Responsive Justice?

Retention Elections, Prosecutors, and Public Opinion

Abstract

Research on the effects of public opinion on judicial decisionmaking has focused on salient cases decided by courts of last resort, yet the majority of cases in the judicial system are low salience and decided by trial court judges. Determining trial judge responsiveness to public opinion is difficult because prosecutors, who often have an electoral connection to the public, shape judges dockets and may also respond to public opinion. In this paper I use a unique measure of public opinion—votes on an initiative to legalize marijuana—to examine the responsiveness of trial court judges and prosecutors to a strong, issue-specific, constituency-level signal of public opinion. I find that, at least in recent drug cases in Colorado, both judges and prosecutors responded to public opinion after they were provided with information about their constituents' wishes. Moreover, judges only became congruent with public opinion after constituent preferences were revealed.

A central question in the study of democratic government concerns the conditions under which political officials make decisions congruent with public opinion. Judicial politics research has established that judges are affected by public opinion regardless of the institution used to select or retain them (Casillas, Enns, and Wohlfarth 2011; McGuire & Stimson 2004; Caldarone, Canes-Wrone, and Clark 2009; Canes-Wrone, Clark, and Park 2012).[1] However, most of the empirical evidence that has been assembled about the effects of issue-specific public opinion on judicial behavior has examined only the most salient cases decided by judges, and most of the scholarly attention on this issue has focused on courts of last resort. However, these courts only decide a small percentage of all disputes, and the vast majority of cases resolved by judges are not the types of high profile issues—like the death penalty and abortion—that have been the focus of most scholarly attention. Thus, while we have a great deal of evidence that high court judges follow public opinion in the sorts of high profile cases that are likely to cause elected judges problems during their campaigns, we know less about judicial behavior in routine cases that are less likely to mobilize the opposition in the next election.

Indeed, some scholars have gone so far as to suggest that we should not expect judges to be responsive to public opinion in low profile cases (Cann and Wilhelm 2011). In this article, I challenge that claim. Examining judicial decisionmaking at the very bottom of the judicial hierarchy, I argue that even judges who face a very low probability of defeat respond to public opinion when deciding routine cases; however, their ability to reflect the will of their constituents in their decisions is facilitated by access to high-quality, constituency-level information about citizens' preferences.

Determining the responsiveness of local judges to public opinion is a difficult task because local prosecutors–who, themselves, are often elected—curate the cases heard by a judge through their decisions about which charges to take to trial and which charges to dismiss. A failure to account for prosecutorial discretion means that researchers are unable to rule out the possibility

[1]There are some notable exceptions. For example, Giles et al. (2008) and Norpoth & Segal (1994) present evidence that U.S. Supreme Court judges are not affected by public opinion.

that what appears to be responsiveness on the part of judges to public opinion may actually be a spurious effect created by the responsiveness of prosecutors, rather than judges, to the will of the people. Data limitations have forced extant research to eliminate prosecutorial discretion indirectly rather than modeling the determinants of prosecutorial discretion directly.

In this paper, I use a unique measure of public opinion—votes on an initiative to legalize marijuana in Colorado—as well as data on the decisions of both prosecutors and judges to assess their responsiveness to a strong, issue-specific, constituency-level signal of public opinion. I find that, at least in recent drug cases in the state of Colorado, both trial court judges and prosecutors exhibit responsiveness to public opinion. However, the effects of public opinion on decisionmaking are only seen after the actors are provided with information about public preferences. Moreover, while judges were incongruent with public opinion before the initiative, they became congruent with public opinion after the initiative. While prosecutors moved toward congruence after the initiative, their decisions remained incongruent with public opinion after the initiative.

This article departs from previous research in three ways. First, while recent studies (Huber and Gordon 2004; Gordon and Huber 2007) have demonstrated that the presence and timing of judicial elections affects the sentences that judges issue, these studies do not include issue-specific public opinion in their empirical analyses. Because the opinions of those citizens which comprise a constituency may vary statewide, a sentence that is popular in one region of the state may be unpopular in another region. By controlling for constituency- and issue- specific public opinion, we can clarify the extent to which judges respond to the opinions of those who have the power to remove them from office. Second, prior research has assumed that judges' dockets are randomly selected (Kastellec and Lax 2008); these studies do not account directly for the fact that prosecutors, through the exercise of their discretion over which cases to pursue, which charges to dismiss, and which cases to plea bargain, shape the types of cases that judges hear. Without accounting for the fact that prosecutors—who also stand for election—might also respond to public opinion, prior research cannot eliminate the possibility that what appears to be judicial responsiveness to public opinion is merely a downstream consequence of a prosecutor's responsiveness to public opinion.

Finally, when compared with judges—even local judges—prosecutors are vastly understudied. By modeling the determinants of a prosecutor's decision to dismiss a charge, this paper presents some of the first empirical evidence about the forces which affect the use of prosecutorial discretion.

Judges, Prosecutors, and Public Opinion

Scholars have produced a variety of evidence that public opinion affects judicial decision-making. At the federal level, scholars have argued that judges attempt to follow public opinion in an attempt to bolster their legitimacy. Rather than assessing the U.S. Supreme Court's congruence with public opinion, these studies generally assess the extent to which the liberal or conservative nature of the Court's decisions coencide with changes in the general ideological leanings of the public. Taken as a whole, these studies indicate that public opinion affects federal judicial policy (Epstein & Martin 2010; Clark 2011; McGuire & Stimson 2004; Flemming and Wood 1997, but see Giles et al. 2008; Norpoth & Segal 1994).

At the state level, there are reasons other than legitimacy to expect that judges will heed public opinion. Because the vast majority of state judges must face voters to retain their seat on the bench, the presence of an electoral connection provides additional incentives for judges to follow public opinion. At the state level, scholars—typically relying on issue-specific rather than general measures of ideology—have demonstrated that elected judges are generally responsive to the will of the people (Brace and Boyea 2008). Still, the type of retention institution used to keep judges in office conditions the magnitude of the effect; Caldarone, Canes-Wrone, and Clark (2009) show that judges retained through nonpartisan elections are more likely than their counterparts who are reliant upon partisan elections to issue abortion decisions that comport with public opinion.

Yet, the bulk of this research has been limited to high-profile constitutional cases decided by state supreme courts. To best test the theory that responsiveness exists even in cases that are unlikely to cause a judge future electoral problems, it is necessary to turn away from hot-button constitutional cases and move down the judicial hierarchy; after all, the more prominent the court and the more high-profile the case, the more likely that organized electoral opposition will assem-

ble. Indeed, some scholars have argued that, while judicial elections, particularly those that are contestable, may encourage judges to respond to public opinion in salient cases, we should not expect to see responsiveness to public opinion by judges who have little reason to fear electoral defeat and are deciding routine, nonsalient caess. For example, Cann and Wilhelm (2011) argue that

> Where circumstances are such that state supreme court judges seek reelection, citizens have enough awareness of a case that they have stable preferences, and judges know that citizens have such preferences and could use them at the ballot box, we tend to see significant judicial responsiveness to public opinion. These conditions are well met for highly visible cases heard by judges who face contestable elections. However, for a much larger proportion of cases that are either less visible or are heard by judges who do not face contestable elections, there is little reason to expect judicial responsiveness.

Thus, the existing literature on state judicial decisionmaking provides some suggestion that the electoral connection may be weakened—or even absent—at the bottom of the judicial hierarchy, when judges are deciding less salient cases, or when the elections judges face are uncontestable.

The literature on congressional elections and legislative politics suggests a different theory. Indeed, while a vast literature in congressional politics has noted that incumbent representatives typically have at least a 90% chance of reelection, congressional scholarship still operates under the assumption that the possibility of an electoral defeat colors nearly every aspect of congressional politics (Cox and Katz 2002; Stonecash 2008; Jacobson 2009; Mayhew 1974). Arnold (1993), for example, argues that, because legislators are uncertain about which issues will mobilize citizens, they act carefully on all issues.

This logic applies equally to judges who rely upon elections to retain their seat on the bench. State judges decide a mixture of salient and nonsalient issues every year; if their election is contestable, they do not necessarily know which decision (or mixture of decisions) will induce a serious challenger to enter the race and drive public support for that challenger the next time the judge is on the ballot. In an effort to increase the likelihood that they will be retained, therefore, I suggest that judges should, as Arnold (1993) suggests, vote carefully on all issues (not just high profile cases) out of a desire to "create [an] acceptable voting record[] one issue at a time" and

thereby ease the road to reelection (411).

