The Local Effect of Executions on Serious Crime

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Abstract

The death penalty is arguably the most controversial criminal justice policy in the US. Despite an extensive body of work, there is no credible causal evidence that it affects crime. In this paper, we show that executions cause a local reduction in serious violent crime (homicides, rapes and assaults with weapon). A simple behavioural model predicts that highly publicised executions reduce crime, albeit only temporarily and as long as capital punishment remains relatively rare. We test these predictions using a panel dataset disaggregated at the county-date level. Controlling for date and county-month fixed effects, we find that serious crime is lower in the days surrounding an execution, in the county where the capital offense was originally committed. An event study analysis and a set of robustness tests reinforce this conclusion. The effect is decreasing in the number of recent executions in the county, and it is higher for executions associated with a lot of media attention. Counties neighbouring the original-crime county also experience a significant, albeit smaller, decrease in crime.

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1 Introduction

The death penalty is arguably the most controversial criminal justice policy in the US. Its divisiveness in the US political debate is demonstrated by the differences across states in its application, by the back-and-forth Supreme Court decisions on its constitutionality, and by the number of pressure groups devoted to advocate against (and sometimes for) it. Proponents claim that it generates respect for law and order and that it constitutes justifiable retribution for heinous crimes. On the opposite side it is argued that it cheapens the value of life and that its uneven use contributes to perpetuate social injustices.

A core issue from an academic perspective is whether the death penalty reduces serious crime, most intuitively through a deterrence mechanism.[1] Unfortunately empirical evidence studying the link between the death penalty and serious crime remains highly flawed, most importantly because of its inability to identify causal effects (Donohue and Wolfers 2005, 2009, National Research Council 2012, Charles and Durlauf 2013, Nagin 2013). Studies using state-year panel datasets are, for instance, hampered by the likely correlation between unobserved determinants of crime (including other features of the sanctions regime) and a state-year use of the death penalty. The instrumental variables sometimes used to account for this endogeneity have been shown to be not credible (Donohue and Wolfers, 2009).

In this paper we use a county-date panel dataset to provide causal evidence on the local effect of executions on crime. For every execution we identify both its exact date and the county where the crime motivating the execution was originally committed (the 'original-crime county'). We then investigate the existence of a local effect, i.e. an effect in the original-crime county and during the days surrounding the execution. Our focus is on serious but not necessarily capital-offense violent crime, as we believe that highly salient penalties could have effects that spill over into criminal behaviour not specifically covered by such penalties.[2]

We start the paper with a simple behavioural model, which we use to understand the rationale for a short-term effect of executions on crime. In our model, the comparison be-

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[1]While deterrence is the most commonly discussed mechanism, National Research Council (2012) note that there are others, such as the social censure that is signaled by such an extreme form of punishment. On the opposite side, it has been argued that the death penalty has a brutalisation effect that leads to an increase in crime (Bailey 1998, Shepherd 2005). In a very different context, Chen (2017) argues that executions increased desertions among Irish soldiers in the WWI British army.

[2]Our measure of serious crime combines homicides, rapes and assaults with weapons. However, we also show the baseline results separately for each category of crime. There are several ways to understand these potential spillovers. In a purely Bayesian framework with partially uninformed criminals and correlated sanctions, learning about an execution could lead any violent offender to update on the likely punishment for his behaviour. In the behavioural model that we outline in Section 2, highly salient punishments could raise general awareness about the cost of crime even for non-capital offense criminals.

tween the cost and benefit of crime is moderated by a psychological variable which we label 'awareness' of the cost of crime (Loewenstein, 1996). Highly salient criminal punishments raise awareness, but can be forgotten as time passes (Mullainathan, 2002). The first prediction of the model is that executions reduce crime, although this effect is only temporary. The second prediction is that the additional effect of an execution decreases in the number of recent executions.

We test the first prediction by regressing serious crime on a local execution dummy, which takes value one for the original-crime county and a short time window around the execution date. The high granularity of the dataset permits the introduction of a rich set of controls (i.e. date and county-month fixed effects), which enhances the credibility of the estimates. For instance, executions are scheduled long in advance, and typically take place in prisons located far away from the original-crime county. Because of this, reverse causality is unlikely in our setting. Additionally, omitted variables such as the sanctions regime remain arguably constant during the short windows of time that we study. The identifying assumption, which we regard as plausible, is that executions do not occur in dates of abnormal levels of crime (in the original-crime county, relative to other counties).

The main finding of the paper is that serious crime is .1 units lower (around 20% of the sample mean) during the local execution window. This finding is robust to using either a one-day or a three-day window, and it is qualitatively similar when studying separately homicides, rapes and assaults with weapon. The baseline finding is also robust to controlling for state and date interactions, to using non-linear models such as the Poisson model, and to alternative sample choices. We complement the baseline specification with an event analysis to study the dynamics of the crime reduction around the execution date. The examination of the estimated leads and lags suggests that serious crime remains largely unchanged in the days leading up to an execution, decreases in the day before and for two additional days, and then returns to its pre-existing level.

We test the second prediction of the model by interacting the local execution variable with the number of past executions in the original-crime county and during the previous five years. The estimated interaction is positive and statistically significant, supporting the notion that the crime reduction effect of an additional execution is lower in counties that are highly prone to the application of the death penalty.

A limitation of the literature studying the effects of the death penalty is the inability to directly measure its impact on criminals' perceptions about the execution risk (Lochner, 2007). While we do not have access to perception measures in this paper, our empirical analysis takes seriously the notion that criminals must be made aware of the existence of

an execution, if such an event is going to affect their behavior. This notion provokes the focus on the original-crime county, as it is residents of this county that should be more likely to receive information (or to be affected by information) about an execution. We also test this notion directly by interacting the local execution variable with a measure of the media attention associated with that execution. We find that the baseline effect is stronger for executions that are associated with a lot of media interest, thus providing evidence on the mechanism through which execution events affect criminal behaviour.

While awareness about an execution is predicted to increase most in the location of the original crime, nearby locations may also be partially affected. We study this in the last heterogeneity exercise of the paper, where we allow the effect of an execution to differ non-parametrically depending on the distance between a county and the original-crime county. The plotted estimates are consistent with the notion that publicity about an execution is highest in the county most associated with it, and then dissipates as one moves away from such epicentre.

