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The deterrent effect of capital punishment: A review of the research evidence

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Capital punishment has been abolished in all Australian States since the mid 1980s. The United States of America is the only Western democracy that has retained the death penalty. The aim of this bulletin is to analyse and summarise the empirical evidence on the deterrent effect of capital punishment. It provides a survey of the results of 74 research projects published between 1952 and 2003. These projects employed a variety of methodologies, including economic modelling, and covered a range of geographical areas and time periods mainly in the United States. The majority of the studies show that the use of capital punishment did not deter the commission of homicide; this remains the case when studies that used relatively unsophisticated research designs were excluded. The bulletin concludes that three decades of deterrence research since Ehrlich's (1975a) economic model has failed to deliver conclusive evidence on the deterrent effect of capital punishment.

1. INTRODUCTION

Capital punishment – the state-sanctioned termination of a criminal offender's life, usually for a serious violent offence – is a topic which raises many issues and emotions. Among Western democracies, the United States is the only country that has retained the death penalty. Capital punishment for any form of murder was abolished in the UK in 1965 and in Canada in 1976. In Australia, all States had abolished capital punishment by 1984 – the last executions took place in the mid 1960s (Potas and Walker 1987). There has not been any indication that a reversal of this policy is being contemplated. Nevertheless, with high-profile international cases of terrorism and war crime being brought to justice, capital punishment has once again come to the forefront of public consciousness.

Historically, the use of different forms of punishment is driven by a range of social, cultural and political factors, often quite unrelated to the effectiveness of these sanctions for crime control (Beattie 1986; Garland 1990). As many have argued, the use of capital punishment is predominantly a political and moral issue: 'the crucial question … concerns its legitimacy and propriety, rather than its efficacy' (Zimring and Hawkins 1986, p. 167). In the United States at least, the decision to use or abolish capital punishment has never been based on a dispassionate assessment of the research evidence regarding its efficacy; rather, research is used by people with committed views to support their convictions or discredit opponents (ibid., p. 184). Indeed, analysts of policymaking have found that when opinions are sharply divided, research is often used to 'supply evidence that will reassure supporters, convince the undecided, and weaken rivals' positions' (Weiss 1991, p. 41; see also Majone 1989). However, this political use of knowledge at times of conflict does not preclude the possibility that research evidence can be used in a more positive way to clarify issues and inform policy, especially where there is a broad societal consensus regarding values and goals (Weiss 1991).

There is by now a vast literature of empirical research on the deterrent effect of capital punishment, often pointing to quite opposite conclusions. Some of the research studies involve statistical models that are highly technical and inaccessible to the general reader. The sheer volume of publications on the topic is itself a barrier to an informed public opinion on the research evidence. The aim of this bulletin is to analyse and summarize this evidence in as accessible a way as possible, without oversimplifying or omitting key issues. The focus of this literature review is on one specific question: how effective is capital punishment in preventing others from committing a similar crime? In other words, does the threat of a death penalty deter people from crime?

This bulletin is organized as follows. We will briefly discuss the theory of deterrence and the history of capital punishment in the rest of this section. Section 2 discusses the techniques researchers have used to assess the deterrent effect of capital punishment. Section 3 provides a survey of the research evidence based on a review of 74 research projects published between 1952 and 2003. Besides presenting research findings on the deterrent effect of capital punishment, we also briefly discuss some of the unintended consequences brought about by the use of capital punishment. In Section 4 we summarize the research evidence and present our conclusions.

THEORY OF DETERRENCE

Before considering the effectiveness of capital punishment, it is important to describe, in specific terms, the underlying theory of deterrence. Deterrence, in its simplest definition, is an effect where a threat of punishment 'causes individuals who would have committed the threatened behaviour to refrain from doing so' (Zimring and Hawkins 1973, p. 71). A common distinction is drawn between *individual* or *special deterrence*, which refers to the deterrent effect of punishment on the individual who was punished, and *general deterrence*, which refers to the 'inhibiting effect of sanctions on the criminal activity of people *other than* the sanctioned offender' (National Research Council Panel on Deterrent and Incapacitative Effects 1978, quoted by Zimring and Hawkins 1986, p. 171). For obvious reasons, only general deterrence is at issue as far as capital punishment is concerned. Another important distinction is that between *absolute deterrence* and *marginal deterrence* – the former compares the effect of one form or level of punishment with the effect of no punishment, while the latter compares the effect of one form or level of punishment against another. Again, it is obvious that with research on capital punishment, it is not absolute deterrence that is of interest, but marginal deterrence. In other words, the crucial question is whether capital punishment is a more effective deterrent than an alternative option such as life imprisonment.

How is deterrence supposed to work? Zimring and Hawkins (1973) have argued that deterrence may work in one or more of the following ways:

1. *Simple deterrence* – the threat of punishment can cause a 'change of heart' in a person who is about to commit a crime. This change of heart is the result of weighing up the 'pleasure' of committing the crime with the risk of the pain of punishment. This is the main source of deterrence.
2. *Punishment as a moralising force* – the threat of punishment conveys the degree of disapproval that society has placed on the crime; this disapproval has an effect on the moral attitudes of people and hence their behaviours.
3. *Punishment as a habit builder* – the threat of punishment can induce and reinforce compliant behaviours to the point that people observe the law as a matter of habit.
4. *Punishment builds respect for the law* – the fact that people cannot break the law with impunity builds respect for the law and the legal system and therefore reduces law-breaking.
5. *Punishment as a rationale for conformity* – the existence of the threat of punishment can provide a reason for conformity, especially when a person is subject to group pressure.

The deterrent effect of punishment can be affected by its *certainty* – the extent to which the threat of punishment is realized in practice, its *celerity* – the swiftness with which punishment is delivered and its *severity* – the level of 'pain' imposed (Tittle 1969).

HISTORY OF DEATH AND DISCRETION

As a background to a review of contemporary research into the efficacy of capital punishment for crime control, it is important to be aware of its historical roots and be informed by the issues this history raised. Capital punishment has a long history – for most of the last millennium, it was the primary judicial sanction in England, a country whose institutions have influenced many others. British imperial expansion into America and Australia carried with it a legal code founded on the death sentence. Punishment was not graded beyond a simple division into minor and major crime. While a minor offence – a misdemeanour – was punishable by a whipping, fine, or a few days in the local gaolhouse, all felonies were capital (Radzinowicz 1948-86). Felonies included crimes of treason, murder and rape, but also included property crimes such as robbery, burglary and simple grand larceny: the theft of property worth more than one shilling (without aggravating circumstances). Faith in capital punishment was governed less by a belief that it deterred crime than by the conviction that eliminating an offender restored society's health, just as the body was restored by the amputation of an infected limb (McGowen 1987). The death sentence was not supplemented by any 'secondary' punishments until the rise of penal transportation in the 18th century and imprisonment in the 19th century. The necessary precursor to these developments was an intellectual shift which embraced concepts of graduated punishment and of the human potential for reform.

Inflicting the death sentence on most crimes should, at least in theory, have led to a very high execution rate. Mitigating the severity of this code was the Benefit of Clergy, described as 'the massive fiction that tempered in practice the harshness of the common law rule that virtually all felonies were capital offences' (Beattie 1986, p. 141). Originally an exemption for clerics allowing them to be punished by an ecclesiastical court, the Benefit of Clergy had broadened over the 14th and 15th centuries to forestall the death penalty for lay people who were deemed to be literate (as assessed by the use of a reading test). Initially, offenders could be granted the benefit on more than one occasion, but by the 15th century, this form of reprieve was limited to one

instance only (at least in theory). To guarantee that offenders did not receive the benefit twice, when guilty felons were granted benefit, they were branded with a 'T' for thief or 'M' for murderer (Beattie 1986). By 1706, the reading test was abolished, in effect permitting the discharge of illiterate felons (ibid, p. 142).

This apparent softening of the system was accompanied by an opposing trend which began in the 15th century and continued for the next 300 years whereby certain offences were reclassified as 'non-clergyable'. A flurry of legislation over the 18th century widened the definition of property crime, creating over 200 capital statutes that together came to be known as England's 'Bloody Code' (Beattie 1986, Jones 1982, Thompson 1975).

