THE PENNSYLVANIA STATE UNIVERSITY
SCHREYER HONORS COLLEGE

DEPARTMENT OF SOCIOLOGY AND CRIMINOLOGY

False Deterrent? An analysis of Death Penalty Filings in Pennsylvania

ETHAN M. GALLEY
SPRING 2025

A thesis
submitted in partial fulfillment
of the requirements
for a baccalaureate degree
in Criminology
with honors in Criminology

Reviewed and approved* by the following:

Jeffrey T. Ulmer
Professor of Sociology and Criminology
Thesis Supervisor

Gary J. Adler Jr.
Honors Program Advisor for Sociology and Criminology
Thesis Honors Adviser

* Electronic approvals are on file.

ABSTRACT

To date, prior research on the relationship between capital punishment and homicide has remained largely inconclusive. At best, the current literature yields mixed evidence, often suggesting either a deterrence effect, a null effect, a brutalization effect, or an admixture depending on model, sample, and statistical techniques used. One such reason for this inconsistency is how this relationship is measured; much of the literature has relied on measurements either 1) following executions or 2) involving the wider presence of death penalty statutes. Notably, national declines in capital sentencing and executions continue to affect the statistical power of these measures. Likewise, as several states have opted toward gubernatorial moratoriums, any measured effect may become volatile to state-specific sanction trends. To this end, it would help to not only take stock of where the current literature lies, but more closely investigate the initial components of capital sentencing, specifically with regard to prosecutorial decisions to seek capital punishment. As such, the present study seeks to analyze the effect of prosecutors' death penalty filings and subsequent sentencing decisions on county homicide counts, focusing on Pennsylvania. Using multivariate analysis of longitudinal data on homicides between the years 2000-2010, this study aims to assess if, at its initial onset, prosecutors' seeking the death penalty, and subsequent sentences, yield any measurable association with deterrence. Given the contemporary shift towards moratoriums and the absence of executions within Pennsylvania, such an analysis is warranted.

TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

ACKNOWLEDGEMENTS

Among the many I owe my gratitude to for the guidance shown throughout the creation of the current thesis, I would like to extend my sincerest appreciation to Dr. Jeffery Ulmer. His kindness, wisdom, and experience has been the keystone to this project, and I cannot do justice how excellent he has been as a thesis supervisor and mentor. I thank you, Dr. Ulmer, for inspiring me to seek out opportunities I would have otherwise believed inaccessible to undergraduates, notably the chance to attend ASC 2023, and soon ASC 2025. I look forward to continuing my growth as a prospective scholar, and earnestly hope to rely on you as a continued source of expertise during my graduate studies.

Among the many faculty and staff of the honors college, I would like to thank both Dr. Stacy Silver and Dr. Gary Adler for their time spent as faculty advisors. I cannot thank either of you enough for your patience and assistance in helping me navigate the SHC guidelines and policies. To my former professors and mentors at the Greater Allegheny campus, I thank you all for providing me with the opportunity to challenge myself to stay curious as a first-generation student, researcher, and advocate. In particular, Dr. Trappen, Dr. Mclean, Dr. White, I hold you all in the highest regard and continue to cherish our many conversations. As I continue my journey within and beyond academia, I hope to lead by your example as ideal mentors.

Mom, Dad, I am confident that I will never know the anxiety and stress you both have endured over these past four years. Nor do I believe that I will ever be able to adequately show how invaluable my time as an undergraduate has been. That said, I thank you both for being the bedrock of this entire experience, for allowing me the chance to pursue the education I desired on my terms (mostly), and for continuing to support me wherever possible. I love you both.

Chapter 1

Introduction

In 2011, the National Research Council (NRC) of the National Academies of Sciences, Engineering, and Medicine launched a study of existing literature examining the effect of capital punishment on homicide. Following an earlier report published in 1978, two years after the Supreme Court's decision in *Gregg v. Georgia*, 428 U.S. 153 (1976)[1], the NRC created the Committee on Deterrence and the Death Penalty to lead this review. The Committee would subsequently evaluate prior work dating as far back as thirty-five years, corresponding with a heightened empirical interest in the capital sanctioning regime post-*Gregg*. On April 18th, 2012, the Committee would publish its findings; within its 127-page report, the Committee would come to a significant, albeit not unprecedented, conclusion: "..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" (NRC, 2012, p. 2.).

Throughout the years following the Committee's report, interest in the application and status of capital punishment has remained steady among practitioners and policymakers. State legislatures and correctional personnel continue to weigh the practical and legal implications of state-sanctioned executions. That is, despite a lack of any conclusive, long-term evidence concerning the effectiveness of capital executions as a method of deterring prospective homicide, it remains the subject of policy decisions. For example, states with capital statutes have opted for

---

[1] *Gregg v. Georgia*: Writing for the 7-2 majority, Justice Stewert notes, following the Court's decision in *Ferman v. Georgia*, 402 U.S. 238 (1972), adequate change was made to state laws with due consideration to the rights of defendants sentenced to capital punishment to justify its constitutionality.

alternative methods of execution despite challenges made by pharmaceutical manufacturers in the acquisition of compounds used in lethal injections. These include the use of firing squads, electrocution, and lethal gas (Berger 2020). In 2024 alone, Alabama became the first state in the union to apply nitrogen hypoxia as a method of execution, a method which would be replicated twice within the state during the same year[2]. Likewise, state assemblies have continuously presented bills regarding defendant eligibility for capital sentencing. Between 2023 and 2024, around thirteen pieces legislation expanding death-eligibility were introduced by eight states and the federal government, whereas nine bills were introduced by four states and the federal government limiting eligibility (DPIC, 2024, 2025a). Furthermore, as some states consider the possibility of gubernatorial moratoria and sentencing alternatives (e.g., lifetime without parole (LWOP), legislatures across the nation have seen their share of requests to outright abolish this practice, be it from citizens, advocacy groups, or lawmakers themselves (DPIC, 2025b).

In spite, or perhaps because of the underlying policy interest within the capital sanctioning regime, academics continue to evaluate the empirical basis of the deterrence question. Heeding the recommendations of the NRC, recent publications within the deterrence literature have attempted to address the limitations of prior death penalty studies. Notably, much of the prior research concerning a hypothesized deterrence relationship has suffered from statistical deficiencies, be it the result of how state-specific differences are treated within models (Dezhbakhsh & Shepherd, 2006; Ehrlich, 1975; Mocan & Gittings, 2003), differences in the assumptions used to model deterrence (Dezhbakhsh et al., 2003; Zimmerman, 2004), or the variables tested to predict deterrence (Kovandzic et al., 2009). Focusing on the latter, much of

[2] See Death Penalty Information Center - Execution Database for executions of Kenneth Smith (January 25th), Alan Miller (September 26th), and Carey Grayson (November 21st).

the known literature following *Gregg* utilizes either 1) a standardized number of executions as a predictor (Dezhbakhsh & Shepherd, 2006; Ehrlich, 1975; Kovandzic et al., 2009), or 2) comparisons of homicide propensity between states with and without capital statues, which is largely common of recent literature (Dezhbakhsh et al., 2003; Kovandzic et al., 2009; Manski & Pepper, 2012; Mocan & Gittings, 2003; Oliphant, 2022; Parker, 2021).. In both cases, studies have yielded contradictory findings of a deterrence relationship, with some authors finding evidence to support such a relation, while others find little to no evidence of any relationship. In some cases, studies have even indicated a positive relationship, or "brutalization" effect, in which the death penalty (be it though execution or statutory presence) yields an increase in subsequent homicide (Cochran et al. 1994; Cochran & Chamlin 2000; Dezhbakhsh & Shepherd, 2006; Donohue & Wolfers 2005; Ehrlich 1975; Kovandzic et al., 2009). Indeed, if such a relationship is to be believed, then it follows that the application of capital sentencing is in fact antithetical to criminal deterrence. However, given the broader inconsistency surrounding any relationship between capital punishment and homicide, it may prove beneficial to reassess how this relationship is initially measured; such is further made imperative as capital moratoriums define state differences in capital sentencing across the United States (Oliphant, 2022).

As such, the present study seeks to guide inquiry of any effect corresponding between capital punishment and prospective homicide. Specifically, by considering the onset of the capital sanctioning process, I provide a novel avenue by which a relationship may be indicated. That is, through the use of prosecutorial and judicial decision-making, this study aims to assess if the desire to seek capital punishment at the onset and conclusion of capital trials yields any value in determining a predictive relationship between capital punishment and homicide. Where the likelihood of the imposition of a death sentence and execution differs dramatically across jurisdiction (NRC, 2012; Shepherd, 2005; Ulmer et al., 2020a), prosecutorial filings provide a basis for measuring a relationship where executions are less common, and allow for the consideration of prosecutorial discretion in the initial course of capital trials (Ulmer et al., 2020b). Furthermore, given the inconsistency of prior findings in conjunction with recent moratoriums, such a measure is not impacted by the absence of executions. Using Pennsylvania as the foundation for multiple analyses, this study employs multivariate regression comparing marginal deterrence between capital filings and sentences at each county across the commonwealth. Notably, cross-sectional and time-series analyses are conducted to elucidate a relationship between both predictors and homicide between the years 2000 and 2010. Change score comparisons are likewise presented to support correlational inferences wherever possible. The benefit of the results are twofold: 1) they will help me understand the effect of the death penalty in the context of mixed findings; and 2) by measuring the initial charging decisions of prosecutors, these results will generate invaluable insight into how prosecutors exercise their authority within capital cases.

Chapter 2

Literature Review

Theoretical Basis

The epistemological underpinning of deterrence as a guiding rationale for criminal punishment, and by extension social control, can be attributed to the philosophical writings of enlightenment thinker Cesare Beccaria. In his treatise *On Crimes and Punishments* (1764), Beccaria posits a tendency of humans to pursue behavior on a basis of optimizing pleasure or desirable outcomes relative to the incidence of those less desirable. This rational choice theory, or the idea that people are guided on the principle of maximizing pleasure while minimizing pain, may therefore be used as a foundation for law and punishment. That is, the occurrence of crime may be curtailed if 1) rational beings are made aware of the legal codes prescribing punishment for the infringement of law, and 2) the specified punishment provides enough marginal deterrence to disincentivize crime while also not being so detrimental to damage society (Beccaria, 1764). In particular, this theory maintains that deterrence is a condition of punishment certainty, swiftness, and severity; future criminality is best minimized when official sanctions are highly likely to occur after the commission of a crime, the sanction occurs with minimal delay, and the sanction outweighs any benefit presented though criminality (Beccaria, 1764; Becker, 1968; Chalfin & McCrary, 2017).