The theory extends even to those judges who are retained using uncontestable retention elections. By definition, these judges are not concerned about facing a challenger when they next seek retention, and the historical record illustrates clearly that judges retained through uncontestable retention elections are, in most cases, unlikely to lose their seat on the bench (Aspin 2011). Still, the presence of these elections affects judicial behavior; Traut and Emmert (1998) show that public opinion plays a role in the calculations of California Supreme Court justices' death penalty decisions even though those judges merely face uncontested retention elections. Likewise, Canes-Wrone, Clark, and Park (2012) demonstrate that, at least in abortion cases, retention elections induce a responsiveness to public opinion that mirrors that demonstrated by judges who face contestable elections. Indeed, while they will never face an opponent on the ballot, these judges still may face an organized opponent urging voters to oppose their retention. Indeed, in those cases in which voters have declined to retain jurists (e.g. the 1986 campaign against Rose Bird and her colleagues, the 1996 campaign against Tennessee's Penny White, and the 2010 campaign against three justices of the Iowa Supreme Court), interest groups mobilized voters using claims that jurists' decisions diverge from public opinion (Schotland 2011; Wold & Culver 1987). Indeed, Gibson (1980) finds that judges who face retention elections who know of other judges who have not been retained in office are more likely to be influenced by the political environment in their sentences. In other words, even though their opponent will never appear on the ballot, judges know that interest groups may work to inform voters about their unpopular decisions, and those actions may hurt their chances of retention. If the presence of an election induces judges to listen to the public, then any election—even a retention election—may lead judges to respond to public opinion, and, contrary to Cann and Wilhelm's (2011) theory, that responsiveness should be evident even in nonsalient cases.

Some literature has addressed the effect of public opinion on the decisions of local judges deciding nonsalient cases, though this work has predominantly looked at the effects of public opinion indirectly by examining the conditions of the electoral process or the effects of the electoral

cycle. While some research indicates that judges who will never face a challenger on election day are less responsive to public opinion than judges who may face challengers at the polls (Gordon and Huber 2007), other research suggests that even the presence of an uncontestable retention election may induce a judge to heed public opinion. Indeed, Huber and Gordon (2004) show that, as the date of their election approaches, Pennsylvania judges who face retention elections issue more punitive sentences.[2] Finally, Kuklinski and Stanga (1979) find that the sentencing behavior of California trial courts changed drastically after a failed 1972 marijuana legalization initiative in California. In particular, comparing the aggregate sentences produced by each county Superior Court before and after the election, Kuklinski and Stanga find that courts whose constituents favored the legalization of marijuana sentenced defendants more leniently.[3]

However, most of the existing literature assumes that the cases judges decide are randomly selected; it does not account for the fact that, in many cases, dockets are either selected by the

[2]See Berdejó and Yuchtman (2012) for a similar finding for trial court judges who stand for contestable nonpartisan elections.

[3]A focus on trial court judges retained through uncontestable retention elections has an added benefit: generalizability. As Nelson (2011) reports, less than one-quarter of general jurisdiction trial court judges who face contestable partisan or nonpartisan elections appear on the ballot with a challenger in the general *or* the primary election; indeed, Nelson shows that, in some states, judges need not ever appear on a ballot to be reelected. Thus, while a focus on retention elections gives theoretical leverage with respect to the purported limited effect of public opinion and the low probability of defeat, it also presents an institutional context generalizes to general jurisdiction trial court judges more widely: like their counterparts who face retention elections, trial court judges who face contestable elections aren't likely to face another candidate as they seek to keep their seat on the bench. In other words, an understanding of the behavior of lower court judges who rely on retention elections may help to untangle the effects of public opinion on other trial court judges who face elections since the probability that *any* trial court judge will face a challenger in the next election is comparatively low.

judges themselves (in the case of the U.S. Supreme Court) or by another political actor. Ignoring these selection effects may lead to biased conclusions (Kastellec and Lax 2008; Priest and Klein 1984). If prosecutors, who are also often elected, are simultaneously responding to public opinion, then what appears to be judicial responsiveness to public opinion may actually be an artifact of the decisions that prosecutors make with respect to the cases that are dismissed, those that are plea bargained, and those that go to trial. Where scholars have attempted to address the effects of prosecutorial discretion, the lack of available data has prohibited a direct exploration of the determinants of prosecutors' use of discretion. Previous attempts at eliminating the effects of this discretion have proceeded by noting differences in the electoral calendars of judges and prosecutors and through the use of fixed effects rather than by exploring the possibility that the decisions of prosecutors are affected by the same factors as judges and, because they make the decision to dismiss a case before a judge has the opportunity to sentence, their actions can manipulate the composition of a judge's caseload (Huber and Gordon 2004; Gordon and Huber 2007).

While only a very small literature (Pritchard 1986) has examined the effect of public opinion on prosecutorial discretion, extant research provides reason to believe that public opinion may sway prosecutorial decisionmaking. Indeed, according to the authors of a prominent criminal procedure textbook,

> the prosecutor occupies the most powerful position in the [criminal justice] system by virtue of control over who is prosecuted and who is not... The prosecutor's broad discretionary power is subject to one... restriction. Under our system of checks and balances, the prosecutor may always risk rejection by the electorate at the next election (Haddad et al. 2008, 903).

This electoral connection gives district attorneys an incentive to heed the will of the people if they wish to remain in office; if elected district attorneys fail to follow public opinion, they risk losing their job. By modeling prosecutors' use of discretion directly, one can observe whether prosecutors are congruent with (or responsive to) public opinion and the extent to which the effects (if any) of public opinion on prosecutors have downstream (and potentially biasing) effects on the effects of public opinion on judges. Moreover, by examining the use of prosecutorial discretion directly, this

paper presents some of the first empirical evidence on the determinants of prosecutorial discretion.

Research Design

A focus on the effect of public opinion local judges and prosecutors as they act on non-salient case presents a series of theoretical and empirical challenges. First, how does one estimate constituency-level public opinion at the local level? As scholars have often noted, it is difficult to estimate the preferences of the comparatively small groups of citizens who vote on a given judge or district attorney (Erikson, Wright and McIver 1993; Lax and Phillips 2009). When applied to the local level, the lack of available data becomes even more severe; even if a representative survey of a state is conducted, it is rarely representative at the county level. Without a sufficient number of respondents in each county, even the survey-based aggregation and weighting techniques used to remedy the lack of available data at the state level cannot estimate local public opinion (Lax and Phillips 2009).

Second, if we are able to estimate constituency-level public opinion, how do we know that judges had access to this information? Indeed, while scholarly work on the effect of public opinion on is quick to note the methodological challenges inherent in estimating public opinion, scholars provide little attention to the question of how judges learned about public opinion. After all, if representative information on public opinion is difficult for scholars to obtain, judges and prosecutors likely face similar difficulties obtaining accurate information about constituent preferences. From a scholarly perspective, ascertaining the effect of public opinion on decisionmaking is difficult because, in the absence of first-hand contact with decisionmakers (Gibson 1978; Hall 1987), it is nearly impossible to discern how elected officials learn about public opinion.

To alleviate these problems, I, following Kuklinski and Stanga (1979), leverage the fact that local politicians in some jurisdictions do have access to some widely-available, disaggregated public opinion data: the results of ballot initiatives. By examining publicly-available election returns on these issues, local politicians have access to public opinion data at the precinct level. The

election results are exemplary signals of public opinion for local politicians for a number of reasons. First, initiatives are put on the ballot *by* members of the public acting either in their capacity as citizens or as members of an organized interest group; an initiative only reaches the ballot once it has some baseline level of public support as demonstrated by the signatures necessary to put the issue on the ballot (Kuklinski and Stanga 1979). Second, the sample is nonrandom. Typical surveys poll a random sample of the population and ask their opinions on issues; yet, a variety of research suggests that the segment of the public that votes on election day is not a random sample of the population (Wolfinger and Rosenstone 1980). The citizens who vote in initiatives, by contrast, are those constituents who are motivated enough by a particular issue that they are willing to go to the polls and make their voice heard. Elected politicians who have a desire to remain in office have a clear incentive to follow these election returns; if they make decisions contrary to the expressed opinions of those who are mobilized by the issue to go to the polls, they risk losing their jobs if they make decisions contrary to public preferences. Third, the questions which appear at the polls are often limited to a single issue dimension; rather than requiring a reliance on a general liberal-conservative dimension, initiatives provide issue-specific measures of constituent support for a particular proposition. Fourth, the results of these initiatives are widely-publicized in the media, and county-by-county breakdowns of the vote are freely published online by media outlets and state elections offices. This publicity allows judges easy access to information about constituent preferences.

