THE ELECTORAL CONNECTION IN COURT:
HOW SENTENCING RESPONDS TO VOTER PREFERENCES

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ABSTRACT. Do elected judges respond to constituents by tailoring criminal sentences to the electorate's exhibited ideology? We diverge from extant contributions that find (1) judges rely on their ideologies, or (2) judges tend to be punitive to avoid negative attention from the media and interest groups. Our strategic responsiveness theory argues that judges are responsive to voters. Utilizing sentencing data from North Carolina's Superior Courts, which transitioned from state-wide to district-level elections in 1996, we find that judges from liberal districts were more lenient than before, while those from moderately conservative districts assigned harsher sentences. We find that responsiveness has a strategic limit—that judges from very conservative districts did not change their sentencing patterns, which also leads to lower re-election rates post-1996. These findings suggest that elected judges strategically adapt their sentencing behavior to retain office.

On June 2, 2016, former-Stanford University student Brock Turner was given a six-month prison and three-year probation sentence for "assault with intent to rape" among other felonies. Turner's case gained national attention for the perceived miscarriage of justice[1], handed down by California Superior Court Judge Aaron Persky, who was re-elected in November 2016 without opposition. Just over two years after the Turner sentencing, Judge Persky was removed from his post—the first time a judge had been popularly recalled from California state office in 80 years.[2]

While the Turner-Persky circumstances might be exceptional, attempts like these to induce popular accountability among judges through elections are common across the American states. Only seven states do not use judicial elections, leaving thousands of judges across 43 states subject to various electoral mechanisms ranging from infrequent retention elections to partisan primaries and general elections.[3] Outside judicial institutions, scholars have troves of evidence regarding electoral accountability; candidates for public office adjust their platforms to suit the electorate's interests and, once elected, avoid making decisions that may elicit future rejection at the ballot box (e.g. Besley and Coate, 2003; Barro, 1973; Ferejohn, 1986; Besley, 2006; Bartels, 1991; Glazer and Robbins, 1985).

A great deal of research has addressed the extent to which constituents can expect their local and state judges to be responsive (e.g. Hall, 1987, 1992, 2001; Hanssen, 2004; Brace and Boyea, 2008; Choi, Gulati and Posner, 2010). Among the more important findings pertains to sentencing in state courts, where evidence suggests that trial judges sentence criminals more punitively as elections approach (Huber and Gordon, 2004). After all, it is difficult for voters to monitor judges, and the

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[1]http://www.nytimes.com/2016/06/08/us/judge-in-stanford-rape-case-is-being-threatened-who-is-aaron-persky.html

[2]http://www.nytimes.com/2018/06/06/us/politics/judge-persky-brock-turner-recall

[3]See information available from the National Center for State Courts (www.judicialselection.us/judicial_selection/methods/selection_of_judges.cfm) and Ballotpedia (ballotpedia.org/Judicial_election_methods_by_state).

judges themselves understand this informational void. As such, both Democratic and Republican judicial incumbents avoid instances of perceived under-punishment, which might "decisively influence an election" (Huber and Gordon, 2004, 248), as was likely the case with Judge Persky in Brock Turner's case. Alternatively, judges might be more systematically responsive to constituents' preferences. For instance, elected high court judges exhibit a stronger degree of opinion congruence on the death penalty than do unelected judges (Brace and Boyea, 2008). Even more, candidates for judicial office often diverge along partisan cleavages (Shepherd, 2009).

We believe there is room within this literature to better understand the electoral connections between judges and voters. We address the following question: do trial court judges closely respond to their constituents by tailoring criminal sentences to the electorate's policy preferences? This research addresses the nature of judicial accountability by examining how incumbent trial court judges respond to changes in their constituencies. We argue that state judges are highly strategic in accommodating the preferences of their voters, and therefore changes in the ideological composition of a judge's electorate should produce corresponding changes in judicial behavior vis-à-vis criminal sentencing. Still, we expect—for several reasons—that strategic judges will not dramatically polarize in their sentencing behavior. First, many judges are ambitious and desire to hold higher judicial offices (Nelson, 2014). Second, judges have sincere policy preferences, and significant deviations from those preferences decreases a judge's overall utility (e.g. Brace, Hall and Langer, 1998).

Our study exploits an exogenous shock in the rules that govern judicial elections in North Carolina. Up until the mid-1990s, Superior Court judges were selected in state-wide elections. In 1996, North Carolina's Bill 41 instituted district-level elections for Superior Courts in the impending judicial elections. Prior to Bill 41, the judges were selected in state-wide elections, and judges rotated across the state's 46 districts. Bill 41 dramatically narrowed the constituencies for over 90 trial judges.

Given the institutional changes leading to district-level elections, we examine the extent to which judges respond to their new, narrowed electorates within the 46 districts. To achieve this, we utilize 135,481 case-level terminations that result in incarceration sentences before and after the passage of Bill 41. We also employ a structural approach regarding how the passage of Bill 41 affected the sentencing behavior of Superior Court judges in a way that accounts for the selection processes determining which cases are settled and which ones result in a conviction at trial.

We find evidence in support of our expectations of strategic responsiveness, suggesting that Bill 41 induced judicial accountability to a new, narrower constituency. We first provide a detailed exploration of the underlying distributions of the data using cumulative distribution plots, which reveal that judges across liberal and conservative districts assign noticeably different sentences after Bill 41. Second, more rigorous regression analyses reveal similar results—that judges in liberal districts sentence criminals more leniently whereas judges in conservative districts sentence more punitively after Bill 41. These results are stronger among judges in districts that are proximate to the state-wide ideological mean. Third, we provide evidence that among those judges who do not tailor their sentencing behavior to district-level preferences, they are more likely to end their judicial service sooner—either through electoral punishment or preemptive retirement.

Strategic responsiveness is largely conditional on the relative ideological polarization of the judge's district. For instance, districts that reflect the broader state's ideology do not yield a change in judicial behavior. More surprisingly, districts that are particularly conservative relative to the entire state do not engender responsive judicial behavior. While these findings are not symmetric, we rely again on our expectations regarding the limits of strategic judicial behavior. That judges are ambitious office-seekers, sincere policy-seekers, or efficiency maximizers are all reasons to expect constrained responsiveness. In subsequent analyses, we find that the relatively

unresponsive conservative judges—that is, those who respond least to the narrower constituencies introduced by Bill 41—are also those who leave office early (i.e. by 2002 or earlier).

Literature, Background, & Hypotheses

This paper contributes to the substantial literature on judicial elections and responsiveness by studying North Carolina's Superior Courts, which experienced a significant exogenous shock; in 1996, Superior Court judges changed from state-wide to district-level elections. We develop a theory of constrained responsiveness—that office-seeking judges strategically defer to their constituents' preferences in order to retain office following the decentralization of judicial elections. As has been documented in existing research, voters are sure to pay attention to their democratically elected judges in cases of "judicial malfeasance" (e.g. Huber and Gordon, 2004). We supplement the existing literature by arguing that trial judges are highly aware of their constituents, and as such they seek to tailor their sentencing behavior to voters' preferences. This is especially the case in localized elections, where it is easier for judges to know their constituents and for constituents to know their judicial representatives. Therefore—when judges are accountable through particular institutional mechanisms—it is necessary for judges to go beyond merely avoiding high-profile instances of leniency. After a brief description of North Carolina's 1996 institutional changes, we delve into some of the existing literature.

Institutional Details. North Carolina's Superior Courts—its main trial court— have jurisdiction over all felony cases, as well as civil cases involving more than $10,000. Superior Court judges also decide misdemeanor cases appealed from North Carolina's lower District Courts. All trials in the Superior Courts are jury trials; a jury of twelve members decides whether or not to convict the defendant. In the event of a conviction, a judge sets a sentence. In choosing a punishment, the judge is

constrained by a set of structured sentencing guidelines, which take into consideration the severity of the crime and the defendant's previous criminal history. The sentence may consist of incarceration time, probation, community service or other alternative penalties.

Within the scholarly study of judicial responsiveness, one overarching question is what influence popular selection has on the realization of justice—in many cases, with regard to criminal sentencing. Given the variations in electoral institutions—partisan vs. non-partisan, contested vs. retention—voters are provided with different levels of information about incumbent judges. As such, judges have different incentives to tailor their behavior to their constituents' preferences. Importantly, findings of systematic variations in judicial dispositions—especially in response to electoral mechanisms—fly in the face of the expectations we have of objective, impartial courts.

