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Facts or Ideology: What Determines the Results of Econometric Estimates of the Deterrence Effect of Death Penalty?

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Facts or Ideology: What Determines the Results of Econometric Estimates of the Deterrence Effect of Death Penalty?

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Facts or Ideology: What Determines the Results of Econometric Estimates of the Deterrence Effect of Death Penalty?

A Meta-Analysis

by

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Summary

Provided that the literature on the deterrent effect of capital punishment is overall inconclusive, the fact that individual authors persistently claim to have found solid evidence in one or the other direction raises two questions. Firstly, what are the causes for these different results? Do different data samples, estimation methods or time periods lead to different results or do the outcomes merely reflect prior convictions of the authors? Secondly, to what extent is it possible to derive such diverging results by slightly changing the specification of the test equations without violating scientific standards? After a survey of the over forty reviews of this literature available so far, we perform a meta-analysis of 102 deterrence studies published between 1975 and 2011. The profession of the author turns out to be the only statistically significant explanatory variable: Economists claim significantly more often to have found a significant deterrence effect than members of law or other social science departments. Furthermore, using a panel data set of U.S. states, we show how easy it is to derive contradictory results by employing alternative specifications. Thus, our results reinforce the claim that the empirical evidence presented to date is by far too fragile in order to base political decisions on it.

Keywords: Death Penalty, Deterrence, Econometric Evidence, Ideology.

JEL Classification: K14, K42

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1 Introduction

[1] Ever since I. EHRLICH's (1975) seminal paper on the deterrence of the death penalty, in which he claimed that each execution in the U.S. might prevent up to eight murders, there has been an intense and on-going debate (mainly) in the United States about whether there is such a deterrence effect or not.¹⁾ Some of the scientific contributions in this field have played a role for testimonies before congress committees.²⁾ Despite the modern econometric techniques that are employed, there is, to date, no consensus among the scientists participating in this debate, who are not only economists, but also researchers from law or other social science departments. This paper intends to shed some light on the underlying reasons for the contradictory results, which have thus far remained unclear.

[2] One can distinguish between three generations of literature on the death penalty. The first generation includes contributions that were published between 1975 and 1978, i.e., around the time when the moratorium on capital punishment was lifted in the United States, while the second one comprises papers written after 1982, covering the time after the moratorium.³⁾ These earlier studies typically relied on time series and cross-sectional data, and except for a few studies which looked at Canada or the United Kingdom, most analyses used evidence from U.S. crime statistics.⁴⁾ In recent years, a third generation of papers has been published.⁵⁾ The main difference in comparison to the first two waves is that more recent publications increasingly rely on the use of panel data, mostly for U.S. states, but partly also for U.S. counties.⁶⁾ Nevertheless, despite the vast number of analyses carried out and the similarity of the underlying data, the results are rather mixed and still far from being decisive.

[3] The debate on the deterrence effect of the death penalty is characterized by a rather pronounced divide between the faculties, with (U.S.) economists being much more likely to support the deterrence hypothesis than other social scientists, in particular law professors.⁷⁾ However, contrary to earlier discussions, the econometric methods used by the two groups are now practically identical: As empirical rigour has become increasingly important in all fields of social science, today, many non-economists are able to use the same advanced statistical techniques equally well as economists. Correspondingly, some of the discussions are concerned with methodological problems, in particular with respect to the reliability of the data and the quality of the instruments used in instrumental variables estimations.

1. The paper by I. EHRLICH (1975) itself was written in response to a book by T. SELLIN (1959).

2. See, for example, J.M. SHEPHERD (2004) or J. FAGAN (2005).

3. See for this also S. CAMERON (1994, p. 197f.).

4. 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).

5. For an overview on this literature see, for example, G. KIRCHGÄSSNER (2011) as well as the literature mentioned in Footnote 2 above and discussed in *Section 3* of this paper.

6. 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).

7. On this, see for example, D. HERMANN (2010) as well as G. KIRCHGÄSSNER (2011).

[4] Meanwhile, the question of deterrence has been subject of economic research for more than four decades and has resulted in over 150 academic publications in this field. Despite the large number of econometric analyses, research on the effect of the death penalty on the behaviour of potential offenders remains far from conclusive. Disagreement among scientists matters even more provided that the subject of deterrence is of high policy-relevance. Since research in this area is likely to have an influence on policy decisions, it is important whether the results suggest the existence of a deterrence effect or not. Furthermore, policy-makers often lack rigorous training in econometric analysis and might therefore not be able to assess the quality of empirical studies on the deterrence effect of the death penalty. Based on these considerations, the NATIONAL RESEARCH COUNCIL (NRC) already conducted a study in 1978 that aimed to “provide an objective assessment of the scientific validity of the technical evidence, focusing on both the existence and the magnitude of any crime-reducing effects” (1978, p. vii). The report came to the conclusion that “available studies provide no useful evidence on the deterrent effect of capital punishment” (1978, p. 9). The NRC report further suggested that both more sophisticated econometric methods as well as improved data - namely more detailed and/or disaggregated data - would be required in order to take a final stance on the matter (1978, pp.12ff.). In the decades following the first NRC report, efforts have been undertaken in both of these directions.

[5] However, more than thirty years later, the academic debate on the issue of deterrence is far from being settled. A second NATIONAL RESEARCH COUNCIL that was conducted in 2012 came to the conclusion that “research to date on the effect of capital punishment on homicide is not informative about whether capital punishment decreases, increases, or has no effect on homicide rates. Therefore, the committee recommends that these studies not be used to inform deliberations requiring judgments about the effect of the death penalty on homicide. Consequently, claims that research demonstrates that capital punishment decreases or increases the homicide rate by a specified amount or has no effect on the homicide rate should not influence policy judgments about capital punishment” (2012, p. 12).

[6] Given this situation, one could easily gain the impression that the economics of crime and the question whether death penalty deters murders or not is nothing but a romping place for ideologists. The notion 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). He states that diverging outcomes do not necessarily imply that authors are shirking; rather, they might be the result of selective perceptions: if contradictory results can be derived using appropriate econometric techniques, authors tend to choose those results that are in line with their a priori convictions and are looking for strong arguments in order to support them.

[7] If we accept that the literature is overall inconclusive, the fact that individual authors persistently claim to have found solid evidence in one or the other direction raises two questions. Firstly, what are the causes for these different results? Do different data samples, estimation methods or time periods lead to different results or, following W.S. McMANUS (1985), do the outcomes merely reflect prior convictions of the authors? Secondly, to what extent is it possible to derive such diverging results by slightly changing the specification of the test equations without violating scientific standards, i.e., do both sides have plausible arguments for their

respective specifications? In the following, we first perform a meta-analysis of 102 papers in order to investigate the causes for the different results (*Section 3*). In this meta-analysis, we have attempted to include all previously published papers on the issue of deterrence that employ statistical or econometric procedures in their analysis.⁸⁾ After this, using a data set originally employed by H. DEZHBAKHSH and J.M. SHEPHERD (2006) and afterwards by J.J. DONOHUE and J. WOLFERS (2006), we show how easy it is to create differing results (*Section 4*). We thus contribute to the literature by both investigating the underlying reasons for contradictory results, as well as by illustrating that contradictory results need not reflect dubious academic behaviour. Rather, the sensitivity of the results supports the claim by the NRC (2012) that the empirical evidence on the deterrence effect of the death penalty is by far too fragile in order to base political decisions on it. Before all this, we present a survey of the over forty reviews of the deterrence literature which are available so far (*Section 2*) in order to set forth the central issues in the discussion among different (groups of) authors.

2 A Survey of Previous Reviews

[8] Generally speaking, one can distinguish between two types of literature reviews, i.e., systematic reviews and meta-analyses. The former aims to provide an overview over the existing literature in a certain field, whereas the latter can have two different aims: (i) to come closer to the true value of a parameter of interest by pooling results of previous studies; (ii) to investigate what causes the different results. These two aims might not always go along with each other. In the following, we first consider the many systematic reviews of the deterrence literature, but finally also discuss the few meta-analyses that have been carried out in this field. Unless noted otherwise, the reviews refer to studies that were conducted using U.S. data.

[9] Many reviews of the literature on the deterrence effect of the death penalty tend to have a strong focus on methodological issues. Namely, the question to which extent the use of different estimation methods leads to different results has often been considered. Provided that the issue of deterrence cannot be analysed using an experimental approach, researchers have to test their hypotheses using historical data. It is however not a priori clear what a correct model for testing deterrence would look like; rather researchers need to argue why a particular type of model setup is appropriate in order to test the deterrence hypothesis. J. FAGAN (2006) therefore claims that model uncertainty is intrinsic to such studies.

