Bargaining in the Shadow of the Trial?

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Abstract

Plea bargaining is the cornerstone of the U.S. criminal justice system and the bargaining in the shadow of the trial framework, where the plea reached is driven primarily by the expected sentence arising from a trial, is the convention for applied economists. Criminologists and legal scholars challenge this framework. There has not been a test of the validity of the conceptual framework. We do so. We use a large data set of felony cases in Florida to estimate the plea discount received. Our identification strategy is to consider deaths of law enforcement officials, which we argue is a newsworthy tragic event affecting a local community and making violent crime salient to the citizens who make up the potential jury pool. Those cases, unrelated to the death, but already in process at the time and in the same location as the death, acts as our treated observations who experience an exogenous shock to their probability of conviction. We show that these individuals plea guilty to substantially longer sentences. This effect is especially strong for deaths of law enforcement officials who die via gunfire and are stronger when there is more internet search behavior out of local population. The reduction in the plea discount occurs across numerous serious crimes, but is essentially zero for less-serious crimes. Theory does not predict, though, what will happen to the trial rate since tougher offers from the prosecutor should lead to more trials, but the heightened conviction probability should encourage negotiation. We find that the likelihood of a trial increases, consistent with the hypothesis that prosecutors are making less-generous plea offers. Thus, we provide strong evidence that plea bargaining occurs in the shadow of the trial.

Keywords: death; jury trial; law enforcement official; plea bargaining; sentencing
JEL Codes: K4; H4; D8

1 Introduction

In the United States, plea bargaining is ubiquitous. It is not an overstatement when Justice Anthony Kennedy referred to U.S. criminal justice as a "system of pleas". For a typical state in the U.S., more than 95% of felony convictions arise from guilty pleas, which are often negotiated via a plea bargaining process. To appreciate the social concerns of crime's costs and consequences, prison overcrowding, and the public financial burden of the legal system, one must appreciate the functioning of our plea bargaining system.

The standard framework in applied theory is to consider the bargaining game between the prosecutor and the defense (treating the defendant and his/her attorney as a single player). In a negotiation, the defendant can agree to a sanction and enter a guilty plea. Presumably, this sanction is less than what could be levied

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if the defendant is convicted in a jury trial and is sentenced by a judge. In practice, plea bargaining can take numerous forms such as sentence bargaining, where the prosecutor agrees to recommend a mitigated sentence to the judge, or charge bargaining where the prosecutor accepts a guilty plea for a less-serious crime in return for dropping a different crime that would result in a longer sentence. Regardless of the form the plea bargaining takes, economists typically argue that it is efficient. If the two parties form reasonably similar expectations of what will happen if the case proceeds to a jury trial, and the trial itself is costly, then agreeing to a penalty close to the expected sanction received at trial would save both sides the costs and, hence, be mutually agreeable. In fact, the defendant may even receive a *plea discount* due to the monetary and time savings he provides the prosecutor's office by pleading guilty.

Thus, standard theoretical models of plea bargaining treat the expected outcomes of a jury trial as the default outcome of the bargaining process. Consequently, changes in the expected outcomes of the trial would then be expected to be reflected in the negotiated guilty plea.

This framework is so well-established and frequently used that it has been named as "bargaining in the shadow of the trial." (Mnookin and Kornhauser, 1979; Cooter *et al.*, 1982)

The question theoretical economists have focused on, then, is what market imperfection can explain the existence of trials. Building from the seminal contribution of Reinganum (1988), asymmetric information models dominate the literature.[1] One party may have private information on guilt, the quality of the evidence, or even risk preferences. Introducing asymmetric information allows for plea bargaining to imperfectly sort (Bjerk, 2007) and can explain the use of jury trials in equilibrium. In other theoretical frameworks, noisy information (Priest and Klein, 1984; Abrams, 2011) or optimism bias (Gould, 1973; Posner 1974; Shavell, 1982; Farmer and Pecorino, 2002; Burke, 2007) is employed to explain bargaining's failure.

Research evaluating the efficacy of legal institutions relies on this theoretical framework. Reinganum (1988) provides an early contribution by exploring sentencing guideline's impact on the plea bargaining process. Bandyopadhyay and McCannon (2014; 2015) evaluate how a prosecutor's re-election concerns distort the plea bargaining rate. Empirical evaluations of prosecutor salary and turnover (Boylan and Long, 2005), for example, implicitly utilize this framework by using the trial rate as an outcome variable.

Outside of economics, criminologists and legal researchers have criticized the bargaining in the shadow framework as an inaccurate description of the plea bargaining process. An influential critique of plea bargaining by Bibas (2004) focuses on two primary shortcomings. First, structural impediments distort bargaining. Poor lawyering and attorney self-interest create a disconnect between the outcome that would arise at trial and the negotiated plea. Pre-trial detention, as another example, may matter more for the decision to enter a guilty plea than the expected outcome at trial (Stevenson, 2018). Second, he challenges the presumption that actors are fundamentally rational. Psychological biases and the defendants' lack of sophistication are pervasive. If a defendant is unable to anticipate the likely outcomes of a jury trial, and his attorney is not incentivized to provide fully effective counsel, then guilty pleas need not reflect expected trial outcomes.

[1] The initial theoretical models using asymmetric information by Bebchuk (1984) and Reinganum and Wilde (1986) focused on the closely related issue of pre-litigation bargaining.

Bushway *et al.* (2014) conduct a novel experiment with actual judges, prosecutors, and defense attorneys. Each is given a hypothetical criminal case and is asked to assign the probability the accused will be convicted at trial (p) and the sanction expected if convicted (s). Also, they ask the professionals what sentence they would recommend the defendant to accept (b). They provide mixed results. In the aggregate, the framework does well ($\overline{ps} = 6.75$ & $\bar{b} = 6.11$). Also, for those who assess a greater conviction probability or a more severe trial sanction, the proposed plea bargain is greater. On the other hand, for the sample of judges their assessed probability of conviction does not correlate with the plea bargain. Instead, their behavior can best be explained by a constant plea discount unrelated to the jury's probability of conviction. For prosecutors and defense attorneys, a nonlinear relationship exists where the plea bargain does not increase at the same rate as the expected trial sanction.

Numerous econometric techniques have been employed to estimate the plea discount. The primary empirical dilemma is that a counterfactual outcome of what would have happened at trial is unknown. One should reasonably expect selection effects to be pronounced so that outcomes experienced by those who choose to go to trial are poor predictors of what would have occurred for those who enter a guilty plea. Bushway and Redlich (2012) engage in a useful exercise. They use a rich data set that includes information on evidence available. They use this information to estimate the probability of incarceration for those who went to trial and use this model to create the counterfactual incarceration probability at trial for those who pled. With this approach they test the accuracy of the shadow of the trial framework. They compare the outcomes of those who went to trial to the expected trial outcome for those who entered a guilty plea. In the aggregate the two values match up, suggesting that plea bargaining occurs in the shadow of the trial. At the individual level there is wide variation suggesting that important individual-level variables are driving behavior other than the expected outcome at trial, supporting Bibas (2004). Again, it is unclear whether negotiated deals capture the outcomes that would arise at trial.

Knowing whether the theoretical framework used commonly describes the bargaining process is crucial for criminal justice reform. Measures to affect jury selection, information, or decision making will have only muted impacts if plea bargaining ignores these changes. Numerous policy debates surround the selection and decision making of judges. Examples include the impact of their selection and retention mechanism (i.e., appointment versus election) (Lim, 2013), partisan affiliation (Lim and Snyder, 2015), and term length and tenure. Efforts to handcuff judicial sentencing discretion is more powerful when plea bargaining responds to the expected sanction at trial. Importantly, the plea bargaining process has been for the most part unregulated. Only recent Supreme Court decisions have even chosen to weigh in on the institution. Most recently in *Lafler v Cooper* (2012) the Supreme Court addressed the issue of whether ineffective assistance of counsel even applied to plea bargaining. At trial, rules of evidence regulate dubious confessions and policing practices. The scrutiny applied by jurors and the public are lost when a plea deal is reached. As summarized nicely by Bibas (2004), "if highly proceduralized and regulated trials serve as a backstop to largely unregulated plea bargaining, we do not need new procedural safeguards for pleas because plea outcomes already incorporate the value of trial safeguards" (p.2466). If the conceptual framework fails to adequately explain outcomes, then "plea bargaining practices need many reforms" (p.2408).

Therefore, our objective is to investigate whether the bargaining in the shadow of the trial framework is the appropriate one to use to study plea bargaining. To do this, we take advantage of a large data set from the state of Florida. It provides case processing information for every individual arrested for a felony offense since 2004. The data set tracks the case as prosecutors file, drop, refile, and pursue a conviction. Whether or not the individual saw his charges dropped, whether he pled guilty, or whether the case went to trial is known. Furthermore, sentencing information is available. The data set includes more than 27 million criminal counts processed.

The empirical dilemma is how to make a causal identification. The prosecutor and defendant's expectations of the jury's likelihood of conviction is not measurable; nor is whether this probability factored into the bargaining process at all. Changes in jury procedures, such as variation in the number of jurors who hear a case or whether unanimity is required for conviction[2], are expected to be done by policymakers who have identified a problem with the criminal justice system. Hence, institutional changes are endogenous and it would not be clear whether it was in fact the jury's proclivity to convict that was driving plea bargaining. Changes in sentencing harshness also fails to provide causal inference since sentencing guidelines are, again, intentional policy tools used in response to crime and promote deterrence; thus potentially influencing the mix of individuals and acts that arise in the criminal justice system.

To make a casual identification of bargaining in the shadow of the trial we use data on every incident in the state of Florida where a law enforcement official died in the line of duty. We argue that the death of a police officer is a salient tragedy in the community. Just as in Philippe and Ouss (2018), we argue that the resulting media coverage of the incident primes those in the local community to the problems of violence and the risks law enforcement officials expose themselves to. We argue that this acts as an exogenous shock to the pool of potential jurors. We consider those felony cases where the arrest had already been made and the initial charges had already been filed, but the case had not been disposed of yet when the police officer's death occurred. For these accused defendants, the law enforcement official's death acts as a quasi-natural experiment and become the treated sample. We use a difference-in-difference estimation to identify whether these cases resulted in different outcomes than those processed without the shock of an officer's death.

If plea bargaining occurs in the shadow of the trial, then we expect the severity of the sentences agreed to in the plea bargaining process, relative to those obtained from a jury trial conviction, to become more severe. Simply put, plea discounts should become less generous. If, on the other hand, the plea bargaining process is unrelated to the jury's willingness to convict, then the sentences arising from plea bargaining will not change.

We find strong evidence that plea bargaining adjusts for cases in process when the exogenous shock occurred. Outside of this period, the sentence received from plea bargaining, relative to the sentence received

[2]For example, in 2018 Louisiana voters passed a referendum to change the requirement for conviction at criminal trials from $\frac{10}{12}^{th}$s to unanimity. In some states, criminal trials only utilize six jurors, rather than the standard twelve.

from a trial, is approximately 46 cents on the dollar (i.e., 30 months shorter). When a law enforcement official dies in the line of duty, guilty pleas serve approximately 57 cents on the dollar (i.e., 26 months fewer). Thus, the plea discount reduces.

Consistent with our hypothesis, we further show that the difference-in-difference coefficient is essentially zero for non-serious felonies and consistently positive and statistically significant across different serious crimes. While for all felonies, the plea discount reduces by 20% when a law enforcement official dies in the line of duty, for serious crimes the plea discount drops by 53%. Additionally, we show that the effect is strong for gun-related deaths of law enforcement officials. Finally, using Google searches for law enforcement official, we show that the effects are larger when there is a greater uptick in Google searches in that area.

The bargaining in the shadow of the trial framework does not, though, give a clear prediction of what will happen to the trial rate when a law enforcement official dies in the line of duty. When prosecutors make less-generous offers, one would expect that the probability a defendant accepts those terms will reduce. On the other hand, an increase in the probability of conviction can reasonably expand the bargaining zone, leading to fewer trials. Using the quasi-natural experiment, we show an important increase in the rate at which cases are taken to trial. The probability a case goes to trial increases by almost 72%. Again, the effect is larger for serious crimes. Thus, not only are accused criminals, whose crimes are unrelated to the circumstances that caused the law enforcement official's death, accepting harsher sanctions when they do agree to a plea (i.e., the intensive margin), but are experiencing harsher sentences from the fact that more are going to trial, which generates stiffer sentences (i.e., the extensive margin).

Finally, we provide suggestive evidence that it is, in fact, changes in the jurors anticipated behavior driving the results. This is important since our argument relies on changes in the probability the jury convicts that trickles down to the plea negotiation. We disentangle the trials into those that go to a jury trial from those where a bench trial, with the judge both determining guilt and the sentence, occurred. Considering sentencing, the reduction in the (relative) trial penalty only occurs for jury trials. The difference-in-difference effect is a precisely estimated zero for bench trials. Similarly, the increase in the trial rate only occurs for jury trials. Thus, it is not changes in beliefs about the judge's sentencing, but rather alterations in the expectations of jurors behavior that can explain our observations. Additionally, evaluating the number of counts filed as the case is processed, the death of a law enforcement official coincides with approximately a doubling of the probability the number of charges filed increases between the initial charging and the final charges pursued by the prosecutor's office. This is evidence consistent with charge stacking and can be expected to be effective with 'noisy' juries (Gay *et al.*, 1989). Plea bargained cases see a reduction in the probability of an increase in the number of counts, consistent with charge bargaining. Weak, but interesting, evidence exists that this increase in the likelihood of an increase in the number of counts with deaths is not as great for the plea bargained cases. This is consistent with prosecutors using an increase in the number of crimes pursued to induce guilty pleas with harsher sentences.

We are the first to explore exogenous variation in the probability a jury convicts[3] to test to bargaining in the shadow of the trial framework. Our work complements Bushway and Redlich's (2012) effort in that rather than try to estimate the counterfactual, as they did, we take advantage of an exogenous shock to the conviction probability to estimate directly the change in negotiated plea deals. We obtain strong evidence, both highly statistically significant and large effects in magnitude, that negotiated discounts adjust to the expected trial outcome.

2 Theory

Consider a simple theoretical model of a prosecutor and a defendant engaged in plea bargaining. The model we present is a simplification of those common in the literature. The defendant has a binary choice: Whether to accept a plea offer or to reject it and go to trial. Let $b > 0$ be the plea bargain offer. Let $s > 0$ be the sanction received if convicted at trial, and let the sanction be 0 if acquitted. The probability of conviction at trial, as assessed by the defendant, is p_d. Furthermore, going to trial incurs an additional cost of c_d for the defendant. For simplicity, assume the defendant is risk-neutral minimizing the loss from incarceration. Hence, he will accept the plea bargain when

$$b \leq p_d s + c_d. \tag{1}$$

The prosecutor on the other hand is assumed to prefer to get the highest sentence possible. One can presume that the sanction s set by, for example, the legislative body via sentencing guidelines can be viewed as representing society's desired sanction. The prosecutor, then, wants the sanction to be as close to this as possible, but is given discretion to accept a less-severe sanction. Let the probability of conviction at trial, as assessed by the prosecutor, be denoted p_p. Also, let the prosecutor's cost of pursuing the case at trial be $c_p > 0$. Again, assume the prosecutor is risk neutral. Hence, she is willing to accept the plea bargain when

$$b \geq p_p s - c_p. \tag{2}$$

Therefore, a mutually-agreeable plea bargain exists if $p_d s + c_d \geq p_p s - c_p$. The interval,

$$[p_p s - c_p, p_d s + c_d] \tag{3}$$

is referred to as the ***bargaining zone***. Plea bargaining fails, on the other hand, when

$$(p_p - p_d)\, s > c_d + c_p. \tag{4}$$

This condition is commonly referred to as the ***trial condition***. Jury trials arise, in this simple framework, when the divergence in beliefs is sufficiently great and the costs to trial are sufficiently small.