Despite the voluminous literature on the death penalty, our findings arguably represent the first causal evidence that executions have an effect on crime. The focus on short-term effects implies however that we are reluctant to draw policy conclusions of the type 'each execution prevents x units of crime'.[3] The amount of crime prevented could certainly be larger than what we identify here. It may be, for instance, that some criminals do not respond to the executions that comprise the variation in this paper, but would however react strongly if capital punishment disappeared completely from the statute book. This would be consistent with our first heterogeneity exercise, which suggests the presence of a non-linear relation between executions and crime. Conversely, the total amount of crime prevented could be lower than our estimates suggest. In particular, the focus on the short-term implies that we cannot comprehensively study whether the effects that we identify lead to a permanent reduction in crime, or instead whether they represent a temporal displacement of criminal activity.[4] As Chalfin and McCrary (2017) argue, both possibilities constitute evidence of responsiveness. However, from a policy perspective distinguishing between them is obviously important.

Our objective in this paper is modest, although at the same time essential given the importance and flaws in the death penalty literature. Prior to arguing that the death penalty

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[3]The reader interested in these prescriptions could consult Ehrlich (1975) or Dezhbakhsh et al. (2003), among others.

[4]We fail to find any evidence of displacement to the week after the execution. However, the lack of statistical power implies that we need to be cautious about this negative finding. The same lack of statistical power makes us reluctant to examine effects beyond a single week.

prevents enough crime to pass any type of cost-benefit analysis, we believe it is important to credibly show that executions affect crime *at all*. We interpret our findings as providing an essential first step in the academic evaluation of the crime effects of the death penalty.

Related Literature The most influential study on the effect of the death penalty is probably Ehrlich (1975), who claimed that each execution saved on average eight lives. This study was comprehensively criticised by a National Research Council report (1978), which successfully defined for two decades the limits of scientific knowledge on the effects of the death penalty.

In the 2000s, a group of studies used state-year panel datasets and often claimed to find very large deterrence effects (Dezbakhsh et al. 2003, Mocan and Gittings 2003, Zimmerman 2004, Ekelund et al. 2006, Kovandzic et al. 2009). These papers have in turn been subject to a wide array of persuasive criticisms (Donohue and Wolfers 2005, 2009, National Research Council 2012, Charles and Durlauf 2013, Nagin 2013). In addition to the endogeneity concerns mentioned earlier, several issues have been raised. Firstly, the fact that the number of executions per state-year is highly skewed implies that estimates are disproportionately caused by a small number of outlying observations (Berk, 2005). Related to this, within-state year-to-year variation in executions is typically low relative to variation in crime, and this makes it difficult to disentangle the effect of executions from other factors affecting the crime rate (Donohue and Wolfers, 2005). A second criticism is that studies have devoted insufficient attention to understanding conceptually how criminals could have altered their perceptions about the sanctions regime, in their potential responses to executions. Thirdly, there has been no consensus or clear criteria in these studies as to what the best empirical measure for execution risk is. This is problematic because estimates have been shown to be highly sensitive to the measure chosen. Lastly, these studies have typically assumed a common effect, while there are reasons to expect effects to vary widely across states.

Our empirical design alleviates or circumvents the limitations above. Firstly, the focus on the days surrounding executions and the original-crime counties facilitates inference. Awareness about an execution is likely to be higher on these counties and dates, so we maximise the statistical power of the study when focusing on them. Secondly, our simple theoretical model is explicit about the assumptions underlying a potential short-term reaction of criminals to news about an execution. The model suggests that this reaction should be stronger when following long periods without an execution, a prediction that we test in the data. Thirdly, the use of disaggregated data implies that the independent variable can only be binary, thereby reducing the scope for arbitrary choices in its generation. The use of a binary

independent variable also reduces concerns about outlying observations.[5] Lastly, we conduct a set of heterogeneity exercises, thereby allowing for the effects to differ across executions and counties.

Our paper is most related to a small set of studies that estimate the short-term effects of executions (Phillips 1982, Grogger 1990, Hjalmarsson 2009). For the reasons that we outlined above, the focus on the short-term constitutes definite progress in terms of identification. The drawbacks in these studies arise from the fact that they are typically single-jurisdiction time-series analyses.[6] In terms of identification, the lack of a control group accounting for potential time effects is an obvious limitation. The restriction to a single jurisdiction also limits the number of executions examined and therefore the power of the study. Lastly, the need for reasonable statistical power implies that the jurisdictions selected are frequent users of the death penalty. Our theoretical model predicts (and our empirical findings suggest) that these areas should precisely be those where the effect of an additional execution is lower.[7]

Lastly, a recent paper by Chen (2016) examines the effects of the death penalty in the British Army during the First World War. Using arguably exogenous variation in commutations and executions, he finds partial evidence that executing deserters reduced absences. Executing Irish soldiers did however reduce the legitimacy of the army and increased absences. Chen (2016) is undoubtedly a fascinating study. It however differs from this paper in its focus on different outcome variables and in its different historical context.

2 Conceptual Framework

In this section we outline a simple framework to rationalise why executions could have a (transitory) effect on crime. At the core of the model is the notion that psychological forces that can change significantly over short time periods can affect behaviour. The effect of these forces is distinct from the Bayesian updating on the expected consequences following conviction that is at the heart of the cost benefit analysis of crime (Becker, 1968). Our

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[5]The number of executions in our sample is 493. Naturally, they represent a very small proportion of county-date observations.

[6]Hjalmarsson (2009) is undoubtedly the best of the three studies. Her sample contains three cities: Dallas, Houston and San Antonio. She conducts the analyses as three separate time series regressions, but mentions that in unreported regressions the estimates are robust to the use of panel data.