The use of a citizen initiative has another valuable inferential feature: it provides us the opportunity to examine responsiveness to public opinion before and after elites receive the public opinion signal from their constituents.[4] By examining the effect of public opinion before the initiative, one can determine whether or not, in the absence of a strong signal of constituency opinion, judges and prosecutors are congruent with public opinion. Likewise, by examining the effect of

[4]This approach assumes that public opinion remains constant over time. While this topic has been hotly debated in the literature, an array of important works in the field (e.g. Erikson, Wright and McIver 2003; Brace et al. 2002) rely on this assumption. As such, I adopt it, as well.

public opinion after the initiative, one can determine whether or not public opinion plays a statistically discernable role in judicial decisionmaking once judges receive a strong signal of their constituent's opinions. By comparing the effect of public opinion before and after the initiative vote, one can determine the extent to which providing judges with information about their constituents' opinions leads them to change the severity of the sentences they issue.

While Kuklinski and Stanga's use of initiative results to measure public opinion is useful, there are limitations to their empirical analysis. First, they are unable to control for the effects of legal factors in sentencing decisions. Kuklinski and Stanga's dependent variable is a sentence severity score that provides a quantitative representation of the severity of the punishment given to the defendant. This score is the average severity of all of the sentences handed down by a court in a given year without regard to the charge for which the sentence was given. This approach treats sentences for the possession of one ounce of marijuana the same as one for the possession of a large quantity of marijuana without accounting for the fact that the expected sentence for the smaller amount of marijuana is lower. Without controlling for differences in the severity of the cases, Kuklinski and Stanga cannot address the possibility that what appears to be responsiveness may be a reaction to two different pools of cases. If the average case decided by the judges in a county before the initiative is less severe than the average case decided after the election, what may appear to be a move to more punitive behavior on the part of judges may actually be the result of judges responding to the legal factors in the case rather than public opinion.

Second, Kuklinski and Stanga's unit of analysis is not ideal. By analyzing the county-year, they mask the possibility that large individual-level change may occur over time. By averaging the sentences handed down by the dozens of judges in Los Angeles county, for example, they eliminate the possibility that individual judges may be more or less susceptible to public opinion. This is particularly problematic because judicial sentences are handed down by *judges*, not *courts*; therefore, the effects of public opinion are felt by *judges*, not *courts*.

The Judicial Politics of Marijuana Sentencing in Colorado

The initiative that forms the basis for the analysis appeared on the Colorado general election ballot in 2006. The ballot asked "Shall there be an amendment to section 18-18-406 (1) of the Colorado revised statutes making legal the possession of one ounce or less of marihuana for any person twenty-one years of age or older?" (Colorado Secretary of State 2007). The proposal failed at the polls; only 41% of voters supported the measure. Still, participation in the election was very high; 97.8% of voters who returned a ballot in the 2006 general election cast a vote on the measure. Moreover, total turnout in this election comprised nearly two-thirds of all active Colorado voters (Colorado Secretary of State 2007).[5]

Support for the initiative varied widely by county.[6] In Kiowa and Washington counties, only 17% of voters supported the ballot measure; conversely, in Pitkin and San Miguel counties, over 70% of voters supported it (Colorado Secretary of State 2007). Figure 1 provides a visual account of the vote on the ballot proposition. The figure indicates that support for marijuana legalization was very low in western Colorado while citizens in central and southwestern Colorado were most likely to support the initiative.

[Figure 1 about here]

[5]Criminal law provides an ideal opportunity to test judicial responsiveness to public opinion because it is an area of law where the public's preferences are directly implicated. After all, "[o]ne characteristic of criminal cases is that they are brought in the name of the government on behalf of the community" (Saltzberg and Capra 1996, 2). In other words, criminal cases represent an affront to the shared norms and values of the community; the mass public is a party to each of these cases, at least theoretically.

[6]Turnout varied very little by county; roll-off varied from 11% in Costilla county to less than 1% in ten counties. The total number of voters who cast votes in the initiative was a majority of registered voters in all but two counties, and it was greater than 49% of registered voters in all counties.

The state of Colorado uses a two-tiered system of trial courts. Each of the 64 counties in the state has its own county court (staffed by one or more judges), while counties are grouped into 22 judicial districts. Each judicial district contains a district court. While the two courts have overlapping jurisdiction in some cases, district courts tend to have jurisdiction over more severe crimes while county courts generally hear less severe cases (Rottman and Strickland 2006). Appeals from these courts are heard by the statewide Colorado Court of Appeals.

Colorado has 22 district attorneys. District attorneys in the state have jurisdictions coterminous with district judges; each judicial district elects a district attorney who oversees cases in both the county and district courts under her jurisdiction. District attorneys are elected in partisan elections to a term of office lasting four years.[7] Colorado's district attorneys are term-limited; they can serve no more than two full terms in office. Elections to select district attorneys are held in November, so district attorneys appear on the ballot at the same time as judges. However, because the lengths of the terms served by district attorneys and the judges in their districts are different, the terms of judges and district attorneys are staggered.

Data

The data for this study come from records kept by the Colorado Judicial Bench. I requested and received data on cases filed in the Colorado County Courts and the Colorado District Courts regarding alleged violations of Colorado Code Section 18-18-406 (titled "Offenses relating to marijuana and marijuana concentrate") between 2004 and 2009.[8] Because I am interested in the responsiveness of judges to the initiative, the empirical analysis in this section is limited to those

[7] If a district attorney retires, resigns, or dies in the middle of his term, a replacement is appointed by the governor to fill the remainder of the term.

[8] The data received from the Colorado Judicial Branch do not include cases heard by the Denver County Courts as these courts are under the purview of the City of Denver and not the Colorado Judicial Branch. Given that fact, as well as the fact that Denver residents passed a ballot measure extremely similar to the one rejected statewide in 2006, in 2005, Denver cases would not be compa-

judges who sentenced defendants both before and after the initiative.[9] The data contain over 9,000 sentences decided by judges who served in both time periods.

The construction of the dependent variable necessary for the analysis of judicial behavior presents a difficult measurement problem. In drug cases in Colorado, judges have the authority to give sentences with a number of different components; they can require defendants to pay fines, perform community service, to go to jail, or they can sentence defendants with a number of other penalties. As is customary in this type of sentencing research (Cook 1977; Gibson 1978; 1980; Kritzer 1979; Kuklinski and Stanga 1979), the dependent variable for the initial analysis of judicial behavior is a `sentence severity scale`. This scale solves the problem of "different" sentence types by assigning a numerical point value to each portion of the sentence, and the total sentence severity can be determined by summing each portion of the sentence given to the defendant.[10] To construct the dependent variable, I updated the scale utilized by Cook (1977), Gibson (1978; 1980), Kritzer (1979), and Kuklinski and Stanga (1979) and printed in the 1968-1970 edition of *Federal Offenders in U.S. District Courts*. Since the Colorado criminal justice system uses punishment types (e.g. placement in community correctional facilities) that were not utilized by the federal district courts at the time the sentencing scale was created, I incorporated "new" sentencing types in the scale using information provided by the Colorado Department of Corrections and the Colorado Judicial Branch. More information on the sentence severity scale, including the point values assigned to each type of offense, is contained in Appendix A. Because the dependent variable is a nonnegative integer, I use Poisson regression and adjust, as necessary, for the slight

rable with those in from the rest of the state because of differences in the law (Colorado Secretary of State 2007). To ensure comparability, cases from Denver are not included in the analysis.

[9]The analysis in this section was also conducted for all judges. While the empirical results are unchanged, the slope of the marginal effect is less steep. This indicates that those judges who only served before or after the initiative "dilute" the results, though the relationship still holds.

[10]For example, a defendant sentenced to two years of supervised probation, a fine, and a year of imprisonment would have a sentence severity of 2+0+5=7 in the data.

overdispersion that is present in the outcome variable (Gelman & Hill 2007).

I estimate a separate model to explore the determinants of prosecutorial discretion. Of course, district attorneys do not sentence defendants; their decision is a dichotomous decision to dismiss a charge or to require the defendant to be tried on this charge. The dependent variable in that analysis is the district attorney's decision to `dismiss` a drug charge. Given the dichotomous nature of this variable, logistic regression was employed.