2.1 Reviews of First and Second Generation Studies

[10] The first and second generation of economic research on the deterrence effect of the death penalty comprise publications that fall into the time period between 1975 and 1978 and after 1982, respectively. These analyses mostly rely on time series data from the U.S. in order to estimate a murder supply function, although some authors also use cross-sectional data. S. CAMERON (1994, pp. 197ff.) as well as L.R. KLEIN et al. (1982, pp. 146ff.) compare numerous earlier studies. Both reviews show the fragility of the results to the inclusion of variables such

8. Preliminary results of this research have been presented at the Thünen Lecture at the 2012 Annual Meeting of the *Verein für Socialpolitik* in Göttingen, Germany. See also G. KIRCHGÄSSNER (2013).

as gun ownership, other crime rates, incarceration terms, or dummies for executing states. In an earlier review, J.P. GIBBS (1977, pp. 304f.) also criticizes that the inclusion of extra-legal correlates of crime (as, for example, the unemployment rate or the proportion of non-whites among the population) is rarely based on a well-defined theoretical foundation regarding the workings of generative and inhibitory classes of extra-legal variables, but rather that the selection of such variables often remains obscure.⁹⁾

[11] S. CAMERON (1994) further depicts to what extent the results depend on the choice of the functional form, i.e., linear vs. multiplicative or log-linearized specifications. Namely, the results by I. EHRLICH (1975) appear to hinge on a logarithmic transformation of the data, an argument first put forward by W.J. BOWERS and G.L. PIERCE (1975, p. 206) as well as P. PASSSELL and J.B. TAYLOR (1977).¹⁰⁾

[12] A more fundamental concern with the econometric analysis following the Ehrlich paradigm is related to the potential for simultaneity bias. According to L.R. KLEIN et al. (1982, pp. 144ff.), many variables in the murder equation are mutually dependent, as, for example, the probabilities of conviction and execution given the probability of arrest can hardly be argued to be exogenous variables. Yet, even when employing a two-stage least squares approach as it is done, for example, by I. EHRLICH (1975), this problem has not been fully resolved. This is because in such models economic factors are assumed to affect criminal behaviour, but not the other way around.¹¹⁾

[13] Furthermore, in a review of first generation studies, A. BARNETT (1981, pp. 364ff.) points out that most of these papers have relied on the assumption that the variance of the error in estimating homicide rates was either the same in all states or inversely proportional to a state's urban population. A. BARNETT (1981) re-estimates these studies and his results suggest that using weighted least squares is more appropriate in order to weigh state-specific data and to achieve homoskedasticity. In addition, one would also want to take into account serial correlation in the stochastic part of the murder supply function by using standard errors that have been adjusted for autocorrelation.¹²⁾

2.2 Reviews of Third Generation Studies

[14] The third generation of deterrence literature uses the natural variation resulting from the fact that not all states restored capital punishment at the same time when the constitutional barrier was lifted in 1976. Most of these papers were written after the year 2000 and rely on state- or county-level data and panel estimation techniques. Besides using highly comparable data, these studies are also based on relatively similar conceptualizations of criminal behav-

9. See for this also P.J. COOK (1977, pp. 183ff.).

10. See for this also J. CHAN and D. OXLEY (2004, p. 7).

11. See L.R. KLEIN et al. (1982, p. 145, p. 151).

12. Ibid.

ious. Yet, these studies come to very different results regarding the existence of a deterrence effect.¹³⁾

[15] In a review of third generation studies, E. COHEN-COLE et al. (2009, p. 337) argue that the different outcomes of these studies are mainly due to different model specifications and the assumptions linked to these. Therefore, a relatively small difference in the covariates included or the econometric method employed can lead to fundamentally different results. G. KIRCHGÄSSNER (2011, pp. 461ff.) also critically reviews deterrence papers written in the last ten years and shows how simple changes in methodology and model specification can produce completely different results, using the same data, notably. Although third generation papers use more sophisticated methodology than earlier research, many problems persist, namely issues related to simultaneity have not been adequately met so far. In contrast to earlier research, however, the functional form does not appear to be a major concern as in the more recent literature; log-linear specifications lead to similar results like linear models.

[16] J.J. DONOHUE and J. WOLFERS (2006) compare panel data studies by L.R. KATZ et al. (2003), H. DEZHBAKHSH and J.M. SHEPHERD (2006), H.N. MOCAN and R.K. GITTINGS (2003), and others. Besides providing a critical review of the evidence, they also use the actual data of these studies in order to re-estimate their equations. The authors find that the results by H. DEZHBAKHSH and J.M. SHEPHERD (2006) are sensitive both to the exclusion of Texas as well as to the definition of the execution variable. Namely, in this paper the execution variable is defined as the number of executions in a given state without controlling for population size. When scaling the execution variable per 100,000 residents, the effect becomes insignificant.¹⁴⁾ In their review of H.N. MOCAN and R.K. GITTINGS (2003), J.J. DONOHUE and J. WOLFERS use lagged values of one year instead of seven when constructing the deterrence variables and the coefficient turns insignificant (2006, p. 816ff.). The authors further show how sensitive the results of H. DEZHBAKHSH and J.M. SHEPHERD (2006) are to the instrumental variable definition. For example J.J. DONOHUE and J. WOLFERS (2006) find that each execution costs more than eighteen lives rather than saving eighteen lives if a minor change is performed to the coding of the instrumental variable measuring partisan influence, i.e., the state level Republican vote share in the most recent presidential election.¹⁵⁾

13. Among these studies are H. DEZHBAKHSH and J.M. SHEPHERD (2006) and H.N. MOCAN and R.K. GITTINGS (2003) who find a deterrent effect, while J.J. DONOHUE and J. WOLFERS (2006), J. FAGAN (2005, 2006), as well as L. KATZ et al. (2003) argue that the evidence is too sensitive with respect to model specification to draw clear conclusions. J.M. SHEPHERD (2005) presents evidence for both, the deterrence as well as the brutalisation hypothesis, and tries to explain the conditions under which either of these hypotheses can be supported.

14. See J.J. DONOHUE and J. WOLFERS (2006, pp. 813ff.). For a similar sensitivity test of the newer deterrence literature, also refer to J. FAGAN (2006, pp. 308-311).

15. Instead of using six variables based on the vote share in each of the six separate presidential elections as it is done by H. DEZHBAKHSH and J.M. SHEPHERD (2006), J.J. DONOHUE and J. WOLFERS merge this information into one partisanship variable (2006, pp. 821-825).

2.3 Heterogeneity and Model Specification

[17] Among other things, model specification reflects what one considers to be an appropriate way of dealing with observed and unobserved heterogeneity. KECKLER (2006) provides an overview over both theoretical models as well as empirical studies in the context of heterogeneity and concludes that if there is a deterrence effect, one would expect this effect to differ between different groups of criminals. In particular, the deterrent effect of the death penalty should be more pronounced for criminals who respond less to other forms of deterrence (such as prolonged prison sentences) because of their particular cost-benefit considerations (2006, pp. 116ff.). Namely this would apply to gangs, serial killers, terrorists and so forth.¹⁶⁾

[18] Also in the context of heterogeneity, P.J. COOK (1980, p. 241; p 252) as well as J. FAGAN (2006, pp. 276ff.) criticize that most deterrence studies do not distinguish between different types of homicides. However, by combining, for example, crimes of jealousy with more or less rationally planned killings, one implicitly assumes that all forms of homicide are equally deterrable.¹⁷⁾ This is particularly questionable as the law explicitly distinguishes between different types of homicide and, therefore, in the presence of felony murder rules, the probability for a capital punishment ruling will vary among different types of murders.

[19] Besides focussing on heterogeneity with respect to groups of criminals and types of crimes, several review studies also focus on geographic, and in particular state-specific, heterogeneity. J.J. DONOHUE and J. WOLFERS (2006, pp. 826f.) show how sensitive the results are with respect to the exclusion of very active states, namely if Texas and California are dropped from the data, the estimated effects in terms of lives saved or lost range from -42 to +34 and -29 to +30, respectively. Again, this can be taken as evidence for the fragility of the results.¹⁸⁾

[20] Furthermore, according to G. KIRCHGÄSSNER (2011, pp. 467f.), the failure to adequately take into account heterogeneity introduces concerns related to simultaneity. Provided that a potential criminal can simultaneously choose among different criminal acts, the most appropriate way of dealing with this simultaneity would be in estimating a system of equations rather than only one equation for murder rates. However, a large part of the deterrence literature continues to rely on one murder supply equation only. Another simultaneity problem for which the literature so far has not found an appropriate response is linked to state-specific heterogeneity. Namely states with lower murder rates will have less need for the use of the death penalty. At the same time, their murder rate might simply be lower due to the fact that by performing fewer executions, the state has experienced less brutalisation.

16. On this, see also J.J. DONOHUE (2009b, p. 796ff.). In this context, it is also worth noting that around the year 2000, violent death rates among street gang members in the U.S. were close to seven per cent, whereas in the same time period the number of executions constituted three per cent of death row. In other words, for particular types of criminals, the risk of violent death appears to be higher than the risk of execution. See for this S.D. LEVITT and D.J. MILES (2006, pp. 156f.).

17. A notable exception in this regard is J. SHEPHERD (2004b).

18. This particular estimation was performed with the data previously used by H. DEZHBAKSH, P. RUBIN and J.M. SHEPHERD. Similar results have been provided by J. FAGAN (2005), as well as R. WEISBERG (2005, p. 159).

2.4 Importance of Particular Assumptions

[21] Another relevant concern of previous review articles is related to the assumptions of economic analyses of the deterrence effect of the death penalty. Namely, economic models of crime assume that criminals are rationally behaving individuals and that the emotionality of murder does not constitute a barrier to deterrence.¹⁹⁾ Further, the literature assumes that the subjective risk perception of a potential criminal individual is equivalent to the objective risk of apprehension, conviction and execution.²⁰⁾ Behavioural economic alternatives to the classical economics of crime approach have been reviewed by N. GAROUPA. According to the author, relaxing the rationality assumption would allow for an enrichment of modelling behaviour, but comes at the cost of losing tractability (2003, p. 12). Another critique has to do with the assumed direction of causality, where most analyses assume that an increase in the risk of execution causes a change in the murder rate, although it might just as well be the other way around.²¹⁾

[22] Implicit and explicit assumptions regarding time- and state-specific heterogeneity are also mentioned as a concern in several reviews. For example, when following a time series approach in the style of I. EHRLICH (1975), one assumes that the way in which the covariates influence murder rates does not change over time. However, it is difficult to justify why, for example, the impact of the share of unemployed or non-white members of the population should remain unaffected by the pronounced changes in the social and civil rights legislation that took place between the thirties and the sixties of the last century.²²⁾

[23] According to CH.F. MANSKI and J.V. PEPPER (2011), conclusive results on the deterrent effect of the death penalty can only be found if one is willing to make rather strong assumptions. The authors start with a set of weakest restrictions and find very ambiguous results. The stronger the assumptions they impose, the smaller the degree of ambiguity. However, since the gain in conclusiveness hinges on the introduction of strong assumptions and therefore the results are very sensitive, it is questionable how much knowledge one gains from this. Similar to other reviews, CH.F. MANSKI and J.V. PEPPER (2011) provide evidence for diametrical results, depending on the assumptions they impose.