[7]An additional body of work uses time series econometric methods to link capital punishment and crime (Stolzenberg and D'Alessio 2004, Land et al. 2009, Cochran et al. 1994, Bailey 1998). The major limitation of these studies is that they are not well placed to identify causal effects (Charles and Durlauf, 2013).

model instead builds upon ideas in psychology, criminology and behavioural economics.[8]

We assume that criminal acts yield instant utility u and the expected cost of crime is c. Note that c is time-invariant and therefore not affected by informational updating on the consequences of crime.[9] Instead we follow Loewenstein (1996) in assuming that the comparison between benefits and costs of actions is distorted by a psychological state s_t that we label 'awareness' of criminal consequences. Specifically, the individual commits a crime whenever $u > s_t c$. While we refer to s_t as 'awareness', it can be alternatively interpreted as the visceral factors of Loewenstein (1996), the perceived social norms of Matza (1964) or the level of self-control in Thaler and Shefrin (1981). The important assumption for our purposes is that s_t is affected by information on events such as executions, especially when these relate to individuals or locations that feel proximate to the decision-maker.

Define $x_j = 1$ as the occurrence of an event (such as a proximate execution) in period j. $x_j = 0$ if the event did not occur. Under full persistence, $s_t = 1$ if at least one event occurred in the past, and zero otherwise. We instead posit that the state variable awareness $s_t = s(X_t, W_t)$ is a function of the history of events $X_t = (x_1, ..., x_t)$ and of a corresponding system of weights $W_t = (w_{1t}, w_{2t}, ..., w_{tt})$. We assume a random recalling process where events increase awareness but their effect is forgotten over time (Mullainathan, 2002). In particular, awareness is a linear combination of past events, $s_t = \sum_{j=0}^{t} w_{jt} x_j / \sum_{j=0}^{t} x_j$, where the weight w_{jt} can be interpreted as the persistence of event x_j in s_t. This implies that at time t event x_j is remembered with probability w_{jt}, which we assume is given by:

$$w_{jt} = m + pR_{j,(t-1)} \tag{1}$$

where the term m is a baseline probability of recalling an event and $R_{j,(t-1)} = 1$ if the event j was remembered in the previous period. We assume that $\alpha \equiv m/(1-p)p < 1$.

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[8]For instance, the drift theory of crime states that criminals often develop the ability to temporally neutralize the internal cost of not complying with social norms (Matza, 1964). In a related explanation, Gottfredson and Hirschi (1990) argue that criminal acts are often non-controlled, impulsive, opportunistic and short-sighted. They then relate poor parental inputs and crime through the development of a low self-control capacity. Building upon the notion that moral codes are not objective or universal, Gibbs (1989) claims that public punishment actions communicate the signal that 'society condemns some acts'. In economics, Loewestein (1996, 2000) assumes that tastes and attention can be affected in the short run by emotions and drives. These visceral factors typically change fast and are predictably correlated with external circumstances. Laibson (2001) proposes a model where trivial variations in situational cues can elicit temporary but powerful changes in marginal utility. Card and Dahl (2011) empirically show that unexpected emotional cues can trigger violent actions by changing the reference point of a 'gain-loss' utility function.

[9]If that was the case, executions could affect these perceived consequences and therefore have medium or even long-term effects. This is of course unless criminals form their beliefs using signals from a short span of time.

The expected recalling probabilities are obtained by backward solving (1) for $E(w_{jt}|x_j)$ using the facts that $E(R_{j,(t-1)}|x_j) = w_{jt-1}$ and that $w_{jj} = 1$ (i.e. the individual always remembers what just happened). Therefore,

$$E(w_{jt}|x_j) = p\alpha + (1 - \alpha)p^{t-j}$$

This equation implies that recalling probabilities decay exponentially after an event if no new events occur. Denote $\widetilde{x}_t$ as the number of accumulated events before period t. The expected level of awareness is given by the following expression:

$$E(s_t|X_t) = p\alpha + \frac{(1 - \alpha)}{\widetilde{x}_t} \sum_{j=0}^{t} x_j p^{t-j}$$

From this expression we can easily derive our first result:

Result 1: *In a setting with imperfect (random) recall probabilities, a new event reduces crime temporarily.*

To prove this result, we calculate $\Delta_t = E(s_t|X_{t-1}, x_t = 1) - E(s_t|X_{t-1}, x_t = 0)$ for a given sequence of events X_{t-1}. We find that:

$$\Delta_t = \frac{1 - \alpha}{\widetilde{x}_t(\widetilde{x}_t + 1)} \sum_{j=0}^{t}(1 - p^{t-j})x_j \tag{2}$$

The assumption that $\alpha < 1$ implies that $\Delta_t > 0$. Because the individual commits a crime whenever $u < s_t c$, this implies that crime is (weakly) reduced when there is a new event. The fact that $E(w_{jt}|x_j)$ decreases over time implies that this negative effect fades over time.

Result 2: *In a setting with imperfect (random) recall probabilities, the effect of a new event decreases with the number of accumulated past events.*

It is straightforward to show in equation (2) that $\frac{\partial^2 \Delta_t}{\partial x_j \partial \widetilde{x}_t} < 0$. Intuitively, the marginal effect of a new execution is lower in counties with lots of executions.

Mapping to the Empirical Exercise

3   Data

We use several data sources to construct an unbalanced date and county panel dataset. The main variables are the level of crime and the presence of an execution caused by a crime

in that county. Table 1 displays a set of descriptive statistics. Figure 1 displays the set of counties in the sample, and highlights the counties with at least one associated execution during the period that the county appears in the panel.

The information on criminal activity is extracted from the National Incident-Based Reporting System (NIBRS). The NIBRS is a voluntary program where participating law enforcement agencies report detailed information to the FBI, on a monthly basis. Our sample contains every county and month reported to the NIBRS during the 1997-2015 period. Note however that the number of counties covered has been increasing over time. Around 30% of the US population was covered in 2013, up from close to zero in 1997.[10] The number of county-date observations in the main estimating sample is 8,462,202.

We use information at the incident level, taking into account that an incident can be associated with multiple offences (e.g. an armed assault being inputted as an assault and an illegal carrying of weapons). Our dependent variable 'serious crimes' is the sum of homicides (murders and non-negligent manslaughters), rapes (forcible rapes and sex assaults) and aggravated assaults accompanied by the use of a fire weapon.[11] In Panel B of Table 1 we find that the average of serious crimes per county-day is .14. This low number results both the relative rareness of these crimes and the fact that many counties covered by the NIBRS are quite small. Rapes and assault with weapons are similarly prevalent, with homicides being less common.