To assess the effect of the initiative on elite behavior, the model includes an interaction term between `initiative support`, a continuous variable representing constituent support for the legalization of marijuana from the 2006 initiative, and a variable indicating if the case was heard after the initiative (`post-initiative`).[11] With this interaction term, it is easy to assess responsiveness. If elites are responsive to the wishes of their constituents, then we should observe a marginal effect which indicates that judges or prosecutors in more conservative districts acted more conservatively while more those in liberal districts acted more liberally.

Of course, this interaction cannot account for the potentially confounding effects of the actor's own ideology and the general ideology of constituents. What may appear to be the effect of the initiative may be due to the public's overwhelming liberal (or conservative) tendencies or an actor's own ideological proclivities. To eliminate these possibilities, I included the percentage of the vote won by the Democratic candidate in the closest U.S. presidential election (`Constituent Ideology`) and another variable (`Republican`) to indicate whether the judge was appointed by a Republican governor or if the prosecutor ran for office as a Republican.[12] The predictions from

[11] Appendix C contains a table with coding rules for all explanatory variables used in this paper.

[12] Ideally, one could use a measure of constituent ideology derived from survey results; however, no poll in the Roper iPoll archive which codes the respondent's county of residence contains even a single respondent from every county in the state. As a result, this type of measure cannot be used in this circumstance. Additionally, it would have been ideal to have measures similar to the Martin-Quinn (2002) or PAJID (Brace, Langer and Hall 2000) scores to utilize as a measure of ideology. However, no such measure exists for local-level actors; the best available measure is the

both variables are straightforward: judges appointed by Republican governors should sentence defendants more harshly in marijuana cases, and judges with more conservative constituencies should, likewise, sentence judges more harshly. Likewise, Republican prosecutors should be less likely to dismiss a given charge.

Of course, legal factors should also matter; Colorado statutes place bounds on acceptable and unacceptable sentences for a given crime. To control for variation in the severity of the offense, I include indicator variables denoting whether or not the charge is a `felony`, a `misdemeanor`, or a `petty offense`. As Colorado law demands, as the severity of the charge decreases, the sentence should, on average, decrease. To further control for statutory effects, I include a variable indicating whether the case was heard in `district court` instead of county court. While the Colorado district and county courts have some overlapping jurisdiction, harsher cases are heard in district court while less severe cases are heard in county court (Rottman and Strickland 2006). Indeed, the mean sentence severity score for cases heard in District Court in the data is 8.94 while the mean severity score for cases heard in County Court is 3.89. I expect that judges give harsher sentences to defendants whose cases are heard in district court and, due to workload considerations, prosecutors should be more apt to dismiss charges from county court.

Additionally, following the criminology literature on the potential importance of defendant characteristics in the criminal justice system (e.g. Steffensmeier, Ulmer, and Kramer 1998; Unnever, Frazier, and Henretta 1980; Curran 1983), I include the defendant's `age`, `gender`, and `minority status` in the model, as well. I expect that younger, nonwhite, and male defendants should receive higher sentences and should be less likely to have their charges dismissed.

The empirical models concerning prosecutorial discretion contain two additional explanatory variables. First, I include a variable for the total `number of charges` in a case. I expect that, as the number of charges in a case rises, the likelihood that one of those charges will be dismissed also rises (after all, dismissing the only charge in a case is akin to dismissing the

political party of the appointing governor for judges and the district attorney's political affiliation (as listed on the ballot), so I employ those measures.

case entirely). Second, because term-limited individuals may behave differently once they achieve lame-duck status, I include a variable to indicate whether the district attorney is in her `second term` and is, because of term limits, unable to serve another term. Colorado judges are not term-limited, so this variable is not necessary in the judge model.

Readers may note that the model does not contain data on the defendant's prior criminal history. Records of the defendant's history with the legal system are not kept by the Colorado judicial system and were, as a result, unavailable for this study. However, Appendix D contains the results of a series of analyses conducted to provide evidence that the omission of this variable does not bias the empirical results presented in this paper. First, the Appendix shows that the empirical results discussed elsewhere hold when the analysis is restricted to those cases which are least likely to involve career criminals: the possession of less than an ounce of marijuana. Second, because it is only need to control for a variable in an empirical model if it is correlated with both the dependent variable and the key independent variable of interest. In this article, the key independent variable of interest is the interaction between `post-initiative` and `initiative support`. Appendix D contains empirical results demonstrating that no demographic variable (gender, minority status, or age) has an effect which is different before and after the initiative.

Given that some aspects of cases, such as the charge and characteristics of the defendant are specific only to the case at hand while others (the judge's or district attorney's response to public opinion and her ideology, for example) are characteristics of the political actor, the data exist at multiple levels of analysis. To appropriately model effects at different levels of analysis while correctly partitioning the variance explained by each level, I used the `lmer` package in `R` to estimate models containing random effects for both the judge (or district attorney) that decided the case (or had the opportunity to dismiss the charge) and the district in which they work (Gelman & Hill 2007).[13] As such, in addition to modeling the effect of public opinion at the correct unit

[13]To account for the fact that some judges were promoted from county court to district court during the time in the data (and therefore received new constituents and perhaps adopted new sentencing tendencies to accommodate the new types of cases they were hearing), the model includes

of analysis, these models can account for sentencing tendencies that are common among judges (or district attorneys) in the same judicial district while simultaneously accounting for individual judges' (or district attorneys') tendencies to be more or less lenient than the average judge.

Results and Discussion

Table 1 displays the results of the multilevel Poisson model with standard errors corrected for the slight overdispersion present in the outcome variable (Gelman & Hill 2007, 115).[14] As a check of facial validity, the estimated coefficients for the severity of the case indicate that, as required by law, the severity of a sentence decreases with the severity of a crime; defendants who committed misdemeanors and petty offenses receive lighter sentences than those who committed felonies. After controlling for the severity of the case, the model indicates that district court judges are not more severe than their counterparts in county court.

[Table 1 about here] [Figure 2 about here]

Turning to the effect of the initiative, the coefficient for initiative support given in Table 1 indicates the effect of public opinion before the initiative because it is a constituent term to the interaction. The fact that the variable is not statistically significant indicates that, before the initiative, public opinion played no statistically discernable role in the judicial sentencing calculus. Graphical evidence (Figure 2) is helpful to determine the effect of the initiative before and after the initiative. In the figure, the black line plots the expected difference for petty offenses before and after the initiative. As Figure 2 shows, judicial behavior underwent a statistically significant change after the initiative. Plotting the difference in expected sentences of the initiative across the range of initiative support, Figure 2 shows that, regardless of their constituents' position on the initiative, judges (except those in the most liberal areas) responded to the electorate's unwillingness to legalize marijuana; the figure illustrates that nearly all judges exhibited a statistically significant change

a random effect for their cases on the district court and another for their cases when they served on the county court.

[14]The estimated overdispersion parameter is 1.2.

toward harsher sentences after the initiative.[15]

The extent of the change is not constant across judges. The negative slope of the marginal effect indicates that, as constituents' support for the legalization of marijuana increases, judges become increasingly lenient. This effect is statistically significant. Additionally, while not readily apparent from the table, initiative support becomes a statistically significant and correctly-signed predictor of sentencing after the initiative.[16]

How meaningful is the effect of constituent support for marijuana legalization on the sentences handed down by judges? When discussing the substantive size of these results, one must remember that there are statutory limits on judicial discretion; obviously, a judge cannot give a defendant a lifelong prison sentence for a single charge of less than an ounce of marijuana. Rather, the variation allowed in these sentences, by law, is very small. Thus, while the numerical size of the coefficients presented in the table may appear small, one must remember that the magnitudes are a function of the scaling of the dependent variable.

The effects are substantively important. For the least severe cases in the dataset—petty offenses—the difference in expected time periods between the two time periods is small; the expected difference in sentence amounts to up to an additional year of supervised probation. The estimated size of this effect represents a change from the lowest legally allowable sentence to the maximum allowable sentence under Colorado law. For misdemeanors, the expected sentence amounts to about an extra two years of supervised probation or an extra one or two months in prison. For felonies, the difference in sentence amounts to about an additional four or five months in prison. However, given the sheer number of cases in the data, the substantive effect of these

[15]Career goals (Baum 1997) provide an explanation for the across-the-board shift. The judges analyzed here are local trial court judges; if they are interested in advancing their career up the judicial hierarchy, their next step is the Colorado Court of Appeals (a court with statewide jurisdiction). As a result, judges interested in advancing their careers may reflect statewide macro opinion (which resulted in the initiative failing to pass).