The code was censured by contemporaries for its inhumanity and for brutalizing society (McGowen 1986). Executions were public spectacles and were increasingly criticized as sanctioning violence and undermining state authority, especially in cases where spectators' sympathy lay with the condemned. The second main criticism, and the one that would carry legal reform, was that the widespread use of capital punishment was ineffective. Without graded punishment, the rational offender 'might as well be hung for a sheep as a lamb'. Prosecution (already private and costly) was uncommon because many victims were reticent to see thieves and robbers hang (Shubert 1981). Juries demonstrated similar qualms, enthusiastically finding 'partial verdicts'. The seriousness of a property crime was measured by the value of the goods stolen; in a partial verdict, the jury found the defendant guilty of a crime of lesser value than their indictment, thus reclassifying the offence as a misdemeanour subject to a different punishment regime (Beattie 1986). Judges, too, had wide discretionary powers in sentencing and in granting pardons. Even though an average of 66 capital convictions were passed every year between 1701 and 1834 at the Old Bailey central criminal court, there were, on average, only 16 executions (Gatrell 1994, p. 616). What distinguished the 16 who would hang from the other 50 is unclear, and was no doubt equally bewildering to those awaiting news of their impending punishment. The whole system was unpredictable and based on an arbitrary use of discretion (Hay, Linebaugh and Thompson 1975, King 2000). Criticisms that the legal code was ineffectual and brutalizing gained political ground as other aspects of criminal justice were reformed: the rise of modern policing, publicly funded prosecutions, and alternative and graduated punishments. In the space of 20 years, the Bloody Code was dismantled, with only murder and treason remaining as capital offences in 1841.

In Britain, the abolition of capital punishment for murder continued to be debated for over a century (see, for example, Bailey 2000) but was finally removed from the statutes in 1965. It was at this time that Australia relinquished the use of the death penalty. Shortly after, in 1968, the US refrained from exercising this form of punishment, and in 1972, the US Supreme Court ruled that capital punishment was unconstitutional. A moratorium on capital punishment followed. When four years later the same court changed its position by deciding that some executions were constitutionally acceptable, it set the stage for States to decide whether to reintroduce executions. The moratorium and subsequent reinstatement of the death penalty in some States created the conditions of a 'natural experiment' on the deterrent effects of this form of punishment. By 2000, 13 jurisdictions (including the District of Columbia) had decided against reintroducing the death penalty; 7 States had reintroduced the death penalty but had not used it; a further 24 States had used it infrequently (less than once per year); and 7 States somewhat more frequently, ranging from South Carolina where 25 people were executed post-moratorium, to Texas with 239 executions (Snell 2001). Not surprisingly, the US has become the focus for research on the deterrent effects of capital punishment.

2. TECHNIQUES FOR ASSESSING THE DETERRENT EFFECT OF CAPITAL PUNISHMENT

The research literature on the deterrent effect of capital punishment is extensive. Our collection is based on keyword searches using the available library databases and a compilation of bibliographic references used by authors in publications. It covers all the major publications cited in reviews such as those carried out by Hood (2002) and Zimring and Hawkins (1986), as well as those referred to in more recent publications. A total of 74 separate empirical studies were identified. These form the basis of the survey, the findings of which will be discussed in Section 3. In this Section we briefly describe the range of techniques used in the literature for assessing the deterrent effect of capital punishment, and discuss the major methodological and theoretical issues that require attention for the research evidence to be appropriately interpreted.

TYPES OF ANALYSIS

As Hood (2002) has pointed out, the research literature is almost exclusively focused on the use of capital punishment for murder. The vast majority of studies have been conducted in the United States of America, where the death penalty is widely practised (see, however, Avio 1979 and Layson 1983 for Canadian studies and Wolpin 1978 for a UK study). The United States provides favourable conditions for empirical research on capital punishment for at least two reasons: first, differential adoption of capital punishment among States provides natural variations for cross-sectional analysis (i.e., comparison across States); and second, the ten-year moratorium (1968-1977) on executions set up a 'natural experiment' for comparing the effectiveness of different punishment regimes. In general, research studies are based on one or more of the following designs (see Hood 2002):

1. *Simple Before/After Comparisons*

   This design helps researchers assess whether the abolition or introduction of capital punishment within a particular jurisdiction has led to an increase or decrease in the rate of murder. Examples of this design include Barber and Wilson (1968), Samuelson (1969), and Archer, Gartner and Beittel (1983).

2. *Cross-Jurisdiction Comparisons*

   This design allows researchers to compare murder rates within a fixed period of time between jurisdictions that have abolished capital punishment with those that have retained or reintroduced it. Comparison is usually made between jurisdictions that are similar in social, demographic and economic characteristics. Some studies are based on simple statistics on homicide rates; others calculate correlation coefficients between execution and homicide rates. Examples include Sellin (1959), Lempert (1983), and Cheatwood (1993).

3. *Short-Term Effect of Executions*

   The effect of well-publicized or notorious executions on subsequent murder rates is assessed using a simple before/after comparison, usually over a short time period (days or weeks) before and after the execution. Examples of these studies include Savitz (1958), McFarland (1983), Grogger (1990) and Thomson (1999).

4. *Multiple Regression Using Time-Series, Cross-Sectional and/or Panel Data*

   The design makes use of linear economic models to estimate the impact of executions on homicides over time and/or across jurisdictions, while 'controlling for' a range of social and legal variables. Examples include Ehrlich (1975a), Passell (1975), Dezhbakhsh, Rubin, & Sheperd (2003) and many others.

Earlier studies of the deterrent effect of capital punishment generally made use of the first three designs, while later studies have tended to employ multiple regression models. Because of its relative complexity, and the increasing proportion of research studies using econometric models, we will explain this approach in some detail below.

ECONOMIC MODELS OF DETERRENCE

Ehrlich (1972; 1973) was the first to make use of an econometric model to analyse the deterrent effect of criminal law enforcement. His approach (Ehrlich 1975a) has since been used by a long line of researchers with minor modifications (Layson 1983, Dezhbakhsh, Rubin and Shepherd 2003). Figure 1 provides a simplified explanation of Ehrlich's framework as described by Barnett (1978, pp. 292-293 and 302-303). Essentially, the model consists of a mathematical equation that links the 'average unhappiness' of a potential murderer with the risks of being arrested, being convicted

Figure 1: Ehrlich's Model

EHRLICH'S ANALYIS OF A RATIONAL MURDERER'S ASSESSMENT OF THE RELATIVE IMPORTANCE OF DIFFERENT PUNISHMENT-RISK VARIABLES

If a person commits a murder, four possible consequences related to punishment can arise:

(1) No arrest, thus no punishment

(2) Arrest, but no conviction for homicide

(3) Arrest, conviction for homicide, some non-lethal punishment

(4) Arrest, conviction, execution.

Let

$p(a)$ = Probability of arrest for a randomly chosen homicide

$p(c|a)$ = Probability of conviction for homicide given arrest for homicide

$p(e|c)$ = Probability of execution given conviction for homicide

Suppose the number U_i is a measure of a potential murderer's happiness (utility) if outcome i occurs. If the person in question is rational, Ehrlich conjectures, the numbers U_1, U_2, U_3, U_4 will be decreasing. (The last three could presumably be negative.) Let p_i be the probability that the murder will result in outcome i. The p_i's are related to the risk-variables $p(a)$, $p(c|a)$, $p(e|c)$ defined earlier by the equations:

$$p_1 = 1 - p(a)$$

$$p_2 = p(a)(1 - p(c \mid a))$$

$$p_3 = p(a)\, p(c \mid a)(1 - p(e \mid c))$$

$$p_4 = p(a)\, p(c \mid a)\, p(e \mid c)$$

These equations follow from basic laws of probability; p_4, for instance, is the product of the probabilities of arrest, conviction given arrest, and execution given conviction (outcome 4). The number U given by $U = U_1 p_1 + U_2 p_2 + U_3 p_3 + U_4 p_4$ is a measure of the "average" unhappiness that commission of the murder will bring to its perpetrator, since it weights the unhappiness associated with each of the four outcomes by the probability that outcome occurs.

(Source: Barnett 1978, p.292-293 and 302-303)

following arrest, and being executed following conviction. For example, if the risk of arrest, conviction or execution is increased, the potential murderer's unhappiness will also increase. A rational offender would want to minimize unhappiness — he or she is therefore likely to be deterred by the increased risks of law enforcement, including the death penalty. Using this basic framework, Ehrlich creates a mathematical form (called the 'murder supply function') that expresses the murder rate in terms of these punishment-risk variables as well as other variables that may affect the murder rate. Because of the technical complexity of the material, this bulletin will not elaborate on the mathematical form and its transformation, although they form the basis of much subsequent debate on the validity of Ehrlich's analysis. Interested readers should consult the original sources listed among the references.