The data on executions is extracted from the website *https:deathpenaltyinfo.org.* For every execution we identify the county where the capital offense was originally committed (i.e. the 'original-crime county') and use this to compute our main independent variables of interest. In total, we find 493 executions for which the original-crime county is covered by the NIBRS during the month on which the execution takes place (see Panel A of Table 1). These executions occur in 143 different counties, with an average of 3.45 executions per county.

Lastly, we use 'death penalty' search results from Google Trends to measure the media attention received by each execution. This data is available only from 2004 onwards. Google Trends does not report the total number of searches, but instead an index of the relative intensity of searches. We compute our measure in the following way. We first compute g_{st}, which is the index for state s (relative to all states in the US) in date t. We then use US-aggregate information to calculate z_{mt}, which is the index for date t in the month m to

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[10]Our empirical strategy, which controls for the interaction of county, month and year, is designed to account for within-county changes in coverage over time.

[11]Aggravated assaults are those regarded as unlawful attacks for the purpose of inflicting severe or aggravated bodily injuries.

which t belongs.

Our measure of death-penalty related media attention in a state-date combination is $M_{st} = g_{st} \times z_{mt}$. The measure captures the relative interest in death-penalty related topics within a state and date. Note that by construction this measure is normalised by the amount of media attention present within a particular month. It is therefore orthogonal to the large increase in the use and coverage of the internet over our sample period.

4 Event Study Analysis

In this section we study the evolution of serious crime in the days preceding and following an execution, in the county where (many years earlier) the crime leading to that execution was originally committed. Our main independent variable of interest is the dummy $Execution_{jit}$, which takes value one (for a county i date t combination) on the day j relative to the date of an execution motivated by a crime in that county. The estimating equation is:

$$crime_{it} = \sum_{j=-6}^{+6} \beta_j Execution_{jit} + \gamma_t + (\alpha_i \times \pi_{m(t)} \times \lambda_{y(t)}) + \epsilon_{it} \qquad (3)$$

where $crime_{it}$ is the number of crimes, γ_t represents a set of date indicators and ($\alpha_i \times \pi_{m(t)} \times \lambda_{y(t)}$) is a set of interactions between county, month and year indicators. The date indicators absorb any US-wide shocks to crime occurring on a particular date.[12] Including the interacted county, year and month indicators controls for any county-specific shocks within the relatively narrow time window of a month. Standard errors are clustered at the state and year level, a choice that we regard as conservative.

The identifying assumption implicit in the estimation of (3) is that executions are not scheduled to take place on days of idiosyncratically high or low crime in the original-crime county, relative to other counties on the same date and to that same county on that same month. We regard this assumption as plausible. Executions are scheduled well in advance and they take place in a single maximum-security prison per state. The prison is typically not located in the county where the original crime occurred.[13]

Figure 2 displays the estimated effects $\hat{\beta}_{-6} \ldots \hat{\beta}_{+6}$, using the number of serious crimes (homicides, rapes and assaults with weapon) as dependent variable. We find that crime

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[12]In one of the robustness tests, we find that the results are essentially unchanged when interacting the date indicators with state indicators.

[13]The Death Penalty Information Center maintains a list of upcoming executions (https://deathpenaltyinfo.org/upcoming-executions). Executions are scheduled up to five years in advance. For a list of the prisons where executions take place in each state, see https://en.wikipedia.org/wiki/Execution_chamber.

remains largely stable prior to the eve of an execution (the dip at $j = -4$ is not statistically significant). A statistically significant decrease on the eve of the execution persists approximately for two additional days, and then crime returns to its pre-existing level.[14] Figure A1 in the Appendix shows that the estimates are essentially unchanged when controlling for the interaction of date and state indicators.

We interpret the evidence in Figure 2 as indicating that executions have a negative effect on crime. This negative effect is consistent with news about the execution affecting would-be criminals' awareness of the potential consequences of crime. Figure 2 further indicates that this effect is very short lived, lasting around three days. In light of this evidence and in order to increase statistical power, our baseline regressions use dummy variables capturing time windows around the execution date.[15] We estimate our main results using a one-day time window (comprising exclusively of the execution date), as well as a three-day window (which additionally includes the day before and the day after the execution). We show below that the results do not depend on the choice of window.

5   Main Results

In this section we display the main results of the paper. We estimate variations of the equation:

$$crime_{it} = \beta ExecutionWindow_{it} + \gamma_t + (\alpha_i \times \pi_{m(t)} \times \lambda_{y(t)}) + \epsilon_{it} \qquad (4)$$

where $ExecutionWindow_{it}$ is a dummy variable taking value one in a time window (either one or three-day) around an execution, in the county where the original crime was committed. The other variables are defined as above. We present first the baseline results and then a set of additional tests exploring their robustness.

Baseline Estimates   Table 2 displays the results of estimating (4), separately for the serious crime variable and for each of its three components. We find first that the number of serious crimes is approximately .10 units lower in the window around the execution date. Note that this is a very large effect: the mean of the variable serious crimes in the sample is only slightly higher, at .14. A better reference point is perhaps the mean in the subset of

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[14]The finding that serious crime starts to decrease already the day before the execution is consistent with Hong and Kleck (2017) finding that newspaper and television stories start to report on executions the day before they take place.

[15]The baseline regressions implicitly assume that dates close to the execution date but outside the chosen time window are not associated with higher or lower levels of crime (relative to that same county in that same month). In light of the evidence in Figure 2, this assumption appears to be justified.

counties associated with at least one execution throughout our sample period. As Panel B of Table 1 shows this is .48. Evaluated against this benchmark, the estimate represents a decrease of approximately 20% in serious crime.