[16]The z-statistic is -2.32.

harsher sentences is marked; while the effect size is relatively small for each case, both the costs to the state of Colorado (in terms of the costs of housing inmates in jail and prison and the number of probation officers necessary to meet with the additional inmates placed on probation) and the potential benefits (in terms of the fines collected by the state) magnify quickly given the thousands of cases in the data.

Prosecutorial Discretion

Table 2 presents the results of the multilevel logistic regression model examining the decision of district attorneys to dismiss cases.[17] To examine the effect of issue-specific public opinion on the district attorneys' decisions to dismiss cases, we first look at the coefficient for issue-specific public opinion (`initiative support`) given in Table 2. Since this coefficient is a constituent term to an interaction, the coefficient given in the table represents the effect of support for the legalization of marijuana before the public voted on the initiative; it is not statistically significant. Thus, the model provides no evidence that, before the initiative, district attorneys' dismissal decisions were congruent with public opinion.

[Table 2 about here] [Figure 3 about here]

Next, we look at Figure 3 to examine the change in the probability of dismissal before and after the initiative. The difference in predicted probabilities shown in the figure was computed holding all variables at their mean (or modal) values. The results strongly suggest that the initiative is associated with a significant change in the behavior of district attorneys. When initiative support

[17]Because the elimination of district attorneys who served in both time periods would eliminate entire judicial districts from the data, this analysis includes district attorneys regardless of whether or not they served in both time periods. Additionally, in preparation for the selection model discussed below, the exclusion of judges who only served in one of the two periods would lead to the exclusion of judges from the data, as well. Thus, to obtain unbiased estimates of the effect of public opinion on *judges* in the selection model, it is necessary to analyze all district attorneys.

is low, the difference in the predicted probabilities between the two time periods is negative. This indicates that district attorneys in jurisdictions with low support for marijuana legalization became less likely to dismiss a given charge after they saw their constituents' opinions on marijuana legalization, *ceteris paribus*. This action is congruent with public opinion; we would expect that, as support for marijuana legalization decreases, district attorneys should be less likely to dismiss charges. This is exactly the story shown in Figure 3.

Conversely, Figure 3 shows that, in areas of high support for marijuana legalization, the difference in predicted probabilities effect is positive. This indicates that district attorneys, after seeing the results of the initiative, exhibited a statistically significant change in behavior toward their constituents' opinions. In other words, Figure 3 shows that district attorneys whose constituents support the legalization of marijuana became more likely to dismiss a given charge after the initiative. This effect is substantively meaningful. As Figure 3 shows, for some districts, this effect results in nearly an 6% difference in the probability that a case will be dismissed. In short, the conclusions from Figure 3 suggest that, while their decisions were markedly incongruent with public opinion before the initiative, district attorneys exhibited a statistically significant change in behavior toward the congruence after the initiative.[18]

The model results also provide insight about the other factors which influence a district attorney's decision to dismiss a charge. The results indicate, as the number of charges facing a defendant increases, the probability that one of those charges will be dismissed increases. Additionally, the model indicates that lame-duck district attorneys (those in their second term) are more likely to dismiss a given charge. Finally, examining the characteristics of the defendant included in the model, only the defendant's gender has a statistically reliable effect; charges facing female defendants are more likely to be dismissed.

[18]However, while this marginal effect indicates that district attorneys' actions moved toward congruence after the initiative, public opinion is still not a statistically significant behavior of the probability that they will dismiss a case in the second time period. The z-Statistic is -0.081.

Selection Effects

Having seen that the initiative affected the behavior of both judges and district attorneys in Colorado, we now examine the extent to which the responsiveness of district attorneys has downstream, and potentially biasing, effects on the effects of public opinion on judicial decisionmaking. To this end, I estimated a Heckman selection model to address the possibility that a selection process is at work (Heckman 1979). Heckman selection models have enjoyed a great deal of usage in the state judicial politics literature (Langer 2002; Bonneau and Hall 2009); in these models, the probability that an observation will advance to the second stage is estimated using a probit model, and the probability of inclusion is added to the second stage linear model as another covariate. In this case, the probability that a charge is not dismissed is estimated in the district attorney model (the first stage probit) and included in the sentencing model (the second stage linear model). Appendix B provides the full results of the selection model along with additional information about the estimation. Importantly, while the sentencing model presented earlier was estimated using maximum likelihood estimation, the second stage of a Heckman model must be a linear model due to Freedman and Sekhon's (2010) admonishment that "the usual Heckman two-step procedure should not be used" to estimate a model where the second stage is something other than a linear regression.

[Table 3 about here]

Table 3 provides the estimated coefficients for the interaction term and its constituent terms for the second stage of the Heckman selection model (the first column) and the analogous simple linear regression that does not correct for selection effects; indeed, the magnitude and reliability of the estimated coefficients is nearly identical in both models, even when the Inverse Mill's Ratio is included in the model.[19] Moreover, though not presented here, the marginal effect plots lead to

[19]When comparing these coefficients with those shown in Table 1, one should remember that these are OLS coefficients and Table 1 contains Poisson coefficients. Full results are available in Appendix B.

substantively similar conclusions to those pictured in Figures 2 and 3. Thus, the model indicates the statistical presence of a selection effect, though the substantive impact of that effect is minuscule.

Discussion and Conclusion

In short, these results indicate that Colorado trial court judges became congruent with public opinion only after they received constituency-level information about the electorate's preferences. The 2006 initiative is associated with an important shift in judicial sentencing behavior toward the will of the Colorado public. While district attorneys were not congruent with public opinion before or after the election, they exhibited responsiveness to the results of the initiative that put their dismissal decisions more in line with the will of their constituents. Finally, the results of the selection model indicate that, while a statistically significant selection effect existed between the dismissal decisions of the prosecutors and the sentences issued by judges, there is no evidence to suggest that a failure to account for prosecutorial discretion would have had any effect on the substantive conclusions one could draw about the effect of public opinion on judicial behavior.

Because they examine only a single state and a single issue area, care must be taken when generalizing from these findings; yet, these results, taken in tandem with Traut and Emmert (1998), Canes-Wrone, Clark, and Park (2012), Huber and Gordon (2004), and Kuklinski and Stanga (1979), indicate that judges selected through appointment and retained through periodic retention elections respond to issue-specific public preferences. Yet, it is clear that information about constituent preferences is essential; before the initiative, judges were not congruent with public opinion. The judges only became congruent with public opinion after they received information about constituent preferences. These results suggest that access to information is a crucial precursor to congruence. Whereas legislators have myriad ways to gauge constituent preferences, judges traditionally do not have access to those resources. If one wishes their judges to be more responsive to public opinion, these results would suggest that they seek to develop new ways for judges to learn about public preferences.

These results also have important implications for the debate about the effects of judicial

elections on the balance between judicial independence and judicial accountability. Bonneau and Hall (2009) have written that "accountability is '*a product of electoral competition*, produced by the willingness of challengers to enter the electoral arena and the propensity of the electorate not to give their full support to incumbents"' (78, emphasis added). These results challenge that argument; they suggest that candidate competition is not a precondition to judicial accountability; judges' decisions may reflect the public will if they merely face retention elections *and* have good quality information about constituent preferences. If one component of judicial accountability is a predisposition to decide cases in a manner that comports with majority will, it appears that Colorado judges respond to issue-specific public opinion even though they face a very low probability of defeat on election day but only after constituent preferences are revealed to them. In this way, the results indicate that judicial elections may induce even lower court judges deciding routine cases to heed public opinion.

These findings suggest that future work needs to examine the conditions under which various methods of selection, retention, and retirement alter the representative nature of judicial institutions. By understanding the conditions under which the institutions which regulate who becomes a judge, how judges retain their seats, and when (or if) judges are required to leave the bench affects congruence with public opinion, scholars will gain a clearer picture of the role that courts play in the larger political environment. By comparing and contrasting the effects of these institutions on judges with the effects that these institutions have on legislators, governors, and bureaucrats, scholars can begin to build more nuanced theories to explain, generally, under what conditions these rules affect the ability of American federal, state, and local government to represent the will of the people.

Moreover, future scholarship explain how judges learn about public opinion and under what conditions the public's signal is strong enough to entice judges to heed the public will. The high level of voter turnout and wide range of public support made the Colorado initiative a very strong signal for judges to follow; future work could illustrate which signals are informative to judges and which are not. This type of research could potentially determine between two theoretical explana-

tions that are observationally equivalent in this study: judges may have already known the level of constituent support for the issue before the initiative but only became enticed to respond to it after the initiative. Conversely, judges may have not known their constituents' preferences before the vote and then adjusted their behavior given new information about public preferences.

