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Measuring the Effect of Capital Punishment on Murder Deterrence

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The efficacy of capital punishment is highly contested. Since the reinstatement of capital punishment in 1976 (*Gregg v. Georgia*), executions have been performed in 34 states.

The United States penal system carries out more executions per year than any other developed nation. Murder deterrence is cited as validation for the preservation of the death penalty. Empirical studies on the deterrent effects of capital punishment are mixed. Meta-analyses conclude that empirical evidence presented to date is too fragile to serve as a basis for policy decisions (Gerritzen & Kirchgässner 2012).

This paper uses econometric analysis to estimate the impact of the death penalty on murder deterrence. Employing county-level panel data we evaluate the use and effectiveness of the death penalty from 1977 to 2013. The nature of panel data allows us to control for systematic differences that would otherwise lead to endogeneity bias. Our panel data set also extends the data to the most recent year possible, enhancing the relevancy of the model.

Analysis utilizing econometrics to quantify the death penalty’s impact on murder implicitly presumes that, in the act of committing a crime, offenders use deliberate logic. Becker (1967) in his seminal work promotes what is known as the Beckerian model of crime, which advances the notion that offender decision-making is driven by cost-benefit analysis. The Nobel Laureate asserts that offenders commit crimes because potential benefits outweigh the potential risks.[1]

Economists’ involvement in death penalty discussions can be traced back to 1975, when the controversial work of Isaac Ehrlich garnered much attention. Utilizing time series data at the national level, Ehrlich’s study found that every state-sanctioned execution carried out between 1933 and 1969 saved eight lives. At the time, Ehrlich’s findings were novel. The first economic inquiry into the death penalty, Ehrlich’s study is credited with influencing the re-emergence of the death penalty in 1976’s *Gregg v. Georgia* (Kovandzic, Vieraitis, & Boots 2009).

Other econometricians were quick to challenge Ehrlich’s analysis in the coming years. Concerns surrounding his use of execution data at the national level called into question Ehrlich’s analysis due to variations in the use of capital punishment across states. Theorists have pushed for a re-examination of the way the impact of the death penalty on murder is estimated, calling into question previous literature (Donahue & Wolfers 2009). For example, county level execution data may easily be overlooked as executions occur at the state level while the death penalty is implemented at the county level.

Despite over 40 years of econometric analysis, the deterrent effect of the death penalty remains unclear. This paper seeks to determine the existence of a deterrent effect by employing a novel approach which utilizes county level panel data to recognize the variation of capital punishment within and across states. Using this approach, we hypothesize that the death penalty has a weak deterrent effect on murder rates. The findings of this study have legislative ramifications in a post-Gregg era, as it is widely accepted in the field of economics that the literature used to overthrow the death penalty moratorium (Ehrlich 1975) was flawed in its experimental design. A clear understanding of the efficacy of the death penalty is necessary in the development of contemporary policy.

I. LITERATRURE REVIEW

A. Ehrlich & The Reemergence of the Death Penalty

In 1972, it seemed as if abolitionists had finally won when, in *Furman v. Georgia*, the Supreme Court placed a moratorium on the death penalty on the grounds it violated the Eighth Amendment. The Supreme Court voted 5-4 that mandatory death sentences violated Eighth Amendment rights because the severity of the punishment had not taken into account the characteristics of the offender (Smith 2012), and it unfairly punished the convicted.[2]

These death sentences are cruel and unusual in the same way that being struck by lightning is cruel and unusual. For, of all the people convicted of rapes and murders in 1967 and 1968, many just as reprehensible as these, the petitioners are among a capriciously selected random handful upon whom the sentence of death has in fact been imposed.[3]

Leading up to the trial, the death penalty had been administered disproportionally, particularly toward black males. The court found no grounds existed to distinguish those not sentenced to death from those sentenced to death for worse or equal crimes (Smith 2012). Capital punishment laws were set at the state level with no uniformity, which lead to an uneven application across states. At the county level where sentences are adjudicated, there could be wide variation with some counties having a high death penalty sentencing rate and other counties sentencing no one to death. Within a state, an individual convicted of murder could receive a lesser sentence than an individual convicted of rape. Some counties handed out harsher sentences than others. Sentences were typically harsher for blacks and the poor, who were readily and expeditiously sped through the judicial process.

The Supreme Court's ruling did not completely rule out the death penalty. Instead, it placed a duty on the states to resolve issues around its disparate use. In 1975, three years after the moratorium, Isaac Ehrlich published a study that would change the landscape of the death penalty debate. Inspired by Gary Becker's economic model of crime (1967), Ehrlich published *"The Deterrent Effect of Capital Punishment: A Question of Life and Death".* Prior to Ehrlich's article, death penalty proponents had made the argument that executions saved lives with no quantitative backing. Ehrlich theorized, based on Becker's model, that murders were influenced by the likelihood that potential offenders believed they would be executed for their crime (Fagan 2006).

An economist by training, Ehrlich was the first to use statistical and econometric methods to study the death penalty, finding that every execution deterred eight murders (Ehrlich 1975). Once published, Ehrlich's study was quickly popularized. That same year the Solicitor General cited Ehrlich's study in *North Carolina vs. Woodso*n and *Gregg v. Georgia*. Decided on the same day, North Carolina made the argument that they had met the 1972 Supreme Court's concerns of ruling out discrimination and arbitrary sentencing through the imposition of mandatory sentences for crimes meeting fixed requirements.[4] North Carolina's law omitted mitigating factors such as the nature of the crime and the history of the individual.

After Ehrlich's findings were published, the Supreme Court, in 1976 *Gregg v. Georgia,* reversed their decision. Ehrlich's study was amongst the evidence presented in support of the death penalty. After the reversal, the argument was no longer philosophical. Instead it became a debate over the measurable strength of the death penalty as a murder supply deterrent. In resolving the case the Supreme Court left open the door to capital punishment, leading to the death penalty's current status in this country. The court's decision was guided by the assumption that legislative fixes would result in greater consistency in the administration of the death penalty. The court established criteria to determine whether state statutes violated the Eighth Amendment. These changes led to the separation of the trial and sentencing phases.[5]

While capital punishment is a state-level policy, the incidence of capital punishment use can vary widely at the county level. In the most active death-sentencing states the majority of counties do not use the death penalty with any regularity. For example, in Texas approximately two-thirds of counties for which we have a complete set of data have never sentenced anyone to the death penalty from 1980 through 2014. This suggests the best way to measure death sentencing activity is at the county level. Concentrations in death sentencing exist amid counties because sentencing juries are a reflection of the county where the crime is committed. County diversity allows for differing demographic, socioeconomic, and political landscapes, affecting juror willingness to give death sentences. The county prosecutor normally decides whether or not to seek the death penalty affecting county-to-county administration of the death penalty. The quality of representation provided by county-level defender organizations also influences the administration of the death penalty (Smith 2011). We feel it is important to match the county-level murder rate with the number of county-level convictions that led to executions.

B. Missteps Along the Way

Ehrlich's study is widely regarded as flawed because it was based on a single cross section of data. The flaws in Ehrlich's study can be partially overcome by using data with both a cross section and time series dimension – what economists often call "panel data" or "longitudinal data" (Chalfin, Haviland and Raphael 2012).[6]

Donahue and Wolfers (2009) point out three major flaws in Isaac Ehrlich's experimental design: (1) the use of ratios, (2) the conviction rate variable and (3) large estimates. As mentioned above, the majority of U.S. counties have never carried out an execution. Large states with large metropolitan areas, such as Texas, can elevate a state's overall execution statistics. Dividing state execution numbers by the number of counties in the state to determine an average execution rate per county is illegitimate and misrepresentative.

Ehrlich proposed three deterrence ratios to measure the efficacy of the death penalty, considering the information a criminal would take into account before committing a murder. Ehrlich draws from Becker (1967) assuming criminals only commit crimes when the potential benefits outweigh potential risks. Ehrlich produces a murder-arrest rate, conviction rate for arrested murders and an execution rate for those convicted to form his model. Unfortunately, Ehrlich's choices are incapable of symbolizing genuine probabilities because they may exceed one, are undefined, or lack empirical measurement (Donahue and Wolfers 2009). A state, or county within in a state, often has multiple unresolved murders. If a county has a murder without an arrest or multiple murders committed by the same individual Ehrlich's probabilities are untenable. Ehrlich's ratios are depicted below.

(1) $\frac{ARRESTS}{MURDERS}$

(2) $\frac{CONVICTIONS}{ARRESTS}$

(3) $\frac{EXECUTIONS}{CONVICTIONS}$

If there are no murders, the arrest to murders ratio is undefined. Because Dezhbakhsh, Rubin, and Shepherd (2003) construct their model at the county-level many counties fall into the category of having no murders or arrests and even arrests without murders. Dezhbakhsh, Rubin, and Shepherd's county arrests to murders often have a zero denominator. To account for this Dezhbakhsh, Rubin, and Shepherd drop variables that are detrimental to their study. Donahue and Wolfers (2009) point out temporal mismatches may arise for arrests made for crimes committed years prior to the arrest. Convictions to arrests is a problematic estimator for a double murder or scenario where the offender commits murder followed by suicide. In many counties murder-suicide is classified as a conviction without arrest. In the case of multiple murders/convictions to arrest, two would be greater than one. In theory, a mass murderer can inflate the number of murders indefinitely with restriction to one or zero arrests. Timothy McVeigh, is an example of someone with a high number of convictions (murders), but he is only able capable of one arrest[7]. For similar reasons, those killed on September 11, 2001 are excluded from our measurement.

Multiple murderers acting cooperatively have the opposite effect, inflating the arrests to murder ratio, causing the arrest rate to appear higher than it may be. Erroneous or improper arrests would also inflate these numbers. Donahue and Wolfers 2009 found that in Dezhbakhsh, Rubin, and Shepherd's study, 8,727 county-year arrest rates were greater than one (27.1% of the non-missing observations) and 8,944 were equal to one (28%). The overall un-weighted mean arrest rate in Dezhbakhsh, Rubin, and Shepherd's county sample is 1.01, with 10% of the observations being two or greater (DW 2009).

C. Conviction Data

There is a noticeable lack of convictions data in the United Sates (U.S.). The U.S. justice system involves cooperation across multiple agencies whereby no single agency is tasked with maintaining a national repository of trial data at the county level. Instead state agencies or county clerk offices are charged with keeping this information. The criminal system is built on precedence, a legal decision or form preceding that serves as an authoritative rule or pattern in future similar or analogous cases (Dictionary 2015). Trial attorneys and judges are tasked with conducting their own research for this information.