Among social scientists, the operationalization of this relationship varies by discipline as a result of differing assumptions made about the decision-making process, thus inviting the risk of conflicting findings even when employing similar analytical techniques. For example, the Beckerain model employed by economists grounds the process of criminal decision-making on the extent to which individual perceptions of punishment certainty and severity correspond with reality. That is, the likelihood of an individual being successfully deterred is contingent on a rational calculus in which the risk of punishment is weighed against any utility presented through criminal behavior, provided that the individual's perception of risk severity and certainty is one-to-one with its objective likelihood. This would suggest that individuals are able to effectively measure and quantify the risk of punishment and act according to the most rational outcome (Becker, 1968; Dezhbakhsh & Shepherd, 2006; Ehrlich, 1975). Alternatively, according to conceptualizations proposed by criminologists and sociologists, criminal-decision making is largely the product of how individuals perceive the risk of punishment based on experiences, which are naturally grounded in place and context. The likelihood that an individual is deterred from future criminality will depend on how an individual's subjective perception of risk is impacted by their environment, and thus requires consideration of how a given individual's perception is moderated by external, criminogenic factors. While these models consider the role of certainty and severity within the decision-making process, these variables remain subject to individual perception, and may vary substantially from reality (Chalfin et al., 2012; Donohue & Wolfers, 2005; Nagin, 1998, 2013; NRC, 2012).

In the context of capital punishment, the application of either economic or sociological conceptions of deterrence theory requires the assumption that capital sanctions are adequately certain and severe so as to deter future homicide (Donohue & Wolfers, 2005). However, the application of deterrence models often suffers from the practical limitations of the contemporary capital sanctioning regime. To elaborate, while executions may present the highest degree of severity, such a claim is only valid based on a comparison of the perceived severity of other non-capital alternatives, specifically lifetime without parole (LWOP). Notably, prior literature suggests that a comparison of the marginal deterrence between the death penalty and LWOP on the basis of severity alone is insufficient to suggest deterrence (Cook, 2009; Katz et al., 2003). Rather, capital sentences must also maintain a level of certainty so as to warrant a high likelihood of a capital sanction and execution following a capital offense (NRC, 2012). However, this assertion relies on the premise that enough defendants are being sentenced to death. Most defendants eligible for capital sentencing are unlikely to be sentenced to death, let alone face execution, relative to lifetime imprisonment (Cook, 2009; Katz et al., 2003; Ulmer et al., 2020b). According to the Bureau of Justice Statistics (2024), from 1976-2022, around 8,577 prisoners received a death sentence, however, ~6,726 were subsequently removed from death row either due to appeals, prisoner death (including execution), or commutations. That said, only 1,558 prisoners have been executed within this time span (BJS. 2024). Furthermore, research has indicated the likelihood of capital sanctioning as predominately a product of "geographic arbitrariness", or a matter of location more than consistent application according to case variables (Donohue, 2014; Ulmer et al., 2020a)..

One less studied aspect of capital sanctioning is its degree of celerity, or swiftness (Bailey, 1980; Jaynes & Wilson, 2022). Notwithstanding the relative nature of actually receiving a death sentence, the time between the imposition of a sentence and subsequent execution remains vast. As of 2022, the average time spent on death row stands at 249 months, or roughly 21 years prior to execution compared to 74 months (~6 years) in 1884 (BJS, 2024). While this figure only accounts for those executed, it underscores the delay latent in the capital sanction regime, one that directly provides de facto lifetime sentences (DPIC, 2025c). Importantly, the stark absence of celerity within the formulation of deterrence models undermines the predictive value of such models when inferring effects on homicide (Bailey, 1980).

Empirical Review

In the years following the Supreme Court's decision in *Gregg* (1765), the bulk of capital deterrence literature has provided contrary and inconclusive findings, ultimately providing minimal consensus among scholars. Within the subsection of literature purporting a deterrence relationship between capital sanctioning and homicide, many analyses, largely by economists, cite the conclusions presented by Issac Ehrlich (1975). Utilizing an econometric, time-series analysis comparing the national rate of capital murders to the probability of arrests, conviction, and execution, as well as broader socioeconomic and demographic controls, Ehrlich predicted that for every execution that occurred between 1933 – 1969, as many as 8 murders were prevented. Notably, within this analysis he suggests the relationship between execution and murder to be so profound as to outweigh murder utility, assuming that individual behavior is conditioned purely on such a calculus (Ehrlich, 1975). Regardless, his findings were influential enough to be cited within an amicus curiae presented by Solicitor General Bork to the Supreme Court in *Gregg*, which ultimately ended the federal hold on state-sanctioned executions (Donohue & Wolfers, 2005).

That said, much of the post-*Gregg* literature, including the observations made within the NRC's initial review on deterrence (NRC, 1978), find the conclusions presented by Ehrlich (1975) to be statistically and theoretically suspect, not least for the fact that the author grounded his findings in a nation-wide estimate. Indeed, critics have argued that by aggregating his analysis at the national level, it fails to account for state-level fluctuations in capital sanction regimes (Bowers & Pierce, 1975; NRC, 1978; Oliphant, 2022). This is likewise indicated within his selected time period and conviction, as Ehrlich's data is largely based on conviction data from larger jurisdictions (Bowers & Pierce, 1975). Further, many reviews have noted the unreliability of Ehrlich's pre-analysis assumptions in conjunction to his wider regression model. Specifically, the notion that potential offenders opt to calculate the costs of homicide relative to the maximum utility prior to offending faces methodological challenges, namely to what extent is an individual's perceptions of punishment risk a reflection of the objective as opposed to purely perceptual (Chalfin et al., 2012; Donohue & Wolfers, 2005; Nagin, 1998, 2013; NRC, 2012)?

Despite these notes, several other studies have indicated a negative correlation between capital punishment and homicide using improved, albeit similar methods and assumptions. For example, in analysis of executions within 3,054 counties from 1977 – 1996, Dezhbakhsh et al. (2003) finds that for each execution that occurs, as many as eighteen murders are prevented (with a margin of error at ten). Additionally, the authors posit that by relying on county aggregates as the basis for analysis, they were better able to account for county-specific effects, minimizing the likelihood in false aggregations among states with varied sentencing trends (Bowers & Pierce, 1975; Ehrlich, 1975). In a follow up study, Dezhbakhsh and Shepherd (2006) compared execution and capital law status data at the state level across all 50 states in a panel analysis of homicide trends of moratoriums. Similar to the prior study, they assert a deterrent effect within most of their samples following a 1-to 2-year window before and after the reinstatement of capital sentencing. Others have yielded deterrence relationships on the sole basis of the legal status of capital statues (Liu, 2004); indicated negative, yet weak relationships with executions (Donohue & Wolfers, 2005); and some have even suggested robust enough effects that a lack of ample executions may be associated with a statistically significant increase in homicides (Mocan & Gittings, 2003; Shepherd, 2005).

In terms of the wider debate, studies with weak or null findings primarily highlight theoretical and analytical shortcomings of prior literature citing any robust relationship; some of the more prominent concerns are identical to those previously expressed of Ehrlich (1975). In response to the analysis posed by Dezhbakhsh et al. (2003, 2006), Donohue and Wolfers (2005) highlight the fragility of prior studies relying on execution risks within their analyses, as the general risk of execution has historically varied across states based on the legals status and application of capital punishment over time. More directly, they argue that any change within sanction regimes, however slight, can yield significant differences within the regression model used, ultimately affecting the statistical validity of a study's findings (Donohue & Wolfers, 2005). Likewise, they mention that prior conceptualizations of the deterrence relationship fail to adequately capture external factors that contribute to homicide trends in recent decades, let alone the assumptions made toward the deterrence cost-benefit analysis. This was similarly noted by Kovandzic et al. (2009); within a review of various capital deterrence studies, including Dezhbakhsh et al. (2003, 2006). Mocan & Gittings, (2003), Shepherd (2004), and Zimmerman (2004) among others, the authors find that calculations of perceived risk, and thus hypothetical deterrence, tend to exhibit little relationship with actual risks of punishment or execution. Put simply, econometric theories and rationalizations of capital deterrence tend to discount the sociopolitical contexts that mediate the actual frequency and status of executions (Chalfin et al., 2012; Donohue & Wolfers, 2005; Kovandzic et al., 2009).

Within a much less acknowledged section of the literature, several authors have additionally noted the presence of an inverse relationship between capital punishment and homicide, known as a "brutalization effect". That is, when an execution and/or the legal status of a capital statute are salient, there may in fact be an increase in the number of homicide in the area under observation (Baily, 1998; Shepherd, 2005). In theory, this positive correlation may be attributed to the notion that executions act as a formal state-sanctioned instance of vigilantism, essentially legitimizing acts of homicide as a means of retributive justice and "brutalizing" those condemned to death. Thus, to the potential murder, the presence and enforcement of a capital sanctioning regime may be seen as providing enough validation for homicide, especially as a means of settling perceived wrongs (Cochran et al., 1994, 2000; Shepherd, 2005). Examples of this effect can be found within the works of Baily (1998), Cochran et al. (1994, 2000), and Shepherd (2005). However, similar to studies promoting a deterrent effect, these have likewise prompted various conclusions, ranging from brutalization of homicides generally (Baily, 1998), moderation based on offender-victim social ties (Cochran & Chamlin, 2000), or difference of state-specific execution "thresholds" (Shepherd, 2005)

Most recently, studies have attempted to readdress the deterrence question by evaluating the types of measure, statistics, and models used. Given contemporary efforts towards capital moratoriums and abolition in numerous state legislatures, as well as the associated monetary and human costs of this punishment (Oliphant, 2022; Steiker & Steiker, 2020; Ulmer et al., 2020b), two recent articles (Oliphant, 2022; Parker, 2021) have focused largely on the broader effect of capital statutes. Specifically, these studies have promoted the value of using synthetic control as opposed to traditional least squares models, which are used to account for state differences in legal status by presenting hypothetical "synthetic" control states with no change in capital punishment legal status over time. Both studies maintain that no deterrence relationship can be identified within their samples; however, as a consequence of their sole reliance on statutes as the basis for their analyses, their results present the same concerns issued by previous authors regarding the inadequacy of capital legal status in homicide decision making (Kovandzic et al., 2009; NRC, 2012).

Hypothesis

Given the extent of the literature finding mixed and largely inconclusive results, I venture to provide an alternative proxy for gauging any relationship between capital punishment and homicide, specifically prosecutorial decisions to file for death as well as death sentences. While several studies have scrutinized measures of punishment probability, including arrest and sentencing among execution (Donohue & Wolfers, 2005; Kovandzic et al., 2009; NRC, 2012), most, if not all, studies relying on either probability or frequency fail to test the initial decision to file for death penalty trial. Moreover, considering the variable effect that legal status may have when analyzing state differences, especially in states with active moratoria, this measure provides an initial estimate for the objective enforcement capital sentencing. Additionally, when supplemented with a measure of death sentences, this provides both a comparison and indication of an increased risk of subsequent punishment. Following the wider trend of mixed findings within the literature, however, I submit that if any relationship is identified, it will require further review to assess validity.