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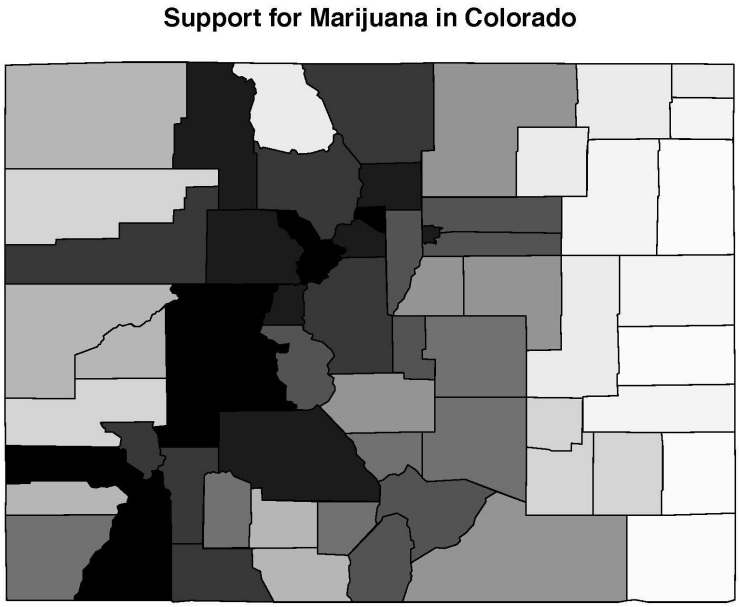

FIGURE 1: Support for marijuana legalization in Colorado as measured by the 2006 ballot measure. Darker colors indicate more support.

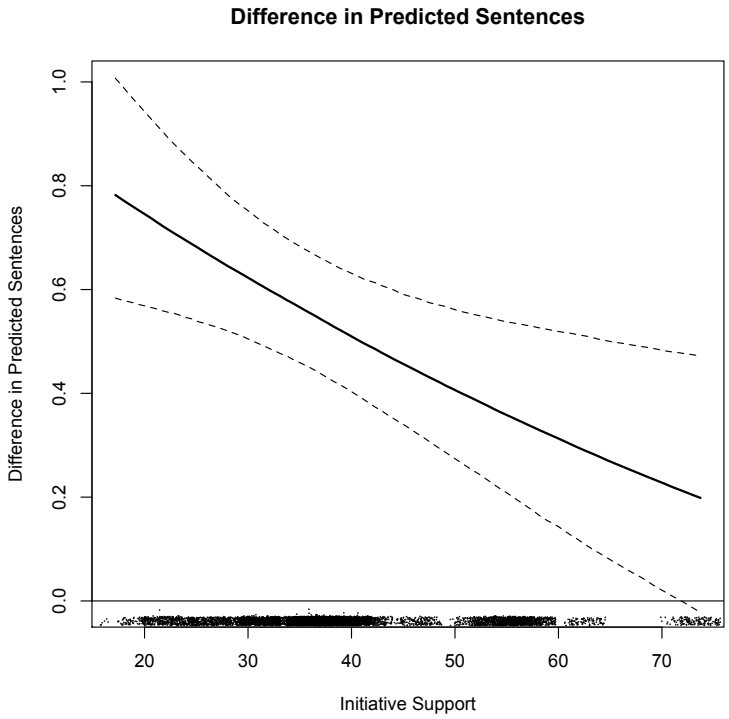

FIGURE 2: Marginal effect of a case being decided after the initiative. The values are simulated holding all other values at their modal (for categorical variables) or mean values (for continuous variables). The line represents the expected difference for petty offenses. As the value of the x-axis increases, constituents' support for the legalization of marijuana rises. The rug at the bottom of the figure illustrates the distribution of cases according to constituents' level of support for the initiative.

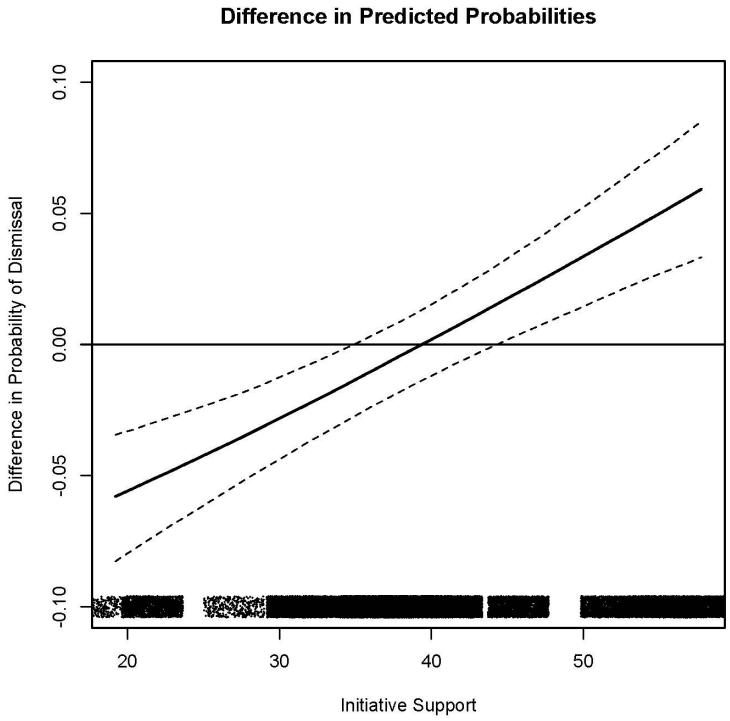

FIGURE 3: Difference in predicted probabilities pre- and post-initiative. The values are simulated holding all other values at their modal (for categorical variables) or mean values (for continuous variables). The line represents the expected difference for petty offenses. While not shown, the pattern for misdemeanors and felonies is the same, although the slope of the line differs slightly. As the value of the x-axis increases, constituents' support for the legalization of marijuana rises. The rug at the bottom of the figure illustrates the distribution of cases according to constituents' level of support for the initiative.

	Estimate	S.E.	z-Statistic
Judge Level:			
Republican Appointee	-0.091	0.029	-3.179
Initiative Support	-0.005	0.003	-1.472
Case Level:			
Post-Initiative	0.223	0.042	5.334
Post-Initiative×	-0.002	0.001	-2.039
Initiative Support			
District Court	-0.015	0.032	-0.478
Minority Defendant	0.037	0.013	2.944
Female Defendant	-0.005	0.013	-0.390
Defendant's Age	0.001	0.001	1.334
Misdemeanor	-0.546	0.013	-42.374
Petty Offense	-1.269	0.023	-55.702
Constituent Ideology	0.004	0.002	1.738
Intercept	2.491	0.102	24.456
σ_{judge}	0.14		
σ_{district}	0.13		
Number of Observations	9,167		
Number of $\text{Groups}_{\text{judge}}$	216		
Number of $\text{Groups}_{\text{district}}$	21		
Deviance	10996		

TABLE 1: Multilevel Poisson model results, with standard errors corrected for overdispersion. The data include only those judges who sentenced defendants both before and after the initiative. The outcome variable in the analysis is the scaled sentence given to the defendant. The model includes random effects for judges and districts. Felonies are the baseline category for charge severity.

	Estimate	S.E.	z-Statistic
District Attorney Level:			
Republican DA	0.020	0.102	0.197
Initiative Support	-0.015	0.008	-1.861
Case Level:			
Post-Initiative	-0.569	0.105	-5.415
Post-Initiative×	0.014	0.003	5.501
Initiative Support			
District Court	0.054	0.030	1.790
Number of Charges	0.413	0.007	63.259
Second Term	0.059	0.029	2.004
Misdemeanor	-1.602	0.038	-41.757
Petty Offense	0.536	0.036	14.884
Minority Defendant	0.014	0.026	0.533
Female Defendant	0.094	0.024	3.835
Defendant's Age	-0.001	0.001	-1.601
Constituent Ideology	0.014	0.006	2.407
Intercept	-1.073	0.293	-3.658
σ_{attorney}	0.22		
σ_{district}	0.24		
Number of Observations	63,399		
Number of $\text{Groups}_{\text{attorney}}$	46		
Number of $\text{Groups}_{\text{district}}$	21		
Deviance	71786		

TABLE 2: Multilevel logistic regression results. The outcome variable in the analysis indicates whether the district attorney dismissed a charge. The model includes random effects for district attorney and the district in which they work. Felonies are the baseline for charge severity.

