

Is Pleading Really a Bargain?: Evidence from North Carolina

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Abstract

The decision to accept a plea bargain is one of the highest stakes decisions under uncertainty an individual can make. As such, we may learn a great deal about individual rationality, agency problems, and risk aversion from studying it. This paper undertakes the most detailed empirical study to date of the plea bargain decision. We use a dataset of over 300,000 observations from a decade of court cases in North Carolina. In order to overcome omitted variables bias, we make use of several instrumental variables strategies. We find that the very high plea rate is often rational, but that it varies substantially by type of crime and other characteristics.

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The authors would also like to thank Bernardo Silveira and others for their assistance with this paper.

1 Introduction

Despite a trial by jury being fundamentally enshrined by the Sixth Amendment¹, it is exceedingly rare. Best estimates suggest that only between 5% and 10% felony cases in both federal and state courts are resolved at trial.² This reliance on negotiated plea bargaining has proved tremendously controversial, and there is certainly no scholarly consensus on the practice.³ Scholars have questioned the impact of plea bargaining on the innocent and on the efficiency of courts.⁴ While others have addressed these questions of efficiency, equity, or even constitutionality, this paper focuses empirically on the decision faced by a defendant offered a plea bargain.

In this article we employ a novel dataset covering all felony cases from the state of North Carolina disposed of between 1995 and 2009. We compute the expected sentence after a plea is accepted and after one is rejected, and seek to determine whether there is a penalty from rejecting a plea. This question is fundamental to a defendant considering whether to accept a prosecutor's plea. The existence and accurate measurement of such a penalty also has direct consequences scholarly understandings of the criminal justice system.

The "trial penalty" is a well-known term among criminal law practitioners, used to indicate that sentences handed down at trial often exceed those from a plea bargain. This must be expected, otherwise defendants would never have incentive to plead guilty, assuming a non-100% probability of conviction upon rejecting a plea. But the relevant question to a defendant is not whether the sentence at trial conditional on guilt is greater than a plea offer. Rather, a risk-neutral defendant will care about the expected sentence at trial versus the plea offer.

Making this comparison empirically is no easy task. We cannot simply ask whether the average sentence received is shorter than that received at trial, as defendants intentionally select plea or trial based on expected outcomes. Selection occurs on both observable and unobservable variables.⁵ To address the selection issues based on observable measures, we present results from ordinary least squared (OLS) regression controlling for relevant observable factors. To address

¹ U.S. CONST. amend. IV §1.

² U.S. Department of Justice *Plea and Charge Bargaining* (2011)

³ It would be impossible to list all relevant papers. For a sampling, see: Moise Berger *The Case Against Plea Bargaining* (1976), Bar-Gill and Gazal *Plea Bargains Only for the Guilty* (2004), Thomas Church *In Defense of "Bargain Justice"* (1979), and Malcolm Feeley *Plea Bargaining and the Structure of the Criminal Process* (1982).

⁴ See Bar-Gill and Gazal *Plea Bargains Only for the Guilty* (2004), Albert Alschuler *The Changing Plea Bargaining Debate* (1981), Scott and Stuntz *Plea Bargaining as a Contract* (1992), and Frank Easterbrook *Plea Bargaining as Compromise* (1992).

⁵ The nature of observable and unobservable factors for the choice to plea or not will be discussed later in Sections 3.1 and 5. An example of an observable factor in the plea decision is the number of charges a defendant faces. This is a measure of the severity of the case, which should influence the choice to plead. The underlying true guilt of the defendant is inherently unknowable to the researcher, but should influence the choice to plead.

the selection issues based on unobservable in addition to observable measures, we present results employing instrumental variable (IV) regression.

In Section 3, we present a simple formalized model characterizing a defendant's choice to plea or go to trial. Originally proposed by Priest and Klein (1994)⁶, this model has been used in *Is Pleading Really a Bargain?* (Abrams, 2011) and *Putting the Trial Penalty on Trial* (Abrams, 2013). Both of these papers analyzed data from Chicago and found that defendants would be better off on average to reject a plea deal in that context.⁷ The model's intuition is easy: defendants and prosecutors have beliefs about the likelihood of guilt at trial. Without any uncertainty, both parties would always choose a plea bargain to save the cost of a lengthy trial, but with uncertainty, disagreements in outcomes arise which lead to trials. While observable and unobservable case characteristics are unchanged, uncertainty decreases partially with judicial and attorney experience - once attorneys have experience with a judge, they are more likely to agree on the likely outcome.

In this paper, we calculate the expected sentence from accepting or rejecting a plea and find that the sentence-minimizing choice varies by type of crime. This result builds upon the work in Abrams (2011,2013), which found that rejecting plea bargains in Chicago lowered average sentences, but didn't have data detailed enough to allow for separate estimates by type of crime.⁸ In Sections 4 and 6, we discuss several possible explanations for these findings, most notably North Carolina's use of a sophisticated structured sentencing scheme. This structured sentencing scheme reduces judicial leeway and may constrain the ability to negotiate substantial sentence length reductions.

Controlling only for observable heterogeneity, our OLS regressions presented in Tables 6 and 7 find estimates that on average, taking a plea bargain reduces expected sentence by 6 months, but has only a marginal impact on the likelihood of facing any length of incarceration.⁹ Because of the compelling need to address unobservable factors changing the choice to plead, we employ IV regression (described fully in Section 3.1). We present three possible candidates for valid instruments: 1) judicial tenure, 2) defense attorney tenure, and 3) number of encounters between judge and defense attorney. Unfortunately, these instruments are not strong, and so produce estimates in Tables 8 and 10 with large estimation error.

This paper takes important steps forward in our understanding of the important role of plea bargaining and prosecutorial power in the criminal justice system. In conjunction with previ-

⁶ See Priest and Klein *The Selection of Disputes for Litigation* (1994).

⁷ See Abrams *Is Pleading Really a Bargain?* (2011) and Abrams *Putting the Trial Penalty on Trial* (2013).

⁸ As will be discussed further in Section 2, Abrams (2011) found evidence that plea bargains produce sentences between 4 and 21 months longer than those produced from trial.

⁹ Specification (8) in Table 6 suggests that pleading reduces expected sentence length by 0.51 years, and Specification (8) in Table 7 suggests pleading increases the rate of incarceration (of any length) by only 2%.

ous estimates, it suggests that variation in sentencing requirements can produce substantial unintended variation in the ‘shadow of the law’. Specifically, this implies that constraining prosecutor and judge discretion by structured sentencing may increase prosecutor bargaining power; this increased bargaining power may allow prosecutors to use the trial-penalty coerce defendants into pleading.

The remainder of this article proceeds as follows: Section 2 situates our article within the relevant literature. Section 3 presents the theoretical model employed in this paper, with Subsection 3.1 specifically detailing the problems presented by defendants’ endogenous choice to plea. Section 4 introduces our data and discusses features specific to the North Carolina court system. Section 5 presents the OLS and IV regression results. We end with a discussion of our results in Section 6 and conclude in Section 7.

2 Background

To situate this article within the relevant literature, we must first begin with a discussion of the term “trial penalty,” as the term has been used in a variety of contexts with slightly different definitions. What is common across all authors is the underlying understanding that a defendant who chooses a trial over a plea bargain will receive a longer sentence, a “trial penalty.”

This basic idea is fundamental to the legal understanding of plea bargaining. As Jones (1978) describes,

Plea bargaining results from an agreement between the prosecutor, defense attorney, and occasionally the defendant. The prosecutor offers the defendant a *quid pro quo* (charge reduction or sentence recommendation) for pleading guilty. . . . Moreover, the participants wield such discretion that in most cases they serve as the final arbiters in the sentencing decision.¹⁰

Without using the phrase, Jones outlines the foundation of a trial penalty - the prosecutor trades a reduced sentence for the certainty of a guilty plea. Jones’s conception of the trial-penalty, however, corresponds directly to an *ex-post* trial penalty; that the sentence received from plea bargaining is lower than that the defendant would have received were they convicted at trial (i.e. conditional on a finding of guilt).

One could, alternatively, consider an *ex-ante* trial-penalty; that the sentence a defendant receives from a plea bargain is less than the *expected* sentence they receive from declining the plea bar-

¹⁰ J. B. Jones *Prosecutors and the Disposition of Criminal Cases: An Analysis of Plea Bargaining Rates* (1978)

gain¹¹. There is no objectively correct definition of the trial penalty. Rather, which definition is appropriate is contextually dependent. When a defendant is deciding whether or not to accept a plea bargain, they should focus on the ex-ante trial penalty. When a lawmaker is trying to determine the appropriate statutory punishment for a crime, the ex-post definition may be better.

The earliest empirical discussion of the trial penalty was published by Rhodes (1979), who examine the presence of an ex-post trial penalty for arrests in Washington, D.C. in 1974¹². Comparing sentences obtained after a plea bargain to those obtained after a finding of guilt at trial, Rhodes shows no distributional differences for charges of larceny, burglary, and assault. Rhodes does find that probation is a more common sentence and jailtime of 3+ years is less common after pleading guilty to the charge of robbery (as opposed to sentences condition on conviction at trial).

This analysis of the ex-post trial penalty continued with the work of Brereton and Casper¹³. Using data from three California jurisdictions in the late 1970s, the authors test for the equality of rates of imprisonment between those who plead guilty and those who go to trial. While the authors control for some relevant observable measures¹⁴, they examine only the fraction of defendants who were imprisoned. This is problematic on two fronts: 1) by looking at imprisoned rather than incarcerated, the authors ignoring the sentences less than a year, and 2) the authors only examine the rate of imprisonment and not sentence lengths. The first problem causes sentences of less than a year to be discounted. The second, more severe problem prohibits the authors from assessing the more plausible margin to observe a trial-penalty; we would expect longer sentences from trial, not necessarily a difference in the rate of jailtime.