We find qualitatively similar results when evaluating separately the effects on homicides, rapes and assaults with weapons. Evaluated against the mean of the dependent variable, the effects are strongest for homicides. The .012 coefficient of the one-day window represents approximately half of the mean of the homicide rate in the counties associated with at least one execution. The relative rarity of homicides implies however that the estimate is not statistically significant for the three-day window. For rapes and assaults with weapons the estimated effects represent between 15% and 25% of the means of the respective dependent variables in the counties associated with at least one execution.

Robustness In Table 3 we evaluate the robustness of the main findings to modifying the set of controls, the estimating equation, the clustering strategy and the estimating sample. An extensive robustness exercise is particularly important in our context, given the well-documented finding that death-penalty estimates can be extremely fragile to specification choices (Donohue and Wolfers, 2009).

We first add interactions between date and state indicators to the baseline regression (4). In doing this, the identification assumption is that executions are not scheduled in days of idiosyncratically high or low crime in the original-crime county, relative to other counties *in the same state*. Because executions do not typically take place in the original-crime county, we regard this assumption as particularly plausible. The estimates are largely unchanged.

Relative to non-linear methods with a large number of fixed effects, OLS has the advantage of being more robust and easy to interpret (Angrist and Pischke, 2009). In the fifth row of Table 3 we estimate however a Poisson model, which may be more appropriate given the count nature of the dependent variable (Hjalmarsson, 2009). The coefficients are strongly statistically significant and similar in magnitude to those in the baseline regression. The estimated coefficients indicate that executions are associated with a 16%-20% decrease in serious crime, in the original-crime county. In the fourth row, we find that the coefficients are strongly significant, albeit much smaller in magnitude, when the number of serious crimes (plus one) is entered in logs.[16]

In the baseline regression, the standard errors allow for correlation within the same state and year. In the fourth row of Table 3, we allow the date to be an additional dimension

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[16]The log model is likely misspecified in our context, given the high proportion of zeros in the serious crime variable.

of error correlation (Cameron, Gelbach and Miller 2006). The two-way clustered standard errors are essentially identical to the baseline one-way standard errors.

In the last three rows of Table 3, we examine the robustness of the estimates to changes in the estimation sample. We first exclude counties from the state of Texas, which can be regarded as an outlier in its enthusiastic application of the death penalty.

Secondly, we limit the sample to including only counties from states where at least one execution took place during the sample period. Note that the non-execution states only contribute in the baseline regression to the estimation of the date fixed effects. It could be argued, however, that excluding these non-execution states generates a better counterfactual to the original-crime counties during the execution window.

In the last row of Table 3 we account for the fact that the coverage of the NIBRS has increased significantly throughout the sample period. Our baseline empirical strategy (where we control for the interaction of county, year and month indicators) is designed to absorb secular trends and even relatively short-term variations in crime. Nevertheless, we repeat the estimations with a (balanced) panel of counties that are present throughout the period 2002-2015.

Overall, we find that the baseline estimates are not sensitive to these reasonable alterations of the estimation sample.

6   Heterogeneity

The theoretical framework in Section 2 provides a number of testable additional predictions. Firstly, it suggests that the awareness impact of an additional execution should be higher in counties where executions are relatively rare. Secondly, the model is based upon the notion that it is news about execution that increases awareness of the negative consequences of crime. This suggests that executions that are widely covered by the media should impact criminal decision-making more strongly than those that receive less attention. It further indicates that, while the strongest effects should be concentrated on the original-crime county, neighbouring counties may also be partially affected, as there may be informational spillovers towards these counties.

Death Penalty Propensity   We first study whether the increase in awareness decreases in the number of past executions of the county. Our measure is the number of past executions in the five year window previous to (the month before) an execution, in the original-crime county that the execution is associated with. Note that by defining the variable in this

13

way, we ensure that it does not mechanically increase over the sample period. We interact this measure with the main independent variable of interest, the time window around an execution. An additional complicating factor is the strong positive correlation between the number of executions in a county and its population, a correlation that one would expect. The complication arises because the population of a county is also related to its level of crime. Therefore, in this heterogeneity exercise we also control for the interaction with the county's (log of) population.

Panel A of Table 4 displays the results. The positive coefficient of the interaction with the county's death penalty propensity indicates that the effect of executions on serious crime is lower in counties with a higher number of past executions, as predicted by our conceptual framework.This finding provides a potential reconciliation of our paper with Hjalmarsson (2009), who finds no effect of execution on homicides in a sample from the state of Texas. Given that Texas carries out a disproportionately high number of executions every year, our findings predict that the effect of each additional execution should be relatively low.

Media Attention   In our second heterogeneity exercise, we differentiate between executions receiving different levels of media attention. Our initial measure of media attention is based on Google Trends search results and explained in detail in Section 3 above. We use this measure to split the main independent variable into two different variables, depending on whether the execution windows coincide with (within state and date) above median media attention regarding death penalty issues.[17] Because our measure of media attention varies within a county/month and also within a date, we need to explicitly control for it in the regression.

We find in Panel B that it is only executions associated with above median media attention that are associated with decreases in crime. For these execution windows, the effect is in fact much larger than the baseline effect. For instance, for the one day window the effect is -.175. The above median and the below median dummies are statistically different from each other at the 7% level.

Neighbouring Counties   Lastly, we examine in Panel C whether counties that neighbour the original-crime county also experience a decrease in crime during the execution window. To study this, we use an additional variable: a dummy taking value one during an execution

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[17]Because the information from Google Trends is only available from 2004 onwards, these regressions exclude all years before that. This reduces the number of executions to 311. Out of these, 189 are classified as coinciding with high media attention.

window and for counties sharing a border with the original-crime county. Naturally, the regression maintains the main independent variable of interest.

The resulting estimate for the one-day window indicates that the number of serious crimes is approximately .025 units lower on the day of an execution, in the counties neighbouring the original-crime county. Note that this effect is economically meaningful and statistically significant. Reassuringly, it is also much smaller than the original-crime county estimate, which remains largely unchanged.[18]

Panel C of Table 4 suggests that the awareness regarding an execution (and its impact on crime) may ripple away from its epicentre at the original-crime county. To study the functional form of this relation, we calculate the distance between (the centre of) a county and (the centre of) the original-crime county. We then create a set of distance dummies and combine these with the original time windows around executions. The corresponding estimates from introducing these in equation (4), for the one-day window, are displayed in Figure 3. At zero kilometres we display the baseline effect for the original-crime county.

