Legal Financial Obligations and Reoffense

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Abstract

This paper attempts to recover the causal effects of criminal debt assigned at sentencing (LFOs) on subsequent criminal behavior. We construct an estimator that uses judge stringency as an instrumental variable. Additionally utilizing the uniform criminal sentencing guidelines in North Carolina since 2009, we circumvent part of the "multi-dimensionality" problem that would otherwise make estimation of this this relationship infeasible. Preliminary evidence shows that $100 of additionally assigned debt decreases the probability that an individual will reoffend within 12 months by 0.6%, though this estimate likely masks significant heterogeneity. This is in contrast to OLS estimates, which predict a (zero or) positive relationship between assigned debt and subsequent reoffense. We explore mechanisms that may explain this difference.

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*Thanks for reading this. I'm sure it can be greatly improved.

1   Introduction

In the United States, 57% of ex-prisoners are rearrested within one year of release, and 77% are rearrested within five years (Durose, Cooper, and Snyder 2014, Alper and Durose 2018). Previous research has established that financial obligations are a primary motivation for ex-prisoners to commit crimes (Harris, Evans, and Beckett 2010). The criminal justice system imposes two penalties which may exacerbate the problem. First, this population is burdened by financial penalties from the court through legal financial obligations (LFOs), or the punitive fines and fees imposed during criminal sentencing. Second, the population is incarcerated for several years, during which they accrue liability but near zero assets. Previous literature has established that these penalties negatively affect the formerly incarcerated. Harris, Huebner, et al. 2017 and Martin et al. 2018 provide some evidence that financial penalties increase the likelihood of incarceration, though both lack the identification necessary for a causal interpretation.

The question of whether sentencing decisions affect subsequent criminal behavior has emerged as a central issue in the crime literature. Most of the recent wave began with Kling 2006, which popularized the use of the quasi-random assignment of judges as an instrument for sentence assignment. Using this instrument, Green and Winik 2010 find that longer sentences lead to higher likelihoods of reoffense, but the paper suffers from a small sample which exacerbates problems with the instrument. More recently, papers have taken advantage of large samples in a number of contexts but consensus remains mixed. For instance, Mueller-Smith 2015 finds that an additional year of incarceration *increases* likelihood of offense within 1-year of release by 15%, while Evan K Rose and Shem-Tov 2021 find that it *decreases* it by 22% (though they rely on a different method of identification).

Our work looks at a different margin altogether: LFOs. The margin of criminal sentencing most similar to LFOs is likely pretrial detention, which is found to have negligible effects on subsequent offenses but large negative effects on future labor market outcomes (Dobbie, Goldin, and Crystal S. Yang 2018). The distinction between bail and LFOs, however, is important to make. Bail decisions primarily impact individuals through detainment rather than through the direct cost of bail. This makes sense since the vast majority of bonds are

posted against assets promised to the court rather than through direct payment, and an individual only forfeits their posted assets if they fail to appear for sentencing. LFOs, on the other hand, are debts promised to the court and are rarely remitted ex-post. More to LFOs, the only paper (to our knowledge) that attempts to identify the effects of court-appointed debt on subsequent behavior is Mello 2018. One of the primary differences in that paper is the focus on fines, which we argue in Section 2.1 are substantially different from fees. In addition, he only looks at the effects on future credit and labor market outcomes.[1]

This paper draws on a particular aspect of the North Carolina criminal justice system—structured sentencing laws—to identify the effect of LFOs on subsequent criminal behavior. Evan K Rose and Shem-Tov 2021, among other recent papers, have utilized these sentencing laws to recover the effects of sentencing on various outcomes. We use this structure to causally identify the effects of court-assigned debt on recidivism. We find that $100 of additionally assigned debt *decreases* the probability that an individual will reoffend within 12 months by XXX. Klaassen 2022 finds a similar result when restricting to a vary narrow subset of the light-misdemeanor population. A major contribution of our paper is estimating this effect on the full population through the utilization of the structured sentencing laws.

The remainder of the paper proceeds as follows. Section 2 provides background information on the North Carolina Court System and describes how our instrument would operate in this setting. Section 3 summarizes our data and main findings. Section 4 discusses possible mechanisms underlying the main findings. Section 5 concludes.

2 Research Design

Here, we describe our research design. We begin by reviewing the court system in North Carolina, drawing attention to the structured sentencing laws and random assignment of criminal court cases to judges. We then describe how we use the combination of this randomization and structured sentencing in order to recover the effects of LFOs on subsequent reoffense.

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[1]Other papers, such as Dobbie and Song 2020, also focus on the impacts of debt relief. The outcomes in those papers, however, are not tied to future criminal involvement.

2.1 The North Carolina Court System

The court system in North Carolina is comprised of three Divisions: Appellate, Superior, and District. The Appellate Division is composed of the Supreme Court and the Court of Appeals, neither of which hears criminal cases. Courts in the Superior Court Division hear primarily felony cases. Courts in the District Court Division hear the remainder of criminal court cases, as well as civil and juvenile cases. The remainder of the paper deals with criminal cases heard at either the District- or Superior-level. Importantly, infractions are not considered.

North Carolina introduced mandatory sentencing guidelines in 1994, as part of the North Carolina Structured Sentencing Act (SSA). This established misdemeanor and felony sentencing "grids" that determine sentence range as a function of the offense class and criminal history of the offender. Felony offenses are grouped into 10 classes based on the severity of the offense. Misdemeanors are similarly grouped into 4 classes. Each offender has a corresponding prior record score based on the severity of their past convictions. 0-1 points are assigned for misdemeanors and 2-10 points are assigned for felonies, dependent on the severity within each class. Offenders are grouped into six different classes based on their prior record points. Sentences are then set for each offense class by prior record class combination. Hereafter, these unique combinations are referred to as "cells". Each cell is assigned allowable sentence types within the corresponding sentencing range: active punishment, intermediate punishment (probation with special conditions), or probation. Figure 1 shows a subset of these guidelines.

Within the court system, there are three broad categories of legal financial obligations (LFOs) that can be assessed at sentencing: restitution, fines, and fees. Restitution is essentially compensation for damages, both financial and emotional, to victims. Restitution is typically extremely case-specific and has the widest variance of the categories of LFOs.[2] Fines are penalties that are levied after a criminal conviction or admission of guilt. These are typically thought of as the "punishment" specific to a crime and are mostly set by statute. Finally, fees are ways for jurisdictions to recoup costs of the "use" of the criminal system.

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[2]Restitution might include such things as recompense for breaking a window when robbing a store, repaying the money one stole, etc. The vast majority of criminal cases have no restitution assessed.

Fees are attached to things like public defenders, probation supervision, base court costs, etc. The assignment (or relief) of fees is where the majority of the between-judge and between-district variance exists. Additionally, most fees can be waived at the discretion of a judge, while fines and restitution cannot be waived (but may be remitted ex-post).[3]

2.2 Case Assignment

For simplicity, the majority of felony cases are heard in Superior Courts, while the majority of misdemeanor cases are heard in District Courts. While the case assignment process differs slightly in Superior and District Courts, both adhere to random assignment of cases to judges within the individual court and both are subject to the same set of statutes regarding LFO assignment. As such, we aggregate both types of cases in our analysis.

The Superior Court system in the state is divided into 100 counties and eight divisions. Each county belongs to only one division. Judges that are part of the Superior Court system rotate from one district (a subset of a county) to another in their division every 6 months; this is referred to as "riding the circuit" and is constitutionally mandated in North Carolina. Superior Court judges serve full-time and are prohibited from practicing law privately. Additionally, at least two judges must sit in the superior court in each district at a given time.

Cases heard by judges in the District Court, on the other hand, operate slightly differently. Judges in this system still operate within a single district. District Court judges, like Superior Court judges, serve full-time and are prohibited from practicing law privately. Judges in this system are assigned to sessions of court, prescribing the times at which magistrates will discharge their duties. District court judges also do not "ride the circuit". In that sense, District Court judges can only be randomly assigned cases within their prescribed session, if such randomness exists. Finally, as was the case with Superior Court judges, at least two judges must sit in the District Court in each district at a given time. Since mandate and

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[3]The distinction between remitting and waiving debt is important in the timing of the forgiveness. A waiver is determined at the time that a sentence is handed down and is essentially removing the debt before it's ever thought to be repaid. Remittance can only be determined after the sentence is handed down. For instance, if an offender is assessed a $10,000 fine, that fine cannot be waived at the time of sentencing. If she has failed to repay some time after release, it can be partially or fully remitted. In North Carolina, this requires proving that you cannot reasonably repay the debt in full.

practice are occasionally misaligned, we verify this basic prerequisite for our instrument in Figure 2. Random assignment of judges to cases within the aforementioned windows should follow from the assignment mechanism in both court systems. This is tested in Section 3.3.