Were this data readily available better measurements could be made regarding the consistency of the death penalty's use. Even calculation of Ehrlich's ratios would be feasible at the county level. In my research efforts, I was unable to locate any national databases containing county-level convictions data. A county-by-county search would have to be conducted. This is unfeasible for the 3,143 county-equivalents. Capital cases tried would highlight differences amongst county prosecutors and their use of the death penalty. Data providing the number of capital cases tried without conviction would provide insight to county constituents and their taste or distaste of capital punishment.

Isaac Ehrlich's (2) convictions to arrests are made problematic due to the unavailability of conviction data. The FBI's *Uniform Crime Reports* (UCR), which we use in our calculations, is the closest repository of statistics measuring the murder rate and crimes within a given state. At the time of Ehrlich's study the UCR kept conviction data it no longer collects leaving an unfilled void. Even at the time of Ehrlich's use of these statistics, the UCR was not kept for every municipality. In 1953, the UCR only reported arrest rates for 197 cities with populations of over 25,000. This means Ehrlich's national survey included 25 million persons. Because this data is unobtainable, Dezhbakhsh, Rubin, Shepherd and others have relied on the "probability of conviction if charged with murder" to replace the arrest rate measure. Measuring death sentences to arrests (4) is still a problem if the arrest rate equals zero.

(4) $$\frac{DEATH\ SENTENCES}{ARRESTS}$$

This leaves the decision of whether to drop the value, treat the value as zero, or find another way of dealing with the rate. Dezhbakhsh, Rubin, and Shepherd decided to drop this data. The measure also does not capture deterrence pressures. A state that captures and convicts every murderer would generate much more deterrence than a state that convicts only a small fraction of its murderers while sentencing a number to death. Death sentence to arrests is not an effective control for conviction rates (Donahue and Wolfers 2009).

Donahue and Wolfers suggest discontinuance of the arrest rate (1) variable discussed in *1.4*. Donahue and Wolfers also find that "death sentences to murder arrests" (2) should be dropped, as conviction data is not available by the state. Lastly Donahue and Wolfers concludes (3) "executions to death sentences" to be impractical whether controlling for lags or not.

D. Debakhsh, Rubin and Shepherd

Debakhsh, Rubin and Shepherd 2003 can be credited with the closest attempt to utilize data at the county level. Unfortunately, they chose not to address the problems arising from missing data in their work. Debakhsh, Rubin and Shepherd also misuse state level executions for county-level executions data. Any county with an execution was given the same statewide execution number for that year. Any county having an execution, even if just one, is credited with the total number of executions for the state. This is a major oversight.

Debakhsh, Rubin and Shepherd's study spans 1977 to 1996 and utilizes a two-stage least squares (2SLS) regression to come to their conclusions, claiming statistical significance between murder rates and the impact of executions. Donahue and Wolfers (2009) provide a comprehensive review of Debakhsh, Rubin and Shepherd (2003). Donahue and Wolfers find no statistical significance using clustered standard errors and corrected 2SLS estimates. Donahue and Wolfers also find Debakhsh, Rubin and Shepherd's instruments to be invalid - police spending, judicial spending, prison admissions, and percent voting for a Republican president. Donahue and Wolfers reject these figures, all of which are measured at the state level instead of at the county level. We do not detail other studies referenced by Donahue and Wolfers because the studies were conducted at the state level and were discredited after they had undergone careful scrutiny. Donahue and Wolfers (2009) show that all the studies utilizing 2SLS regression claim to find deterrence, while studies relying on ordinary least squares (OLS) show no evidence of deterrence.[8]

II. DATA AND VARIABLE DEFINITIONS

A. Data Description

Our study covers the years 1977 to 2013, making it the most current county level death penalty examination to date. We utilize four main sources: the U.S. Census Bureau, FBI *Uniform Crime Reports (UCR)*, execution data from the Death Penalty Information Center (Deathpenaltyinfo.org), and data collected by John Lott (2000).

Intercensal data from the U.S. Census Bureau provides county level population data while data from the UCR focuses on three types of violent crime - murder, rape and robbery. Data utilized from John Lott's (2000) study centers on his collection of UCR data for the years 1977 to 1996. Due to shortcomings in the UCR database, the years 2001 to 2003 are excluded from our study.

The year 1977 is established as our first year in the study as it was the first complete year of the death penalty's reinstatement following the *1976 Gregg v. Georgia* ruling. It is not surprising that very few states actively used capital punishment in the late 1970s as most were rewriting or reestablishing their death penalty statutes to conform to the Supreme Court's ruling. It is also important to note that FBI Uniform Crime Reports data (UCR) is known to be of low quality for the late 1970s. Martz and Targonski (2002) have found UCR data to have major gaps and to utilize imputation algorithms that are inadequate, inconsistent and prone to either double counting or undercounting of crimes depending on jurisdiction (CHR 2012).[9] We therefore recognize and take into account corrections and or notations made by the FBI at the agency level. Cases of under- or over-reporting have been removed from our model. In addition to UCR data incorporated into the model we obtained county level convictions that led to executions from the Death Penalty Information Center. Intercensal population estimates are utilized transform murder and execution data from the UCR into murder and execution rates per ten thousand people.

Table 1 provides descriptive statistics for the counties. Our model covers thirty-six years. The average number of murders is approximately 5.4. The murder rate, which is the number of murders per ten thousand people, averages about 0.486, or about half a murder per ten thousand people. The murder rate ranges from a low of zero to a high of almost one hundred and sixty-seven. More than 99% of the observation years have zero executions, and so we are identifying the deterrent effect from a small fraction of counties, with the other counties acting as controls. Harris County, Texas has the highest number of executions in a single year, eleven in 1997.

Table 1. Descriptive Statistics

Variable	Mean	Standard Deviation	Minimum	Maximum
Murder	5.385	36.786	0	1,944
Murder Rate (Per 10,000 people)	0.486	1.543	0	166.667
Executions	0.012	0.150	0	11
Execution Rate (Per 10,000 people)	0.001	0.033	0	4.952
Populations	84900.67	278,574.1	39	10,000,000
Robbery Rate	43.268	268.703	0	4,3339.23

Note: observations; murder N = 91278, murder rate N = 91244, executions N = 116250, execution rate N = 115809, population N = 115946, robbery rate N = 91240.

B. The Demography of the Death Penalty[10]

There are thirty-one states with death penalty statutes in place (Table 2). Nineteen states and the District of Columbia do not have death penalty statutes. The U.S. government and the U.S. military also have death penalty statutes (NAACP 2015).[11]

There are 3,002 death row inmates nationwide. Whites make up the majority with 1,284 (42.77%) inmates on death row. Blacks are second with 1,251 (41.67%) death row inmates. The remaining death row population is comprised of other ethnic groups: Latino/Latina 386 (12.86%), Native American 31 (1.03%), Asian 49 (1.63%), and one unknown (0.03%). In 2014, there was a stark comparison between whites and blacks. Whites made up 77.36% of the US population, but only 42.77% of individuals on death row. Blacks made up 13.22% of the population and 41.67% of the death row population (U.S. Census Bureau 2015).

Considering gender, the male death row population overwhelmingly exceeds that of the female population. There are a total of 2,984 males making up 98.20% of the death row population while there are only 54 women on death row amounting to a minuscule 1.80% of death row inmates. For every female on death row, there are approximately 55 males serving death sentences. Although men commit the majority of murders in the United States, about 90% since 1990, they are convicted and receive the death sentence at a disproportionately higher rate than their female counterparts. These facts serve to highlight the arbitrary and unequal nature within the system of capital punishment.

As previously discussed, *Gregg v. Georgia* reflected the Supreme Court's belief that state sentencing schemes could account for and fix disparities in sentencing rates against those accused. Demographic data demonstrates state guidelines are not enough as they fail to eliminate individual beliefs and perceptions, which unduly burden black males.

"There is also overwhelming evidence that the death penalty is employed against men, and not women. Only 32 women have been executed since 1930, while 3,827 men have met a similar fate. It is difficult to understand why women have received such favored treatment, since the purposes allegedly served by capital punishment seemingly are equally applicable to both sexes."[12]

Thurgood Marshall's concurrence in *Furman v. Georgia* cites a similar pattern observed pre-trial. The disparities in application across gender were as troubling as race. Today's numbers provide evidence the conditions put forth in *Furman v. Georgia* are not being met.

Table 2. States with and without the Death Penalty

States With the Death Penalty (31)			**States Without the Death Penalty (19)**	
Alabama	Louisiana	Pennsylvania	Alaska	Michigan
Arizona	Mississippi	South Carolina	Connecticut	New Jersey
Arkansas	Missouri	South Dakota	Hawaii	New Mexico
California	Montana	Tennessee	Illinois	New York
Colorado	Nevada	Texas	Iowa	North Dakota
Delaware	New Hampshire	Utah	Maine	Rhode Island
Florida	North Carolina	Virginia	Maryland	Vermont
Georgia	Ohio	Washington	Massachusetts	West Virginia
Idaho	Oklahoma	Wyoming	Minnesota	Wisconsin
Indiana	Oregon		Nebraska	
Kansas				
Kentucky				Dist. of Columbia

C. Clustering Suggests a Concentration of the Death Penalty[13]

Sentencing occurs at the county level where prosecutor and jurors' predispositions affect the frequency in which the death penalty is sought and carried out. Clustering among a few U.S. counties accounts for the majority of executions. In Texas, Harris (123), Dallas (53), Bexar (41), and Tarrant (38) counties account for approximately 49% of the total number of executions since 1975 (523).[14] Of the 1,399 executions carried out since 1975, only 876 have occurred outside Texas, meaning the Lone Star State accounts for approximately 37% of all executions. The numbers are sobering, but highlight the concentration of death penalty executions in certain regions. One hundred seventy-six Texas counties have participated in death sentences leading to executions. Out of the 176 counties, 41 counties have participated in only one death penalty execution. The top ten counties account for 63% of all Texas executions.[15]

D. An Examination of Harris County, Texas & Its High Execution Count

Harris County, Texas exceeds all other counties and even some states by a wide margin in its implementation of the death penalty. Since 1975, Harris County has executed 123 individuals. Dallas, Bexar and Tarrant counties in Texas also report high execution numbers - 53, 41 and 38 respectively. The combined populations of Bexar, Dallas, Harris and Tarrant Counties total over 10.8 million.[16] Each county utilizes private lawyers to represent its capitally charged defendants. In Harris County, appointed attorneys are compensated with a flat fee regardless of the time and effort they provide. If a case is resolved pretrial with a plea, the trial court reserves the right to reduce the flat fee creating perverse incentives for lawyers to spend minimal time trying to obtain a plea or preparing for trial. Poor regulations exist surrounding the request for additional sums or assistance by the appointed counsel such as secretarial expenses, psychological evaluations or expert witnesses. The county is known to deny these requests on a regular basis.