[3]There is recent work evaluating whether the probability a jury convicts responds to the harshness of the sanction. Blinder and Hjalmarsson (2018) use changes in the use of capital punishment in a prominent 1700s & 1800s English court, along with the shock of the American Revolution.

The conundrum of the bargaining in the shadow of the trial literature is that with rational expectations $p_p = p_d$. That is, if both the prosecutor and the defense attorney are experienced and knowledgeable with the local criminal justice system, if exculpatory evidence is shared (which is mandatory given the *Brady v. Maryland* Supreme Court ruling)[4], and if the rules of evidence and court procedures are commonly known, then the two parties should share common prior beliefs. With this observation, the trial condition always fails with non-zero trial costs and no cases go to trial.

Theoretical models, then, look to market imperfections to explain jury trials. If one side holds private information, then the two beliefs can diverge. If there is noise in the assessments or optimism bias, then again the two probabilities do not necessarily have to equal. Hence, trials might arise in equilibrium.

Staying within this simple framework, suppose a shock occurs to p_i increasing it to $\alpha_i p_i \in (0, 1)$. If one assumes $\alpha_d = \alpha_p = \alpha > 1$, so that the increase in the conviction probability affects all parties' assessments uniformly, then the trial condition becomes

$$(p_p - p_d)\,\alpha s > c_d + c_p. \tag{5}$$

and the left-hand-side is greater than that in (4), resulting in more jury trials. Similarly, the bargaining zone becomes

$$[\alpha p_p s - c_p, \alpha p_d s + c_d]. \tag{6}$$

The interval stays the same 'length' but shifts rightward. Thus, one would expect the negotiated plea, when plea bargaining arises, to be with a higher sanction.

If, on the other hand, the shock does not affect expectations uniformly, as it might in an asymmetric information environment, then the bargaining zone might expand or contract as it shifts rightward. Thus, while the set of mutually-agreeable plea bargains becomes more severe, a contraction of the bargaining zone may lead to more trials (as bargaining fails at a higher rate), and an expansion of the bargaining zone would lead to fewer trials. Deck and Farmer (2006) provides experimental evidence that the size of the bargaining zone corresponds to the disagreement rate. Thus, the exogenous shock to the probability of conviction increases the sanction agreed to, if the participants are bargaining in the shadow of the trial, but has an ambiguous effect on the trial rate.

3 Data

To test the hypothesis that bargaining occurs in the shadow of the trial, we evaluate data from Florida. First, we describe the data on law enforcement deaths, which will act as of quasi-natural experiment. Second, we elaborate on the data available on case prosecution decisions.

[4]See Daughety and Reinganum (2018) for a recent theoretical analysis of prosecutors' incentives to share exculpatory evidence.

Table 1: Causes of LEO Deaths

Cause of Death	#
Automobile related	47
Gunfire	41
Heart attack	8
Assault	3
Duty related illness	1
Heat exhaustion	1
Fall	1

3.1 Police Deaths

Information on police officer deaths in Florida was collected from the *Officer Down Memorial Page*, which collects information on each law enforcement official killed in the line of duty from 1791 to present date.[5] For our period, 116 members of Florida law enforcement agencies were killed in the line of duty, including 14 K9's. These K9 deaths were removed from our sample, leaving us with 102 officer deaths from 2004 to 2014 (see Figure 1 for a time-series of the deaths). Table 1 presents the cause of death in a broad classification. Gunfire deaths are 40% of our data, with automobile related deaths being the most prevalent at 46%.

This data comes at the department level, which we connect to the county it operates within. Approximately 10% of the deaths were part of the same incident (same department, date, and cause of death). Of these ten officers who were killed with a fellow officer, eight are the result of gunfire and only two are automobile related.[6] Additionally, 10 of the deaths are female officers, while the other 92 are male.

Thus, each law enforcement official's death has recorded the county of the incident and the date. We will use these two identifiers to connect the death to the criminal cases being handled at that time in that location.

3.2 Charging and Prosecution in Florida

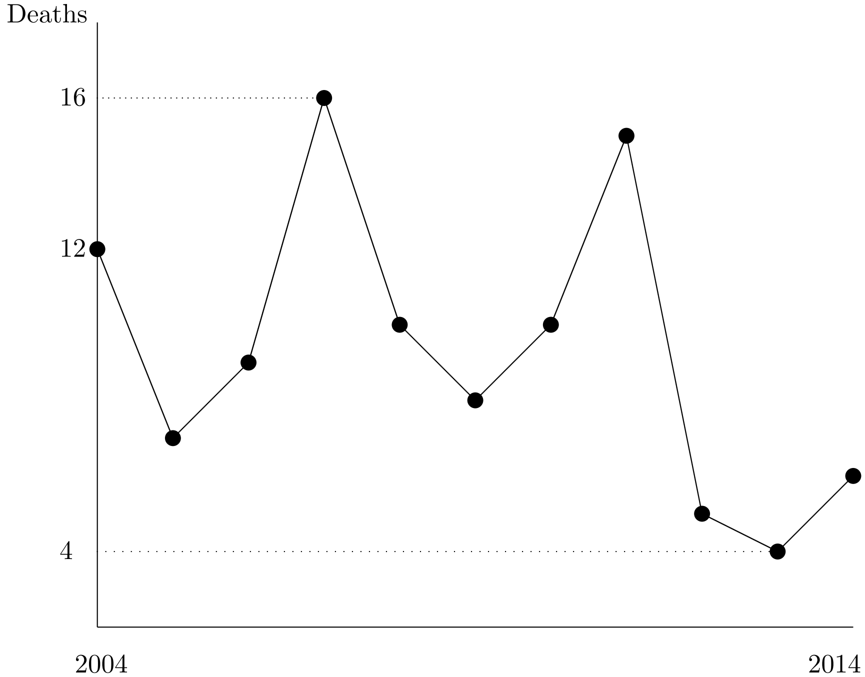
We obtained the universe of charges within the state of Florida from the *Office of the State Courts Administrator*. This rich data set includes approximately 27 million charges filed during the period 2004-17. Each charge is referenced with a court docket number, which represents a single case for a particular defendant.[7] Associated with each charge is a sequence number. This sequence number is unique, and allows us to track changes to each charge within an individual docket. Each charge is divided into four distinct phases: Initial phase, prosecutor phase, court phase, and sentencing phase. Each level details the specific criminal violation for each charge with Florida's criminal code chapter, section, and subsection of violation included. These

[5]The scrape was conducted by FiveThirtyEight; https://github.com/fivethirtyeight/data/tree/master/police-deaths

[6]Automobile related: Palm Beach County Sheriff, November 28, 2007. Gunfire: Okaloosa County Sheriff, April 25, 2009; Tampa Police Department, June 29, 2010; Miami-Dada Police Department, January 20, 2011; St. Petersburg Police Department, January 24, 2011.

[7]Dockets are not split between co-defendants. If there are multiple people charged in the commission of the same crime, each person will be available under a different court docket number.

Figure 1: LEO Deaths in Florida



Each data point represents the number of law enforcement officials who died in the line of duty in the state.

charges may vary across each phase.

The initial phase includes all information from the initial arrest and provides us with demographic information for the defendant including race, gender, and date of birth. Additional dates in this section include the offense date, date of warrant issuance (if applicable), and the arrest date. The birth date and arrest date are used to construct a defendants age at arrest.[8]

The prosecutor phase begins when the suspect has officially been arrested and booked. When the prosecutor receives the case details, a determination of the formal charges that will be filed moving forward is made. The prosecutor has discretion to dismiss, re-file, or add additional charges to a docket. Since the prosecutor has the decision of whether to pursue to the case, included in the data is the specific date that the prosecutor made his/her final decision of whether to pursue the case. Additionally included within this phase is information on the defendant's counsel type: Public defender, court-appointed attorney, private attorney, and self-counsel[9]

The court phase provides us with the last period of potential variation in the number or type of charges for a particular docket. This phase includes the final charges that were brought to court (or plea bargained on) at the same granular level as the other two sections, and the date of court appearance. In addition, if a guilty plea is not entered the type of trial used, either bench or jury trial, is identified. It is important to note

[8]Since birth date is only provided at the month of year level, the day of birth is imputed to be the 15^{th} of every month.
[9]An "other" category is included in the data set.

that in the case of plea bargaining, a defendant will still have a court date in which they formally submit their guilty plea to the judge and the agreement is stated for court record. Following this phase, a sentencing date will be provided for each individual who is found (or pleas) guilty on at least one charge. This particular phase details the method of incarceration (prison, jail, restitution, or other diversionary program) and the length of the sentence for each charge.

We aggregate the individual charges to the docket level where the data are then cleaned to remove cases where all charges were dismissed.[10] The total number of counts at each phase is then calculated, allowing us to see potential changes in the filing decisions across the life of the criminal procedure. From these counts, we select only those dockets that had at least one felony charged at some point in the case's handling. That is, if a case had a felony charge in either the initial, prosecutor, or court phase, they will remain in our data set. Any case that did not experience a felony charge is removed. This process leaves us with a final data set of 1,575,560 felony dockets that were disposed of between 2004 and 2017.

With this aggregation, some concessions need to be made on the amount of data we are able to carry over from the baseline charge information. All dates used are the latest dates that existed within that particular phase for the docket.[11] Perhaps most importantly, we needed to sacrifice the rich charging information at each phase. There is no identifier for how the sentence was served. Importantly, we are unable to discern if the sentence was to be served concurrently or consecutively. Thus, we focus on the top charge. While the basic information (including charge level and degree) was kept for each charge, specific charge information (chapter, section, and subsection) is only for the most serious crime. The most serious crime was decided on the grounds of the highest charge level (i.e., felony) and highest degree (i.e., 1^{st} degree). We then use this specific charge information to control for the most serious crime an individual was charged with in the analysis that follows.

The outcome variable for the primary analysis is the length of the sentence. This information is included within the sentencing phase, and details the length of the sentence in days. Any charge that did not receive formal incarceration (defined as not being prison or jail) is changed to have a zero sentence. Our sentencing variable represents the number of months an individual is to be incarcerated for.[12]

To create this sentence variable, being consistent with the current literature on sentencing, we consider the highest sentence received by a defendant. If the defendant is convicted of only one crime, then this upper bound measurement captures the potential severity of the sentence, which is the focus of our analysis. For a defendant convicted of more than one count, we take the highest sentence of all counts as our measurement

[10] In this case, dismissed can refer to all charges being formally dismissed by the prosecutor, re-filed to be under a different court docket number, transferred to a different court, or extradited. Appellate cases are also removed because the initial filing information is overwritten to only reference specific charges under appeal. That is, if a person was convicted on three charges but is only appealing one, the other two charges would not exist within our data set. Also, information for the prosecutor and court dates would be overwritten with the new information for the appellate hearing.

[11] For example, if an individual was arrested on January 1 and then charged again on January 2 for a different crime, January 2 is the date used for the arrest date in the docket.

[12] We transform sentences provided in either days (e.g, 120 days) or years (e.g., 10 years) to their equivalent in months (3 and 120, respectively).

of the sentence.[13] Individuals convicted of life sentences or receiving capital punishment are coded as having 100 years (1200 months) of incarceration.

The primary focus of our analysis is the exogenous shock of a law enforcement official's death. As mentioned, we record the date and county of the incident. Using the arrest data and final decision date of the prosecutor, we create an indicator variable equal to one if an observation is in process at the time of a law enforcement officials death within the county. Cases that have already been resolved (with a dismissal, acquittal, or conviction) prior to the incident, cases where the arrest has not yet occurred, and cases outside the county become the control group for our "treated" cases. These treated cases are unrelated to incident that resulted in the law enforcement official's death. Thus, the death is an exogenous shock.

Table 2 presents the summary statistics for this final data set.

With our final data set only including those felony cases that were disposed of through plea bargaining or trial, our plea bargaining variable is defined for any docket that remains in our data set but did not go to trial. Plea bargaining occurs 97% of the time, consistent with previous literature on the topic.[14] The average sentence is approximately 12 months, but as to be expected experiences a wide variation. Approximately 4% of our observations occur in the county during the time of a law enforcement official's death.

Our defendants are typically male (82.5%), with the white race having a slight majority (59%).[15]. Florida provides a state-funded public defender office in every district in the state. Thus, a majority of our sample is represented by a public defender (54%).[16]

The average number of counts on a docket is 2.6. Thus, at some time during the case's processing a typical defendant will have 2 to 3 charges filed on their case. As stated, this can be either the stacking of multiple charges or the re-filing with one count being removed and another one added, for example.

4 Sentencing and the Plea Discount

Our primary question is whether sentences received respond to a death of a law enforcement official. We begin by simply partitioning our sample into those observations where there is a death and those observations where there is not a death. Acquittals are removed for the moment from the data set so that only convictions are considered.[17]

A noticeable change arises. When there is a law enforcement official who dies in the line of duty, those cases already in process are treated differently. They see an increase in the sentence length. The mean

[13]To account for the other charges a defendant is convicted of we also consider the midpoint of the upper bounds for all charge with convictions. All results in all tables of the paper persist (sign and statistical significance) if this alternative measurement is used. Thus, we do not believe that the decision on how to account for the sentencing ranges influences our paper's main message.

[14]Bandyopadhyay and McCannon (2014) show that plea bargaining occurs for just less than 97% of convictions in North Carolina. Federal data tends to see plea bargaining occur for 95% of convictions.

[15]White and Hispanic are grouped together in the data file provided. Also, due to potential coding errors more than one race can arise. To handle this, overlap is assigned to the minority race.

[16]With the "other" category and missing observations, the proportion of reported cases with a public defender is substantially higher. In our data it is possible (but not frequent) that multiple representation arises. Thus, for a very small number of observations more than one form of representation arises.

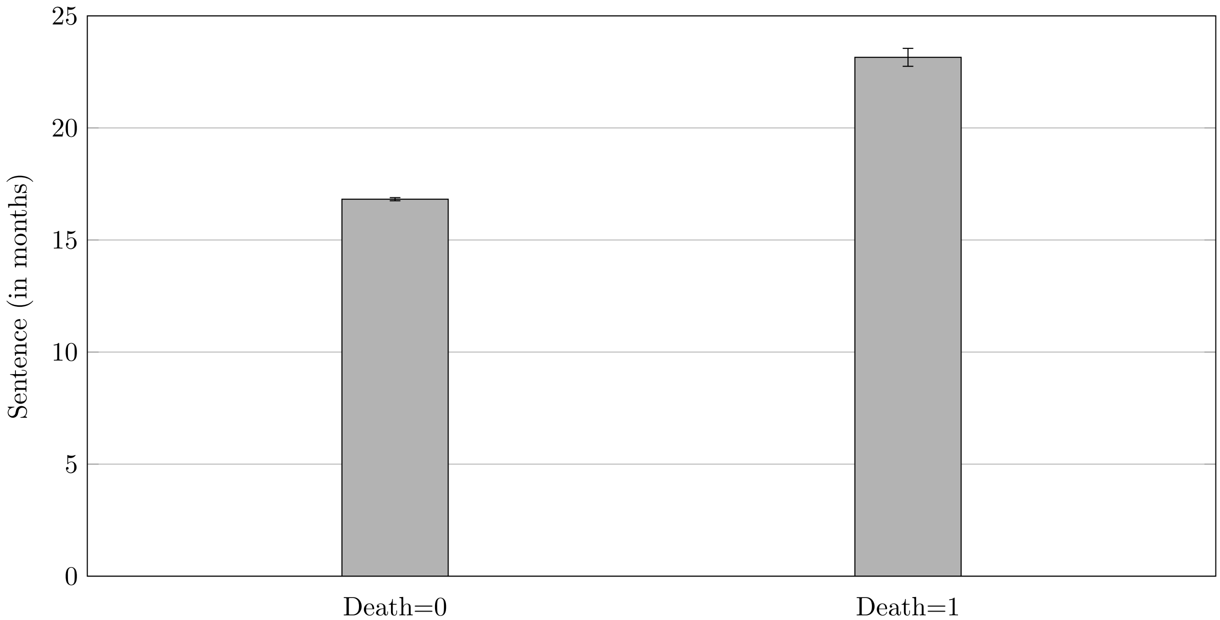
[17]In this sample, 4.0% of the observations have $Death = 1$.