Literature abounds with the discussion of the ex-post trial penalty in many court system. Authors such as Schulhofer and Nagel (1989, 1992, and 1997) discuss how the federal sentencing guidelines build in a 35 percent sentence reduction in sentence length for pleading¹⁵. This happens as a consequence of the U.S. Sentencing Guidelines Manual permitting a reduction in sentence for

¹¹ When we say expected sentence, this effectively is the sentence a defendant expects conditional on being found guilty at trial weighted by the probability that they would be found guilty at trial. Mathematically:

$$E[\text{Sentence}] = \text{Sentence} \cdot \text{Pr}(\text{Found Guilty}) + 0 \cdot \text{Pr}(\text{Found Not Guilty})$$

¹² W.N. Rhodes *Plea Bargaining: Its Effect on Sentencing and Convictions in the District of Columbia* (1979)

¹³ Brereton and Casper *Does it Pay to Plead Guilty?* (1982)

¹⁴ The authors include controls for: jurisdiction, two types of crime (robbery and burglary), prior record, number of charges, and seriousness of charge.

¹⁵ See Schulhofer and Nagel *Negotiated Pleas Under the Federal Sentencing Guidelines: The First Fifteen Months* (1989), Nagel and Schulhofer *Tale of Three Cities: An Empirical Study of Charging and Bargaining Practices Under the Federal Sentencing Guidelines* (1992), and Schulhofer and Nagel *Plea Negotiations Under the Federal Sentencing Guidelines: Guideline Circumvention and its Dynamics in the Post-Mistretta Period* (1997).

“accepting responsibility” for one’s action¹⁶. Despite the possibly 35 percent trial penalty, Ulmer et al. (2010) estimate between a 3 and 15 percent penalty¹⁷.

Prior to Abrams (2011), the only author to empirically discuss the ex-ante trial penalty was Smith (1986), who examined whether a defendant receives a prison sentence of at least a year¹⁸. Controlling for several observable case and defendant characteristics, he finds that after controlling for observable characteristics, there was no statistically significant difference between the unconditional expected sentence from trial and that obtained after a plea bargain. As the author does not address potentially unobserved selection issues, we still may worry of bias in these estimates.

It is in this conceptual position that Abrams (2011,2013) estimated the ex-ante trial penalty using data from Cook County, Illinois state courts¹⁹. In this paper, Abrams finds direct evidence against the existence of a positive trial-penalty. In fact, his OLS estimates provide evidence that a risk-neutral defendant could expect a shorter sentence by declining a plea bargain than by accepting. Unlike previous literature, Abrams (2011) used IV regression to control for unobservable selection issues. The results of these estimates provide no evidence of the existence of a trial penalty.

Abrams (2011) presented a counter-intuitive result, that the trial penalty was actually a trial discount, and unsurprisingly was received with some skepticism²⁰. The most compelling critique comes from Alschuler (2013), who criticized (among other features) the construction of the dataset from Cook County, Illinois²¹. In particular, Alschuler commented “He [Abrams] was confident that had he actually compared post-trial sentences to post-guilty-plea sentences, the posttrial sentences would have been less severe. Abrams might be correct, and it would be worth the effort to find out. The following section of this article considers circumstances that might explain findings like Abrams if these findings were replicated in a better conceived and better executed study ” (691).

With Alschuler’s request squarely in mind, the current paper proceeds as a conceptual descendant

¹⁶ See U.S. Sentencing Guidelines Manual 3E1.1 (2010).

¹⁷ Ulmer, Eisenstein, and Johnson *Trial Penalties in Federal Sentencing: Extra-Guidelines Factors and District Variation* (2010).

¹⁸ Douglas Smith *The Plea Bargaining Controversy* (1986).

¹⁹ See supra note 7.

²⁰ It is worth mentioning A.C. Kim *Underestimating the Trial Penalty: An Empirical Analysis of the Federal Trial Penalty and Critique of the Abrams Study* (2014). This paper seems to fundamentally misunderstand the conceptual difference between the ex-ante and ex-post trial penalties. Additionally statements such as “Abrams’s methodology implicitly assumes that defendants who pled guilty would have had the same odds of being acquitted as those defendants who actually went to trial” suggests a lack of understanding about the entire discussion of observable and unobservable endogenous group selection. This is explicitly not the assumption that is made in Abrams (2011,2013). This assumption would obviate the need for instrumental variables thereby rendering pages 206-207 and 214-218 unnecessary.

²¹ See Albert Alschuler *Lafler and Frye: Two Small Band-Aids for a Festering Wound* (2013).

of Abrams (2011,2013). The current article uses a significantly larger dataset involving all felony cases in the state of North Carolina between 1995 and 2009. After significant cleaning of the data to ensure it truly characterizes the observed incidents, we retain over 300,000 observations²².

3 Theory

We begin with a simple mathematical model describing a criminal defendant's choice to go to trial rather than accept a plea bargain. This conceptual framework was introduced by Priest and Klein (1994) and has been subsequently used in Abrams (2011) in analyzing the choice to take a plea in Chicago.

Assume that for every criminal defendant, all information relevant to the case is known, and that this information can be summarized by a single variable Y . We may think of Y as the weight of evidence that a given defendant is guilty of their charged crimes. Let Y^* represent the burden of proof necessary for a finding of guilt. We denote the realized value of Y for a particular case as Y' . Therefore, the outcome of a trial will be:

$$\begin{aligned} \text{Defendant is found guilty if } Y' > Y^* \\ \text{Defendant is found not guilty if } Y' \leq Y^* \end{aligned}$$

As in any situation, however, there is uncertainty about the true value of Y' . We will model this uncertainty as if the defendants and prosecutors receive a noisy signal about the true value of Y' :

$$\begin{aligned} \hat{Y}'_p &= Y' + \varepsilon_p \\ \hat{Y}'_d &= Y' + \varepsilon_d \end{aligned}$$

Where ε_p and ε_d are the prosecution and defendant's uncertainty about Y' respectively. We will assume that ε_p and ε_d are drawn from the same distribution, specifically:

$$\varepsilon_p, \varepsilon_d \sim N(0, \sigma^2)$$

As $E[\varepsilon_p] = E[\varepsilon_d] = 0$, this means that $E[\hat{Y}'_p] = E[\hat{Y}'_d] = Y'$. Thus, in expectation, both the prosecution and defendant's beliefs about Y' are correct (i.e. they have rational expectations). Without any loss of generality, we can normalize $Y^* = 0$.

As both the prosecution and the defendant know $\varepsilon_p, \varepsilon_d \sim N(0, \sigma^2)$, given their respective signals

²² It is important to acknowledge, and will be described in Section 4 that these remaining observations are not capturing the entire universe of felony cases in North Carolina. For example we exclude homicide cases, cases only involving the violation of probation, and crucially for the identification strategy, cases where we cannot identify the judge.

\hat{Y}'_p and \hat{Y}'_d , they form beliefs about the probability of a finding of guilt at trial:

<p><i>Prosecution</i></p> $P_p = Pr(Y' > 0 \hat{Y}'_p)$ $= Pr(\hat{Y}'_p - \varepsilon_p > 0)$ $= Pr(\hat{Y}'_p > \varepsilon_p)$ $= F(\hat{Y}'_p)$	<p><i>Defendant</i></p> $P_d = Pr(Y' > 0 \hat{Y}'_d)$ $= Pr(\hat{Y}'_d - \varepsilon_d > 0)$ $= Pr(\hat{Y}'_d > \varepsilon_d)$ $= F(\hat{Y}'_d)^{23}$
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Now, for a given charge, let us temporarily assume that the length of a jail sentence is fixed upon conviction is fixed at a length of J . Given this assumption, the expected jail time that the defendant and prosecution can expect given a trial is:

$$E [J \text{ at trial}] = E [P_p \cdot J] = E [P_d \cdot J] = F(Y') \cdot J$$

3.1 Choice to Plea vs. Trial

Let us define:

<p><i>Prosecution</i></p> $C_p \equiv$ cost of going to trial $S_p \equiv$ cost of settling	<p><i>Defendant</i></p> $C_d \equiv$ cost of going to trial $S_d \equiv$ cost of settling
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If we assume that both the prosecutor and the defendant are risk neutral and linearly value jail time, then the prosecutor would be willing to accept any sentence length A , so long as:

$$A - S_p \geq P_p \cdot J - C_p$$

Thus, we can define the minimum sentence, \underline{A} , the prosecutor would accept as:

$$\underline{A} \equiv P_p \cdot J - C_p + S_p$$

Similarly, we can define the maximum sentence the defendant would be willing to accept, \bar{B} as:

$$\bar{B} \equiv P_d \cdot J + C_p - S_p$$

This allows us to conclude that the two parties will go to trial if:

$$\underline{A} > \bar{B} \text{ or } P_p - P_d > \frac{C - S}{J}$$

²³ Because we have assumed that $\varepsilon_p, \varepsilon_d \sim N(0, \sigma^2)$, we know that $F(\hat{Y}'_p) = \Phi(\frac{\hat{Y}'_p}{\sigma})$ and $F(\hat{Y}'_d) = \Phi(\frac{\hat{Y}'_d}{\sigma})$, where $\Phi(\cdot)$ corresponds to the standard normal distribution.

where $C = C_d + C_p$ and $S = S_d + S_p$.