Other problems exist with this model. The pitting of appointed counsel against institutional counsel poses various problems. In counties like Harris, the appointment process can be problematic because the trial judge is responsible for appointing the lawyer to try the case in his court. Judges may appoint friends, campaign donors or even lawyers with reputations for not causing too many headaches for the judge. On the other hand, lawyers appointed through defenders offices are not burdened by these limitations as they are compensated regardless of outside influence. These lawyers are much more likely to be aggressive in litigation, pretrial motions or requests for funds without fearing retribution by a frustrated judge, even if their requests are denied. Public defender organizations operate more like law firms than solo practices. Defender offices have the freedom to decide which attorneys to assign to a case, internal support staff, are able to share resources such as motions or case law files, and often employ staff investigators and social workers. Finally, defender offices may allow the office to allocate resources more efficiently through the benefit of institutional case tracking (Smith 2012).[17]

With these hurdles in place, Harris County's execution rate seems readily explainable. Regardless of the existence of a brutalization effect, the county's court structure systematically puts defendants at a disadvantage, particularly individuals without resources to hire their own representation.

E. The Death Belt

Clustering is not limited to the state and county level data. Regionally, from 1976 to 2009, there existed a concentration of death sentences in the Southern United States. Ominously referred to as the Death Belt, Alabama, Florida, Georgia, Louisiana, Mississippi, South Carolina and Texas together account for over 90% of all executions since 1976. It is of no coincidence the Death Belt overlaps the southern states with the highest incidences of extra legal violence and killings during the Jim Crow era (Ogletree 2002).

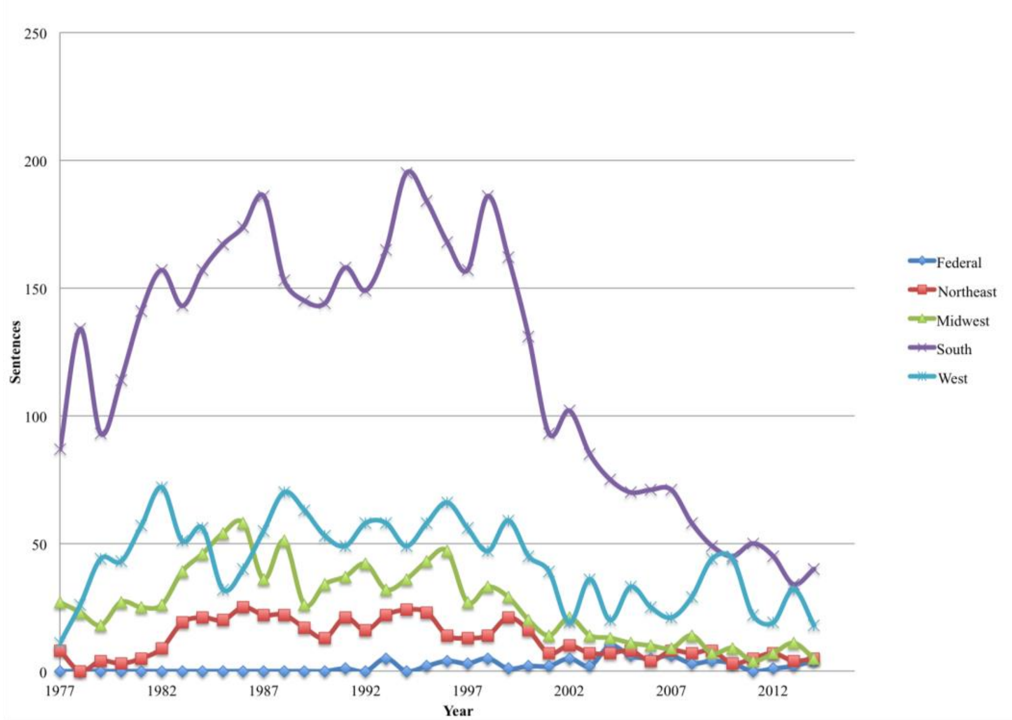

Figure 1. Death Sentences by Region 1977 – 2014[18]

Recent data shows the high number in death sentences in the south has drastically fallen relative to the rest of the country (Figure 1). In fact, capital punishment in the South is in line with death sentencing rates of other regions of the United States. Smith (2012) asserts that since 2009 western states have, and will continue to make up, an ever larger share of death sentences. Our findings, displayed in Figure 1, affirm the reduction of death sentencing rates in the South. This calls into question the misuse of The Death Belt as an epithet to describe the current status of death sentencing in the South.[19]

Other regions have also experienced declining death sentencing rates. This is attributable to current inabilities by states to humanely carry out death sentences. Due to outside forces, states have found it increasingly difficult to procure the drugs necessary to carry out what are considered to be more humane executions. As a result, there have been experimentations with lethal injection drugs resulting in high profile botched executions. As a result, many states have suspended the death penalty. Appendix Figure A1 displays the current status of the death penalty in each state. Later we will discuss the increasing number of state suspensions due to inability to carry out successful executions.

F. Interpreting Capital Punishment

In the post-Gregg era, capital punishment's sentencing schemes have not lived up to expectations. The boundary between a capital and non-capital offense remains elusive. States with differing laws and jurors with differing opinions have not adjudicated the death penalty evenly. We are compelled to measure the death penalty's validity disregarding its impartiality to determine if the death penalty is an effectual murder deterrent. If the death penalty saves lives by preventing would-be murders, capital punishment's infirmities may be overlooked in light of its benefit to society. Death penalty proponents may argue the benefits exceed the costs of its implementation. We solely seek to quantify this measure and place the information in the hands of policymakers and the public to debate the merits of execution in society.

III. EMPERICAL MODEL

We analyze the death penalty from 1977 to 2014, updating data from existing studies to accomplish this. Using original data from John Lott spanning 1975 to 1996, as well as Dezhbakhsh, Rubin, and Shepherd's 2003 study covering 1977 to 1996, we continue to the year 2014 using our own population, violent crime and executions databases. We correct for mistakes in previous data sets and update Lott's data to make it current. For example, our intercensal database covers the years 1970 to 2014 and includes data unavailable to Lott at the time of his study. By having our own data we are able to bring forth new or missing knowledge, while sharing unchanged historical data from the earlier studies.

Because we are studying the data at the county level we have created a database using Federal Information Processing Standard Code (FIPS) codes for the entire United States. Information on executions was retrieved from DeathPenaltyinfo.org. Updated crime data 1994 to present comes from the Uniform Crime Reports (UCR) while demographic data comes from the U.S. Census Department. Populations and demographics have been reconciled with the most up to date census data going back to 1970. All others sources of demographic data are conciliated with Lott and Dezhbakhsh, Rubin, and Shepherd. The combination of updated data and use of previous data from earlier studies increases the credibility of our study as we have used the most up to date information possible and have accounted for all the data in previous studies, which if used correctly would be in line with our impending calculations.

A. Fixed Effects Model

We use a fixed effects model to account for heterogeneity amongst states. A fixed effects model uses panel data where unobserved effects are arbitrarily correlated with the explanatory variables in each time period (Wooldridge 2013). Panel data consists of multiple cross sections, snapshots over time to provide observations for a given time period. Referred to as "longitudinal data", panel data can be used to observe and compare data over time. A cross section is similar to a snapshot in that it takes a wide array of measurements during a period to provide a time series of how elements interact or change with time. Due to having multiple cross sections, different time periods can be compared against one another to study temporal movements over time.

The key advantage of the fixed effects approach is that it allows for inter-county variation (across counties), intra-county variation (within a county), variation in the average from one county to the next, and variation within each county in comparison to another county. Ordinary least squares (OLS) and two stage least squares (2SLS) regressions are only able to account for across-county variation while, by using multiple cross sections over an extended period of time, fixed effects allows within-county variation accounting for trends within each county over time. Regressions relying on inter-county variation are problematic due to potential omitted variable bias where the endogeneity between murders and execution rates are unaccounted for. In most instances, in order for an execution to be carried out, a murder must take place. Thus, there is an existing interrelationship between independent and dependent variables that must be accounted for. Using fixed effects, we make the following identifying assumption, “Unobservable factors that might simultaneously affect the left hand side and right hand side of the regression are time-invariant.” (Blumenstock 2014)

By incorporating thirty years into our model we encapsulate unobserved effects. By beginning in 1977, even as states change death penalty statutes, switching from death penalty to non-death penalty states, our model incorporates enough data (i.e. observations of such changes), that the effects of shifting policies are captured in our model. Unlike models such as Dezhbakhsh, Rubin, and Shepherd, which utilize regression techniques to try to capture unobserved trends such as demographic changes, political leanings, and county income, our approach with its assumption that things change slowly over time automatically incorporates these effects and takes away the guesswork and infeasibility of trying to incorporate hundreds of variables which have the potential to affect murder rates. Dezhbakhsh, Rubin, and Shepherd and others utilize smaller time periods that prohibit them from utilizing our approach. By being current and incorporating more years than any other study, we provide an avant-garde calculation of the death penalty’s impact on murder rates.

B. Lagged Effects Model

A fixed effects model removes the need to run 2SLS to account for time lags. There are two forms of time lag in our model. Economists have struggled with the task of accounting for the time that passes from pre-trial to execution and how this affects individuals who would be deterred by threat of exaction. To account for this time, termed “lags”, the lagged effects model has been used in estimates to simulate anything from sentencing to execution to changes in state law. Careful fixed effects modeling allows us to avoid overburdening our model by attempting to control far too many extraneous variables. With our approach we enhance the validity and ability of our work to garner consistent results. A particular problem in the execution debate is the over-complication of models which nullify most attempts at measuring deterrent effects.