2.5 Prior Beliefs of the Researcher

[24] Using Bayesian statistical methods, W.S. MCMANUS (1985) shows how prior beliefs of the researcher and selective perceptions can influence the results in deterrence studies. Given that the deterrence question cannot be answered in an experimental setting, the researcher has to decide among many different specifications which ones he is going to select

19. For critiques see J. CHAN and D. OXLEY (2004, p. 5) and D. GLASER (1977, pp. 254ff.). On the rationality assumption in the economics of crime literature see also J. PALMER (1977, pp. 4ff.), as well as K.D. OPP (1989).

20. See for this J. FAGAN (2006, pp. 292ff.) or J.P. GIBBS (1977, pp. 281ff.). The fact that those states without death penalty have – on average – lower homicide rates supports the existence of such reversed causality.

21. See for this J. CHAN and D. OXLEY (2004, p. 5) or P.J. COOK (1980, pp. 255ff.).

22. See for this D. GLASER (1977, pp. 248ff.) or A. BARNETT (1978, pp. 294ff.).

for publication. In a situation where contradictory results can be found, researchers might tend to pick the specification that leads to a result close to their prior hypothesis. In other words, researchers could be tempted to try and find convincing support for the position that is the most aligned with their prior beliefs. According to W.S. McMANUS (1985, p. 425) this issue cannot be resolved unless one is willing to give up the single-equation framework used in most deterrence studies.²³⁾

[25] In the case of the death penalty, the results appear to be not only influenced by ideology and prior beliefs, but also by the scientific discipline of the researcher. Economists, in particular, are much more likely to find evidence for a deterrence effect than other social scientists, notably, using the same data sets as the researchers from disciplines such as law or sociology.²⁴⁾ G. KIRCHGÄSSNER (2011, p. 469) explains this with the fact that “economists believe in incentives more than other social scientists do”. Similarly, S. CAMERON (1988, p. 308) asks “why are economists so keen to endorse deterrence when they could reasonably suppose that it doesn't work? Prior social conditioning could be responsible as economists grow up in a particular culture and are no more immune to its myths than anyone else”.

2.6 Data Quality

[26] Most systematic reviews dedicate a section to data quality issues. Early deterrence research heavily relied on the use of national time series data from the FBI, based on the Uniform Crime Reporting Scheme (UCR). The use of this data is insofar problematic as the UCR scheme was voluntary in its early years and many agencies did not participate. After 1960, the number of agencies that complied with the UCR and reported arrests and convictions increased drastically.²⁵⁾ Homicide figures were adjusted ex post by the FBI on the basis of current data, although several authors have raised doubts regarding the validity of these estimates as, for example, W.J. BOWERS and G.L. PIERCE (1975, p. 190). Starting with S.K. LAYSON (1985), the Vital Statistics, which can be considered to be a more appropriate measure of homicide, have been used as a source of homicide data.²⁶⁾

[27] D. NAGIN (1978, pp. 99-110) reviews 23 studies out of which most of them rely on the use of the National Prisoner Statistics (NPS) or the Uniform Crime Records (UCR). Although, at first sight, the evidence might be interpreted as supporting the deterrence hypothesis, D. NAGIN (1978, pp. 111ff.) demonstrates the inadequacy of the evidence that is due to substantial flaws in the data. D. NAGIN (1978, p. 97, p. 113) shows that measurement error (intended or unintended distortions in crime data either across jurisdictions or across time) can generate an inverse association between published crime rates and any sanction variable that has published crime rates in its denominator. For example, states where police departments record fewer crimes (than actually happened) will then tend to have lower crime rates

23. See for this also G. KIRCHGÄSSNER (2011, p. 451).

24. See for this also D. HERMANN (2010, p. 798).

25. See for this J. CHAN and D. OXLEY (2004, pp. 7f.)

26. See for this J.A. FOX and M.L. RADELET, 1990, pp. 35f.).

and higher measures of sanction rates - which could create the illusion of deterrence.²⁷⁾ Since many deterrence studies use the same denominator in the execution rate as the numerator in the conviction rate, the potential bias resulting from measurement error ought to be taken very seriously.²⁸⁾

[28] Furthermore, D. NAGIN (1978, p. 98, p. 129) points out that in jurisdictions with tougher sanctions (i.e., in states where conviction probability is higher and/or the time served is longer), the inverse association between crime and sanctions is likely to reflect an incapacitation effect rather than a deterrence effect. This is because during the time while a criminal individual is incarcerated, he cannot commit another crime. So naturally, states that keep a larger share of their (potentially criminal) population locked up in prisons will reduce their crime rate. Confounding of incapacitation and deterrence effects will then bias the results.²⁹⁾

[29] According to G. KIRCHGÄSSNER (2011, p. 466) data issues persist even in more recent, methodologically more sophisticated studies. In particular, the results are still extremely sensitive to the definition of the deterrence measure and the dependent variable. Furthermore, data on important states such as Florida is still far from being complete which, according to J. FAGAN (2005) and R. WEISBERG (2005, pp. 159f.), is likely to bias the results.

[30] In addition, several reviews mention issues that are linked to the use of national aggregated data, namely that causal inference is rather challenging, i.e., how an execution in one state is meant to deter murder in another state.³⁰⁾ An alternative is represented by performing time series analyses on individual states, although it remains unclear to what extent the results can be applied to other states.³¹⁾ Using cross-sectional data on all US states alleviates this problem to some extent, but, as J. CHAN and D. OXLEY (2004, p. 8) as well as P.J. COOK (1980, pp. 256f.) state, it introduces difficulties related to unobserved heterogeneity. This problem is alleviated in newer studies that mainly rely on panel data and state-specific fixed effects, often combined with an instrumental variable approach as, for example, in J.M. SHEPHERD (2004a, pp. 6f.). However, as J.J. DONOHUE and J. WOLFERS (2006, pp. 804f.) argue, these studies remain very fragile when it comes to changes in the model specification and/or the construction of the instrumental variables.

2.7 Sample Period

[31] S. CAMERON (1994, p. 204) presents numerous studies that show that the finding of a deterrence effect crucially depends on the exclusion of post-1962 data in the US and exclu-

27. See for this K.L. AVIO (1988), but also S. CAMERON (1988, p. 308ff.), J. CHAN and D. OXLEY (2004, pp. 5ff.), P.J. COOK (1977, pp. 187f.), as well as J.A. FOX and M.L. RADELET (1990, pp. 38f.).

28. See also S. CAMERON (1994, p. 204).

29. See also J.P. GIBBS (1977, pp. 293f.) or D. NAGIN (1978, p. 98).

30. See for this B. YANG (1987, pp. 46f.).

31. See for this D.C. BALDUS and J.W. COLE (1985, pp. 175-f.), J.A. FOX and M.L. RADELET (1990, pp. 39f.) or D. GLASER (1977, pp. 248f.)

sion of the years 1956-1968 in the UK.³² The combination of execution-free data with earlier data has been criticized in several review studies, for example by D.C. BALDUS and J.W. COLE (1975, pp. 184f.), S. CAMERON (1994, p. 209) as well as by D. GLASER (1977, pp. 243f.).³³ S. CAMERON (1994) argues that since there is evidence for a structural break in the data, pooling the two series would not be appropriate.

[32] In a review of the new deterrence literature using panel data, J. FAGAN (2006, pp. 284ff.) shows that the results are very sensitive to extensions in the observation window. According to the author this is mainly due to the fact that while the number of executions sharply decreased from 1999 to 2004, the murder rate remained relatively stable.³⁴

2.8 Meta-Analyses

[33] As aforementioned, it is not a priori clear what a correct model for testing deterrence would look like, but rather it is up to researchers to explain why a particular type of model setup is appropriate in order to test the deterrence hypothesis. Needless to say, this entails the risk of subjective beliefs and ideology influencing the choice of data or the estimation technique. Combining the results of different studies into one meta-study therefore represents one way to reach more objective results. B. YANG and D. LESTER (2008) have carried out such a meta-analysis covering 104 peer-reviewed journal articles that were published following I. EHRLICH (1975). Yet, only 95 studies have been considered by B. YANG and D. LESTER (2008, pp. 457ff.) to be based on adequate data. Out of these and according to their evaluation, 60 studies provide evidence in favour of the deterrence theory and 35 studies provide evidence against it. However, the results appear to hinge on the type of study carried out: Time series and panel data studies on average find a deterrence effect. The results from cross-sectional studies, studies of single executions and those taking into account the publicity of executions, however, were inconclusive, i.e., the average effect was not statistically different from zero.