Table 2: Descriptive Statistics

	μ	σ	min.	med.	max.
All Pursued Felony Cases					
Plea Bargain	0.9735	0.1605	0	1	1
Trial	0.0265	0.1605	0	0	1
Sentence	16.980	40.910	0	4	1354
Death	0.0398	0.1954	0	0	1
Demographic Controls					
Female	0.175	0.380	0	1	1
Age	33.36	11.34	13	31	80
White	0.585	0.493	0	0	1
Black	0.407	0.491	0	0	1
Native American	0.001	0.027	0	0	1
Asian	0.002	0.043	0	0	1
Business	0.000	0.003	0	0	1
Case Characteristic Controls					
Total Counts	2.60	6.44	1	2	899
Private Attorney	0.210	0.407	0	0	1
Public Defender	0.539	0.498	0	0	1
Court Appointed Attorney	0.053	0.224	0	0	1

Docket-level data set considered excluding any that does not involve at least one felony and excluding those where all charges are dismissed. Thus, it includes guilty pleas and both trial convictions and trial acquittals.
The sentence variable is measured in the number of months. It is the midpoint of the upper and lower bound to the top charge the defendant is convicted of. Non-incarceration sentences and acquittals take values of zero.
Other forms of representation not presented are self-defense, an "other" category, and missing observations. Each of these have indicator variables that are used in the upcoming specifications.
Trials include both jury trials and bench trials.
$N = 1,575,760$

Figure 2: Does Sentencing Severity Correspond to LEO Deaths?



Confidence intervals depicted by the black lines.
A two-sided, difference-in-means t-test (allowing for unequal variances) has $t = 30.6$. A Wilcoxon Ranksum test for differences in the distributions has $z = 31.5$. A Kolmogorov-Smirnov stochastic dominance test has $D = 0.062$. For all three, $p < 0.001$.

number of months of incarceration jumps up from 9.59 to 12.55 months, which is a 31% increase.

4.1 Baseline Results

Are these harsher sentences coming from the plea bargaining process, or simply an adjustment in what judges hand out after a jury trial conviction?

To explore this, we estimate a difference-in-difference model. Considering the sample of felony convictions, we include an indicator variable for whether the conviction was the result of a guilty plea or not, *Plea Bargain*, as an explanatory variable. The inclusion of *Death* captures whether or not the observation occurred in a county during the time when a law enforcement official dies in the line of duty. The coefficient on the interaction term between these two is the difference-in-difference metric of interest.

We include numerous fixed effects to separate out other factors that could be influencing sentencing. We include the demographic controls presented previously recognizing that there are important gender disparities (Starr, 2014) and racial disparities (Rehavi and Starr, 2015) in sentencing. We include case characteristics acknowledging that there are important differences in who is able to afford private attorneys, and that public defenders may be generating more favorable outcomes than court-appointed attorneys for the indigent (Agan *et al*, 2018). Furthermore, county fixed effects are included to pull out time-independent differences in areas of the state and monthly fixed effects allow us to account for temporal variation that may affect outcomes, such a macroeconomic well-being of the state and law/policy changes. We present standard errors that are clustered at the county by year level to account for both differences in cross-sectional and temporal variation. There are 695 clusters.[18]

Regarding the dependent variable, we consider the length of the sentence handed down for the top charge convicted of. Specifically, for the charge that has the longest potential sentence, we identify the maximum number of months that the conviction of that crime results in and the minimum. We use the midpoint of these two as our measurement of the sentence given.

Two issues arise with this variable. First, it is right-skewed. That is, while the bulk of the observations are centered around the mean of 17, the right-tail extends out including sentences with extremely long lengths of incarceration. For data such as this, it is common to use a transformation that pulls in the tail. The most common is the log transformation, which we will use in our primary specification, *ln(sentence)*. As is well known, this is not defined for sentences of zero length. In data of criminal sentencing, zero-length incarcerations are common. An alternative transformation is the inverse hyperbolic sine transformation (Burbridge *et al.*, 1988); denoted *IHS(sentence)*. In it, $\widetilde{z} = log(z + \sqrt{z^2 + 1})$. This transformation is defined for zero and, while not needed here, is defined for negative values of z as well. Also, importantly, it approximates the log transformation for all but the lowest values of z (Bellemare and Wichman, 2018) so that the estimated coefficients can be interpreted just as a log transformation. With the log transformation,

[18] The results presented are not sensitive to these choices. If standard errors are clustered at the county level (or are even left unadjusted), the statistical significance of the main results persist. If a more saturated county by year set of fixed effects, along with month of the year fixed effects, are included the statistical significance of the DiD coefficient is maintained.

Table 3: Bargaining in the Shadow of the Trial?

Dep. variable:	**ln(sent.)**	**ln(sent.)**	**IHS(sent.)**	**IHS(sent.)**
Model:	**OLS**	**Tobit**	**OLS**	**Tobit**
Plea Bargain	-0.781 ***	-0.950 ***	-0.800 ***	-0.981 ***
	(0.132)	(0.152)	(0.073)	(0.127)
Death	-0.087	-0.175 *	-0.132 **	-0.233 **
	(0.065)	(0.090)	(0.073)	(0.092)
Death x Plea Bargain	0.221 ***	0.347 ***	0.264 ***	0.379 ***
	(0.059)	(0.080)	(0.066)	(0.083)
Demographic Controls?	Yes	Yes	Yes	Yes
Case Characteristics?	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes	Yes	Yes
R^2	0.089	0.031	0.093	0.031
AIC	5.7x10^6	5.2x10^6	6.2x10^6	5.8x10^6

Standard errors clustered by County x Year presented in the parentheses (695 clusters).
*** 1%; ** 5%; * 10% level of significance.
Demographic controls are indicator variables for the defendant being female, Black, Asian, Native American, or a business. Also, a control variable for age is included.
Case characteristics are indicator variables for whether the defendant had a public defender, court appointed attorney, self-defense, "other", or missing-info defense (hence, a white male with a private defense attorney is the omitted category). A variable for the total number of counts charged is also included.
$N = 1,566,892$

we follow the standard in the literature by setting $ln(0) = 0$ in effect treating non-incarcerations as if they received one month in jail.[19] With the alternative transformation, this problem can be avoided.

Second, the data is left-censored at zero. In fact, 30% of the convictions resulted in alternatives to active incarceration.[20] Along with presenting results from the fixed effect linear regression, we also present results from a Tobit specification left-censored at zero. Tobit estimations of sentencing is a commonly employed technique in empirical criminology research (Albonetti, 1997). In upcoming robustness checks, we will consider alternative estimations; namely hurdle models and quantile regressions. Table 3 presents the initial results.

Importantly, across all specifications, the difference-in-difference coefficient is positive and statistically significant. Given the quasi-natural experiment affecting these defendants, the results are consistent with the hypothesis that an increase in the probability a jury votes to convict defendants causes negotiated plea deals to be less favorable, relative to sentences arising from trial convictions.

Plea bargains are associated with shorter sentences. Using the first column an individual who is sentenced after entering a guilty plea receives an incarceration that is only 45.8% as long as an individual who is convicted at trial. Rather, he receives a 54% plea discount. When a law enforcement official dies in the line

[19] This also requires setting incarcerations of less than one month equal to one month as well.
[20] Overall, 30.62% of all pursued felony convictions and 30.23% of convictions do not have active incarceration.

of duty, using the first column, the plea discount is only 43%.[21] Thus, the difference between trial and plea bargaining sentencing shrinks.

The significance of the difference-in-difference coefficient remains if a Tobit is estimated, (2), and if the alternative transformation is considered. In the former, the magnitude of the change in the plea discount is larger. In addition, county and time fixed effects are separately included. If a more saturated model, with County x Year fixed effects are used, the main results are unaffected. Also, if the standard errors are clustered only at the county level, or if two-way clustering is estimated, the difference-in-difference coefficient's statistical significant (at the 1% level) remains in all specifications. Thus, the results are not sensitive to the estimation techniques presented.

It is reasonable to presume that law enforcement responds to the death of a law enforcement official. This would be important in our analysis if arrest and charging immediately after the death changes. The results in Table 3 consider those cases already in process at the time of the death, but those cases arising immediately after the death are pooled with those not close (either temporally or spatially) to the death. If arrests made within the week after the death, or arrests made within the month after the death, are excluded from the analysis, the analysis is relatively unchanged. For example, using the specification in the first column of Table 3, the plea discount drops by 20.9% with the death of the law enforcement official. This effect grows to a 21.1% decrease and 21.4% decrease if the arrests occurring within the week and month after the death, respectively, are omitted.

The results in Table 3 pool together all crimes. One would not reasonably expect the death of a law enforcement official to affect all crimes the same. Violent and serious crimes are expected to have stronger impacts.

Table 4 separates serious from non-serious crimes. Specifically, we consider the criminal chapters that have at least 10,000 observations. From those, we select seven crimes viewed commonly as serious: Homicide, sexual battery, robbery, burglary, assault, arson, and weapons-related charges.

Considering serious crimes, the difference-in-difference coefficients are greater in magnitude than those presented in Table 3. Using the first column, guilty pleas receive a 47.4% plea discount relative to those convicted at a trial, and this shrinks to 22.9% when a law enforcement official dies. Thus, the plea discount decreases by substantially more than for overall felonies. This is further consistent with our hypothesis that it is juror's salience in the problems of violent crime that is driving the plea bargaining process.

While not presented here, if serious crimes are removed from the data set, so that only non-serious felonies are considered, the impact of a death of a law enforcement official can be evaluated. If all specifications are re-estimated for this subsample, the difference-in-difference coefficient is positive and statistically insignificant in all. For example, using the first column, the difference-in-difference coefficient is only 0.012 with a p-value of 0.89. Thus, the effect on plea bargaining is essentially zero for non-serious crimes.

To further appreciate how plea bargaining over serious crimes react to the exogenous shock, the next table

[21] Using the inverse sine hyperbolic transformation, the estimated plea discounts are 55.1% and 41.5% with the law enforcement official deaths. Thus, the two provide nearly identical estimates.

Table 4: Serious Crimes

Dep. variable:	**ln(sent.)**	**ln(sent.)**	**IHS(sent.)**	**IHS(sent.)**
Model:	**OLS**	**Tobit**	**OLS**	**Tobit**
Plea Bargain	-0.643 ***	-0.670 ***	-0.609 ***	-0.607 ***
	(0.079)	(0.095)	(0.085)	(0.097)
Death	-0.279 ***	-0.404 ***	-0.330 ***	-0.457 ***
	(0.076)	(0.102)	(0.083)	(0.111)
Death x Plea Bargain	0.383 ***	0.526 ***	0.434 ***	0.585 ***
	(0.074)	(0.099)	(0.083)	(0.110)
Demographic Controls?	Yes	Yes	Yes	Yes
Case Characteristics?	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes	Yes	Yes
R^2	0.115	0.040	0.121	0.039
AIC	2.3x10^6	2.1x10^6	2.4x10^6	2.3x10^6

Standard errors clustered by County x Year presented in the parentheses (649 clusters).
*** 1%; ** 5%; * 10% level of significance.
Demographic and case characteristics controls are the same as in Table 3.
$N = 608,622$

separates the seven crime categories. Each row represents a different estimation with a differing subsample of observations.

The alteration in guilty plea's sanction is not confined to one particular type of crime, but arises across numerous serious crimes. Statistically significant difference-in-difference coefficients arise for five different crime categories: Robbery, burglary, assault, homicide, and sexual battery.

The magnitude of the plea discount's reduction varies by the type of crime committed. This can be due to the differing lengths of incarceration, but also differences in the probability of conviction if the crime is taken to trial. Homicide and sexual battery have the largest reductions in the plea discount. The negative marginal effect of arson is an artifact of its statistical insignificance, and we interpret it as a zero effect.

Surprisingly, the difference-in-difference coefficient is positive, but statistically insignificant for weapons-related felonies. One would expect the death of a law enforcement official, frequently caused by a gunfire shooting, would be especially salient in gun-related crimes. These observations, though, are those crimes where the top charge is a weapons charge and, thus, can include less-serious violations such as violations of gun registrations.

Therefore, we drill down into the sections within this criminal chapter. We engage in the following procedure. We first select those sections with more than 1000 observations. There are three (Sections 1, 19, and 23). We then separate the data into three subsamples and estimate the main specification (the first column of Table 3) on each subsample. One and only one section emits a positive and statistically significant difference-in-difference coefficient − Section 23. The difference-in-difference coefficient is 0.410

Table 5: Breakdown by Crime

	DiD	(s.e.)	N	**%Δ plea discount**
Robbery (812)	0.467 ***	(0.123)	275,290	-82%
Burglary (810)	0.341 **	(0.143)	144,370	-95%
Assault (784)	0.232 *	(0.124)	114,040	-68%
Weapons (790)	0.303	(0.190)	34,708	-60%
Arson (806)	-0.400	(0.470)	17,431	+47%
Homicide (782)	0.926 ***	(0.248)	11,529	-241%
Sexual Battery (794)	0.804 ***	(0.239)	11,254	-156%

Standard errors clustered by County x Year presented in the parentheses.
*** 1%; ** 5%; * 10% level of significance.
The specification reported is the reproduction of the first column of Table 3 and Table 4 for each subsample. Only the difference-in-difference coefficient is presented.
The crime's chapter in Florida's criminal code is provided in parentheses.

($p < 0.05$). For all other sections within the weapons chapter, the difference-in-difference coefficient is statistically insignificant. We then consulted the Florida criminal codes to identify which crime is captured in Section 23. It is felons in possession of firearms. Thus, once we drill down to specific crimes within the broad charge of weapons offenses, the one that involves felons illegally having guns is shown to have plea bargaining affected by the shock to the jury pool.

We also engaged in this drilling down procedure for homicides. While all homicide crimes are serious, there is a substantial amount of heterogeneity in potential sentence lengths. One can be concerned that a shift in the mix of cases within this chapter occurs. Here, we separate any section that has more than 200 observations (there are fewer homicides overall). There are four sections. We then re-estimate our main model on each subsample independently. Again, one and only one section has a positive and statistically significant difference-in-difference coefficient − Section 4. Its difference-in-difference coefficient is 1.012 ($p < 0.001$). Consulting the criminal codebook, this is the killing (without design) when engaged in another felony. Again, this is a prime example of a violent activity that we can reasonably expect to have a jury pool who is affected by the violent death of a law enforcement official.[22]

As mentioned previously, estimated the plea discount in sentencing data is difficult due to the high prevalence of zeros. As is common, we presented results from a Tobit regression, which accounts for the left censoring of the data. While the statistical significance of the results persist with this alternative estimation, there are two shortcomings. First, the coefficients are notoriously difficult to interpret. Second, the Tobit model does not account for the two-stage decision process in the sentencing of less-serious crimes. It may be more reasonable to think of a judge first deciding whether or not to incarcerate. Conditional on making the decision to incarcerate, the sentence length is determined. A hurdle model more appropriately estimates this two-stage process. Doing so allows us to separately identify the impact of the law enforcement official's

[22]Also, since the number of observations of this section are sufficiently high ($N = 9335$) the heterogeneous effects can be explored (see the upcoming subsection for details). Gun related deaths of law enforcement officials see a substantially greater difference-in-difference effect than non-gun related deaths (1.318 vs. 0.706), high levels of Google searching after the death have a much larger effect (1.354 vs. 0.781), and spikes in Google searches also has a larger effect (1.309 vs. 0.880).