From this logic, we conclude that a trial will occur if the minimum sentence a prosecutor will accept is greater than the maximum sentence that a defendant will accept. This model has features that match intuition. Trials are more likely in cases where prosecutors have a high expectation of conviction, while defendants have a lower expectation. Increasing the cost of going to trial makes a trial less likely to occur, while increasing the cost of settling makes a trial more likely to occur. Trials are also more likely as the length of sentence given conviction increases (assuming settlement costs are less than trial costs). This occurs because the relative cost savings from settling rather than going to trial decrease in sentence length²⁴

If, however, $\underline{A} \leq \overline{B}$, then any negotiated sentence length $J' \in [\underline{A}, \overline{B}]$ from a plea bargain would be preferable to going to trial for both the prosecution and defendant. If we assume that $C_p = S_p$ and $C_d = S_d$, then the defense and prosecution would be willing to accept any negotiated sentence length $J' \in [P_p \cdot J, P_d \cdot J]$. By our assumption of rational expectations, we have that $E[P_p] = E[P_d]$, and therefore:

$$E[J \text{ from plea}] = E[P_p \cdot J] = E[P_d \cdot J] = J \cdot F(Y')$$

Inspecting above, we see that this produces the result that $E[J \text{ from plea}] = E[J \text{ at trial}]$. So, if there is no cost difference between going to trial and settling, we would expect to see no difference between the expected negotiated sentence and the expected sentence at trial.

Allowing now the costs to differ, we can discuss the “shadow of the law” result. Let us define:

$$D \equiv \frac{C - S}{J}$$

Assuming $D > 0$, our decision to go to trial can be reframed as²⁵:

$$\text{Trial if } F(\hat{Y}'_p) - F(\hat{Y}'_d) > D \tag{1}$$

Additionally, because of our assumption of rational beliefs,

$$E[F(\hat{Y}'_p) - F(\hat{Y}'_d)] = 0 < D \tag{2}$$

²⁴ This is directly visible from the term equation $P_p - P_d > \frac{C-S}{J}$. Assuming $C - S > 0$, then $\frac{C-S}{J}$ falls as J increases. This increases the likelihood that $P_p - P_d$, or that the two parties go to trial.

²⁵ This assumption is not strictly necessary. If $C < S$, then $D < 0$ for all possible sentence lengths. Since the prosecutor and defendant will end at trial if $P_p - P_d > \frac{C-S}{J} = D$, and because $E[P_p - P_d] = 0$, having $D < 0$ would imply that in expectation each case will go to trial. Moreover, it will imply that, in actuality, a majority of cases will go to trial. This result would be wildly inconsistent with reality where fewer than 10% of cases go to trial. Thus, we can safely restrict our attention to the case where $D > 0$.

Equation (2) tells us that in expectation, cases will be settled with a plea bargain and not a trial, a feature that matches both intuition and reality. Equation (1) tells us that *some* cases will go to trial, and that these cases will be those with the greatest divergence between prosecutor confidence and defendant pessimism.

This paper seeks to determine whether the expected sentence length differs between a plea bargain and a trial. We as researchers cannot possibly observe \hat{Y}'_p and \hat{Y}'_d . Moreover, we cannot even observe a complete panel of variables necessary to fully determine \hat{Y}'_p and \hat{Y}'_d , as there are unobservable characteristics. As such, we will almost certainly have estimation bias if we ignore the fact that the choice to take a plea bargain is a function of unobservable case quality²⁶

To properly handle this estimation bias, we will need to construct an instrument for the probability of taking a plea bargain that is unrelated to the probability of guilt at trial. Given our above model, any factor that randomly disturbs settlement costs, trial costs, sentence lengths, the distribution of Y , or the distribution of the ε 's would suffice – all variables that enter into Equation (1).

Any variable that increases (decreases) $Var \left[F(\hat{Y}'_p) - F(\hat{Y}'_d) \right]$ increases (decreases) the probability that Equation (1) is satisfied (i.e. that the parties go to trial). Thus, any such variable that satisfies our exclusion restriction²⁷ could serve as a good instrument for whether the case goes to trial.

Up to this point, we have introduced uncertainty only insofar as the defense and prosecutor receive a noisy signal about the true case quality Y' . We could easily change the source of uncertainty to be Y^* , the bar for conviction. This uncertainty, $\varepsilon_p, \varepsilon_d \sim N(0, \sigma^2)$ would manifest because different judges will have idiosyncratically different definitions of “reasonable doubt” in a case, and the formulae above will be functionally unchanged²⁸. Given this new source of error, any information about judge proclivities will decrease the variance of ε_p and ε_d . This will directly decrease σ^2 , which reduces $Var \left[F(\hat{Y}'_p) - F(\hat{Y}'_d) \right]$.

The primary way where prosecutors and defendants learn about judge proclivities is through inspection of prior decisions. Judges new to the bench will have few prior decisions, thereby providing little information about σ^2 for that judge. Conversely, an experienced judge will have a

²⁶ One unobservable characteristic is the underlying true guilt of the defendant. It is possible that in situations where the defendant is actually guilty, the defendant and prosecutor get higher values for \hat{Y}'_p and \hat{Y}'_d . This would raise both P_p and P_d causing a change in defendant and prosecutor behavior that is completely unmeasurable to us as researchers.

²⁷ We have a situation where the choice to go to trial is correlated with unobserved case characteristics. Prosecutors are more likely to require a trial for cases with smaller ε_p (i.e. easier cases to prosecute), as such $E[\varepsilon_p | trial] \neq 0$. Thus, our exclusion restriction is that we need a variable that is related to the choice to go to trial that is uncorrelated with ε_p .

²⁸ We have chosen to present the above model with the error on the case quality and not judge proclivity for ease of exposition only. The intuition is slightly more natural, but the logic is identical.

large amount of prior casework, allowing both parties to develop accurate assessments of σ^2 . We will now show that changing σ^2 changes, $Var [F(\hat{Y}'_p) - F(\hat{Y}'_d)]$, and can serve as an instrument for the likelihood of pleading:

First, let us denote $Var [F(\hat{Y}'_p) - F(\hat{Y}'_d)]$ as σ_τ^2 . Then, by definition:

$$\begin{aligned}\sigma_\tau^2 &= E \left[\left(F(\hat{Y}'_p) - F(\hat{Y}'_d) \right)^2 \right] - \underbrace{E^2 [F(\hat{Y}'_p) - F(\hat{Y}'_d)]}_{=0 \text{ from Equation 2}} \\ \sigma_\tau^2 &= E \left[\left(F(\hat{Y}'_p) - F(\hat{Y}'_d) \right)^2 \right] \\ \sigma_\tau^2 &= E \left[F(\hat{Y}'_p)^2 - 2F(\hat{Y}'_p)F(\hat{Y}'_d) + F(\hat{Y}'_d)^2 \right]\end{aligned}$$

As ϵ_p and ϵ_d are drawn from the same distribution, we have:

$$\sigma_\tau^2 = 2E [F(Y')^2] - 2E [F(\hat{Y}'_p)F(\hat{Y}'_d)]$$

Lastly, since ϵ_p and ϵ_d are independent, we have:

$$\begin{aligned}\sigma_\tau^2 &= 2E [F(Y')^2] - 2E^2 [F(Y')] \\ \sigma_\tau^2 &= 2Var [F(Y')]\end{aligned}$$

As $F(Y') = \Phi(\frac{Y'}{\sigma})$, we have that σ_τ^2 is monotonically increasing in σ . This result is shown using numerical methods.

4 Data

The data we use in this paper comes from the North Carolina courts. With nearly 10 million residents, North Carolina is a tremendously diverse state with substantial geographic and socio-economic variation. The state is broken into 100 counties with eight metropolitan areas²⁹ with county populations ranging from 4,115 people in Tyrrell County to 1,012,539 people in Mecklenburg County (Charlotte)³⁰.

As we restrict attention in this paper to felony charges, our data primarily comes from cases resolving the Superior Courts of North Carolina. The Superior Court is broken into 50 districts,

²⁹ We use the Office of Management and Budget's definition of a combined statistical area (CSA) as our definition of a metropolitan area. The eight CSAs in North Carolina are: 1) Charlotte-Gastonia-Salisbury, 2) Raleigh-Durham-Cary, 3) Greensboro-Winston-Salem-High Point, 4) Rocky Mount-Wilson, 5) Asheville-Brevard, 6) Fayetteville-Lumberton-Laurinburg, 7) Greenville-Washington, and 8) New Bern-Morehead City.

³⁰ Population estimates from the U.S. Census Bureau's 2014 population estimates. See: http://quickfacts.census.gov/qfd/maps/north_carolina_map.html

which are aggregated into eight divisions. These divisions and districts are shown in Figure 1³¹. Important for our identification strategy, cases are randomly assigned to judges. Using a similar methodology to that employed in Abrams et al. (2012), we use Monte-Carlo simulation techniques to verify that the distribution case characteristics are consistent with random assignment³². While this cannot prove random assignment, we find this check in concert with multiple conversations with officials in the North Carolina court system compelling to support that randomization is occurring. Interestingly, not only are judges randomly assigned to cases, but every six months, Superior Court judges rotate districts within their elected division³³.

Another distinct feature of the North Carolina court system is the use of a strong structured sentencing scheme. Implemented through the Structured Sentencing Act of 1993, North Carolina's structured sentencing program separates felony charges into ten classes (Class A as the most severe and Class I the least) and convicted felons into six different criminal history levels (Level I the least severe and Level VI the most). Over the 20 years of North Carolina's structured sentencing scheme's use, several small tweaks have been implemented, including a revision to the criminal history levels, and the formalization of aggravating and mitigating factors³⁴. Table 1 presents the current sentencing guidelines.