2.3 The Judge IV and Simultaneous Decisions

We are interested in recovering the causal effects of debt assignment (LFOs) on subsequent reoffense. Consider the estimation of the simple model:

$$Y_{it} = \beta_t LFO_{i0} + X_i'\theta_t + \eta_{it}$$

where β_t is the parameter of interest, LFO_{i0} is the legal financial obligation assigned to defendant i at the time of sentencing, X_i is a vector of controls, and Y_{it} is the dependent variable of interest t periods after i's sentencing (for instance, cumulative subsequent offenses). Here, we are immediately confronted by concerns of selection bias in the OLS estimation of β_t, and thus we turn to the judge instrument, popularized by Kling 2006 and used in a number of papers and contexts since then (Dahl, Løken, and Mogstad 2014, Arnold, Dobbie, and Crystal S Yang 2018, Sampat and Williams 2019, etc.)[4]

One problem that we face when constructing this instrument is the fact that sentencing decisions are multi-dimensional.[5] At the forefront of the criminal justice process is the incarceration sentence and/or probation sentence—a set of decisions that is typically made jointly with the LFO assignment. In the standard approach, 2SLS estimation of β_t would dictate a first stage specified as:

$$LFO_{i0} = \gamma Z_{j(i)} + X_i'\delta + v_{i0}$$

where $Z_{j(i)}$ is the stringency of i's assigned judge (measured using leave-out means). Under

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[4]Note that this is not unique to criminal cases and should likely just be called an "evaluator IV", or similar. The papers cited use this instrument by taking advantage of random assignment of evaluators or judges in disability insurance determinations, pretrial decisions, and patent applications.

[5]This problem is made apparent in recent work such as Bhuller et al. 2020. Though our description of the problem is similar, our solution is essentially accepting it's a problem and side-stepping it through the use of the sentencing guidelines, rather than testing whether it affects results. Another approach to resolving the problem of multiple dimensions in sentencing is found in Autor et al. 2017.

exogeneity and monotonicity, the 2SLS estimand is the weighted average of the causal effect of a \$1 increase in LFOs among the group of defendants who could have received a different LFO assignment had they been assigned a different judge. For simplicity, assume now that there are only 2 decisions that the judge is making simultaneously: (1) what LFO to assign and (2) whether to incarcerate. Further assume that the LFO assignment is in $\{0, 1\}$ and so it's basically just a decision of whether to assign anything at all. Then, our 2SLS setup can be summarized by 3 equations:

$$I_{i0}^{LFO} = \zeta Z_{j(i)}^{Inc} + \lambda Z_{j(i)}^{LFO} + X_i'\psi + u_{i0} \tag{1}$$

$$I_{i0}^{Inc} = \alpha Z_{j(i)}^{Inc} + \gamma Z_{j(i)}^{LFO} + X_i'\delta + v_{i0} \tag{2}$$

$$Y_{it} = \theta_t I_{i0}^{Inc} + \beta_t I_{i0}^{LFO} + X_i'\omega_t + \eta_{it} \tag{3}$$

where I_{i0}^{LFO} is an indicator for whether i is assigned an LFO, I_{i0}^{Inc} is an indicator for whether i is sentenced to prison, $Z_{j(i)}^{LFO}$ is the stringency instrument for LFO assignment, and $Z_{j(i)}^{Inc}$ is the stringency instrument for the incarceration decision. As before, we would measure $Z_{j(i)}^{LFO}$ and $Z_{j(i)}^{Inc}$ using leave-out means on LFO assignment and incarceration, respectively. The first potential violation of the exclusion restriction in the baseline IV model would be if $Z_{j(i)}^{LFO}$ is correlated with $Z_{j(i)}^{Inc}$ and $Z_{j(i)}^{Inc}$ directly affects Y_{it} (conditional on our set of controls X_i). Controlling for $Z_{j(i)}^{Inc}$ in the baseline IV model would resolve this problem. The second potential violation is typically more problematic. Consider the case where $Z_{j(i)}^{LFO}$ is correlated with I_{i0}^{Inc} conditional on $Z_{j(i)}^{Inc}$, and I_{i0}^{Inc} affects Y_{it} holding I_{i0}^{LFO} fixed. In this case, the exclusion restriction would be violated because $Z_{j(i)}^{LFO}$ affects Y_{it} through I_{i0}^{LFO} *and* through its relationship with I_{i0}^{Inc}. In words, if LFO stringency correlates with the incarceration decision (conditional on their incarceration stringency), and the incarceration decision affects subsequent criminal behavior (holding the LFO decision fixed), then LFO stringency affects subsequent criminal behavior through the LFO decision and through its relationship with the incarceration decision.

As noted in , there exist mandatory sentencing guidelines in North Carolina that have remained fixed since 2009. So, consider the same setup as above with one significant

change: an individual's position on the sentencing grid (i.e. their cell) entirely determines their sentence (and whether or not they're incarcerated).[6] Then, it cannot be the case that the LFO stringency of the judge assigned affects subsequent criminal behavior through its relationship with the incarceration decision because the incarceration decision is determined ex-ante (conditional on guilt). Maintaining notation above, this means that both α and ζ go to 0 since there is no first stage. It also means that γ will go to 0 because there is no relationship between the incarceration outcome and the LFO lenience of the judge assigned. In essence, we have returned to our simple 2SLS setup as long as we also control for the offender's cell position. We provide an alternative approach with multiple instruments in Section XXX that does not strictly rely on the sentencing grid or a zero first stage for other margins.

Now, we briefly describe the construction of the instrument $Z_{j(i)}^{LFO}$. For any individual assigned to a single judge, let:

$$Z_{j(i)}^{LFO} = \frac{1}{n_{j(i)} - 1} \sum_{k \neq i} \mathbb{1}\{j(k) = j(i)\} LFO_k \tag{4}$$

where $j(i)$ is i's assigned judge and LFO_i is i's assessed LFO. This is the simple leave-out mean construction that we hinted to above. We model LFOs as a joint decision between waivers and assignment. Essentially, an LFO is assigned as the culmination of fines, restitution, and fees in a case. Part or all of the fee portion of the LFO can also be waived, as discussed in Section 2.1. In addition, because fines are typically determined by statute and restitution is extremely crime-specific, we define LFO_i as the total fees that individual i is deemed to be responsible for (i.e. initial fee assessment less those waived).[7]

Some recent work has estimated similar parameters without relying on judge instruments.[8] This is particularly appealing if we think that judges are systematically assigned to specific cases (or types of types of cases), or if the assumption of random assignment is violated in some other way. We make our case for judge random assignment in our sample

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[6]We verify this is true in Figure Figure 4 and discuss briefly in Section 3.3.

[7]In Appendix XX, we replicate our analysis using the judge's decision to impose *any* fee. The results using this extensive margin decision are qualitatively similar to our main findings.

[8]See Evan K Rose and Shem-Tov 2021 and Evan K. Rose 2020 for North Carolina, in particular. Their discussion of the institutional setting may prove useful.

in Section 3.3

3   Data and Findings

We use administrative information on criminal court cases provided by the North Carolina Administrative Office of the Courts (AOC) covering 2013 to 2019, inclusive. The data includes information on defendants, offenses, convictions, and sentences for all cases disposed in either the superior court or district court system. Construction of a number of variables, including indicators for reoffense, proxies for sentence and probation assigned, etc. are described in detail in Appendix A.

3.1   Sample Selection

Data are unique at the record-level and there are potentially multiple records within each case.[9] At least one offense is associated with each case, but offenders are frequently charged with multiple offenses (each of which belongs to the same case). If this is the case, we keep the most severe offense since that, in combination with their prior record score, will uniquely identify their position on the sentencing grid.

Since we are interested in using judge discretion, we limit the cases considered to those with sitting judges.[10] Of the $\sim$ 7.8 million cases in our data, 2.75 million have a judge sit. Since construction of our instrument further relies on a single evaluator, we drop any case that is heard by more than one judge.[11] Finally, we drop cases where prior record points are not identified. This leaves us with the primary sample that we use in our analysis. The list of sample restrictions is shown in Table C.1. [12]

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[9]Examples of records are offenses, judgments, criminal court information system (CCIS) entries, etc.

[10]Judges are identified by a unique 3-character code in the data provided by the AOC.

[11]This can occur, for instance, if charges are levied at drastically different times in the superior court, where "rotation" may occur. In this example, the original sitting judge recorded may not be available. This event is relatively uncommon.

[12]Exceptions are noted as we proceed; for instance, part of the analysis requires that the individual have the opportunity to reoffend in the future, which is explained in Section 3.3.

3.2 Summary Statistics

Summary statistics are shown in Table 1. Note that the second panel is the primary sample (before IV restrictions) constructed above and so we focus discussion primarily on that. Defendants are predominantly male and disproportionately black when compared to the population of North Carolina, as is typical in other settings in the United States. Around 50% of the cases that we look at have some sentence component and 25% have a probation component. 45% of the sample has a criminal history score of 0. Note that this does *not* mean that 45% of the sample has no criminal history, since some offenses result in no score accumulating.

Turning to the LFOs, we note that the average fees are around \$350 and the average LFO is around \$850. Restitution has a long right-tail, as can be seen by the fact that even the 90th percentile of restitution is \$0. Previous papers, such as Dobbie, Goldin, and Crystal S. Yang 2018, focus on the burden of bonds, which might affect offenders similarly. In our sample, pretrial detention is ordered in 45% of cases and the average bond is around \$3850 with a very long right-tail. As discussed in Section 1, the potential channel through which bonds affect reoffense is different than LFOs since the channel there seems to primarily be through pretrial detention (and not through future accumulation of debt). Additionally, we note that bail is posted the majority of the time, but is rarely paid in cash (4%). Instead, bonds tend to be secured against assets promised to the court in the case of pretrial misconduct. In this way, one should not think of the financial implications of large bond assessments as dwarfing the comparatively smaller LFOs, since (re)payment obligations are quite different. Variance in LFO assignment across county and judge is shown in a series of figures in Appendix C.