C. Differencing Model

To account for trends in murder rates and executions that change at the county level, we difference to remove the level and then use fixed effects on the differences. Differencing affects the coefficients, as we have allowed each county to have its own trend. Results from a differentiated model can be compared with a non-differentiated fixed effects model to determine if unaccounted for trends exist. Failure to account for trends can lead to uncontrolled collinearity.

To describe the mechanics, differencing is a time series technique. The fixed effects model takes the year observation and differs from the mean while the differencing model differs the year observations year over year, computing all of the differences (2004 from 2003, 2005 from 2004, 2006 from 2005…). Each year is highly correlated, for example 1979 is highly correlated with 1977 because of the adjacency of time. An execution, when carried out, is for a murder that occurred in a year prior creating correlation in observations across years (i.e. autocorrelation). To remove auto-correlation we difference to account for the effect.

D. Two-Stage Least Squares (2SLS)

The two-stage least squares estimator is an instrumental variable where the independent variable for an endogenous explanatory variable is obtained as the fitted value from regressing the endogenous explanatory variable on all exogenous variables (Wooldridge 2013).

Donahue and Wolfers (2009) claim weak instruments to be the root cause of error in Dezhbakhsh, Rubin, and Shepherd. Donahue and Wolfers' findings for Dezhbakhsh, Rubin, and Shepherd and other studies using 2SLS report weak estimators in each (Table 4). Utilizing a two-stage model entails utilizing data correlated to the number of executions (first stage), but also "excludable" from the murder rate equations (second stage). The second stage requires that instrument does not directly influence murder except through its influence on executions. This variable also cannot be a proxy for a variable that should be included in the murder rate equation, but is not. Donahue and Wolfers claim that Dezhbakhsh, Rubin, and Shepherd, Mocan and Gittings (2003) and Zimmerman (2004) do not satisfy this requirement as shown in column 7. Donahue and Wolfers also forewarn of clustering error (discussed prior) failing to adjust for correlation.

Table 4. Three Panel Data Studies on Impact of Executions on Murder Rates[20]

(1) Study	(2) Geographical unit of analysis (time period)	(3) Original OLS estimates	(4) Original 2SLS estimates	(5) Corrected 2SLS estimates and evaluation	(6) Instruments	(7) Assessment of instrument validity
(1) Dezhbakhsh, Rubin, Shepherd (2003)	County-level (1977 – 1996)	None	Negative (statistically significant)	Negative (not significant, with clustered standard errors). Invalid if the instruments are deemed invalid (see next two columns).	(1) Police spending, (2) Judicial spending, (3) Prison admissions, (4) % voting for a Republican president; all instruments are state-level	Not Valid
(2) Mocan and Gittings (2003)	State-level (1984 – 1998)	Negative (statistically significant)	Negative (statistically significant)	Negative (not significant with clustered standard errors).	(1) Deterrence variables $t-2$, (2) Death penalty law $t-1$, (3) Death penalty law $t-2$	Not valid
(3) Zimmerman (2004)	State-level (1978 – 1997)	Negative (not significant)	Negative (statistically significant)	Invalid if the instruments are deemed invalid (see next two columns)	Proportion of murders committed at t and $t-1$: (1) by strangers, (2) with non-felony murder circumstances, and (3) by non-white offenders; (4) indicator for release from death row at $t-1$, (5) indicator for “botched execution” at $t-1$	(1)-(4) not valid; (5) weak

E. Econometric Approach

An important impediment to determining whether capital punishment has a deterrent effect is that the policy is not randomly assigned at the state level. Many factors determine whether a state adopts capital punishment, including how serious murder has been in a state's history. Moreover, at the county level, the incidence of sentencing convicted murderers to death likely varies in ways that are related to murder rates. The fact that use of capital punishment is endogenous is what makes pure cross sectional studies difficult to believe.

Here we exploit the panel nature of our data set and use econometric methods that allow us to control for unobserved county level variables that affect both the murder rate and the use of capital punishment. Technically, these are "fixed effects" models, as described, for example, in Wooldridge (2013, Chapter 14). By putting in county fixed effects we allow for historical and cultural factors, which are difficult to collect data on, to determine both murder rates and to influence the use of capital punishment.

Because we use a long stretch of data, from 1977 through 2013, it is important to allow aggregate differences in both murder rates and propensity to use capital punishment over time. We do this in a very flexible way - by allowing a different intercept for each year in our data set. In other words, along with county fixed effects we include a full set of time fixed effects.[21]

It is unlikely that any deterrent effect occurs only contemporaneously. For example, if someone is executed in a county in November of a given year, that would be expected to have little if any effect on the murder rate in the same year. It could, however, have a small effect, and then a larger effect in the following year, or even two years later. The dynamic impact of capital punishment, if any, is an empirical issue. We provide evidence that a modest effect does occur with a lag.

A simple equation that allows for a lagged effect of up to two years is:

$$(5) \qquad MURDRRATE_{it} = w_t + \beta_0 EXECRATE_{it} + \beta_1 EXECRATE_{i,t-1} + \beta_2 EXECRATE_{i,t-2} + C_i + U_{it}$$

The fixed effects estimator that we use controls for both the time and the county effects. Because we think the unobserved variables in u_{it} are probably correlated over time, we compute standard errors and *t* statistics that allow for any kind of correlations. See Wooldridge (2013, Chapter 14) for further discussion. In Stata, which we use for our analysis, the robust inference is obtained using the "cluster" option with Stata's fixed effects command.

IV. RESULTS

A. Empirical Findings

The estimated equations are reported in Table 5 and 6. Table 5 utilizes a fixed effects model while Table 6 differences then runs fixed effects. Row (1) of Tables 4 and 5 estimates the model in equation (1) without any lagged effects. The coefficient is approximately $-.023$ in Table 5 and

$-.005$ in Table 6. The cluster-robust t statistic is only -1.61 and -0.29 respectively. Thus, there is no evidence of a contemporaneous deterrent effect when county and year effects are included.

Table 5. Regression Coefficient Estimates

Murder Rate	Coefficient	P > |t|
Executions		
--.	-.023 (-1.61)	.107
L1	-.054 (-3.87)	.000***
L2	-.039 (-2.49)	.013*
L3	-.050 (-2.72)	.007**
L4	-.042 (-2.13)	.033*
L5	-.047 (-2.86)	.004**
L6	-.045 (-2.60)	.009**
L7	**-.067** **(-4.24)**	**.000*** **
L8	-.011 (-6.12)	.000***
Lpopul	-.549 (-4.12)	.000***

Note: Fixed effects model. Uniform Crime Report data for 2001 to 2003 is incomplete. Values in parenthesis are t-statistics. N = 67636 *p < .05, ** p < .01, and *** p< .001 levels (two-tailed test). All time lags are in yearly increments. Beginning with year one we use L1, L2, L3, L4, L5, L6, L7 and L8 to indicate the number of time lags. For differenced time lags we will use a similar notation with the exception of an upper-case D following the year (i.e. L1D). Time Lags do not progress beyond 8 years.

Table 6. Regression Coefficient Estimates w/ Differencing

D. Murder Rate	Model	P > |t|
Executions		
D1	-.005 (-0.29)	.733
L1D.	**-.036** **(-2.57)**	**.010***
L2D.	-.012 (-0.70)	.482
L3D.	-.019 (-0.76)	.449
L4D.	-.003 (0.12)	.903
L5D.	-.017 (-1.13)	.257
L6D.	-.017 (-1.19)	.236
L7D.	-.009 (-0.54)	.590
L8D.	-.021 (-1.10)	.272
Lpopul D1.	3.04 (1.29)	.196

Note: Table 6, fixed effects model with differencing. To account for trends in murder rates and executions that change at the county level we difference to remove the level and then used fixed effects on the differences. Now the coefficients are much smaller and statistically insignificant. So the failure to account for trends seems to be driving the fixed effects. Years 1978 – 1984 & 2000 – 2003 are omitted due to collinearity; incomplete Uniform Crime Report data for 2001 to 2003. Values in parenthesis are t-statistics. *N* = 59329 *p < .05, ** p < .01, and *** p < .001 levels (two-tailed test).

Row (2) of Tables 5 and 6 is inclusive of a one-year time lag of the execution rate variable. The fixed effects (Table 5) coefficient on the contemporaneous effect is $-.054$ with a t statistic of -3.87, which is statistically significant. The differenced fixed effects model (Table 6) is also statistically significant with a coefficient of $-.036$ and t statistic of -2.57. Both are statistically significant, however, the differenced model's coefficient of $-.036$ is smaller than the non-differentiated coefficient of $-.054$ meaning the differenced model has a significant but weaker deterrent effect.

For a more tangible result, we perform a transformation and present the coefficients as an estimate of the number of executions necessary to deter one murder per population of ten thousand. By dividing one by the coefficient we are provided with the number of executions it takes to deter one murder and therefore save one life. The smaller the coefficient the larger the result, therefore the differenced model's smaller coefficient of $-.036$ equates to a weaker deterrent effect. Performing the transformation on the differenced model, we find twenty-eight executions are necessary at a one-year lag to provide a deterrent effect. This is significantly larger than the nineteen executions necessary to save one life in the non-differenced model.

Differencing then running fixed effects has a weaker effect, as it requires more executions to deter one murder. Table 6 also shows there is only statistical significance at a one-year lag. In comparison, we find statistical significance out to eight time lags when performing fixed effects without differencing. Table 5's strongest effect occurs at lag 7 where a coefficient of $-.067$ with $t = -4.24$ equates to minimum of 15 executions necessary to save one life where as a high of 91 executions occurs at time lag 8 where the coefficient is smallest at $-.011$ with $t = -6.12$. Overall, we find that Table 6, differenced fixed effects has coefficients that are much smaller and statistically insignificant. We find that Table 5 has a negative statistically significant coefficient of $-.549$ with $t = -4.12$ when regressing the population on the murder rate while Table 6 has a positive non-statistically significant coefficient of 3.04 with $t = 1.29$. In tables 4a and 5a we extend our results to show effect by year.

Figure 2 is a scatter plot showing the relationship between the murder rate and execution rate by year. Demonstrated is the fact that there are many more murders than executions in a given year. This is a necessary condition, as we know from our discussions of auto-correlation that a murder is a precondition for an execution to take place. Therefore, endogeneity exists in the deterrence model. This relationship can be seen visually in a scatter plot matrix which are used to depict the linear correlation between multiple variables (Appendix Figure A2).