The initial analysis herein proceeds on a data set containing 316,161 cases with a final date of disposition between 1995 and 2009. In order to ensure that our estimates are correct, we have extensively cleaned the data including the manual cleaning of judicial and lawyer information. The entire data cleaning procedure is described in Appendix X³⁵. Of this initial pool of cases, 289,676 or 91.7 percent were resolved through plea bargain. Table 2 presents some of the case characteristics by method of case resolution (plea or not plead).

Table 4 breaks down two facets on which cases are not randomly assigned to plea bargain or not – defendant criminal history and crime severity. Cases that are plead out involve defendants with substantially longer existing felony records; nearly two-third of pleas are by defendants with a level 2 record or higher as compared to only one-seventh of trials. While less extreme, we also

³¹ This map is current as of 2015 and is available at <http://www.nccourts.org/Courts/Trial/District/Documents/SuperiorCourtmap.pdf>.

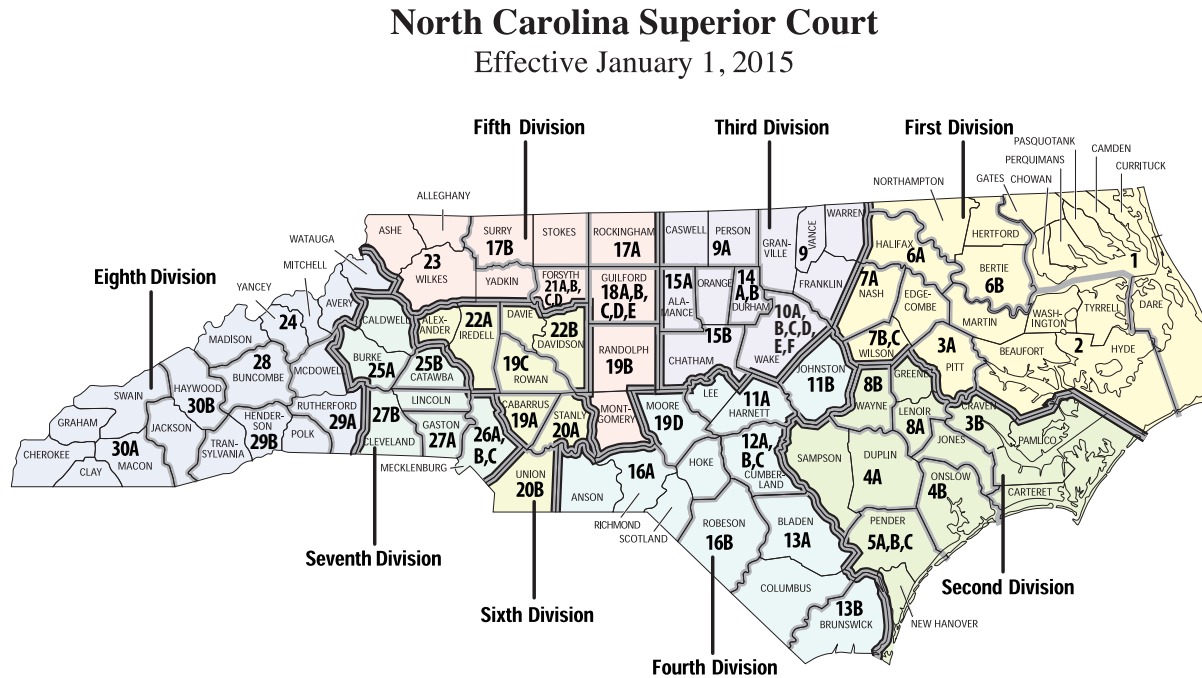
³² See Abrams, Bertrand, and Mullainathan *Do Judges Vary in Their Treatment of Race?* (2012).

³³ This constitutionally mandated rotation is employed to "avoid any favoritism that might result from always having a judge hold court where he or she lives, has close friends among the lawyers and might be more personally familiar with and interested in the particular cases tried. It also contributes to uniformity of procedure. The frequent changes of judges tend to discourage the development of local rules that are unique to that area" (<http://www.nccourts.org/news/documents/judicialsystem.pdf>, pg 5).

³⁴ For an overview of North Carolina's sentencing, see "The North Carolina Sentencing and Policy Advisory Commission: A History of its Creation and its Development of Structured Sentencing" available at http://www.nccourts.org/Courts/CRS/Councils/spac/Documents/commission_history_aug2011.pdf.

³⁵ Appendix X is currently a placeholder, but will be added in the immediate future.

Fig. 1: North Carolina Superior Courts



Note: Districts that have more than one letter associated with the district number (i.e., 10A, B, C, D) are divided into separate districts for electoral purposes. For administrative purposes, they are combined into a single district.

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can see some composition differences in the severity of crimes that are plead or not. Additionally, Table 2 shows us that cases that cased resolved with a plea have 1.5 more charges. All else equal, having more charges is a measure of having a more serious case. Lastly, while we see no difference in the method of resolution by race, cases that resolve with a plea have fewer female defendants and have defendants who are nearly two years younger.

Table 2 also illustrates that we have substantial variation in the method of case resolution by type of crime. We see that the breaking and entering represents 15% of all plea bargains but only 6% of cases that end without a plea. Drug possession is more likely to be resolved without a plea, whereas drug distribution, which is generally more severe, comprises a substantially higher fraction of pleas. Weapon related crimes are slightly less likely to be resolved via a plea, but theft, which is generally less serious, makes up a higher proportion of pleas than trials. This breakdown

is consistent with the idea that more serious crimes are less likely to be resolved with a plea bargain; this feature would drive up the unconditional trial penalty as shown in Table 2.

It is worth clarifying at this point why Tables 2 to 5 are labeled “No Plea” and “Plea” rather than “Trial” and “Plea.” In this paper, we are discussing an *ex-ante* trial penalty rather than an *ex-post* trial penalty. That is, we are not discussing whether, conditional on conviction of a crime, an individual is better off having plead or gone to trial in terms of the post-conviction sentence; there is little reason to doubt the existence of such an *ex-post* trial penalty. Instead, we discuss an *ex-ante* trial penalty, such that before any criminal proceedings (or findings of guilt), a defendant’s expected sentence is shorter from pleading than going to trial. This expectation differs from the sentence condition on conviction not just because there is a chance of being found not guilty, but because there are possibilities that a case will be dismissed before judgment is rendered. Many of these avenues of dismissal are available only after deciding not to accept a plea. Thus, the group that decides not to plea is labeled “No Plea,” because some did not see their cases resolved via judgment at trial.

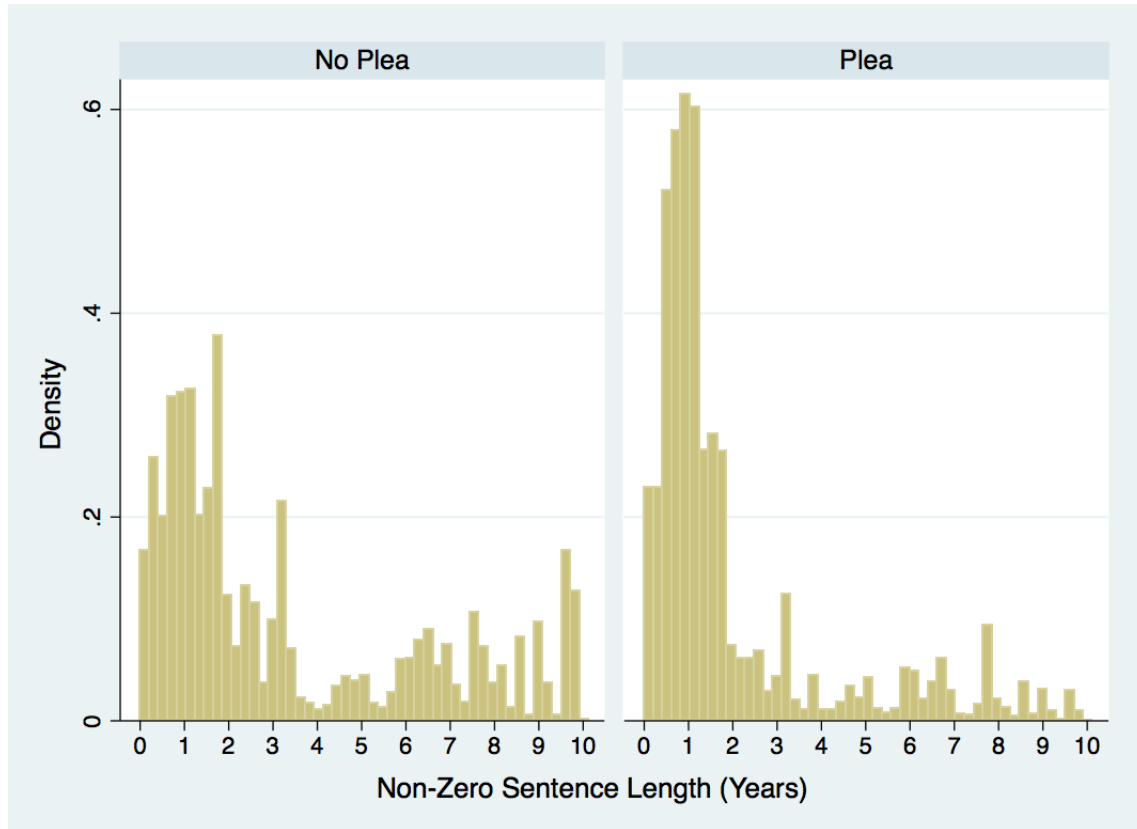
It is additionally useful to compare the full sentencing distribution and not just look at averages. Figure 2 shows the distribution of nonzero sentences determined by our two methods of resolution and is truncated at 10 years for ease of display. In both graphs, probation but not time served is excluded from the length of the sentence. We immediately observe that both sentencing distributions are highly skewed, and the distribution of sentence lengths for cases settled through a plea is substantially more concentrated between 0 and 2 years, while those cases settled without a plea have a less condensed distribution.