Whether and how judges alter their sentencing behavior in response to perceived changes in the preferences of their voters is particularly relevant question given North Carolina's change in electoral institutions in 1996 and thereafter. Indeed, other states have experimented with different electoral rules for their judicial offices.[4] North Carolina's Superior Courts, with about 90 judges elected to eight-year, overlapping terms[5], are clustered in 46 districts.[6] Until the mid 1990s, Superior Court judges were selected in state-wide partisan elections. This system changed with Bill 41, ratified by the state Senate on August, 2 1996, which introduced two important exogenous shocks. First, starting in 1996, Superior Court judges would be selected in district-level elections. Second, judicial elections from 1998 on would be non-partisan.[7]

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[4]See reforms documented by the National Center for State Courts: http://www.judicialselection.com/judicial_selection/reform_efforts/formal_changes_since_inception.cfm.

[5]In addition to the elected judges the Governor can appoint a limited number of special Superior Court judges to serve for up to five years.

[6]Although each judge is elected to seat in a particular district the state constitution mandates judges to constantly rotate from one district to another. In practice this rotation takes place roughly every six months.

[7]A potential concern is that, besides changing the preferences of each judge's electorate, the bill also instituted non-partisan elections. Non-partisan elections differ from partisan ones in that

Existing Contributions. We study North Carolina's change from state-wide to district-wide elections—and the impact of electoral accountability on judicial behavior vis-à-vis criminal sentencing. Bill 41 created strikingly different electorates—some more liberal, and others more conservative—for Superior Court judges seeking re-election. Given this dynamic, we focus on how the existing empirical evidence suggests that judges are re-election-seeking actors. In other words, prior research provides a great deal of evidence regarding how (1) different electoral systems induce certain degrees of judicial responsiveness, and (2) judges avoid the attention and ire of the media and interest groups as elections approach.

For instance, Huber and Gordon (2004) speak directly to the dynamic of judicial responsiveness whereby strategic judges avoid negative attention from the media, interest groups, and voters. Using sentencing data from Pennsylvania's judges, Huber and Gordon (2004) contend that state trial judges avoid appearing "soft on crime," causing judges to treat criminal defendants more punitively, especially as elections approach. A judge's desire to avoid the public's attention stems, at least in part, from the non-partisan, retention elections in Pennsylvania, whose voters lack other signals about the judges. Gordon and Huber (2007), using data from Kansas's partisan (contested) races and retention elections, provide evidence that partisanly elected judges exhibit greater punitiveness than those judges elected in retention elections. In sum, across their contributions, Gordon and Huber provide evidence (1) that judges in low information environments (e.g. Pennsylvania retention elections) are increasingly punitive as elections approach, and (2) that greater information (e.g.

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voters lack an important piece of information when deciding who to vote for. One could argue that such a lack of information affects the electorate of all judges identically. But a more cautious interpretation of the results reported in this paper should consider that we cannot disentangle the effects of the change in the electorate from those associated with the introduction of non-partisan elections. As mentioned above, district wide elections were introduced in 1996, while non-partisan elections only started in 1998. In principle, we could try to identify the effects of each of these changes by separately looking at judges whose seats were up for election in 1996 and judges whose terms would finish in later years. But in practice there are too few judges in the first group for such a strategy to be implemented with a reasonable precision.

Kansas partisan election) augments the demands on incumbent judges making them more punitive.

Still, scholars do not agree that an informational void uniformly demands judges to exhibit more punitive sentencing behavior. Indeed, existing research provides evidence that state judges often (1) rely on their personal preferences, and (2) attempt to tailor their behavior to mass preferences. Lim (2013) finds evidence that sentencing patterns are considerably more heterogeneous among Kansas judges facing partisan re-election than among those under retention. By separately quantifying how (1) electoral pressure and (2) judicial preferences affect judicial behavior, Lim (2013) shows that systematic variations in sentencing are attributable to the judge's own preferences. At the same time, Canes-Wrone, Clark and Park (2010) provide strong evidence that low information judicial elections induce high levels of democratic accountability—more so than in high information (partisan, contested) elections, where judges are often free to adjudicate in line with their partisan identification. These results hold outside the criminal sentencing data utilized across much of the established literature; Canes-Wrone, Clark and Park (2010) utilize judge votes on abortion cases, with retention judges aligning with state-level preferences at a much higher level than partisanly elected judges.[8] When institutional rules make it difficult for voters to obtain information about judicial candidates—as in non-partisan or retention elections—those judges are pressured "to cater to public opinion on hot-button issues" (Canes-Wrone, Clark and Park, 2010, 214).

Even given that different electoral institutions induce systematic variations in judicial accountability, other research on electoral ambition gives us good reason to expect judges—indeed many elected officials—to exhibit responsiveness throughout

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[8]It is worth noting that the evidence regarding state supreme court judges differs somewhat from that of trial courts; while electoral mechanisms still vary across states, courts of last resort receive a great deal more media and interest group attention. Therefore, even in low information electoral systems, such as non-partisan retention elections, we might expect high court judges to be more responsive to mass preferences than would their trial court counterparts.

their terms. Nelson (2014) argues that the desire to seek higher office might cause a local office-holder to respond to broader public opinion. Indeed, the cyclical nature of election-induced congruence could be broken if we consider the overlapping nature of elections. To utilize an example outside law and courts, presidential candidates often run for office strategically—that is, when they have no risk of losing their current office. In 2016, that was the case for U.S. Senators Ted Cruz, Lindsey Graham, and Bernie Sanders, and Governors Chris Christie, John Kasich, and Scott Walker. While this is certainly not the only predictor for seeking higher office, the most basic conclusion from such a scenario is that it may not be safe to assume a particular cycle or model for responsiveness. This could likewise be the case for ambitious judges seeking higher state judicial office or federal appointment.

Our Contribution: Assessing Strategic Responsiveness. Despite many significant contributions to understanding judicial responsiveness to mass preferences—some of which we examine above—we nonetheless see an opening in the literature to address how judges alter their behavior in response to perceived changes or differences in the preferences of voters. We identify three realistic possibilities, though there are undoubtedly others. First, a judge—in anticipation of changes in her electorate— might always assign more punitive sentences in order to satisfy voters and deter negative media and interest group attention. This would be consistent with existing evidence of judicial accountability, especially in low-information retention elections (e.g. Huber and Gordon, 2004; Canes-Wrone, Clark and Park, 2010). Second, perhaps judges' policy preferences are strong enough that sentencing behavior would not systematically vary in response to changes in the electorate. Third, judicial sentencing could be a dynamic, strategic process, whereby judges attempt to retain office by tailoring their sentences to the preferences of their voters. In short, the existing literature does not provide sufficient adjudication among these hypotheses.

In support of the third and final hypothesis above, we put forth a theory of constrained responsiveness and we apply our theory to North Carolina's Bill 41 judicial election reforms.[9] We argue that electoral change incentivizes office-seeking judges to strategically defer to their constituents' preferences; judges in liberal districts will sentence criminals more leniently while judges in conservative districts will sentence more punitively. As judges' district-level electorates are increasingly ideologically polarized, we expect to observe corresponding increases and decreases in judges' punitiveness. Still, we expect this district-level accountability to have a strategic limit; we do not believe judges will polarize in districts that are drastically liberal or conservative.

We condition our expectation in this way for several reasons. First, the North Carolina judges we examine were initially elected by a state-wide electorate, suggesting that they may have moderate preferences that closely approximate that of the state's median voter. Second, judges may attempt to hedge their bets by not exhibiting polarizing behavior. Our strategic story of judicial behavior suggests that judges should accommodate voters just enough to dissuade negative downstream consequences— that is, losing office at the next election. Third, district-level preferences can change due to a variety of circumstances, meaning that judges may avoid too closely following a polarized constituency.[10] Fourth, state trial court judges may have some

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[9]The institutional change we study here—where North Carolina's judges move from partisan state-wide elections to partisan district-level elections to non-partisan district-level elections—is not unique across the American states. Indeed, North Carolina has, once again, changed its judicial elections as of 2017. In March of that year, North Carolina legislators passed and overrode NC Gov. Roy Cooper's veto of a bill making the state's judicial elections partisan once again. (See: https://www.newsobserver.com/news/politics-government/state-politics/article140327188.html) State-level changes from partisan to non-partisan and from contested to retention elections have been frequent in an effort to maximize judicial independence, though there has been some backlash towards re-instituting partisan, contested elections to re-assert voters' influence (e.g. Canes-Wrone, Clark and Park, 2010). In the case of North Carolina's Bill 41, the primary exogenous shock we are interested in is the change in electoral accountability whereby judges are subjected to a particular subset of their prior electorate.