Table 6: Hurdle Model

Dataset:	full		serious	
Model:	**Intensive Margin**	**Selection Model**	**Intensive Margin**	**Selection Model**
Plea Bargain	-0.990 ***	-0.144 ***	-1.052 ***	0.067 ***
	(0.008)	(0.008)	(0.011)	(0.011)
Death	0.193 ***	-0.148 ***	0.058	-0.206 ***
	(0.029)	(0.025)	(0.040)	(0.034)
Death x Plea Bargain	-0.012	0.183 ***	0.081 **	0.217 ***
	(0.030)	(0.026)	(0.041)	(0.035)
Demographic Controls?	Yes	Yes	Yes	Yes
Case Characteristics?	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes	Yes	Yes
R^2	0.043		0.055	
AIC	4.9x10^6		1.9x10^6	

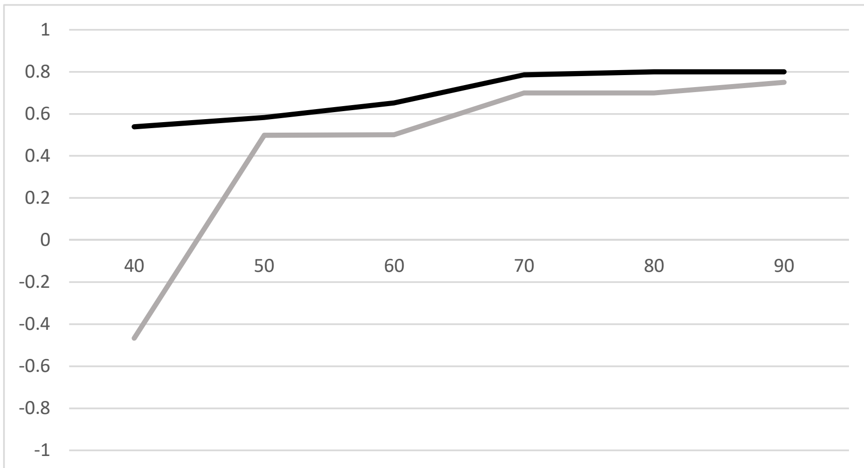
The dependent variable is *ln(sentence)*.
Cragg Hurdle model estimated where age and being a business are used in the unique controls in the selection model and total number of counts is used as the unique variable in the extensive margin estimation. The race, gender, and type of counsel controls are included in both specifications (along with county and time fixed effects).
Standard errors clustered by County x Year presented in the parentheses (659/649 clusters).
*** 1%; ** 5%; * 10% level of significance.
$N = 1,566,892$ for the first two columns and $N = 608,622$ for the last two.

death on the likelihood of receiving a sanction with active incarceration (akin to its extensive margin) and the length of that sentence when incarceration arises (the intensive margin).

Thus, we estimate a Cragg hurdle model (Cragg, 1971). This requires unique variables to be used to estimate the selection model first, and then unique variables to be used in the intensive margin estimation. For the former, we choose to use the age of the defendant and whether the defendant is a business. Our belief is that a judge is more likely to choose a non-incarceration sentence for younger defendants. Also, for the small number of cases where a business is the criminal defendant, incarceration is not an option. For the latter model, we use the total number of counts as the explanatory variable unique to the estimation of the sentence length under the presumption that the number of counts most dramatically affects the length of the incarceration. Each estimation includes the gender, race, and type of counsel controls because we feel that the disparities existing in these three dimensions is likely to affect both whether a defendant is incarcerated and how long the incarceration lasts. County and time fixed effects are included in both as well. Table 6 presents the results.

Interestingly, for the full data set of all crimes the death of the law enforcement official only affects the plea discount through the decision of whether or not to incarcerate the criminal. Conditional on incarceration, the difference-in-difference coefficient is essentially zero. Once the data set is restricted to only convictions

Figure 3: Quantile Regression Results



The black line is the estimated plea discount without a law enforcement death. The gray line is the plea discount with it.
Results from a quartile regression estimated at the 0.4, 0.5, 0.6, 0.7, 0.8, and 0.9 deciles for the full data set of convictions. For computational ease, the model without the fixed effects and controls is presented.

of serious crimes, both the selection model and the intensive margin model show that the death acts to influence the plea bargaining outcome. Those who enter a guilty plea for a serious crime are more likely to be incarcerated. This effect is larger when there is also a death of a law enforcement official. Conditional on receiving active incarceration, an individual who pleads guilty receives a sizeable discount. It is mitigated when a death has occurred.

An alternative way to death with the “zero problem” in sentencing data was proposed by Rehavi and Starr (2014) in their study of racial disparities in sentencing. They argue that estimating quantile regressions is best. By focusing on the 40^{th} through the 90^{th} deciles of sentencing, the effects of race on the length of the sentence can be evaluated across these different lengths of sentences without being distorted by the bulk of cases with zero sentence sanctions. Our data is similar to theirs in that just less than 30% of the observations have a non-incarceration sanction. Thus, we too begin the estimation at the 40^{th} decile. Hence, we follow their lead and estimate a quantile regression.

We choose to depict it graphically. The black line is the plea discount received normally. The gray line is the plea discount received when a law enforcement official dies in the line of duty.

Table 7: Gun Related Deaths

Dep. variable: Model:	ln(sent.) OLS	ln(sent.) Tobit	IHS(sent.) OLS	IHS(sent.) Tobit
Plea Bargain	-0.639 ***	-0.401 ***	-0.614 ***	-0.613 ***
	(0.078)	(0.177)	(0.082)	(0.096)
Gun Death	-0.261 *	-0.455 ***	-0.319 **	-0.464 **
	(0.136)	(0.170)	(0.141)	(0.175)
Non-Gun Death	-0.319 **	-0.424 ***	-0.366 ***	-0.476 ***
	(0.125)	(0.162)	(0.149)	(0.186)
Gun Death x Plea Bargain	0.405 ***	0.572 ***	0.465 ***	0.639 ***
	(0.128)	(0.167)	(0.140)	(0.178)
Non-Gun Death x Plea Bargain	0.416 ***	0.554 ***	0.466 ***	0.617 ***
	(0.125)	(0.156)	(0.133)	(0.164)
Demographic Controls?	Yes	Yes	Yes	Yes
Case Characteristics?	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes	Yes	Yes
R^2	0.115	0.040	0.121	0.039
AIC	2.3x10^6	2.1x10^6	2.4x10^6	2.3x10^6

Standard errors clustered by County x Year presented in the parentheses (649 clusters).
*** 1%; ** 5%; * 10% level of significance.
Demographic and case characteristics controls are the same as in Table 3.
Results from convictions of serious crimes presented; $N = 608,622$.

Across all deciles the plea discount shrinks when the law enforcement official dies in the line of duty. Also, for each decile both the coefficient on the plea bargain and the difference-in-difference coefficient are statistically significant at the 5% level. The plea discounts grow when the severity of the sentence is greater, but the gap between the two is relatively constant. Interestingly, for the sanctions with the least-severe sentences, the death of a law enforcement official coincides with a trial penalty. Similar results arise if the data set is restricted to convictions of serious crimes.

4.2 Heterogeneous Effects

Our hypothesis is that the death of the law enforcement official is working through the community's salience with the problems of crime. One would not expect all deaths to have equivalent impacts. Incidents that resonate louder with a community should have larger impacts on the plea bargaining process.

To identify heterogeneous effects, we first distinguish gun related deaths from non-gun related deaths. Presumably, a shooting primes potential jurors on the problem of violence. We disaggregate the variable *Death* into those caused by gunfire, *Gun Death*, and those not related to gunfire, *Non-Gun Death*.[23]

Disaggregating the cause of the law enforcement official's death, both gun-related deaths and non-gun

[23] Hence, *Gun Death* + *Non-Gun Death* = *Death*. In the specifications presented here, we use not only deaths occurring between the initial arrest phase and the prosecutor phase, but also include deaths that occur the week before the trial if one occurs.

related deaths see an increase in the sentences received when a guilty plea is entered, relative to the trial sanction. Thus, both types of events matter.

Anecdotally, quite a few of the non-gun related deaths were newsworthy events. Oftentimes, the vehicle-related deaths are impactful. Law enforcement officials have died in high speed pursuits, for example. Also, the stabbing of a deputy sheriff transporting a prisoner to a court hearing which resulted in a widespread manhunt, and a corrections officer being stabbed by a inmate on a new, controversial work release program, are examples of non-gun related deaths.

Another way we identify heterogeneous effects is through Google search data. We use Google Trends using the search phrase "Law Enforcement Official". Data from 2004-14 is collected. Specifically, Google Trends provides a normalized number of searches for the phrase by month. The data is normalized by the peak observation over that time period.

We drill down to just searches in the state of Florida. Within Florida, the state is separated into regions. We collect the normalized Google Trends search data for each region for each month our data covers. Then, for each law enforcement official death, we use the county in which s/he was employed and link it to the region defined by Google. This process gives us information about the changes in Google searches related to law enforcement in the region at the time of the death.

Figure 4 depicts a case study of Google searches in the Tampa region of Florida from late 2005 to 2007. In September 2006 a deputy sheriff of Polk County died. A rather stable level of searches existed before the incident and a notable spike occurs during the month of the death. Following the death, search behavior drops off again until, again and unfortunately, a deputy sheriff of Jackson county dies. Once again, Google searches in the Tampa region spike during the month. Shortly afterward, they return to their normal levels.

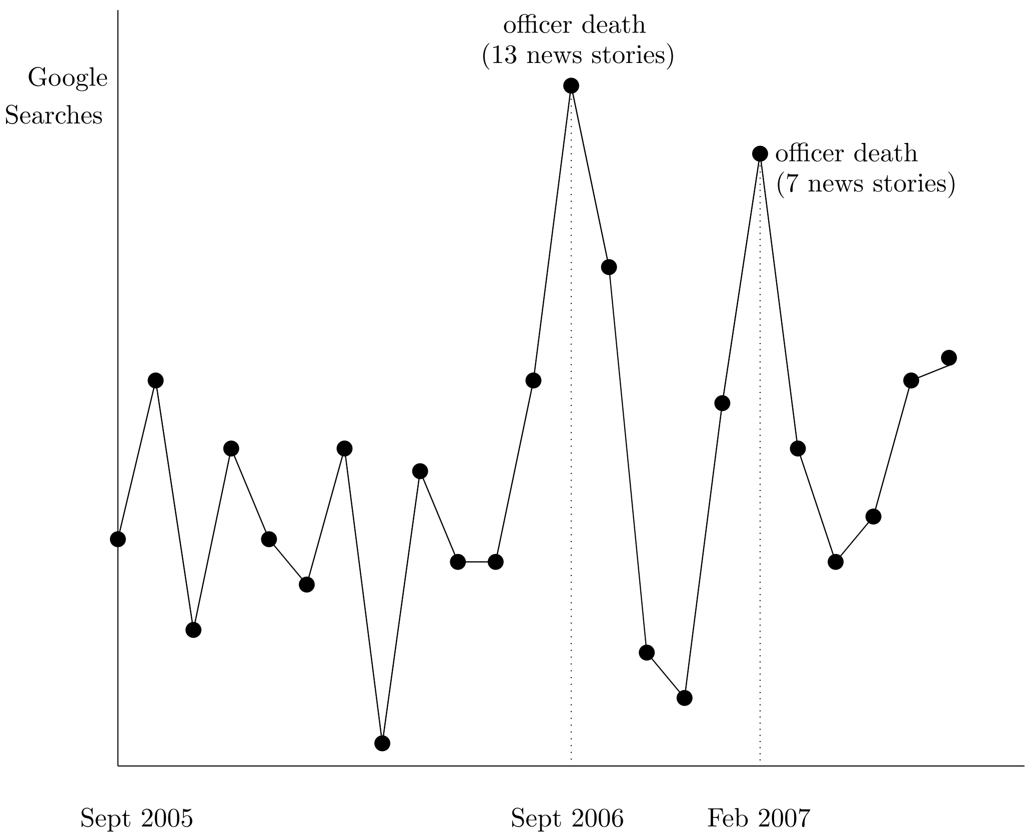
One way we choose to code a high-level of internet searches in a region is to consider that region's mean and standard deviation in his monthly searches. We then identify those months where the searches exceed one standard deviation above that region's mean. For those law enforcement deaths that coincide with the Google Trends data having this high of a level of searches, we denote the death as *Many Searches*. For law enforcement deaths that do not record high levels of searches, we denote that death as a *Few Searches*. It follows, then, that *Many Searches* + *Few Searches* = *Death*.

Thus, as we did with gun related deaths, we disaggregate in the econometric investigation law enforcement deaths by whether they corresponded to a high level of Google searches or not. The following table presents the results.

While both difference-in-difference coefficients are positive and statistically significant, those events that are associated with many searches have a greater effect. Without a law enforcement death, the plea discount is 47%. When a death occurs that corresponds with high level of Google searching, the plea discount shrinks to only 16%. If there is not a high level of searching, the plea discount is 26%. The change is less dramatic if residents are not searching for "Law Enforcement Official" as frequently.

A second way we take advantage of the Google Trends data is to consider those deaths where there is an

Figure 4: Google Trends Case Study



Each data point represents the number of Google searches for “Law Enforcement Official” in the Tampa region of Florida.
The first peak represents the death of Deputy Sheriff Vernon Matthew Williams of the Polk County Sheriff’s Office on September 28, 2006. The second peak coincides with the death of Deputy Sheriff Harold Michael Altman of the Jackson County Sheriff’s Department on January 30, 2007.

Table 8: High Levels of Google Searches

Dep. variable:	**ln(sent.)**	**ln(sent.)**	**IHS(sent.)**	**IHS(sent.)**
Model:	**OLS**	**Tobit**	**OLS**	**Tobit**
Plea Bargain	-0.635 ***	-0.670 ***	-0.608 ***	-0.607 ***
	(0.079)	(0.095)	(0.083)	(0.097)
Many Searches	-0.324 **	-0.497 ***	-0.397 **	-0.575 ***
	(0.144)	(0.189)	(0.157)	(0.197)
Few Searches	-0.252 **	-0.350 **	-0.291 **	-0.390 ***
	(0.111)	(0.140)	(0.120)	(0.147)
Many Searches x Plea Bargain	0.475 ***	0.666 ***	0.542 ***	0.739 ***
	(0.135)	(0.180)	(0.149)	(0.192)
Few Searches x Plea Bargain	0.330 ***	0.445 ***	0.372 ***	0.497 ***
	(0.107)	(0.136)	(0.117)	(0.147)
Demographic Controls?	Yes	Yes	Yes	Yes
Case Characteristics?	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes	Yes	Yes
R^2	0.115	0.040	0.121	0.039
AIC	2.3x10^6	2.1x10^6	2.4x10^6	2.3x10^6

Standard errors clustered by County x Year presented in the parentheses (649 clusters).
*** 1%; ** 5%; * 10% level of significance.
Demographic and case characteristics controls are the same as in Table 3.
Google searches in that region for that month for "Law Enforcement Official" greater than one standard deviation above that region's mean.
Results from convictions of serious crimes presented; $N = 608,622$.