If we look at the rate of incarceration, we see that (again excluding probation) those who choose to take a plea bargain face *some* amount of incarceration at nearly double the rate of those who do not plea. It is important to acknowledge again these numbers are unconditional of any observable or unobservable characteristics. Also, while this incarceration rate includes time-served as incarceration, the removal of those who only receive time-served as their sentence still maintains the above relationship.

We see evidence that this relationship may differ when we inspect incarceration rates for different types of crimes. As shown in Table 5, incarceration rates vary after a plea bargain for different types of crime. Some categories of crimes appear to, unconditionally on any selection issues, have shorter sentences after a plea bargain is accepted while others have longer sentences. Full regression results on these differences are forthcoming.

One explanation for this higher rate of incarceration after a plea bargain could be the directly product of the structured sentencing scheme used by North Carolina coupled with our observa-

Fig. 2: Histogram of Non-Zero Sentence Lengths by Method of Resolution



tion in Table 4 that defendants who choose to plead have significantly more prior points (more felony convictions). This combination produces a situation where those accepting plea bargains are inherently facing longer jail sentences even conditional on committing identical crimes with identical observable and unobservable characteristics (except prior points). For example, a first-time defendant facing a class H felony charge faces potentially only half the jail term that another defendant with 10 prior points would face for the same crime.

Thus, Tables 2 to 5 provide strong evidence that we are going to need to control for both observable variation (e.g. a defendant's prior points) and unobservable variation (e.g. presence of eye witnesses) in cases to measure a causal relationship between method of case resolution and expected sentence length. In order to handle observable variation, we use ordinary least squares (OLS) regression specifications, and to handle unobservable variation, we use instrumental variable (IV) regressions with several possible instruments (discussed in Section 5).

Tab. 1: Share of Cases: Crime Class Vs. Prior Points Table

		<i>Level 1</i>	<i>Level 2</i>	<i>Level 3</i>	<i>Level 4</i>	<i>Level 5</i>	<i>Level 6</i>
Factor		0-1	2-5	6-9	10-13	14-17	18+
A		Death or Life Without Parole					
B1	Aggravated	240-300	276-345	317-397	365-456	Life Without Parole	
	Presumptive	192-240	221-276	254-317	292-365	336-420	386-483
	Mitigated	144-192	166-221	190-254	219-292	252-336	290-386
B2	Aggravated	157-196	180-225	207-258	238-297	273-342	314-393
	Presumptive	125-157	144-180	165-207	190-238	219-273	251-314
	Mitigated	94-125	108-144	124-165	143-190	164-219	189-251
C	Aggravated	73-92	83-104	96-120	110-138	127-159	146-182
	Presumptive	58-73	67-83	77-96	88-110	101-127	117-146
	Mitigated	44-58	50-67	58-77	66-88	76-101	87-117
D	Aggravated	64-80	73-92	84-105	97-121	111-139	128-160
	Presumptive	51-64	59-73	67-84	78-97	89-111	103-128
	Mitigated	38-51	44-59	51-67	58-78	67-89	77-103
E	Aggravated	25-31	29-36	33-41	38-48	44-55	50-63
	Presumptive	20-25	23-29	26-33	30-38	35-44	40-50
	Mitigated	15-20	17-23	20-26	23-30	26-35	30-40
F	Aggravated	16-20	19-23	21-27	25-31	28-36	33-41
	Presumptive	13-16	15-19	17-21	20-25	23-28	26-33
	Mitigated	10-13	15-19	13-17	15-20	17-23	20-26
G	Aggravated	13-16	14-18	17-21	19-24	22-27	25-31
	Presumptive	10-13	12-14	13-17	15-19	17-22	20-25
	Mitigated	8-10	9-12	10-13	11-15	13-17	15-20
H	Aggravated	6-8	8-10	10-12	11-14	15-19	20-25
	Presumptive	6-8	6-8	8-10	9-11	12-15	16-20
	Mitigated	4-5	4-6	6-8	7-9	9-12	12-16
I	Aggravated	6-8	6-8	6-8	8-10	9-11	10-12
	Presumptive	4-6	4-6	5-6	6-8	7-9	8-10
	Mitigated	3-4	3-4	4-5	4-6	5-7	6-8

The only categories that do not automatically yield an active sentence are: E(1-2), F(1-3), G(1-4), H(1-5), and I(1-6). Community punishment is an option only for H(1), and I(1-2). An active sentence isn't available for only I(1-3). Community punishment is the only option for I(1).

Tab. 2: Summary Statistics

<i>Variable</i>	<i>No Plea</i>	<i>Plea</i>	<i>Difference</i>	<i>t-Statistic</i>
Offender & Case Characteristics				
Charges	2.11	3.65	-1.54	-51.07
Race (black = 1)	0.56	0.55	0.00	1.43
Sex (female = 1)	0.18	0.16	0.02	8.52
Age	32.13	30.44	1.70	25.26
Incarceration	0.16	0.29	-0.14	-47.47
Sentence	1.15	1.14	0.01	0.67
Sentence No Prob	1.10	0.67	0.43	27.40
Sentence No Prob OR Time Served	1.06	0.61	0.46	29.67
Non-zero Sentence	6.15	1.38	4.77	125.16
Non-zero Sentence No Prob	8.08	2.52	5.56	79.12
Guilty	0.21	1.00	-0.79	-1039.04
Bond Amount (\$Thousands)	27.71	24.26	3.45	3.53
Prior Points	1.03	4.07	-3.04	-96.08
Offense Type				
Arson	0.00	0.00	0.00	0.41
Assault	0.05	0.04	0.01	9.61
Breaking & Entering	0.06	0.15	-0.08	-37.42
Burglary	0.01	0.02	-0.01	-9.98
Death	0.00	0.00	0.00	2.30
Drug Distribution	0.23	0.26	-0.03	-11.08
Drug Possession	0.16	0.09	0.07	39.18
Forgery	0.02	0.04	-0.02	-18.08
Habitual Felon	0.02	0.02	0.00	5.23
Robbery	0.07	0.07	0.00	-1.41
Sex Crime	0.09	0.05	0.04	29.69
Theft	0.15	0.17	-0.02	-9.96
Weapon	0.04	0.03	0.00	3.83
Other	0.10	0.06	0.03	21.39

Notes: There are 316,181 total incidents in this table – 26,505 that weren't plead out and 289,676 that were plead. Observations were included if the judge was able to be identified by name, and was plausibly serving in the region at the time, and was seen more than 100 times in the data. Additionally, we exclude if the case involved murder, and the date associated with the case is not between 1995 and 2009.

Tab. 3: Share of Cases: Crime Class Vs. Prior Points Table

	<i>Level 1</i>	<i>Level 2</i>	<i>Level 3</i>	<i>Level 4</i>	<i>Level 5</i>	<i>Level 6</i>	<i>Total</i>
<i>Overall</i>							
Crime A	0.48	0.55	0.33	0.11	0.02	0.01	1.50
Crime B1	1.06	0.49	0.20	0.12	0.02	0.02	1.92
Crime B2	0.04	0.02	0.01	0.01	0.00	0.00	0.09
Crime C	1.02	0.61	0.56	0.64	0.22	0.18	3.22
Crime D	2.50	1.70	0.97	0.60	0.15	0.11	6.03
Crime E	1.29	0.91	0.36	0.20	0.05	0.04	2.85
Crime F	2.02	1.36	0.90	0.48	0.13	0.10	4.99
Crime G	3.08	3.45	2.42	1.53	0.34	0.24	11.06
Crime H	16.02	14.20	8.00	4.71	1.25	0.87	45.05
Crime I	9.16	7.26	3.55	1.83	0.47	0.38	22.65
Total	37.23	30.61	17.33	10.21	2.66	1.95	100.00

Notes: There are 316,181 total incidents in this table – 26,505 that weren't plead out and 289,676 that were plead. Observations were included if the judge was able to be identified by name, and was plausibly serving in the region at the time, and was seen more than 100 times in the data. Additionally, we exclude if the case involved murder, and the date associated with the case is not between 1995 and 2009.