[10]We can imagine how and when an official's electorate would change substantially in terms of its preferences. This is frequently the case with congressional re-districting, which sometimes drastically changes the geographic boundaries that define a member's electorate. Even more, when states lose districts, incumbent office-seekers are forced to compete against other incumbents. Indeed, in the extreme, we can imagine that small states—with only one at-large member of Congress—could

ambition for higher office. These considerations suggest a moderating effect on judi-
cial behavior, as maintaining reasonable proximity to moderate voters allows a judge
to maintain her appeal to (1) a broader electorate and (2) a potentially changing
district. Judges who dramatically polarize in their criminal sentencing behavior are
likely deviating greatly from the state's median voter.

Our expectation diverges from Huber and Gordon (2004), whose theory of "judicial
malfeasance" strongly predicts Judge Persky's 2018 recall in California, as discussed
above. Indeed, Gordon and Huber provide evidence of unidirectional punitiveness in
low information judicial elections (e.g. Pennsylvania's retention elections) and high
information judicial elections (e.g. Kansas's partisan contested elections). Our case
from North Carolina is more akin to Kansas, though we are not studying variation
between election types.

Still, we have several concerns regarding the connection between "fire-alarm" theory
and the data generation process for judicial elections. Across all of these concerns, we
regard the responsiveness a judge exhibits towards her constituents as being able to
trump any threat of a fire-alarm. First, judicial elections are not unique (1) in being
of relatively low salience to voters, and (2) in providing little information to voters.
Scholars have long established that voters look for and are adept at using shortcuts in
order to maximize their utility (e.g. Lupia, 1994). Second, judges' perceived under-
punishments matter a great deal given (1) certain mass preferences, and (2) certain
institutional rules (e.g. non-partisan or retention elections) (Canes-Wrone, Clark and
Park, 2010). This is certainly not the case with partisan elections, where voters have a

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grow substantially to a level that requires a second congressional district, resulting in a subsetted
electorate for the existing member of Congress. Even more, we can imagine how changes in electoral
institutions, which aggregate voters' preferences, can matter as well. California's jungle primaries,
which require all candidates across parties to compete in a single primary, do not compel candidates
to pander to a primary, party-based constituency. Rather, the candidates can appeal to the district
as whole. While this occurs in the opposite direction from Bill 41 in North Carolina, the example
provides even more evidence that electoral reforms such as those under investigation here are not
isolated, idiosyncratic instances of changes in the mechanisms of accountability.

strong signal regarding which judicial candidate would better represent constituents'
preferences on average. Third, consistent with the theoretical argument and empirical
evidence from Canes-Wrone, Clark and Park (2010), partisan labels are a strong signal
to voters, such that voters are often willing to overlook one-off policy differences in
order to vote for their ideal candidate on average:

> "[S]ay a conservative voter who favors the death penalty learns a judge
> voted to overturn it in a particular case. If the voter observes a party
> label and sees that the judge in question is a Republican, she may well
> prefer that candidate to the Democrat, despite the decision. After
> all, the voter could reasonably expect the Republican candidate to be
> more supportive of the death penalty, despite the one vote that was
> publicized during the campaign. [footnote omitted] By comparison,
> absent the party cue, the voter may be inclined to vote against the
> incumbent, even if she does not have an explicit challenger." (Canes-
> Wrone, Clark and Park, 2010, 217)

In short, the interaction between electoral rules and constituent preferences would
seem to be primary motivators for elected officials' behavior.

At the crux of our argument is the notion that judges can take cues across con-
texts (e.g. Shepherd, 2009; Canes-Wrone, Clark and Park, 2010; Westerland et al.,
2010).[11] Therefore, we expect that the North Carolina trial court judges will perceive
differences between their former and new electorates. Given that North Carolina's
1995 elections were partisan, we formulate the following strategy for the office-seeking
judge. First, voters receive a partisan signal regarding their judicial representative.
Second, rather than voting on several state-wide judicial offices as was the case prior
to Bill 41's enactment, voters now vote only on judicial elections within their district.

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[11]That is to say that judges can take cues or instructions from voters, co-equal branches of
government, high courts (e.g. with regard to legal doctrine), etc.

Therefore, one would expect voters to support co-partisan judges at the polls. Third, strategic, office-seeking judges will anticipate the preferences of their district-level constituents by conforming their sentencing practices towards voters' preferred outcomes. In short, we argue that incumbent judges can tell the difference between sets of voters, and voters are able to tell the difference between partisan judges. The result is an environment in which judges tailor their sentencing to the relative preferences of their district-level voters.

To be sure, North Carolina's institutional change leads to seemingly countervailing influences with regard to voters and interest groups. On one hand, as we argue, North Carolina voters have a simplified informational environment after Bill 41 insofar as they only have to vote for judicial candidates in their home districts rather than all judicial candidates in the state. This should heighten the effect of constituent ideology on judicial behavior. On the other hand, interest groups and media actors have a more difficult job in attracting negative attention toward local races after Bill 41. Prior to the reforms, statewide judicial candidates facing a statewide electorate would necessarily be concerned about attracting unwanted outside attention, à la Huber and Gordon (2004). After Bill 41, outside groups can no longer use the races as an opportunity to mobilize voters at the state level, which lowers the amount of information provided to voters. We regard the balance of these considerations as supporting our theory, as voters are better able to identify their policy maximizing candidate, and the threat of interest group and media fire alarms are diminished across decentralized elections.

Judicial Elections and Sentencing in North Carolina

The strategic responsiveness hypothesis we identify above—that North Carolina's judges will strategically defer to their district voters' preferences in terms of criminal sentencing—requires data of several different types. Therefore, we employ data from

three main sources. Our sentencing data, which we obtained from the North Carolina
Administrative Office of the Courts, comprises every case filed at the North Carolina
Superior Courts from January 1995 to October 2010 and includes detailed informa-
tion on case disposition, charged offenses, and characteristics of the defendants. In
addition to these data, we use the annual editions of the North Carolina Manual to
organize a complete list of Superior Court judges active in each year of our analysis.
Finally, we measure the conservativeness of the judicial districts as the district-level
Republican vote share in the 2000 Presidential elections. Voting records at the district
level are available online in the website of the North Carolina Board of Elections.

Next, we describe the relevant aspects of the sentencing data and voting data. We
then present descriptive statistics that serve as preliminary evidence of the effect of
Bill 41 on the sentencing behavior of judges. For further details on the North Carolina
Superior courts and the manipulation of the data, see Silveira (2017).

Sentencing decisions. North Carolina uses a system of structured sentencing. In
such a system, in the event of a conviction at trial, the judge chooses a sentence from
a predetermined range. The sentence range depends on (1) the severity of the offense
and (2) the offender's previous criminal record. The sentence may generally consist
of incarceration time or alternative punishments such as probation.[12]

Although the system imposes constraints on the choices of judges, it still leaves
considerable discretion for the assignment of sentences. For example, an offender
with no prior criminal history that is convicted of assault with a deadly weapon
with intention to kill may be may be sent to prison or be assigned an alternative
punishment (e.g., probation). If the judge decides to assign incarceration time, the
minimum sentence length is set from a range that goes from 15 to up to 31 months.

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[12]The judge only sets a minimum incarceration length. The maximum length is determined
according to a formula, and is roughly 120 percent of the minimum. See Silveira (2017) for details.

As we are interested in the sentencing behavior of the judges, we only consider in the analysis cases in which the defendant was found guilty. We consider both cases decided by a jury and by a plea bargain. The latter are decided under the shadow of the judge—i.e., the defendant and the prosecutor negotiate taking into consideration the harshness of the sentence to be assigned in the event of a conviction at trial. Hence the judge's sentencing behavior also affects cases decided by a plea bargain, although such an effect is only indirect. In our regressions, we use the method of disposition as a control variable.

In the sentencing data, judges are only identified by their initials. In most cases, three initials are used. We match such initials to the full names of the judges as reported annually on the North Carolina Manual. In the period comprised by the sentencing data, only two pairs of judges had the same three initials. Cases decided by these judges were excluded from the data. We also excluded all the cases in which the judge was either identified by less than three initials or not identified at all. Finally, we could not consider in the analysis cases in which it is not specified whether or not the offender was incarcerated, as well as cases where incarceration time was assigned but the minimum sentence is not reported.[13]

Table 1 presents summary statistics for the sentencing data after all the exclusions mentioned above. These statistics only take into consideration cases that resulted in an incarceration sentence, either at trial or by plea bargain. As it happens in all American states, the vast majority of the cases are resolved by plea bargain. There are many more male than female defendants in the sample. Whites and African Americans defendants appear in similar proportions and represent most of the sample, and the average age of the defendants is 30.84 years. There are less observations for

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[13]Sentences in multiple count cases may be either consecutive or concurrent. We don't have this information in the data. In the analysis, we assume that all sentences are concurrent.