Table 9: Spike in Google Searches

Dep. variable: Model:	**ln(sent.) OLS**	**ln(sent.) Tobit**	**IHS(sent.) OLS**	**IHS(sent.) Tobit**
Plea Bargain	-0.635 ***	0.670 ***	-0.609 ***	-0.607 ***
	(0.079)	(0.095)	(0.083)	(0.097)
Search Spike	-0.317 **	-0.485 **	-0.391 **	-0.570 ***
	(0.154)	(0.202)	(0.168)	(0.212)
No Search Spike	-0.261 **	-0.367 ***	-0.302 ***	-0.406 ***
	(0.105)	(0.133)	(0.114)	(0.139)
Search Spike x Plea Bargain	0.526 ***	0.735 ***	0.607 ***	0.825 ***
	(0.123)	(0.163)	(0.134)	(0.172)
No Search Spike x Plea Bargain	0.319 ***	0.432 ***	0.356 ***	0.477 ***
	(0.102)	(0.131)	(0.112)	(0.141)
Demographic Controls?	Yes	Yes	Yes	Yes
Case Characteristics?	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes	Yes	Yes
R^2	0.115	0.040	0.121	0.039
AIC	2.3x10^6	2.1x10^6	2.4x10^6	2.3x10^6

Standard errors clustered by County x Year presented in the parentheses (649 clusters).
*** 1%; ** 5%; * 10% level of significance.
Demographic and case characteristics controls are the same as in Table 3.
Google searches in that region for that month for "Law Enforcement Official" where the number of searches increases by 5 points or more from the previous month.
Results from convictions of serious crimes presented; $N = 608,622$.

uptick in the month, as compared to the previous month. Since the Google Trends data is normalized by that region's peak observation over the time period, we define a death as *Search Spike* if there is an increase in searches by five or more points. An event that does not see a spike in the Google Trends data is *No Search Spike*. Again, *Search Spike* + *No Search Spike* = *Death*.

The following table presents the disaggregation using changes in Google searches.

Once again, both difference-in-difference coefficients are positive and statistically significant. Importantly, though, the coefficient on the spike in searches is 69% larger. Thus, community interest is correlated with large reductions in the plea discount.

Finally, we consider the spatial dimension to the law enforcement official's death. It is reasonable to presume that the unfortunate incident not only affects those within the community but, due to media coverage, spills over and affects residents across the state. Thus, we create a new indicator variable, *Death State*, which equals one if a death of a law enforcement official occurs anywhere in the state while that particular observation has his/her case in process at the time. We include it in our primary specifications (those presented in Table 3). This allows us to separate the local community effect from the statewide effect.

The results come up as one would expect. The difference-in-difference coefficients are both positive and

Table 10: Affect Across the State

Dataset:	**full**	**serious**
Plea Bargain	-0.830 ***	-0.701 ***
	(0.132)	(0.084)
Death	0.014	-0.161 **
	(0.006)	(0.078)
Death State	-0.177 ***	-0.198 ***
	(0.036)	(0.049)
Death x Plea Bargain	0.153 ***	0.283 ***
	(0.137)	(0.075)
Death State x Plea Bargain	0.115 ***	0.166 ***
	(0.037)	(0.048)
Demographic Controls?	Yes	Yes
Case Characteristics?	Yes	Yes
County Fixed Effects?	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes
R^2	0.089	0.116
AIC	5.7x10^6	2.3x10^6

Standard errors clustered by County x Year presented in the parentheses (659/649 clusters).
*** 1%; ** 5%; * 10% level of significance.
Demographic and case characteristics controls are the same as in Table 3.
$N = 1,566,892$ for the first column and $N = 608,622$ for the second.

statistically significant. The local effect is substantially greater than the statewide effect. The statewide effect, though, still matters. Within the county, the plea discount drops by 23.8%, but in the rest of the state a death of a law enforcement official reduces the plea discount by 9.4%.[24] Considering only serious crimes, the statewide effect is stronger. The plea discount reduces by 32.2% across the state for serious crimes, and 55.8% locally. Therefore, the spatial effects coincide with the death having an important impact regionally. By ignoring the statewide effect in the main result (Table 3), the magnitude of the estimated effect was slightly downward biased.

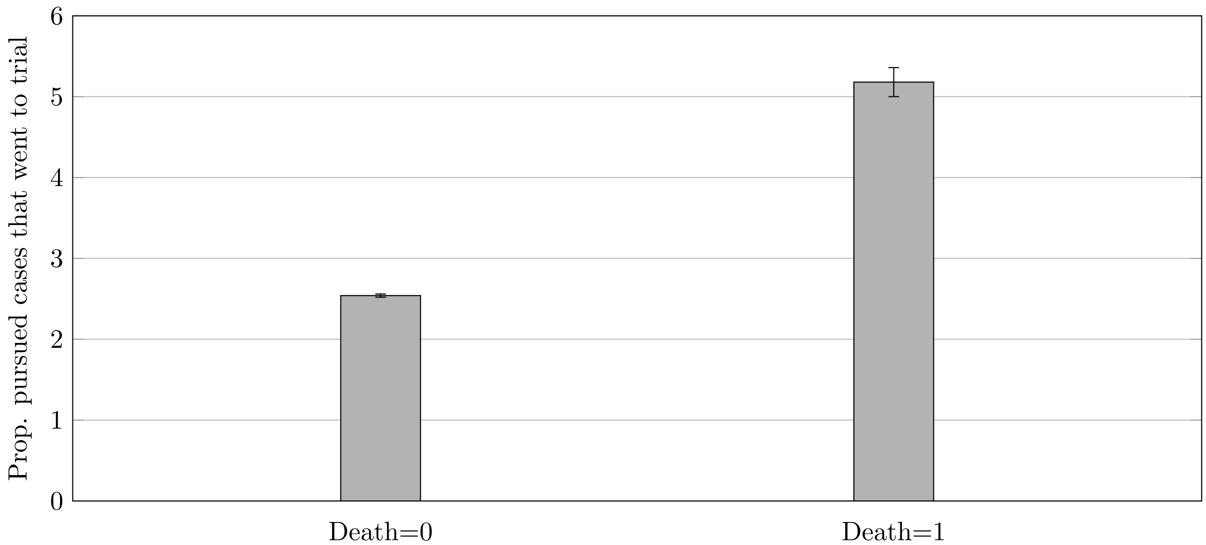
5 Going to Court

Up to this point, the analysis has focused on sentences received. We now turn to an analysis of whether the case, when pursued by the prosecutor's office, ends up at trial. Therefore, we consider the data set of all cases where at least one count is not dismissed. Unlike the previous section, though, we also include cases that receive an acquittal at trial.

First, we separate the sample into those observations coinciding with a law enforcement official's death

[24]While not presented here, the court circuit can also be disentangled. In Florida, the 67 counties are organized into 20 judicial circuits. Including it the difference-in-difference coefficient is statistically insignificant. The difference-in-difference coefficients for *Death* and *Death State* retain their sign and statistical significance. Thus, the circuit-wide effect does not differ from the statewide effect.

Figure 5: Does LEO Death Correspond to Trials?



A two-tailed, difference-in-means t-test (allowing for unequal variances) has $t = 29.5$ ($p < 0.001$).

in the county and those without. Figure 5 presents the subsample average proportion of cases that went to trial.

There is a noticeable difference. Trials are more prevalent when the shock of a death occurs. This is consistent with the argument that the plea offers made by prosecutors become substantially less generous, pushing the accused to take their chances at trial instead.

The following table provides results from a linear probability model with the dependent variable equal to one if the case resulted in a trial.[25] We explore whether there is a systematic relationship between the exogenous shock of an official's death and the probability of a trial. All fixed effects previously used are included, and standard errors are again clustered at the County x Year level.

The death of a law enforcement official results in an increase in the probability the case results in a trial, consistent with Figure 5. Using the estimated coefficient in the first column, the trial rate increases by 71%. The effect is even larger if only serious crimes are considered.

Consequently, accused criminals are treated more harshly in two margins. Facing an escalated chance of conviction at trial, they agree to longer sentences in the plea negotiation. This is, then, an increase in the intensive margin of plea bargaining. More do not accept those terms and go to trial, where they face the prospect of the full sanction without the plea discount. Thus, the extensive margin moves as well.

As before, it is instructional to disaggregate the serious crimes.

The trial rate increases for the most serious crimes. The exceptions are the last two crimes: Homicide and sexual battery. These show reductions in the trial rate. This is noteworthy because they were the two

[25]Here, we do not differentiate between jury trials and non-jury, bench trials. The former is rather rare in Florida. In the full data set 2.2% of cases result in a jury trial, while only 0.5% result in a bench trial.

Table 11: Trial Rate

Dataset:	**full**	**full**	**serious**
Dep. variable:	**Trial**	**Trial**	**Trial**
Model:	**Linear Prob.**	**Logit**	**Linear Prob.**
Death	0.0190 ***	0.5042 ***	0.0246 ***
	(0.0028)	(0.0645)	(0.0038)
Demographic Controls?	Yes	Yes	Yes
Case Characteristics?	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes	Yes
R^2	0.016	0.060	0.019
AIC	-1.3x10^6	3.6x10^5	-3.1x10^6
N	1,575,760	1,575,760	614,201

Standard errors clustered by County x Year presented in parentheses (695/649 clusters).
*** 1%; ** 5%; * 10% level of significance.
Dependent variable equal to one if any count charged is disposed at trial (jury or nonjury & convicted or acquitted).
Demographic and case characteristics controls are the same as in Table 3.

Table 12: Breakdown by Crime

	DiD	(s.e.)	N
Robbery (812)	0.0207 ***	(0.0039)	276,623
Burglary (810)	0.0156 ***	(0.0049)	145,031
Assault (784)	0.0194 ***	(0.0052)	116,286
Weapons (790)	0.0163 **	(0.0069)	34,969
Arson (806)	0.0187 ***	(0.0065)	17,576
Homicide (782)	-0.0180	(0.0214)	11,966
Sexual Battery (794)	-0.0224 *	(0.0134)	11,750

Standard errors clustered by County x Year presented in parentheses.
*** 1%; ** 5%; * 10% level of significance.
Dependent variable equal to one if any count charged is disposed at trial (jury or nonjury & convicted or acquitted).
The demographic controls, case characteristics controls, county fixed effects, and month x year fixed effects are all included.

crimes that saw the largest reduction in the plea discount. For these two types of violent offenses, a harsher sanction is agreed to in an attempt, presumably, at avoiding the conviction at a trial. Rather, the motivation to avoid the conviction at trial dominates the less-generous plea offer.

6 Is it Really the Jurors?

Our argument is that the exogenous shock affects the community and, hence, affects the jury pool. The salience of violence and crime in society, we argue, makes the jury more likely to convict a defendant at trial. Recognizing this, the other actors in the criminal justice system, namely the prosecutor and defense attorney & defendant, respond in their negotiations and the resulting "price" of the crime increases.

While direct measurement of the actors' beliefs about jurors is not measurable, the data available can be used to explore the existence of suggestive evidence.

One interesting feature of the criminal justice system is that a small proportion of cases go to bench trials. In bench trials there is not a jury, but instead the judge both makes the conviction/acquittal decision and sentences (conditional on conviction). While the decision to not exercise one's right to a jury trial is endogenous, we separate jury trials from bench trials and evaluate the relationship between the death of a law enforcement official and the sentencing that arises from these two separate institutions.

Distinguishing judge behavior from jury behavior is important. If it is the case that judges simply sentence harsher, both after a negotiated plea and a trial conviction, then it is not necessarily obvious that the plea negotiations existed in the shadow of the trial. If, on the other hand, the jury changes its anticipated behavior, then changes in the plea discounts verify that the bargaining in the shadow of the trial framework is correct. If bench trials also see harsher sentencing, then our hypothesis can be questioned.

Therefore, Table 13 re-estimates the main results presented previously. The important change is that while the results in Section 4 use *Plea Bargain* as the explanatory variable (with trials as the omitted variable), here we leave plea bargains as the omitted variable and include the trial as the explanatory variable. We separate these trials into those that occurred with a jury, *Jury Trial*, and those that did not, *Bench Trial*. Hence, *Plea Bargain* + *Jury Trial* + *Bench Trial* = 1 (for the sample of convictions). Thus, the difference-in-difference coefficient can be disaggregated as well. Table 13 includes all fixed effects as previously used and clusters the standard errors as before.

Across the specifications in the top panel, those who go to a jury trial receive harsher sanctions than those who enter a guilty plea (the omitted category). This difference is less when a law enforcement official dies in the line of duty. This effect does not exist for nonjury, bench trials. While bench trials experience less-severe sanctions for serious crimes (and essentially a zero difference for overall felony crimes), there is not any change in sentencing with the death. This is evidence strongly suggesting that the change in the plea discount is driven by anticipated changes in the juror's behavior and not necessarily the judge's behavior.

The bottom panel considers whether the case goes to trial. The dependent variable is equal to one if the case ends up in a bench trial, for the first and third columns, and whether the case goes to a jury trial, for

Table 13: Is it Really Affecting Jurors?

Dataset:	**convict.**	**convict.**	**convict.**	**serious**	**serious**	**serious**
Dep. variable:	**ln(sent.)**	**IHS(sent)**	**IHS(sent.)**	**ln(sent)**	**IHS(sent.)**	**IHS(sent.)**
Model:	**OLS**	**OLS**	**Tobit**	**OLS**	**OLS**	**Tobit**
Death	0.132 ***	0.130 ***	0.143 ***	0.103 ***	0.103 ***	0.126 ***
	(0.025)	(0.029)	(0.037)	(0.030)	(0.034)	(0.046)
Jury Trial	1.034 ***	1.053 ***	1.129 ***	0.797 ***	0.779 ***	0.795 ***
	(0.039)	(0.044)	(0.056)	(0.042)	(0.047)	(0.062)
Bench Trial	-0.159	-0.142	-0.063	-0.331 **	-0.402 ***	-0.554 ***
	(0.144)	(0.130)	(0.112)	(0.127)	(0.125)	(0.156)
Death x Jury Trial	-0.268 ***	-0.313 ***	-0.435 ***	-0.447 ***	-0.513 ***	-0.692 ***
	(0.039)	(0.074)	(0.091)	(0.057)	(0.089)	(0.116)
Death x Bench Trial	-0.040	-0.076	-0.154	0.057	0.085	0.138
	(0.062)	(0.074)	(0.122)	(0.100)	(0.116)	(0.175)
Demographics?	Yes	Yes	Yes	Yes	Yes	Yes
Case Char.?	Yes	Yes	Yes	Yes	Yes	Yes
County?	Yes	Yes	Yes	Yes	Yes	Yes
Month x Year?	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.091	0.095	0.032	0.117	0.122	0.040
AIC	5.7x10^6	6.2x10^7	5.8x10^6	2.3x10^6	2.4x10^6	2.3x10^6
N	1,566,892	1,566,892	1,566,892	608,622	608,622	608,622

Dataset:	**full**	**full**	**serious**	**serious**
Dep. variable:	**Bench Trial**	**Jury Trial**	**Bench Trial**	**Jury Trial**
Model:	**Linear Prob.**	**Linear Prob.**	**Linear Prob.**	**Linear Prob.**
Death	0.0019	0.0171 ***	0.0029	0.0217 ***
	(0.0020)	(0.0022)	(0.0029)	(0.0031)
Demographic Controlss?	Yes	Yes	Yes	Yes
Case Characteristics?	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes	Yes	Yes
R^2	0.036	0.008	0.020	0.016
AIC	-3.9x10^6	-1.6x10^6	-1.6x10^6	-3.9x10^6
N	614,201	614,201	1,575,760	1,575,760

Standard errors clustered by County x Year presented in parentheses (695/649 clusters).
*** 1%; ** 5%; * 10% level of significance.
Demographic and case characteristics controls are the same as in Table 3.

the second and fourth columns. The first two columns consider the full data set of all felony cases and the second two columns consider only the serious crimes (acquittals are included in all four specifications). The death of a law enforcement official coincides with an increase in jury trials, but there is no change in the prevalence of bench trials. This is consistent with the findings in the top panel. If plea bargaining is not changing when a bench trial is the default outcome, then the rate at which these negotiations fail and go to trial should also be unaffected. Again, this is suggestive that the deaths are working through the behavior of the juries in their willingness to convict which is driving the results.