We find that surrounding counties with a (centre to centre) distance of less than 50 kilometres experience a .04 decrease in the number of serious crimes, during the execution day. The effect is .02 decrease for counties between 50 and 100 kilometres away, smaller at higher distances, and statistically insignificant beyond 2,000km. Overall, these results are consistent with the notion that awareness about an execution (and its effect on behaviour) is strongest for would-be criminals that are closer to the location of the original crime.

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[18]It is one fourth of the original-crime estimate, although as we can see in Panel B of Table 1 the average number of serious crimes is also smaller, at .21 rather than .48.

Figure 1: Sample of Crimes and Executions

NIBRS Data
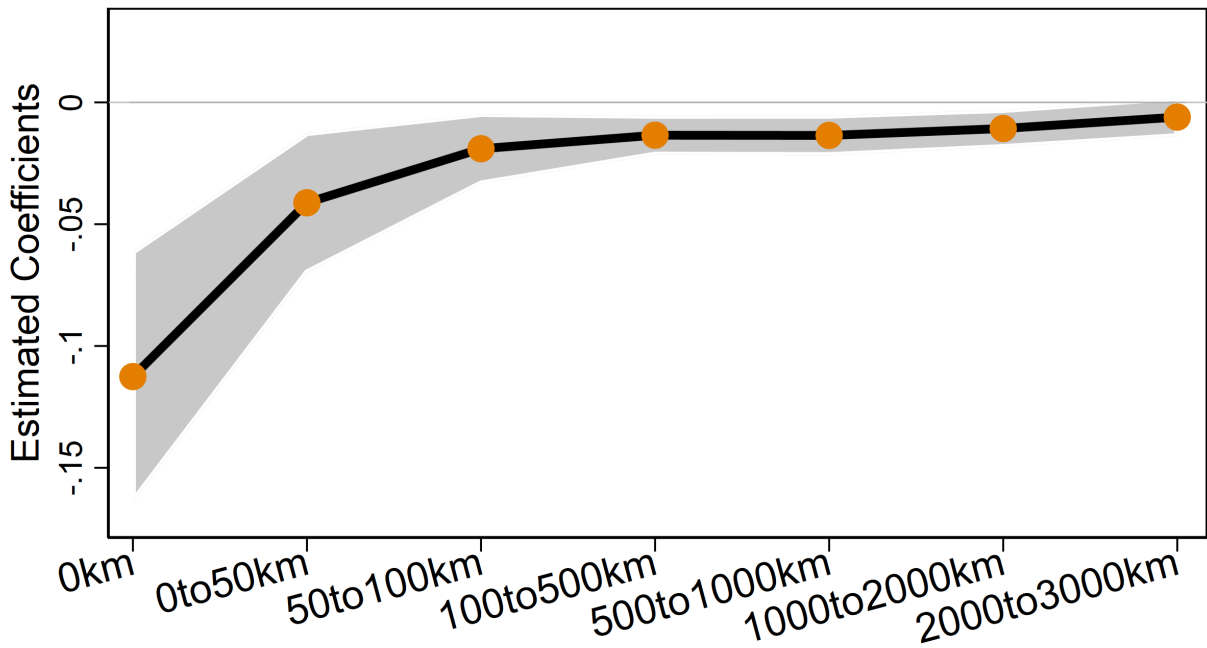**No NIBRS Data**
• **Original-Crime Counties**
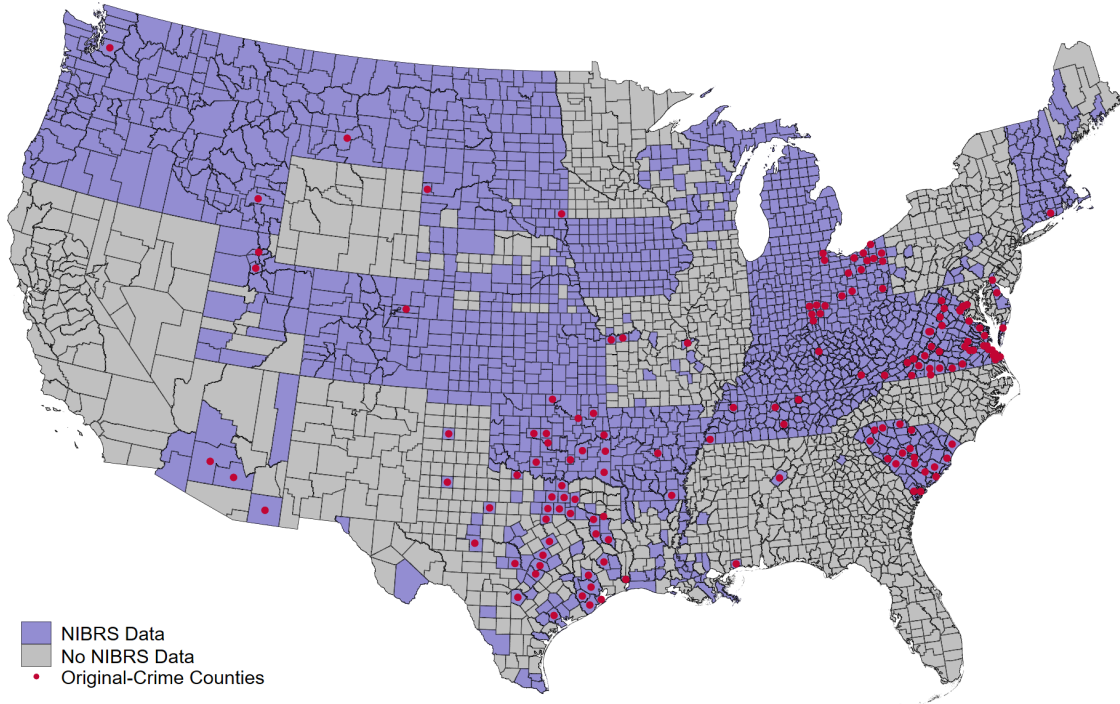
The graph displays information on the counties with crime information (NIBRS) in the sample. The graph also displays markers for the counties with at least one execution during the sample period.