Table 1. Descriptive statistics—Incarceration convictions

Distribution of cases by outcome		
Method of disposition	Observations	Frequency
Plea bargain	127,393	94.03%
Trial	8,088	5.97%
Total	135,481	100%
Date of disposition	Observations	Frequency
Prior to Bill 41	4,950	3.65%
After Bill 41	130,531	96.35%
Total	135,481	100%
Distribution of cases by severity of the charge		
Main charged offense	Observations	Frequency
Felony	121,222	89.43%
Other	14,259	10.57%
Total	135,481	100%
Defendant's characteristics		
	Observations	Frequency
African-American	80,733	59.59%
Hispanic	3,588	2.65%
Female	10,197	7.53%
	Mean	Standard deviation
Age (years)	30.85	9.94
Sentences' length by conviction method (months)		
	Mean	Standard deviation
Trial convictions	91.25	134.27
Settlement convictions	23.48	42.76

Notes: This table, which is based on data form the North Carolina Administrative Office for the Courts, refers to criminal cases decided at the North Carolina Superior Courts from January 1995 to October 2010. We exclude from the sample all homicide cases, as well as cases with missing information on any of the following: the sentence assigned, the method of disposition, the main charged offense or the defendant's age, gender or race/ethnicity.

cases disposed in 1996 than for other years. Still, there are 5,498 cases disposed before the approval of Bill 41.

Table 2. Descriptive statistics—District conservativeness

District conservativeness	
Mean	54.33%
Median	56.40%
Min	19.28%
Max	70.17%

Judges by district conservativeness—full sample		
	Observations	Frequency
Conservative[†]	74	39.36%
Liberal[††]	114	60.64%
Total	188	100%

Judges by district conservativeness—judges active in 1996		
	Observations	Frequency
Conservative[†]	39	41.94%
Liberal[††]	54	58.06%
Total	93	100%

Notes: This table contains information on our measure of judicial district conservativeness, the Republican vote share in the 2000 Presidential elections. We obtained vote share data from the North Carolina Board of Elections. †. Republican vote share above 56.47% (state-wide vote share). ††. Republican vote share below 56.47%.

Electorate's conservativeness. Ideally, our analysis should use a direct measure of the harshness of judicial sentencing desired by voters at the judicial district level. To the extent of our knowledge, there is no such data available. As an approximation of preferences for harshness, we employ the district-level Republican vote share in Presidential elections. Presidential voting outcomes are readily available at the precinct level for the 2000 election, which allows for an almost exact matching with the judicial districts. In the analysis that follows, we use the vote share for George W. Bush in 2000 as a measure of judicial district conservativeness.[14]

There are 66 judicial districts represented by at least one judge in the sample. Vote shares for George W. Bush in the 2000 elections lie between 19.28% and 70.17%. The

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[14]For calculating the vote shares, we use only the votes received by the Republican and the Democratic candidates.

unweighted mean of vote shares for Bush across the districts in the sample is 54.02%. As a basis for comparison, the statewide vote share for Bush was 56.47%. Thus voting records for the 2000 Presidential election indicate a high variance in the preferences of the electorate across districts. This high variance is welcomed, as it helps identifying whether judges with different electorates reacted differently to the passage of Bill 41.

It is useful to classify judges according to the conservativeness of their districts. We define a judge's district as conservative if its Republican vote share in the 2000 Presidential election was greater than 56.47%, the statewide vote share. Otherwise we classify a district as liberal. Table 2 presents the distribution of judges in the sample according to their districts' conservativeness. There are 188 judges in the whole sample. When all such judges are accounted for, those from liberal districts constitute a majority (60.64%). The distribution is very similar if we only consider the 93 judges in activity in 1996, when Bill 41 was approved.
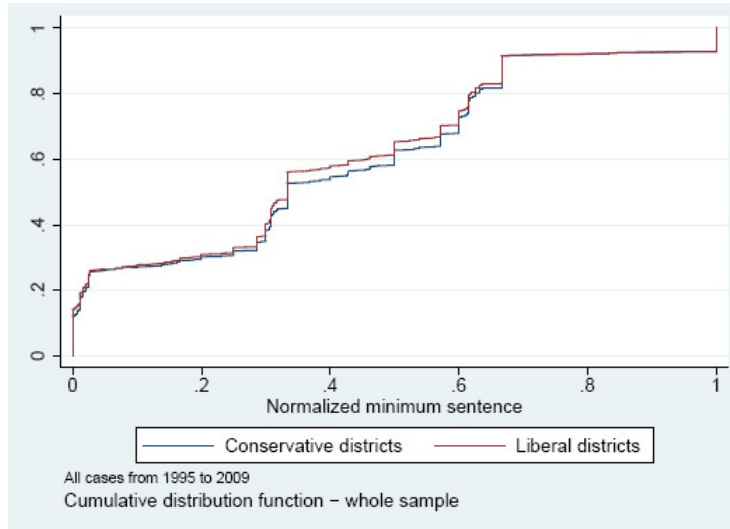
Electorate's preferences and sentencing decisions. As a first endeavor toward examining our strategic responsiveness hypothesis, we consider several baseline comparisons before and after Bill 41. As we consider how strategic, office-seeking judges anticipate their constituents policy preferences, Figures 1, 2, and 3 illustrate how the divergence in judicial behavior stems from sentences issued after Bill 41. Once judges are assigned to particular districts, we observe sentences that are more tailored to the exhibited policy preferences of those district-level voters.

Cross-sectional comparison. Figure 1 depicts the cumulative distribution functions of the incarceration sentence length assignments by judges from liberal and conservative districts in all years in the sample. To compute the CDFs, we normalize each sentence, so that it equals zero if it is the lowest possible sentence under the structured sentencing guidelines and one if it is the maximum possible sentence.[15] The figure

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[15]The normalization also holds for the few sentences assigned outside of the guideline bounds, so that such sentences are censored.
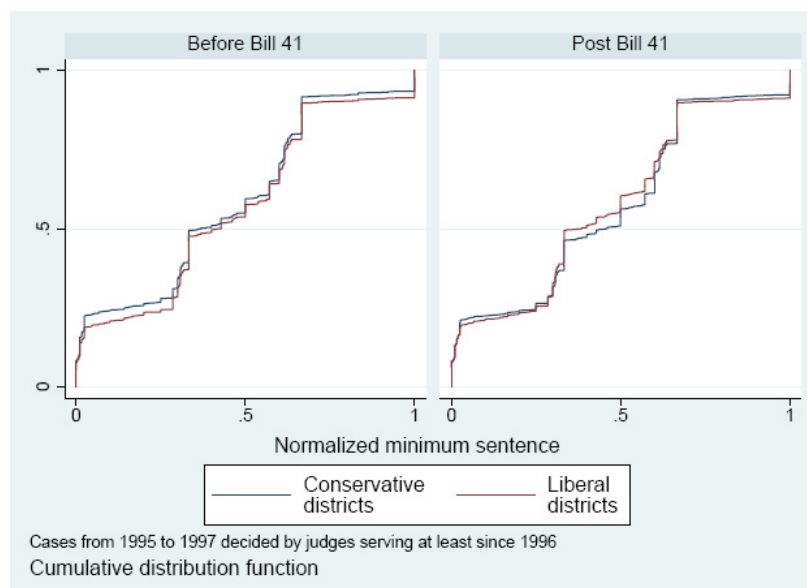
Figure 1. Assigned sentences' length—Judges from liberal and conservative districts

All cases from 1995 to 2009
Cumulative distribution function – whole sample

reveals that judges from liberal districts tend to assign sentences in the most lenient range of the scale (less than 1/3) more often than their counterparts from conservative districts. The latter group of judges assign relatively more sentences in an intermediate range (from 1/3 to 2/3). Both groups of judges assign sentences in the harshest range (from 2/3 on) with similar frequency. A two-sample Kolmogorov-Smirnov test rejects the null hypothesis of equality of the two CDFs at a confidence level of 1%.

