for them is whether the change from life imprisonment to death penalty has a positive marginal deterrence effect. In recent decades, the deterrent effect of criminal law has been questioned by psychologists and by people from law departments following them; besides deterrence, there are many other reasons why people obey the law.[37] Consequently, the marginal deterrence effect of death penalty has been questioned, too. This does not necessarily imply that there is no deterrent effect at all. But whether the marginal effect is significant and large enough to justify legislation is still a question to be discussed.

[46] In recent years, economists learned to take the results of psychologists more seriously, developing the new approach of ‘behavioural economics’.[38] There, the effect of deterrence is highly questioned as well. Moreover, they learned that in many situations people do not act rationally, at least not according to conventional standards of rationality. This holds for murderers, too. But, as R.H. MCADAMS and T.S. ULEN (2009) correctly argue, for deterrence to become effective it is sufficient that those who might, but actually do not commit murders are deterred. Moreover, even though the reaction of individuals is in many situations weak, be-

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34. See, for example, J. FAGAN (2005, p. 2): “These new studies are fraught with technical and conceptual errors: inappropriate methods of statistical analysis, failures to consider all the relevant factors that drive murder rates, missing data on key variables in key states, the tyranny of a few outliers and years, weak to non-existent tests of concurrent effects of incarceration, statistical confounding of murder rates with death sentences, failure to consider the general performance of the criminal justice system, and the absence of any direct test of deterrence. These studies fail to reach the demanding standards of social science to make such strong claims, standards such as replication, responding to counterfactual claims, and basic comparison with other scenarios. Some simple examples and contrasts, including the careful analysis of the experience in Massachusetts compared to other states, lead to a rejection of the idea that either death sentences or executions deter murder.”

35. See, for example, G. KIRCHGÄSSNER (2005) with respect to environmental policy or G. KIRCHGÄSSNER (2007) with respect to natural damage insurance in Switzerland.

36. See for this, for example, G. KIRCHGÄSSNER (2005a).

37. See, for example, K.-D. OPP (1989), T.R. TYLOR (1990, 1997), P.H. ROBINSON and J.M. DARLEY (2004) or A.M. LICHT (2008).

38. See, for example, N. GAROUPA (2003) or R.H. MCADAMS and T.S. ULEN (2009).

havioural economists do not reject the notion that sufficiently strong incentives have an impact on human behaviour. Thus, the question at stake here is ‘only’ whether the marginal effect of a change from lifelong imprisonment to death penalty is a strong enough incentive, generally and in particular given today’s situation in the United States.

[47] Because of the contradictory evidence that is available there will be no consensus about this question, at least not in the near future. The same (as well as other) people will present similar evidence as before, with ever improved statistical techniques. There is some hope that these improved techniques will lead to some convergence of the results, not only of the methods, but there is a high probability that this hope which has been frustrated over the last thirty years will also be frustrated in the (near) future. There are too many possibilities for different specifications which can be justified with arguments of plausibility to expect a full convergence. Selective perceptions will again have the effect that researchers believe in those results that seem to be plausible from their a priori point of view.

[48] This perception of the scientific process might seem to be rather pessimistic. It is, however, only realistic. It is partly due to the fact that this is an area where we cannot perform controlled experiments, and the quasi-natural experiments which are sometimes unintentionally conducted by political authorities also do not give clear answers, as the recent history of the United States, (and the papers analysing these experiments) show. This is not specific for economics and the other social sciences as one might think, because, as K.R. POPPER (1962, p. 95) writes: “It is a mistake to assume that the objectivity of a science depends on the objectivity of the scientist. And it is a mistake to believe that the attitude of the natural scientist is more objective than that of the social scientist. ... [T]he objectivity of science is not a matter of the individual scientists but rather the social result of their mutual criticisms, of the friendly-hostile division of labour among scientists, of their co-operation and also of their competition.”