Figure 2: Event Study
Evolution of Serious Crime in Days around Execution Day
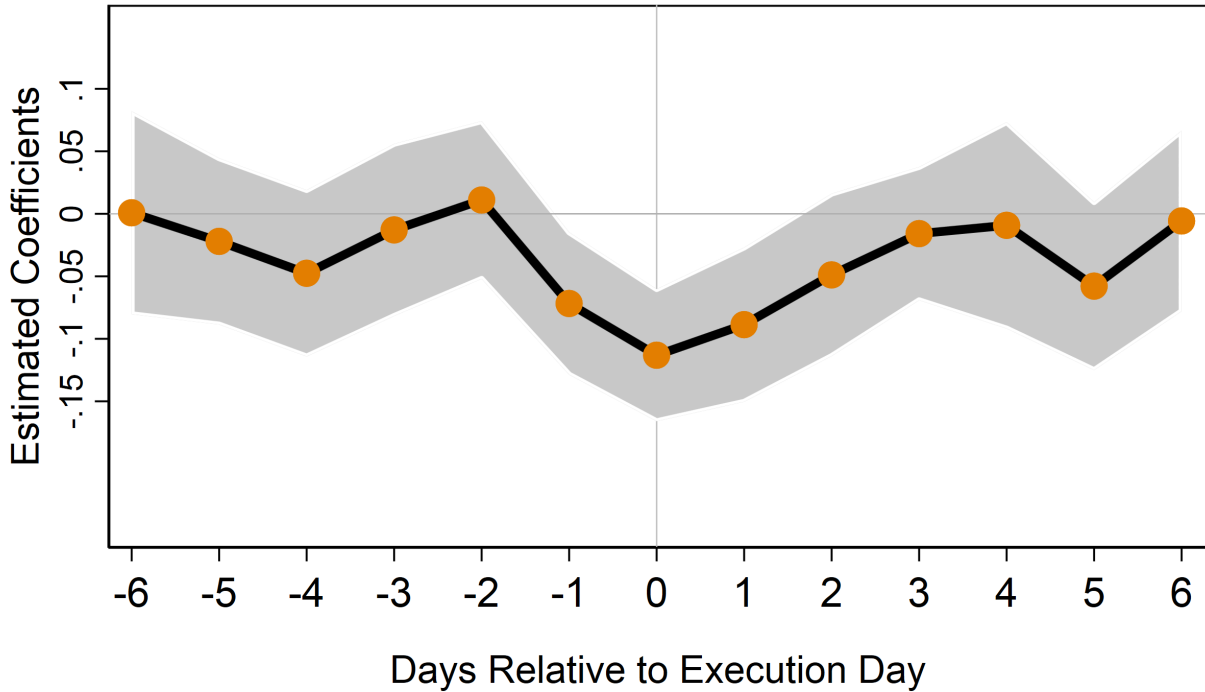
This figure displays the coefficients for the combinations of the days relative to the execution date and the county where the capital crime relating to that execution was originally committed. The dependent variable in the regression is the number of serious crimes (homicides, rapes, assaults with weapon). The regression includes Date and County X Month X Year indicators. The standard errors are clustered at the State X Year level. The number of observations is 8,462,202.

Figure 3: Effects by Distance to Original-Crime County

This figure displays the coefficients for the combinations of the one-day time window around an execution and the distance to the centre of the county where the capital crime relating to that execution was originally committed. At zero we have the original-crime county. The dependent variable in the regression is the number of serious crimes (homicides, rapes, assaults with weapon). The regression includes Date and County X Month X Year indicators. The standard errors are clustered at the state X year level. The number of observations is 8,462,173 for every regression.

Table 1: Descriptive Statistics

Panel A: Executions

	N in Sample	N of Counties	Only Execution or Neighboring Counties			
			Avg N	Std	Min	Max
Executions	493	143	3.45	7.86	1	70
Executions in Neighboring County	1,496	350	4.27	8.04	1	70

Panel B: Crimes

	All Counties in Sample			Execution Counties		Neighboring Counties	
	Total	Avg	Std	Avg	Std	Avg	Std
Serious Crimes	1,173,252	0.14	0.66	0.48	1.27	0.21	0.69
Homicides	50,257	0.01	0.09	0.02	0.17	0.01	0.11
Rapes	586,928	0.07	0.35	0.21	0.64	0.10	0.43
Assaults Weapons	540,346	0.06	0.43	0.25	0.84	0.10	0.41

The table displays descriptive statistics for executions and crimes for the sample period. The sample consists of 8,462,202 day-county observations during the period 1997-2015.

TABLE 2: BASELINE ESTIMATES

Dependent Variable	(1) Serious Crime	(2) Homicides	(3) Rapes	(4) Assaults Weapons
1 Day Execution Window	-.101***	-.012**	-.053***	-.036*
	(.026)	(.006)	(.021)	(.019)
3 Days Execution Window	-.085***	-.006	-.045***	-.033***
	(.016)	(.004)	(.012)	(.011)

This table displays estimates of OLS regressions of crime on the combinations of a time window surrounding an execution and the county where the crime motivating that execution was originally committed. The 1 Day Execution Window comprises of the execution date. The 3 Days Execution Window includes also the day before and the day after the execution. Every coefficient results from a different regression. All regressions control for Date indicators and County X Month X Year indicators. The standard errors are clustered at the State X Year level. The dependent variable in column (1) is the sum of the dependent variables in columns (2), (3) and (4).