Potentially, a more direct way to evaluate whether it is in fact jurors increasing their likelihood of convicting is to explore the distinction between convictions and acquittals. Conditional on a case going to trial, the probability of conviction should be higher for those cases that experienced the shock of the death. Additionally, in bench trials this effect should not be there if our hypothesis is correct.

As shown, though, there is a change in the volume of cases that go to trial and there is no reason to believe that those new cases that go to trial when there was a death are representative of the cases that would have gone to trial regardless. Thus, one should be hesitant to interpret the results as a causal identification of a change in the jury behavior. Nevertheless, if the probability of conviction at a trial does not increase, or if the conviction probability at a bench trial also increases, then doubt can be cast on the underlying argument we are using in our causal identification.

Thus, Table 14 considers only the data set of cases that go to trial. As before, we consider both the full data set and the subsample of serious felony crimes. This smaller sample of observations is then further partitioned into those cases that were heard by a jury and those having a nonjury, bench trial. As always, a saturated model with demographic and case controls, along with county and fixed effects are included along with clustered standard errors.

The death of a law enforcement official is associated with a decrease in the likelihood of an acquittal arising at trial. This strongly suggests that jurors are responding to the recent event. The magnitude of the effect is slightly greater when only serious crimes are considered. Separating jury trials from bench trials, the effect is fully contained within the sample of jury trials. Here, the probability of acquittal reduces by 39.2%.

It is common to presume that juries provide noise in the system in that they open up the possibility of conviction with weak evidence and acquittal with strong evidence simply because they are nonprofessionals made up of a potentially random, small sample of the population. Gay *et al.* (1989) use this distinction, for an example, in their theoretical analysis of the use of bench versus jury trials. Prominent models of plea bargaining, such as Priest and Klein (1984) and Abrams (2011), which use a random noise component to negociants' expectations of the trial conviction probability, can be motivated by juries distorting accurate expectations in the bargaining process.

One claim is that prosecutors engage in a practice known as "charge stacking" where they add charges to the primary charge. These added charges act as threats in the bargaining process as they are included in

Table 14: Convictions versus Acquittals

Dataset:	**full**	**serious**	**full, jury**	**full, bench**
Death	-0.0753 ***	-0.0812 ***	-0.0910 ***	-0.0058
	(0.0092)	(0.0095)	(0.0081)	(0.0103)
Demographic Controls?	Yes	Yes	Yes	Yes
Case Characteristics?	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes	Yes	Yes
R^2	0.053	0.067	0.039	0.215
AIC	4.0x10^4	2.4x10^4	3.7x10^4	-1.3x10^3
N	41,704	22,753	33,953	7751
DV μ	0.2010	0.2325	0.2323	0.0639

The dependent variable is equal to one if the accused is acquitted of all charges and zero otherwise.
Standard errors clustered by County x Year presented in parentheses (695/649 clusters).
*** 1%; ** 5%; * 10% level of significance.
Demographic and case characteristics controls are the same as in Table 3.

the charges taken to a jury trial if a deal cannot be reached. The offered deal, then, proposes to drop these additional charges in return for a guilty plea for the primary offense. With a noisy jury, the charge stacking strategy can potentially be effective.

Therefore, we look for evidence of charge stacking in our data. Of course, we do not know the circumstances of each case such as when new evidence arises, when witnesses come forward, or when investigation leads to information on the occurrence of additional crimes. We can though consider the number of charges filed as the case proceeds through its process. We create an indicator variable equal to one if the number of charges filed increases from the initial charging phase coming out of the police arrest and the number of charges pursued in the prosecution phase. The following table estimates a linear probability model with the *Increase in Counts* variable as the dependent variable. As always, a full set of controls and fixed effects are included and clustered standard errors are calculated.

The death of a law enforcement official coincides with an increase in the probability of the number of counts increasing. This is consistent with the prosecutor using the heightened chance of a jury convicting on these other counts to secure a less-generous guilty plea. Plea bargained cases see fewer increases in the number of counts and this difference slightly expands with the death of a law enforcement official.

7 Conclusion

This paper seeks to contribute to the scholarly understanding of the mechanism behind the plea bargaining process. As academics have began to study this institution, divergent beliefs have developed. Economists largely argue that plea bargaining occurs in the shadow of the trial, where the trial sentence is treated as the default outcome, and defendants are rewarded with a *plea discount* for sparing the prosecutor the time

Table 15: Charge Stacking

Dataset:	**full**	**full**	**serious** (w/ acquittals)
Dependent var.:	**Increase in Counts**	**Increase in Counts**	**Increase in Counts**
Model:	**Linear Prob.**	**Linear Prob.**	**Linear Prob.**
Death	0.0260 ***	0.0406 ***	0.0362 ***
	(0.0063)	(0.0130)	(0.0111)
Plea Bargain		-0.0225 ***	-0.1750 ***
		(0.0035)	(0.0023)
Death x Plea Bargain		-0.0159	-0.0163 *
		(0.0106)	(0.0089)
Demographic Controls?	Yes	Yes	Yes
Case Characteristics?	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes
Month x Year?	Yes	Yes	Yes
R^2	0.015	0.015	0.016
AIC	-1.4x10^6	-1.4x10^6	-5.2x10^5
N	1,575,760	1,575,760	614,201

Dependent variable equals one if the total number of counts is higher after the prosecutor phase than the initial charging phase.
Standard errors clustered by County x Year presented in parentheses (695/649 clusters).
*** 1%; ** 5%; * 10% level of significance.
Demographic and case characteristics controls are the same as in Table 3.

costs of trial. Criminologists and legal scholars, on the other hand, question the validity of this conceptual framework arguing that structural and psychological factors drive a wedge between expected trial outcomes and the plea bargaining process leaving the two essentially unrelated. Using a quasi-natural experiment we propose to empirically test the accuracy of the bedrock model using case-level data.

Disentangling the plea bargaining process is difficult. As the theory models proposed by both sides of the plea bargaining argument display, there are many moving parts to the process involving the prosecutors and defense teams. We believe that this empirical investigation contributes significantly to the conversation by utilizing an identification strategy that relies on a plausibly exogenous event effecting the jury pool – law enforcement officer deaths. The results offer strong support for the plea bargaining in the shadow of trial framework. Namely, those cases that were in process at the time a police officer was killed in the line of duty within the same county as the arrest face a reduced plea discount of about 20% across all cases, with a larger reduction for cases we classify as serious crimes. To determine if our *ex ante* assumption that this acts through altering the beliefs of the probability of conviction at trial, we use additional measures of our death variable to capture what we argue supports the salience of the police officer death to members of the community. For this, Google Trends information was used to differentiate between those deaths that were more, or less, salient as measured by a standard deviation increase in searches within the region, or a spike above the previous months mean searches. These results are consistent with the hypothesis that more salient deaths will have a larger reduction in the plea discount offered to plea bargaining defendants.

Potential avenues were explored to explain the underlying mechanism. Firstly, we differentiated between jury and bench trials. There is some concern that criminal justice officials could be substantially changing their behavior so as to not be indicative of a change in the expected sanction at trial, or not through the channel of jurors. The results show that there is no effect of these deaths on those cases that had a bench trial, where the result is strong and consistent with previous results for jury trials. Secondly, the phenomenon of charge stacking was addressed. We find evidence suggestive of this practice becoming more prevalent for cases in our defined treatment group. This increase in the probability of the number of counts increasing between the initial and prosecutor phases of the criminal case suggest that prosecutors may be using the increased likelihood of conviction at trial to secure a less-generous guilty plea. This last result potentially offers the channel through which many of these plea discounts are shrinking, though other avenues such as idiosyncratic preferences or beliefs surely contribute, they are unable to be addressed within this present study, and are thus left for future research to consider.

In addition to contributing to the empirical sentencing literature, we provide empirical support for the applied theoretical research on plea bargaining. It is appropriate to consider the default outcome in the bargaining to be the expected outcome at trial. For example, Bjerk (2007) incorporates how the sorting mechanism changes the jury's beliefs about guilty or innocent and explores how this feeds back to the plea bargaining process. The participants response to the change in the jury's beliefs receives justification in our findings. Our results do not, though, weigh in on whether the asymmetric information frameworks, pioneered by Reinganum (1988) or the mistaken beliefs model of Priest and Klein (1984) is a more accurate depiction of plea bargaining. Furthermore, our results do not imply that principal-agent problems, pretrial detention, risk and loss aversion, or other cognitive limitations and behavioral biases do not matter. What are results suggest is that these other considerations are not so strong to negate the relationship between the expected trial outcomes and the plea discount.

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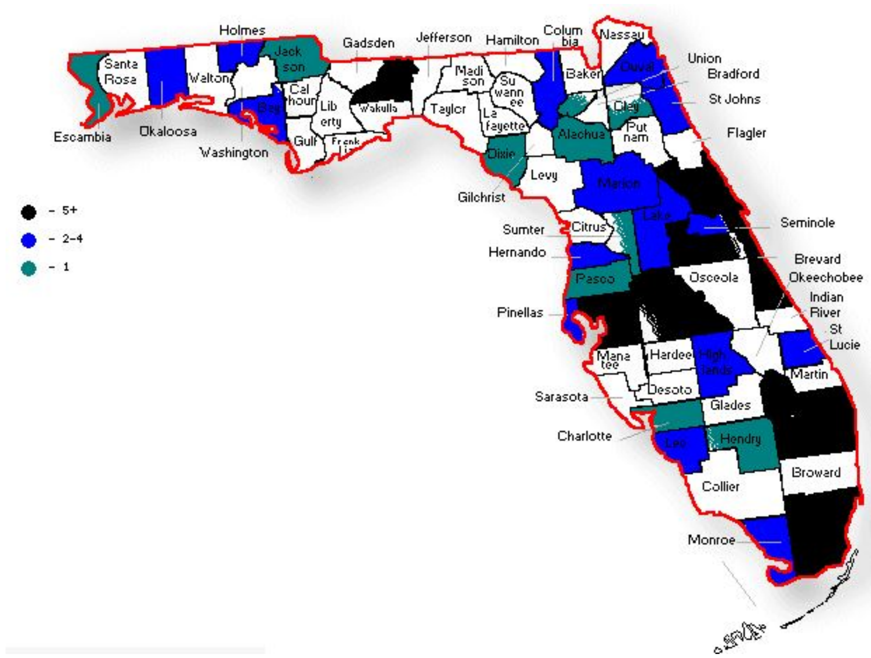
9 Additional Results

As a final dimension to the investigation of heterogeneous effects, we attempt to directly measure the community's exposure to the information of the death of the law enforcement official. We collect data on the number of newspaper articles and local news stories related to the death.

Specifically, we follow Lim *et al.* (2015) who collect data on newspaper coverage of judges in their evaluation of sentencing harshness and media exposure. We use the same database that they do, NewsLibrary.com.

Figure 6: LEO Deaths Across the State



The black counties are those with five or more deaths over the time period. The blue counties are those with two, three, or four deaths. The green counties are those with one death. The counties in white did not have any death during the time period.

Table 16: Trial Rate; Breakdown by Crime; Gun & Non-Gun Deaths

	Gun Death		Non-Gun Death	
Robbery (812)	0.0130 ***	(0.0040)	0.0239 ***	(0.0069)
Burglary (810)	0.0039	(0.0054)	0.0235 **	(0.0093)
Assault (784)	0.0018 *	(0.0063)	0.0223 ***	(0.0054)
Weapons (790)	0.0219 **	(0.0091)	0.0074	(0.0095)
Arson (806)	-0.0385	(0.0278)	-0.0064	(0.0282)
Homicide (782)	-0.0315	(0.0208)	-0.0119	(0.0176)
Sexual Battery (794)	-0.0251 ***	(0.0096)	0.0175 *	(0.0097)

Standard errors clustered by County x Year presented in parentheses.
*** 1%; ** 5%; * 10% level of significance.
Dependent variable equal to one if any count charged is disposed at trial (jury or nonjury & convicted or acquitted).
The demographic controls, case characteristics controls, county fixed effects, and month x year fixed effects are all included.

Table 17: Trial Rate

Salience:	Gun	Many Searches	Search Spike
More-Salient Death	0.0126 ***	0.0152 ***	0.0135 ***
	(0.0048)	(0.0051)	(0.0066)
Less-Salient Death	0.0217 ***	0.0211 ***	0.0214 ***
	(0.0050)	(0.0040)	(0.0047)
Demographic Controls?	Yes	Yes	Yes
Case Characteristics?	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes
Month x Year Fixed Effects	Yes	Yes	Yes
R^2	0.016	0.016	0.016
AIC	-1.3x10^6	-1.3x10^5	-1.3x10^6

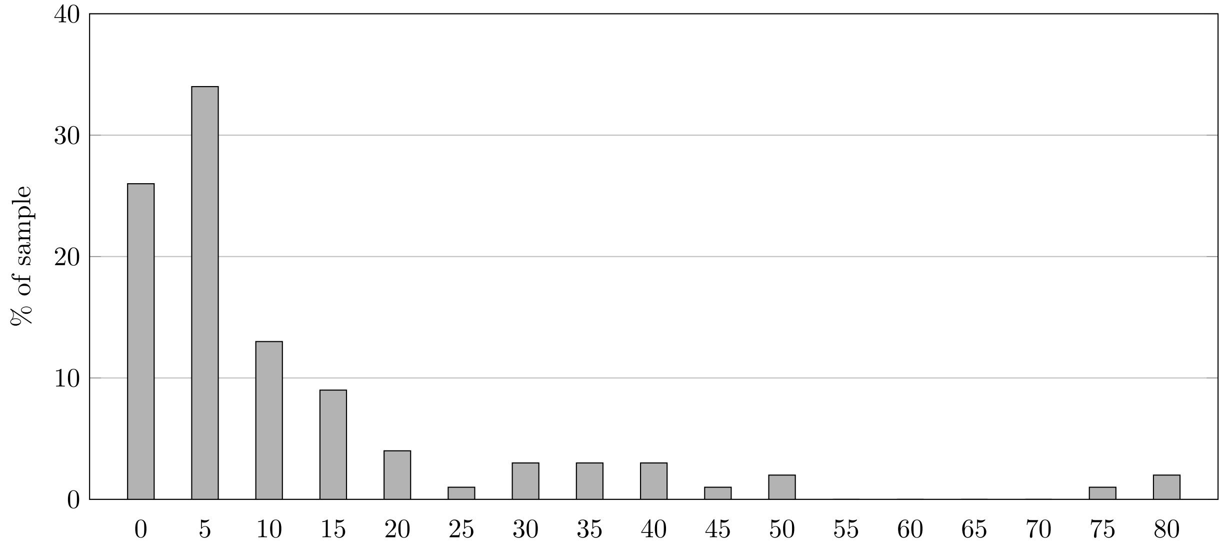
Linear probability model estimated with an indicator variable equal to one if any count charged is disposed at trial (jury or nonjury & convicted or acquitted) as the dependent variable.
Standard errors clustered by County x Year presented in parentheses (649 clusters).
*** 1%; ** 5%; * 10% level of significance.
Demographic and case characteristics controls are the same as in Table 3.
Serious crimes only (with acquittals included); $N = 614,201$.