Hypothesis 1 (H1): Prosecutorial filings will decrease the number of county-level homicides from 2000 – 2010

Hypothesis 2 (H2): Death sentences will decrease the number of county-level homicides from 2000 - 2010

Chapter 3

Methods

Dataset

The present study relies on secondary data collected by Jeffery Ulmer, John Kramer, and Gary Zajac for a pair of studies published in 2020 measuring racial and geographic disparities in death penalty decision-making and representation across Pennsylvania (Ulmer et al., 2020a, 2020b). The data consists of 4274 cases (N = 4274) from 2000 to 2010 consisting of defendants charged under Pennsylvania's general homicide statute (18 Pa.C.S. §2502)[3] across all 67 counties across the commonwealth. Cases were made available to these authors from the Administrative Office of Pennsylvania Courts (AOPC) and supplemental data from the Pennsylvania Commission on Sentencing (PCS), and includes information on decisions regarding capital sentencing for death eligible defendants, including case filings, retractions, and sentences. For the purpose of this study, cases were included if they indicated a prosecutorial decision to filing for the death penalty (n = 495) under Pennsylvania's sentencing procedure for case charged under first degree or criminal homicide (42 Pa.C.S § 9711(a))[4], as well as whether death sentences were actually imposed (n = 145). Additionally, each case was initially binarily coded by the authors within their articles; to allow for consistent analyses, case data was aggregated to the county-level for the present study.

---

[3] 18 Pa.C.S. § 5202: (a) Murder of the first degree; (b) Murder of the second degree; (c); murder of the third degree.

[4] 42 Pa.C.S. § 9711(a)(1): After a verdict of murder of the first degree is recorded and before the jury is discharged, the court shall conduct a separate sentencing hearing in which the jury shall determine whether the defendant shall be sentenced to death or life imprisonment.

County-level homicide and demographic data were collected from the Federal Bureau of Investigation's Uniform Crime Report (UCR) program and U.S. Census Bureau, respectively. UCR data was accessed through the Inter-University Consortium for Political and Social Research (ICPSR) database, which provides crime rates and count estimates for Part I and Part 2 offenses in jurisdictions with agencies participating in UCR reporting.[5] Importantly, all homicides, regardless of offense level (e.g., first-degree, second-degree, etc.) or victim-defendant relationship were considered to maximize the external validity of the sample. As a note on homicide data generally, homicide data within the capital punishment literature is typically gathered from several sources depending on the purpose of the study and level analysis. These include the UCR and the CDC's WONDER database. UCR data were ultimately selected over the latter database. CDC morality data was primarily discounted due to data confidentiality[6]; when using the WONDER database, county-specific homicide trends become suppressed when homicide counts fall below ten in a given county for a single year. While this may seem of little consequence in areas with high population density, data is lost for less populated counties, especially when considering the general rarity of homicide as an event (Pridemore, 2005). Control data for county-level population, unemployment counts, and poverty counts were taken from the 2000 and 2010 decennial censuses for Pennsylvania. References to data sources can be found in Appendix C.

---

[5] Part I offenses: murder, rape, robbery, aggravated assaults, burglary, larceny, auto theft, and arson; Part II offenses: forgery, fraud, embezzlement, vandalism, weapons, violations, sex offenses, drug and alcohol abuse violations, gambling, vagrancy, curfew violations, and runaways (at the time publication).

[6] See *CDC WONDER: Compressed Mortality File, Data Release Questions:* For deaths in 1999 and later years, the term "Suppressed" replaces death counts, births counts, death rates and associated confidence intervals and standard errors, as well as corresponding population figures, when the figure represents one to nine (1-9) persons.

Variables and Conceptual Model

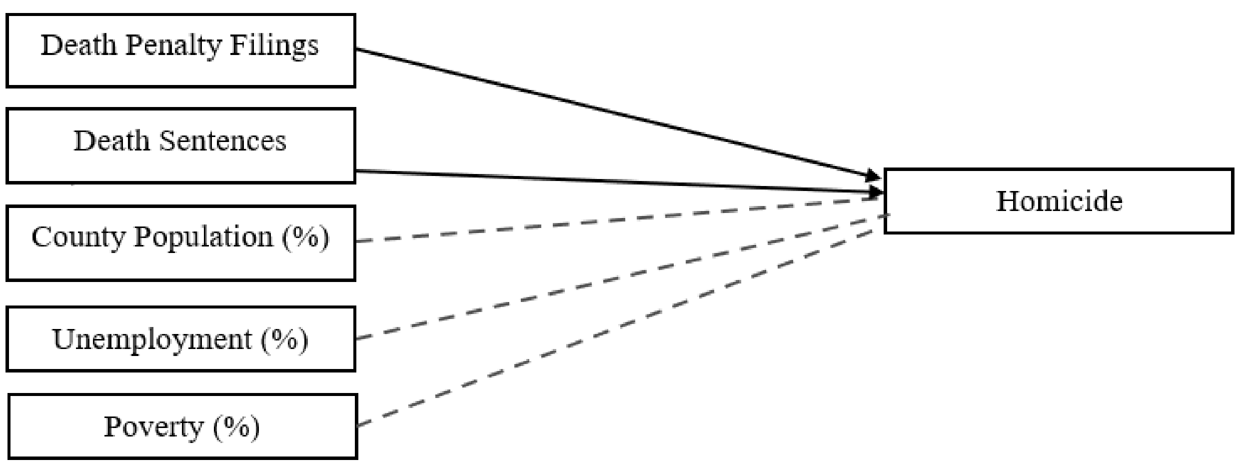
The key variables of interest are death penalty filings, death sentences received in court, and the count of homicides from 2000 to 2010 across the commonwealth of Pennsylvania, all aggregated at the county-level. As noted earlier, the present study largely departs from prior literature in its usage both capital filings and sentences, as opposed to actual executions or the perception of death penalty statutes (NRC, 2012). This was done for two reasons. First and foremost, as previously highlighted, the actual occurrence of executions remains low relative to those sentenced to death row. Across the twenty-seven states currently with a death penalty statute, only five (Texas, Oklahoma, Florida, Missouri, and Alabama) have predominately executed since 1976, with most occurring in a handful of counties in Texas (BJS, 2022; DPIC. 2025d, 2025e).. Put simply, most states do not execute defendants on death row; such is the case with Pennsylvania. In particular, the last execution within the state, involving one Gary M. Heidnick[7], occurred in 1999, well over two decades ago. Moreover, with the imposition of a state moratorium in 2015 by Governor Tom Wolf (ABA, 2015) and a continued decline in homicide (FBI, 2025), the absence of executions within the state would not be conducive to inferring any relationship with this measure.

Secondly, while the use of statues presents an alternative amid sentencing and execution declines, especially for states with capital moratoriums (Oliphant, 2022), the substitution of the legal status as a predictor of deterrence, as noted previously, relies on the assumption that a capital statute is so effective as to maximize the perceived likelihood of punishment, something historically contingent on state-by-state execution and enforcement trends (Kovandzic et al.,

[7] See *Com. V. Heidnick* 526 Pa. 458 (1991).

2009; Shepherd, 2006). Moreover, in the context of the current study, the level of analysis presented is at the county-level, which enables a more precise comparison of county differences as opposed to utilizing the state as a single unit of analysis (i.e., a state-level analysis of Pennsylvania). Within Pennsylvania, death penalty enforcement particularly occurs at the county-level, so this also provides a basis to account for within-state geographical differences (Ulmer et al., 2020a). Additionally, studies of deterrence have previously, albeit rarely, relied on county-level analyses (Dezhbakhsh et al., 2003), so such a project as the present thesis is not unprecedented.

Figure 1. Conceptual Model



Note. Controls denoted by dashes.

A conceptual representation of a prospective relationship among the variables is presented in *Figure 1*. Additionally, aggregated counts for each dependent and independent variable can be found in *Table 1*. Naturally, for the forgoing analyses, there are two independent variables, prosecutorial filings to seek the death penalty (n = 495) and the actual imposition of a death sentence (n = 145) among the 4724 cases gathered by Ulmer et al. (2020a, 2020b). As stated within the initial description of the dataset, cases charged under first degree homicides or criminal homicide are eligible for death filings in Pennsylvania. Generally, in the case of capital punishment, death filings require a two-phase trial, the second determining life imprisonment or death (42 Pa.C.S § 9711(a)). The discretion for prosecutors to seek the death penalty is contingent on the evidence of one or more aggravating factors as required by law (42 Pa.C.S § 9711(d))[8] in conjunction with any mitigating circumstances found present at the punishment phase of a trial (42 Pa.C.S § 9711(e))[9]. However, it should also be noted that not all cases involving affirmative decisions to file for the death penalty ultimately are convicted as first-degree homicides, nor are such decisions always maintained throughout an entire case. Rather, prosecutors hold similar discretion to retract filings for death prior to the punishment phase of a trial; absent these retractions, a case may also be convicted below the first degree at the first trail phase, ultimate requiring the withdrawal of a death filing (Ulmer et al., 2020a, 2020b). In terms of death sentencing, this represents the number of cases where a death sentence was imposed by either a jury or judge.

County homicide counts from 2000 to 2010 are the dependent variable for this study. Two concerns initially arise regarding this choice, namely the use of the county-level as the as the level of analysis as well as the choice of absolute counts over rate per 100,000 people (Kovandzic et al., 2009; Pridemore, 2005). Regarding the former, issues over the reliability and validity of county-level crime reporting have been noted by several previous authors (Allison & Harris, 2018; Pridemore, 2005; Singleton et al., 2022). Of most relevance, Pridemore (2005)

[8] 42 Pa.C.S. § 9711(d): there are a total of 18 aggravating factors available.
[9] 42 Pa.C.S. § 9711(e): there are a total of 8 mitigating circumstances available.

warns that by the voluntary nature of UCR data reporting as well as the infrequency of homicides, the disaggregation of data to a particular county in a single year presents a risk of biasing any subsequent analysis, especially when considering the spatial variation of crime generally (Singleton et al., 2022). Regarding the latter, it is best practice for crime reporting, particularly within deterrence studies, to be done at a scale of per 100,000 people; such is the case of all, if not the majority of capital deterrence literature (Donohue & Wolfers, 2005; Kovandzic et al., 2009; NRC, 2012; Oliphant, 2022; Parker, 2021). While these points are of value, I submit that the use of a county-level analysis provides a significant remedy for addressing concerns over geographic differences over time. Additionally, as the focus of this study is a single state, the use of count-level data provides a bounty of data points to provide an overall representation of the state (Donohue, 2014). As for homicide rates, because of the nature of the analyses (i.e., cross-sectional and time-series regressions), I opted for two points in time to compare change in homicide trends (see analytical framework). As such, I found it most appropriate to provide raw counts for each year as opposed to generalizing homicide rates for years missing population data. Year-to-year frequencies for each county, as well as county-level homicide means and standard deviations can be located within Appendix A.

Lastly, to allow for a comparison within each regression, population, percent unemployment, and poverty percent were selected as control variables. As mentioned prior, population density has previously been used as a reliable indication of crime counts and rates (notwithstanding other socioeconomic mediators and confounders; Pridemore, 2005). By proximity, population count provides a proxy-measure for density. The percents of those unemployed and under the poverty line were also measured as socioeconomic predictors of homicide (Kovandzic et al., 2009). Descriptives for the controls for both time points can be located in *Table 2* and *Table 3*, respectively.