	Heckman	Traditional OLS
Initiative Support	-0.025	-0.026
	(0.024)	(0.025)
Post-Initiative	1.483	1.456
	(0.282)	(0.285)
Initiative Support×	-0.02	-0.02
Post-Initiative	(0.007)	(0.007)
IMR	0.27	
	(0.13)	
Adjusted R^2	0.51	0.52

Standard errors in parentheses.

TABLE 3: Comparison of OLS coefficients and coefficients from the second stage of a Heckman selection model. The outcome variable for the OLS is the scaled sentence. Models include fixed effects for judge (not shown here).

Online Appendix

Appendix A: Sentence Severity Scale

As discussed in the text, the sentence severity scale used as the outcome variable in the sentencing analysis follows that used by Cook (1977), Gibson (1978; 1980), Kritzer (1979), and Kuklinski and Stanga (1979) in their prior work on the relationship between public opinion and judicial decisionmaking. The scoring system comes from a Bureau of Justice publication entitled *Federal Offenders in U.S. District Courts*. The scoring system is found in Table 4.

Type of Sentence	Duration	Point Value
Suspended Sentence		0
Deferred Sentence		0
Unsupervised Probation		0
Fine		0
Supervised Probation	Under 12 Months	1
	13 to 26 months	2
	Over 36 months	4
Community Corrections Program	Under 12 Months	1
	13 to 26 months	2
	Over 36 months	4
Community Service	Under 12 Months	1
	13 to 26 months	2
	Over 36 months	4
Diversion Program	Under 12 Months	1
	13 to 26 months	2
	Over 36 months	4
Imprisonment	Less than 1 month	1
	1-6 months	3
	7-12 months	5
	13-24 months	8
	25-36 months	10
	37-48 months	12
	49-60 months	14
	61-120 months	25
	Over 120 months	50

TABLE 4: Sentence Severity Scale used to compile outcome variable for sentencing model. The point values come from *Federal Offenders in U.S. District Courts*.

Appendix B: Full Heckman Results

This appendix presents the full results of the selection model presented in the body of the paper. The model was estimated using the `sampleSelection` package in `R`. There are a few important points to keep in mind with this analysis. First, the Heckman selection model implemented in `R` is a single-level model; thus, these models were estimated with fixed effects for district and judge/district attorney rather than varying intercepts. Second, the Heckman selection model utilizes a probit model in the first stage and a simple linear model in the second stage. The sentencing models estimated elsewhere in this paper use a Poisson count model rather than a linear model. However, the range of the estimated counts is quite large, suggesting that the a linear model may also be appropriate. Finally, the routine drops all "selected" observations that have missing values in the second stage. In other words, the selection model drops those cases in which a defendant was found not guilty at trial or where a judge dismisses the charge facing the defendant. In the multilevel district attorney model, these charges are included, as the district attorney would not know what the judge would do when the charge goes to trial. Still, the substantive results are the same, as discussed above.

	Probit	Heckman OLS	Traditional OLS
District Court	0.148	0.618	0.634
	(0.021)	(0.162)	(0.163)
Minority Defendant	0.015	0.247	0.244
	(0.019)	(0.086)	(0.087)
Female Defendant	-0.111	-0.039	-0.023
	(0.019)	(0.086)	(0.086)
Republican	2.066	4.32	4.145
	(0.344)	(1.743)	(1.762)
Defendant's Age	0.002	0.007	0.007
	(0.001)	(0.003)	(0.003)
Misdemeanor	1.090	-4.846	-5.039
	(0.026)	(0.137)	(0.105)
Petty Offense	-0.323	-8.954	-8.888
	(0.025)	(0.147)	(0.145)
Constituent Ideology	0.004	0.052	0.051
	(0.005)	(0.018)	(0.018)
Initiative Support	0.128	-0.025	-0.026
	(0.02)	(0.024)	(0.025)
Post Initiative	0.274	1.483	1.456
	(0.078)	(0.282)	(0.285)
Initiative Support$\times$	-0.005	-0.020	-0.020
Post-Initiative	(0.002)	(0.007)	(0.007)
Number of Charges	-0.212	0.000	0.000
	(0.004)	(0.000)	(0.000)
Final Term	0.060		
	(0.023)		
Intercept	-6.873	8.171	8.662
	(1.074)	(1.305)	(1.3)
IMR		0.274	
		(0.127)	
N	51,644	11,009	11,009
$\log L$	-41588.93		
Adjusted R^2		0.51	0.52

Standard errors in parentheses.

TABLE 5: Results of two-stage analysis. Because the outcome variable in the first stage of a Heckman selection model indicates whether or not the observation moves to the second stage, the outcome variable for Probit indicates whether the charge was *not dismissed* (this is the opposite of the outcome variable in the logits presented above). Outcome variable for the OLS is the scaled sentence. Models include fixed effects for district attorney/judge (not shown here).

Appendix C: Coding Rules

Variable	Coding Rule
Defendant	
Female Defendant	Dichotomous. Takes a value of 1 if the defendant is female
Minority Defendant	Dichotomous. Takes a value of 1 if the defendant is a minority
Defendant's Age	The defendant's age (in years) at the time of the sentence
District Attorney	
Republican DA	Dichotomous. Takes a value of 1 if the district attorney is listed on the ballot as a member of the Republican party
Final Term	Dichotomous. Takes a value of 1 if the district attorney is in his or her second term.
Judge	
Republican Appointee	Dichotomous. Takes a value of 1 if the judge was originally appointed to the bench by a Republican governor
Case Characteristics	
District Court	Dichotomous. Takes a value of 1 if the case was heard in district court
Number of Charges	The number of charges faced by the defendant.
Felony	Dichotomous. Takes a value of 1 if the charge is a felony
Misdemeanor	Dichotomous. Takes a value of 1 if the charge is a misdemeanor
Petty Offense	Dichotomous. Takes a value of 1 if the charge is a petty offense
Constituency Characteristics	
Constituent Ideology	The percentage of the vote cast by constituents for the Democratic candidate in the closest U.S. presidential election
Initiative Support	The percentage of voters who supported legalizing marijuana in the 2006 general election

TABLE 6: Coding of Explanatory Variables

Appendix D: Robustness

Here, I discuss the results of a series of analyses conducted to ensure the robustness of the empirical results and the substantive conclusions drawn from them. Readers may be concerned that, if both district attorneys and judges are changing their behavior after the initiative, the reported results may be the result of selection bias; because the probability that a district attorney dismisses a given charge may change after the initiative if district attorneys (who also face popular election) also respond to the initiative. Additionally, the failure of the model to include the defendant's prior criminal record also merits discussion.

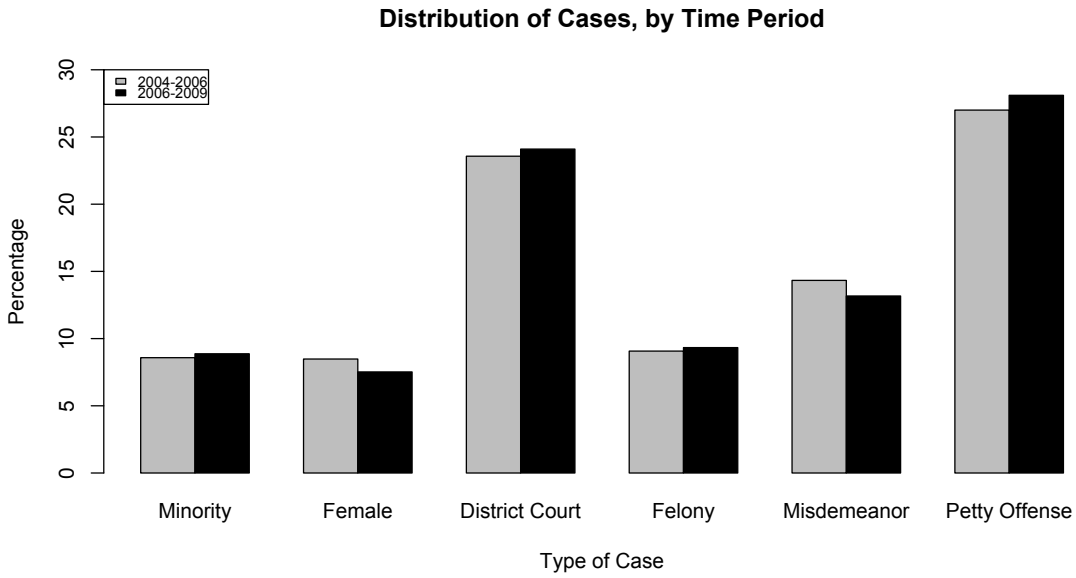

FIGURE 4: Distribution of case facts in marijuana cases decided in the three years before and after the initiative.