Tab. 4: Share of Cases: Crime Class Vs. Prior Points Table By Plea

	<i>Level 1</i>	<i>Level 2</i>	<i>Level 3</i>	<i>Level 4</i>	<i>Level 5</i>	<i>Level 6</i>	<i>Total</i>
<i>No Plea</i>							
Crime A	0.09	0.21	0.20	0.06	0.02	0.01	0.59
Crime B1	2.86	0.50	0.31	0.23	0.07	0.06	4.03
Crime B2	0.16	0.03	0.02	0.02	0.01	0.01	0.25
Crime C	2.75	0.52	0.58	0.52	0.19	0.21	4.76
Crime D	4.16	0.75	0.63	0.51	0.16	0.08	6.29
Crime E	3.38	0.39	0.17	0.13	0.04	0.03	4.13
Crime F	4.35	0.48	0.35	0.25	0.06	0.04	5.53
Crime G	4.40	0.55	0.48	0.32	0.10	0.05	5.90
Crime H	29.37	1.67	1.10	0.66	0.18	0.17	33.15
Crime I	28.94	0.81	0.45	0.22	0.07	0.04	30.52
Total	85.29	5.91	4.29	2.92	0.89	0.69	100.00
<i>Plea</i>							
Crime A	0.51	0.58	0.34	0.11	0.02	0.01	1.58
Crime B1	0.90	0.49	0.19	0.11	0.02	0.01	1.73
Crime B2	0.03	0.02	0.01	0.01	0.00	0.00	0.08
Crime C	0.86	0.62	0.56	0.65	0.22	0.17	3.08
Crime D	2.35	1.78	1.00	0.60	0.15	0.12	6.01
Crime E	1.10	0.96	0.38	0.21	0.05	0.04	2.73
Crime F	1.81	1.44	0.95	0.50	0.14	0.10	4.95
Crime G	2.96	3.72	2.60	1.64	0.36	0.26	11.53
Crime H	14.80	15.34	8.63	5.08	1.34	0.94	46.14
Crime I	7.35	7.85	3.84	1.97	0.51	0.41	21.93
Total	32.83	32.87	18.53	10.88	2.82	2.07	100.00

Notes: There are 316,181 total incidents in this table – 26,505 that weren't plead out and 289,676 that were plead. Observations were included if the judge was able to be identified by name, and was plausibly serving in the region at the time, and was seen more than 100 times in the data. Additionally, we exclude if the case involved murder, and the date associated with the case is not between 1995 and 2009.

Tab. 5: **PENDING:** Case Outcomes by Offense and Method of Disposition

Offense	<i>No Plea</i>			<i>Plea</i>			<i>Difference</i>
	<i>Sentence</i>	<i>Incarceration</i>	<i>Finding of Guilt</i>	<i>Sentence</i>	<i>Incarceration</i>	<i>Finding of Guilt</i>	
Characteristics							

Notes: There are 316,181 total incidents in this table – 26,505 that weren't plead out and 289,676 that were plead. Observations were included if the judge was able to be identified by name, and was plausibly serving in the region at the time, and was seen more than 100 times in the data. Additionally, we exclude if the case involved murder, and the date associated with the case is not between 1995 and 2009.

5 Results

In the previous Section, we discuss the myriad of observable and unobservable ways where cases settled via plea bargain differs substantially from those resolved by other means. As a first step for controlling for these differences, we begin with an OLS specification to control for observable characteristics. Specifically, we estimate:

$$Sentence_{i,j} = \alpha + \beta_1 \cdot plea_i + \sum \gamma X_{i,j} + \varepsilon_{ij} \quad (3)$$

Where $Sentence_{i,j}$ is the non-probation sentence in years, $plea_i$ is a indicator for whether the case was resolved via a plea bargain. The subscript i indexes the incident, and the subscript j the superior court judge. The term $\sum \gamma X_{i,j}$ is the set of case, defendant, and judge controls that differ depending on the specific regression specification.

The results of estimating Equation 5.1 are presented in Table 6. Specifications (1) examines the relationship between sentence length and plea bargain without any controls, and thus produces the same -0.43 estimate as seen in Table 2. Specification (2) introduces controls for observed defendant and case characteristics. Specifications (3)-(6) introduce fixed effect controls for year (3), superior court judge (4), lead charge (5), and defense lawyer type (6). Regression (7) includes all of these previous fixed effects and controls for the proper structured sentencing cell. Regression (8) replicates (7) but clusters the estimated standard errors at the judge to allow for intrajudge correlation.

Across all specifications we see a consistently negative and significant effect for pleading guilty on expected sentence length, with a coefficient around -0.5 years and a standard error near 0.016 . This result suggests that on average, defendants who plead guilty can expect a half a year shorter sentence than those who do not. The decrease in our estimated coefficient from specification (1) to (2) matches intuition; as shown in Tables 2 and 4, defendants who take plea bargains tend to have more serious cases (based on observable features such as the number of charges, the class of the charge, and the level of prior points). Therefore, by controlling partially for measures of case severity should remove some of the impact of this selection, and make the choice to plead seem more appealing.

In fact, only in specification (5) do we see a lessening of our estimates of the benefit to pleading. Specification (5) includes demographic controls for the defendant, and then controls for the lead charge being faced as well as the appropriate structured sentencing cell (from Table 1). With only these controls, our estimates fall by almost 2 months from nearly 6.5 months (from specifications 2 to 4) to 4.5 months. The drop in the trial penalty is somewhat surprising; given that Table 4 shows that defendants taking a plea bargain tend to have longer criminal histories, we should expect

that failing to account for structured sentencing points would actually reduce the apparent trial penalty. Instead, we see the opposite, and controlling for criminal history actually increases the penalty.

Specifications (7) and (8) combine all our controls, trying to account for all possible observable features (year, judge, criminal statue, defense attorney type, structured sentencing criminal case, and prior points level). Even clustering our estimated standard errors at the judge to allow for intra-judge correlation, our estimated 6 month trial penalty persists³⁶

Table 7 presents similar regressions as in Table 6, but with incarceration and not sentence length as the dependent variable. Specification (1) replicates the simple mean estimate presented in Table 2. Unlike Table 6, in Table 7, we see a trend that higher incarceration rate after pleading guilty decline when we control for the panel of observable characteristics. In our specifications (7) and (8), we see that our estimate of increased incarceration rate after pleading guilty has fallen by approximately 85% (from 14% to 2%).

To some extent this result should be unsurprising given North Carolina's structured sentencing scheme. For example, we expect defendants with longer criminal histories to be more likely to face jail time, so controlling for criminal history should account for much of the unconditional difference. In specifications (2)-(4) and (6), the flag for first offense serves as a proxy for the amount of prior points³⁷, and in (5), (7), and (8), we fully control for the lead charge and structured sentencing cell. Thus, this is suggestive that much of the difference in incarceration rates evident in Table 2 is attributable to observable case characteristics and North Carolina's structured sentencing scheme.

5.1 Addressing Unobservable Variation

Up to this point, we have explored only those observable differences between cases resulting with and without a plea. As originally described Section 3.1, we have reason to believe that there are important selection effects occurring based on unobservable heterogeneity. This selection on unobservables may bias the coefficient on plea and prohibit causal interpretation of our OLS results.

As described in Table 4, we have reason to think potentially worse cases go to trial, while defendants with longer criminal histories are more likely to plead³⁸ We could easily expect to see

³⁶ A 95% confidence interval around our estimated trial penalty in specification (8) is $[-0.59, -0.43]$, which translates to a trial penalty between 5.16 months and 7.08 months.

³⁷ The first offense variable is functionally an indicator variable that equals 1 if the defendant has 0 prior points.

³⁸ For example, 36% of trials involve a lead charge of a class above H whereas only 32% of plea bargains are above class H. Based on the observables, it appears that more severe cases are not resolved with a plea bargain. Cases resolving with a plea involve a defendant with a prior points level of 2 or higher 67% of the time, as compared to 15% in cases

similar trends with unobservables. District attorneys may be less inclined to plead unobservably worse cases in order to not seem “light on crime.” Were such unobserved selection occurring, our estimate of the trial penalty would be inflated (i.e. our coefficient on plea would be overly negative). We could also construct a story where defendants with unobservably worse cases may plead guilty knowing that they have an idiosyncratically higher chance of losing at trial. If this form of selection were occurring, we would expect a lower trial penalty (i.e our coefficient on plea should be more negative).

Regardless of the direction that bias manifests, we have sufficient fear of unobservable selection to motivate the use of instrumental variable regression. To do so, we make use of three possible instrument for likelihood of pleading: 1) judicial tenure, 2) defense attorney tenure, and 3) the number of judge and defense attorney interactions. The IV specification is as follows:

$$\text{First Stage: } plea_i = \tilde{\alpha} + \tilde{\beta}_1 \cdot instrument_{i,j} + \sum \tilde{\gamma} X_{i,j} + v_{ij} \quad (4)$$

$$\text{Second Stage: } Sentence_{i,j} = \alpha + \beta_1 \cdot \widehat{plea}_i + \sum \gamma X_{i,j} + \varepsilon_{ij} \quad (5)$$

Where \widehat{plea}_i is the estimated value of $plea_i$ that comes from first first stage regression, and $instrument_{i,j}$ is the instrument(s) used in the specific regression³⁹.

As we discuss at the end of Section 3.1, we require an instrument that is related to the propensity for a defendant to plead guilty but unrelated to the idiosyncratic probability of guilt at trial. As such, a variable such as the defendant’s prior points would not work as an instrument as it is suggestive of a criminal propensity that may be related to the probability of guilt at trial.

We propose three possible instruments that all relate to the knowledge of prosecutors and defendants about judge proclivities. Prosecutors and defendants primarily learn about judge behavior through inspection of prior decisions. Judges new to the bench will have few prior decisions, thereby providing little information and an experienced judge will have a large amount of prior casework, allowing both parties to develop accurate assessment. This provides the motivation for our use of judicial tenure as an instrument⁴⁰.

Similarly to judicial tenure, an inexperienced lawyer will have less relevant experience and knowledge about judicial temperament regardless of the judge’s tenure. As such, we estimate Equation

that do not plead.

³⁹ Our estimates are obtained using two stage least squares regression (2SLS), but are qualitatively similar if we use generalized method of moments (GMM) to estimate.

⁴⁰ Judicial tenure we are classifying as the amount of time (in days) that the judge has served on the superior court. Lawyers may have previous experience with judges from district court or their professional life. Additionally, we assume that judges have stable sentencing proclivities that do not systematically evolve over their tenure.