Table 1. Descriptive Statistics, Independent and Dependent Variables, Aggregated

County	Homicides	(%)	Filings	(%)	Sentences	(%)
Adams	21	(.28)	2	(.40)	1	(.68)
Allegheny	894	(12.16)	11	(2.22)	4	(2.75)
Armstrong	14	(.19)	3	(.60)	1	(.68)
Beaver	59	(.80)	0	(.00)	0	(.00)
Bedford	14	(.19)	2	(.40)	1	(.68)
Berks	208	(2.83	24	(4.84)	12	(8.27)
Blair	32	(.43)	8	(1.61)	4	(2.75)
Bradford	21	(.28)	4	(.80)	2	(1.37)
Bucks	82	(1.11)	9	(1.81)	1	(.68)
Butler	19	(.25)	2	(.40)	0	(.00)
Cambria	41	(.55)	4	(.80)	0	(.00)
Cameron	0	(.00)	0	(.00)	0	(.00)
Carbon	9	(.12)	2	(.40)	0	(.00)
Centre	23	(.31)	1	(.20)	0	(.00)
Chester	61	(.83)	11	(2.22)	10	(6.89)
Clarion	9	(.12)	0	(.00)	0	(.00)
Clearfield	8	(.10)	2	(.40)	0	(.00)
Clinton	10	(.13)	2	(.40)	2	(1.37)
Columbia	7	(.09)	1	(.20)	1	(.68)
Crawford	19	(.25)	2	(.40)	0	(.00)
Cumberland	28	(.38)	11	(2.22)	4	(2.75)
Dauphin	162	(2.20)	13	(2.62)	2	(1.37)
Delaware	318	(4.32)	10	(2.02)	8	(5.51)
Elk	3	(.04)	0	(.00)	0	(.00)
Erie	76	(1.03)	5	(1.01)	2	(1.37)
Fayette	46	(.62)	3	(0.60)	1	(.68)
Forest	0	(.00)	0	(.00)	0	(.00)
Franklin	29	(.39)	3	(.60)	1	(.68)
Fulton	5	(.06)	0	(.00)	0	(.00)
Greene	12	(.16)	1	(.20)	1	(.68)
Huntingdon	19	(.25)	2	(.40)	1	(.68)
Indiana	19	(.25)	1	(.20)	1	(.68)
Jefferson	19	(.25)	4	(.80)	0	(.00)
Juniata	6	(.08)	1	(.20)	1	(.68)

Table 1. *Continued*

County	Homicides (%)		Filings (%)		Sentences (%)	
Lackawanna	33	(.44)	7	(1.41)	1	(.68)
Lancaster	118	(1.60)	12	(2.42)	5	(3.44)
Lawrence	37	(.50)	4	(.80)	1	(.68)
Lebanon	34	(.46)	1	(.20)	1	(.68)
Lehigh	178	(2.42)	26	(5.25)	11	(7.58)
Luzerne	150	(2.04)	1	(.20)	2	(1.37)
Lycoming	20	(.27)	4	(.80)	0	(.00)
McKean	7	(.09)	3	(.60)	0	(.00)
Mercer	19	(.25)	0	(.00)	0	(.00)
Mifflin	9	(.12)	0	(.00)	0	(.00)
Monroe	51	(.69)	11	(2.22)	5	(3.44)
Montgomery	159	(2.16)	14	(2.82)	4	(2.75)
Montour	1	(.01)	0	(.00)	0	(.00)
Northampton	74	(1.00)	10	(2.02)	5	(3.44)
Northumberland	18	(.24)	9	(1.81)	4	(2.75)
Perry	15	(.20)	0	(.00)	0	(.00)
Philadelphia	3710	(50.48)	208	(42.02)	31	(21.37)
Pike	14	(.19)	4	(.80)	0	(.00)
Potter	5	(.06)	0	(.00)	0	(.00)
Schuylkill	27	(.36)	4	(.80)	1	(.68)
Snyder	8	(.10)	0	(.00)	0	(.00)
Somerset	62	(.84)	0	(.00)	0	(.00)
Sullivan	1	(.01)	1	(.20)	1	(.68)
Susquehanna	7	(.09)	0	(.00)	0	(.00)
Tioga	2	(.02)	0	(.00)	0	(.00)
Union	4	(.05)	1	(.20)	1	(.68)
Venango	10	(.13)	0	(.00)	0	(.00)
Warren	14	(.19)	2	(.40)	0	(.00)
Washington	54	(.73)	10	(2.02)	3	(2.06)
Wayne	13	(.17)	0	(.00)	0	(.00)
Westmoreland	65	(.88)	3	(.60)	3	(2.06)
Wyoming	8	(.10)	1	(.20)	0	(.00)
York	129	(1.75)	15	(3.03)	5	(3.44)

Table 2. Descriptive Statistics, Control Variables, 2000

County	Population	(%)	Unemployment	(%)	Poverty (%)
Adams	91292	0.74	2042	2.9	7.1
Allegheny	1281666	10.44	38388	3.7	11.2
Armstrong	72392	0.59	1996	3.4	11.7
Beaver	181412	1.48	4667	3.2	9.4
Bedford	49984	0.41	1359	3.4	10.3
Berks	373638	3.04	9671	3.3	9.4
Blair	129144	1.05	3833	3.7	12.6
Bradford	62761	0.51	1640	3.4	11.8
Bucks	597635	4.87	11128	2.4	4.5
Butler	174083	1.42	3812	2.8	9.1
Cambria	152598	1.24	5889	4.7	12.5
Cameron	5974	0.05	172	3.7	9.4
Carbon	58802	0.48	1536	3.3	9.5
Centre	135758	1.11	3743	3.3	18.8
Chester	433501	3.53	8214	2.5	5.2
Clarion	41765	0.34	1279	3.8	15.4
Clearfield	83382	0.68	2634	3.9	12.5
Clinton	37914	0.31	1042	3.4	14.2
Columbia	64151	0.52	2370	4.5	13.1
Crawford	90366	0.74	2502	3.5	12.8
Cumberland	213674	1.74	3503	2	6.6
Dauphin	251798	2.05	5806	2.9	9.7
Delaware	550864	4.49	13310	3.1	8
Elk	35112	0.29	796	2.9	7
Erie	280843	2.29	8012	3.7	12
Fayette	148644	1.21	5318	4.5	18
Forest	4946	0.04	139	3.5	16.4
Franklin	129313	1.05	2385	2.3	7.6
Fulton	14261	0.12	273	2.5	10.8
Greene	40672	0.33	1545	4.7	15.9
Huntingdon	45586	0.37	1126	3	11.3
Indiana	89605	0.73	3353	4.6	17.3
Jefferson	45932	0.37	1417	3.9	11.8
Juniata	22821	0.19	390	2.2	9.5
Lackawanna	213295	1.74	5442	3.2	10.6
Lancaster	470658	3.83	7329	2	7.8
Lawrence	94643	0.77	2680	3.6	12.1
Lebanon	120327	0.98	2479	2.6	7.5
Lehigh	312090	2.54	6997	2.8	9.3

Table 2. *Continued*

County	Population	(%)	Unemployment	(%)	Poverty (%)
Luzerne	319250	2.6	8256	3.2	11.1
Lycoming	120044	0.98	3701	3.9	11.5
McKean	45936	0.37	1289	3.5	13.1
Mercer	120293	0.98	3353	3.5	11.5
Mifflin	46486	0.38	877	2.4	12.5
Monroe	138687	1.13	4467	4.2	9
Montgomery	750097	6.11	17965	3.1	4.4
Montour	18236	0.15	639	4.4	8.7
Northampton	267066	2.17	6196	2.9	7.9
Northumberland	94556	0.77	2282	3	11.9
Perry	43602	0.36	847	2.5	7.7
Philadelphia	1517550	12.36	71582	6.1	22.9
Pike	46302	0.38	1117	3.2	6.9
Potter	18080	0.15	517	3.7	12.7
Schuylkill	150336	1.22	3987	3.2	9.5
Snyder	37546	0.31	701	2.4	9.9
Somerset	80023	0.65	2094	3.2	11.8
Sullivan	6556	0.05	267	4.9	14.5
Susquehanna	42238	0.34	860	2.6	12.3
Tioga	41373	0.34	1179	3.6	13.5
Union	41624	0.34	663	1.9	8.8
Venango	57565	0.47	1899	4.2	13.4
Warren	43863	0.36	1096	3.2	9.9
Washington	202897	1.65	5038	3.1	9.8
Wayne	47722	0.39	1226	3.3	11.3
Westmoreland	369993	3.01	9106	3.1	8.6
Wyoming	28080	0.23	664	3	10.2
York	381751	3.11	7301	2.4	6.7

Table 3. Descriptive Statistics, Control Variables, 2010

County	Population	(%)	Unemployment	(%)	Poverty (%)
Adams	101407	0.8	2693	3.3	7.6
Allegheny	1223348	9.63	43527	4.3	12.3
Armstrong	68941	0.54	2495	4.4	11.7
Beaver	170539	1.34	5528	3.9	11.1
Bedford	49762	0.39	1649	4.1	13.5
Berks	411442	3.24	16649	5.2	12.4
Blair	127089	1	4446	4.3	12.9
Bradford	62622	0.49	1864	3.7	13.6
Bucks	625249	4.92	20111	4.1	4.9
Butler	183862	1.45	5570	3.8	8.3
Cambria	143679	1.13	5060	4.2	13.7
Cameron	5085	0.04	138	3.2	11.4
Carbon	65249	0.51	2625	5	10.5
Centre	153990	1.21	4634	3.6	18.5
Chester	498886	3.93	13288	3.5	6.2
Clarion	39988	0.31	1338	4	15.8
Clearfield	81642	0.64	3581	5.3	14.7
Clinton	39238	0.31	1328	4.2	15.5
Columbia	67295	0.53	1924	3.4	13.7
Crawford	88765	0.7	3533	4.9	15.8
Cumberland	235406	1.85	6045	3.2	6.5
Dauphin	268100	2.11	8695	4.1	11.9
Delaware	558979	4.4	21098	4.8	9.4
Elk	31946	0.25	983	3.7	11
Erie	280566	2.21	10856	4.9	15.6
Fayette	136606	1.08	5723	5	19.2
Forest	7716	0.06	214	3.2	11.7
Franklin	149618	1.18	4129	3.6	8.2
Fulton	14845	0.12	614	5.2	13.3
Greene	38686	0.3	1123	3.5	16.7
Huntingdon	45913	0.36	1358	3.6	11.4
Indiana	88880	0.7	3548	4.8	18.6
Jefferson	45200	0.36	1390	3.8	13.7
Juniata	24636	0.19	787	4.1	8.3
Lackawanna	214437	1.69	6980	4	13.2
Lancaster	519445	4.09	15057	3.8	9.7
Lawrence	91108	0.72	3376	4.5	12.7
Lebanon	133568	1.05	4154	4	8.9
Lehigh	349497	2.75	13473	5	11.9