3.3 Testing the Relevance and Validity of the Instrument

Following the procedure for the construction of our instrument, we turn to two important tests: (1) the relevance, and (2) the validity of our instrument. The sample used in the construction of the instrument is our primary sample described above, but with two caveats: we only include judges who see at least 50 cases and we exclude cases where the fee responsible exceeds \$2000 (less than 1% of the sample). We provide robustness checks for both of these

restrictions in Appendix XXX. After these restrictions, we are left with XXX judges. Over the 5-year period that we observe, the median number of cases per judge is XXX.[13]

Figure 3 and Table 2 summarize first stage results (a regression of the fees assessed on the leave-out mean assignment of the judge to which the defendant was assigned). Panel A in Table 2 includes fully interacted court, year and cell dummies, while panel B also includes all controls from Table 3. The first column reports the estimate of the first stage on the full estimation sample described above, while the other columns restrict to whether that individual is released within some horizon of our sample. For instance, column 3 drops observations where the individual is incarcerated until within 12 months of the end of our sample (and so would have less than 1-year to reoffend). A simple interpretation of these first-stage results is that being assigned a judge that assesses $1 higher fees on average across all other cases she oversees would result in receiving approximately $XXX higher fee assessment in expectation. The variance in judge stringency can be visualized Figure 3.

Table 3 reports results for a test of random assignment. Column 1 reports results from a regression of fees assigned on a number of variables that are measured before the decision of the court. It is important to note that, due to our design, we cannot include covariates that completely determine a defendant's position on the structured sentencing grid. Instead, we include covariates that are determined in pretrial hearings or in prior trials.[14] We proxy for individual income with an indicator for whether the defendant is determined to be indigent. Column 3 tests whether our judge stringency measure can be predicted by the same set of covariates. This is similar to a test of random assignment seen in randomized controlled trials. The variables are not jointly significant and none are individually significant at the 5% level, which we take as evidence that court cases are randomly assigned to judges conditional on court-by-year-by-cell fixed effects.

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[13]Figure C.5 shows the full distribution of cases across the judges remaining in this estimation sample. The long right-tail concerns us slightly, as that may hint towards non-random selection into cases or non-random assignment. However, this setting, we only require that the populations overseen by each judge are the same, which we test in XXX. Additionally, a regression of judge lenience on number of cases overseen shows no relationship.

[14]For instance, "Any Prior Conviction" is an indicator for whether the defendant has a prior conviction. This only partially determines their position on the grid, in the sense that it is positively correlated with prior sentencing points. It does not, however, guarantee any prior sentencing points, let alone a specific class of points accumulated.

As discussed in Section 2.3, circumventing the multi-dimensionality problem requires that the sentencing guidelines are "strict". In order to use the instrument from Equation (4) in a basic 2SLS setup, we require that an individual's position on the grid entirely determines their sentence (conditional on guilt). Figure 4 provides an informal test of this assumption. Essentially, we are testing whether the sentencing length handed down to other individuals assigned to a defendant's judge partially determine her own sentence length. We find a non-zero, but very weak, first-stage here, which we take as evidence that our assumption is reasonable. Appendix XXX provides alternative results that do not necessitate a zero first-stage for other sentencing margins.

The final condition that we need satisfied is monotonicity of the instrument. In this context, that requires that individuals that receive some LFO by a lenient judge would need to receive a weakly higher LFO by a stringent judge. One implication of this assumption is that first stage estimates should be non-negative for any subsample. We confirm this is the case in Table 4. Another implication is that judges should be stricter for specific case types if they are stricter in other case types. Following Bhuller et al. 2020, we test this by redefining the instrument for each subsample to be the judge's LFO assignment for cases outside of that subsample. Results are shown in the second column of Table 4 and suggest that the monotonicity assumption holds.

3.4   Effects of Debt Assignment on Subsequent Reoffense

Table 5 contains our main results. We find that, on average, increasing the LFO of an offender by $100 leads to a reduction in the probability that individual will reoffend within 12 months by around XXX%. An alternative way of looking at this is that the treatment will reduce the number of offenses over the same horizon by around XXX. These findings are in stark contrast with the OLS estimates which predict a positive relationship between LFO assignment and subsequent reoffense. This could be due to selection bias caused by correlated unobservables. This could also be due to treatment effect heterogeneity.

Though our paper is the one of the first to look explicitly at the role of LFOs in subsequent criminal behavior, there exist a number of previous papers that investigate other mechanisms and margins. Mueller-Smith 2015 finds that an additional year of incarceration *increases*

likelihood of offense within 1-year of release (↑ 15%), while Evan K Rose and Shem-Tov 2021 find that is *decreases* it (↓ 22%). When looking at the extensive margin, Bhuller et al. 2020 find that incarceration itself reduces likelihood of reoffense within 1-year of release (↓ 24%). Finally, Dobbie, Goldin, and Crystal S. Yang 2018 find that pretrial detention has no meaningful effect on subsequent offenses, but does have a large negative effect on future employment (↓ 4%) and earnings (↓ 11%). Our results are in line with the more recent literature (finding a negative relationship) if one simply maps incarceration and LFOs to a common "punishment" plane.

4   Discussion

Our results suggest that being assigned court debt at reduces the probability of recidivism for defendants once released from incarceration. There are several possible explanations for this effect, which we briefly discuss here.

First, the results are consistent with belief updating, where individuals who are assigned more court debt update to view the costs of committing future crime as more costly. Likewise those individuals who received less punitive debt assignments could update to see crime as less costly than they initially believed. While we cannot directly test this in the data, Klaassen 2022 tests for this by comparing the effect on recidivism separately for first-time offenders and repeat offenders. He finds that first-time offenders have a larger response to LFO assignment, which he interprets as evidence that the first-time offenders had more inaccurate beliefs. Figure XXX shows that this result holds more broadly in our data, but we caution against attributing this to the belief-updating channel for a few reasons. First, there is no reason to believe that first-time offenders systematically underestimate LFOs, and thus receiving a smaller than expected LFO would actually reduce the deterrence effect. Similarly, first-time offenders could have their beliefs update to be more inaccurate if their LFO realization if they encounter a particularly strict or lenient judge. Second, due to the structure of the sentencing grid in North Carolina, this result could simply be a validation of the design. Mechanically, first-time offenders are assigned to less-strict sentencing ranges (i.e., columns farther left on the sentencing grid; see 1). As such, a weaker response in

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offenders belonging to "higher" cells could be an artifact of confounding variables that are also at the discretion of the judge (i.e. probation vs. prison and sentencing length).

Additionally, there are hypothesized legal channels through which those assigned more court debt could experience less recidivism. While we cannot verify their prevalence in our data, we will briefly mention them here. First, in North Carolina, one of the conditions of ending probation is the repayment of court debt (Hunt and Nichol Jr 2017). Thus individuals who have outstanding court debt are in touch with their probation officer for longer durations than those without court debt, as their probation lengths are mechanically longer. Previous literature has shown that contact with the probation officer reduces recidivism (Lerman et al. 2022), which could also be a mechanism behind our results. Complicating this explanation is the fact that judges do have the authority, and are by some statues encouraged, to switch individuals to unsupervised probation who would otherwise exit probation but have outstanding court debt (Markham 2018). As such, it is not clear how often our sample population experiences this extended contact with a probation officer as a result of unpaid LFOs.

5   Conclusion

The question of whether, and to what extent, court-appointed debt affects future outcomes is critical. Our paper is a first pass at answering this question and suggests that LFOs can serve as a deterrent, but several more nuanced questions arise. Is the optimal LFO crime-specific? Individual-specific? How does it affect credit? Labor market outcomes? We capture a very small part of the picture and more work is necessary before any paper can be submitted (or policy recommendation can be given).

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Table 1: Summary Statistics

	Judge-Restricted Sample					Primary Sample				
	Mean	SD	P50	P10	P90	Mean	SD	P50	P10	P90
Demographics										
Age at trial	34.5	12.4	31.6	20.7	52.7	34.4	11.8	31.9	21.1	51.9
Male	0.68					0.73				
White	0.50					0.48				
Black	0.37					0.41				
Hispanic	0.08					0.07				
LFOs										
Fees ($)	252	467	210	0	470	345	677	216	0	626
Fines ($)	112	3,328	0	0	200	173	4,641	0	0	200
Restitution ($)	126	5,545	0	0	0	330	9,063	0	0	0
Total LFO ($)	489	6,529	250	0	644	848	10,264	266	0	949
Bond Assigned	0.32					0.45				
Bond Amount ($)	2,367	10,738	0	0	3,000	3,852	13,698	0	0	7,500
Bond Secured (given bond)	0.79					0.82				
Bond Paid (given bond)	0.04					0.04				
Sentence										
Sentence Length (D)	56	318	0	0	60	125	496	10	0	345
Probation Length (D)	82	215	0	0	360	161	292	0	0	540
Criminal History Score						2.38	3.80	1.00	0.00	5.00
Obs.	2,489,665					929,312				
Individuals	1,479,905					605,973				

NOTES: This table provides summary statistics for a number of variables related to demographics, debt, and sentence assignment. There are two primary samples shown here. The first is the "Judge-Restricted Sample" which restricts cases to those that are overseen by precisely one judge. This primarily excludes cases that include no judge, but also excludes cases that are overseen by multiple judges. Multiple judges can be assigned to a case for any number of reasons: length of trial, nature of charges, etc. We restrict to cases with one judge for reasons related to the estimator discussed in Section 2.3. The "Primary Sample" *additionally* requires that the case include information on Criminal History Score. This does not seem to substantially change the demographic breakdown, but does affect the nature of the crimes and subsequent LFOs that we analyze. Variables are described in detail in Appendix A.