The Pool of Cases

First, one may be concerned that the empirical results presented above result from different pools of cases. Perhaps—especially given the positive across-the-board change discussed above—attributes of the cases are different in the two time periods. This is not the case. Figure 4 displays summary statistics of the full set of cases decided by Colorado judges in the three years before and after the initiative. As the figure indicates, there were no substantive differences in the types of cases decided by the judges with regard to the characteristics of the defendants or the severity of the charges.

Prior Criminal Record

Readers may note that the model does not contain data on the defendant's prior criminal history. Records of the defendant's history with the legal system are not kept by the Colorado judicial system and were, as a result, unavailable for this study. The only way such information would be available would be to travel to individual county clerks' offices in the state to seek information

about every defendant in the data. As such, this information is unavailable for the data discussed in this paper. However, to ensure that the omission of this variable does not bias the empirical results presented throughout the paper, I have conducted a series of empirical analyses.

As Fox (2008) notes, a control variable must be included in the model if it "(1) is a cause of Y, and (2) is correlated with an explanatory variable in the regression equation" (112). Here, the key independent variable of interest is the interaction between `initiative support` and `post-initiative`. While a defendant's prior criminal record may be a cause of the sentence he receives, the results presented here show that there is no evidence that it would be correlated with the key explanatory variable of interest.

First, examining the distribution of cases in Figure 4, one can see that the pool of cases stayed the same in both time periods. In fact, the two pools of cases are nearly identical on every observed variable, including characteristics of the defendant such as minority status, gender, and age. As a defendant's prior criminal record is, like age, race, and gender, a characteristic of the defendant, there is no evidence that the distribution of prior criminal records is correlated with the interaction term.

Second, there is no evidence that the observed demographic characteristics have different effects in the two time periods. If the omission of prior criminal record is problematic, the issue would stem from the fact that it had a different effect in the two time periods. As Table 7 shows, a model including interaction terms between the observed characteristics of the defendant and `post-initiative` yields no statistically significant coefficients on the interaction terms that include the demographic characteristics of the defendant. Thus, not only is the pool of defendant characteristics the same in both time periods, but those characteristics have the same effect in both time periods. Again, this is evidence that the exclusion of the defendant's prior criminal record is not problematic.

Finally, I examine whether or not the empirical results hold for the pool of cases for we expect defendants' prior criminal records to be most similar: the posession of less than one ounce of marijuana. These empirical analyses also serve another function. Readers may be concerned

	Estimate	S.E.	z-Statistic
Intercept	2.492	0.053	47.099
District Court	-0.022	0.033	-0.665
Post-Initiative	0.080	0.034	2.319
Minority Defendant	0.058	0.020	2.953
Female Defendant	-0.006	0.020	-0.293
Defendant's Age	0.001	0.001	0.991
Misdemeanor	-0.594	0.019	-30.908
Petty Offense	-1.383	0.027	-50.658
Post-Initiative×Misdemeanor	-0.000	0.001	-0.256
Post-Initiative×Petty Offense	-0.000	0.001	-0.256
Post-Initiative×Minority	-0.033	0.025	-1.319
Post-Initiative×Female	-0.001	0.027	-0.019
Post-Initiative×Age	-0.000	0.001	-0.256
σ_{judge}	0.15		
σ_{district}	0.14		
Number of Observations	9167		
Number of Groups$_{\text{judge}}$	216		
Number of Groups$_{\text{district}}$	21		
Deviance	10941		

TABLE 7: Examining the effect of demographic characteristics on judicial sentences in both time periods. Multilevel Poisson model with random effects for district and judge.

that the empirical results discussed above are too broad; while the initiative text asked voters whether or not they wanted to "[legalize] the possession of one ounce or less of marihuana for any person twenty-one years of age or older," the empirical analyses presented above examine all marijuana cases, including those for which defendants were charged with the possession of more than one ounce of marijuana (Colorado Secretary of State 2007). In this set of analyses, I reestimate the models described above on the subset of cases in which the defendant was charged with the possession of one ounce or less of marijuana—the same subset of cases which would have been affected by the passage of the initiative. Table 8 presents multilevel models for district attorneys and judges in these cases.

Figure 5 plots the marginal effects for district attorneys and judges, respectively. In both cases, the marginal effects are very similar to those discussed in the previous section and shown in Figures 2 and 3. The patterns are identical.

	Estimate	S.E.		Estimate	S.E.
District Attorney Level:			*Judge Level:*		
Republican DA	0.037	0.123	Republican Appointee	-0.091	0.034
Initiative Support	-0.007	0.011	Initiative Support	-0.003	0.004
Case Level:			*Case Level:*		
Post-Initiative	-0.530	0.120	Post-Initiative	0.340	0.053
Post-Initiative×	0.016	0.003	Post-Initiative×	-0.003	0.001
Initiative Support			Initiative Support		
District Court	0.518	0.034	District Court	-0.179	0.035
Number of Charges	0.498	0.008			
Last Year	0.027	0.038			
Minority Defendant	-0.035	0.030	Minority Defendant	-0.008	0.022
Female Defendant	0.145	0.027	Female Defendant	0.024	0.020
Defendant's Age	-0.006	0.001	Defendant's Age	-0.000	0.001
Constituent Ideology	0.006	0.008	Constituent Ideology	0.005	0.003
Intercept	-0.815	0.401	Intercept	1.059	0.103
σ_{attorney}	0.25		σ_{judge}	0.114	
σ_{district}	0.37		σ_{district}	0.20	
Number of Observations	54589		Number of Observations	5162	
Number of Groups$_{\text{attorney}}$	46		Number of Groups$_{\text{judge}}$	196	
Number of Groups$_{\text{district}}$	21		Number of Groups$_{\text{district}}$	20	
Deviance	82916		Deviance	2479	

TABLE 8: Multilevel results for petty offenses. The outcome variable in the district attorney analysis indicates whether or not the district attorney dismissed a charge. The model d includes random incercepts for both district attorneys and districts. For judges, the outcome variable in the analysis is the scaled sentence given to the defendant. The model nests cases within judges within districts and includes random intercepts for both judges and districts.

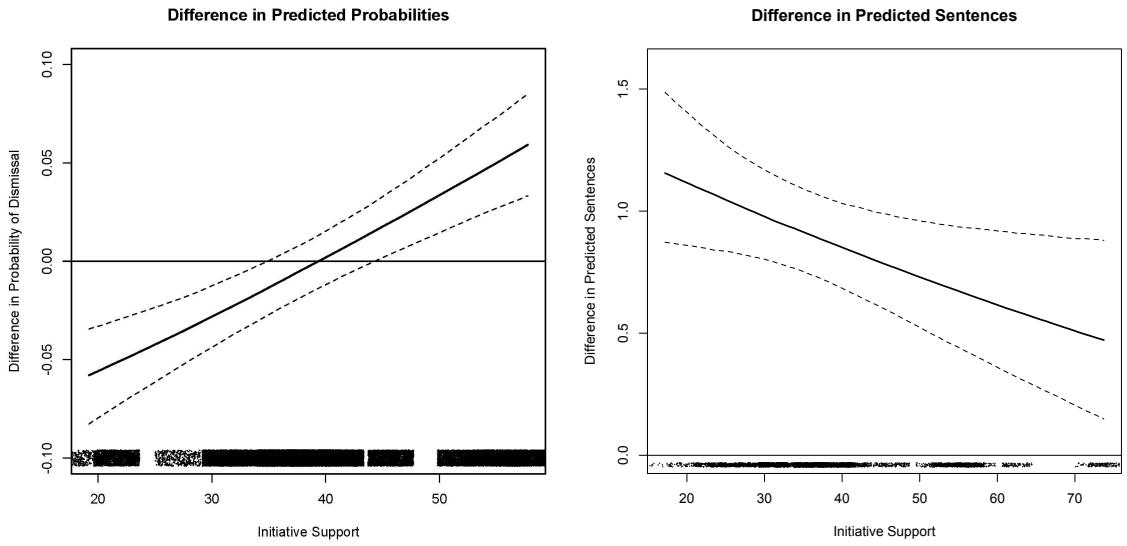

FIGURE 5: Marginal effect of a petty offense being decided after the initiative. The values are simulated holding all other values at their modal (for categorical variables) or mean values (for continuous variables). As the value of the x-axis increases, constituents' support for the legalization of marijuana rises. The rug at the bottom of the figure illustrates the distribution of cases according to constituents' level of support for the initiative.