[49] This mutual criticism is the driving force of scientific progress, and it allows ‘bad research’, theoretical as well as empirical, to be discredited. Thus, ideology or, in other words, the personal convictions of the researchers will nearly always play a role, but competition and open discussion can (and hopefully will) have the effect that the majority opinion in the scientific community goes into the correct direction in the long-run, even if not all members of this community will be convinced. M. PLANCK (1933, p. 299) already wrote with respect to scientific progress: “An important scientific innovation rarely makes its way gradually winning over and converting its opponents: it rarely happens that Saul becomes Paul. What does happen is that its opponents gradually die out and that the growing generation is familiarized with the idea from the beginning.” This holds generally, for all sciences, but for the issue at stake, the discussion about the potential deterrence effect of capital punishment, in particular. Given the demands for the precision of scientific advice that are often expressed, this does not seem to be very much, but more cannot be expected in a realistic perspective. Moreover, this does not imply that policy advice is useless, but if competing experts are asked, it is hardly the case that advice is unanimous. Thus, policy advice can clarify facts, and this holds also for econometric estimates of the deterrence effect of capital punishment, but ideology will always also play a role.

[50] Besides the disputed facts there is, however, a moral question with respect to capital punishment as well, which has also been raised again recently. If, (but only if) capital punishment deters, the question of its moral justification or rejection becomes more complicated. Based on a strong belief in the deterrent effect of the death penalty, C.R. SUNSTEIN and A. VERMEULE (2005) defend the position that capital punishment might not only be morally acceptable but even be morally required. The basis of their argument is the proposition that the difference between act and omissions is, for the government as a moral actor, misleading in this case (and in other situations as well). The usual example of a 'tragic decision' is whether it is allowed or even morally obliged to kill an innocent person in order to save the lives of several other people. Despite the fact that consequentialism might demand this, it contradicts our usual moral intuitions, because it is not allowed to do any harm to an innocent person. A murderer is definitely not an innocent person. One might question whether deliberate killing of a human person can be morally justified at all, aside from situations of (personal) self-defence or whether it should, like torture, be negated categorically. If the latter holds, there is no possibility to justify the death penalty. Executing murderers might, however, be seen as a societal self-defence if it really prevents further murders. Then, the problem arises that sometimes innocent people are executed. We do, however, neither know those people who are innocent and, due to judicial errors, are executed, nor those whose lives are saved due to the deterrence effect of the death penalty. Thus, we face a new variant of the act-versus-omission problem.

[51] Given this situation, C.R. SUNSTEIN and A. VERMEULE (2005) question the distinction between acts and omissions in this context (and also in the more general context of public policy altogether) and argue in favour of the death penalty because we have to compare on both sides 'statistical lives', and it saves more lives than it risks. C.S. STREIKER (2005) argues against this position from a deontological point of view because "executions constitute a distinctive moral wrong, (purposeful as opposed to non-purposeful killing) and a distinctive kind of injustice (unjustified punishment)."[39)] In their reply, C.R. SUNSTEIN and A. VERMEULE (2005a) reject this by restating the argument that innocent people are involved on both sides, that deterrence should play a major role in moral judgements, and "if capital punishment has significant deterrent effects, then the moral argument for the ultimate penalty is greatly strengthened – even, we think, to the point of raising the possibility that capital punishment may be morally required" (p. 857).[40)]

[52] Even if one does not follow this conclusion, one can hardly reject the notion that deterrence should play a major role in legal judgements and, because legal judgments should be morally justified, also in moral judgements. Thus, the debate about the possible deterrent ef-

39. Another interesting aspect raised by her but which cannot be discussed here is that giving up the distinction between act and omission with respect to governmental policies would lead to far more extensive government interventions than we experience today.

40. D.R. WILLIAMS (2006) argues that the whole debate is futile because it is about ideal systems and does not take the reality into account. However, the fundamental question demands an answer even if it is acknowledged that the reality of the death penalty in the United States is far from a (perhaps possible) ideal situation. To refer to the non-ideality of the current situation might be taken as an excuse that an answer is not necessary at the moment, it is, however, not a substitute for such an answer.

fect of the death penalty is morally relevant, as long as legal judgements are supposed to take into account their consequences, a demand that can hardly be rejected. On the other hand, as long as the evidence for a serious deterrent effect of the death penalty is so weak, it can hardly be used as a justification for this kind of punishment. Moreover, today the most serious murderers internationally are the suicide bombers. They cannot be deterred by the death penalty at all. Thus, the potential deterrent effect of the death penalty as well as its role in justifying this kind of punishment, collapse with respect to perhaps the worst crimes of all today.