TABLE 3: ROBUSTNESS

Dep. Variable = Serious Crimes	(1) 1 Day Window	(2) 3 Days Window
Baseline	-.101***	-.085***
	(.026)	(.016)
Adding State X Date Indicators	-.094***	-.078***
	(.026)	(.016)
Poisson Regression	-.202***	-.157***
	(.064)	(.038)
Dep. Variable in Logs	-.041***	-.032***
	(.012)	(.007)
Multi-Way Clustering	-.101***	-.078***
	(.026)	(.016)
Sample Excludes Texas	-.12***	-.094***
	(.045)	(.026)
Sample Includes only States with Executions	-.094***	-.078***
	(.026)	(.016)
Balanced Panel 2002-2015	-.095***	-.076***
	(.028)	(.019)

This table displays estimates of OLS regressions of the number of serious crimes on the interaction between a time window surrounding an execution and the county where the crime motivating that execution was originally committed. Every Panel/Column combination displays a different regression. In Column (1) the time window comprises of the execution date. In Column (2) the time window includes also the day before and the day after the execution. The first row replicates the baseline estimates from Table 2. In the second row we substitute the Date indicators by State X Date indicators. The third row displays the result from a non-linear Poisson regression. In the fourth row the OLS regression uses the log of the number of serious crimes plus one as the dependent variable. In the fifth row the standard errors are clustered both at the State X Year and at the Date level. The sixth row replicates the standard regression but excludes all counties from the state of Texas. The seventh row uses a sample of states with at least one execution throughout the 1997-2015 period. The eight row uses a balanced panel of counties and limits the sample period to 2002-2015. All regressions except for those in the second row control for Date indicators and County X Month X Year indicators. The standard errors in all rows except for the fifth row are clustered at the State X Year level.

TABLE 4: HETEROGENEITY

Dep. Variable = Serious Crimes	(1) 1 Day Window	(2) 3 Days Window
Panel A: Death Penalty Propensity		
Execution Window	.362	.405***
	(.288)	(.171)
Execution Window X Death Penalty Propensity	.075*	.045***
	(.039)	(.019)
Execution Window X County Population	-.043*	-.043***
	(.026)	(.015)
Panel B: Media Attention		
Execution Window X High Media Attention	-.175***	-.125***
	(.048)	(.034)
Execution Window X Low Media Attention	.008	-.062
	(.074)	(.048)
High Media Attention	-.001	-.001
	(.001)	(.001)
Panel C: Effect on Neighbouring Counties		
Execution Window	-.102***	-.085***
	(.026)	(.016)
Execution Window (Neighbouring County)	-.024**	-.027***
	(.012)	(.008)

This table displays estimates of OLS regressions of the number of serious crimes on the interaction between a time window surrounding an execution and the county where the crime motivating that execution was originally committed. Every Panel/Column combination displays a different regression. In Column (1) the time window comprises of the execution date. In Column (2) the time window includes also the day before and the day after the execution. In Panel A the independent variable is interacted with the propensity to carry out the death penalty in that county, measured as the (log of the) accumulated number of executions in the county. The regression also controls for the interactions with the (log of the) number of days since the last execution, and the (log of the) county's population. In Panel B an additional independent variable is added to the regression, capturing the interaction between a time window surrounding an execution and the counties neighbouring the county where the crime motivating the execution was originally committed. In Panel C, the main independent variable is interacted with a weekly indicator of media attention, measured using the Google Trends result at the state level for death penalty topics. All regressions control for Date indicators and County X Month X Year indicators. The standard errors are clustered at the State X Year level.

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APPENDIX

Figure A1: Event Study
Controlling for State X Date Indicators
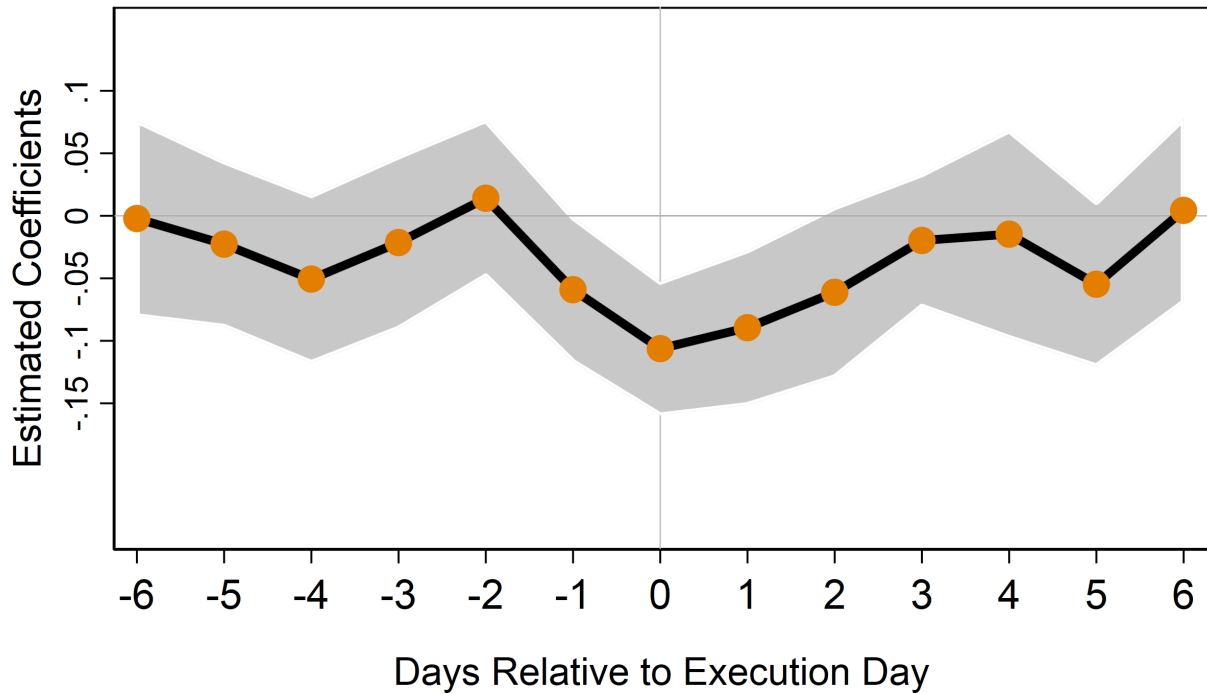
This figure displays the coefficients for the combinations of the days relative to the execution date and the county where the capital crime relating to that execution was originally committed. The dependent variable in the regression is the number of serious crimes (homicides, rapes, assaults with weapon). The regression includes Date X State and County X Month X Year indicators. The standard errors are clustered at the State X Year level. The number of observations is 8,462,202.