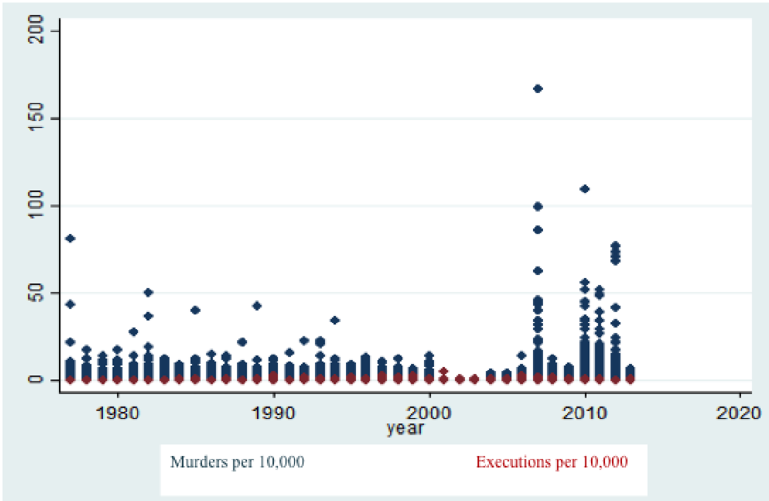

Figure 2. Scatter Plot of Murder Rate and Execution Rate by Year 1977 – 2013

Table 5a. Regression Coefficient Estimates

D. Murder Rate	Model	P > |t|
Executions		
--.	-.023 (-1.61)	.107
L1	-.054 (-3.87)	.000***
L2	-.039 (-2.49)	.013*
L3	-.050 (-2.72)	.007**
L4	-.042 (-2.13)	.033*
L5	-.047 (-2.86)	.004**
L6	-.045 (-2.60)	.009**
L7	-.067 (-4.24)	.000***
L8	-.011 (-6.12)	.000***
lpopul	-.549 (-4.12)	.000***
Year		
1986	-.020 (-0.88)	.377
1987	-.010 (-0.45)	.649
1988	-.028 (-1.47)	.142
1989	-.008 (-0.30)	.768
1990	.007 (0.29)	.770
1991	.009 (0.37)	.710
1992	-.002 (-0.09)	.929
1993	.033 (1.24)	.214
1994	-.006 (-0.27)	.785
1995	-.034 (-1.43)	.153
1996	-.043 (-1.63)	.104
1997	-.055 (-2.10)	.036*
1998	-.122 (-5.08)	.000***
1999	-.136 (-5.71)	.000***
2000	-.101 (-3.90)	.000***
2004	-.193 (-5.78)	.000***
2005	-.276 (-10.49)	.000***
2006	-.256 (-9.07)	.000***
2007	.324 (2.69)	.007**
2008	-.257 (-9.36)	.000***
2009	-.267 (-9.42)	.000***
2010	.193 (2.43)	.015*
2011	.109 (1.79)	.073
2012	.133 (1.71)	.088
2013	-.279 (-9.42)	.000*

Note: Table 5A, fixed effects model extended to show effect by year. Values in parenthesis are t-statistics. N = 67636 *p < .05, ** p < .01, and *** p < .001 levels (two-tailed test). Years 1978 – 1985 & 2000 – 2004 are omitted due to collinearity. Uniform Crime Report data for 2001 to 2003 is incomplete.

Table 6a. Regression Coefficient Estimates w/ Differencing

D. Murder Rate	Model	P > |t|
Executions		
D1	-.005 (-0.29)	.733
L1D.	-.036 (-2.57)	.010*
L2D.	-.012 (-0.70)	.482
L3D.	-.019 (-0.76)	.449
L4D.	-.003 (0.12)	.903
L5D.	-.017 (-1.13)	.257
L6D.	-.017 (-1.19)	.236
L7D.	-.009 (-0.54)	.590
L8D.	-.021 (-1.10)	.272
lpopul	3.304 (1.29)	.196
Year		
1985	-6.127 (-5.29)	.000***
1986	-5.710 (-5.27)	.000***
1987	-5.261 (-5.21)	.000***
1988	-4.841 (-5.18)	.000***
1989	-4.412 (-5.12)	.000***
1990	-3.964 (-5.05)	.000***
1991	-3.542 (-4.98)	.000***
1992	-3.132 (-4.93)	.000***
1993	-2.718 (-4.92)	.000***
1994	-2.351 (-4.95)	.000***
1995	-1.973 (-5.01)	.000***
1996	-1.562 (-4.93)	.000***
1997	-1.165 (-4.84)	.000***
1998	-0.784 (-4.72)	.000***
1999	-0.374 (-3.88)	.000***
2004	-3.775 (-5.55)	.000***
2005	-3.366 (-5.58)	.000***
2006	-2.932 (-5.57)	.000***
2007	-1.917 (-4.22)	.000***
2008	-2.087 (-5.59)	.000***
2009	-1.663 (-5.55)	.000***
2010	-0.821 (-3.70)	.000***
2011	-0.454 (-3.24)	.001**

Note: Table 6A, fixed effects model with differencing extended to show effect by year. Values in parenthesis are t-statistics. N = 59329 *p < .05, ** p < .01, and *** p < .001 levels (two-tailed test). Years 1978 – 1984 & 2000 – 2003 are omitted due to collinearity. Uniform Crime Report data for 2001 to 2003 is incomplete.

B. Fixed Effects with Differencing Results

To account for trends in murder rates and executions that change at the county level, we differenced to move the level and then used fixed effects on the differences. A fixed effects regression without differencing fails to account for the trend of differencing for murder rate. Now the coefficients are much smaller and statistically insignificant. The variation in results between the models is likely due to an underlying trend that is not being captured, had we found significance at other levels than L1 the variation across models would be less pronounced. We can only speculate as to what is driving this difference. It appears that failure to account for trends seems to be driving the fixed effects results.

C. Capital Punishment on Robbery

We model the effect of capital punishment on robbery with the expectation that execution rates have a limited influence on would-be robbers (Table 7). We rationalize that crimes not subject to capital punishment law will be limited in their interrelationship between execution rates and occurrence. We expect criminals whose crimes fall outside of these laws to be undeterred by others executions. As such, a differenced fixed effects model should result in no significant findings. Here, we use robbery as a proxy for such a crime finding significance one, two, five and eight lags. The inconsistency in results supports our hypothesis.

We attribute negative influences to spillover effects that executions have on deterring robbery. Due to the nature of robbery, murder cannot be ruled out as a potential outcome and can therefore have the potential of deterring would-be robbers. This is true particularly in instances of armed robbery. If a robber is prepared to kill during the course of the crime, he/she therefore will be affected by executions, explaining the negative coefficients depicting an inverse relationship. Compared to lesser crimes, the thought process of a robber is likely to be greyed by the potentiality of murder when proceeding with such crimes.

For juxtaposition, we suggest in future studies the utilization of crimes such as larceny or motor theft as the incidence of murder during these acts is far less likely. Both crimes are captured by the UCR; however, we leave out these examinations in this study, as these crimes are not consistently covered by UCR for the time period we examine. Studies examining more recent periods can use these proxies as means for testing the effectiveness of their models. Though we include calculations of the spillover effect of execution on robbery, its precise measurement is outside the scope of this study. We include these results only as means of affirming differenced fixed effects' veracity.

Table 7. Impact of Execution on Robbery

D. Robrate	Model	P > | t|
Executions		
D1.	-0.482 (-0.28)	.782
LD.	**-4.407** **(-2.42)**	**.015***
L2D.	-3.325 (-1.67)	.095
L3D.	-2.329 (-0.74)	.461
L4D.	-0.467 (-0.15)	.878
L5D.	**-4.268** **(-2.04)**	**.041***
L6D.	-1.582 (-0.74)	.461
L7D.	-2.341 (-1.07)	.283
L8D.	**-5.551** **(-2.40)**	**.016***
Lpopul D1.	-26.541 (-0.13)	.900

Note: Table 7, fixed effects model with differencing. We use a differenced fixed effects regression to model the effect of capital punishment on robbery. We expect criminals whose crimes fall outside of these laws to be undeterred by others executions. As such, a differenced fixed effects model should result in no significant findings. Here, we use robbery as a proxy for such a crime finding significance at time lags one, five and eight. The inconsistency in results supports our hypothesis. Years 1978 – 1985 & 2000 – 2004 are omitted due to collinearity; incomplete Uniform Crime Report data for 2001 to 2003. Values in parenthesis are t-statistics. N = 59330 *p < .05, ** p < .01, and *** p < .001 levels (two-tailed test).

Table 7a. Impact of Execution on Robbery

D. Murder Rate	Model	P > |t|
Executions	-0.482 (-0.28)	.782
D1.	-4.407 (-2.42)	.015*
LD.	-3.325 (-1.67)	.095
L2D.	-2.329 (-0.74)	.461
L3D.	-0.467 (-0.15)	.878
L4D.	-4.268 (-2.04)	.041*
L5D.	-1.582 (-0.74)	.461
L6D.	-2.341 (-1.07)	.283
L7D.	-5.551 (-2.40)	.016*
L8D.	-26.541 (-0.13)	.900
Lpopul D1.	-26.541 (-0.13)	.900
Year		
1986	3.814 (2.20)	.028*
1987	2.085 (0.64)	.522
1988	3.827 (0.89)	.375
1989	5.565 (1.02)	.309
1990	7.045 (1.22)	.224
1991	11.282 (1.83)	.067
1992	12.229 (1.89)	.059
1993	11.445 (2.38)	.017*
1994	12.406 (2.99)	.003**
1995	10.976 (3.28)	.001**
1996	8.188 (2.77)	.006**
1997	7.588 (2.52)	.012*
1998	2.857 (0.87)	.382
1999	-1.300 (-0.35)	.729
2005	.496 (0.69)	.498
2006	1.339 (1.12)	.262
2007	79.902 (4.46)	.000***
2008	-2.081 (-0.34)	.733
2009	-3.515 (-0.54)	.587
2010	70.201 (3.26)	.001**
2011	45.163 (4.00)	0.00***
2012	52.910 (3.51)	0.00***
2013	-5.314 (-0.65)	0.514

Note: Table 7A, fixed effects model with differencing extended to show effect by year. Values in parenthesis are t-statistics. *N* = 59330 *p < .05, ** p < .01, and *** p < .001 levels (two-tailed test). Years 1978 – 1985 & 2000 – 2004 are omitted due to collinearity. Uniform Crime Report data for 2001 to 2003 is incomplete.