Table 2: First Stage: Fees Assessed on Judge Stringency

	Time available to reoffend:			
	(1)	(2)	(3)	(4)
Estimation Sample:	Full	100 Days at most	12 Months at most	24 Months at most
Dependent Variable:		Fees Assessed		
Panel A: Court x Year x Cell FE				
	0.709***	0.721***	0.707***	0.722***
	(0.025)	(0.023)	(0.026)	(0.025)
Panel B: Add Controls				
	0.709***	0.704***	0.708***	0.723***
	(0.025)	(0.025)	(0.026)	(0.025)
Number of Cases	664,671	576,953	491,415	373,520

NOTES: This shows first stage results following the instrument construction in Section 3.3. Panel A includes fully interacted court, year, and cell dummies. Panel B adds controls for demographics and other variables that do not completely determine the offense class or prior record class of the defendant. The first column reports the estimate of the first stage on the full estimation sample, while the other columns restrict to whether that individual is released within some horizon of our sample. For instance, column 2 would drop observations where the individual is incarcerated until within 100 days of the end our of sample (and so has at most 99 days to reoffend and reappear in the dataset). Standard errors are two-way clustered at the judge and defendant level and are shown in parentheses ($^*p < .1$, $^{**}p < .05$, $^{***}p < .01$).

Table 3: A Test of Random Assignment

	Dependent Variables:			
	Fees Assigned		Judge Stringency	
	(1)	(2)	(3)	(4)
Explanatory Variable:	Coefficient	Standard Error	Coefficient	Standard Error
Demographics				
Age at trial	-0.444***	(0.032)	0.014	(0.016)
Male	-11.027***	(0.949)	-0.129	(0.147)
White	-0.142	(0.902)	0.207	(0.165)
Indigent	-7.280***	(2.497)	-0.585	(0.400)
Other Case Info.				
In Jail	-66.014***	(21.07)	9.398*	(5.269)
Any Prior Conviction	2.378	(1.870)	-0.678	(0.493)
Bond Set	-9.334***	(3.170)	-0.471	(0.457)
Bond Paid in Cash	53.916***	(5.009)	-0.116	(0.058)
Pretrial Days Served	-0.674***	(0.054)	0.003	(0.008)
F-Stat	65.69		0.59	
[P-Value]	[0.000]		[0.210]	
Number of Cases	491,415		491,415	

NOTES: This shows our test of random assignment of cases to judges as described in Section 3.3. All estimations include controls for court x year x cell fixed effects. The sample used is the same as in column (3) of Table 2. Standard errors are two-way clustered at the judge and defendant level and are shown in parentheses ($^*p < .1$, $^{**}p < .05$, $^{***}p < .01$).

Table 4: A Test of Monotonicity

	Main Instrument First Stage	Reverse-Sample Instrument First Stage
Demographics		
Subsample: Age ≥ 35		
Coefficient	0.707***	0.509***
Standard Error	(0.028)	(0.031)
Subsample: Age < 35		
Coefficient	0.696***	0.509***
Standard Error	(0.030)	(0.033)
Subsample: Male		
Coefficient	0.682***	0.503***
Standard Error	(0.026)	(0.039)
Subsample: Female		
Coefficient	0.759***	0.447***
Standard Error	(0.033)	(0.033)
Subsample: White		
Coefficient	0.729***	0.463***
Standard Error	(0.031)	(0.031)
Subsample: Black		
Coefficient	0.729***	0.510***
Standard Error	(0.034)	(0.050)
Subsample: Hispanic		
Coefficient	0.345***	0.314***
Standard Error	(0.034)	(0.072)
Subsample: Indigent		
Coefficient	0.779***	0.471***
Standard Error	(0.031)	(0.069)
Subsample: Not Indigent		
Coefficient	0.451***	0.235***
Standard Error	(0.032)	(0.028)
Other Case Info.		
Subsample: Prior Conviction		
Coefficient	0.788***	0.488***
Standard Error	(0.030)	(0.041)
Subsample: No Prior Conviction		
Coefficient	0.587***	0.326***
Standard Error	(0.031)	(0.031)
Subsample: Bond Set		
Coefficient	0.782***	0.359***
Standard Error	(0.037)	(0.042)
Subsample: No Bond Set		
Coefficient	0.552***	0.248***
Standard Error	(0.031)	(0.032)

NOTES: This table shows first stage results on a number of subsamples in reference to our test in Section 3.3. The first column uses our main instrument tested on a subsample. For clarity, that would be a regression of LFO assignment on our LFO instrument (judge stringency), including as controls all of the covariates from Table 3 and court x year x cell fixed effects, on the subsample shown. The second column reconstructs the instrument excluding own-type and then performs the same test. For instance, the reverse-sample instrument for subsample age ≥ 35 was reconstructed by taking mean of the LFO assignments to individuals *younger* than 35 of the judge to which each individual is assigned. Since an individual cannot be over and under 35, this naturally results in a leave-out mean instrument since only those under 35 are included in the regression. Standard errors are two-way clustered at the judge and defendant level and are shown in parentheses (*$p < .1$, **$p < .05$, ***$p < .01$).

Table 5: The Effects of $100 Increased LFO on 1-Year Reoffense

	Probability of Reoffense	Subsequent Charges (#)
OLS: LFO ($100)	0.00810**	0.0699*
	(0.00330)	(0.0357)
RF: Judge Stringency	-0.00422**	-0.0532**
	(0.00201)	(0.0226)
IV: LFO ($100)	-0.00596**	-0.0750**
	(0.00289)	(0.0321)
Dependent Mean	0.44251	3.2512
Observations	491,415	491,415

NOTES: This table shows OLS and 2SLS estimates of the effect of $100 increased LFO assignment on 1-year reoffense. Controls include all covariates from Table 3, as well as court x year x cell fixed effects. The precise 2SLS approach is outlined in Section 2.3. Standard errors for OLS estimates are clustered at the defendant level, while standard errors for the 2SLS estimates are two-way clustered at the judge and defendant level. These are shown in parentheses ($^*p < .1$, $^{**}p < .05$, $^{***}p < .01$).

Figure 1: Felony Sentencing Grid

|	I 0-1 Pt	II 2-5 Pts	III 6-9 Pts	IV 10-13 Pts	V 14-17 Pts	VI 18+ Pts		
A	**Death or Life Without Parole** **Defendant Under 18 at Time of Offense: Life With or Without Parole**						**DISPOSITION**	
B1	A *240 - 300* **192 - 240** *144 - 192*	A *276 - 345* **221 - 276** *166 - 221*	A *317 -397* **254 - 317** *190 - 254*	A *365 - 456* **292 - 365** *219 - 292*	A *Life Without Parole* **336 - 420** *252 - 336*	A *Life Without Parole* **386 - 483** *290 - 386*	*Aggravated Range* **PRESUMPTIVE RANGE** *Mitigated Range*	
B2	A *157 - 196* **125 - 157** *94 - 125*	A *180 - 225* **144 - 180** *108 - 144*	A *207 - 258* **165 - 207** *124 - 165*	A *238 - 297* **190 - 238** *143 - 190*	A *273 - 342* **219 - 273** *164 - 219*	A *314 - 393* **251 - 314** *189 - 251*		
C	A *73 – 92* **58 - 73** *44 - 58*	A *83 - 104* **67 - 83** *50 - 67*	A *96 - 120* **77 - 96** *58 - 77*	A *110 - 138* **88 - 110** *66 - 88*	A *127 - 159* **101 - 127** *76 - 101*	A *146 - 182* **117 - 146** *87 - 117*		
D	A *64 - 80* **51 - 64** *38 - 51*	A *73 - 92* **59 - 73** *44 - 59*	A *84 - 105* **67 - 84** *51 - 67*	A *97 - 121* **78 - 97** *58 - 78*	A *111 - 139* **89 - 111** *67 - 89*	A *128 - 160* **103 - 128** *77 - 103*		

NOTES: This figure shows the sentencing guidelines for some felony offenses committed after 11/30/2009. The rows of the grid refer to the offense class, while the columns refer to the prior record points, or criminal history, of the offender. Prior record points are a weighted sum of past convictions based on severity. These points are grouped into six classes, as shown in the headers of each column. Numbers in each cell indicate minimum sentences for each offense class by prior record class combination. These minimums are specified over three ranges: aggravated, presumptive, and mitigated, as noted in the disposition key to the right. The maximum sentence is approximately 120% of the minimum sentence, which gives the range shown. "A" indicates active incarceration, which is the only option for each of these cells (due to the serious nature of these offenses). See the full grid for both felonies and misdemeanors in Appendix C for more detail.