UNDERLYING ASSUMPTIONS OF ECONOMIC MODELS

The adequacy of a model for representing a real-life situation is often judged by the extent to which its underlying assumptions are valid. Estimates of the deterrent effects of capital punishment based on economic models share a number of assumptions, some of which relate to highly technical issues such as the mathematical form of the murder supply function (see, for example, Dezhbakhsh, Rubin and Shepherd 2003). In this section, we will focus on the less technical, but nevertheless important assumptions that are relevant to these models.

First, econometric models typically assume that people are rational in their commission of crime (usually murder) and that they respond to incentives and disincentives in making their decision to commit or refrain from crime (Hood 2002). This assumption is often considered unrealistic, especially since murders are often committed when the perpetrator is angry, fearful, or otherwise out of control. There is usually little opportunity for the murderer to reflect on the likely consequences of action. Ehrlich, however, did not see the emotionality of murder as a necessary barrier to deterrence: 'There is no reason a priori to expect that persons who hate or love others are less responsive to changes in costs and gains associated with activities they may wish to pursue than persons indifferent towards the well-being of others' (Ehrlich 1975a, p. 399). Even so, he acknowledged that not all murderers respond to incentives, but 'it is sufficient that at least some so behave' for the theory to be useful in explaining murder rates (ibid, p. 415). Dezhbakhsh, Rubin and Shepherd demonstrated mathematically that inferences about deterrence are unaffected by the inclusion of non-negligent manslaughter and non-premeditated crimes of passion in murder statistics (2003, pp. 355-6).

The second assumption relates to the offender's perception of the risks involved in the crime. The model equates perceived risks with the objective risks of apprehension, conviction and execution (Gibbs 1977). Given that offenders' subjective evaluation of the risks involved may vary according to their knowledge of the *actual* risks or their personality (e.g. an optimist might underestimate their risks of apprehension, see Barnett 1978, p. 294), such an assumption is not always warranted. In general, it may be argued that offenders' perceptions about the risks of execution may be more accurate than their perceptions about the risks of apprehension and conviction, especially if executions are widely publicized.

Finally, regression analyses generally assume that the dependent variable (here the murder rate) is 'caused' by the independent variables (risk of execution, etc). However, it is conceivable that higher murder rates can lead to tougher law enforcement and punishment policy, and hence the causal direction is reversed. This problem is sometimes dealt with by using a system of simultaneous equations, or time-lagged variables (e.g. murder rates for a particular year is used as dependent variable while the average execution rate for earlier years is used as independent variable).

MEASUREMENT OF VARIABLES

Regardless of which approach researchers take, they are limited by the data, both in terms of quality and in terms of availability. While the usual problems with official crime statistics (e.g., under-reporting or discretionary decisions regarding recording) are not regarded as serious for homicide, other variables typically used in empirical studies of deterrence are not as easily available or as reliable as homicide data. For example, Fox and Radelet (1989, p. 36) pointed out that disposition data (in relation to the number of people charged and convicted) used by Layson (1985) are 'of such poor quality that they are useless for research purposes'. Ehrlich's (1975a) model originally took into account legitimate and illegitimate income opportunities, but these measures were not readily available, so certain demographic variables were used as substitutes or 'proxies' (Dezhbakhsh, Rubin and Shepherd 2003, p. 351).

3. RESEARCH EVIDENCE ON DETERRENT EFFECT

Table 1 summarizes the results of the 74 research projects identified in the literature review. The projects used a variety of methodologies, covered a range of geographical areas (mainly in the United States) and time periods. The majority of the studies show that the use capital punishment did *not* deter the commission of homicide. If we exclude the studies that used relatively unsophisticated research designs, and only count the 61 that involved regression or ARIMA (autoregressive integrated moving average, a statistical modelling technique for time-series data) analysis, only 14 (23%) found evidence consistent with a deterrent effect, while 40 (66%) concluded that there was no deterrent effect, and the remainder (11%) were inconclusive or found contradictory results. It is noteworthy that all but two of the studies that reported a deterrent effect used an economic-type model, although not all studies that used such models came to the same

conclusion. Since research studies using econometric models represent some of the 'best' and most sophisticated in this area (Hood 2002, p. 226), it is important to devote the next few sections to a more in-depth discussion of the issues raised by the studies using economic models.

THE BEGINNING OF ECONOMIC MODELS: THE WORK OF ISAAC EHRLICH

In 'The deterrent effect of capital punishment: A question of life and death', Ehrlich (1975a) developed his model of the rational would-be murderer who weighs up the costs and benefits of committing homicide according to the risks of arrest, conviction and execution (see Figure 1). Ehrlich's model predicted the murder rate (expressed as homicides per 1,000 of the civilian population) from the three punishment-risk variables – the probabilities of arrest, conviction, and execution – and a number of other relevant variables: the civilian unemployment rate, the fraction of the civilian population in the labour force, estimated real per capita income, the fraction of the residential population aged between 14 and 24 years, chronological time, the fraction of nonwhites in the population, the size of the civilian population, real government expenditure (excluding defence), and real expenditure of police lagged by one year. The model was then tested empirically with national time-series data from the US covering the period 1933-69. Instead of using the natural values of the data, Ehrlich worked with logarithmic values. This is a common technique employed where relationships between variables are not linear but take the form of a curve.

Ehrlich's model predicted that murderers are most sensitive to changes in the likelihood of being arrested, less sensitive to changes in the probability of conviction, and least sensitive to changes in the probability of being executed. This is because the latter risk variables are conditional on the earlier risk variable(s). For example, an increase in the likelihood of arrest also increases the overall (unconditional) probability of conviction and that of execution. A number of empirical regressions confirmed his 'rational murderer' hypothesis. The 'partial elasticity' of the murder rate – the percentage change in murder rate achieved by a one per cent change in the risk of arrest – ranged between -1.0 and -1.5. The corresponding elasticity associated with the risk of conviction varied between -0.4 and -0.5, while that associated with the risk of execution varied between -0.039 and -0.068. While the figures for the risk of execution were small, they were statistically significant, indicating that executions exerted a deterrent effect on those contemplating homicide. Calculating a marginal 'trade-off' between executions and murders using these values of execution risk, Ehrlich tentatively estimated that 'an additional execution per year over the period in question may have resulted, on average, in 7 or 8 fewer murders' (p. 414). It is this finding that has been frequently cited in subsequent literature and political debates. Ehrlich noted, however, that this was an example only. Depending upon the confidence interval chosen, the figure could be as high as 24 fewer deaths, or as low as zero (1975a, p. 414).

In addition to finding that murders could be decreased by increasing the risk of arrest, conviction, and execution, Ehrlich's model found other variables associated with reductions in homicide. Reducing unemployment exerted the same size negative effect as did raising the risk of execution (p. 410). Labour force participation and income levels were also significantly linked with the murder rate. Ehrlich concluded from his regression that:

> Given the validity of the analysis pursued above, incarceration or execution are not exhaustive alternatives for effectively defending against murders. Indeed, these conventional punishments may be considered imperfect means of deterrence relative to monetary fines and other related compensations because the high "price" they exact from convicted offenders is not transferable to the rest of society. Moreover, the results of the empirical investigation indicate that the rate of murder and other related crimes may also be reduced through increased employment and earning opportunities. The range of effective methods for defense against murder thus extends beyond conventional means of law enforcement and crime prevention. (p. 416-7)

Ehrlich's model offered qualified support for the deterrent effect of capital punishment. However, it also indicated that increased rates of arrest and conviction had a more powerful effect on deterring homicide, and that socio-economic factors were as influential as executions on the murder supply function.

Ehrlich followed up his time-series analysis with a cross-sectional study focused on just two years – 1940 and 1950 – and disaggregated the data by State, noting which States executed offenders and which did not. This research design was similar to work by Passell (1975) who had found evidence to support imprisonment as a deterrent to murder but no evidence to suggest executions deterred. The regression model used by Ehrlich differed slightly from his earlier study, but again tried to capture a series of socio-economic variables as well as the risk of conviction, execution, and previous time in prison (1977, p. 750). Ehrlich reported that the findings corroborated his earlier study, yielding consistent results that linked executions to reductions in homicide, and also to reductions in robbery which was not subject to capital punishment (1977, p. 778). By including a separate variable for imprisonment, and by disaggregating executing and non-executing States, Ehrlich was able to more precisely measure the impact of executions, estimating that among executing States, each additional execution reduced the number of murders by between 20 and 24 (p. 779). He also noted the 'intriguing' finding that 'persistently non-executing States face substantially lower risks of victimization through murder and related crimes than do the persistently executing States' (1977, p. 780), a distinction he partly attributed to ethnic and other demographic variations, and to

misguided perceptions about the risk of execution. Ehrlich signalled the need for further research to make sense of this finding.