D. Capital Punishment on Rape

We do not measure the effect of capital punishment on rape as it outside the focus of our study. We seek to measure the death penalty's effect on murder, however, a follow-up study focusing on capital punishment and rape may be warranted. For those conducting these studies, we advise an abundance of caution. It is likely that capital punishment will have no direct effect on a rapist's behavior as there are no laws in the United States resulting in capital punishment for committing rape.

Future studies looking at specific crimes such as rape may suffer from multicollinearity, more specifically the dummy variable trap.[22] The death penalty is only an eligible sentence for rapes resulting in murder, therefore conventional economic models would classify rapes into one of two categories: rapes ending in murder or rapes ending in non-murder. This is problematic because murder is an all encompassing variable (Wooldridge 2013). To directly measure capital punishment on rape, instances must be selected where both rape and murder occur. This also leaves the question of how to measure the death penalty's effect on rapes that do not result in murder. Because we are controlling for rape this is no small issue. Like robberies, we expect execution rates have a limited influence on would-be rapist.

This problem could be solved if there were a sample of cases where execution was given as a punishment for rape without murder. This provides a direct link between rape and capital punishment. Studies arguing the use of proxies would be measuring spillover effects.

Post *Gregg v. Georgia* (1976) there are no such cases where an individual has been put to death for crimes involving rape without murder. In an econometric model this is problematic because every rape must include murder to be directly influenced by capital punishment. For convictions, the sample size is extremely small. Only two persons have been given the death penalty for rape. Neither case resulted in execution.

In 1977 *Coker v. Georgia*, the Supreme Court ruled the death penalty for the rape of an adult to be "grossly disproportionate" and an "excessive punishment" thereby making it unconstitutional under the Eighth Amendment. In 2003 Patrick Kennedy was given the death penalty for the rape of his step-daughter, however, in 2008 *Kennedy v. Louisiana*, the Supreme court ruled 5-4 in favor of ruling it unconstitutional to sentence individuals to death for the rape of a child (Deathpenaltyinfo.org). In 1964, Ronald Wolfe became the last person to executed rape. For this reason, we suggest a better measure of the effect of capital punishment on rape in the United States would be to study cases pre-dating 1972.

V. CONCLUSION

We find a slight deterrent effect that is much smaller than that found by Ehrlich (1975) and Dezhbakhsh, Rubin and Shepherd (2003). Using a differenced fixed effects model we estimate that it takes approximately twenty-eight executions to deter one murder in a population of ten thousand.[23] We find significance at a one-year time lag identifying a coefficient of $-.036$ and a t statistic of -2.57. Our results provide an inverse to the eight lives saved per execution ratio found by Ehrlich (1975) or the eighteen lives per execution found by Dezhbakhsh, Rubin, and Shepherd (2003). Both sets of results are in terms of lives saved per execution. To put our estimate in their terms, we divided the number of executions necessary to save one life by one and find that one execution saves one twenty-eighth of a life.

At these numbers the death penalty is nowhere near one to one in its outcome (one execution saves one life). Ehrlich and DRS's studies had outcomes greater than one. Accepting these numbers the death penalty comes at a high cost. We find that the smaller the coefficient the more executions it will take to deter one crime. We understand, however, that opinions may vary on whether the death penalty is desirable at these rates as some would argue that the death penalty is valuable even if a one to one relationship does not exists. It can be argued that capital punishment has some deterrent effect and is therefore warranted.

As economists, this is where we leave the debate, as the philosophy of morality is outside of our field of study. Our aim with this study is to provide lawmakers and scholars with accurate facts to support these types of discussions and to make policy decisions. Current and accurate data can inform policymakers as they decide whether or not to uphold current statutes. With this study, death penalty and non-death penalty advocates now have proper analysis relevant to their decision-making.

A. Future Study

When questioning the certainty of our results, one must realize our model does not purport to be 100% accurate. Rather, we aim to provide the best available information to date with the most recent data covering a time span longer than any other capital punishment study. Our study is broad in its approach, capturing the influence of the effects of capital punishment across all demographic sectors, to include race, age, gender, and wealth. The research could be expanded to break down the study amongst any, or all, of the subfields to examine the effects of capital punishment on specific demographic sectors.

For future studies we would like to model other proxy crimes such as larceny to see the effects of executions on crimes ineligible for the death penalty. These cross-examinations could not only be compared to our findings, but they could also address the issue of spillover effects and the possibility of the death penalty having an influence on lesser crimes.

We also would like to examine race in an upcoming study. Using U.S. Census data we could perform examinations of race and age and their influence on outcomes. We could select various age groups and different racial groups to determine if the death penalty affects these groups disproportionately. Data shows the death penalty is applied disproportionately across racial groups, with blacks having the highest sentencing rates: 42% of the death row population is made up of blacks, though blacks make up only 13% of the population.

The census also provides data that could be used to perform analysis along gender lines. Since 1976 there have been fifteen women executed in the U.S. Currently, there are thirty-four women on death row, which makes up 1.8% of the death row population (NAACP 2015). Likewise, data exists that shows socioeconomic status plays a role in sentencing. We could conduct a study utilizing demographic data such as household income, poverty rates, and/or employment status to examine these subfields in depth.

The Supreme Court's ruling in *Furman v. Georgia 1972* requires death penalty laws be applied equally to all ethnic and socioeconomic groups. U.S. Census data, as previously referenced, indicates such is not the case. Fixed effects regression is not required to demonstrate the disparity. All one has to do is look at summary statistics.

Future studies should aim to include more information from the FBI's *Uniform Crime Report* (UCR) to account for trends in the data missed by our model. Though we advocate the use of widely accepted variables that influence crime, we are cautious against returning to OLS and 2SLS for analysis. It is improbable for these models to capture all the modifying variables. We recommend fixed effects regressions or similar methods that involve large data sets that are extended over time. This enables the models to incorporate unobserved variables for increased accuracy. We advocate that a large panel data set include at least twenty years of data to help mitigate omitted variable bias.

Having more information will provide a more detailed picture as to how well our model is performing. Using data from the UCR for crimes that do not qualify for the death penalty, such as larceny, could be compared to crimes such as murder, which can lead to the death penalty. In practice, those committing crimes such as larceny should go unfettered by the death penalty, as these individual's crimes do not lead to the death penalty. If the death penalty is not a possible punishment then those crimes that do not meet death penalty criteria should be undeterred as they will not receive such punishment. Using this comparison, if a significant decrease in larceny rates occurs in relation to executions then we know our model is missing important trends, which should not be the case.

B. The Death Penalty Today

Executions are at an all-time low as states deal with ethical dilemmas caused by a shortage of lethal injection drugs. In 2011, European countries placed an export ban on pharmaceutical drugs used to carry out executions in the United States. A key anesthetic, sodium thiopental, has now become so scarce that states have resorted to experimenting with new drugs inducing a spike in the number botched executions across the country. These botched executions at times leave the individual gasping for air and convulsing for long periods of time. In response, many governors have been forced to indefinitely suspend executions until another method of execution can be found. It is not just European drug manufactures that have stopped supplying states with lethal injection drugs. Pressure from activists and fear of lawsuits are reasons not to sell sodium thiopental and other lethal-injection drugs to U.S. states (Ford 2015).

There are four states, Washington, Oregon, Colorado and Pennsylvania, with governor-imposed moratoriums. Arizona, Nevada and Oklahoma have suspended executions after botched executions, while Kansas and New Hampshire have not carried out an execution since 1976 (Appendix Figure A1). California by far has the largest death row population, seven hundred forty-six, followed by Florida with four hundred. Even with the largest death row population, California has not carried out an execution since 2006, when Clarence Ray Allen was executed after 23 years and one month on death row (Deathpenaltlyinfo.org) [24].

Allen was on death row for more than two decades, which is indicative of the upward trend in the time between sentencing and execution (Figure 3). We find that from 1984 to 2013 the average time between sentencing and executions rapidly rose, adding to a state's predicament. In 2014, Nebraska cited costs, irregular use, and delayed time between sentencing and executions as reason for its May 26, 2014 decision to abolish the death penalty. At the time, Nebraska had an average lag time of nineteen years between sentencing and execution.

Using anecdotal evidence we have shown that the preconditions of *Furman v.*

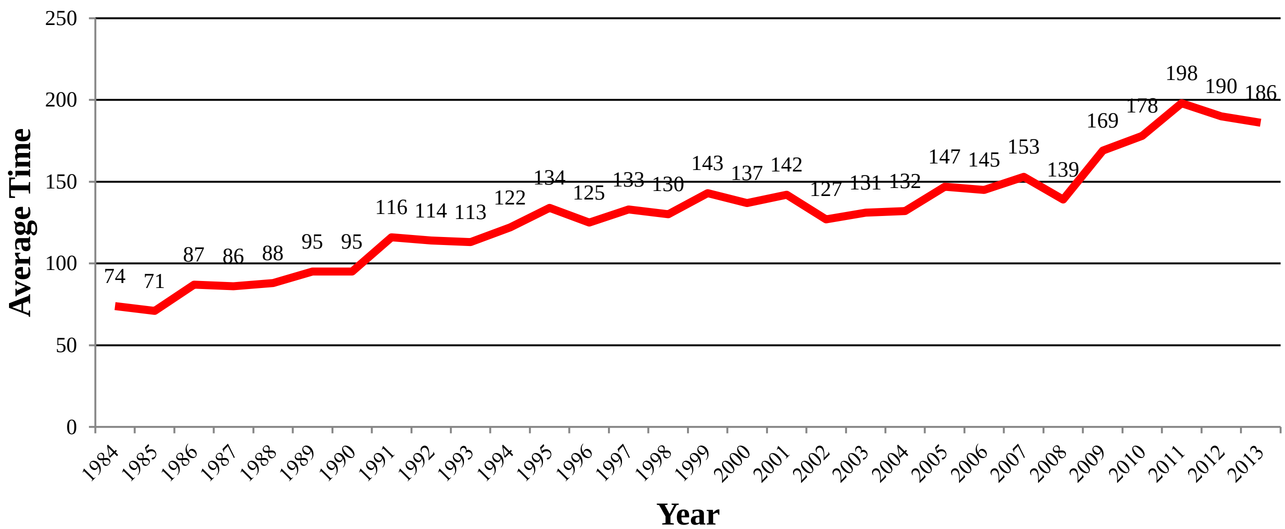

Figure 3. Average Time between Sentencing and Execution, 1985 – 2013

Georgia 1972 have not been met. States disproportionately apply the death penalty not only along race, sex or religion, but also by geographic location ranging from state to state and county to county. An individual in Texas can be executed, while an individual in New York who commits and act of equal or increased brutality receives a lesser sentence. This also happens at the county level, such as in Texas where one third of the counties have never carried out an execution.