Reaction to Bill 41. Figure 2 shows CDFs similar to the ones from figure 1. This figure only consider judges that were serving at the time Bill 41 was approved. The two plots on the left-hand side depict cases disposed by judges from liberal and conservative districts before the passage of the bill. The plots on the right-hand side depict cases disposed by the same two groups of judges after the passage of the bill. The distinction between the two groups of judges is not very clear in the period prior to Bill 41 but becomes much more evident after it. Judges from conservative districts start assigning sentences that are harsher than the ones chosen by judges from liberal districts. It is worth noticing that the CDFs in the right-hand side of Figure 2 are
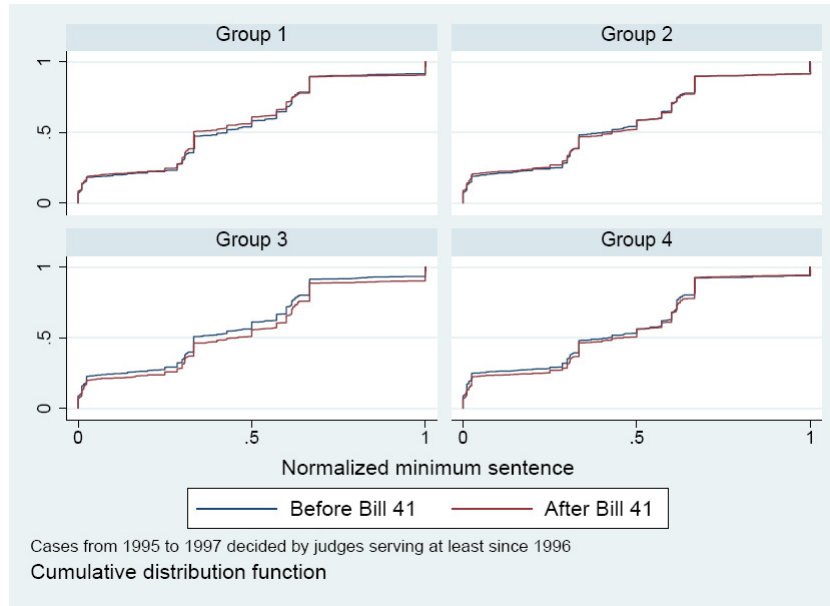
Figure 2. Reaction to Bill 41—Judges from liberal and conservative districts

Cases from 1995 to 1997 decided by judges serving at least since 1996
Cumulative distribution function

similar to the ones in Figure 1. Figure 2 thus suggests that judges responded to the change in the preferences of their electorate caused by Bill 41.

However a more detailed examination of the data reveals that the relationship between voters' preferences and judges' behavior is not as simple as the previous paragraphs suggest. In Figure 3 we separate judges in four groups, according to the conservativeness of their districts. Group one consists of judges from districts with Republican vote share in the 2000 Presidential elections bellow the first quartile of the distribution of district-level vote shares. Group two contains judges whose districts have vote shares between the first and the second quartiles. Group three refers to judges from districts with vote shares between the second and the third quartile. Group four contains the remaining judges.

Figure 3 depicts the CDFs of assigned incarceration time before and after the approval of the bill for each of the four groups of judges. The CDFs point to an interesting pattern. Judges from group one become more lenient after the passage of the bill, while judges from the third group start assigning harsher sentences. Judges

Figure 3. Reaction to Bill 41—Four groups of judges

Cases from 1995 to 1997 decided by judges serving at least since 1996
Cumulative distribution function

from the second group, who are elected by districts roughly as conservative as the state as a whole, seem to be much less affected by the passage of the bill. All these effects are consistent with the ones suggested by figure 2. But judges from group four, who have the most conservative districts, appear not to change their sentencing behavior at all after the bill.

One interpretation for the pattern suggested by Figure 3 is that judges are willing to pander to voters, but only to some extent. Indeed, if judges derive utility from their own sentencing decisions, they may adapt their decisions to suit the preferences of voters who are not too distant from them in the liberal-conservative spectrum. But, if the electorate's preferences for harshness are different enough from those of the judges, it may be a better option for the latter simply to assign their favorite sentences. The cost of ignoring the will of the voters is a reduced likelihood of success in the following elections but, as long as judges and voters are sufficiently different from each other, it may be worth it for the judges to incur this cost.

Furthermore, any significant divergence between local voters' preferences and a judge's sentencing practices might also be a byproduct of the judge's career goals. As we mention above, elected judges—along with all other elected politicians—are often ambitious to attain higher office. The overlapping nature of elections paired with (1) the hierarchical nature of courts and (2) the different constituencies/dynamic mass preferences results in ambitious judges needing to anticipate preferences of voters external to their current districts.

To be sure, a variety of confounding factors can make the simple analysis of histograms misleading. In the next section we investigate the effects of Bill 41 in a more rigorous manner.

Empirical Analysis

To evaluate the impact of Bill 41 on sentencing behavior, we begin by defining a measure of the preferences of the electorate of the judge responsible by each case i, based on the district-level Republican vote share in the 2000 Presidential elections. The state-wide Republican vote share in those elections was 56.47%. We define the following two variables:

$$\text{Liberal}_i = \max\left\{0.5647 - \text{District Conservativeness}_i, 0\right\}$$

$$\text{and} \quad \text{Conservative}_i = \max\left\{\text{District Conservativeness}_i - 0.5647, 0\right\}, \quad (1)$$

where *District Conservativeness$_i$* is the Republican vote share in the district of the judge in charge of case i. The variables *Liberal$_i$* and *Conservative$_i$* separately capture the distance from the state-wide center in the liberal-conservative spectrum for liberal and conservative districts. To evaluate the impact of Bill 41 on sentencing behavior,

we then estimate the following specification:

$$\text{Sentence's Length}_i = \alpha + \gamma_C \text{ Conservative}_i * \text{Bill } 41_i + \gamma_L \text{ Liberal}_i * \text{Bill } 41_i$$

$$+ \delta \text{ Bill } 41_i + \lambda_{\text{judge}_i} + \beta X_i + \epsilon_i. \tag{2}$$

The dependent variable *sentence* consists of the logarithm of the assigned sentence length; the dummy *Bill* 41 indicates whether the disposition of case i took place after the ratification of the bill; the variable λ_{judge_i} is a judge-specific dummy; and X_i is a vector of controls, which includes the defendant's ethnicity, gender, previous criminal history, age and age squared. The vector X_i also contains dummies indicating the year of disposition and the county of prosecution of the case, as well as the severity of the offense charged against the defendant. Finally we include a dummy indicating whether the case was resolved by plea bargain. In our regression analysis we only consider cases in which incarceration time was assigned.[16]

The coefficients γ_C and γ_L are the main parameters of interest. A positive value for γ_C and a negative one for γ_L indicate that the sentencing behavior of Superior Court judges tends to correspond to the desires of their voters after the passage of the bill. Therefore, positive (negative) estimates for γ_C (γ_L) are consistent with the hypothesis that judges are responsive to the electorate's preferences.

We also estimate the following specification, which allows for non-linearities in the effects of the interactions between the passage of Bill 41 and the variables Conservative and Liberal:

$$\text{Sentence's Length}_i = \alpha + \gamma_{C,1} \text{ Conservative}_i * \text{Bill } 41_i + \gamma_{C,2} \left[\text{Conservative}_i * \text{Bill } 41_i\right]^2$$

$$+ \gamma_{L,1} \text{ Liberal}_i * \text{Bill } 41_i + \gamma_{L,2} \left[\text{Liberal}_i * \text{Bill } 41_i\right]^2$$

$$+ \delta \text{ Bill } 41_i + \lambda_{\text{judge}_i} + \beta X_i + \epsilon_i. \tag{3}$$

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[16]We also investigated the relationship between the judges' electorate preferences and the decision of assigning any incarceration time, conditional on a conviction. We found no evidence of such effects.

The main parameters of interest in specification 3 are $\gamma_{C,1}$, $\gamma_{C,2}$, $\gamma_{L,1}$ and $\gamma_{L,2}$. The specification is flexible enough to allow judges from moderate and extreme districts to react differently to the passage of Bill 41.

Results obtained from OLS estimation of specifications 2 and 3 are shown in Table 3.[17] Across the five columns of the table, our specifications allow for linear and curvilinear relationships between our interaction terms and criminal sentencing. Column (1) allows for a straightforward linear relationship between county-level ideology and criminal sentencing. Column (2) includes linear and quadratic terms for both of our interactions, which allows for a curvilinear relationship across the polynomial in specification 3 above. Column (3) in similar to the previous column, but only includes a squared term for *conservative * Bill 41*. Finally, columns (4) and (5) use subsetted versions of our data, which provide clean—if incomplete—tests regarding whether conservative or liberal districts, respectively, lead to more lenient or punitive sentencing.