CRITICISMS OF EHRLICH

Ehrlich's (1975a; 1977) research has spawned 'an industry of critiques' (Ehrlich 1982, p. 125), subjecting his work to a closer scrutiny than perhaps any other piece of work on the deterrent effect of punishment. While it is beyond the scope of this bulletin to give an independent evaluation of the validity of all the criticisms, we hope to guide the reader through some of the major criticisms, partly as an illustration of the complexity of the issues, and partly to stimulate further research and debate. As Cameron (1994, p. 209) observes, the jury on the econometric evidence on the effects of capital punishment is still out – Ehrlich's results have as yet not been 'thoroughly discredited' as claimed by Lampert (1983, p. 89). There is clearly room for further research in different jurisdictions using more reliable data and improved techniques.

Mathematical Form

Subsequent studies have replicated Ehrlich's earlier time-series work but not always his results (Passell and Taylor 1977; Albert 1999). Bowers and Pierce (1975) managed to repeat Ehrlich's study and obtain the same findings. However, they drew attention to the mathematical form used by Ehrlich and a problem with the use of logarithms (see Ehrlich 1975b for his response and Passell and Taylor 1977 for further problems). There is no logarithmic value for zero, so in years when the execution rate was zero (from 1964), Ehrlich substituted a value of 1. This was subsequently identified as an important random choice. When Bowers and Pierce ran the regression model using the data in a non-logarithmic form (i.e., using the natural values), they found no evidence of a deterrent effect of executions. Indeed, the only statistically significant results were *positive*, associating executions with high homicide rates. Barnett (1978, p. 298) suggested that Bowers and Pierce might have 'exaggerated the weaknesses of Ehrlich's model relative to others when $p(e|c)$ is small'. In his view, Bowers and Pierce's linear form has its own problems.

Passell and Taylor (1977) also re-examined Ehrlich's time series model, finding it sensitive to the choice of mathematical form and the explanatory variables included. Employing an alternative form, they found no evidence to support deterrence. What constitutes the appropriate mathematical form of the murder supply equation has continued to be an area of debate (Peck 1976; Hoenack and Weiler 1980; Layson 1985; Dezhbakhsh, Rubin and Shepherd 2003).

Period Covered

In addition to questioning Ehrlich's functional form, Bowers and Pierce (1975) ran the original logarithmic-based analysis excluding the problematic years when there were no executions. Again, the deterrent effect disappeared. Bowers and Pierce concluded that Ehrlich's analysis 'yields evidence of a deterrent effect only by relying on the unusual nature of the years after 1964 and on the logarithmic transformation of the data' (ibid, p. 206).

Sensitivity to the choice of years had been highlighted by Baldus and Cole (1975) who found that, depending on their choice of time period, the model could be made to generate a *positive* association between risk of execution and the homicide rate. Klein, Forst and Filatov (1978) produced an insignificant negative coefficient by terminating the data in 1962. Ehrlich's model was not robust to changes in selection of years.

Another related issue arose for the time series analysis. Researchers found reason to question the assumption that there was a constant relationship over time between the predictor variables and the homicide rate. For example, in the United States, unemployment rate may have a declining impact on homicide between 1933 and 1969 since 'social legislation and the growth of union power reduced considerably the financial hardships of unemployment' (Barnett 1978, pp. 294-5). Fox and Radelet (1989) also note that the factors and conditions underlying homicide trends are many and diverse, with those influencing the homicide rate in the 1930s different from those influencing the homicide rate in the 1970s. It is therefore not reasonable to assume that the same set of variables would exert the same influence on homicide rate during these decades.

Omitted Variables

The model was also criticized for omitting variables that may potentially influence homicide rates; these include: mean sentence length of those convicted of murder but not executed (Bowers and Pierce 1975); levels of migration into urban centres, the extent of weapon ownership, levels of violent property crime (Baldus and Cole 1975); racial tensions, changes in length of prison sentences, changing aspirations and perceptions of relative deprivation (Passell and Taylor 1977).

Data Problems

Ehrlich (1975a) had used national time-series figures from the FBI for homicides, arrests and convictions. Three problems arose from this. Firstly, these had been generated by the Uniform Crime Reporting Scheme which, in its early years, was considered an unreliable source as it was voluntary and a large number of agencies did not comply (Bowers and Pierce 1975, Peck 1976, Cantor and Cohen 1980). Homicide figures were subsequently readjusted by the FBI on the basis of current data and these were employed by Ehrlich. Low-level reporting was at its worst with regard to arrests and convictions. These data were collected from 'such small and unrepresentative samples of law enforcement agencies' that it was not possible to even attempt a re-estimation of the actual figures (Bowers and Pierce 1975, p. 190). It was not until 1960-61 that the number of agencies reporting arrests and convictions increased significantly. Problems with arrest and conviction figures adversely affect all punishment variables, as execution-risk is conditional on arrest and conviction.

Secondly, general homicide rates included less rational offenders, for example, domestic and non-felonious murderers, and might therefore obscure the effect of executions on rational thinkers (Cochran, Chamlin and Seth 1994).

Thirdly, some scholars have argued that there are problems with using national statistics. Aggregated data can obscure the real relationship between homicide and punishment, and create problems for causal inference — to conclude that executions deter homicides, the drop in homicide rate should occur in the same jurisdiction as the executions.

The alternative approach was to use cross-sectional data (Ehrlich 1977). This enabled comparison across and between jurisdictions. Cross-sectional data have the disadvantage of limiting the number of cases to the number of units of analysis (e.g. States in the case of Ehrlich's work, or counties in some subsequent studies). Further, the approach suffers from 'unobserved heterogeneity'; that is, because the data deal with a given place and time, unusual conditions specific to that place and time may not be apparent (Dezhbakhsh, Rubin and Shepherd 2003).

Ceteris Paribus

Ehrlich has emphasized that the execution-murder trade-off estimate was subject to the conditions that all other factors were held constant: 'The actual tradeoffs between executions and murders thus depend partly upon the ability of law enforcement agencies to control simultaneously the values of all the parameters characterizing law enforcement activity' (Ehrlich1975a, p. 415). In practice, it is not possible to hold arrest and conviction probabilities constant while execution rates change. Since the abolition of capital punishment in England and Wales in 1965, it had been easier to secure convictions for murder, with the proportion of offenders convicted of murder rising from 28 per cent in 1965 to 49 per cent in 1998-99 (Hood 2002, p. 223. For the US see Glaser 1979). Wolpin's (1978) findings for England and Wales indicated that by far the largest reduction in homicide was not brought about by executions, but by an increase in the proportion of homicides cleared as murder (rather than manslaughter). This was so in Ehrlich's (1975a) formulation in which changes in the probability of conviction exert a larger influence than equivalent changes in the risk of execution.

THE CASE FOR DETERRENCE

Different forms of multivariate analysis, some with and some without economic modelling, have continued to be employed in an attempt to reach a definitive answer regarding the deterrent effect of capital punishment. Primarily, the case for executions deterring further offences is made by studies which adopt econometric models of analysis. These research papers have become highly technical, typically identifying problems with previous studies before positing a new technique or modified model. The following will sketch out some of the broad arguments and approaches; more detailed summaries can be found in Freeman (1993), Cameron (1994), and Avio (1998). Economic studies which found evidence consistent with a deterrent effect, and which have not already been mentioned include: Yunker (1976), Cloninger (1977, 1987, 1992), Cloninger and Marchesini (2001), Layson (1985, 1986), Chressanthis (1989), and Dezhbakhsh, Rubin and Shepherd (2003).

Using annual US national data from 1933 to 1972, Yunker (1976) tested a model which assumed that executions are positively related to the current homicide rate, and the homicide rate is inversely related to the past level of executions. His execution-murder trade-off predicted that one execution would deter 156 murders (p. 381). For a critique of this remarkable finding, see Fox (1977) who demonstrated the serious methodological flaws in Yunker's analysis.

Cloninger (1977) carried out a multiple regression analysis on cross-sectional data from 48 US States for 1960, finding results consistent with deterrence theory. His later paper (Cloninger 1987) rectified some technical errors in the original study and found stronger support for deterrence. In 1992, he conducted a new study using FBI crime data for each US State and the District of Columbia during 1983-1988. Results of this second analysis were consistent with the deterrence hypothesis: 'both the relative risk and residual risk of homicide is significantly reduced in the presence of executions' (Cloninger 1992).