Table 18: Quantile Regression

Full Data set	40	50	60	70	80	90
Plea Bargain	-0.560 —	-0.591 ***	-1.375 ***	-1.389 ***	-1.609 ***	-1.649 ***
	(0.111)	(0.056)	(0.036)	(0.058)	(0.000)	(0.082)
Death	-1.253 ***	-0.408 ***	-0.288 ***	-0.421 ***	0.000	-0.039
	(0.111)	(0.090)	(0.188)	(0.120)	(0.130)	(0.091)
Death x Plea Bargain	1.658 ***	0.594 ***	0.682 ***	0.647 ***	0.288 **	0.376 ***
	(0.102)	(0.089)	(0.189)	(0.123)	(0.131)	(0.092)
R^2	0.002	0.001	0.005	0.007	0.013	0.024
Serious Crimes						
Plea Bargain	-0.773 ***	-0.875 ***	-1.057 ***	-1.542 ***	-1.609 ***	-1.609 ***
	(0.088)	(0.001)	(0.034)	(0.003)	(0.000)	(0.000)
Death	-0.773 ***	-0.003	-0.181 *	0.000	0.000	0.000
	(0.120)	(0.067)	(0.096)	(0.000)	(0.004)	(0.077)
Death x Plea Bargain	1.156 ***	0.185 ***	0.363 ***	0.338 ***	0.405 ***	0.223 ***
	(0.144)	(0.067)	(0.096)	(0.003)	(0.004)	(0.077)
R^2	0.001	0.002	0.002	0.007	0.010	0.015

Standard errors reported in parentheses.
*** 1%; ** 5%; * 10% level of significance.
$N = 1,566,892$ in the top panel and $N = 608,622$ in the bottom panel.

Figure 7: News Hits in the Month Following the Death



Each column depicts the number of news articles in newspaper and local television news within the month after the law enforcement official's death.
The number on the x-axis is the upper bound to the bin of length 5. Thus, '20' represents those events where there was 16, 17, 18, 19, or 20 news stories.
The '80' bin includes all events with more than 80 news stories as well.

There, the search tool is limited to media outlets in the state of Florida. For each law enforcement official we enter their name as the keyword, including their rank/title. We also enter the word "death" as a second keyword. We collect the number of media hits that arise in that search. We also restrict the hits to the one-month period immediately following the death. We do this because the officer could have been involved in a case previously in life where his/her name is used, but also it is common for memorial services to list the name of all fallen officers in the past.

The following histogram provides information on the number of media hits that arose for the deaths in our sample.

A right-skewed tail exists. While the bulk of the law enforcement official's deaths have just a handful of news stories, quite a few have extensive media coverage. We presume that those incidents with more media hits are those that will reach a greater share of the local population and be a more intense treatment to them when they reach the jury.

9.1 Disparities

Sentencing disparities have been well documented. Thus, it is prudent to explore their existence in our data. Prominent among them is gender and racial differences in outcomes. While these disparities are obviously a concern, exploring whether they are heightened in the plea bargaining process is a potentially interesting question.

To conduct the analysis, we simply disaggregate the difference-in-difference coefficient by, first, the gender

Table 19: Dropping Arrests Within a Week of the Death

Dep. variable:	**ln(sent.)**	**IHS(sent.)**	**ln(sent.)**	**IHS(sent.)**
Model:	**OLS**	**OLS**	**Tobit**	**Tobit**
Plea Bargain	-0.783 ***	-0.802 ***	-0.953 ***	-0.884 ***
	(0.130)	(0.129)	(0.150)	(0.126)
Death	-0.090	-0.135 *	-0.178 **	-0.235 **
	(0.065)	(0.073)	(0.090)	(0.091)
Death x Plea Bargain	0.224 ***	0.266 ***	0.394 ***	0.380 ***
	(0.059)	(0.066)	(0.081)	(0.083)
Demographic Controls?	Yes	Yes	Yes	Yes
Case Characteristics?	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes	Yes	Yes
R^2	0.089	0.093	0.031	0.032
AIC	5.7x10^6	6.1x10^6	5.2x10^6	5.8x10^6

Standard errors clustered by County x Year presented in the parentheses (695 clusters).
*** 1%; ** 5%; * 10% level of significance.
Demographic and case characteristics controls are the same as in Table 3.
$N = 1,559,117$

Table 20: Dropping Arrests Within a Month of the Death

Dep. variable:	**ln(sent.)**	**IHS(sent.)**	**ln(sent.)**	**IHS(sent.)**
Model:	**OLS**	**OLS**	**Tobit**	**Tobit**
Plea Bargain	-0.794 ***	-0.813 ***	-0.966 ***	-0.897 ***
	(0.127)	(0.126)	(0.148)	(0.123)
Death	-0.100	-0.144 *	-0.189 **	-0.242 ***
	(0.068)	(0.076)	(0.094)	(0.095)
Death x Plea Bargain	0.231 ***	0.274 ***	0.358 ***	0.386 ***
	(0.063)	(0.070)	(0.085)	(0.087)
Demographic Controls?	Yes	Yes	Yes	Yes
Case Characteristics?	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes	Yes	Yes
R^2	0.090	0.094	0.032	0.032
AIC	5.6x10^6	6.0x10^6	5.1x10^6	5.6x10^6

Standard errors clustered by County x Year presented in the parentheses (695 clusters).
*** 1%; ** 5%; * 10% level of significance.
Demographic and case characteristics controls are the same as in Table 3.
$N = 1,527,959$

Table 21: Non-Serious Crimes

Dep. variable: Model:	**ln(sent.) OLS**	**IHS(sent.) OLS**	**ln(sent.) Tobit**	**IHS(sent.) Tobit**
Plea Bargain	-0.749 ***	-0.803 ***	-1.001 ***	-0.953 ***
	(0.169)	(0.165)	(0.198)	(0.151)
Death	0.119	0.083	0.087	0.014
	(0.088)	(0.102)	(0.126)	(0.126)
Death x Plea Bargain	0.012	0.044	0.088	0.117
	(0.087)	(0.100)	(0.124)	(0.125)
Demographic Controls?	Yes	Yes	Yes	Yes
Case Characteristics?	Yes	Yes	Yes	Yes
County Fixed Effects?	Yes	Yes	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes	Yes	Yes
R^2	0.075	0.079	0.028	0.027
AIC	3.4x10^6	3.7x10^6	3.1x10^6	3.4x10^6

Standard errors clustered by County x Year presented in the parentheses (695 clusters).
*** 1%; ** 5%; * 10% level of significance.
Demographic and case characteristics controls are the same as in Table 3.
$N = 958,270$

Table 22: Pleas or Trials?

	Guilty Pleas All felonies	**Serious crimes**	**Trials All felonies**	**Serious crimes**
Death	0.128 ***	0.097 ***	0.069	-0.101
	(0.025)	(0.031)	(0.049)	(0.068)
R^2	0.086	0.115	0.106	0.077
AIC	5.6x10^6	2.2x10^6	1.4x10^5	7.7x10^4
N	1,533,571	591,160	33,321	17,462
Gun Death	0.167 ***	0.135 ***	0.099	-0.090
	(0.044)	(0.054)	(0.074)	(0.088)
Non-Gun Death	0.103 ***	0.071 *	0.001	-0.142
	(0.035)	(0.037)	(0.063)	(0.088)
R^2	0.086	0.115	0.106	0.077
AIC	5.6x10^6	2.2x10^6	1.4x10^5	7.7x10^4
N	1,533,571	591,160	33,321	17,462

Standard errors clustered by County x Year presented in the parentheses (659/649 clusters).
*** 1%; ** 5%; * 10% level of significance.
County fixed effects and Month x Year effects included in each specification. Demographic and case characteristics controls are also included and are the same as in Table 3.

Table 23: Disparities

	X = Female Y = Male	X = Black Y = White
Death	-0.087	-0.044
	(0.065)	(0.054)
Plea Bargain	-0.789 ***	-0.778 ***
	(0.132)	(0.132)
X	-0.465 ***	0.068 ***
	(0.011)	(0.014)
Death x Plea Bargain x Y	0.237 ***	0.158 ***
	(0.059)	(0.049)
Death x Plea Bargain x X	0.134 ***	0.206 ***
	(0.064)	(0.053)
Demographic Controls?	Yes	Yes
Case Characteristics?	Yes	Yes
County Fixed Effects?	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes
R^2	0.089	0.089
AIC	5.7x10^6	5.7x10^6

Standard errors clustered by County x Year presented in the parentheses (695 clusters).
*** 1%; ** 5%; * 10% level of significance.
Demographic and case characteristics controls are the same as in Table 3.
$N = 1,566,892$

of the accused and then, second, by whether s/he is white or black.[26] For convenience, we also report the gender/race baseline disparity as well. With this disaggregation, we re-estimate our primary specification − the first column of Table 3, which uses the log transformed sentence as the dependent variable in a fixed effects regression with clustered standard errors.

Women receive shorter sentences than men overall, which is in line with previous findings (Starr, 2015). Interestingly, the reduction in the plea discount when a law enforcement official dies in the line of duty is greater for men than women. Thus, male defendants are treated relatively harsher and this effect is escalated with the LEO death.

The second column differentiates white from black defendants. As to be expected given past research on the topic, black defendants receive longer sentences than white defendants. Using these estimated coefficients, the reduction in the plea discount is approximately 5 percentage points higher for black defendants.

Thus, not only do the results suggest that male and black defendants are receiving relatively harsher sentences when the law enforcement death occurs, but they indicate that some of these disparities are begin driven by the plea bargaining process. The results are consistent with prosecutors anticipating different juror responses based on the gender and race of the defendant. Anticipating relatively greater changes in

[26]For the sake of ease of the analysis, we do not include disaggregations for the other race variables.

Table 24: Comparing Subsamples

	No Death = 0	No Death = 1	$\|t\|$	Death = 0	Death = 1	$\|t\|$
Sentence	16.817	18.019	12.6 ***	16.519	22.993	38.8 ***
Plea Bargain	0.9726	0.9796	18.7 ***	0.9738	0.9482	38.4 ***
Male	0.8232	0.8308	8.6 ***	0.8220	0.8474	16.3 ***
Business	$7.3\text{x}10^{-6}$	$1.9\text{x}10^{-5}$	1.6	$7.7^{*}10^{-6}$	–	0.7
White	0.5891	0.5610	24.5 ***	0.5895	0.5796	4.9 ***
Black	0.4036	0.4339	26.5 ***	0.4032	0.4122	4.5 ***
Asian	0.0018	0.0019	0.5	0.0019	0.0014	2.4 **
Native	0.0007	0.0007	1.2	0.007	0.0006	1.1
Public Counsel	0.5277	0.6119	72.7 ***	0.5309	0.4607	34.4 ***
Private Counsel	0.2160	0.1698	48.7 ***	0.2137	0.2621	28.7 ***
Appointed Counsel	0.0515	0.0626	21.4 ***	0.0508	0.0646	15.2 ***

the jury's proclivity to convict, prosecutors are making less generous offers.

9.2 Balance Analysis

Clearly, the death of a law enforcement official is unrelated to the individual who has already been arrested and charged, but has not had his/her case disposed at that time in the county. One can be concerned that the timing and location of the law enforcement death is not random, in that there are important (unobserved) drivers of the deaths. These omitted variables can be correlated with the types of crimes that have arisen. Therefore, it is appropriate to check the randomness of the treatment on the dockets.

We create the variable *No Death*, which is a county fixed effect. It is equal to one if the county did not experience a death of a law enforcement official at any time in the data set. It is equal to zero if there was a death. One can be concerned that those counties that experience a death of a law enforcement official are different from those that did not. For example, deaths of law enforcement officials may occur in high violent crime areas. If this is the case, then treated dockets can be expected involve more serious crimes. As a result, one can be concerned that the harsher sentencing observed can be due to the more serious crimes that are occurring in the areas that happen to have these deaths, so that there is not a direct effect of the death on plea bargaining at all.

First, the data set can be partitioned into those observations that come from counties that had at least one death (first column) from those observations coming from counties that did not (second column). A two-sided, difference-in-means t-test (allowing for unequal variances) compares the subsamples. We then omit those observations from counties that never had a death leaving us with only the subset of observations coming from counties that at some point in the time range experienced a law enforcement official's death. We compare observations directly during the period of the law enforcement official's death to those outside of that time period, but in counties treated at some point in time. Again, two-sided, difference-in-means t-tests (allowing for unequal variances) are used to see whether the treated observations are different in our observable variables.

Deaths occur in those areas that are less black and more white (at least measured by who is in the criminal justice system). They arise in places with less use of publicly-provided, indigent defense and more use of private attorneys. Contrary to expectations, they occur in areas with lower sentences and less plea bargaining. While these last two effects are small, given the large sample size their differences are statistically significant.

This is an important observation for the validity of our primary results. LEO deaths are occurring in places where sentences are less harsh and trials are less rare. Absent locational controls (which we do use), this would act to downward bias our difference-in-difference estimate.

Removing observations from counties that never experienced a death of a law enforcement official, as to be expected, deaths are associated with longer sentences and less plea bargaining. This is consistent with our econometric results. Also, regarding the observable characteristics of the case and defendants, the treated observations are different. The LEO deaths happen to affect accused who are more likely to be male, black, and using private attorneys.

Consequently, the use of county fixed effects, along with demographic and case controls is important.

Our threat to identification, though, is whether the observations within a county prior to being treated look similar to observations in the non-treated counties. If they do not follow "parallel trends" prior to the treatment, then the divergence observed after the treatment may not be reflecting the casual impact of the death, but instead capturing the naturally-occurring distinction between the areas that happen to experience the death and those that do not. Thus, we engage in a parallel trends analysis.

To do so, we conduct the following method. First, we collapse the data into a monthly, county-level panel data set. In it, the mean sentence, proportion of dockets plea bargained, and the proportion of dockets with outcomes resulting in non-incarceration are recorded. In addition, for the demographic and case characteristic controls, the proportion of dockets with values of one are recorded.[27] Importantly, for each county in each month the proportion of resolved cases where a law enforcement official's death occurred during the period in which the case was being prosecuted are recorded. Since there is substantial variation in the length of time it takes for a case to be resolved, the *Death* variable takes on values between zero and one, and a given LEO death records $Death > 0$ over numerous months.

With this county by month panel data set we focus on a subset of law enforcement official deaths. The first objective is to match the deaths in their calendar time to the timing of the treated observations case resolution. In the panel data set, we record a death observation occurring if a county had experience a string of months with zero resolved cases having been treated. Then a shock occurs where a nontrivial proportion of the disposed cases are treated. We required that at least 10% of the resolved cases must be treated to identify the month of the treatment.[28]

This procedure does not record every law enforcement death. Some counties experience multiple deaths.

[27]For the total number of counts and the defendant's age, the mean values are used.

[28]In a few instances, a string of zeroes is followed by a month with a very small proportion of disposed cases being treated. The following month, this proportion jumps up substantially. In these cases, we record the month of the incident as the month with the large proportion.

To be treated, we require that prior to the incident there was multiple months without any treated observations. Also, our of the incidents involved two law enforcement officials being killed in the same incident. Thus, we are considering the 38 distinct deaths that occurred in counties without any incidents in the recent past.

For these treated counties, we record the outcome variables values for the 12 months preceding the death, and the 12 months following the death. We also record the outcome variable values for the other counties in Florida that were not treated.[29] Again, we record these control counties in the 12 months before and the 12 months after the incident. We then pool the 38 events into a new data set.