Figure 2: Judges Available in Each Court

Superior Courts   **District Courts**
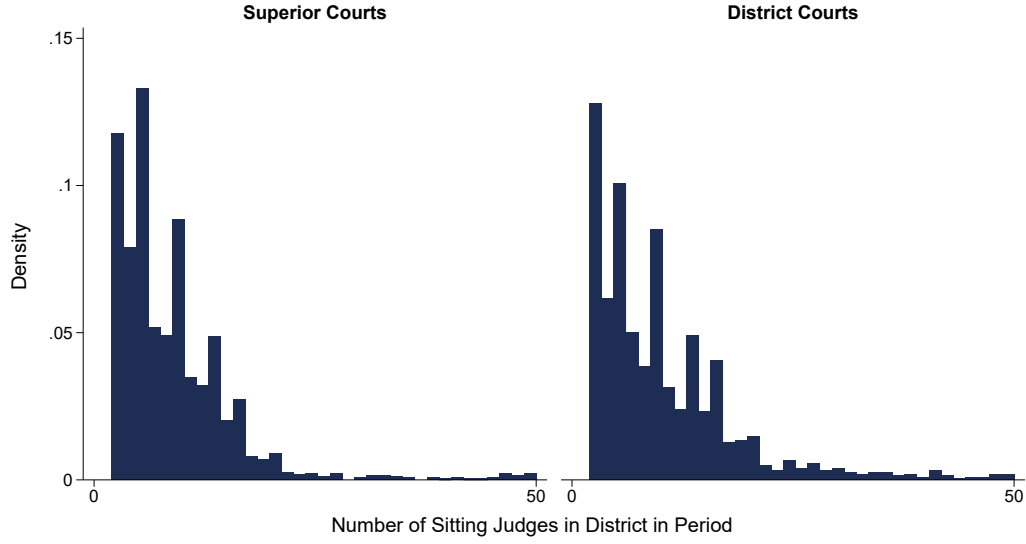
NOTES: These figures show the number of judges sitting in a district in a given time period, either in the Superior Court System or District Court System. The distinction between these courts is outlined in Section 2.1. A simple, necessary condition for the instrument to operate as intended is that there exists variance in case assignment. If there were 1 judge in each court, the variance in debt assignment could not arise from random judge assignment since there would be no randomness (within a district x time). Note that this is not a test of the exclusion restriction; i.e. this says nothing about the random assignment of judges to cases.

Figure 3: First Stage: Fees Assessed on Judge LFO Stringency
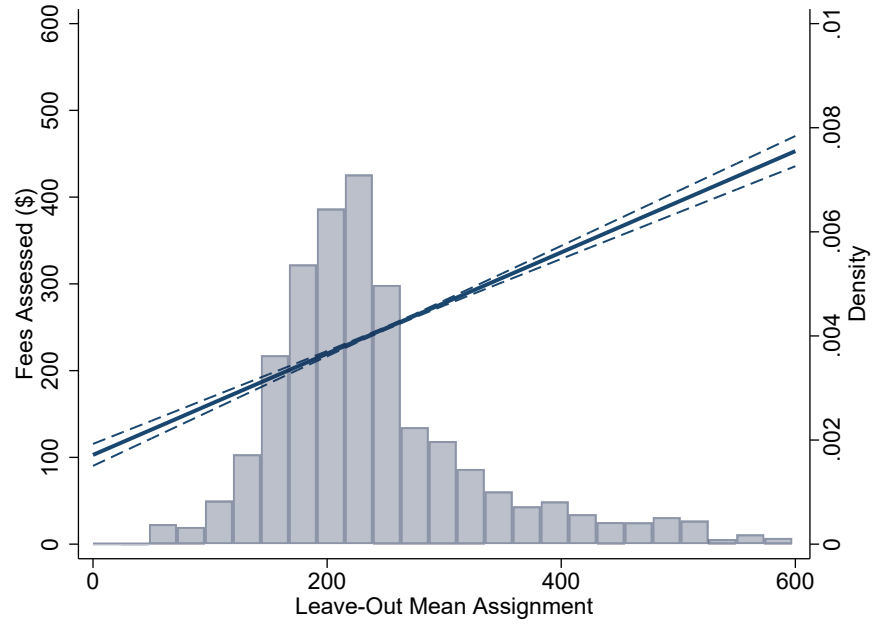
NOTES: This figure summarizes first stage results following the instrument construction described in Section 3.3. Fees assessed is plotted on the left y-axis against the leave-out mean stringency of the assigned judge. The solid line is the result of a local linear regression of fee assignment on the instrument, all of the controls from Table 3, and court x year x offense class x prior record class fixed effects. Dashed lines show 95% confidence intervals. A histogram showing the density of judge stringency is overlaid.

Figure 4: First Stage: Sentence Length on Judge Sentence Stringency
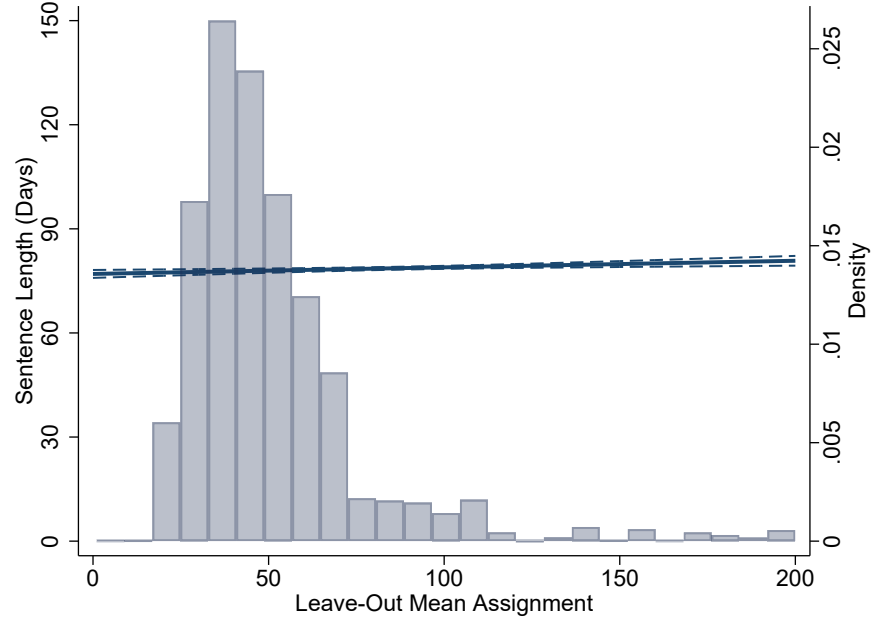
NOTES: This figure summarizes first stage results following the instrument construction described in Section 3.3, but for sentence length. Sentence length (in days) is plotted on the left y-axis against the leave-out mean stringency of the assigned judge. The solid line is the result of a local linear regression of sentence length assigned on the instrument (for sentence length), all of the controls from Table 3, and court x year x offense class x prior record class fixed effects. Dashed lines show 95% confidence intervals. A histogram showing the density of judge stringency is overlaid.

A   Data Appendix

This section briefly describes of a number of variables used in the analysis, as well as their construction if necessary. This is an attempt to make clear the construction or definition of any variable referenced.

1. **Fees:** Each record can have at most one fee entry associated with it. These commonly combine multiple fees, but are entered singly due to the structure of the AOC data. We take the sum of the fees that appear across the records in a case to be the fees in a case. These are also referred to as initial fees, or similar, in the text. They are essentially the fee total before any waiver determination is applied. *None of the analysis uses this variable. Any reference to fees in our analysis uses the LFO definition described below.*

2. **Fines:** Construction is analogous to fees.

3. **Restitution:** Construction is analogous to fees. It's important to note that restitution corresponds to the offense and so it will only initially appear in the offense entries (and not in CCIS entries, judgments, or otherwise).

4. **Fees Waived:** Fees have indicators for whether or not they were waived. This is unique at the entry-level and so the total fees that a defendant is responsible for can be partially or fully waived.

5. **LFO:** Also referred to as LFOs, fees assessed, or fee responsible in the text. As discussed at the end of Section 2.3, this is equal to fees less fees waived. This is tantamount to the total debt the defendant is responsible for at the time of sentencing (less fines and restitution, for reasons discussed in the text).

6. **Bond Set:** Indicator for whether pretrial detention was ordered (and so bail was set). Note that the decision to detain a defendant before trial (virtually) always set by a judge, or magistrate, other than the judge that oversees sentencing for the charges.

7. **Sentence Length:** Sentence ranges are given for all entries corresponding to the sentencing grids in Figure C.1 and Figure C.2. In most cases, only a minimum exists or the minimum is equal to the maximum. For any entry where the verdict is "guilty" or "responsible", we take the sentence as given. If a verdict of "not guilty" is entered, no sentence is assigned (but also no fee is given, so the case is null in our analysis). If a verdict of "responsible to lesser" or similar is handed down, we take the lesser of the sentence ranges among the charges levied against the defendant. Otherwise, if multiple charges exist, the maximum of the potential sentence minimum sentences across the

charges is taken. If any other uncertainty exists about the range, we take the mean of the range of the most serious offense (again, assuming the verdict for that offense is not "responsible to a lesser", or similar). Sentence length is measured in days throughout the paper.

8. **Criminal History Score:** The prior record points are entered at the charge-level. In the vast majority of cases, that means that every charge entry within a case has the same prior record points entry. In cases where they don't, we take the maximum recorded points within the case.

9. **Indigent:** Indicator for whether an defendant's counsel is flagged as either a public defender or court-appointed attorney. Recent work, such as Shem-Tov 2020, make clear that the two types of counsel are not necessarily the same. The intent from our perspective, however, is to proxy for poverty, and indigence seems a reasonable proxy.

10. **In Jail:** Simple indicator for whether the defendant is being held in jail at the time of sentencing.

11. **Any Prior Conviction:** Simple indicator for existence of a prior conviction. This is not limited to the data that we see, since it's not constructed by us. For instance, if an individual was convicted of a crime in 2001, we wouldn't be able to see that but this indicator should still take value 1 (unless it was expunged ex-post).

12. **Pretrial Days Served:** Number of days served in jail (due to pretrial detention) prior to sentencing.

13. **Reoffense:** Indicator for being charged again within t days of either release from prison or, if no incarceration was assigned, the date of sentencing. The only analysis shown here is on 1-year reoffense probabilities, so the variable simply sets t to 365.