A similar approach was adopted by Cloninger and Marchesini (2001) when studying homicides in Texas. Between 1996 and 1997, the number of executions in Texas each month varied substantially. As a result of a Court of Criminal Appeal decision to stay an execution, only three prisoners were executed in 1996. Once the Court had lifted the stay, the number executed increased to 37 prisoners in 1997. Using FBI data for the years 1989-1997, the researchers were able to calculate 'expected values' for homicides during 1996-1997. These expected values were then compared with the actual numbers of homicide cases. Since the number of homicides in 1996 was greater than expected, and in 1997 fewer than expected, the researchers concluded that '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' (Cloninger and Marchesini 2001, p. 576).

In addition to his work on Canada mentioned earlier, Layson (1985) attempted to correct for faults in Ehrlich's work and also extended the analysis up to 1977. He employed an alternative data source (vital statistics). Testing for variations over time, using different explanatory variables, and a variety of mathematical forms, Layson found 'solid support' for a powerful deterrent effect associated with increases in probabilities of arrest, conviction and execution. He calculated a trade-off between executions and murders of 18.5 lives, plus or minus 10 (p. 80). Layson (1986) confirmed that similar findings could be achieved with

the FBI data on homicides used by Ehrlich by omitting the first few years (1934-1937).

Chressanthis (1989) examined the relationship between murder and non-negligent manslaughter and capital punishment in the US for the period 1965-1985. The analysis found support for a deterrent effect of capital punishment. It also found that changes in law enforcement, judicial, demographic, and economic control variables were significant and in line with the economic model of criminal behaviour.

The most recent econometric study is that of Dezhbakhsh, Rubin and Shepherd (2003). They used a system of simultaneous equations and panel data from 3,054 US counties over the years 1977-1996. They include in their model control variables such as criminal justice expenditures, political pressure to get 'tough on crime', rates of other crimes, economic and demographic variables, and the rate of gun ownership. The results show support for a strong deterrent effect. Calculating a trade-off for the US in 1996, they estimated that one execution resulted in 18 fewer murders, with a margin of error of 10.

Dezhbakhsh, Rubin and Shepherd (2003) tested the robustness of their results with regard to variations in aggregation level, mathematical form, sampling period, and ways of modelling death penalty laws and the probability of execution. They concluded that their results were not sensitive to differences in model specifications. However, it is worth noting that this still does not mean universal support for the deterrence hypothesis: of the 55 models analysed, the estimated coefficient of the probability of execution was negative and statistically significant in 49, negative but insignificant in four, and positive in two (one of which is insignificant, the significance test of the other was not shown). Note also that two of 37 significant results in the robustness check were significant only at the 90 per cent confidence level (see Dezhbakhsh, Rubin and Shepherd 2003: Table 5).

While the work of Dezhbakhsh, Rubin and Shepherd (2003) is too recent for published rejoinders, this is not so with the other studies (e.g. Fox 1977 on Yunker 1976). Cover and Thistle (1988) tested the time-series properties of US homicide rate and found that the data violated some of the basic assumptions of regression analysis. When they reanalysed Layson's (1985) data using different methods to get around this problem, they obtained a mixture of insignificant and significant results. They concluded that the results are sensitive to the way the probabilities of arrest, conviction and execution are defined. In addition, Fox and Radelet (1989) criticized Layson (1985) for failing to adequately account for changes over time in the underlying relationships between variables.

Public Awareness

If executions exert a general deterrent effect on homicide one would expect more publicized executions to exert a bigger deterrent effect. Using a published set of weekly homicide statistics for London from 1858 to 1921, and a standard casebook of 'notorious murderers' to produce a list of widely publicized English executions, Phillips (1980) found that, on average, homicides decreased by 35.7 per cent during the two weeks immediately following a publicized execution. The more publicity devoted to the execution, the more homicides decreased thereafter. This effect is only short-term, as the number of homicides rose again within five to six weeks after the execution.

Stack (1987) examined publicized execution news stories in the US during 1950-80 and found an inverse relationship between publicized executions and homicide. He estimated that, on average, the months with highly publicized stories of executions had 30 fewer homicides than those without such stories. A review of this work by Bailey and Peterson (1989) identified shortcomings which, when corrected for, indicated a very limited correlation between publicized executions and lower homicide rates which evaporated over the following months.

Further work by Stack (1993) on Georgia failed to repeat the original result. Monthly data were used with the outcome that a publicized execution was associated with an *increase* of 2.6 homicides, or a 6.8 per cent increase in the month of the publicized execution. The results provided no support for deterrence theory. However, when Stack (1998) examined California for 1946-1955, the deterrent effect was again found. Using monthly data on publicized executions in California, Stack found that a publicized execution was associated with a *decrease* of 2.6 homicides, or a 12.7 per cent decrease, in the month of the publicized execution. These findings are inconsistent with Grogger's (1990) study of California for the later period 1960-1963. Using daily homicide data and a Poisson regression model, Grogger found no evidence to support the short-term deterrent effect of executions.

Bailey's (1998) research on Oklahoma considered print media attention to executions along with the frequency of executions and other socio-demographic variables in explaining various types of homicides. The regression analysis employed weekly data for the period 1989-91. Instead of a deterrent effect, Bailey found that media coverage of executions was accompanied by an overall increase in ten categories of homicide. One exception to this pattern was the decrease of non-felony killings involving strangers, which occurred for two weeks after the media coverage.

THE CASE AGAINST DETERRENCE

As mentioned earlier, the majority of the better designed research studies presented in Table 1 were not able to demonstrate that capital punishment had a deterrent effect on homicide. This is consistent with the opinions of eight out of ten experts (67 present and former presidents of the top US academic societies of criminology) surveyed in the mid 1990s (Radelet and Akers 1996). The experts based their opinion on 'their knowledge of the literature and research in criminology' (ibid, p. 7).

Table 1 gives a summary of the research studies that found no deterrent effect (as indicated by a minus sign in the last column); it provides for each study the date of publication, the geographical area covered, the time period examined, the type of crime focused on, the methodology used and the final conclusion. We will not attempt to summarize each study further, but will make some general points about the range of methods employed. Note, however, that some of the studies are replications or extensions of each other, based on overlapping sets of data, rather than totally independent projects. For example, Bailey, working alone or with Peterson, was responsible for 22 of the published works cited in Table 1; while the time periods varied, fourteen of the studies examined the US as a whole or a large number of States, while eight focused on an individual State or city. Except for two on rape (Bailey 1976b, 1977a) and one on non-capital felonies (Bailey 1991), these studies consistently found no evidence that capital punishment had a deterrent effect on homicide.

Many of these studies have used research designs similar to those discussed above in which evidence of deterrence was found: multivariate regression analysis using time series data (e.g. Stack 1993, Decker and Kohfeld 1984), others using cross-sectional data (e.g. Boyes and McPheters 1977, Black and Orsagh 1978), or a combination of both (e.g. Albert 1999, Knorr 1979, Rahav 1983). Some studies have been careful to distinguish between different categories of homicide (e.g. Cochran and Chamlin 2000, Thomson 1999, Sorensen, Wrinkle, Brewer, and Marquart 1999, Cochran, Chamlin and Seth 1994), and different categories of offenders (Bailey and Peterson 1999, Stack 1995), highlighting problems with the generalisability of deterrent findings. On occasions, crimes other than homicide have been the focus (e.g. rape, Bailey 1977a) or specific types of homicide (e.g. police killings, Bailey and Peterson 1994). The choice of variables fed into these models can differ, including the length of imprisonment (Bailey 1980a), the celerity of the death penalty (Bailey 1980b), and gun ownership (Kleck 1979), to name but three.

Other related methods have also been employed. Archer, Gartner and Beittel (1983) did a cross-national study of trends in homicides following the abolition of capital punishment, finding that, more often than not, absolute numbers of murders fell. Forst (1983) did a similar study of US States, examining if homicide rates rose most in States with the greatest decline in the use of capital punishment. Forst found no perceptible impact of the death penalty on homicide rates. A related approach was adopted by Cheatwood (1993; the same study was reported in Harries and Cheatwood 1997) who analysed how differences in violent crime rates related to the practice of capital punishment in different US counties using 'pairs'. In order to enhance comparability but also capture differences in punishment regimes, Cheatwood selected the 293 pairs of contiguous counties which crossed a State line and therefore jurisdictions. Differences between violent crime rates were calculated for each pair and fed into a regression analysis as the dependent variable. Information on social, demographic and economic variables known to influence violent crime rates was also entered, and for the two States in each matched pair, capital punishment was measured in three ways: the existence of capital statutes, numbers executed since 1976, and numbers currently on death row. The resulting analysis offered no support for deterrence theory. The capital punishment variables did not have any significant effect on the level of violent crime; and what effect they did have was positive, not negative.