First we examine the results as they pertain to liberal counties. The linear terms in column (1) provide a straightforward test of our expectation that judges assigned to more liberal counties sentence more leniently after Bill 41. Given the concomitant effects of (i) the interaction variable *liberal * Bill 41* paired with (ii) the term *Bill 41*, we observe a negative effect on sentence length. Therefore, as a North Carolina county is increasingly liberal (i.e. then-Gov. Bush's county-level vote-share is < 0.5647 and decreasing), judges are decreasingly punitive in their criminal sentencing following Bill 41's passage. These results for judges in liberal counties hold across Table 3's columns (1), (3), and (5) (subsetted data). Column (4) does not include data for liberal counties. Column (2) addresses whether we observe a tempering of the relationship

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[17]In all regressions in this section, the reported standard deviations are robust to clustering at the judge level. The estimates reported in Table 3 are based on our entire sample, which comprises the years 1995-2010. Restricting our sample to a period closer to the passage of Bill 41 (for example, the years 1995-2002) does not qualitatively change our findings.

Table 3. Reaction to Bill 41

	(1)	(2)	(3)	(4)	(5)
$conservative * Bill\ 41$	-0.418	2.225**	1.829**	3.682*	-
	(0.292)	(1.122)	(0.876)	(1.986)	
$[conservative * Bill\ 41]^2$	-	-19.699***	-17.442***	-25.886**	-
		(6.781)	(5.572)	(11.637)	
$liberal * Bill\ 41$	-0.406***	0.257	-0.274*	-	-0.263*
	(0.131)	(0.539)	(0.150)		(0.120)
$[liberal * Bill\ 41]^2$	-	-1.620	-	-	-
		(1.391)			
$Bill\ 41$	0.071**	0.028	0.044	-0.072	0.079**
	(0.030)	(0.040)	(0.032)	(0.077)	(0.032)
$settled$	-0.519***	-0.518***	-0.518***	-0.514***	-0.522***
	(0.016)	(0.016)	(0.016)	(0.022)	(0.022)
age	0.013***	0.013***	0.013***	0.015***	0.011***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
age^2	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
$female$	-0.172***	-0.172***	-0.172***	-0.160***	-0.184***
	(0.010)	(0.010)	(0.010)	(0.014)	(0.015)
$black$	0.008	0.008	0.008	0.020**	-0.006
	(0.007)	(0.007)	(0.007)	(0.010)	(0.009)
$hispanic$	-0.112***	-0.113***	-0.113***	-0.106***	-0.118***
	(0.016)	(0.016)	(0.016)	(0.025)	(0.020)
$attorney : private$	-0.047***	-0.047***	-0.047***	-0.054***	-0.041***
	(0.008)	(0.008)	(0.008)	(0.011)	(0.013)
$attorney : public\ defender$	-0.076***	-0.076***	-0.076***	-0.073***	-0.079***
	(0.008)	(0.008)	(0.008)	(0.013)	(0.009)
$convict\ history\ 2$	0.024**	0.024**	0.024**	0.034**	0.017
	(0.010)	(0.010)	(0.010)	(0.014)	(0.014)
$convict\ history\ 3$	0.339***	0.339***	0.339***	0.339***	0.341***
	(0.012)	(0.012)	(0.012)	(0.017)	(0.016)
$convict\ history\ 4$	0.594***	0.594***	0.594***	0.585***	0.607***
	(0.015)	(0.015)	(0.015)	(0.021)	(0.020)
$convict\ history\ 5$	0.809***	0.808***	0.808***	0.826***	0.794***
	(0.017)	(0.017)	(0.017)	(0.022)	(0.024)
$convict\ history\ 6$	1.017***	1.017***	1.017***	1.017***	1.021***
	(0.019)	(0.019)	(0.019)	(0.026)	(0.027)
Observations	135,481	135,481	135,481	65,920	69,561
R^2	0.658	0.658	0.658	0.654	0.663

Notes: OLS estimates. The unit of observation is a case. Standard errors, provided in parentheses, are adjusted for two-way clustering at the judge-period levels, where period refers to pre- and post-Bill 41; $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. In columns (1)-(3), we include all non-homicide criminal cases prosecuted from Jan 1995-Oct 2010, except for those cases in which critical information was missing, as explained in Table 1. Columns (4) and (5), includes only cases decided by judges from conservative and liberal districts, respectively. The variables *convict history* 2-6 indicate the defendant's previous number of criminal record points, as employed by the NC structured sentencing rules (2 = 1-4 points; 3 = 5-8 points; 4 = 8-14 points; 5 = 14-18 points; and 6 = 19 or more points). Further controls: judge dummies, county dummies, prosecution year and offense severity.

between sentencing leniency and county-level liberalness. We include a quadratic term *liberal* $*$ *Bill 41*^{2}, which does not provide evidence in support of our expectation that responsiveness will have a strategic limit. Rather, judges in liberal districts may polarize with their constituents' preferences. The subsetted data we utilize in Column (5)'s specification provides further evidence in support of our claim.

Second, we turn to the results regarding whether judges assigned to more conservative counties sentence more punitively following Bill 41. Column (1)'s linear term, *conservative* $*$ *Bill 41*, is not statistically significant, suggesting that judges in conservative districts may not have changed their behavior.[18] Columns (2), (3), and (4) reveal the strategic nature of the changes judges in conservative counties exhibited in terms of their criminal sentencing. Column (2), which includes a quadratic term for the interaction, reveals two key findings. The first one pertains to the linear *conservative* $*$ *Bill 41* variable, which is significant; when combined with the constitutive term *Bill 41*, it reveals that small increases in county-level conservativeness (i.e. counties that are above, but reasonably proximate to then-Gov. Bush's statewide, county-level mean vote-share, 0.5647) corresponds to more punitive sentences.

Column (2)'s second takeaway pertains to the parabolic relationship we find in the interaction variable's squared term: *conservative* $*$ *Bill 41*^{2}. This effect suggests that judges in increasingly conservative counties begin to sentence less punitively. These results hold across columns (2), (3), and (4), the last of which utilizes a subsetted version of the dataset, with only the conservative counties. We argue that this is evidence of strategic judicial behavior, as judges are not willing to dramatically polarize with their increasingly conservative districts. Judges who are ambitious or have

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[18]It is possible, given the findings with regard to judges assigned to more liberal districts, that, by maintaining prior sentencing practices, judges in conservative districts were nonetheless satisfying their respondents. Further below, we examine this possibility as it pertains to retention of judges in downstream elections.

countervailing preferences to their Bill 41 assigned counties do not necessarily comply with their constituents' preferences.

Although they are not the focus of the analysis, it is worth commenting on the other coefficient estimates, especially as they pertain to spot-checking the overall reasonableness of our findings. We see that settled cases all result in shorter sentences, as do cases resolved by both public defenders or private attorneys, as they are more likely to facilitate a plea or settlement. The coefficients for the defendants' age and age squared are, respectively, positive and negative. Both are highly significant, suggesting that shorter sentences are assigned to the very young and very old defendants. The results also indicate that female defendants tend to receive shorter sentences than males. Moreover, Hispanics tend to be assigned shorter sentences than non-Hispanic Whites, whereas the coefficients associated with African-American defendants are not significant in most specifications. Finally, criminal defendants with longer criminal histories are assigned more punitive sentences. Overall, we regard these findings as comporting with our overall understandings of the justice system.

The analysis in this section abstracts away from two sources of endogeneity problems. First, it only considers cases that resulted in an incarceration conviction. Second, it only addresses the difference between cases that were settled and those that were resolved at trial by incorporating a plea bargain indicator as a control variable in the regression specifications. However, most models of pre-trial negotiations suggest that both the likelihood of a successful plea bargain and that of an incarceration conviction depend on the severity of the trial sentence expected to be assigned by the judge in the event of a conviction at trial.[19] These problems could, in principle, be solved by resorting to instruments for an incarceration conviction and the plea bargain indicator. In practice, however, it is very challenging to obtain these instruments. In

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[19]For a detailed review of pre-trial negotiation models, see Daughety and Reinganum (2012).

Appendix A, we propose a structural approach for dealing with the selection problems, based on the techniques developed by Silveira (2017). The findings from the structural analysis fully corroborate those from the present section.

DISCUSSION

The results above suggest that judges reacted to the passage of Bill 41 by changing their sentencing behavior according to the preferences of voters in their judicial district. These changes have the expected direction, with the judges from relatively conservative districts assigning longer sentences and the ones from relatively liberal districts acting in a more lenient way. The sentencing behavior of judges whose districts are extremely conservative, however, does not seem to be affected by the bill.

Previously, we proposed several explanations for this sort of behavior, one of which is that judges are policy motivated. Under these circumstances, the judges may accept pandering to voters who are sufficiently close to them in the liberal-conservative spectrum since, by doing so, they increase their chances of re-election. But, as the ideological distance between the preferences of judges and voters increases, it may be preferable for the former simply to implement their preferred policies vis-à-vis criminal sentences. The consequences of alienating the electorate are lower chances of re-election.