Table 3. *Continued*

County	Population	(%)	Unemployment	(%)	Poverty (%)
Luzerne	320918	2.53	11040	4.2	13.7
Lycoming	116111	0.91	4672	4.9	14.4
McKean	43450	0.34	1813	5.1	13.9
Mercer	116638	0.92	4293	4.5	13.2
Mifflin	46682	0.37	1782	4.9	13.9
Monroe	169842	1.34	8767	6.6	10.4
Montgomery	799874	6.3	23160	3.7	5.6
Montour	18267	0.14	543	3.7	11
Northampton	297735	2.34	9910	4.2	8.8
Northumberland	94528	0.74	3419	4.4	14.9
Perry	45969	0.36	1502	4.1	9.1
Philadelphia	1526006	12.01	89513	7.5	25.1
Pike	57369	0.45	2673	5.9	8.7
Potter	17457	0.14	661	4.7	14.8
Schuylkill	148289	1.17	5305	4.3	11.9
Snyder	39702	0.31	1241	3.9	11.7
Somerset	77742	0.61	2693	4.2	12.9
Sullivan	6428	0.05	137	2.5	15.5
Susquehanna	43356	0.34	1521	4.3	11.3
Tioga	41981	0.33	1641	4.8	15.8
Union	44947	0.35	1162	3.1	12.6
Venango	54984	0.43	2118	4.8	15.7
Warren	41815	0.33	1255	3.7	12.2
Washington	207820	1.64	7122	4.2	10.4
Wayne	52822	0.42	1404	3.3	10.9
Westmoreland	365169	2.87	11038	3.7	9.8
Wyoming	28276	0.22	913	4	10.9
York	434972	3.42	14768	4.4	9

Analytical Framework

Following the practice of prior capital punishment research (Dezhbakhsh et al., 2003; Donohue & Wolfers, 2005; Kovandzic et al., 2009; NRC, 1978, 2012), regression models are used within the present study. Two common types are presented as the basis for drawing conclusions about the overall presence and temporal direction of any relationship between capital punishment and homicide: cross-sectional and time-series regressions. While cross-sectional regressions fall under the category of time-series analysis, the key difference is the ability of the former to provide a general overview of the relationship among variables. Cross-sectional regressions allow for an analysis of multiple units (in this case counties) over a wider period of time (Shepherd, 2005). Alternatively, time-series regressions are used to study the relationship of variables over a shorter time period, typically utilizing a smaller number of units (NRC, 2012). Regardless, both models can allow the use of homicide counts, and on that matter the counts of filings and sentences, as opposed to homicide rates and execution probability (largely a feature of panel studies, NRC, 2012).

To account for the two time periods where controls were gathered, cross-sectional and times series regressions will be presented within four separate models. Model 1 and Model 2 will measure death filings, with Model 1 accounting for the control measures in 2000 whereas Model 2 will contain controls for 2010. Similarly, Models 3 and 4 will account for death sentences, with each also measuring yearly differences for 2000 and 2010, respectively. Additionally, for the time-series regressions, the independent and dependent variables will be separated into two time-series groups, TS1 composing the years 2000-2005, and TS2 composing the years 2006-2010, which will be used to determine relationship directionality. That is, TSI predictors will be used to predict TS2 homicide trends. Lastly, in order to estimate causality following the time series regressions, change score regressions were conducted using the mean differences between TS2 and TS1, as well as the net population change between 2000 and 2010. Standardized coefficients are reported for the cross-sectional and time series regressions; complete models with standardized and unstandardized coefficients are provided in Appendix B.

Chapter 4

Results

Cross Sectional Regression

Standardized coefficients for Models 1 and 2 are presented in *Table 4*. Notably, for each predictor (excluding unemployment in each regression presented within this section), betas indicate statistically significant relationships, varying from strong to weak. Both models regarding death filings indicate rather high coefficients of determination (.968 and .963, respectively). This is similarly true for Models 3 and 4, both represented in *Table 5* (.848 and .836, respectively). In short, this suggests that almost all of the relationship with homicides among the sample is explained by each of the predictors. Having said that, and as a general note, for each cross-sectional and time-series regression, the model standard errors are also relatively high, ranging from 72.428 to 194.756. This may be an indication of either 1) outliers within the sample, which is likely considering the spatial relativity of homicides among population dense counties (i.e., Allegheny and Philadelphia); 2) the infrequency with which death sentences are

Table 4. Cross Sectional Regressions, Death Filings

	Standardized Betas	
Predictor	Model 1	Model 2
Death Penalty Filings	.759***	.781***
Population	.236***	.218***
Unemployment	.006	-.025
Poverty	.115**	.121***
Adjusted R Squared	.966	.960
Std. Error of Estimates	86.426	92.656

Note. * $p \leq .05$, ** $p \leq .01$, *** $p \leq .001$

Table 5. Cross Sectional Regression, Death Sentences

	Standardized Betas	
Predictor	Model 3	Model 4
Death Sentences	.482***	.445***
Population	.399***	.405***
Unemployment	.045	.048
Poverty	.274***	.297***
Adjusted R Squared	.838	.825
Std. Error of Estimates	187.115	194.756

Note. * p ≤ .05, ** p ≤ .01, *** p ≤ .001

imposed relative to initial decisions to file for the death penalty, which may likewise be unequally distributed based on county population; or 3) the fact that absolute counts are provided as opposed to standardized rates per 100,000 people.

Regarding the independent variables, for each standard deviation increase in death penalty filings, there is an associated .759 and .781 standard deviation increase, respectively, in the amount of homicides in a county for a given year. Both increases indicate a high degree of statistical significance ($p \leq .001$). Similarly, when analyzing the relationship with death sentences, the standardized coefficients indicate a similarly positive, yet moderate relationship; Model 3 indicates a .482 standard deviation increase in the number of homicides for each standard deviation increase in death sentences, whereas Model 4 indicates a .445 standard deviation increase. In each case, the independent variables yields a greater increase in standard deviations, and thus effect size accounting for homicides, relative to the controls. Translated into absolute, unstandardized units (see Appendix B), the regressions indicate the following: for models 1 and 2 respectively, for each additional death penalty filing in any county for a given year, there is an associated 13.7 and 14.1 extra homicides. Likewise, for Models 3 and 4, for each additional death sentence, there is a statistically significant increase in the number of homicides by 50 and 46.1, respectively.

Regarding the controls, in all four models, population count and poverty percentage indicate weaker relationships to homicide counts relative to the effect of both capital filings and sentences. That said, both maintain a statistically significant moderate to weak relationship with homicide, with population below the .001 level in all models, whereas poverty sits at the .002 in Model 1, and below .001 in all other models. Conversely, in all models, unemployment does not indicate any statistical significance with homicide, with the relationship in all models being relatively nominal.

As of current, these results could largely indicate the presence of a brutalization effect among the studied counties and time period. However, this alone is insufficient to indicate any directionality within the relationships; as the word “associated” indicates, the likelihood that death filings and sentences lead to an increase in homicides cannot be separated from the equally likely probability that the number of homicides in a given county and year may in fact increase enforcement trends of death filings and sentences (NRC, 2012). It is with this that I turn to the time-series models

Time-Series Regression

Coefficients for death filings in TS1 are displayed in *Table 6*. Similar to the models in the cross-sectional regressions, both Models 1 and 2 provide a strong indication of model predictive power. In Model 1, 89% of the relationship is predicted by each variable, whereas 88% of the relationship is predicted in Model 2. Additionally, model standard error is lower, albeit marginally, than the previous regressions, sitting between 72 and 76. In both cases, death filings provide a stronger relationship to homicides compared to the controls. Particularly, for each standard deviation increase for filings, there is a .622 and .635 standard deviation increase in homicides for both models respectively, statistically significant below the .001 level. Compared to the cross-sectional regressions, however, these values would also indicate an overall decrease in the strength of this relationship. That said, Models 1 and 2 interpreted alone further indicates a brutalization effect among death filings (TS1) within the sample, at least to the extent for homicides committed within TS2. When looking closer at the controls, the strength of the relationships to the dependent variable have marginally increased relative to the cross-sectional regression, with a standard deviation increase in population specifically associated

Table 6. Time-Series, Death Filings

	Standardized Betas	
Predictor	Model 1	Model 2
Death Filings	.622***	.635***
Population	.303***	.288***
Unemployment	.034	-.012
Poverty	.194**	.216***
Adjusted R Squared	.892	.880
Std. Error of Estimate	72.428	76.318

Note. * $p \leq .05$, ** $p \leq .01$, *** $p \leq .001$

Table 7. Times-Series, Death Sentences

Predictor	Standardized Betas Model 3	Model 4
Death Sentences	-.057	-.127
Population	.788***	.774***
Unemployment	.076	.164*
Poverty	.314***	.319***
Adjusted R Squared	.745	.769
Std. Error of Estimate	111.072	105.827

Note. * $p \leq .05$, ** $p \leq .01$, *** $p \leq .001$

with a .303 and .288 standard deviation increase in homicide. Statistical significance for each control remains identical for both models between each regression.

Death sentence coefficients for TS1 are presented in *Table 7*. Interestingly, compared to each prior regression and model, death sentences for time-series Models 3 and 4 provide weak and negative relationships with homicide, at -.057 and -.127 respectively. Additionally, both models lack statistical significance for this measure, suggesting the absence of a substantial relationship with this variable. On the other hand, the strength of controls is noticeably higher compared to the sentencing variable. Population, in particular, has the highest coefficient value in both models, .788 and .774 respectively ($p \leq .001$), indicating that county population better predict increases in homicide relative to the death sentences variable. Further, in both models, poverty exerts a stronger and more significant relationship than sentences (.314 and .319, respectively). Lastly, in Model 4, unemployment has a weak yet statistically significant relationship with homicides ($p \leq .05$); that is, for every standard deviation increase in poverty, there is a .164 standard deviation increase in homicides for TS2. However, given the strength of this predictor to other variables, especially poverty, it is not unlucky that the strength of this variable to homicides is mediated by a spurious relationship.

Change-Score Comparison

The unstandardized and standardized coefficients are presented for each change-score regression. Concerning the effect of change in fillings (see *Table 8)*, the comparison indicates a strong relationship with filings when controlling for wider county population changes. That is, when assessing the change in homicide count trends between both time series groups (i.e., TS2 – TS1) there is a statistically significant ($p \leq .001$) decline in homicides for change in filings; for each additional standard deviation increase in the independent variable, the is a .850 standard deviation decline in homicides. In raw units, for each additional increase in the change in filing (increase in filings from between TS1 to TS2), the regression suggests that an average of 2 homicides are prevented. Alternatively, the effect from net population change is miniscule according to the model (standardized and unstandardized betas at .024 and <.000, respectively. Regardless, the data presented within Table 8 indicate a statistically strong deterrence effect (as shown by via the effect size .725), but practically small in raw units (holding net population constant).