Cross-national studies such as Archer, Gartner and Beittel (1983) draw attention to the paucity of research into capital punishment in countries outside the US, a problem which will remain a major barrier to informing public debate. What governs crime rates in one country will not necessarily be universal. Capital punishment in Australia is little researched: homicide rates are lower, and the death penalty has not operated in any State since 1966. The little work that has been undertaken found no evidence of deterrence. Barber and Wilson (1968) undertook a study of Queensland data constructing a 5-year interval time-series over half a century, comparing conviction rates for manslaughter (non-capital) with murder (capital until 1922). For the majority of five-year periods the conviction rate for manslaughter was higher than for murder, indicating juries' reluctance to convict in capital cases. Murder rates fell from 1911, a trend not halted by the abolition of capital punishment in Queensland in 1922, when it was replaced by a mandatory sentence of life imprisonment. Their findings fail to support a deterrent effect, and suggest that harsh penalties promote discretion and lenience in practice.

Perhaps one of the most interesting approaches was adopted by Marquart and Sorensen (1997, originally published in 1989) who effectively undertook a counter-factual analysis of capital punishment when the opportunity arose to conduct a 15-year follow-up study of commuted capital offenders in the US, asking what threat such offenders posed to society. In 1972, 558 capital offenders had their sentences commuted to terms of imprisonment, with 239 of the inmates being released into the community on parole during the course of the study. The project examined both prison and release behaviour. Only one offender committed a second homicide. There were few recorded cases of institutional misconduct; of those on parole, 12 per cent committed new felonies, and a further nine per cent violated their parole and returned to prison. Seventy-nine percent had no further convictions. Marquart and Sorensen found that most capital offenders did not engage in violent behaviour and did not represent a significant threat to society.

What Marquart and Sorensen's (1997) research highlights is the need to wed research questions and methodologies appropriately. While regression analysis, for example, is an excellent technique for assessing association between variables, it cannot measure the direction of causation: the working

hypothesis determines this for a specific model, but cannot prove it.
If the question is about the rationality of offenders, there may be more direct ways of penetrating the individual decision-making process. Psychologists in particular have begun to explore individual-level causes of crime: criminogenic needs, the influence of violence on television, substance abuse etc. The debate over whether capital punishment exerts a deterrent effect on would-be offenders has been intractable, with no definitive answers. Perhaps new methodologies need to be developed and applied before a resolution will be found.

UNINTENDED CONSEQUENCES

An argument is mounted in a number of the studies cited for what is termed a 'brutalization effect' (see, for example, Bailey 1983,1998; Bowers, Pierce and McDevitt 1984; Harries and Cheatwood 1997). While deterrence is hypothesized as a negative relationship between executions and homicides (executions go up, homicides go down), brutalization is the reverse, a positive relationship (executions go up, homicides go up). As the name suggests, in this model citizens are 'brutalized' when the state sanctions death as punishment, becoming inured to acts of violence. The state is seen as both legitimatizing violence as a solution to problems, and as devaluing human life. This has been an enduring argument against the use of capital punishment since the 18th century. Cochran and Chamlin's research on California suggests that the 1992 execution of Harris after a 25-year moratorium 'may have produced two simultaneous but opposing effects: a deterrent effect on nonstranger felony-murders and a brutalization effect on argument-based stranger homicides' (2000, p. 700).

Another unintended consequence is that capital punishment may be applied differentially on racial grounds. Paternoster's (1983;1984) research on prosecutorial discretion in requesting the death penalty in South Carolina found that the race of the victim had a strong influence on this decision, even when legally relevant variables have been controlled for. In particular, 'black killers of whites were more likely and black killers of black less likely to have the death penalty requested' (Paternoster 1984, pp. 466-7).

4. CONCLUSION

The efficacy of capital punishment as a deterrent against crime, as this review of the literature shows, is still a vexed issue among researchers almost thirty years after the publication of Ehrlich's (1975a) econometric model. The weight of the research evidence, covering different jurisdictions at different time periods, still favours the 'no deterrence' conclusion. Most of these research studies, however, have not been subjected to the kind of critical scrutiny that Ehrlich's results have been subjected to. Recent research that supported the deterrence hypothesis (e.g. Dezhbakhsh, Rubin and Shepherd 2003) is bound to revive interest in the capital punishment debate. If the debate since the late 1970s is any guide, the published literature is likely to become polarized and increasingly inaccessible to all but the most technically competent readers, while policy advocates will continue to invoke evidence selectively to support their positions.

It is important for those who believe in the value of research for informing policy and clarifying issues to understand the nature and limitations of social research. Given that it is ethically and politically indefensible to conduct randomised experiments to test the effectiveness of capital punishment as a deterrent, there are only a limited range of methods researchers can use to analyse this issue. These methods have all been tried in the literature reviewed in this bulletin, but they did not lead to consistent findings. Researchers must now look for reasons behind this divergence. However, it is in the nature of research that theories and hypotheses can be tested but the results do not constitute a definitive 'proof' of any theory. What statistical analysis does is produce evidence that can be said to be consistent, or inconsistent, with a given model or theory. All such testing is limited by the data, the research design and statistical inference. If they had yielded a consistent set of conclusions, researchers would have more confidence that the results are invariant under different conditions and not sensitive to variations in research methods.

We agree with Zimring and Hawkins (1986) that the use of capital punishment should be a matter for political and moral choice, rather than based simply on its efficacy as a deterrent against crime. While policy making is in the domain of democratic processes and the political use of knowledge often an inevitable part of that process, researchers can be drawn into the politics simply by doing their job. Despite Ehrlich's disclaimer that he had never advocated the use of capital punishment (1975b, p. 227), his repeated statement that 'the efficacy and desirability of capital punishment are separate issues' (Ehrlich 1982, p. 137) and that he was more concerned with establishing an economic model of general deterrence than focusing on capital punishment (Ehrlich 1982, p. 124), it will not be forgotten that his findings were presented by the US Solicitor General in the Supreme Court as an argument to justify the constitutionality of the death penalty and hence its return (Baldus and Cole 1975, p. 170).

Capital punishment, as Ehrlich (1975a) rightly points out, is 'a question of life and death'. Such questions require conclusive evidence which, as this review of the literature shows, three decades of deterrence research has failed to deliver.

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Table 1: Summary of Research Studies on the Deterrent Effect of Capital Punishment

	Year	Author	Data location	Period covered	Type of crime	Time Series	Cross-sectional	Control variables	Regression	Deterrent effect**	CP a deterrent Yes (+)/ No (–)
1.	2003	Dezhbakhsh, H., Rubin, P. & Sheperd, J	US – 3,054 counties	1977-1996	Homicide	Panel	X	X	X	Deterrent effect, with each execution resulting in 18 fewer murders; increase in arrest, sentencing and execution all deter	+
2.	2001	Cloninger, D. & Marchesini	Texas	1996-1997	Homicide				X	Deterrent effect of executions, comparing low with high execution periods	+
3.	2000	Cochran, J. & Chamlin, M.	California	1989-1995 (Execution 1992)	Homicide: felony murders of non-stranger; argument-murder of stranger	X A*				Confirmed predictions that executions reduced felony-murders of non-strangers, but increased argument-based murders of strangers (brutalization effect)	+/–
4.	1999	Sorensen, J., Wrinkle, R., Brewer, V. & Marquart, J.	Texas	1984-1997	Homicide: general and felony murder	X		X	X	Failed to find any deterrent effect between execution and murder rates, or between executions and felony-murder rates	–
5.	1999	Thomson, E.	California	Before and after 1992 execution	Homicide: varied					Findings indicate that there were both short-term deterrent and long-term brutalization effects of the execution on different types of homicide, but the net effects included increases in overall homicides and most disaggregated types of homicides consistent with the brutalization theory	+/–
6.	1999	Albert, C.,	US – 50 States plus District of Columbia	1982-1994	Homicide	X	X	X	X	No evidence that executions deter homicide; coefficients are statistically insignificant	–
7.	1999	Bailey, W. C. & Peterson, R.	US	1976-1991	Homicide: female offenders different varieties	X		X	X	No evidence for deterrence; evidence for possible brutalization effect	–
8.	1998	Stack, S.	California	1946-1955	Homicide			X	X	Publicizing executions was associated with fewer homicides (12.7% in month of execution; total decrease of 99 homicides during 1946-1955)	+
9.	1998	Bailey, W. C.	Oklahoma	1989-1991	Homicide	X		X	X	Support for brutalization hypothesis; possible lagged deterrent effect for level of media coverage for non-felony murders involving strangers	–