We now present evidence supporting this hypothesis. Specifically we examine the performance of the 93 judges in office during the passage of Bill 41 in subsequent elections. If it is the case that judges from very conservative districts decided not to pander to voters, then the turnover for these judges should be higher than that of their peers from more liberal districts.

Since Superior Court judges serve eight-year terms, the seats of every judge serving during the passage of Bill 41 were up for re-election at some point between the 1996 and the 2002 judicial elections. In the reminder of this section, we set the unit of

Table 4. Electoral performance after Bill 41

	Probit
$liberal_j$	0.46
	(1.45)
$conservative_j$	7.08**
	(3.33)
$constant$	-0.40*
	(0.23)
Observations	93

Notes: This table reports OLS estimates. The unit of observation is a judge. Standard errors are provided in parentheses; $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. The sample includes all judges active at the moment of the passage of bill 41. The dependent variable, $early\ exit_j$, indicates whether a judge stopped serving before 2002.

analysis to be a judge. Let the dummy $early\ exit_j$ indicate whether judge j served until before 2002.[20] Such a dummy captures the success of judge j in the first election to take place after the passage of the bill.

To verify whether judges from the more extreme districts performed worse than their counterparts from moderate districts in the wake of Bill 41, we consider the following specification:

$$\text{Early Exit}_j = \alpha + \theta_l\,\text{Liberal}_j + \theta_c\,\text{Conservative}_j + \epsilon_j, \qquad (4)$$

where Liberal_j and Conservative_j are defined as in 1. The parameters of interest are θ_l and θ_c. A positive θ_l indicates that, among judges from liberal districts, the turnover following the approval of Bill 41 is higher for judges whose districts are more extreme. Similarly, a positive θ_c indicates that, among judges from conservative districts, judges from extreme districts have higher turnover than those from relatively moderate districts.

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[20]We obtained this information in the annual editions of the North Carolina Manual, which contain judicial directories listing every judge in activity for every year between 1996 and 2009.

Table 4 presents the results of Probit estimation of specification 4. The estimated θ_l is 0.46 but it is not significant at conventional levels. In contrast the estimated θ_c is 7.08 and significant at 5%. Hence distance from the statewide preferences is correlated with judicial turnover following the approval of Bill 41 only in conservative districts. These results are consistent with the evidence that judges from very conservative districts were the ones to react the least to the passage of the bill. Taken together the results from this section and the previous ones suggest that judges from the most conservative districts decided not to pander to voters and, as a consequence, were subsequently punished in the ballot box.

Conclusions

This paper examines how the sentencing behavior of elected trial judges is affected by changes in electoral incentives. With this intent, we explore a unique natural experiment taking place in North Carolina. Bill 41, which passed in 1996, changed the selection method of Superior Court judges from state-wide to district-wide elections. We argue that a direct consequence of the bill was a shock in the preferences of the judges' electorates, and we verify how judges reacted to the passage of the bill.

We provide evidence that judges adapted their sentencing decisions to suit the voters' will. Specifically, judges from liberal districts became relatively more lenient, while those from moderately conservative districts started assigning harsher sentences. However, judges from the most conservative districts did not seem to be affected by the passage of the bill.

Our findings comport with our theory of constrained responsiveness or strategic judicial behavior, which suggests that there is a limit to how mass opinion motivates elected judges. As such, we revisit alternative explanations with regard to the limits of responsiveness—chiefly, that judges (1) are ambitious office-seekers, (2) sincere policy-seekers, and (3) efficiency maximizers. We cannot under-emphasize judges'

policy motivations; we do not find that judges are willing to adapt their sentencing decisions to suit the preferences of voters who are very distant from them in the liberal-conservative spectrum.

In turn, we then explore one possible implication of these results—namely, whether judges' strategic responsiveness, or lack thereof, impacts their chances of re-election. We provide support for this hypothesis by comparing the electoral performance of judges from moderate and extreme districts in the wake of Bill 41. We show that judges from the most conservative districts, which are precisely the ones whose sentencing patterns were not affected by the bill, face lower chances of re-election than their counterparts from liberal and moderately conservative districts.

In this research, we have sought to contribute to the literature that seeks to understand the electoral connections between judges and voters. While it is well-established that variations in electoral institutions leads to disparate policy outcomes, it is not altogether clear that this comports with the expectations we have for objective or impartial courts. At the very least, this research presents a step forward in understanding the fine-grained nature of how judges' exhibit responsiveness to voters. Furthermore, the enduring judicial reform movement results in regular changes in judicial selection and retention. As such, we expect to observe many more systematic variations in how disputes are resolved in court.

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APPENDIX A. DEALING WITH SELECTION: A STRUCTURAL APPROACH

As explained in the main text, our reduced-form analysis abstracts away from two selection issues: First, it only considers cases that resulted in an incarceration conviction. Second, it only address the difference between cases that were settled and those that were resolved at trial by incorporating a plea bargain indicator as a control variable in the regression specifications. However, whether a case settles may depend on the severity of the expected trial sentence, creating a potential endogeneity problem.

In this Appendix, we explore a structural approach for dealing with the selection problems described above. Our strategy is based on the techniques proposed by Silveira (2017). It assumes that, for each case, the data generating process involves a defendant and a prosecutor who bargain over the sentence to be assigned. If bargaining is successful, the case is settled. Otherwise, it proceeds to the trial stage, in which the defendant is found guilty with a given probability. Both the potential trial sentence and the probability of conviction at trial are allowed to vary across cases. The bargaining protocol is take-it-or-leave-it: in every case, one of the bargaining parties offers to settle for a sentence. If the other party rejects the offer, the bargaining stage ends and a trial takes place.

As opposed to Silveira (2017), we do not fully specify a structural econometric model. As we argue in the remainder of the section, it is possible to investigate how the passage of Bill 41 affected the sentencing behavior of Superior Court judges by only partially recovering the primitives of the model. As a consequence, we do not need to take a stance on the identity of the proposal-maker at the bargaining stage or on the nature of the asymmetric information in the model. Accordingly, several of the assumptions that we make below are in terms of high-level objects. The structural

econometric model presented in Silveira (2017), as well as the assumptions on its primitives discussed there, are fully consistent with the analysis developed here.

Assume that each case i is associated with characteristics Z_i, which are observable by the econometrician. These characteristics may, in principle, include the type of the main charge against the defendant, the defendant's demographics, variables related to the date and place of prosecution, etc. In our application below, Z_i consists of measures of the conservativeness of the judge responsible for the case and an indicator of whether the case was resolved after the passage of Bill 41. To every case correspond a trial sentence T_i. Such a sentence is assigned by the judge in the event that the case reaches trial and results in a conviction. We assume that the trial sentence is known by the prosecutor and the defendant at the plea bargaining stage but, from the econometrician's perspective, it is a random variable with a mixture distribution. Specifically, it may assume value zero with positive probability, which we allow to depend on Z_i. The interpretation for T_i is that, in the event of a conviction at trial, a non-incarceration sentence (such as a sentence to probation or community service) is assigned by the judge. Conditional on being strictly positive, T_i is described by the density function $g(\cdot|Z_i)$ with full support over $[\underline{t}, \bar{t}]$.

In every case i a settlement offer is made. Such an offer is represented by S_i, which, for the econometrician, is a random variable. Assume that there exists a strictly increasing continuous function $\tilde{s}(\cdot, \cdot)$ such that $S_i = \tilde{s}(T_i, Z_i)$. In other words, given realizations z_i and t_i of Z_i and T_i, respectively, the realization s_i of S_i satisfies $s_i = \tilde{s}(t_i, z_i)$. Assume that $\tilde{s}(0, Z_i) = 0$ for every Z_i. Under these assumptions, S_i is equal to zero with positive probability (the same probability that T_i is equal to zero) and, conditional on being strictly positive, S_i is described by a density function $b(\cdot|Z_i)$ with full support over $[\tilde{s}(\underline{t}, z), \tilde{s}(\bar{t}, z)]$.

For each case i the trial sentence T_i is only observed in the event of a conviction at trial. Similarly, the settlement offer S_i is only observed if it is accepted—i.e., the

plea bargain is successful. Let Ψ_i denote a random variable indicating the way case i is resolved. Assume $\Psi_i = 1$ when the case is settled and $\Psi_i = 2$ if it results in a conviction at trial. We can then write the density of trial sentences, conditional on a conviction at trial, as

$$g(t|\Psi = 2, Z = z) = \frac{P[\Psi = 2|T = t, Z = z]g(t|Z = z)}{P[\Psi = 2|Z = z]}. \tag{5}$$

Also, we can write the density of settlement offers, conditional on a plea bargain, as

$$b(s|\Psi = 1, Z = z) = \frac{P[\Psi = 1|S = s, Z = z]b(s|Z = z)}{P[\Psi = 1|Z = z]}. \tag{6}$$

Both $g(\cdot|\Psi = 2, Z = z)$ and $b(\cdot|\Psi = 1, Z = z)$ are observed by the econometrician. Moreover the conditional probabilities $P[\Psi = 2|Z = z]$ and $P[\Psi = 1|Z = z]$ are also observed.