14. **Subsequent Charges:** Number of charges accumulated within t days of either release from prison or, if no incarceration was assigned, the date of sentencing. Note that a charge is an offense entry, so reconstruction of this variable is equivalent to counting the number of offense entries corresponding to a defendant within some horizon following sentencing (or release).

15. **Judge ID:** Judges are identified by a unique 3-character alphanumeric code in the AOC data.

16. **Defendant ID:** Defendants are (fuzzy) matched on full name, race, sex, and date of birth. Last 4 of SSN is only available for a subset of the data, but the match seems (mostly) fine without it. This can probably be improved.

B Judge Rotations and Multiple Instruments

2.1 Multiple Instrument Approach

Here, we reconsider how judge rotations affect our instrument and how we might augment it to ensure a similar interpretation. Our basic judge instrument is defined as as $Z_{j(i)}^{LFO}$. For any individual assigned to a single judge, let:

$$Z_{j(i)}^{LFO} = \frac{1}{n_{j(i)} - 1} \sum_{k \neq i} \mathbb{1}\{j(k) = j(i)\} LFO_k \tag{5}$$

This then gives the "LFO stringency" of the judge assigned to offender i. However, if randomization occurs for any judge assignment, it can only happen among the pool of judges to select from and so we also need to condition on court-by-time fixed effects. This is where it gets a bit tricky, because judges are set to "rotate" every 6 months. We give examples of set schedules for some superior courts, which rotate every 6 months (roughly January 1st-June 30th and July 1st-December 31st) in Figure C.6.

Let us index the set of judges available to sentence individual i at time t by ct. Then, if we want conditioning on ct in the construction of $Z_{j(i)}$ to do anything, do we also need the schedule that the judges are assigned to also be assigned "randomly?"[15]
So, let's augment the basic IV construction conditioning on ct (the judges available to the defendant when they're sentenced). Take $Z_{j(i)}^{LFO}$ from Equation (5) (the leave-out mean judge stringency) and then regress it on ct dummmies as:

$$Z_{j(i)}^{LFO} = \beta \mathbb{1}\{\text{Court} = c, \ \text{Time} = t\}_{c \in C, t \in T} + e_{j(i)} \tag{6}$$

where $e_{j(i)}$ then serves as our measure of "judge lenience" (which I'll just call $Z_{j(i)}^{LFO}$ again for ease). This is not dissimilar from Dahl et al. (2014) yet and controls for differences across courts or over time in both offenders and the "stringency" of the pool of judges. If we take this, our 2SLS system would look like:

$$LFO_{i0} = \gamma Z_{j(i)}^{LFO} + X_i'\theta + u_{i0} \tag{7}$$

$$Y_{it} = \beta_t LFO_{i0} + X_i'\zeta + v_{it} \tag{8}$$

---

[15]If there are two schedules (call them A and B) within a district and more than one court (call them 1 and 2) within that district, then we're assigning a judge to oversee a court in any given week. If judge j is randomly assigned to schedule A, then as long as offenders are not systematically assigned (based on any observables) to the week that judge is (or isn't) hearing cases, we're fine. This is testable in the standard way.

The goal of the above is consistently estimating β_t, which is the effect of \$1 extra of LFOs assigned because of a more stringent judge. There are two ways that we see to use the SSA grid (as we alluded to in **??**):

1. As a reasonable set of interacted controls that pass through X_i (in all of the regressions). This is *much* more feasible than controlling for offense type (which has hundreds of categories) and more precise than controlling for offense class and number of prior convictions alone (as most other papers do). In this vein, we would have a system with 4 endogenous variables and 4 IVs (partially supressing t):

$$\begin{bmatrix} LFO_{i0} \\ I_{i0}^G \\ I_{i0}^{Inc} \\ Sen_{i0} \end{bmatrix} = Z_{j(i)}^{LFO}\Lambda + Z_{j(i)}^G\Delta + Z_{j(i)}^{Inc}\Gamma + Z_{j(i)}^{Len}\Theta + X_i'\Omega + \vec{u}_{i0} \tag{9}$$

$$Y_{it} = \lambda LFO_{i0} + \delta_t I_{i0}^G + \gamma_t I_{i0}^{Inc} + \theta_t Sen_{i0} + X_i'\omega_t + \eta_{it} \tag{10}$$

2. Or as a means to argue that the above system can be simplified. This presents itself as either an artifact of the NC system or as weak IVs and I'm not sure that the two can be disentangled.

There are a number of issues with the first approach, but we outline what we can (and should do following):

1. $Z_{j(i)}^G$ is likely to be weak by virtue of the (very) high probability of conviction. $Z_{j(i)}^{Inc}$ faces a similar problem (but due to inc/prob margin only being available in $\sim 10\%$ of cases).

2. $Z_{j(i)}^{Sen}$ has already been tested independently and is weak; first stage coefficient around .05. Haven't recovered the effective first stage F-stat (Olea and Pflueger 2013) or really explored further yet. This paper is really directed towards 1 Endog/1 IV problems and I couldn't find much about detecting weak IVs with multiple endog regressors. Have thought about using LASSO, or similar, but penalization only seems useful when #IVs large (and here, we can't really "select").

3. Interpretation of the LATE becomes fucked up if there are heterogeneous treatment effects.

4. Still leaves us with the judge shopping problem.

It's possible that conviction is just a nuisance parameter for us, so we can try to instrument for it and hope it doesn't bleed in. Otherwise, it may be best to just condition on being convicted. As the conviction rate approaches 1, the instrument becomes infinitely weak but then just conditioning on being convicted would be fine (so really either approach should be fine given the reality of the conviction rate). After talking with Chris briefly, he agreed that we want to be able to say, "it's selection, but it's not correlated with the main treatment". In that respect, Lee bounds or similar might be useful. For the sentence/incarceration problem, we should try to model them jointly. That is, "sentence length" is the variable and it's just 0 if they're not incarcerated. In the second stage, sentence length can just be coded as 0 for those not incarcerated.

Judge shopping can be summarized by the density of the cases. For instance, if some people are stalling, we might ask whether there is a change in the density when the next judge comes in. Here, a McCrary density test should summarize the extend of the problem (McCrary 2008). If we don't know if it's random, we can also show which key controls get it to stabilize. For instance, maybe putting in courthouse and time matter, but courthouse linear time effects don't matter.

The second approach (simply using the SSA as a means of simplifying the system by choosing cells that leave no judge-specific lenience in other margins) seems ad-hoc, but we can alternatively just use the shift that Evan and Yotam use to get away from this instrument entirely. Unfortunately, the "shift" only incidentally affects the burden of the LFOs and there are no direct (lawful) changes imposed. If we went that route, the fine (rather than fee) margin would be more appropriate to analyze since the shift is affecting the felony (or misdemeanor) class that each offender falls into.

2.2 Judge Shopping

"Judge shopping" in this setting refers to the purported tendency for prosecutors to delay cases until a more stringent judge is rotated in from another court. For instance, one could imagine a scenario where a defendant is being charged with a serious drug trafficking crime but the current set of judges in the court is sympathetic to those charged under such a code. In this case, if possible, the prosecutor might want to wait until a different judge can be assigned to the case. As discussed previously, a new set of judges is rotated into both District and Superior Courts from other counties every 6 months. The rotation schedule is known in advance and, as such, one could imagine that prosecutors might employ this strategy.

Evidence of judge shopping could present itself as either observed changes in judge cases

or in court cases at, or around, judge rotations. Appendix Figure XXX shows how number of cases evolves over time in the four largest counties.[16] Grey bars indicate when these rotations occur. Though it seems clear from the figure itself, we also use XXX (whatever rddensity test is formally called) to formally test the hypothesis that temporal discontinuities in cases overseen within a court exist. Results of this test are shown in Appendix Table XXX and show that this kind of judge shopping is not occuring at the court-level.

More precise targeting on the part of the hypothetical prosecutor might involve a specific judge. As such, we can perform the same test for the XX judges that oversee the most cases within our data. The results of this test are shown in Appendix Table XXX. Noise in this test is unsurprising when compared to the earlier one. Despite that, evidence of this kind of judge shopping also appears to be weak in North Carolina.

Finally, it's possible that judges are being systematically assigned riskier defendants despite the intended random assignment within courts.[17] Though covariates that indicate risk are included in the control vector in equation XXX, we want to separately test whether stricter (by construction) judges are seeing riskier defendants. Simply, we can take each judge's estimated leave-out-mean LFO assignment score (IV) and regress the instrument on different observed "risk" groups for defendants. Table XXX shows the results of this regression on prior record score, highest offense category for their charged crime, and highest offense category upon reoffense (coded as 0 if no reoffense is observed).

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[16]Appendix Figure XXX demeans these numbers using seasonal changes across courts, since it's clear that there is a seasonality component to the trends that is independent from the judge rotation schedule. This seasonality arises from the fact that courts across the state are closed on XXX annually. Trends across all counties look similar.

[17]In other work, such as XXX, this arises in particular types of cases due to specializiation, such as those involving driving infractions, domestic abuse, etc. In the construction of our data, all driving infractions are excluded. Other specific types of misdemeanor or felony crimes that might be considered specialized is where we think this non-randomization arises, if at all.