Similarly, *Table 9* suggests a significant deterrence effect for each additional positive change in sentences between TS1 to TS2 (a net increase in death sentences suggest an average

Table 8. Change Score, Death Filings

Predictor	Unstandardized Coefficients B	Std. Error	Standardized Beta
Filings	-2.410	.197	-.850***
Net Population	4.176E-5	<.000	.024

R^2 = .725
Adjusted R Squared = .715
Std. Error of Estimate = 17.201

Note. * $p \leq .05$, ** $p \leq .01$, *** $p \leq .001$

Table 9. Change Score, Death Sentences

Predictor	Unstandardized Coefficients B	Std. Error	Standardized Beta
Sentences	-6.239	.814	-.712***
Net Population	5.042E-5	<.000	.029
R^2 = .508			
Adjusted R Squared = .491			
Std. Error of Estimate = 22.990			

Note. * p ≤ .05, ** p ≤ .01, *** p ≤ .001

of 6 fewer homicides between each time period with a higher standard error at .814). Admittedly, the strength of death sentences within the model is lower (-.712) relative to the prior filings model (-.850, see *Table 8*), notwithstanding the strength of the overall model (R^2 = .508). That said, the statistical significance of the predictors within both regressions are similar, with sentences below .001 and net population change yielding no significance (p = .754). Focusing further on the latter, the unstandardized coefficient and standard errors are also fairly small (<.000 for both). Regardless, these data provide rather conflicting evidence for relationships with both capital filings and sentences. For filings, the time-series regressions (see *Table 6*) suggests a brutalization effect (with roughly 17 additional homicides in TS2 for each additional death filing in TS1), whereas the change-score comparison (see *Table 8*) provided evidence of deterrence (2 homicides prevented). Conversely, the relationship with sentences has flowed between brutalization, null, and deterrence within the cross-sectional (see *Table 5*), time-series (see *Table 7*), and change-score (see *Table 9*) models, respectively. In short, while the filing models provide support for a relationship (deterrence or brutalization), sentence models indicate less certainty for any clear relationship.

Chapter 5

Discussion

The initial intent of this study was to evaluate the presence of any relationship between the capital punishment and county-level homicides by using prosecutorial death filings and death sentences as independent predictors. Given the placement of this study within Pennsylvania, and thus the lack of readily available recent execution data, these measures were seen as contemporary, more generalizable proxy-measures. Restating the hypotheses, both were suspecting the presence of a negative relationship between the independent and dependent variables (i.e., an increase in filings (H1) or sentences (H2) will decrease county-level homicide between 2000 – 2010). Following the analysis, the present study indicates not only mixed support for either hypothesis, but a debatable lack of certainty for any relationship present for capital sentences as they are structured within this analysis (H2). As such, the hypotheses may be thought of as follows:

H1: Prosecutorial filings will decrease the number of county-level homicides from 2000 – 2010 (**Mixed support**)

H2: Death sentences will decrease the number of county-level homicides from 2000 – 2010 (**Mixed Support**)

Beginning with capital filings, the initial cross-sectional regressions indicated a strong and statistically significant positive relationship with homicides when accounting for population, unemployment, and poverty (β = .759 and .781, respectively). This result could suggest the presence of a relationship based on the brutalization hypothesis, in that each additional capital filing creates a risk of brutalizing the defendant subject to them, thus potentially leading to an additional 13-14 homicides. As noted previously, however, this result alone would not be adequate to determine the direction of this relationship; just as it is possible to infer brutalization, it is equally reasonable to suggest that an increase in homicides, particularly within more populace, socioeconomically disadvantaged counties, may be associated with an increase in use of death filings by prosecutors. Such could be explained the wider presence of a "tough-on-crime" sanction regime, with prosecutors more inclined to enforce the upper limits of legal penalties (Chalfin & McCrary, 2017). Regardless, directionality and time-order are needed to substantiate any conclusion about a causal relationship. This subsequently leads to the time-series regressions. In both cases, Models 1 and 2 illustrated a similarly positive relationship, with a relatively strong and significant effect size (β = .622 and .635, respectively). Accordingly, these regressions would indicate an additional 17 homicides between 2006 to 2010 per capital filing prior to 2006, evident supporting a brutalization relationship. However, when change scores were subsequently calculated, the death filing model in *Table 8* provided a strong and significant negative relationship. Specifically, it indicated a deterrent relationship with a stronger beta and comparable model effect (.850 and .725, respectively) relative to the prior regressions, finding 2 homicides prevented by positive changes in filings over time. Taken together, each regression suggests that a relationship can be identified for capital filings, yet the specific type of relationship (i.e., deterrence or brutalization) cannot be firmly established within the current models, especially following the latter two analyses.

When turning toward capital sentences, the relationship becomes even less clear. While cross-sectional Models 3 and 4 provide moderate support for significant, positive correlation with homicide ($\beta = .482$ and $.445$, respectively), suggesting an average increase of 46-50 homicides, the time-series regressions show weak evidence of a negative relationship. Specifically, compared to the prior models, time-series Models 3 and 4 (see *Table 7*) show fairly weak and statistically insignificant negative effects from death sentences ($\beta = -.057$ and $-.127$). While not illustrated in the current study, a supplementary time-series analysis was also conducted to assess if the lost statistical power could be attributed to a relationship within the opposite direction; in doing so, standardized measures indicated significant coefficients over 1 (between 1.1 and 1.024). While the use of a multiple regression model allows for coefficients to exceed +/- 1, this does indicate a sign of multicollinearity[10] within this alternative model, thus affecting its reliability (Mori, 2020). While further investigation of this issue exceeds the scope of this project, future studies should consider the presence and possible implications of such a relationship (see implications below). Regardless, despite the overall finding of a negative relationship within time-series Models 3 and 4, the mere absence of statistical power suggests that such a finding is likely the product of chance. Indeed, as noted within the results, the population and poverty measures, and even unemployment to a lesser extent, were stronger predictors for county homicides. This null finding, however, is of larger concern when acknowledging the results of the subsequent change-score model. As noted, both filings and

[10] Multicollinearity may be broadly defined as an instance within a multiple regression model where a given independent variable shares a high statistical correlation with one or more independent or other (control) predictors, affecting the overall reliability of a model in predicting the effect of a given independent variable.

sentences show evidence of a significant deterrent relationship within the regression, with sentences indicating high predictive power of a negative relationship (-.712) despite a moderate model effect (R^2 = .508). Considering the variability of the relationships displayed in each model, I submit that the overall state of a relationship between death sentences and county-level homicides is inconclusive.

In summary, neither capital filings nor sentences indicated any firm evidence of a single relationship with homicide. Rather, these results suggested the potential for multiple conflicting relationships, with sentences in particular showing null findings. These findings follow the general consensus, or more accurately the lack thereof, among capital deterrence scholars. Indeed, it is not necessarily unprecedented to yield a mix of relationships depending on the given model; as mentioned at the onset, many authors have conducted original tests or replicated prior studies only to find their results largely dependent on the techniques or data used (Donohue & Wolfers, 2005; Kovandzic et al., 2009; NRC, 2012; Shepherd, 2005). When reviewing inconsistencies among prior capital deterrence studies, Donohue and Wolfers chiefly note that "the existing evidence for deterrence is surprisingly fragile, and even small changes in specifications yield dramatically different results…we remain unsure even of whether they are positive or negative [effects] " (2005, p. 794). That said, this study was not without promise. While the sentencing variable followed prior literature indicating the lack of any firm relationship associated with measures of sentencing (Dezhbakhsh et al., 2003 Donohue & Wolfers, 2005), this is the first study, to my knowledge, to test capital filings as a measure for any relationship with homicide, let alone find any effect (see implications).

Limitations

Naturally, this study was conducted within a subsection of literature rife with error, uncertainty, and confounding conclusions. This is noted not to discredit the contributions of prior authors, but to contextualize where possible shortcomings may appear within the current project, especially those that have been previously identified. The most evident of these is the lack of any thoroughly defined or substantiated theoretical basis for predicting a relationship between the chosen independents and homicide counts. While deterrence literature, especially the subsection of capital deterrence studies, relies in part on the fundamental principles thought to guide rational decision making between crime and punishment (i.e., certainty, swiftness, and severity; Beccaria, 1764; Bailey, 1980; Becker, 1968; Chalfin & McCrary, 2017), the current study makes no effort to explain nor significantly demonstrate how prospective murders would consider prosecutorial filings or death sentences within their choice calculus. Though it can be suggested that the risk of being subject to capital trial proceedings, and thus risk closer exposure to a death sentence, could influence the likelihood of future homicides, such an assertion is met with further theoretical and practical barriers.

As noted within the initial theoretical review, capital deterrence studies, regardless of independent variable, suffer from the assumption that rational-choice models, specifically regarding perceptions of punishment risk, correspond directly with objective calculatable risks (Chalfin et al., 2012; Donohue & Wolfers, 2005; Nagin, 1998, 2013; NRC, 2012). This remains absent without illustrating how exactly a prospective murder may consider each perceived factor that influences the likelihood of facing execution, notwithstanding between-state variability. In the context of the current study, the use of filings or sentences, regardless of controls, makes a similar assumption, in that homicide defendants are presumed to weigh the frequency with which capital trials are sought and subsequently sentenced. This, however, leads to a practical matter, namely the disparities and infrequency of active capital sanction regimes. Assuming these measures would influence decision-making, the rarity with which they are enforced would undermine any significant relationship they have with homicide (Cook, 2009; Katz et al., 2003).

Similarly absent from this study is the use of a larger formula utilizing cost function as a predictor of decision-making for capital homicide, something often found within panel and time-series designs. While concerns have been expressed about the validity of these functions with respect to the variable being weighed (e.g., risk of execution, frequency of execution, presence of capital punishment; Kovandzic et al., 2009; NRC, 2012), they can still provide a useful baseline for testing decision-making models in lieu of founded explanations of risk perceptions. Importantly, wider models provide a robust medium for understanding potential influences within decision-making. In the context of this study, however, no such robust model is used. Specifically, given that the regression models used were fairly rudimentary, no particular formula was established to effectively account for unobserved errors, or county-level/yearly changes within the frequency of filings and sentences. Whule, two different types of regressions were conducted to account for such effects between two points within the eleven-year period, the current models cannot account for year-to-year differences. Likewise, the selected controls, while used within prior studies, are not sufficient within the current study as data is largely lacking for each year between 2000 and 2010 (NRC, 2012). A subsequent study would need to address this to adequately account for the effect of these variables.

Lastly, as a concurrent note on data measurement, a point of caution requires acknowledgement. As mentioned within the methodology, most of the data used through this study were unstandardized, raw counts, particularly in the case of homicides. It is possible that the presence of homicide counts, as opposed to standard rate per 100,000 people, increased the risk of possible outliers within the data and thus the presence of bias within the wider analyses. Such was made apparent by the inclusion of Philadelphia County. As shown within *Table A1* (see Appendix A), homicide counts within Philadelphia County far exceeded counts from all other county aggregates combined. In other words, a single county (1.49% of the sample) comprises 3,710 homicides (50.48% of the dependent variable). This is fairly concerning with respect the analyses and essentially indicates that most, if not a majority, of the results identified are highly expressive of Philadelphia County. While the use of a standard rate would not have outright eliminated this disparity, it would have at least allowed for a much more amenable comparison of county-level inputs on the aggregate relationship between variables.

Implications for Further Inquiry

I provide two key points of interest that may be of use to future studies. First and foremost, it is beyond imperative that future projects regarding capital deterrence make an empirically-grounded attempt to base its predictions within a valid rational decision-making model as opposed to ad hoc assumptions. With the lack of an established theoretical model privy to the contemporary capital sanctioning regime (NRC, 2012), further contributions to this research risk perpetuating decades-old shortcomings in predictive capability. Additionally, while recent authors have been retrofitting statistical techniques to address similar problems regarding the applicability of this literature between states with and without capital moratoria (Oliphant, 2022; Parker, 2021), there remains the issue of having to rely on assumptions to these quires. As such, it may prove beneficial to conduct a series of mixed-method studies identifying and testing themes related to common factors considered when weighing the risks associated with homicide.