Let z' and z'' be two values of Z_i such that

$$P[\Psi = 2|T = t, Z = z'] = P[\Psi = 2|T = t, Z = z'']$$

$$\text{and} \quad P[\Psi = 1|S = s, Z = z'] = P[\Psi = 1|S = s, Z = z''] \tag{7}$$

for all $t \in [\underline{t}, \bar{t}]$ and $s \in [\tilde{s}(\underline{t}, z), \tilde{s}(\bar{t}, z)]$. Then, the following equations hold:

$$\frac{g(t|\Psi = 2, Z = z')}{g(t|\Psi = 2, Z = z'')} \frac{P[\Psi = 2|Z = z']}{P[\Psi = 2|Z = z'']} = \frac{g(t|Z = z')}{g(t|Z = z'')}$$

$$\text{and} \quad \frac{b(s|\Psi = 1, Z = z')}{b(s|\Psi = 1, Z = z'')} \frac{P[\Psi = 1|Z = z']}{P[\Psi = 1|Z = z'']} = \frac{b(s|Z = z')}{b(s|Z = z'')}. \tag{8}$$

The first equation in 8 shows that, using the observed unconditional probabilities of conviction at trial and the densities of trial sentences, conditional on a conviction at trial, we can recover some information on the unconditional distribution of trial sentences.[21] More precisely, we are able to identify the ratio of unconditional densities

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[21]To be sure, when we refer to unconditional probabilities and densities, we mean to condition these objects on Z.

of trial sentences for z' and z''. Similarly, the second equation of 8 shows that, using the unconditional probabilities of successful plea bargain and the conditional densities of settlement offers, we can recover the ratio of densities of settlement offers for z' and z''.

The equations in 8 suggest a method for analyzing how the passage of Bill 41 affected the sentencing behavior of Superior Court judges in a way that accounts for the selection processes determining which cases are settled and which ones result in a conviction at trial. The vector z' can be set to indicate cases that were under the responsibility of a particular judge or group of judges and that were decided before the passage of the bill. Vector z'' can then be set to indicate cases under the same judges that were decided after the bill was approved. The conditional probabilities and densities on the left-hand side of the equations are observed and can be estimated. By examining the ratios of estimates of the conditional densities of trial sentences for z' and z'' (weighted by estimates of the ratios of unconditional probabilities of conviction at trial) we are able to estimate how the passage of Bill 41 affected the unconditional distribution of trial sentences. In the same way, we can estimate how the bill affected the unconditional distribution of settlement offers by recurring to an empirical version of the second equation in 8.[22] Since the trial sentence and the settlement offer of a case are related by the strictly monotonic function $\tilde{s}(\cdot, \cdot)$, the way Bill 41 affected the unconditional distribution of settlement offers is very informative of how it affected the sentencing behavior of judges.

The estimation of the conditional densities of settlement offers and trial sentences can, in principle, be done by kernel. But the supports of the distributions of trial sentences and settlement offers are bounded. Standard kernel density estimation

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[22]Of course simply employing the weighted ratios of conditional densities only identifies the ratios of unconditional densities of sentences and settlement offers. To independently obtain the unconditional distributions of trial sentences and settlement offers, a full structural estimation procedure, as the one implemented by Silveira (2017), is needed.
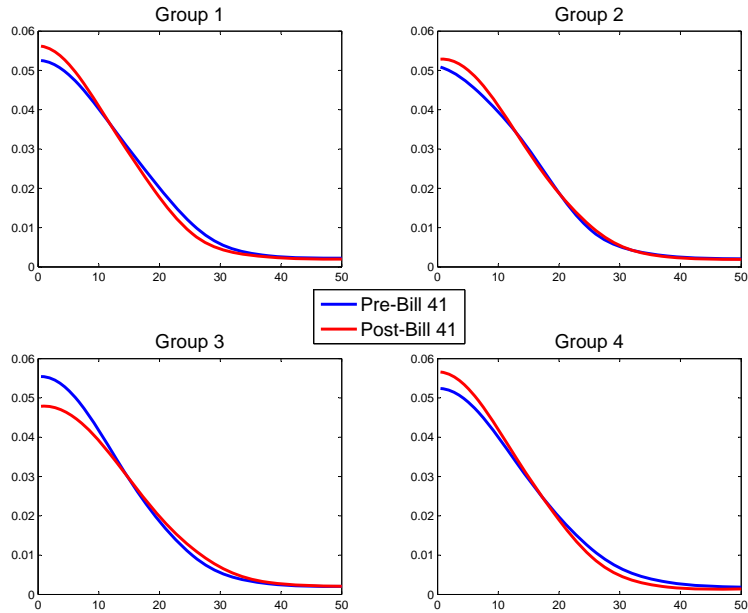
techniques result in inconsistent estimates near the boundaries of the support. To overcome such a problem we employ a boundary correction strategy proposed by Karunamuni and Zhang (2008).[23]

Since the vast majority of the cases in the sample are resolved by plea bargain, we focus our analysis on estimating the ratio of densities of accepted settlement offers before and after the passage of Bill 41. Due to the sample size, these densities are much more precisely estimated than those of trial sentence and, as argued above, they are still informative on the sentencing behavior of the Superior Court judges.

Figure 4 depicts the ratios of estimated densities of settlement offers before and after the passage of Bill 41 for four groups of judges. As described earlier, the judges are divided according to the conservativeness of their districts. The groups are numbered from the least to the most conservative, as implied by the Republican vote share in the 2000 Presidential elections. The plots on the top left and top right, which respectively refer to groups one and two, indicate that cases under judges from liberal districts started being settled for more lenient sentences after the passage of the bill. For settlement offers shorter than about ten months, the densities referring to the period after the passage of the bill are above those referring to the period prior to the bill. For settlement offers between ten and about 40 months, the order of the density functions switches. In contrast, the plot on the left bottom of figure 4 indicates that cases under judges from group three, whose districts are moderately conservative, settled for harsher sentences after Bill 41 passed. For settlement offers shorter than about 15 months, the density referring to the period after the bill is below that referring to the post-bill period. The order of the density functions is the inverse for settlement offers between 15 and about 40 months. The plot on the bottom left of the figure depicts cases under judges from group four, whose districts are the most

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[23]Away from the boundaries, bandwidth selection follows Silverman's "rule-of-thumb" (Silverman, 1986). Near the boundaries, a modified bandwidth is employed. The kernel function is the tri-cube. See Karunamuni and Zhang (2008) for details.

Figure 4. Reaction to Bill 41—Four groups of judges

conservative ones. This plot is very similar to the plots on the top of the figure, which refer to judges from the liberal districts. It indicates that cases under judges from extremely conservative districts started being settled for more lenient sentences after the passage of Bill 41.

A simple way of assessing the statistical significance of these results is to recur to two-sample Kolmogorov-Smirnov tests comparing the distribution of settlement offers made before and after the passage of Bill 41 for every group of judges. Table 5 presents the results of such tests. The null hypothesis is that the sample of pre-Bill 41 cases is drawn from the same distribution as the that of post-Bill 41 cases. The null is strongly rejected for groups one, two and four. For group three, the null is rejected at a confidence level of 5.57%.

Table 5. Kolmogorov-Smirnov tests: results per district group

	Group 1	Group 2	Group 3	Group 4
p-value	0.01%	0.00%	3.97%	0.02%

Notes: This table reports the results of Kolmogorov-Smirnof tests of equality of the settlement sentence distributions before and after the passage of Bill 41. The null hypothesis is that the two distributions are the same. The tests are conducted separately for cases decided by judges from district groups one (most liberal) to four (most conservative).

The plots in figure 4 confirm the findings from the reduced form analysis. They suggest that, after the passage of Bill 41, judges from liberal and moderately conservative districts adapted their sentencing behavior to suit the preferences of their new electorate. These shock in sentencing patterns resulted in a change in the distribution of settlement offers, as captured by figure 4. Judges from the most conservative districts, however, did not alter their sentencing behavior in a way that was consistent with their voters' will. Instead, figure 4 suggests that these judges become more lenient after the bill passed. These results, therefore, corroborate the findings from the regression analysis presented in the main text.