C   Additional Figures and Tables

Table C.1: Sample Waterfall

	Records	Cases	Individuals
Full Data	22,348,538	7,822,510	3,312,236
At Least One Judge	10,468,211	2,750,271	1,546,570
One Judge	8,910,900	2,489,665	1,479,905
Header (of Case)	2,489,665	2,489,665	1,479,905
Identifiable Prior Record	929,312	929,312	605,973
IV Cuts:			
≥ 50 Cases	927,612	927,612	604,964
Good Match	684,285	684,285	527,476
Fees $\leq$ \$2000	673,699	673,699	521,947
12 Month Horizon	491,415	491,415	397,003

NOTES: This table shows the precise way that the data structure changes as we implement the sample restrictions described in Section 3.1. Note that each case has at least one record associated with it. "Full Data" is essentially the cleaned data structure that we received from the AOC. "At Least One Judge" drops any case with no judge sitting. "One Judge" drops any case with more than one judge sitting. "Header (of Case)" essentially collapses information to the case-level. "Identifiable Prior Record" drops any case with no identifiable prior record information. After that, any cuts that are noted are for the sake of the IV approach in the analysis. As noted in Section 3.3, we also exclude cases overseen by judges with fewer than 50 cases total, poor defendant matches (on date of birth, name, etc.), cases where a fee higher than \$2000 is assessed, and any case where the defendant is incarcerated until within 12 months of the end of our sample. The remaining observations are those included in our primary analyses, matching column 3 of Table 2.

Figure C.1: Full Felony Sentencing Grid

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

NOTES: This figure shows the full set of sentencing guidelines for felony offenses committed after 11/30/2009. The rows of the grid refer to the offense class, while the columns refer to the prior record points, or criminal history, of the offender. Prior record points are a weighted sum of past convictions based on severity. These points are grouped into six classes, as shown in the headers of each column. Numbers in each cell indicate minimum sentences for each offense class by prior record class combination. These minimums are specified over three ranges: aggravated, presumptive, and mitigated, as noted in the disposition key to the right. The maximum sentence is approximately 120% of the minimum sentence, which gives the range shown. "A" indicates active incarceration, "I" indicates intermediate punishment (special probation), and "C" indicates community punishment (standard probation).

Figure C.2: Full Misdemeanor Sentencing Grid

| CLASS	PRIOR CONVICTION LEVEL		
	I	**II**	**III**
	No Prior Convictions	**One to Four Prior Convictions**	**Five or More Prior Convictions**
A1	C/I/A 1 - 60 days	C/I/A 1 - 75 days	C/I/A 1 - 150 days
1	C 1 - 45 days	C/I/A 1 - 45 days	C/I/A 1 - 120 days
2	C 1 - 30 days	C/I 1 - 45 days	C/I/A 1 - 60 days
3	C Fine Only* 1 - 10 days	**One to Three Prior Convictions**: C Fine Only* 1 - 15 days / **Four Prior Convictions**: C/I 1 - 15 days	C/I/A 1 - 20 days

NOTES: This figure shows the full set of sentencing guidelines for misdemeanor offenses committed after 11/30/2009. The rows of the grid refer to the offense class, while the columns refer to the prior record points, or criminal history, of the offender. Prior record points are a weighted sum of past convictions based on severity. These points are grouped into six classes, as shown in the headers of each column. Numbers in each cell indicate minimum sentences for each offense class by prior record class combination. These minimums are specified over three ranges: aggravated, presumptive, and mitigated. "A" indicates active incarceration, "I" indicates intermediate punishment (special probation), and "C" indicates community punishment (standard probation).

Figure C.3: Proportion of Fees Waived by Judge
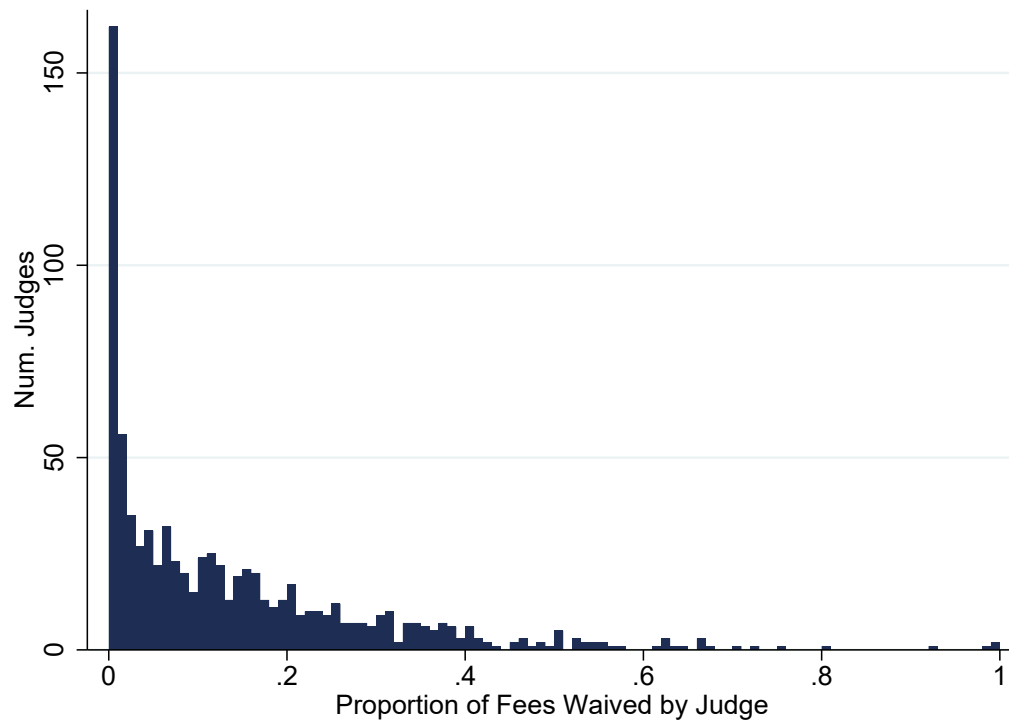
NOTES: This figure is a simple histogram showing the proportion of fees waived by judge. Judges who oversee fewer than 50 cases are omitted.

Figure C.4: Fees and Waiver Rates by County
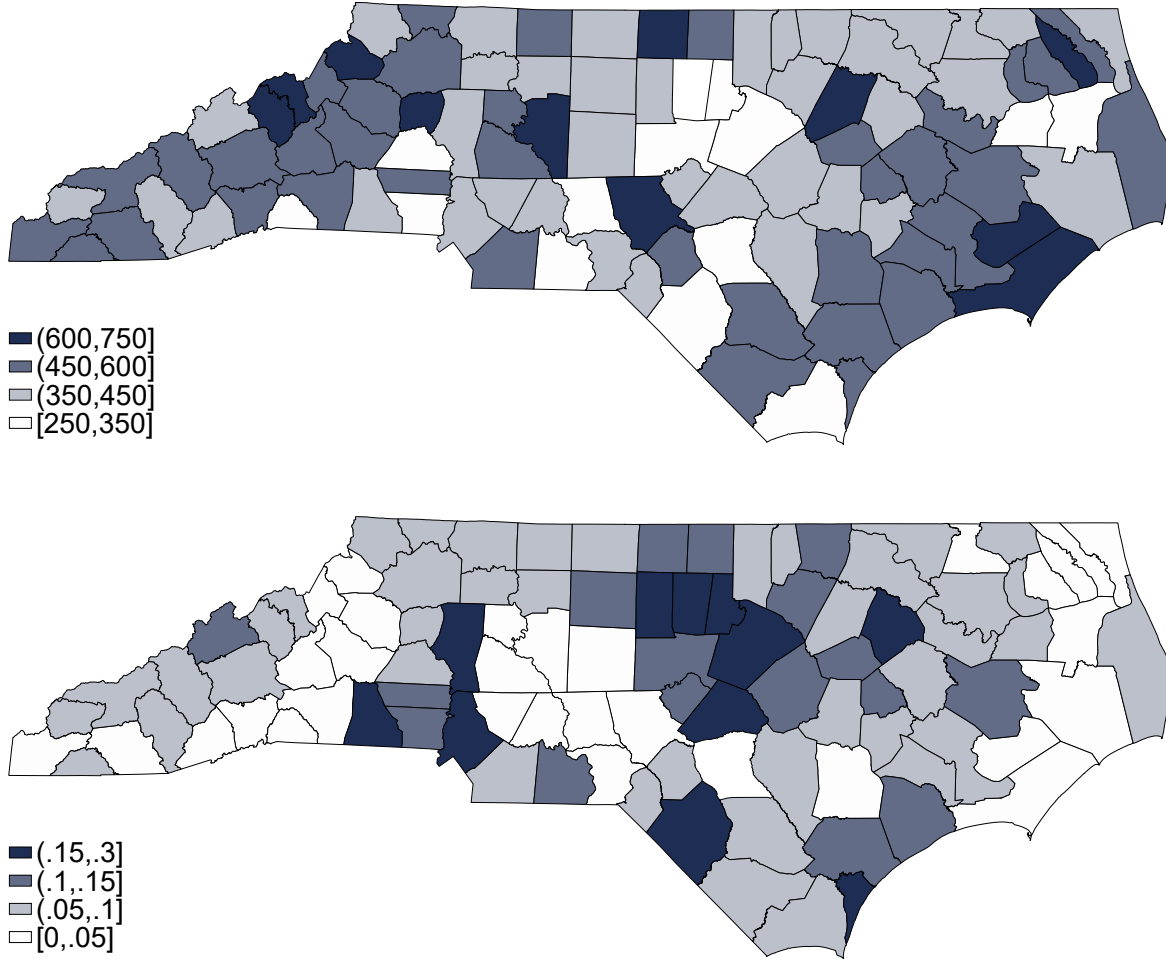
NOTES: The top figure is the average fee assigned in a case by county. The bottom figure is the proportion of fees waived by county. One interesting thing to note here is that these seem to be inversely related (or at least not positively related). One might expect that counties that assign higher fees initially are more likely to waive them, but it seems to go the other direction. That is, it seems to be the case that counties that have higher fees also tend not to waive them, which would exacerbate the burden of LFOs on the defendant. This is evidence *against* the way that we currently model the LFO assignment, since we model the waiver and assignment decisions jointly. It seems that a better approach would be to model the assignment as county-specific and the waiver as judge-specific.