(4) using the tenure of the defense attorney instead of the judicial tenure.⁴¹

Lastly, we might think that the knowledge of judge proclivity is related not just to the amount of time the lawyer and judge have been practicing in their roles, but the number of times the two have previously met. To create this measurement, we restrict our attention to cases where: 1) the lawyer was barred in North Carolina after 1995, 2) the judge took their seat after 1995, or 3) we see no judge-lawyer interactions before 1998. We make this restriction to ensure that we are accurately counting all interactions between the specific judge and lawyer. If we did not make these cuts, we would only be measuring the number of interactions observed in our data and not that the pair has *ever* had.

We as we expect learning to be greatest early in the process, we restrict our attention to the early periods for each of the instruments. For the first two, judicial and lawyer tenure, this translates to focusing on cases within the first two and a half years of the relevant tenures. For the third instrument, we restrict our attention to the first twenty five times the judge and lawyer have met.

Additionally, the curvature evident suggests that squared tenure (both judicial and lawyer) as well as the squared number of encounters may be worth including as instruments rather than just the base level. As such, equation (3) in Tables 8 and 10 have a first stage with squared versions of the instruments. This alternative specification takes the form:

$$\text{Second Stage: } \text{Sentence}_{i,j} = \alpha + \beta_1 \cdot \widehat{\text{plea}}_i + \sum \gamma X_{i,j} + \varepsilon_{ij} \quad (4)$$

$$\text{First Stage: } \text{plea}_i = \tilde{\alpha} + \tilde{\beta}_1 \cdot \text{instrument}_{i,j} + \beta_2 \cdot \text{instrument}_{i,j}^2 + \sum \tilde{\gamma} X_{i,j} + v_{ij} \quad (5a)$$

The inclusion of the squared instrument term is both appropriate due to the visible curvature of the relationship as well as to address the possibility of weak instruments.

As shown in Tables 8 to 10, regardless of the instrument used, we find no evidence suggestive of a significant difference between sentences received as a result of a plea bargain from those received after choosing not to plead. As discussed in sections 6 and 7, this should not necessarily be taken as evidence suggesting that there is no difference, as we struggle with weak instruments.

6 Discussion

The results presented above provide the first evidence of the existence of an *ex-ante* trial penalty. From simple summary statistics, we find that the average sentence from a plea bargain is 5 months

⁴¹ Lawyer tenure is defined as the amount of time (in days) that the lawyer has been barred in North Carolina. Lawyers may have previous experience in other states, or may not have been continuously practicing as criminal attorneys.

Tab. 6: Effect of Pleading on Sentence Length (OLS)

	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)	OLS (6)	OLS (7)	OLS (8)
Plea	-0.43** (0.015)	-0.54** (0.016)	-0.56** (0.016)	-0.57** (0.016)	-0.38** (0.015)	-0.55** (0.016)	-0.51** (0.014)	-0.51** (0.041)
Defendant Race (black == 1)		0.10** (0.009)	0.10** (0.009)	0.13** (0.009)	0.09** (0.009)	0.07** (0.009)	-0.01 (0.008)	0.00 (0.008)
Defendant Sex (female == 1)		-0.55** (0.012)	-0.55** (0.012)	-0.55** (0.012)	-0.21** (0.012)	-0.57** (0.012)	-0.12** (0.010)	-0.11** (0.006)
Defendant Age		0.01** (0.000)	0.01** (0.000)	0.01** (0.000)	0.01** (0.000)	0.01** (0.000)	0.00** (0.000)	0.00** (0.000)
Number Charges		0.02** (0.001)	0.02** (0.001)	0.02** (0.001)	0.02** (0.001)	0.02** (0.001)	0.01** (0.001)	0.01** (0.001)
First Offense		-0.33** (0.009)	-0.38** (0.009)	-0.35** (0.009)	-0.47** (0.008)	-0.31** (0.009)	0.02 (0.011)	0.01 (0.010)
Time FE	No	No	Yes	No	No	No	Yes	Yes
Judge FE	No	No	No	Yes	No	No	Yes	Yes
Statue FE	No	No	No	No	Yes	No	Yes	Yes
Lawyer Type FE	No	No	No	No	No	Yes	Yes	Yes
Structured FE	No	No	No	No	Yes	No	Yes	Yes
Clustered SE	No	No	No	No	No	No	No	Yes
Observations	3163,30	309,171	309,171	309,171	309,171	308,482	307,070	307,070
Adjusted R^2	0.002	0.021	0.024	0.028	0.210	0.029	0.408	0.406

Notes: (**) refers to significance at the 1% level and (*) at the 5% level. Clustering in regression (8) is done at the judge level to handle heterogeneity across charges.

shorter than the average sentence of those who did not plead. After controlling for observable heterogeneity (OLS regressions in Table 6), this difference actually increases to 6 months. Our use of instrumental variables to control for additional unobserved heterogeneity provides no contradictory evidence to this penalty, although none of our instruments are tremendously strong.

The results presented here are consistent with prior empirical literature measuring sentences received from pleading or trial. As shown in Table 2, we see a substantially longer sentence after a

Tab. 7: Effect of Pleading on Incarceration (OLS)

	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)	OLS (6)	OLS (7)	OLS (8)
Plea	0.14** (0.003)	0.08** (0.003)	0.09** (0.003)	0.09** (0.003)	0.08** (0.003)	0.08** (0.003)	0.02** (0.003)	0.02** (0.010)
Defendant Race (black == 1)		0.03** (0.002)	0.03** (0.002)	0.03** (0.002)	0.03** (0.002)	0.02** (0.002)	0.01** (0.001)	0.00 (0.002)
Defendant Sex (female == 1)		-0.14** (0.002)	-0.14** (0.002)	-0.14** (0.002)	-0.08** (0.002)	-0.15** (0.002)	-0.05** (0.002)	-0.05** (0.002)
Defendant Age		0.002** (0.000)	0.001** (0.000)	0.002** (0.000)	0.003** (0.000)	0.002** (0.000)	-0.001** (0.000)	-0.001** (0.000)
Number Charges		0.001** (0.000)	0.002** (0.001)	0.002** (0.000)	0.001** (0.000)	0.001** (0.000)	0.000 (0.000)	0.000 (0.000)
First Offense		-0.21** (0.002)	-0.21** (0.002)	-0.21** (0.002)	-0.22** (0.002)	-0.20** (0.002)	-0.04** (0.002)	-0.03** (0.003)
Time FE	No	No	Yes	No	No	No	Yes	Yes
Judge FE	No	No	No	Yes	No	No	Yes	Yes
Statue FE	No	No	No	No	Yes	No	Yes	Yes
Lawyer Type FE	No	No	No	No	No	Yes	Yes	Yes
Structured FE	No	No	No	No	Yes	No	Yes	Yes
Clustered SE	No	No	No	No	No	No	No	Yes
Observations	317,553	310,357	310,357	310,357	310,357	309,664	307,271	??
Adjusted R^2	0.007	0.087	0.088	0.101	0.197	0.114	0.327	0.349

Notes: (**) refers to significance at the 1% level and (*) at the 5% level. Clustering in regression (8) is done at the judge level to handle heterogeneity across judges.

finding of guilt at trial than after a plea bargain⁴² This corresponds to the *ex-post* trial penalty, and is robust to the inclusion of our panel of controls for observable case differences⁴³. That said, despite push-back against the analysis of the *ex-ante* trial penalty, we still believe it to be the proper

⁴² Specifically, we can look at variables “Non-zero Sentence” and “Non-zero Sentence No Prob”, which excludes observations where probation is received and the sentence is not active. In both these rows, we see nearly 4 times longer average sentences from trial than pleas.

⁴³ The coefficient on plea for this estimate when we use Specification (8) from Table 6 suggests a trial penalty of nearly 3 years. This is less than the raw summary statistic difference of 5.5 years, emphasizing the importance of controlling for observable case heterogeneity.

Tab. 8: Effect of Pleading on Sentence Length (IV)

	(1)	(2)	(3)
Instrument	First Stage	Tenure	Tenure, Tenure ²
Judge Tenure	-0.002** (0.0001)		
Plea		4.95 (4.16)	1.88 (1.89)
Stature FE		y	y
Year FE		y	y
Structured FE		y	y
First Stage F-stat	74.36	74.36	74.50
Observations	156,901	156,901	156,901
Adjusted R^2	0.21	0.06	0.30

Notes: (**) refers to significance at the 1% level and (*) at the 5% level. Clustering is done at the judge level to handle heterogeneity across judges.

Tab. 9: Effect of Pleading on Sentence Length (IV):
Judge-Lawyer Encounters as Instrument

	(1)	(2)	(3)
Instrument	First Stage	J-L Meetings	J-L Meetings, J-L Meetings ²
Judge & Lawyer Meetings	0.0007 (0.0001)		
Plea		-49.56 (457.47)	6.72 (14.98)
Stature FE		y	y
Year FE		y	y
Structured FE		y	y
First Stage F-stat	31.78	31.78	31.78
Observations	113,430	113,430	113,430
Adjusted R^2	31.78	–	–

Notes: (**) refers to significance at the 1% level and (*) at the 5% level. Clustering is done at the judge level to handle heterogeneity across judges. In order to improve the strength of the instrument, judge-lawyer encounters were excluded over 25, as we expect the greatest learning to occur early.