Lastly, the justification for the death penalty comes at a high cost. Twenty executions must occur to deter one murder. In his original 1975 study, Ehrlich found that one execution deterred eight murders. DRS (2003) found that one execution saves eighteen lives plus or minus ten. Our solution can be restated in terms of lives saved per execution as opposed to our calculation of the number executions to deter one murder. Instead of stating twenty-eight executions deter one murder, we would state one execution saves $1/28^{th}$ of a life.

VI. APPENDIX

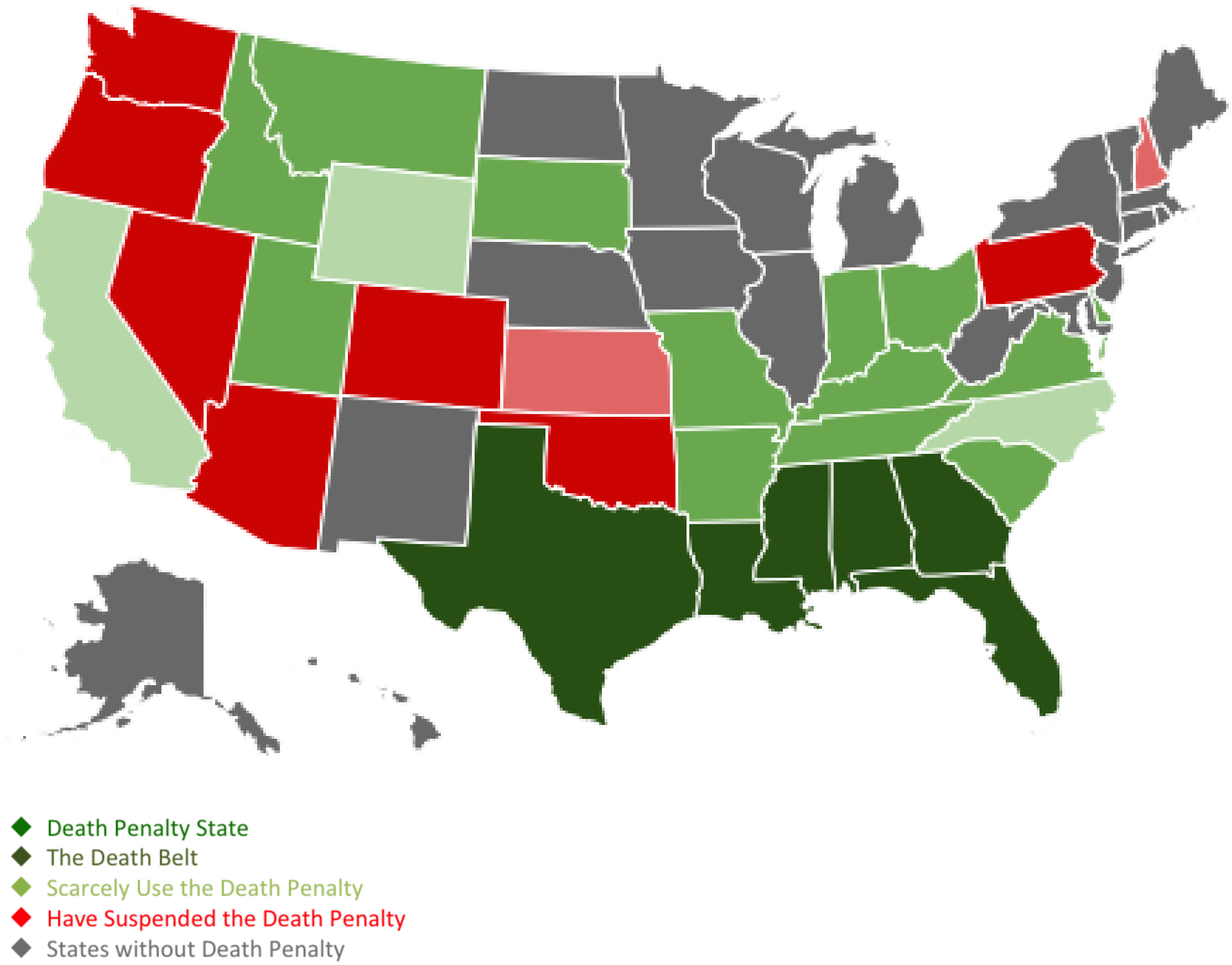

Figure A1. Death Penalty Map

Table A1. Top 40 Counties

Count	State	County	Count
1.	TX	Harris	123
2.	TX	Dallas	53
3.	OK	Oklahoma	41
4.	TX	Bexar	41
5.	TX	Tarrant	38
6.	MO	St. Louis	16
7.	OK	Tulsa	16
8.	TX	Montgomery	16
9.	TX	Jefferson	15
10.	MO	St. Louis City	14
11.	FL	Miami-Dade	12
12.	TX	Brazos	12
13.	TX	Nueces	12
14.	AZ	Maricopa	11
15.	TX	Potter	11
16.	TX	Smith	11
17.	AL	Jefferson	10
18.	AL	Mobile	10
19.	AZ	Pima	10
20.	OH	Hamilton	10
21.	TX	Lubbock	10
22.	DE	New Castle	9
23.	VA	Prince William	9
24.	FL	Orange	8
25.	MO	Jackson	8
26.	NV	Clark	8
27.	OH	Cuyahoga	8
28.	SC	Charleston	8
29.	TX	McLennan	8
30.	TX	Travis	8
31.	VA	Chesterfield	8
32.	VA	Virginia Beach City	8
33.	DE	Kent	6
34.	FL	Duval	6
35.	FL	Pinellas	6
36.	OH	Summit	6
37.	OK	Comanche	6
38.	TX	Cameron	6
39.	TX	Galveston	6
40.	TX	Navarro	6