Second, as noted at the beginning of this chapter, the use of prosecutorial decision-making as a factor that influences the relationship with capital punishment warrants further analysis. While, again, no clear theoretical basis was provided for the use of capital filings in this study, it may be worth evaluating how such a measure might impact criminal decision-making for capital sanction regimes. As others have identified, prosecutorial discretion plays a central role within the course of determining criminal punishment (Peterson & Lynch, 2012; Ulmer et al., 2020b). As such, studies relying on either the perception or influence of court actors within the overall deterrence formula could benefit from considering how the frequency of filings may shape sentencing trends and thus perceptions of capital deterrence. Likewise, for alternative regression models, such a measure could be used as a court-influence covariate.

Conclusion

In closing, the current debate about the utility provided through capital punishment remains of focus for scholars, policymakers, practitioners, and the public writ-large. And while the current study provides minimal assistance to guide the wider conversation, it does provide a potential avenue by which capital punishment may alternatively be associated with homicide, if not through a matter of chance alone. Within Pennsylvania specifically, a state continuing its ten-year moratorium on executions, it could prove useful to reassess how the human-impact of this punishment is measured, if not at least by a relative proxy. Though considering the general state of this literature, I believe that this study only further indicates the need to ground any proposed and tested measure with the confines of theory.

Appendix A: County Homicide 2000 - 2010

Table A1. *Homicide by County 2000-2010*

County	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	Mean (SD)
Adams	1	2	3	1	1	1	3	0	3	2	4	1.91 (1.22)
Allegheny	61	76	74	96	73	84	84	78	104	80	84	81.27 (11.52)
Armstrong	1	0	0	2	3	6	1	0	0	1	0	1.27 (1.85)
Beaver	4	2	5	4	2	9	6	2	4	11	10	5.36 (3.26)
Bedford	0	3	3	0	0	2	0	0	2	1	3	1.27 (1.35)
Berks	33	22	21	19	19	23	12	14	16	16	13	18.91 (5.94)
Blair	2	4	2	2	3	7	1	3	4	3	1	2.91 (1.70)
Bradford	2	2	2	1	5	0	2	5	0	0	2	1.91 (1.76)
Bucks	3	9	6	11	6	16	8	7	7	4	5	7.45 (3.62)
Butler	1	1	4	0	3	1	4	0	1	1	3	1.73 (1.49)
Cambria	3	3	4	3	5	3	6	2	6	1	5	3.73 (1.62)
Cameron	0	0	0	0	0	0	0	0	0	0	0	.00 (.00)
Carbon	0	0	0	0	2	1	1	0	1	2	2	.82 (.87)
Centre	2	6	4	1	3	1	3	1	0	2	0	2.09 (1.81)
Chester	7	4	3	3	3	7	8	6	9	3	8	5.55 (2.38)
Clarion	0	1	1	0	3	2	0	1	0	1	0	.82 (.98)
Clearfield	0	0	1	1	1	2	1	1	1	0	0	.73 (.65)
Clinton	0	0	0	3	2	2	0	1	1	0	1	.91 (1.04)
Columbia	2	1	0	0	1	0	2	0	0	1	0	.64 (.81)
Crawford	2	2	3	1	0	2	1	1	1	1	5	1.73 (1.35)
Cumberland	5	4	2	3	1	2	3	3	2	2	1	2.55 (1.21)
Dauphin	14	13	17	9	14	15	11	17	14	21	17	14.73 (3.26)
Delaware	27	27	21	16	33	37	24	41	36	23	33	28.91 (7.69)
Elk	0	0	1	0	0	0	0	0	1	0	1	.27 (.47)
Erie	6	9	7	6	1	8	4	5	9	9	12	6.91 (2.98)
Fayette	2	4	8	2	5	6	2	8	2	5	2	4.18 (2.40)
Forest	0	0	0	0	0	0	0	0	0	0	0	.00 (.00)
Franklin	2	0	2	1	2	0	4	3	4	2	9	2.64 (2.50)
Fulton	0	1	2	0	0	0	0	1	0	1	0	.45 (.69)
Greene	2	0	3	0	1	1	1	0	1	3	0	1.09 (1.14)
Huntingdon	3	5	2	0	4	0	2	0	0	2	1	1.73 (1.74)
Indiana	1	2	3	0	1	2	1	4	0	2	3	1.73 (1.27)
Jefferson	2	1	1	1	0	3	3	0	4	2	2	1.73 (1.27)

Table A1. *Continued*

County	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	Mean (SD)
Juniata	1	2	0	1	0	0	0	0	0	1	1	.55 (.69)
Lackawanna	2	2	1	5	4	4	0	2	9	3	1	3.00 (2.49)
Lancaster	12	12	9	9	7	6	21	13	8	12	9	10.73 (4.10)
Lawrence	4	2	2	2	4	8	3	2	1	3	6	3.36 (2.06)
Lebanon	4	0	4	2	3	1	4	1	2	8	5	3.09 (2.26)
Lehigh	12	13	12	14	17	23	18	22	19	17	11	16.18 (4.12)
Luzerne	1	8	15	20	9	13	21	13	15	26	9	13.64 (6.98)
Lycoming	0	1	2	1	2	4	1	3	2	2	2	1.82 (1.08)
McKean	0	3	0	0	0	1	1	0	0	0	2	.64 (1.03)
Mercer	2	0	1	1	3	1	3	0	5	1	2	1.73 (1.49)
Mifflin	1	1	0	2	0	1	1	2	0	0	1	.82 (.75)
Monroe	2	7	4	1	9	3	4	4	4	10	3	4.64 (2.84)
Montgomery	10	15	19	13	9	18	14	10	15	19	17	14.45 (3.64)
Montour	1	0	0	0	0	0	0	0	0	0	0	.09 (.30)
Northampton	5	13	6	2	3	7	9	12	3	4	10	6.73 (3.80)
Northumberland	2	4	0	0	2	4	2	0	3	0	1	1.64 (1.57)
Perry	1	2	0	1	0	1	1	1	3	4	1	1.36 (1.21)
Philadelphia	319	309	288	348	330	377	407	392	331	302	307	337.27 (39.20)
Pike	0	0	1	1	3	2	1	3	0	3	0	1.27 (1.27)
Potter	1	0	0	0	1	0	0	1	2	0	0	.45 (.69)
Schuylkill	0	1	2	2	7	1	1	4	4	2	3	2.45 (1.97)
Snyder	0	2	0	1	1	0	0	0	2	1	1	.73 (.79)
Somerset	0	40	3	5	3	1	0	1	2	7	0	5.64 (11.61)
Sullivan	0	0	0	0	1	0	0	0	0	0	0	.09 (.30)
Susquehanna	0	1	0	0	1	0	0	0	3	2	0	.64 (1.03)
Tioga	0	0	0	0	0	0	0	0	1	0	1	.18 (.40)
Union	2	0	0	0	0	0	0	0	1	0	1	.36 (.67)
Venango	2	0	1	1	0	0	1	1	1	0	3	.91 (.94)
Warren	0	2	1	2	0	4	0	0	2	3	0	1.27 (1.42)
Washington	4	1	9	3	3	3	2	7	7	7	8	4.91 (2.74)
Wayne	0	1	0	1	1	0	3	2	2	1	2	1.18 (.98)
Westmoreland	4	10	6	3	5	4	6	7	5	7	8	5.91 (2.02)
Wyoming	0	0	2	0	2	1	0	2	0	0	1	.73 (.90)
York	3	2	6	6	18	18	10	16	20	19	11	11.73 (6.77)
Total	583	658	599	633	645	749	732	725	705	666	654	668.09 (54.14)

Appendix B Individual Regression Models

Cross-Sectional

Table 10. Death Filings – (Model 1)

Predictor	Unstandardized Coefficients B	Unstandardized Coefficients Std. Error	Standardized Beta
(Constant)	-254.543	53.534	
Death Penalty Filings	13.742	.684	.758***
Population	50.500	7.519	.236***
Unemployment	3.529	19.904	.006
Poverty	16.110	4.856	.115**
R^2 = .968			
Adjusted R Squared = .966			
Std. Error of Estimate = 86.426			

Note. * p ≤ .05, ** p ≤ .01, *** p ≤ .001

Table 11. Death Filings – (Model 2)

Predictor	Unstandardized Coefficients B	Unstandardized Coefficients Std. Error	Standardized Beta
(Constant)	-205.014	80.904	
Death Penalty Filings	14.158	.794	.781***
Population	47.907	8.432	.218***
Unemployment	-14.726	17.368	-.025
Poverty	16.288	4.044	.121***
R^2 = .963			
Adjusted R Squared =.960			
Std. Error of Estimate = 92.656			

Note. * p ≤ .05, ** p ≤ .01, *** p ≤ .001

Table 12. Death Sentences – (Model 3)

Predictor	Unstandardized Coefficients B	Std. Error	Standardized Beta
(Constant)	-633.953	104.113	
Death Sentences	50.060	8.106	.482***
Population	85.631	16.561	.399***
Unemployment	27.081	42.976	.045
Poverty	38.358	10.064	.274***

R^2 = .848
Adjusted R Squared =.838
Std. Error of Estimate = 187.115

Note. * p ≤ .05, ** p ≤ .01, *** p ≤ .001

Table 13. Death Sentences – (Model 4)

Predictor	Unstandardized Coefficients B	Std. Error	Standardized Beta
(Constant)	-736.134	151.568	
Death Sentences	46.173	9.269	.445***
Population	88.975	18.348	.405***
Unemployment	27.941	36.275	.048
Poverty	39.983	7.809	.297***

R^2 = .836
Adjusted R Squared =.825
Std. Error of Estimate = 194.756

Note. * p ≤ .05, ** p ≤ .01, *** p ≤ .001

Time-Series

Table 14. Death Filings – (Model 1)

Predictor	Unstandardized Coefficients B	Unstandardized Coefficients Std. Error	Standardized Beta
(Constant)	-225.530	43.653	
Death Penalty Filings	17.178	1.890	.622***
Population	30.535	6.620	.303***
Unemployment	9.901	17.174	.034
Poverty	12.830	3.990	.194**

$R^2 = .899$
Adjusted R Squared = .892
Std. Error of Estimate = 72.428

Note. * p ≤ .05, ** p ≤ .01, *** p ≤ .001

Table 15. Death Filings – (Model 2)

Predictor	Unstandardized Coefficients B	Unstandardized Coefficients Std. Error	Standardized Beta
(Constant)	-206.699	65.990	
Death Penalty Filings	17.556	2.236	.635***
Population	29.705	7.422	.288***
Unemployment	-3.329	14.891	-.012
Poverty	13.651	3.188	.216***

$R^2 = .887$
Adjusted R Squared = .880
Std. Error of Estimate = 76.318

Note. * p ≤ .05, ** p ≤ .01, *** p ≤ .001

Table 16. Death Sentences – (Model 3)

Predictor	Unstandardized Coefficients B	Std. Error	Standardized Beta
(Constant)	-359.616	64.003	
Death Sentences	-6.753	8.464	-.057
Population	79.465	7.279	.788***
Unemployment	21.819	26.272	.076
Poverty	20.757	5.988	.314***

$R^2 = .761$
Adjusted R Squared = .745
Std. Error of Estimate = 111.072

Note. * $p \leq .05$, ** $p \leq .01$, *** $p \leq .001$

Table 17. Death Sentences – (Model 4)

Predictor	Unstandardized Coefficients B	Std. Error	Standardized Beta
(Constant)	-495.295	75.967	
Death Sentences	-14.922	8.243	-.127
Population	79.861	7.240	.774***
Unemployment	45.340	19.589	.164*
Poverty	20.163	4.279	.319**

$R^2 = .783$
Adjusted R Squared = .769
Std. Error of Estimate = 105.827

Note. * $p \leq .05$, ** $p \leq .01$, *** $p \leq .001$

Appendix C Data Sources

In the process of collecting homicide data for the present study, the analyses relied on those data made available through the Inter-University Consortium for Political and Social Research (ICPSR), specifically from the Uniform Crime Report program for all of Pennsylvania's 67 counties between 2000 – 2010. For the multivariate controls (i.e., population, unemployment, and poverty), data was collected the U.S. Census Bureau for the 2000 and 2010 decennial censuses, respectively. I obtained death penalty cases data used by Ulmer et al. (2020) from the primary author (see data citations below).