[34] In the above-mentioned literature review of third generation studies, E. COHEN-COLE et al. (2009, p. 337) argue that relatively small methodological differences can lead to fundamentally different results. A priori, one can find theoretical support for both assumptions and both statistical methods - which makes it very challenging to compare such studies, as they are both “right” in some way. In order to deal with this model uncertainty, E. COHEN-COLE et al. (2009, pp. 338ff.) follow a model averaging approach where model-specific estimates are averaged using posterior probabilities.³⁵ The main result from the analysis by COHEN-COLE et

32. On this, see also J.A. FOX and M.L. RADELET (1990, pp. 36-38), as well as L.R. KLEIN et al. (1982, pp. 146-147).

33. The sensitivity of I. EHRLICH’s results with respect to the sample period or the choice of the functional form has first been put forward by W.J. BOWERS and G.L. PIERCE (1975), as well as P. PASSELL and J.B. TAYLOR (1977).

34. G. KIRCHGÄSSNER (2011) also discusses the fragility of the results with respect to the choice of the sample period.

35. A different approach has been put forward by E. LEAMER (1983). On this, see also E. COHEN-COLE et al. (2009, p. 339).

al. (2009, p. 364) is that there is a large, but imprecise deterrence effect and that the strong results that have been put forward by, for example, DEZHBAKHSH et al. (2004) are mainly due to particular model choices.

[35] In a meta-analysis of eighteen deterrence studies of the third generation, G. KIRCHGÄSSNER (2011, pp. 466ff.) comes to a similar conclusion. Provided that the results of the included studies differ widely, it is challenging to draw firm conclusions. Overall, the mean of the reported t-statistics is negative, which provides some support for the deterrence hypothesis.³⁶⁾ However, the evidence remains inconclusive, particularly because of the fragility of the results to specification changes.

[36] D. DÖLLING et al. (2009, pp 209f.) perform a meta-analysis of the deterrence effects of different types of punishment. In total, 391 studies are considered in this meta-analysis out of which fifty-two studies are concerned with the death penalty. The authors find a significant deterrence effect of punishment in minor crime cases, particularly in studies relying on experimental data. However, the meta-analysis does not indicate that the death penalty deters murder (2009, pp. 219-221). Again, there appears to be a pronounced influence of methodological and statistical choices on the results.³⁷⁾

[37] A comparable set of studies has been analyzed by D. HERMANN (2010) who conducts a meta-analysis of eighty-two papers on crime and punishment out of which fifty-two study the effect of the death penalty. Among the death penalty studies, he finds a pronounced divide between the economic and other social science faculties with respect to the result of the deterrence studies. Namely, forty-nine per cent of publications by economists in economic journals find a deterrence effect that is statistically significant. However, if a contribution by another social scientist, for example, a criminologist, sociologist, or a member of a law faculty, is issued in a publication outlet in the respective field, only twenty-eight percent of the publications state a statistically significant deterrence effect of the death penalty. He also shows that there is a correlation between the publication medium and the result of a study, as papers by economists who claim to prove the existence of a deterrence effect are mainly published in economic journals. He does, however, not employ formal statistical tests. It remains, therefore, unclear whether this latter effect simply reflects that economists mainly publish in economic journals or whether there is an independent influence of the editors of economic journals such as that they prefer papers that are in line with the existence of a deterrence effect.

[38] So far, two of the meta-analyses available, D. HERMANN (2010) as well as G. KIRCHGÄSSNER (2011), provide some evidence that the profession of the authors might have an effect on the presented results, but both papers do not employ formal statistical tests in order to assess this hypothesis. In addition, the results of B. YANG and D. LESTER (2008) suggest that time series analyses are more likely to produce a significant deterrence effect than

36. On average, the reported t-statistics are -0.78 with a standard deviation of 5.710 (See G. KIRCHGÄSSNER (2011, p. 466).)

37. Preliminary results of this study can be found in J. ANTONY and H. ENTORF (2003), D. DÖLLING et al. (2006), D. DÖLLING et al. (2007), D. DÖLLING et al. (2009), as well as in T. RUPP (2008).

studies with cross-sectional data. However, this finding has not been statistically tested, either. Thus, there is room for another meta-analysis investigating these hypotheses more seriously. Moreover, because more studies are available, there exists today a larger data set than the ones used in these studies.

3 A New Meta-Analysis

[39] We have tried to include in this meta-analysis all papers with original analyses, starting with the paper by I. EHRLICH (1975) and up to 2011. Altogether, we have found 102 papers employing statistical procedures, out of which 87 come to definite conclusions. In 34 papers, results in favour of a significant deterrence effect are presented which might justify the imposition of the death penalty. 53 papers come to the conclusion that if there is any evidence for a deterrent effect at all, it is so precarious that the imposition of the death penalty cannot be justified by the results of empirical research. The remaining 15 papers are inconclusive, i.e., the authors did not want to derive any conclusion or policy-recommendation from their analysis.³⁸⁾ The literature review presented above as well as the results of the previous meta-analysis by D. HERMANN (2010) suggest that a major cause for contradictory results might be whether the authors of the studies are economists or not. However, other factors might also play a role, such as the kind of data, as, for example, B. YANG and D. LESTER (2008) state, but also different methods or estimation procedures. Moreover, economic conditions might also have an effect because the models attempt to explain the number of homicides in a certain area and year. Finally, if not only economists who are participating in this debate but also editors of economic journals have stronger a priori beliefs in favour of the deterrent effect of death penalty than, for example, editors of sociological or law journals, it might be easier to publish results supporting a deterrent effect in an economic than, for example, in a law journal. If this is the case, we face reverse causality, but this should also become obvious in the estimation equation of a meta-analysis.

[40] We start with the following equation:

$$(3) \quad \text{DET} = f(\text{AECON}, \text{ANECON}, \text{ECONJ}, \text{NECONJ}, \text{TS}, \text{CS}, \text{OLS}, \text{INST}, \text{WLS}, \text{US}, \text{YEAR}, \text{UER}, \text{Y})$$

with:

| | |
|--------|---|
| DET | The study claims that there is a significant deterrence effect. |
| AECON | The (one) author is an economist. |
| ANECON | The (one) author is not an economist. |
| ECONJ | The paper has been published in a (scientific) economic journal. |
| NECONJ | The paper has been published in a scientific journal, but not in an economic one. |
| TS | Time series data are used. |
| CS | Cross-section data are used. |

38. The classification of the corresponding papers is given in *Table A1* of the Appendix.

| | |
|------|--|
| OLS | The equation is estimated using OLS. |
| INST | More advanced estimation methods (IV estimators, GMM) are employed. |
| WLS | The observations are weighted. |
| US | The data are from the United States. |
| YEAR | Publication year. |
| UER | The unemployment rate is included in the estimation equation. |
| Y | The growth rate of real income is included in the estimation equation. |

Because the dependent variable is a binary one, we employ a probit estimator.³⁹⁾ Except for YEAR, all explanatory variables are also binary variables. The standard errors are clustered according to the (groups) of authors; the 87 observations lead to 51 clusters.⁴⁰⁾

[41] In the empirical analysis, the dependent variable DET takes on the value ‘1’ if a paper finds a deterrence effect, and ‘0’ if it does not. The undecided papers will be denoted as missing. We include two dummy variables for time series (TS) and cross-section (CS) data; for panel data both take on the value ‘1’. We also include two variables for the publication medium, for Economic (ECONJ) and for other (NECON) scientific journals, be they, for example, from Sociology or Law. The default category is newspaper articles. We use three variables in order to capture the methods the authors have employed: OLS for ordinary least squares, WLS for weighted estimation methods, and INST for instrumental variable estimators and other advanced estimation methods such as GMM or, for example, Poisson regression. The reference category in this case includes studies employing simple comparisons. Economic development is represented by the development of real income (Y) and unemployment (U). The publication year (YEAR) is included in order to test whether there is a trend in the results, if, for example, the death penalty has gained or lost acceptance in the scientific community over time. The variable for the United States might indicate that there are differences between the United States on the one hand and the two other countries for which studies exist, i.e., the United Kingdom and Canada.

[42] In a first attempt, we include the variable for the author being an economist as well as the one for the author not being an economist in the equation, because there are also papers written jointly by economists and non-economists. The results are given in *Table 1* (Model 1).⁴¹⁾ None of the explanatory variables turns out to be significant. This also holds for the two variables describing the professions of the authors. There are, however, only very few papers with economists and non-economists as co-authors. Thus, these two variables are very highly negatively correlated. Taken together, the coefficients of the two author variables are highly significant: the χ^2 -value of the Wald-test is 15.34, which is significant at a level below 0.5 per cent. Similar tests for the other groups of variables failed to provide evidence for any signifi-

39. We also used a logit estimator and an ordered probit one including all 102 papers where we classified the dependent variable as ‘+1’ if the authors state a deterrent effect, ‘-1’ if they deny it and ‘0’ if they remain undecided. The differences between the results of these estimation methods are negligible.