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Appendix

Sources of the data

Executions (U.S.):	Death Penalty Information Center, Executions in the U.S. 1608-2002: The Espy File, http://www.deathpenaltyinfo.org/executions-us-1608-2002-espy-file; Bureau of Justice Statistics, Number of Executions 1930-2008, http://www.ojp.usdoj.gov/bjs/glance/exe.htm

Homicides

Homicide Rates (U.S., 1900 – 1932):	D.L. ECKBERG (1995)
Homicide Rates (U.S., 1933 – 2008):	National Center for Health Statistics, Vital Statistics, Homicide rates from the Vital Statistics, http://www.ojp.usdoj.gov/bjs/glance/tables/hmrttab.htm
Homicide Rates, (U.S., Canada):	Data from J.J. DONOHUE and J. WOLFERS (2005), http://islandia.law.yale.edu/donohue/pubsdata.htm.
Homicide Rates in States with and without Death Penalty	Data from J.J. DONOHUE and J. WOLFERS (2005), http://islandia.law.yale.edu/donohue/pubsdata.htm.

Table A1: Summary of the t-Statistics of Panel-Studies With State or County Data							
Authors	Journal	Profession	Data	Estimation Method	t-statistics		
					N	Mean	Standard Deviation
DEZHBAKHSH, RUBIN and SHEPHERD (2003)	*American Law and Economic Review*	Economics	3054 Counties, 1977-1996	Panel, TSLS	48	-5.469	4.833
DEZHBAKHSH. and RUBIN (2007)	*mimeo*	Economics	States, 1960-2000	Panel, OLS, TSLS	47	-3.616	2.936
DEZHBAKHSH and SHEP-HERD (2006)	*Economic Inquiry*	Economics	States, 1960-2000	TS, Panel, OLS, TSLS	26	-5.813	2.457
DONOHUE and WOLFERS (2005)	*Stanford Law Review*	Law	States, different samples between 1934 and 2000	Panel, GLS, TSLS (DiD)	83	-0.892	7.372
EKELUND, JACKSON, RESS-LER and TOLLISON (2006)	*Southern Economic Journal*	Economics	States, 1995-1999	Panel, ML (logit)	32	3.547	5.749
FAGAN (2006)	*Ohio State Journal of Criminal Law*	Law	States, 1977-1997	Panel, OLS	13	-1.159	1.166
FAGAN, ZIMRING and GELLER (2006)	*Texas Law Review*	Law	States, 1978-2000	Panel, Poisson Regressions	24	-0.311	1.239
FRAKES and HARDING (2009)	*American Law and Economics Review*	Economics (Law)	States, 1977-2004	Panel, OLS (DiD)	42	-2.857	1.784
KATZ, LEVITT, and SHUSTOROVICH (2003)	*American Law and Economics Review*	Law	States, 1950-1990	Panel, WLS	22	-0.199	1.421
KOVANDZIC, VIERAITIS, and BOOTS (2009)	*Criminology and Public Policy*	Criminology	States, 1977-2006	Panel, WLS	98	-0.558	0.999
LOTT and WHITLEY (2007)	*Economic Inquiry*	Economics	States, 1976-1998	Panel, Poisson Regressions	5	-0.048	0.236
MOCAN and GITTINGS (2003)	*Journal of Law and Eco-nomics*	Economics	States, 1977-1999	Panel, WLS	11	-1.982	0.238
MOCAN and GITTINGS (2006)	*NBER Working Paper*	Economics	States, 1977-1999	Panel, WLS	195	-1.969	0.825

Table A1: Summary of the t-Statistics of Panel-Studies With State or County Data (continued)							
SHEPHERD (2004)	*Journal of Legal Studies*	Economics	States, 1977-1999, monthly data	Panel, WLS	32	-3.167	2.076
SHEPHERD (2005)	*Michigan Law Review*	Economics	3054 Counties, 1977-1996	Panel, TSLS	112	0.489	5.931
ZIMMERMANN (2004)	*Journal of Applied Economics*	Economics	States, 1978-1997	Panel, OLS, TSLS	6	-1.738	0.720
ZIMMERMANN (2006)	*American Journal of Economics and Sociology*	Economics	States, 1978-1997	Panel, WLS	31	-1.053	1.194
ZIMMERMANN (2009)	*American Law and Economics Review*	Economics	States, 1978-1997	Panel, TSLS	14	-2.274	1.761