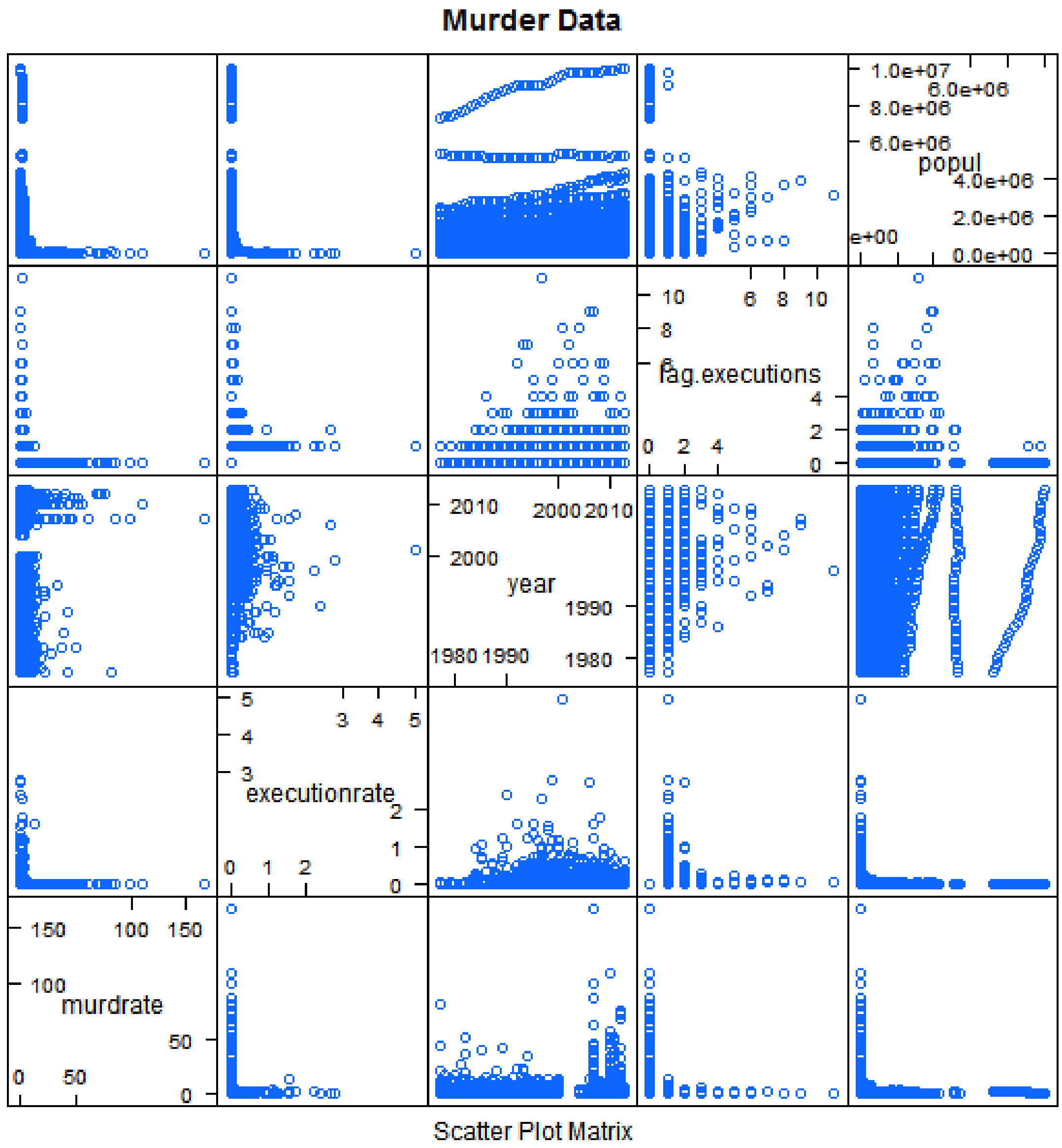

Figure A2. Scatter Plot Matrix Measuring Linear Relationships Among Variables

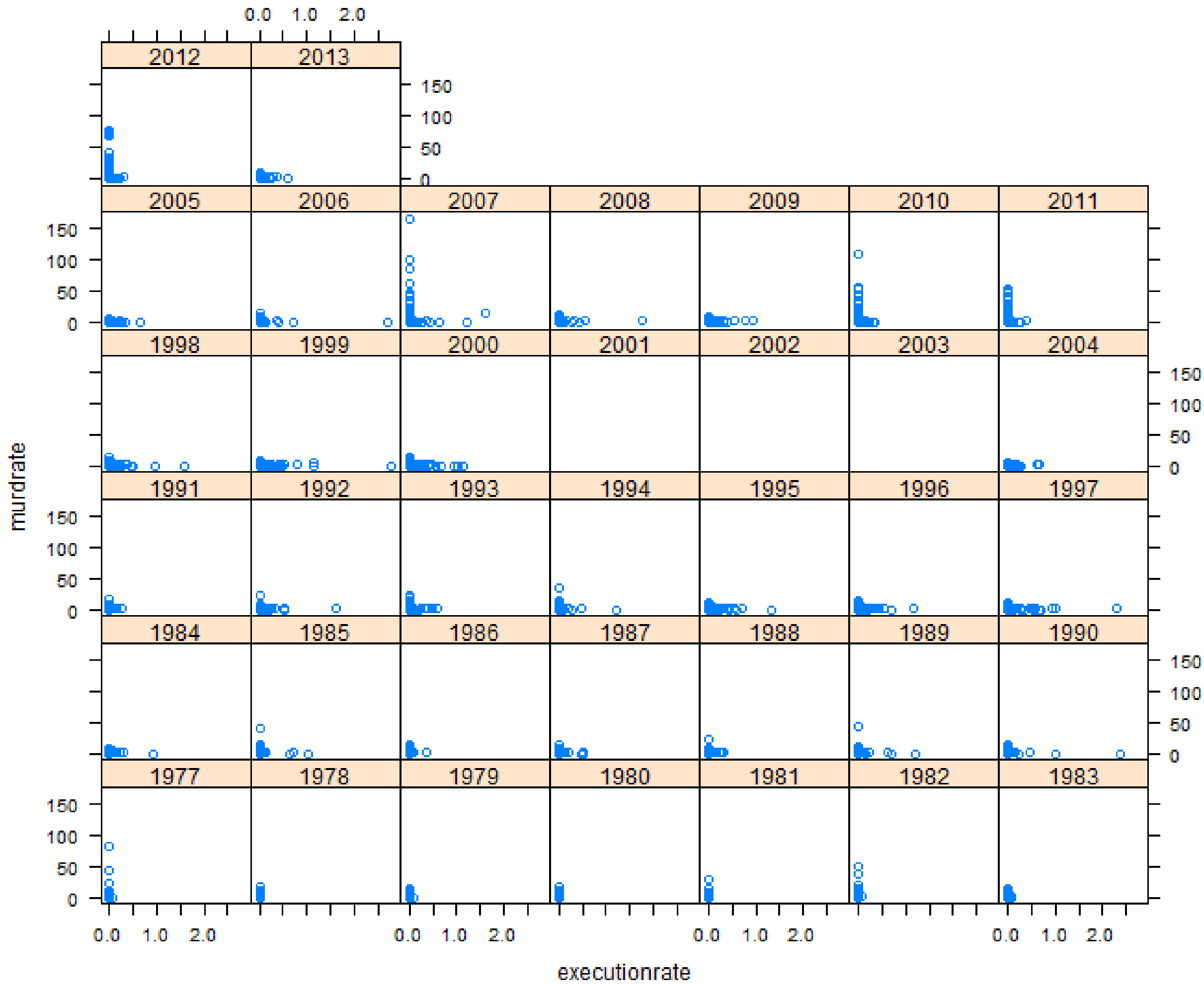

Figure A3. Murder to Execution Rate 1983 – 2013

Table A2. Federal Murder Sentencing Guidelines

Offense	Definition	Mandatory Sentencing
Involuntary Manslaughter	Unintentional; intentional or negligent act leading to death	Fine or up to 8 years imprisonment
Voluntary Manslaughter	Intentional; no prior intent to kill, "Heat of Passion"	Fine or up to 15 years imprisonment
Second Degree Murder	Intentional; not premeditated/planned	Any term up to Life Imprisonment
First Degree Murder	Intentional; willful, deliberate, pre-meditated	Life imprisonment or Death Sentence

Note[3] 428 US 253 (1976)
Gregg v. Georgia (No.74-6257)
Dissent, Thurgood Marshall {p.233-236}

"The Solicitor General in his amicus brief in these cases relies heavily on a study by Isaac Ehrlich, reported a year after Furman*, to support the contention that the death penalty does deter murder. Since the Ehrlich study was not available at the time of Furman and since it is the first scientific study to suggest the death penalty may have a deterrent effect, I will briefly consider its import.*

The Ehrlich study focused on the relationship in the nation as a whole between the homicide rate and "execution risk" - the fraction of persons convicted of murder who were actually executed. Comparing the differences in homicide rate and execution risk for the years 1933 to 1969, Ehrlich found that increases in execution risk were associated with increases in the homicide rate. But when he employed the statistical technique of multiple regression analysis to control for the influence of other variables posited to have an impact on the homicide rate, Ehrlich found a negative correlation between changes in the homicide rate and changes in execution risk. His tentative conclusion was that for the period from 1933 to 1967 each additional execution in the United States might have saved eight lives.

The methods and conclusions of the Ehrlich study have been severely criticized on a number of grounds. It has been suggested, for example, that the study is defective because it compares execution and homicide rates on a nationwide, rather than a state-by-state, basis. The aggregation of data from all states—including those that have abolished the death penalty—obscures the relationship between murder and execution rates. Under Ehrlich's methodology, a decrease in the execution risk in one state combined with an increase in the murder rate in another state would, all other things being equal, suggest a deterrent effect that quite obviously would not exist. Indeed, a deterrent effect would be suggested if, once again all other things being equal, one state abolished the death penalty and experienced no change in the murder rate, while another state experienced an increase in the murder rate.

The most compelling criticism of the Ehrlich study is that its conclusions are extremely sensitive to the choice of the time period included in the regression analysis. Analysis of Ehrlich's data reveals that all empirical support for the deterrent effect of capital punishment disappears when the five most recent years are removed from his time series—that is to say, whether a decrease in the execution risk corresponds to an increase or a decrease in the murder rate depends on the ending point of the sample period. This finding has cast severe doubts on the reliability of Ehrlich's tentative conclusions. Indeed, a recent regression study, based on Ehrlich's theoretical model but using cross-section state data for the years 1950 and 1960, found no support for the conclusion that executions act as a deterrent.

The Ehrlich study, in short, is of little, if any, assistance in assessing the deterrent impact of the death penalty."

VII. REFERENCES

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VIII. ENDNOTES

[1] Becker is credited as the first economist to combine economic theory and human behavior. His work, *"Crime and Punishment: An Economic Approach,"* brought about a transformation in the way criminal behavior is studied. Becker's work popularized the study of behavioral economics which covers wide-raging topics. For more on the broader uses of behavioral economics see Dan Ariely' *"Predictably Irrational: The Hidden Forces That Shape Our Decisions"* and Daniel Kahneman's *"Thinking Fast and Slow*".
[2] *See* Banner (2003) for a complete history of the death penalty.
[3] Furman v. Georgia, 408 U.S. 238, 309 (1972) (Stewart, J., concurring).
[4] Note: *Gregg v. Georgia Dissent* (Thurgood Marshall 1976).
[5] See Appendix Table A2.
[6] Chalfin, Haviland and Raphael [CHR] (2012) review the use of panel data to measure murder deterrence. See "*Existing Panel Data Studies"* for review of Dezhbakhsh, Rubin, and Shepherd (2003).
[7] Executed June 11, 2001. Carried out the Oklahoma City bombing killing 168 people and injuring over 600.
[8] See Table 4 for a comparison of studies.
[9] Martz and Targonski (2002) Reporting errors are inconsistent and highly non-random with certain jurisdictions and certain states reporting more consistently than others. Martz and Targonski provide detailed analysis of the UCR concluding, "…county-level crime data cannot be used with any degree of confidence." (CHR 2012).
[10] As of April 1, 2015 *See Death Row U.S.A. Spring 2015.* Statistics includes federal and U.S. military death row populations.
[11] New Mexico (2009) and Connecticut (2012) both repealed their death penalty statutes, but not retroactively, leaving 2 and 12 persons on death row respectively. *See Table 1 for complete list.*
[12] *Furman v. Georgia,* 408 U.S. 238, 365 (1972) (Marshall, concurring).
[13] See Appendix Table 1 for a list of 40 counties with the highest number executions
[14] As of the date of this publication, May 12th, 2015.
[15] Top 10 Texas counties: Harris (123), Dallas (53), Bexar (41), Tarrant (38), Montgomery (16), Jefferson (15), Brazos (12), Nueces (12), Potter (11) and Smith (11). [332/523≈.63]
[16] *See Texas Population, 2015 (Projections)*, Texas Department of State Health Services (DSHS). Bexar (1,882,834), Dallas (2,496,859), Harris (4,471,427), Tarrant (1,959,449).

[17] February 2011, Harris County opened a public defender office as a pilot project (http://www.chron.com/news/houston-texas/article/Harris-County-taps-experienced-hand-for-public-1702895.php).
[18]Note:
North East States: CT, NH, NJ, NY, PA, RI, VT
Mid-West States: IL, IN, KS, MO, NE, OH, SD
South States: AL, AR, DE, FL, GA, KY, LA, MD, MS, NC, OK, SC, TN, TX, VA
West States: AZ, CA, CO, ID, MT, NV, NM, OR, UT, WA, WY
Source: http://www.deathpenaltyinfo.org/death-sentences-united-states-1977-2008?scid=9&did=847
[19] See Smith (2012) for more on the geography of the death penalty.
[20] *See* Donahue and Wolfers (2009) Table 2.
[21] Figure 2. Average time between sentencing and execution (Snell 2014).
[22] The Dummy variable trap occurs when an overall intercept is in the model and a dummy variable is included for each group (Wooldridge 2013, Chapter 7).
[23] Murder and execution rate is per every 10,000 people in the population. Calculation for differenced fixed model in terms of the number of executions necessary to save one life [1/-.036 ≈ 27.778].
[24] As of July 1, 2015.