40. If we include all 102 papers, we get 64 clusters.

41. Preliminary results of this model are presented in G. KIRCHGÄSSNER (2013).

**Table 1: Meta-Analysis of the Deterrence Results,
87 observations**

| Model | (1) | | (2) | | (3) | |
|---------------------------------------|--------------------------|------------------------|--------------------------|------------------------|--------------------------|------------------------|
| | parameters (z-values) | χ^2 (p-values) | parameters (z-values) | χ^2 (p-values) | parameters (z-values) | χ^2 (p-values) |
| Constant term | -27.873 (-0.65) | | -26.907 (-0.65) | | -35.409 (-0.88) | |
| Author is an economist | 1.292 (1.27) | 15.34*** (0.001) | 2.086*** (3.38) | | | |
| Author is not an economist | -1.132 (-1.13) | | | | -2.102*** (-3.51) | |
| Economic journal | 0.442 (0.60) | 0.37 (0.829) | 0.658 (0.95) | 1.10 (0.578) | 0.481 (0.63) | 0.40 (0.818) |
| Non-Economic Journal | 0.322 (0.51) | | 0.251 (0.41) | | 0.319 (0.48) | |
| Time series data | -0.126 (-0.22) | 3.89 (0.143) | -0.064 (-0.11) | 2.48 (0.289) | -0.281 (-0.45) | 3.90 (0.142) |
| Cross-section data | -0.881(*) (-1.94) | | -0.752 (1.52) | | -0.824 (-1.97) | |
| OLS | -0.122 (-0.22) | | -0.195 (-0.36) | | -0.066 (-0.13) | |
| Advanced estimation methods | -0.468 (-1.12) | 2.09 (0.553) | -0.456 (-1.09) | 1.81 (0.614) | -0.433 (-1.06) | 1.83 (0.608) |
| Weighted regressions | 0.434 (1.05) | | 0.364 (0.90) | | 0.395 (0.92) | |
| Data from the US | 1.072 (1.64) | | 0.909 (1.35) | | 1.008 (1.57) | |
| Publication year | 0.014 (0.63) | | 0.013 (0.60) | | 0.018 (0.88) | |
| Unemployment rate included | 0.105 (0.27) | 0.97 (0.615) | 0.054 (0.15) | 1.03 (0.597) | 0.153 (0.42) | 0.62 (0.733) |
| Growth rate of real income included | -0.476 (-0.98) | | -0.487 (-1.02) | | -0.344 (-0.75) | |
| Overall Wald test | | 8.68 (0.652) | | 8.22 (0.694) | | 9.00 (0.622) |
| Pseudo R ² | 0.378 | | 0.363 | | 0.360 | |
| Standard error of the regression | 0.405 | | 0.407 | | 0.404 | |
| Log of the pseudo-likelihood function | -36.216 | | -37.067 | | -37.260 | |
| Hannan-Quinn criterion | 1.314 | | 1.299 | | 1.304 | |

‘***’, ‘**’, ‘*’ or ‘(*)’ indicates that the corresponding null hypothesis can be rejected at the 0.1, 1, 5, or 10 per cent significance level, respectively.

**Table 1a: Meta-Analysis of the Deterrence Results,
87 observations**

| Model | (1a) parameters (z-values) | χ^2 (p-values) | (2a) parameters (z-values) | (3a) parameters (z-values) |
|---------------------------------------|----------------------------------|------------------------|----------------------------------|----------------------------------|
| Constant term | -0.213 (-0.23) | | -1.221** (-2.65) | 0.577* (2.24) |
| Author is an economist | 0.790 (0.89) | 12.16** (0.002) | 1.718*** (3.30) | |
| Author is not an economist | -1.007 (-1.26) | | | -1.727*** (-3.57) |
| Pseudo R ² | 0.299 | | 0.284 | 0.291 |
| Standard error of the regression | 0.394 | | 0.397 | 0.393 |
| Log of the pseudo-likelihood function | -40.807 | | -41.684 | -41.285 |
| Hannan-Quinn criterion | 1.041 | | 1.027 | 1.018 |

‘***’, ‘**’, ‘*’ or ‘(*)’ indicates that the corresponding null hypothesis can be rejected at the 0.1, 1, 5, or 10 per cent significance level, respectively.

cant impact, neither for the publication medium, nor the kind of data, the estimation method or the inclusion of economic variables. The use of cross-sectional data appears to be associated with a lower probability to find a deterrence effect, but this effect is only significant at the ten per cent level. The publication year did not have a significant impact, either. The same holds for the comprehensive test whether all these variables together have a significant impact: the p-value of the χ^2 -statistic is 0.652.

[43] To take account of the high multicollinearity, we have re-estimated the equation by only including the variable for an economist (Model 2) or a non-economist (Model 3) as author. The results are also given in *Table 1*. The z-values of the two author variables indicate that they have a highly significant impact in their respective equations. At the same time, the value of the Hannan-Quinn information criterion improves. On the other hand, the impact of all other variables remains insignificant, irrespective of whether we consider the single variables separately, the groups of variables or all variables together.

[44] Besides the individual coefficients, the overall predictive quality of a model is also of interest, i.e., we would want to know whether our model is able to correctly classify the papers into the categories “deterrence” (DET=1) and “no deterrence” (DET=0). The results for all models are given in *Table 2*. If we assume that a deterrent effect is stated whenever the probability estimate is equal to or higher than 0.5, and that it is denied if this probability is below 0.5, then, in Model 1 and 3, 28 out of 34 papers that affirm a deterrent effect are correctly predicted and 27 out of 34 in Model 2. With respect to the 53 papers in our meta-

analysis that deny the presence of a deterrent effect, our prediction is correct for 43 papers in Models 1 and 2 and for 44 papers in Model 3. Thus, in all three models over 80 per cent of all papers are correctly classified.

| Table 2: Predictive Quality of the Meta-Analyses | | | |
|--|--------------------------------------|---------------------------------|--|
| Model | Number of papers correctly predicted | | Percentage of papers correctly predicted |
| | Significant deterrence effect | No significant deterrent effect | |
| 1 | 28 / 34 | 43 / 53 | 81.6 % |
| 1a | 28 / 34 | 42 / 53 | 80.5 % |
| 2 | 27 / 34 | 43 / 53 | 80.5 % |
| 2a | 29 / 34 | 40 / 53 | 79.3 % |
| 3 | 28 / 34 | 44 / 53 | 82.8 % |
| 3a | 28 / 34 | 42 / 53 | 80.5 % |
| 4 | 23 / 34 | 46 / 53 | 79.3 % |
| 5 | 18 / 34 | 55 / 68 | 71.6 % |
| 5a | 29 / 34 | 44 / 68 | 71.6 % |
| 6 | 19 / 30 | 38 / 45 | 76.0 % |
| 6a | 23 / 30 | 29 / 45 | 69.3 % |
| 7 | 23 / 28 | 32 / 38 | 83.3 % |
| 7a | 24 / 28 | 29 / 38 | 80.3 % |
| Models 5a, 6a, and 7a refer to the restricted specification, i.e., with an economist as author as the only explanatory variable. In Model 7 we employ our own classification on the sample that has been used by B. YANG and D. LESTER (2008). | | | |

[45] For all three equations, the overall Wald test shows that all explanatory variables together - except those for the profession of the authors - do not have a significant impact. Therefore, we can reduce these equations by only including the latter variables. This leads to the results presented in *Table 1a*. The pseudo R^2 s are now somewhat lower, but the standard errors of the regression have been reduced as well and the Hannan-Quinn information criterion has considerably improved. In Models 1a and 3a, 28 papers that affirm a deterrent effect and 42 papers that deny it are correctly predicted. Model 2a correctly classifies 29 papers that support the existence of a deterrence effect and 40 papers that deny it. This leads to a slightly lower share of correct classifications of 80.5 per cent and 79.3 per cent, respectively. The estimated values of the parameters of interest are, in absolute terms as well as with respect to their statistical significance, quite similar. Thus, this reduced model works astonishingly well: the professional orientation of the authors seems to be the only relevant factor for the basic message of these papers.

Table 3: Robustness Tests

| Model | (4) | | (5) | | (6) | |
|---------------------------------------|--------------------------------|------------------------|--------------------------|------------------------|--------------------------|------------------------|
| | parameters (z-values) | χ^2 (p-values) | parameters (z-values) | χ^2 (p-values) | parameters (z-values) | χ^2 (p-values) |
| Constant term | -67.041 (-1.99) | | -20.997 (-0.60) | | -45.995 (-0.93) | |
| Author is an economist | | | 1.705** (3.26) | | 1.030* (2.06) | |
| Economic journal | 1.413* (2.20) | 14.69*** (0.001) | 0.412 (0.81) | 1.13 (0.569) | 0.823 (1.18) | 1.85 (0.397) |
| Non-Economic Journal | -0.081 (-0.13) | | 0.479 (1.04) | | 0.491 (0.65) | |
| Time series data | -0.275 (-0.55) | 0.90 (0.639) | -0.003 (-0.01) | 1.03 (0.598) | -0.590 (-0.71) | 0.78 (0.676) |
| Cross-section data | -0.417 (-0.94) | | -0.402 (0.91) | | -0.028 (-0.04) | |
| OLS | -0.205 (-0.40) | | -0.067 (-0.16) | | 0.226 (0.46) | |
| Advanced estimation methods | -0.308 (-0.85) | 1.28 (0.733) | -0.187 (-0.52) | 0.55 0.907 | -0.005 (-0.01) | 0.84 0.839 |
| Weighted regressions | 0.286 (0.77) | | 0.224 (0.56) | | -0.343 (-0.78) | |
| Data from the US | 0.414 (0.59) | | 0.447 (0.91) | | -1.489* (-2.27) | |
| Publication year | 0.033 ^(*) (1.95) | | 0.010 (0.54) | | 0.024 (0.93) | |
| Unemployment rate included | 0.194 (0.57) | 0.33 (0.850) | -0.152 (-0.44) | 0.87 (0.649) | -0.452 (-1.11) | 4.16 (0.125) |
| Growth rate of real income included | -0.068 (-0.15) | | -0.262 (-0.61) | | -0.576 (-1.23) | |
| Overall Wald test | | 25.00*** (0.009) | | 5.37 (0.912) | | 12.84 (0.304) |
| Pseudo R ² | 0.236 | | 0.230 | | 0.235 | |
| Standard error of the regression | 0.445 | | 0.439 | | 0.425 | |
| Log of the pseudo-likelihood function | -44.505 | | -49.996 | | -38.627 | |
| Hannan-Quinn criterion | 1.436 | | 1.371 | | 1.379 | |
| Number of observations | 87 | | 102 | | 75 | |

‘***’, ‘**’, ‘*’ or ‘(*)’ indicates that the corresponding null hypothesis can be rejected at the 0.1, 1, 5, or 10 per cent significance level, respectively.