Tab. 10: Effect of Pleading on Sentence Length (IV)

	(1)	(2)	(3)
Instrument	First Stage	Tenures, J-L Meetings	Tenures & J-L Meetings, Tenures ² & J-L Meetings ²
Judge Tenure	-0.0005 (0.0005)		
Lawyer Tenure	-0.0002** (0.0000)		
Judge & Lawyer Meetings	0.0000 (0.0000)		
Plea		-117.85 (457.54)	--6.28 (3.55)
Stature FE		y	y
Year FE		y	y
Structured FE		y	y
First Stage F-stat	71.96	71.96	71.46
Observations	111,163	111,163	111,163
Adjusted R ²	0.18	.	.

Notes: (**) refers to significance at the 1% level and (*) at the 5% level. Clustering is done at the judge level to handle heterogeneity across judges.

measurement for understanding the implications of the shadow of the law theory.

The theory of The Shadow of the Law was first proposed by Mnookin and Kornhauser in their 1979 article discussing divorce settlements. The authors propose the notion that despite the vast majority of divorces resolving with a negotiated settlement, the law shapes these negotiations. As either party is always free to opt out of the negotiations and request judicial intervention, the divorce negotiations are inherently framed by this outside option. The shadow of the law has proven compelling, with many other authors employing the framework⁴⁴ It is straightforward to extend Mnookin and Kornhauser's original idea to both criminal and civil cases. This paper and most every paper concerning a trial penalty fundamentally rests on the shadow of the law argument to provide a logical connection between plea bargains and trials.

⁴⁴ For two early examples of its use see: Cooter, Marks, and Mnookin *Bargaining in the Shadow of the Law: A Testable Model of Strategic Behavior* (1982) and Priest and Klein *The Selection of Disputes for Litigation* (1984).

In recent years, the shadow of the law argument has come under pragmatic scrutiny. Bibas addresses many features of the criminal justice system that complicate the real-world function of the shadow of the law model⁴⁵. Bibas also invokes well-documented Behavioral Economics results as problematic for the assumptions of the model. Stuntz's criticism focuses on the motivation and discretion of prosecutors⁴⁶. Stuntz questions whether prosecutors' goal is to maximize sentences, and questions the functioning of the shadow of the law model when prosecutors have discretion in what cases to see.

At this point, let us consider how the shadow of the law argument translates into the trial penalty. Essentially the formal model presented in Section 3 mathematically characterizes the shadow of the law; defendant and prosecutors are able to negotiate any plea they choose⁴⁷, and their willingness to accept a given plea deal is made only in comparison to the opt-out value of a trial. Recalling the notation of Section 3, if $\underline{A} \leq \bar{B}$, then any negotiated sentence length $J' \in [\underline{A}, \bar{B}]$ from a plea bargain would be preferable to going to trial for both the prosecution and defendant. That is, prosecutors and defendants are willing to accept any plea bargain sentence length that is above the minimum required by the prosecutor, \underline{A} , and below the maximum permitted by the defendant, \bar{B} .

In Section 3 we proceeded to say that "if we assume that $C_p = S_p$ and $C_d = S_d$ ", then, in conjunction with our rational expectation assumptions, \underline{A} and \bar{B} coincide. In other words, if the cost of going to trial C is the same as settling S , the defendant and prosecutor do not need to negotiate, agreeing on the value to avoiding a trial. In this case, our trial penalty is simply $E[J] - \underline{A} = E[J] - \bar{B}$.

What if we assume instead that trials are more costly for prosecutors than plea bargains (ie. $C_p > S_p$), as trials are significantly more time-consuming and expose the possibility of a not guilty verdict. If this is true, then we have a range of acceptable negotiated pleas, any sentence length in the range $[\underline{A}, \bar{B}]$.

We must then ask: what will the outcome of the negotiated plea be? If the defendant has all power in negotiation, then the prosecutor will be pushed to accept his least desired outcome, \underline{A} . If, on the other hand, the prosecutor has all bargaining power, the defendant will be driven up to their least desired outcome, \bar{B} . Translating in terms of trial penalty, assuming the probability of conviction at trial is independent of bargaining power, the trial penalty is largest when the defen-

⁴⁵ Stephanos Bibas *Plea Bargaining Outside the Shadow of Trial* (2004).

⁴⁶ W. Stuntz *Plea Bargaining and Criminal Laws Disappearing Shadow* (2004)

⁴⁷ In the model only. Innumerable constraints exist on plea bargain negotiations in the real world.

dant has all the bargaining power, and smallest when the prosecutor has all the power⁴⁸.

Given the enormous time cost to trial relative to a plea bargain for prosecutors and the cost to failing to obtain a conviction at trial, one may suspect that the defendant has *some* power in the negotiation. Given this, we may then ask: “what impact would structured sentencing have on the outcome of the negotiation?” Temporarily ignoring the option of allowing the defendant to plead to a lesser charge, we may think of a structured sentencing scheme as imposing bounds on the range of acceptable plea bargains. An unconstrained prosecutor may be willing to accept a sentence length of \underline{A} , but the structured sentencing scheme may not allow any sentence less than $\bar{J} > \underline{A}$ for the given crime. Were this the case, the imposition of the structured sentencing scheme would have the same effect as raising prosecutor power in negotiation⁴⁹.

Now, let us permit the prosecutor and defendant to agree to a plead to a lesser charge. Introducing this power increases the span of feasible bargains on the lower end of sentence lengths only. Consider a first-time defendant currently facing a charge of assault with intent to kill in North Carolina. Given the structured sentencing scheme, this defendant if convicted at trial faces 44 to 92 months in jail. If he were to plead instead to assault with intent to inflict serious injury, he faces only 15 to 31 months in jail.

Now, let us consider the case where $\underline{A} = 30$ and $\bar{B} = 60$, and let us say the defendant’s bargaining power is sufficient to always force the prosecutor to agree to a sentence 20% of the way between the minimum and maximum available. Without the ability to plead to a lesser charge, this means that the defendant and prosecutor would settle on a sentence of 53.6 months⁵⁰. With the ability to plead to a lesser charge, the minimum possible sentence length is reduced without any change to the maximum. This then means that the defendant and prosecutor will settle on a charge of only 30.4 months⁵¹.

Without the structured sentencing guidelines, we might think that the negotiated plea would be more of a direct product of \underline{A} and \bar{B} . For example, we might think that a similar defendant’s bargaining power would be sufficient to always force the prosecutor to agree to a sentence 20% of the way between the minimum and maximum available. Without the formality of the structured sentencing scheme, this would yield a sentence from pleading of 36 months in jail⁵². Now, assuming

⁴⁸ To see this, we assume that $J * Pr(\text{guilt at trial})$ doesn’t depend on power. Then, we have $J * Pr(\text{guilt at trial}) - \bar{B} < J * Pr(\text{guilt at trial}) - \underline{A}$. That is the trial penalty is maximized with full defendant bargaining power.

⁴⁹ Theoretically, it could also be the case that the upper sentence limit is binding. That is, $\bar{J} < \bar{B}$. In this case, the structured sentencing scheme would have the effect of reducing prosecutor power.

⁵⁰ The math behind this is straight forward. The minimum sentence is 44 months and the maximum 92. The sentence 20% of the way between 44 and 92 months is 53.6 months.

⁵¹ The underlying math: $((92 - 15) \cdot 0.2 + 15) = 30.4$.

⁵² The underlying math: $((60 - 30) \cdot 0.2 + 30) = 36$.

the expected sentence from trial is unchanged, at $E[J]$, we can see that the imposition of a structured sentencing scheme actually increases the trial-penalty: $E[J] - 30.4 > E[J] - 36$, regardless of $E[J]$.

This discussion is not meant to necessarily reconcile the positive ex-ante trial penalty observed in North Carolina with previous literature. Rather, this discussion is important to understand the immeasurable ways in which the structure of the criminal law may intentionally or unintentionally shape the shadow of the law.

7 Conclusion

The article seeks to further our understanding of whether defendants are better off accepting or rejecting a plea bargain. Unlike the vast majority of previous scholarship, we focus on comparing the unconditional average sentence from taking a plea bargain or declining to do so. While asking ex-post whether taking a plea bargain would have yielded a lower sentence is certainly of some interest, we assert that the shadow of the law model is predicated on the ex-ante decision. Using OLS regression to control for issues of selection based on observable case characteristics, we find evidence suggesting that on average the expected sentence is longer in North Carolina's for those who reject a plea, but that there is substantial heterogeneity. One particularly interesting form of heterogeneity is the type of crime committed⁵³. Early results suggests that the benefits acquired via a plea bargain may vary substantially depending on the nature of the crime the defendant is facing.

There are real concerns that unobservable case characteristics may bias these estimates. For example, we might worry that the underlying true guilt of a defendant may influence the choice to take a plea bargain and may be (hopefully) related to the ex-post sentence lengths, but is not directly observable to even the courts. As such, we employ instrumental variable regression using judge tenure, lawyer tenure, and judge-lawyer encounters as instruments for likelihood of plea bargain. The underlying logic justifying these three instruments are straight-forward: uncertainty about case outcomes changes the propensity to go to trial. As experience (both judge and council) is one of the primary way that uncertainty about case outcomes is reduced, then we should be able to use experience to instrument for the propensity to accept a plea bargain.

In terms of direct learning about judge temperament, the number of previous times a specific lawyer has interacted with a specific judge is reasonably the optimal measurement. Regardless of the instrument used, we never obtain estimates that come close to being statistically different from

⁵³ At time of writing, this particular avenue of heterogeneity has not been fully explored, but will be in the immediate future.

zero. This result provides no evidence to counter the results from the OLS regression estimates. We should, however, be somewhat careful of the IV estimates. Regardless of the instrument, we have issues with weak instruments, given the low F statistic and large standard errors.