From this, we estimate the time trends for the outcome variables. That is, for the sample of treated counties, we want to estimate,

$$y_{cm} = \alpha_0 + \alpha_1 T_c + \alpha_2 T_c \times Post_m + \epsilon_{cm}. \tag{7}$$

The outcome variable, y_{dt}, is county c's mean value in month m. Our primary outcome variable is the sentence received, but we will also consider the proportion of cases resulting in non-incarceration and the proportion of cases plea bargained. The time trend variable, T_c, is equal to the distance from the recorded incident. Thus, in a treated county c, the month of the recorded incident is $T_c = 0$. Six months prior to the law enforcement official's death, for example, would have a value of $T_c = -6$, while six months after the death has $T_c = 6$. The indicator variable $Post_m$ is equal to one if the observation occurs after the incident.

In this specification, the coefficient α_1 captures the time trend for outcomes in the treated counties. The coefficient α_2 measures the change in the time trend when the county is treated (where $\alpha_1 + \alpha_2$ is the new time trend).

Alternatively, a similar specification can identify the trends for the control observations.

$$y_{cm} = \beta_0 + \beta_1 T_c + \beta_2 T_c \times Post_m + \epsilon_{cm}. \tag{8}$$

Again, β_1 is the time trend prior to the law enforcement official's death, and $\beta_1 + \beta_2$ is the trend after.

These two specifications can be combined and estimated on the constructed data set. Specifically, we estimate,

$$y_{cm} = \gamma_0 + \gamma_1 Treat_c + \gamma_2 T_c + \gamma_3 T_c \times Treat_c + \gamma_4 T_c \times Post_m + \gamma_5 T_c \times Post_m \times Treat_c + \epsilon_{cm}, \tag{9}$$

where $Treat_c = 1$ if the county is a treated county, equation (7), and zero otherwise.

From this estimation, we can test statistically whether the there were parallel trends prior to the treatment in the treated and control counties. They are parallel is $\alpha 1 = \beta_1$. In (9), it follows that $\gamma_2 = \beta_1$ and $\gamma_2 + \gamma_3 = \alpha_1$. Hence, asking if $\alpha 1 = \beta_1$ is equivalent to asking if $\gamma_3 = 0$. We estimate (9) with and without county[30] and month x year fixed effects[31] and both with and without the use of demographic and case

[29]That is, we delete a control counties observation for any month where the proportion of disposed cases treated by a law enforcement death is not zero.

[30]Since a county can be treated at one period of time but will be a control at other periods of time, the treatment variable is not co-linear with the county fixed effects.

[31]As described, the time variable is centered on the date of the incident measuring the months of months since/prior to the incident. Thus, it is unrelated to time fixed effects.

Table 25: Test of Parallel Trends

	Without Any Controls	With County FEs	With County and Month x Year FEs	County, Month x Year Demographics , Case Characteristics
Sentence Length	-0.3862	-0.3395	-0.3134	
	(0.3283)	(0.3292)	(0.3422)	()
	{0.245}	{0.307}	{0.364}	{ }
Prop. Not Incarcerations	-0.0013	-0.007	-0.005	
	(0.0019)	(0.0019)	(0.0020)	()
	{0.497}	{0.730}	{0.793}	{ }
Prop. Plea Bargained	0.0008	0.009	0.0010	
	(0.0009)	(0.008)	(0.0010)	()
	{0.450}	{0.291}	{0.338}	{ }

Each row is the results from a specification with a different dependent variable.
Estimated value of γ_3 presented.
Standard errors, clustered by county, are presented in the parentheses.
The p-values are reported in curly brackets.

characteristic controls. Standard errors are clustered at the county level[32] The following table presents the estimation results.

As one can see, consistently across the specifications, the treated and control counties exhibit parallel trends.[33] Thus, there is not anything noticeably different about how criminal cases are being handled in the treated and control areas of the state prior to the law enforcement official's death that would suggest our causal identification to be questioned.

While this test evaluates a potential discrepancy in the time trend prior to the event, it presumes the trend is linear. An alternative estimation, allowing for nonlinear is to create separate indicator variables for each of the twelve months prior to the event and use them rather than only the one variable T_m in (9). In addition each of the twelve indicator variables is interacted with the treatment variable, $Treat_c$. Each of the resulting twelve coefficients on these interaction terms can be tested, using a t-test, to evaluate whether there is a break in the outcomes of cases prior to the incidents in those areas that were treated and those that were not.

We depict these coefficients, along with their standard errors, in the following figures. The three figures differ in whether the mean sentence length, proportion of sentences with non-incarceration outcomes, or the proportion of cases plea bargained is used as the dependent variables.

Across the three outcome variables, the coefficients on each of the twelve interaction terms have confidence intervals that overlap zero.[34] Thus, we can be confident that in the periods leading up to the death of a

[32]This is done to match as closely as possible the specifications presented in the main results (since they calculate standard errors clustered at the county by year). The lack of significance remains if unadjusted standard errors are used in the hypothesis testing.

[33]We also consider the specifications where we also add an intercept jump at the time break, $Post_m$ and $Post_{mc}$, to the specification given in (9). The test of the parallel trends is unchanged in that the difference is statistically insignificant.

[34]The only exception is for sentence length six months prior to the incident. This effect only present in the one month.

Figure 8: Test of Parallel Trends: Sentence Length

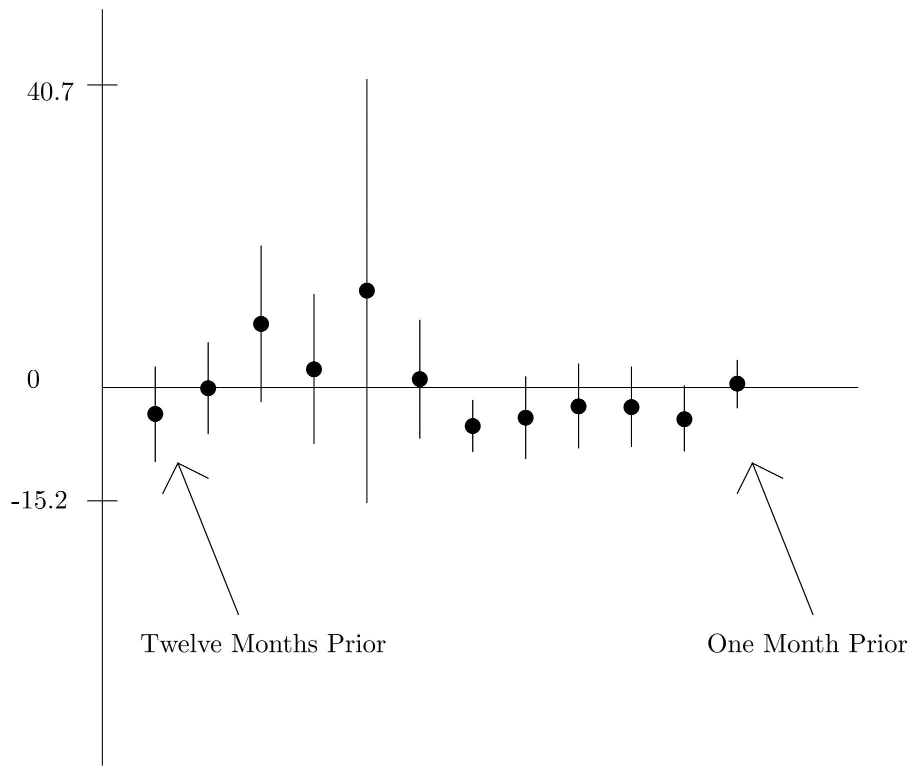


Figure 9: Test of Parallel Trends: Proportion Not Incarcerated

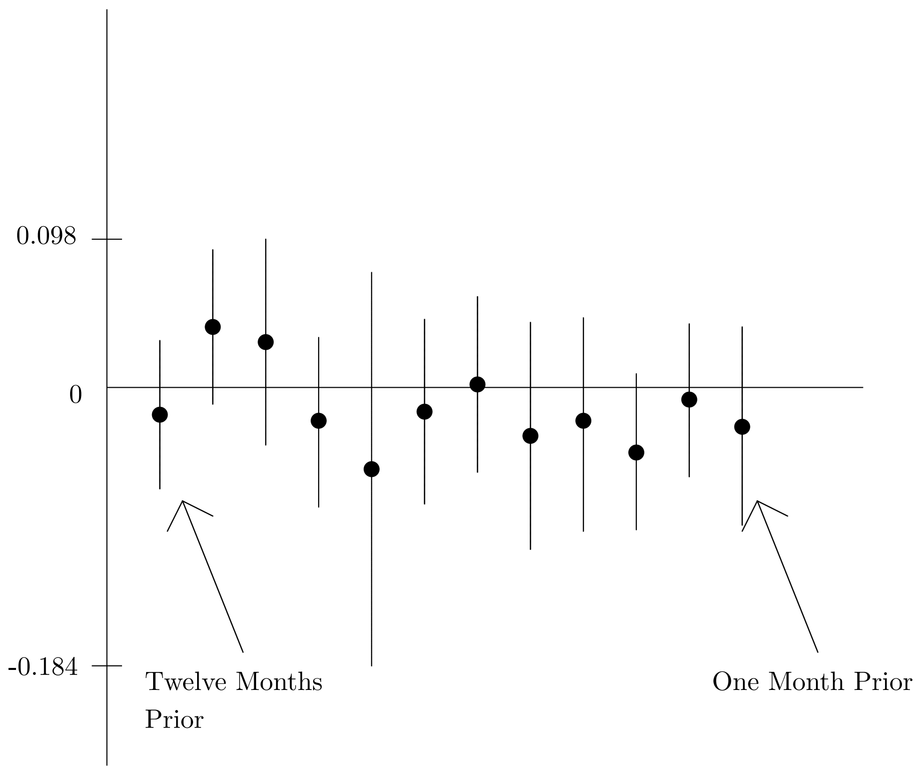
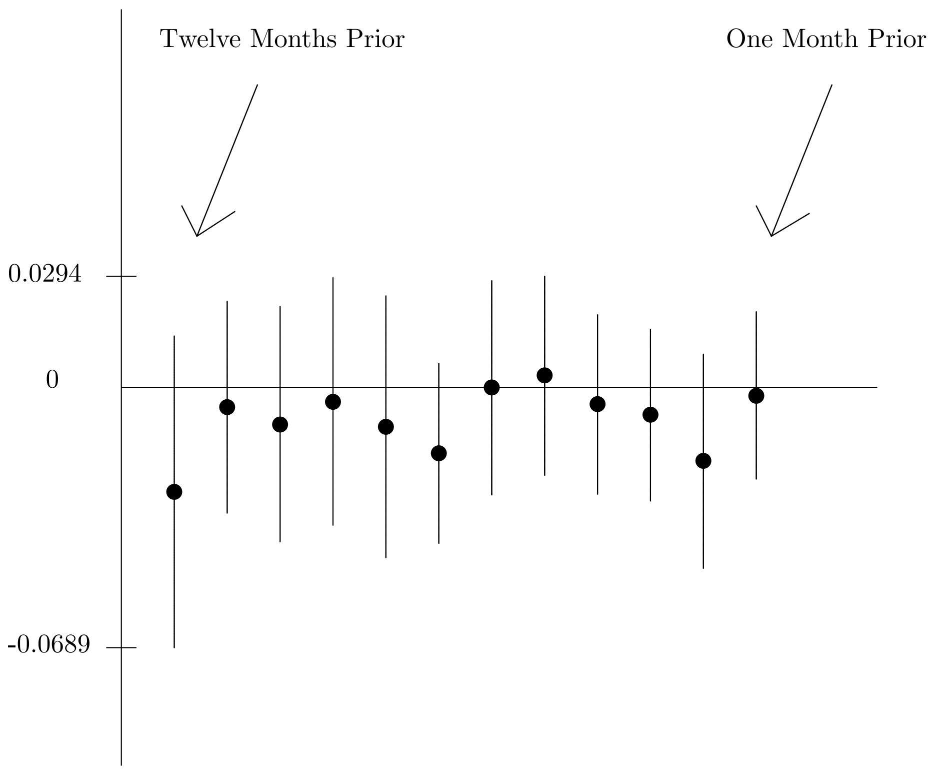


Figure 10: Test of Parallel Trends: Proportion Plea Bargained



law enforcement official the outcomes observed in those counties criminal justice systems resemble outcomes observed in the untreated counties.[35]

We do not test the divergence in the time trend after the incidents because our identification strategy does not predict a persistent trend shift. Only those cases directly treated are expected to be affected by the shift. We do not expect that the death causes a permanent effect on case disposition, but should be a transitory shock to those cases already being processed. We can, though, test the difference at time $T_c = 0$. If our observed outcomes in the individual-level data set continue to arise in the panel data set, then further confidence in our findings arise. For our control counties, the mean value for the (county-level) sentence is X with a confidence interval of []. The mean value for the treated counties is X, which obviously lies above this interval.

Thus, not only do the treated and control counties follow parallel trends prior to the death of a law enforcement official, but a noticeable jump in sentences arises precisely in the month of the incident for those treated.

9.3 Panel Analysis

The data are collapsed into a monthly, county-level panel data set. In it, the mean sentence, proportion of dockets plea bargained, and the proportion of dockets with convictions resulting in non-incarceration are recorded. In addition, for the demographic and case characteristic controls, the proportion of dockets with values of one are recorded.[36] Importantly, for each county in each month the proportion of resolved cases where a law enforcement official's death occurred during the period in which the case was being prosecuted are recorded. Since there is substantial variation in the length of time it takes for a case to be resolved, the *Death* variable takes on values between zero and one and a given LEO death records $Death > 0$ over numerous months.

First, with this collapsed data set a traditional panel-data analysis can be conducted. Since the mean values for the sentences no longer have the right-skew concern or the clustering of observations at zero, which the docket-level data set had, the dependent variable is no longer adjusted. The following table presents the results. Both month of year and county fixed effects are included. The standard errors are clustered by county.

Surprisingly, the difference-in-difference coefficient is negative. This suggests that while counties that experience more plea bargaining see lower average sentences, the plea bargained sentences experience an escalation in the sentence reduction. Interestingly, effect of plea bargaining on the rate of incarceration is statistically insignificant. When there are more cases disposed that are affected by LEO deaths, the incarceration rate decreases as plea bargaining increases. This does not coincide with the results from the Hurdle model presented previously, which showed that in the selection model the death of the law enforcement

[35] Also, the evidence also suggests that there is not an intercept shift in the outcome variables. Specifically, a test that $\alpha_0 = \beta_0$ is equivalent to a test that $\gamma_1 = 0$. Using the three specifications presented in the figures, t-tests have p-values of 0.10, 0.52, and 0.40, respectively. Thus, they are exhibiting common trends.

[36] For the total number of counts and the defendant's age, the mean values are used.

Table 26: Panel Data Analysis

	Sentence	Proportion not Incarcerated
Death	238.91 ***	0.2490 **
	(22.37)	(0.1137)
Plea Bargain	-69.97 ***	0.0313
	(16.23)	(0.0528)
Death x Plea Bargain	-235.47 ***	-0.2380 *
	(23.63)	(0.1390)
Demographic Controls?	Yes	Yes
Case Characteristics?	Yes	Yes
County Fixed Effects?	Yes	Yes
Month x Year Fixed Effects?	Yes	Yes
R^2	0.211	0.284
AIC	5.6x10^4	-1.7x10^3

Standard errors clustered by County presented in the parentheses (54 clusters).
1%; ** 5%; * 10% level of significance.
Demographic and case characteristics controls are the same as in Table 3 (but aggregated to the county level).
There are 54 cross-sectional units and X temporal units in the fixed effects.
$N = 6403$
The dependent variable's mean is 18.58 ($\sigma = 21.7$). The mean value of *Death* is 0.0188 ($\sigma = 0.0889$), and the mean value of *Plea Bargain* is 0.9712 ($\sigma = 0.0840$).

official increased rate at which a zero was observed.