1. United States Department of Justice. Federal Bureau of Investigation. Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense Data, 2000. Inter-university Consortium for Political and Social Research [distributor], 2006-03-30.

2. United States Department of Justice. Federal Bureau of Investigation. Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense Data, 2001. Inter-university Consortium for Political and Social Research [distributor], 2006-03-30.

3. United States Department of Justice. Federal Bureau of Investigation. Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense Data, 2002. Inter-university Consortium for Political and Social Research [distributor], 2006-03-30.

4. United States Department of Justice. Federal Bureau of Investigation. Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense Data, 2003. Inter-university Consortium for Political and Social Research [distributor], 2006-01-31.

5. United States Department of Justice. Federal Bureau of Investigation. Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense Data, 2004. Inter-university Consortium for Political and Social Research [distributor], 2006-07-26.

6. United States Department of Justice. Federal Bureau of Investigation. Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense

Data, 2005. Inter-university Consortium for Political and Social Research [distributor], 2007-07-24.

7. United States Department of Justice. Federal Bureau of Investigation. Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense Data, 2006. Inter-university Consortium for Political and Social Research [distributor], 2008-12-12.

8. United States Department of Justice. Federal Bureau of Investigation. Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense Data, 2007. Inter-university Consortium for Political and Social Research [distributor], 2009-08-20.

9. United States Department of Justice. Federal Bureau of Investigation. Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense Data, 2008. Inter-university Consortium for Political and Social Research [distributor], 2011-04-21.

10. United States Department of Justice. Office of Justice Programs. Federal Bureau of Investigation. Uniform Crime Reporting Program Data: County-Level Detailed Arrest and Offense Data, United States, 2009. Inter-university Consortium for Political and Social Research [distributor], 2014-06-16.

11. United States Department of Justice. Office of Justice Programs. Federal Bureau of Investigation. Uniform Crime Reporting Program Data: County-Level Detailed Arrest and Offense Data, United States, 2010. Inter-university Consortium for Political and Social Research [distributor], 2014-06-16.

12. United States Census Bureau. (2000). *Decennial Census*. Web.

13. United States Census Bureau. (2000). *Decennial Census*. Web.

BIBLIOGRAPHY

1. Allison, K., & Harris, C. T. (2018). Predicting bias homicide across victim groups: A county-level analysis. *Social Science Research. 74*, 108-119.

2. American Bar Association. (2015). *Pennsylvania Governor Declares Moratorium on Death Penalty.* Web.

3. Baily, W. C. (1980). Deterrence and the celerity of the death penalty: A neglected question in deterrence research. *Social Forces,*58(4), 1308-1333.

4. Baily, W. C. (1998). Deterrence, brutalization, and the death penalty: Another examination of Oklahoma's return to capital punishment. *Criminology, 36*(4), 711-898.

5. Beccaria, C. (1764). *An Essay on Crimes and Punishment.* Oxford: Clarendon Press.

6. Becker, G. S. (1968). Crime and punishment: An economic approach. *Journal of Political Economy, 76*(2), 169-217.

7. Bowers, W. J., Pierce, G. L. (1975). The illusion of deterrence in Isaac Ehrlich's research on capital punishment. *The Yale Law Journal, 85*(2), 187-208.

8. Bureau of Justice Statistics. (2024). *Capital Punishment. 2022 – Statistical Tables.* Web.

9. Chalfin, A., Haviland, & Raphael, S. (2012). What do panel studies tell us about a deterrent effect of capital punishment? A critique of the literature. *Journal of Quantitative Criminology, 29*(1), 5-43.

10. Chalfin, A., & McCrary, J. (2017). Criminal deterrence: A review of the literature. *Journal of Economic Literature, 55*(1), 5-48.

11. Cochran, J. K., Chamlin, M. B., & Seth, M. (1994). Deterrence or brutalization? An impact assessment of Oklahoma's return to capital punishment. *Criminology, 32*(1), 107-134.

12. Cochran, J, K., & Chamlin, M. B. (2000). Deterrence and brutalization: The dual effects of executions. *Justice Quarterly, 17*(4), 685-706.

13. Cook, P. J. (2009). Potential savings from abolition of the death penalty in North Carolina. *American Law and Economic Review, 11*(2), 498-529.

14. Berger, E. (2020). Courts, culture, and the lethal injection stalemate. *William and Mary Law Review, 62*(1), 1-81.

15. Death Penalty Information Center. (2024). *Recent Legislative Activity: 2023 Legislation.* Web.

16. Death Penalty Information Center. (2025a). *Recent Legislative Activity: 2024 Legislation.* Web.

17. Death Penalty Information Center. (2025b). *State by State*. Web.

18. Death Penalty Information Center. (2025c). *Time on Death Row.* Web.

19. Death Penalty Information Center. (2025d). *Executions by State and Region.* Web.

20. Death Penalty Information Center. (2025e). *Death Penalty Census: Key Findings.* Web.

21. Dezhbakhsh, H., Rubin, P. H., & Shepherd, J. M. (2003). Does capital punishment have a deterrent effect? New evidence from postmoratorium panel data. *American Law and Economics Review, 5*(2), 344-376.

22. Dezhbakhsh, H., & Shepherd, J. M. (2006). The deterrent effect of capital punishment: Evidence from a "judicial experiment". *Economic Inquiry, 44*(3). 512-535.

23. Donohue, J. J. (2014). An empirical evaluation of the Connecticut death penalty system since 1973: Are there unlawful racial, gender, and geographic disparities? *Journal of Empirical Legal Studies, 11*(4). 637-696.

24. Donohue, J. J., & Wolfers, J. (2005). Uses and abuses of empirical evidence in the death penalty debate. *Stanford Law Review, 58*(3), 791-845.

25. Ehrlich, I. (1975). The deterrent effect of capital punishment: A question of life and death. *The American Economic Review, 65*(3), 397-417.

26. Federal Bureau of Investigation. (2025). *Crime Data Explorer*. Web.

27. Gallup. (2025). *Death Penalty*. Web.

28. Jaynes, C. M. & Wilson, T. (2022). Dreading delayed punishment: Reconceptualizing sanction "celerity". *Journal of Crime and Justice, 45*(3). 285-303.

29. Katz, L., Levitt, S. D., Shustorovich, E. (2003). Prison conditions, capital punishment, deterrence. *American Law and Economics Review, 5*(2), 318-343.

30. Kovandzic, t. V., Vieraitis, L. M., & Boots, D. P. (2009). Does the death penalty save lives? New evidence from state panel data, 1977 to 2006. *Criminology & Public Policy, 8*(4), 803-843.

31. Liu, Z. (2004). Capital punishment and the deterrence hypothesis: Some new insights and empirical evidence. *Eastern Economic Journal, 30*(2), 237-258.

32. Manski, C. F., & Pepper, J. V. (2012). Deterrence and the death penalty: Partial identification analysis using repeated cross sections. *Journal of Quantitative Criminology, 29*(1), 123-141.

33. Mocan, H. N., & Gittings, R. K. (2003). Getting off death row: Commuted sentences and the deterrent effect of capital punishment. *The Journal of Law and Economics, 46*(2), 453-478.

34. Mori, D. (2020). Deterrent effect of capital punishment in Japan: An analysis using nonstationary time-series data. *Supreme Court Economic Review, 28*(1), 61-116.

35. Nagin, D. S. (1998). Criminal deterrence research at the outset of the twenty-first century. *Crime and Justice, 23*, 1-42.

36. Nagin, D. S. (2013). Deterrence: A review of the evidence by a criminologist for economists. *Annual Review of Economics, 5*(1), 83-105.

37. National Research Council. (1978). *Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates.* Washington, DC: The National Academies Press.

38. National Research Council. (2012). *Deterrence and the Death Penalty.* Washington, DC: The National Academies Press.

39. Oliphant, S. N. (2022). Estimating the effect of death penalty moratoriums on homicide rates using the synthetical control method. *Criminology & Public Policy, 21*(4), 915-944.

40. Parker, B. (2021). Death penalty statutes and murder rates: Evidence from synthetic controls. *Journal of Empirical Legal Studies, 18*(3), 488-533.

41. Peterson, N., & Lynch, M. (2012). Prosecutorial discretion, hidden costs, and the death penalty: The case of Los Angeles County. *The Journal of Criminal Law and Criminology, 102*(4), 1233-1274.

42. Pridemore, W. A. (2005). A cautionary note on using county-level crime and homicide data. *Homicide Studies, 9*(3), 256-268.

43. Shepherd, J. M. (2005). Deterrence versus brutalization: Capital punishment's different impacts among states. *Michigan Law Review, 104*(2), 203-256.

44. Singleton, C. R., Winata, Adams, A. M., McLafferty, S. L., Sheehan, K. M., & Zenk, S. N. (2022). County-level associations between food retailer availability and violent crime rate. *BMC Public Health, 22*(1), 1-9.

45. Steiker, C. S., & Steiker, J. M. (2020). The rise, fall, and afterlife of the death penalty in the United States. *Annual Review of Criminology, 3*(1), 299-315.

46. Ulmer, J. T., Zajac, G., & Kramer, J. H. (2020a). Geographic arbitrariness? County court variation in capital prosecution and sentencing in Pennsylvania. *Criminology & Public Policy, 19*(4), 1073-1112.

47. Ulmer, J. T., Kramer, J. H., & Zajac, G, (2020b). The race of defendants and victims in Pennsylvania death penalty decisions: 2000-2010. *Justice Quarterly, 37*(5), 955-983.

48. Zimmerman, P. R. (2004). State executions, deterrence, and the incidence of murder. *Journal of Applied Economics, 7*(1), 163-193.