[46] As the results in *Table 3*, Model 4 show, we get a significant result for the publication medium if we exclude the profession of the authors from the test equation. If we consider the single parameters, the estimated coefficient for the economic journals is significant at the 5 per cent level. This result holds despite the fact that the two variables for the publication media are highly correlated; the correlation coefficient is -0.808. The Wald test for the combined effect of both variables is, with $\chi^2 = 14.69$ and 2 degrees of freedom, significant even at the 0.001 level. This effect is, however, not robust to the inclusion of the profession of the authors, which is not surprising because the canonical correlation between the two groups of variables, (ECONA, NECANA) and (ECONJ and NECONJ) is -0.772. This reflects the trivial fact that economists publish mainly in economic journals and other social scientists mainly in other journals; once we include the profession variable, this spurious result vanishes.

[47] The distinction between papers that deny a deterrence effect and those that do not draw a conclusion might be somewhat artificial because authors who argue against this effect often do so because the evidence is too fragile to draw any serious conclusions. Therefore, in an additional equation we re-specify our dependent variable, asking only whether the authors claim that a significant deterrence effect exists or not. Thus, we get 34 papers claiming and 68 papers denying a significant deterrence effect. Results from estimating the full model but excluding the variable for authors who are not economists are given in *Table 3*, Model 5. We get nearly the same results as in Model 2 which has the same specification but uses a restricted data set: only the variable for the profession of the author is significant. If we re-estimate the model excluding all insignificant variables, we get the result that 29 of the 34 papers which claim a deterrence effect are correctly predicted, but only 44 of the 68 papers that do not claim it.⁴²⁾ Thus, now there are only 71.6 per cent correctly classified.

[48] Our sample differs somewhat from the earlier sample of B. YANG and D. LESTER (2008). On the one hand, we have introduced new studies which were not yet available when these authors performed their analysis. On the other hand, we have excluded some of the studies that they have taken into account because these studies, mainly older ones, did not employ econometric methods. With respect to the 75 studies included in both samples, there are also 6 papers where we differ with respect to the classification. This holds, for example, for the two papers by J.M. SHEPHERD (2005) and P.R. ZIMMERMANN (2006). Both authors believe in the existence of a deterrence effect, but not under all circumstances. J.M. SHEPHERD (2005) finds a deterrence effect if there are ‘enough’ executions in a state: she finds a threshold of approximately 9 executions during her sample period from 1977 to 1996. According to P.R. ZIMMERMANN (2006) the execution method matters: only electrocution is effective in this respect. Because both authors in principle believe in the existence of a deterrence effect, we have classified them accordingly, whereas B. YANG and D. LESTER (2008) classified these papers as ‘undecided’.

[49] To check whether different classifications have an impact on the results, we re-estimated our model including only those 75 papers in our sample which have also been con-

42. This corresponds to Model 5a in *Table 2*.

sidered by B. YANG and D. LESTER (2008). The results that we receive when employing their classification are presented in *Table 3*, Model 6. The author variable for economists is still significant, but only at the 5 per cent level. The dummy variable indicating that the data are from the United States is now also significant at the 5 per cent level. Taking all explanatory variables together, except for the economist as author dummy, we again fail to reject the null hypothesis that the variables jointly do not have an impact at any conventional significance level. This reinforces our result that the professions of the authors are the only variables with a significant impact on the results of the studies. If we, therefore, estimate the restricted model with only the profession of the author as an explanatory variable, 23 out of the 30 papers which claim a deterrence effect are correctly predicted and 29 of the 45 paper that do not claim this.⁴³⁾ Thus, 69.3 per cent of all papers are correctly classified.

[50] If we use our own classification (Model 7), the t-statistic for an economist as author is 3.24, i.e., the estimated coefficient is again significant at the 1 per cent level. The Wald test for the combined hypotheses leads to $\chi^2 = 12.38$. With 11 degrees of freedom, this is again not significant at any conventional level: the p-value is 0.336. Now, 23 papers that claim and 32 papers that do not claim a significant deterrence effect are correctly classified, which implies a correct classification of 83.3 per cent.⁴⁴⁾ Thus, the difference in the classification scheme has some effect, but it does not change the overall result: the only robust and significant effect is related to the profession of the author(s).

[51] *Table 2* summarises the predictive qualities of the estimated models. Except for Model 5 and 6, in all other specifications about 80 per cent of all cases are correctly classified. Given the fact that in nearly half of the models we only use one explanatory variable, and that we use two different classifications, this is a rather high percentage rate.⁴⁵⁾

[52] As nearly all papers are written by American researchers, these results suggest that the majority of (American) economists who are active in this field believe that the incentives provided by the threat of the death penalty are sufficiently severe in order to deter a significant number of potential murderers from committing homicide. The majority of other scientists, in particular members of the law and other social sciences faculties, appear to believe that most homicides are committed for reasons which can hardly be influenced by the threat of severe punishment. Thus, even if they might exist, the incentives resulting from the threat of capital punishment are too small in order to justify the use of the death penalty in the light of its heavy moral problems. This basic divide of convictions might cause authors to come to such different conclusions, even if they use the same data and method. As is shown in the next section, by minor changes in the estimation equations, this is possible without violating the rules of serious academic work.

43. This corresponds to Model 6a in *Table 2*.

44. Coefficient estimates for Model 7 and 7a not presented. The share of correct predictions for Model 7 and 7a is provided in *Table 2*.

45. Due to the fact that DONOHUE, JOHN J., and JUSTIN WOLFERS (2006) who are a Law and an Economic professor and deny a deterrent effect, the predictive quality of the estimated model is slightly higher if we employ the variable for a non-economic author instead of the one for an economic author.

4 Some Re-Estimations

[53] In deterrence research, scientists tend to derive highly contradictory results, even though they base their decisions on the same data sets and comparable methodologies. The meta-analysis carried out in the previous chapter has shown that in a comparison of 102 previously published papers on the deterrence effect of the death penalty, the only relevant predictor of different outcomes appears to be the affiliation of the authors.

[54] If we accept the result from the previous section that economists are significantly more likely to find a deterrence effect than other social scientists, the second question stated above becomes relevant: is it possible to reach such different conclusions without violating scientific standards? As mentioned above, W.S. MCMANUS (1985) explains the diverging results with selective perceptions of the authors, given their a priori beliefs. But this presupposes that contradictory results can be reached by minor modifications of the estimation equations. In order to show that this is possible we employ a panel data set of U.S. states already used by H. DEZHBAKHS and J.M. SHEPHERD (2006) and J.J. DONOHUE and J. WOLFERS (2006).⁴⁶⁾

[55] The sensitivity of the death penalty data has been shown before, for example, by L. KATZ, S.D. LEVITT and E. SHUSTOROVICH (2003), but also by J.J. DONOHUE and J. WOLFERS (2006). Their approach is insofar particular as they provide exact re-estimations of original results from previous papers and subsequently show how minor modifications can turn around main conclusions. We want to add to their results by putting an estimation strategy that has previously been employed by H. DEZHBAKHS and J.M. SHEPHERD (2006) under further scrutiny. In the first column of *Table 4*, we provide their original estimation results (*Table 8*, p. 525), followed by first modifications by J.J. DONOHUE and J. WOLFERS (2006, p. 806, p. 814, *Table 5*) in columns 2 and 3.⁴⁷⁾ The following four columns show our own manipulations of the data and estimation strategy. All estimated equations displayed in *Table 4* employ weights that represent population size together with time and state-specific fixed effects. Standard errors have been clustered at the state level.

[56] H. DEZHBAKHS and J.M. SHEPHERD (2006) used a panel data set of 50 U.S. states during the period from 1960 to 2000. Their coefficient estimate for the number of executions suggests that additional executions have large and highly significant deterrence effects on the murder rate in the same state and year. As can be seen in column 1 in *Table 4*, the estimate of -0.145 is significant at all conventional significance levels. The execution variable used is defined as the number of executions in a given state in a given year over the time period from

46. We use the data as being reconstructed by J.J. DONOHUE and J. WOLFERS (2006, see p. 805). These data have been made available online together with the exact specifications of the estimated models. J.J. DONOHUE and J. WOLFERS (2006) were unable to exactly reproduce the results of H. DEZHBAKHS and J.M. SHEPHERD (2006), but the differences are negligible (see for this also Footnote 43 in J.J. DONOHUE and J. WOLFERS, 2006, p. 805.).

47. Since both the original papers have been published in the same year (2006), it has to be noted in this context that the critique of J.J. DONOHUE and J. WOLFERS (2006) of the analysis of H. DEZHBAKHS and J.M. SHEPHERD refers to the working paper version of their paper, published in 2004. However, there are no differences with respect to the results that we discuss here.

1960 until 2000. The authors further use controls for per capita real income, the unemployment rate, police employment, the share of minorities in the population, as well as the percentage of the population that is between 15 and 19 years old or between 20 and 24 years, respectively. In addition, they employ time and state-specific control variables. Contrary to what has been used in most other papers, they use decade-specific rather than year dummy variables in order to capture long-term trends in crime (2006, p. 524).⁴⁸⁾

[57] J.J. DONOHUE and J. WOLFERS (2006, *Table 5*) replicate this estimation and extend it by including year instead of decade-specific fixed effects; column 2 in *Table 4* depicts our re-estimation of this specification. The use of decade instead of year fixed effects has been criticized by J.J. DONOHUE and J. WOLFERS (2006, p. 805f.) on the grounds that a decade dummy could not sufficiently capture time trends in a decade like the 1970s where both criminal policies as well as murder rates have seen pronounced changes. In particular, the death penalty moratorium from 1972 through 1976 represents a temporary, but dramatic change in crime policy; furthermore the murder rates were lower during the early or late 1970s than during the moratorium. A decade fixed effect implies a constant time trend over the entire decade and will therefore not be able to reflect these trends.