Figure C.5: Number of Cases Overseen by Judges in Estimation Sample
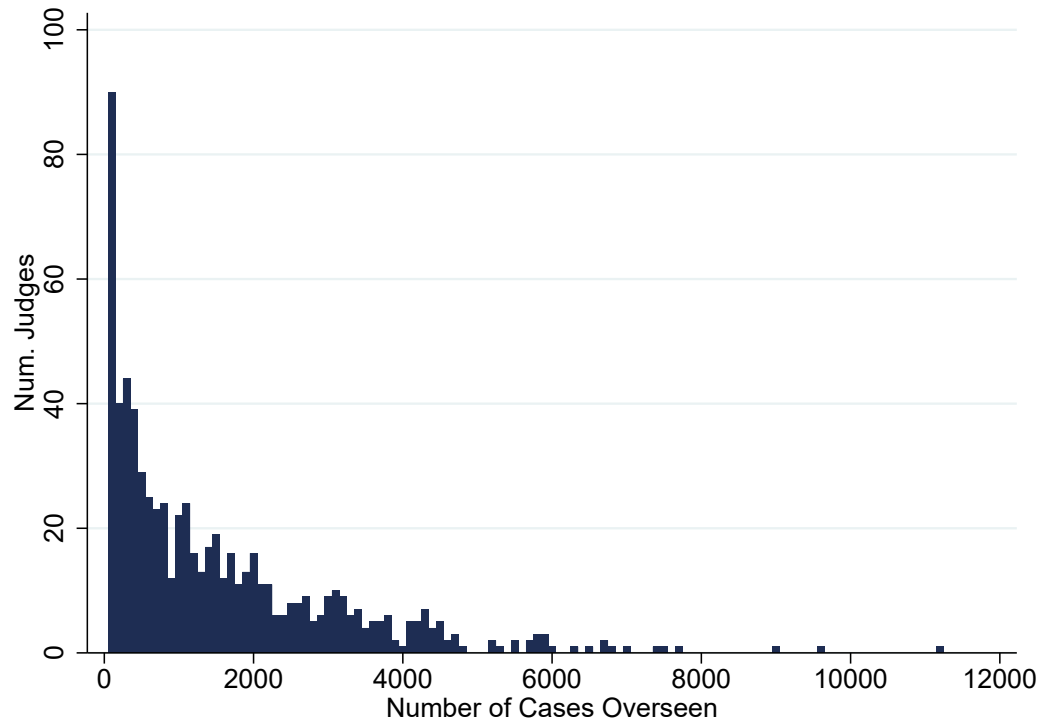
NOTES: This figure presents a histogram summarizing the number of cases overseen by each judge remaining in our estimation sample. As described in Section 3.3, this takes our primary sample and then restricts to judges that oversee at least 50 cases over the period we see (2014-2019). The median number of cases overseen in this sample is 1,251 and there are 561 unique judges.

Figure C.6: Examples of Judge Rotations in Superior Courts

District	Schedule	JUDGES	JANUARY 7	JANUARY 14	JANUARY 21	JANUARY 28	FEBRUARY 4	FEBRUARY 11	FEBRUARY 18	FEBRUARY 25
1	A	Hinton	Currituck *+	Perquimans *+	Camden *+ (A)	Dare +*	Camden +*	Dare *+ # (V)	[S]	District *+ # (A) [S]
	B	Wiggins	Gates *+	Dare *+	Pasquotank *+	Chowan *+	Currituck *+ # (A) [S]	Chowan +*	Pasquotank *+ # (A)[S]	Pasquotank +*
2	A	Grant	Beaufort *+	Washington *+	Martin +*	Martin *+	Beaufort *+	Hyde *+	Beaufort *+ # (A) [S]	Washington +*
	B	TBA	District *+ # (A)	Beaufort *+ # (A)	Beaufort +* (A)					Martin *+ (A)
3A	A	Tillett	Pitt +*	[S]	Pitt *+	[S]	Pitt *+ # (A)	[S]	Pitt +*	Pitt *+ (A)
	B	Godwin	Pitt *+ # (A) [S]	Pitt *+ (A)	[S]	Pitt *+	[S]	Pitt *+	Pitt *+ (A)	[S]
6A	A	Foster	Halifax *+	Halifax +*	Halifax *+ # (A) [S]	[S]	Halifax *+	[S]	Halifax *+ # (A) [S]	[S]
6B	A	Blount	Hertford *+ (A)	Bertie +*	Bertie *+	Northampton *+	Hertford *+ (A)	Hertford *+	Northampton *+	Hertford +*
7A	A	Cole	Nash +*	Nash *+	Nash +*	Nash *+ # (A) [S]	Nash *+	Nash +*	Nash *+ (A)	[S]
7BC	A	Q. Sumner	Wilson +*	Edgecombe *+ # (A) [S]	Edgecombe +*	[S]	Wilson *+	Wilson *+	[S]	[S]
	B	Sermons	[S]	Wilson *+	Wilson *+ # (A) [S]	Edgecombe *+	Edgecombe +*	Edgecombe *+	Wilson *+ # (A)	Edgecombe *+ # (A)[S]
9	A	Hardin	Vance *+	District *+ #	Person +*	Warren *+	Person *+	Franklin +*	Warren +*	Granville +*
	B	Thompson	District +* #	[S]	District *+ #	District *+ #	Granville +*	[S]	Vance *+	District *+ #
	C	TBA	Franklin *+ (A)	Vance +* (A)	Granville *+ (A)				Person *+ # (A)	
14	A	Hudson	Durham *+ #	Durham *+	Durham *+	Durham +*	Durham *+ #	Durham +*	Durham *+	Durham *+
	B	O'Foghludha	Durham +*	Durham +*	Durham +*	Durham *+	Durham *+ #	Durham *+	Durham *+	Durham +*
	C	TBA (Hight)	[S]	[S]	Durham *+	Durham *+	Durham +*	Durham *+	Durham +*	Durham *+
	D	TBA (O'Neal)	[S]	[S]	Durham *+	Durham *+	[S]	Durham +* #	Durham *+	Durham *+

District	Schedule	JUDGES	JULY 1	JULY 8	JULY 15	JULY 22	JULY 29	AUGUST 5	AUGUST 12	AUGUST 19
1	A	Tillett	Camden +*	Chowan *+	Dare *+	Dare +*	Chowan +*	[S]	District *+ #	Currituck *+ #
	B	Sermons	Gates *+ (V)	Perquimans *+	Camden *+	Currituck *+ (A)	Pasquotank *+	VACATION	Currituck +*	[S]
2	A	Godwin	[S]	Beaufort *+ (A)	Martin *+	Beaufort *+ # (A)[S]	Washington *+ (A)	Beaufort *+	Martin *+	Beaufort *+ # (A)
	B	TBA		Martin +* (A)	Beaufort +* (A)			Tyrrell +* (A)		
3A	A	Foster	Pitt *+	Pitt *+ # (A) [S]	Pitt *+	Pitt *+	[S]	[S]	Pitt *+	Pitt +*
	B	Q. Sumner	[S]	[S]	Pitt +*	[S]	Pitt *+	Pitt *+ # (A) [S]	[S]	Pitt *+
6A	A	Grant	[S]	Halifax *+	Halifax +*	Halifax *+ # (A) [S]	[S]	Halifax *+	Halifax +*	Halifax *+ # (A) [S]
6B	A	Cole	Hertford *+	Northampton *+	Hertford +*	[S]	Bertie +*	Bertie *+	Bertie +*	Northampton *+
7A	A	Thompson	Nash +*	Nash *+	Nash +*	Nash *+ # (A) [S]	[S]	Nash *+	Nash +*	[S]
7BC	A	Wiggins	[S]	Edgecombe *+ # (A) [S]	Wilson *+	Edgecombe *+	[S]	Edgecombe *+	Wilson *+	Edgecombe *+ # (A) [S]
	B	Blount	[S]	[S]	Edgecombe +*	Wilson +*	Wilson *+ # (A) [S]	[S]	[S]	[S]
9	A	Hinton	Granville *+	District *+ #	District *+ #	District *+ # (V)	Vance *+	Granville *+	Franklin +*	District *+ # (A)
	B	TBA (Hight)	District +* #	Granville +*	Person +*	Vance +*	Franklin +*	[S]	Warren +*	[S]
	C	TBA		Franklin *+ (A)	Warren *+ (A)		Person *+ (A)			
14	A	Hardin	Durham *+ #	Durham +*	Durham *+	Durham +*	Durham *+	Durham *+ #	Durham +* #	Durham *+
	B	Hudson	Durham *+	Durham +* #	Durham +*	Durham *+	Durham +*	Durham *+ #	Durham *+	Durham *+
	C	TBA (O'Neal)	Durham +*	Durham *+	Durham *+	Durham *+	Durham +*	Durham +*	Durham *+	Durham *+
	D	O'Foghludha	[S]	Durham *+	Durham *+	Durham *+	[S]	[S]	Durham +*	Durham *+