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10.	1998	Godfrey, M. & Schiraldi, V.	California	1992-1993 before & after executions; also 1952-1967	Homicide	X				No evidence of deterrence; evidence of brutalization	–
11.	1997	Harries, K. & Cheatwood, D., - same as Cheatwood 1993	1,725 US counties	1988	Violent crime		X	X	X	'virtually' no evidence of deterrent effect; positive correlation between capital punishment and level of violent crime (consistent with brutalization)	–
12.	1995	Stack, S.	US	1977-1984	Homicide			X	X	No deterrent effect for African Americans (already outsiders); deterrent effect for Caucasians	+/–
13.	1994	Bailey, W. C. & Peterson, R.	US	1976-1989	Homicide – various (police killings)	X		X	X	No consistent evidence was found that capital punishment influenced police killings during 1976-1989; no evidence of deterrence effect	–
14.	1994	Cochran, J., Chamlin, M. & Seth, M.	Oklahoma	1989-1991	Homicide – general, stranger & felony murder	X A*				No evidence that Oklahoma's reintroduction of executions produced a statistically significant decrease in the level of criminal homicides during 1989-1991; some evidence of brutalization (increased stranger homicides)	–
15.	1993	Stack, S.	Georgia	1950-1965	Homicide	X			X	No support for deterrence; a publicized execution was found to be associated with an increase of 2.6 homicides, or 6.8%, in the month of the publicized execution. Publicized executions were associated with an increase of 55 homicides during 1950-1965.	–
16.	1992	Cloninger, D.	US States plus DC	1983-1988	Homicide		X		X	Support for deterrent effect	+
17.	1991	Peterson, R. & Bailey, W.	US	1976-1987	Capital homicide	X		X	X	This investigation found no consistent evidence that executions and the television coverage they receive are associated significantly with rates for total, index, or different types of felony murder	–
18.	1991	Bailey, W. C.	US	1950 & 1960	Non-capital felonies		X	X	X	No support for deterrence hypothesis	–
19.	1990	Grogger, J.	California	1960-1963	Homicide	X		X	X	The data provides no support for the notion that executions deter homicides in the short term	–

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20.	1990	Bailey, W. C.	US (as a whole)	1976-1987	Homicide	X		X	X	Homicide rates were not found to be related to either the amount or the type of execution publicity over the period	–
21.	1990	Decker, S. & Kohfeld, C.	Five States in US – Georgia, New York, Texas, California, and North Carolina.	1930-1980	Homicide	X		X	X	Neither the existence of the death penalty, its imposition, nor the level of imposition explains significant amounts of the variation in homicide rates in 50-year period analysed	–
22.	1989	Bailey and Peterson	US	1950-1980 & 1940-1986	Homicide	X		X	X	No evidence that execution publicity influenced the rate of homicide during the 1950-1980 or 1940-1986 period. Some evidence suggests that higher levels of execution are associated with lower murder rates – however, the apparent deterrent effect is very slight and short term	–
23.	1989	Chressanthis, G. A.	US	1965-1985	Homicide	X		X	X	Finds a deterrent effect of capital punishment does exist	+
24.	1988	Cover, J. & Thistle, P.	US	1937-1977	Homicide	X			X	Results provide mixed support for deterrence hypothesis.	+/–
25.	1988	Peterson, R. & Bailey, W.	US	1973-1984	Homicide	X	X	X	X	The analysis produced no indication that the national return to capital punishment since *Furman* has had a systematic downward impact on homicide	–
26.	1987	Stack, S.	US	1950-1980	Homicide	X		X	X	Evidence of deterrence; negative relationship between publicized executions and homicides in short term (a publicized execution story is associated with a drop of 30 homicides in the month of the story); no effect for executions that were not publicized.	+
27.	1987	Bailey, W. C. & Peterson, R.	US	1973-1984	Lethal assaults against police			X	X	Present analysis lends no support to the view that the death penalty provides a more effective deterrent to police homicides than alternative sanctions	–
28.	1987	Decker, S. & Kohfeld, C.	US – Missouri	1933-1980	Homicide	X		X	X	No deterrent effect	–
29.	1987	Adeyemi, A.A.	Nigeria	1967-1985	Homicides and armed robbery	X				There is no support for the efficacy of the death penalty in Nigeria. Celerity was also examined but no valid evidence was found to exist in relation to the effect of delays in executions on the efficacy of the death penalty.	–

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	Year	Author	Data location	Period covered	Type of crime	Time Series	Cross-sectional	Control variables	Regression	Deterrent effect**	CP a deterrent Yes (+)/ No (–)
30.	1986	Layson, S. K.	US	1934-1969	Homicide	X		X	X	Evidence consistently supports deterrence theory in general and specific hypothesis that capital punishment is a deterrent	+
31.	1985	Layson, S.K.	US	1936-1977	Homicide	X		X	X	Evidence of deterrence using Vital Statistics rather than FBI series. Trade-off between executions and murders of 18.5 lives, plus or minus 10	+
32.	1984	Bailey, W. C.	District of Columbia	1890-1970	Murder	X		X	X	No support for deterrence, however suggests immediate effects of executions may increase not decrease murders. However this effect is extremely slight and short term.	–
33.	1984	Bowers, Pierce & McDevitt	New York State	1906-1963	Homicide	X				Brutalization effect (executions associated with increased homicides)	–
34.	1984	Decker, S. & Kohfeld, C.	Illinois	1933-1980	Homicide	X		X	X	No evidence for a deterrent effect for the death penalty on homicides in Illinois	–
35.	1983	Layson, S.	Canada	1927-1977	Homicide	X		X	X	Supports the hypothesis that capital punishment is a deterrent	+
36.	1983	Lempert, R.	Same States as Sellin 1959	1920-1955	Homicide		X	X		Using correlations they find the data provide no reason to believe that executions deter homicide	–
37.	1983	Rahav, G.	From 17 countries	1955-1972	Homicide. Larceny is used as a comparison	X	X	X	X	The analysis shows that the death penalty has a very low, inconsistent effect upon both homicide and murder rates	–
38.	1983	Bailey, W. C.	US – city of Chicago, Illinois	1915-1921	Murder	X		X	X	In line with the brutalization argument, this analysis suggests that the net effect of executions may well have been to increase, not decrease, Chicago first-degree murders and total criminal homicides.	–
39.	1983	Archer, D., Gartner, R. & Beittel, M.	From 14 countries	Varies by country; generally one year before/after abolition, five years before/ after abolition & maximum length periods before/ after abolition	Homicide	X	X	X		The evidence fails to support, and indeed, repeatedly contradicts the proposition that if capital punishment is a more effective deterrent than life imprisonment, its abolition ought to be followed by homicide rate increases.	–

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	Year	Author	Data location	Period covered	Type of crime	Time Series	Cross-sectional	Control variables	Regression	Deterrent effect**	CP a deterrent Yes (+)/ No (–)
40.	1983	McFarland, S.	US	Looks at weeks surrounding 4 executions (one in 1977, two in 1979, and one in 1981)	Homicide	X A*	X			No evidence that capital punishment has either a short-term deterrent or a rebound effect on homicides	–
41.	1983	Forst, B.	US States data	1960-1970	Homicide	X	X	X	X	The findings suggest that on balance the death penalty does not have a perceptible influence on the homicide rate.	–
42.	1982	Bailey, W.C.	50 US States	1961-1971	Lethal assaults against Police		X	X	X	Contrary to the deterrence hypotheses, no support is found for the argument that the provision and use of the death penalty provides an added measure of protection for the police	–
43.	1982	Yunker, J. A	US	1907-1979	Homicide	X		X	X	Indirect test of deterrence hypothesis. Extrapolation of a predicted homicide rate estimated over the capital punishment era into the postcapital punishment era suggests that the mid 1960s surge in crime is unlikely to be caused by socioeconomic factors. Author argues that the moratorium on capital punishment may be an explanation, although there may be other causes.	+/–
44.	1980	Philips, D	London	22 executions 1858-1921	Homicide			X		On average homicides decrease by 35.7% following a publicized execution. The more publicity, the more homicides decrease. Capital punishment appears to have a short-term, but not a long-term, deterrent effect on homicides.	+/–
45.	1980a	Bailey, W. C.	39 US States	1910-1962 (used 28 years selected from this period)	Homicide		X	X	X	Only small negative correlations between executions and homicide rates; socio-demographic indicators and length of imprisonment both better determinants of murder rates than executions	+/–
46.	1980b	Bailey, W. C.	40 US States	1960	Homicide		X	X	X	The analysis consistently fails to provide support for the deterrence argument for the certainty and celerity of executions	–
47.	1979b	Bailey, W. C.	Oregon	1918-1962	Murder			X	X	No evidence to support the deterrence hypothesis – execution rates and homicide rates were found to be largely independent factors	–
48.	1979a	Bailey, W. C.	Ohio	1910-1962	Homicide			X	X	No evidence to support the deterrence hypothesis. A very slight nonsignificant negative association between certainty of execution and offence rates. Sociodemographic factors proved to be better predictors.	–