[58] As already pointed out in *Chapter 2.2*, J.J. DONOHUE and J. WOLFERS (2006) further show that the results by H. DEZHBAKHSH and J.M. SHEPHERD (2006, *Table 8*) are highly sensitive, both to the exclusion of Texas as well as to the definition of the execution variable. Namely, the execution variable in their paper does not control for population size. According to J.J. DONOHUE and J. WOLFERS (2006, p. 815) “this specification implies that one more execution in Wyoming would deter three-fourths of a homicide, while in California it would deter fifty homicides”. In order to avoid such an arbitrary weighting of individual states in their modification, J.J. DONOHUE and J. WOLFERS (2006) scale the execution variable per 100,000 residents. Thus, the effect of executions on murder rates becomes insignificant.⁴⁹⁾ Our re-estimate of this modification is provided in column 3 in *Table 4*.

[59] We further extend this analysis by using the modified execution variable (i.e., the number of executions in a given state in a given year per 100,000 residents) over a longer time horizon; we extend the survey period to cover all periods from 1934-2000.⁵⁰⁾ Our estimate of the coefficient on the execution variable (column 4 in *Table 4*) is highly comparable to the one received by J.J. DONOHUE and J. WOLFERS (2006) (column 3 in *Table 4*), both in terms of economic as well as statistical significance. Also the adjusted R^2 values of 0.836 and 0.835 are very similar. Thus, in the present specification, using a longer sample does not appear to affect the results; in both estimations the effect of executions on murder rates is statistically insignificant.

48. J.J. DONOHUE and J. WOLFERS (2006, Footnote 46) list a number of previous papers that use year fixed effects rather than decade fixed effects. Interestingly enough, the list includes several publications by J.M. SHEPHERD (2004, 2005) as well.

49. See TABLE 5, column 4 in J.J. DONOHUE and J. WOLFERS (2006).

50. For this extended sample period, we use a dataset that J.J. DONOHUE and J. WOLFERS (2006) have employed in order to re-estimate the results by L. KATZ, S.D. LEVITT and E. SHUSTOROVICH (2003).

Table 4: Some Re-Estimations

| Model | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|-----------------------------------|-----------------------|-----------------------|----------------------|----------------------|-----------------------|----------------------|------------------|
| Constant term | Not reported | -5.891 (-1.34) | 4.979 (-1.08) | 5.031 (-1.07) | -6.006 (-1.35) | -18.158** (-3.22) | 1.236 (0.34) |
| Executions | -0.145*** (-10.52) | -0.138*** (-10.71) | | | -0.140*** (-10.96) | | |
| Executions per 100'000 population | | | -8.356 (-1.43) | -8.393 (-1.40) | | 5.449(*) (1.69) | 7.395* (2.11) |
| Per capita real income | -0.001*** (-5.16) | 0.000 (0.01) | -0.003 (-0.22) | -0.002 (-0.14) | 0.001 (0.10) | 0.036(*) (1.91) | 0.007 (0.32) |
| Unemployment rate | -0.147* (-2.44) | -0.194** (-3.06) | -0.224*** (-3.51) | -0.214*** (-3.36) | -0.185** (-2.92) | | |
| Police Employment | 0.000 (-0.63) | -0.000 (-0.31) | -0.000 (-0.76) | -0.000 (-0.68) | -0.000 (-0.25) | | |
| Per cent minority | 0.167*** (-6.04) | 8.891(*) (1.76) | 9.729(*) | 9.022(*) | 8.118 | 17.911*** | 30.921*** |
| Per cent 15 to 19 years old | -1.607*** (-8.38) | 90.088(*) 1.78 | 74.598 (1.44) | 77.082 (1.44) | 93.655(*) (1.81) | 224.502*** (4.94) | |
| Per cent 20 to 24 years old | 2.152*** (-9.51) | 66.681* (2.23) | 74.233* (2.38) | 73.075* (2.13) | 64.490(*) (1.96) | 5.961 (0.16) | |
| Prisoner death rate | | | | | | | 0.046 (0.73) |
| Time Fixed Effects | Decade | Year | Year | Year | Year | Year | Year |
| State Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Adjusted R ² | 0.817 | 0.846 | 0.836 | 0.835 | 0.845 | 0.804 | 0.785 |
| Standard error of the regression | Not reported | 1.416 | 1.460 | 1.462 | 1.416 | 1.9344 | 2.0257 |
| Log of the likelihood function | Not reported | -3500.88 | -3561.55 | -3362.09 | -3301.09 | -6080.36 | -6217.13 |
| Sample period | 1960 – 2000 | | | | 1934 – 2000 | | |
| Clusters | 49 | 49 | 49 | 49 | 49 | 51 | 51 |
| Number of observations | 2009 | 2009 | 2009 | 1986 | 1986 | 2954 | 2954 |

The numbers in parentheses are the t-statistics of the estimated parameters. ‘***’, ‘**’, ‘*’ or ‘(*)’ indicates that the corresponding null hypothesis can be rejected at the 0.1, 1, 5, or 10 per cent significance level, respectively.

[60] The estimation equation in column 5 of *Table 4* also uses data from 1934 until 2000, but relies on the execution variable as defined by H. DEZHBAKSH and J.M. SHEPHERD (2006), i.e., the total number of executions in a given state without controlling for population size. The resulting coefficient estimate is negative and highly statistically significant and therefore comparable to the original result that H. DEZHBAKSH and J.M. SHEPHERD (2006) received using a shorter sample (column 1). The use of an extended sample period can be considered as a robustness test, i.e., if a relationship between two variables is only significant for a given time period, one would want to be reluctant to base policy recommendations on it. As the results in column 3 and 4, as well as in column 1, 2, and 5 in *Table 4* are highly similar, this does not seem to be a problem in these data. The sensitivity of the results with respect to the definition of the execution variable appears to be more worrisome. In column 6 of *Table 4*, we provide yet another modification where we use the number of executions per 100,000 inhabitants, together with the longer sample from 1934-2000, but this time we exclude the unemployment rate and the police variable. The estimated coefficient of the execution variable is now positive and statistically significant at the ten per cent level. In other words, increasing the number of executions in a given state would be associated with an increase in the murder rate of that state. This result is contrary to what H. DEZHBAKSH and J.M. SHEPHERD (2006) find and yet, the differences with respect to the estimation equation in column 1 and column 6 of *Table 4* are neither excessive, nor based on dubious scientific behaviour. Putting the number of executions in relation to population size is arguably more appropriate than using the absolute number of executions. The same holds for the use of year-specific rather than decade-specific fixed effects.⁵¹⁾ Omitting the police variable that has been insignificant across all previously employed specifications appears to be reasonable as well. The omission of the unemployment rate is, however, somewhat more difficult to justify, particularly since we find in all other estimations presented in *Table 4* a statistically significant relationship between the unemployment and the murder rate. However, one could argue that per capita real income captures, to a certain extent, changes in the unemployment rate. This can also be seen in column 6 of *Table 4* where the estimated coefficient on per capita real income becomes significant at the 10 per cent level once we omit the unemployment variable. Including controls for income, but not for unemployment in the estimation equation is a strategy that has been employed by several authors, who notably do not agree with each other regarding the existence of a deterrence effect.⁵²⁾

[61] A positive and statistically significant association between executions and the murder rate is also found in column 7 in *Table 4* where we include the prison death rate per 1'000 prisoners in order to reflect prison conditions. This variable has previously been used by L. KATZ, S.D. LEVITT and E. SHUSTOROVICH (2003, p. 319ff.) who argue that if potential criminals are indeed rational, then prison conditions in general and the probability of dying in prison in particular can be expected to enter potential criminals' utility calculus. In addition, we

51. See for this also J.J. DONOHUE and J. WOLFERS (2006, p. 805f.).

52. See, for example, I. EHRLICH (1977), B.E. FORST (1983), L. ZHIQIANG (2004), J. FAGAN et al. (2006), or A.K. DILLS et al. (2010).

control for per capita real income and the share of minorities in the population. The resulting coefficient of the execution variable is positive and significant at the five per cent level. The adjusted R^2 of 0.785 is comparable to the values received in the other specifications.

5 Concluding Remarks

[62] Despite nearly fifty years of econometric research, there is still an intense discussion on whether the death penalty has a deterrent effect or not. Many economists believe in the deterrent effect, but most other scientists who participate in this debate do not. Supporting this impression, our meta-analysis shows that the major and only significant driver of whether the author(s) claim(s) that the death penalty deters potential murderers is his/her profession. But these divergent results do not necessarily imply that scientific standards are violated. There are, of course, also recent studies that do not keep up with today's scientific (econometric) standards, but our re-estimates using data originally employed by H. DEZHBAKHSH and J.M. SHEPHERD (2006) and by L. KATZ, S.D. LEVITT and E. SHUSTOROVICH (2003) show that by employing relatively few as well as justifiable modifications can lead to contradictory results. This clearly shows the sensitivity of the findings and, therefore, supports the claim of the NATIONAL RESEARCH COUNCIL (2012) that such results ought not to be used to influence policy decisions regarding the appropriateness of the death penalty.