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49.	1979	Avio, K.	Canada	1926-1960	Murder	X		X	X	A statistically significant independent deterrent effect of capital punishment is not found for Canada over the period 1926-1960 in this study	–
50.	1979	Kleck, G.	US	1947-1973	Homicide	X		X	X	Evidence does not support death penalty exerted any general deterrent effect on the homicide rate in the US during period since WWII. No consistent support for brutalization effect.	–
51.	1979	Knorr, S.	45 US States examined on a national, regional & State level	1940-1960 (regressions performed over sub-period 1950-1960)	Homicide	X	X	X	X	In none of the equations could the death penalty be regarded as an effective deterrent – only the probability of apprehension was found to be significant and even then only on the State level	–
52.	1978	Black, T. and Orsagh, T.	43 US States for 1950 & 47 US States for 1960	1950 and 1960	Homicide		X	X	X	Unable to find a consistent relationship between sanctions and homicide, therefore no deterrent effect	–
53.	1978	Wolpin, K. I.	England and Wales	1929-1968	Homicide	X		X	X	Executing an additional convicted murderer reduces the number of homicides by 4.08 potential victims	+
54.	1978a	Bailey, W. C.	California	1919-1962	Homicide			X	X	No evidence that the certainty of execution provides an effective deterrent to murder to California	–
55.	1978b	Bailey, W. C.	Utah	1910-1962	Homicide			X	X	No evidence that the certainty of execution has provided an effective deterrent to murder in Utah	–
56.	1978c	Bailey, W. C.	North Carolina	1910-1962	Homicide			X	X	No deterrent effect; For the years considered, changes in the certainty of execution and homicide rates were found to be generally unrelated factors	–
57.	1977a	Bailey, W. C.	16 US States	1951 and 1961 - using 5 lag periods for each	Rape			X	X	Certainty of imprisonment is consistently found to have at least twice the deterrent effect of executions for rape. Similarly, severity of imprisonment was found to be at least a 50% better deterrent than the death penalty for rape. Does not conclude that death penalty does not have a deterrent effect on rape but rather that this investigation shows that imprisonment has a relatively better deterrent effect than the death penalty	+/–

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58.	1977b	Bailey, W. C.	US States – some States were excluded from various years due to missing data	1950 & 1960 (1920, 1930 & 1940 census years are also used as a comparison)	Homicide			X	X	The certainty of execution and homicide rates were found to be generally unrelated. Also the significant negative bivariate relationship between the severity of prison sentence and homicide rates found in this and earlier studies is shown to be a statistical artefact resulting from a failure to control for the effects of alternative legal sanctions and sociodemographic factors	–
59.	1977	Cloninger, D. (error corrected in 1987 publication)	48 US States	1960	Homicide		X	X	X	The evidence was found to be consistent with the deterrence hypothesis with respect to execution rates and to a lesser extent to confinement rates	+
60.	1977	Boyes, W. J. & McPheters, L.R.	47 US States	1960	Violent crime – homicide, rape and assault		X	X	X	Overall, the deterrent effect of capital punishment is insignificant	–
61.	1977	Ehrlich, I,	US States – number of States used in each regression varies	1940 and 1950	Murder & related violent crimes		X	X	X	Findings indicate a substantial deterrent effect of punishment on murder and related violent crimes and support the economic and econometric models used in investigations of other crime	+
62.	1977	Passell, P. & Taylor, J.	US	1935-69	Homicide	X		X	X	Finds Ehrlich's model sensitive to functional form and choice of variables. On the basis of Ehrlich's research, it is prudent neither to accept nor reject the hypothesis that capital punishment deters murder.	+/–
63.	1977	Forst, B.	32 US States	Change from 1960-1970	Homicide		X	X	X	Findings do not support the hypothesis that capital punishment deters homicides.	–
64.	1976	Yunker, J. A.	US States	1933-1959 for estimation of E function 1960-1972 for estimation of HR function	Homicide	X		X	X	In 1960-1972 there has been a strong inverse association between executions and homicide rate. One execution will deter 156 murders. 1933-1959 shows a strong positive relation between AE (aggregate executions in past three years) but interpreted this as not causal but association	+
65.	1976a	Bailey, W. C.	US States (42)	1967 and 1968	Murder		X	X	X	Findings are inconsistent with deterrence theory	–

Table 1: Summary of Research Studies on the Deterrent Effect of Capital Punishment

	Year	Author	Data location	Period covered	Type of crime	Time Series	Cross-sectional	Control variables	Regression	Deterrent effect**	CP a deterrent Yes (+)/ No (–)
66.	1976b	Bailey, W. C.	US States	1933-1936 & 1944-1967	Forcible rape	X	X			Contrary to deterrence theory, rape rates were found to be generally higher in death penalty retentionist States for most years compared with abolitionist States, although the differences were slight. A cross-sectional analysis of levels of execution and rape rates revealed a negative association between these two variable for most years, but with some positive association in some years. A longitudinal analysis of changes in the levels of execution and corresponding changes in rape rates within death penalty States showed the correlation to be negative in only about half of the cases.	+/–
67.	1975	Passell, P.	States in US – 41 in 1950 and 44 in 1960	1950 and 1960	Murder and non-negligent manslaughter		X	X	X	No evidence of execution as a deterrent. Conviction rates, average prison sentences, poverty, age, and rural-urban migration + southern states explain a great deal of the State-to-State murder rate variations. But variations in execution rates add no explanatory power	–
68.	1975a	Ehrlich, I.	US	1933-1969	Homicide	X		X	X	Evidence of deterrent effect	+
69.	1975	Bowers, W. & Pierce, G.	US	1933-1969	Homicide	X		X	X	Fails to find evidence supporting deterrent effect; major critique of Ehrlich	–
70.	1969	Samuelson, G.	Delaware – USA – CP abolished in 1958 & reintroduced in Dec 1961	1956-1966	Manslaughter and murder					Annual rate of murder commitments higher before and after than during abolition – CP does not serve as a deterrent to criminal homicide	–
71.	1968	Barber, R.N. & Wilson, P.R.	Queensland, Australia – abolition of CP in 1922	1900-1939	Murder					Murder rate in QLD fell from 28.4 per million population in 1901 to 14.0 per million in 1931. Abolition did not cause increase in murder rate. CP no better deterrent than mandatory life imprisonment for murder in QLD	–
72.	1959	Sellin, T.	US – 5 groups of 3 contiguous States (States are matched and at least one of three had CP)	1940-1955	Homicide		X			Average annual rate of homicide bore no relationship to whether or not death was the maximum penalty for murder	–

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73.	1958	Savitz, L.D.	Philadelphia – four cases sentenced to death 1944, 1946, 1946, 1947	1944, 1946, 1946, 1947	First degree murder – Philadelphia police 'murder books'					No pattern that would indicate deterrence. There was no significant decrease or increase in the murder rate following the imposition of the death penalty on four separate occasions	–
74.	1952	Schuessler, K.	US States Sweden Netherlands	1925-1949 1754-1942 1850-1927	Homicide Murder and attempted murder	X	X			Homicide rate does not drop consistently as the certainty of death penalty increases; geographic correlations between risk of execution and homicide rate is not statistically significant; homicide rate and execution risk as time series move independent of one another	–

** Summaries of findings may contain verbatim text from the cited papers – for simplicity of presentation, quotation marks will not be used to indicate these texts.
A* Autoregressive integrated moving average (ARIMA) analysis

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