[63] The literature on the deterrent effect of capital punishment is overall inconclusive; thus, the fact that individual authors persistently claim to have found solid evidence in one or the other direction, raises two questions. Firstly, what are the causes for these different findings? Do different data samples, estimation methods or time periods lead to different results, or do the outcomes merely reflect prior convictions of the authors? Secondly, to what extent is it possible to derive such diverging results by slightly changing the specification of the test equations without violating scientific standards? After a survey of the over forty reviews of this literature available so far, we perform a meta-analysis of 102 studies published between 1975 and 2011. The profession of the author is the only significant explanatory variable: Economists claim significantly more often to have found a significant deterrence effect than scientists from the law or other social science departments. On the other hand, using a panel data set of U.S. states, we show how easy it is to derive contradictory results by employing alternative specifications. Thus, our results support the claim that the empirical evidence presented to date is by far too fragile in order to base political decisions on it.

[64] Our results also reinforce the considerations of W.S. MCMANUS (1985) that selective perceptions might be the cause for divergent findings. If rather different results can be obtained under reasonable assumptions, researchers will consider those outcomes as being reliable which correspond to their pre-conceptions. In this respect, economists are no different from other scientists. And because economists tend to believe more in incentives than other (social) scientists, these differences in the results obtained are not really surprising.

[65] If, following the conclusion of the NATIONAL RESEARCH COUNCIL (2012), the deterrence argument can no longer be used to defend the use of the death penalty, it becomes difficult to defend it at all. In this situation, moral arguments play a major role. There is, however,

also a debate about the moral status of this penalty, as, for example, the discussion between C.R. SUNSTEIN and A. VERMEULE (2005, 2005a) and C.S. STREIKER (2005) shows. But the former, who try to defend the death penalty for moral reasons, rely on its deterrent effect. Taking our results seriously, this position is hard to justify. There is, of course, the possibility to believe in the deterrent effect even if one acknowledges that the empirical evidence is weak.⁵³⁾ But then, one should at least also acknowledge that there is no scientific basis for this belief.

Zusammenfassung

Angesichts dessen, dass die Literatur zur Abschreckungswirkung der Todesstrafe keine eindeutigen Schlussfolgerungen erlaubt, führt die Tatsache, dass einzelne Autoren beharrlich behaupten, zuverlässige Evidenz in der einen oder anderen Richtung gefunden zu haben, zu zwei Fragen: Was sind zum einen die Ursachen für diese unterschiedlichen Ergebnisse; führen unterschiedliche Stichproben, Schätzverfahren und/oder Untersuchungszeiträume zu diesen Unterschieden oder widerspiegeln sie schlicht die vorgängigen Überzeugungen der verschiedenen Autoren? In welchem Ausmass ist es zweitens möglich, solche unterschiedlichen Ergebnisse durch geringfügige Modifikationen der Spezifikationen der Testgleichungen zu erhalten, ohne wissenschaftliche Standards zu verletzen. Nach einer Übersicht über die mehr als vierzig Surveys dieser Literatur, die heute verfügbar sind, führen wir eine Meta-Analyse von 102 ökonometrischen Studien durch, die zwischen 1975 und 2011 veröffentlicht wurden. Dabei zeigt sich, dass die Fachrichtung der Autoren als einzige Variable einen signifikanten Erklärungsbeitrag liefert: Ökonomen behaupten signifikant häufiger einen statistisch und auch materiell signifikanten Abschreckungseffekt gefunden zu haben als andere Sozialwissenschaftler oder Juristen. Mit einem Paneldatensatz für die Vereinigten Staaten demonstrieren wir, wie leicht es ist, mit Hilfe alternativer Spezifikationen der Testgleichungen sich widersprechende Ergebnisse zu erzielen. Insofern unterstützt unsere Untersuchung die Auffassung, dass die bisher vorgelegte empirische Evidenz viel zu unsicher ist, als dass man darauf politische Entscheidungen abstützen könnte.

53. See for this, for example, G.S. BECKER. (2006).

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Appendix

Table A1: Classification of Authors and Papers

| Author(s) | Affiliation(s) |
|--|-----------------------|
| I) Deterrent Effect | |
| BRUMM, H.J., AND D. CLONINGER (1996) | Economics |
| CHRESSANTHIS G.A. (1989) | Economics |
| CLONINGER, D.O. (1977, 1992) | Economics |
| CLONINGER, D.O., and R. MARCHESINI (2001, 2006) | Economics |
| DEZHBAKHSH H., P.H. RUBIN and J.M. SHEPHERD (2003) | Economics |
| DEZHBAKHSH H., and J.M. SHEPHERD (2006) | Economics |
| EHRlich, I. (1975, 1977, 1977a) | Economics |
| EHRlich I., and Z. LIU (1999) | Economics |
| FAJNZYLBER P., D. LEDERMAN and N. LOAYZA (2002) | Economics |
| LAYSON, ST. (1983, 1985, 1986) | Economics |
| LIU, Z. (2004) | Economics |
| MERRIMAN, D. (1988) | Economics |
| MOCAN N.H., and K.R. GITTINGS (2003, 2006) | Economics |
| PHILLIPS D.P., and J.E. HENSLEY (1984) | Sociology, Psychology |
| SHEPHERD J.M. (2004a, 2005) | Economics |
| STACK, ST. (1987, 1990, 1995, 1998) | Sociology |
| WOLPIN K.I. (1978) | Economics |
| YANG, B. and D. LESTER (1988) | Economics, Psychology |
| YUNKER, J.A. (1977, 2001) | Economics |
| ZIMMERMAN P.R. (2004, 2006, 2009) | Economics |
| II) No Deterrent Effect | |
| ALBERT, C.J. (1999) | Law |
| ARCHER, D., R. GARTNER and M. MARC (1983) | Sociology |
| AVIO, K.L. (1979, 1988) | Economics |
| BAILEY, W.C. (1977, 1978, 1978a, 1979, 1979a, 1979b, 1979c, 1980, 1980a, 1983, 1984, 1990, 1998) | Sociology |
| BAILEY, W.C., and R.P. LOTT (1977) | Sociology |
| BAILEY, W.C., and R.D. PETERSEN (1989, 1994) | Sociology |
| BECHDOLT, B.V. (1977) | Economics |
| BLACK, TH., and T. ORSAGH (1978) | Economics |

Table A1: Classification of Authors and Papers (continued)

| Author(s) | Affiliation(s) |
|--|---|
| BOWERS, W.J., and G.L. PIERCE (1975, 1980) | Sociology |
| BOYES, W.J., and L.R. MCPHETERS (1977) | Economics |
| CHEATWOOD, D. (1993) | Sociology, Criminal Justice |
| DECKER, S.H., and C.W. KOHFELD (1984, 1986, 1990) | Criminology |
| DILLS A.K., J.A. MIRON and G. SUMMERS (2010) | Economics |
| DONOHUE J., and J. WOLFERS (2006) | Law, Economics |
| FAGAN, J. (2006) | Law, Public Health |
| FAGAN J., F.E. ZIMRING and A. GELLER (2006) | Law, Public Health |
| FORST, B.E. (1977, 1983) | Law |
| FOX, J.A. (1977) | Law |
| GROGGER J. (1990) | Economics |
| HJALMARSSON R. (2009) | Economics |
| KATZ L, ST. D. LEVITT and E. SHUSTOROVICH (2003) | Economics |
| KING, D.R. (1978) | Law |
| KLECK, G. (1979) | Sociology |
| KNORR, ST.J. (1979) | Law |
| KOVANDZIC T., L.M. VIERAITIS and D.P. BOOTS (2009) | Criminology, Criminal Justice, Psychology |
| LEMPERT, R. (1983) | Law |
| MARVELL, TH.B., and C.E. MOODY (1999) | Sociology, Economics, Econometrics |
| NEUMAYER, E. (2003) | Economics |
| PARKER R.N., and D.M. SMITH (1979) | Sociology, Psychology |
| PETERSON, R.D. and W.C. BAILEY (1988) | Sociology |
| RAHAV, G. (1983) | Sociology |
| SORENSEN J., et al. (1999) | Mathematics, Computer Science, Political Science, Sociology |
| STACK, ST. (1993) | Sociology |
| STRINGHAM E.P., and J. LEVENDIS (2010) | Economics |
| THOMSON, E. (1997, 1999) | Sociology, Criminology |

Table A1: Classification of Authors and Papers (continued)

| Author(s) | Profession(s) |
|---|------------------------|
| III) Undecided | |
| CANTOR, D., and L.E. COHEN (1980) | Sociology |
| COCHRAN, J.K., and M.B. CHAMLIN (2000) | Sociology, Criminology |
| COHEN-COLE E., S. DURLAUF, J. FAGAN and D. NAGIN (2009) | Economics |
| COVER, J.P., and P.D. THISTLE (1988) | Economics |
| EKELUND, R.B., et al. (2006) | Economics |
| KLEIN, L.R., B. FORST and V. FILATOV (1982) | Economics, Law |
| LEAMER, E.E. (1983) | Economics |
| MANSKI, CH. and J.V. PEPPER (2011) | Economics |
| McFARLAND, S.G. (1983) | Social Psychology |
| MCKEE D.L., and M.L. SESNOWITZ (1977) | Economics |
| NARAYAN P.K., and R. SMYTH (2006) | Economics |
| PASSEL, P. (1975) | Economics |
| PASSEL, P., and J.B. TAYLOR (1977) | Economics |
| PHILLIPS, D.P. (1980) | Sociology |
| SESNOWITZ M., and D. MCKEE (1977) | Economics |