

## Measuring and Explaining Charge Bargaining

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**Abstract** Charge bargaining is a potentially important form of discretion in criminal sentencing that is obscured in many studies of sentencing outcomes. Our procedure to measure the difference in sentencing outcomes caused by plea bargain emphasizes the amount, in months, that the sentence length is reduced. Using this measure, we compare prosecutorial discretion across counties in two different states. We conclude that charge bargaining plays an empirically important role in determining sentencing outcomes. Furthermore, we find that measuring the distance (in months of prison time) moved during a charge bargain may provide a very different estimate of the discretion than is given by the rate of bargaining, which is the usual measure used. Although the *rate* of charge bargaining was higher in the voluntary guidelines state, its *impact* on sentences was greater in the presumptive guidelines jurisdiction, as predicted by Reitz (1998). We further observe a dramatic difference in predictions from shifting the case characteristics underlying the summary measure. This result reveals that distributional differences (either due to the underlying criminal activity or due to the overall level of severity of punishment) can easily obscure the inferences necessary for understanding the operation of the systems. Our finding of differential charge bargaining in these two jurisdictions should provide a caution when comparing the results of studies of disparity in sentencing across jurisdiction types.

**Keywords** Sentencing · Charge bargaining · Plea bargaining · Sentencing disparity

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Much of the work on this manuscript was completed while Bushway was a professor at the University of Maryland

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## Introduction

Over the last 20 years, the field of criminology has produced numerous prominent studies of sentencing and sentencing disparity in single jurisdictions (state or federal government) using data on criminal convictions.<sup>1</sup> In most cases, researchers are interested in identifying and then explaining the discretion of various actors in the system, including judges, prosecutors, bail administrators and defense attorneys. Yet the simple conviction outcome used as the dependent variable in most quantitative sentencing research is the result of the discretion of all the actors in the system combined, and research which focuses solely on the sentenced outcome is unable to comment on the discretion of any given actor. Discretion can be identified methodologically, however, if the researcher can identify the counterfactual punishment that would be meted out if a given actor did not in fact exercise discretion.

The logic of this type of approach is straightforward and is common in qualitative work. For example, research on the application of the death penalty first gathers exhaustive data on all cases that are “death penalty eligible” by the facts of the crime available in the prosecutor and police files. Then, the facts are examined to determine what factors are correlated with the various decisions that are made along the way by various actors, decisions that ultimately lead to a death penalty conviction (Paternoster et al. 2005).

In the death penalty case, the counterfactual is created through detailed examination of the facts of the case, facts that are not often available in the large conviction data sets used in the quantitative analysis of sentencing discretion. However, recently researchers have begun to solve this problem through a greater use of the institutional features of the sentencing process, most notably sentencing guidelines. In the structure of sentencing guidelines, a natural reference point is the recommended sentence length, operationalized as the midpoint or minimum of the guideline range. Absent additional action by the judge (or the prosecutor in conjunction with a judge for plea bargains), the convicted offender should receive this presumptive sentence. Any departure from this presumptive sentence can then be viewed as an exercise of discretion that can be placed directly in the hands of the actor ultimately responsible for assigning sentencing lengths (Bushway and Piehl 2001; Engen and Gainey 2000). Numerous studies have attempted to study the causal elements that drive whether or not someone gets a departure (Kramer and Ulmer 1996, 2002; Johnson 2003; Miethe and Moore 1986; Mustard 2001). At least three studies have attempted to study the distance or value of this departure by studying the difference between the minimum or maximum guideline recommendation and the actual sentence (Mustard 2001; Albonetti 1997, 1998).<sup>2</sup>

Another example is the study of mandatory minimums. Again, researchers use a fact in the data to signal the counterfactual penalty. For example, the presence of a gun enhancement on the guideline worksheet could signal that the person is eligible for a mandatory sentence for carrying a gun. Then the researcher examines whether the person did indeed face a mandatory gun charge. Because the prosecutor is

<sup>1</sup> For a comprehensive review of sentencing research, see Spohn (2000).

<sup>2</sup> Albonetti adds a departure variable into a Tobit for sentence length, and then interacts race and gender with that variable. This interaction in effect looks to determine if the “value” of a departure varies by race and gender. Mustard’s analysis of the departure distance as the dependent variable is a more fully specified version of this model.

ultimately responsible for bringing charges, the decision to apply the mandatory statute is evidence of a prosecutorial discretion. Having identified this discretion, it can then be studied and explained (Kessler and Piehl 1998; Bjerck 2005; Farrell 2003; Bynum 1982; Loftin et al. 1983; Hofer 2000; Kramer et al. 2003).

The mandatory minimum is perhaps the easiest type of prosecutorial discretion to study because the counterfactual penalty in question is fixed. But, typically, the counterfactual penalty is not fixed. In charge bargaining more generally, the value of the initial charge in terms of a penalty has a wide variance, so we are unable to discern immediately what the penalty would have been in the absence of the plea bargain. This has the potential disadvantage of forcing the researcher to focus on the existence of a charge bargain, rather than on the value of the bargain. But in a world in which 90% of the convictions end as a result of a plea bargain, we believe the substantive value of interest is the difference between what the person would have received if they had not pled and what they received as the result of the plea bargain. Moreover, the correlates of these two facts may in fact be different. Mustard (2001) found that the factors that determine which cases received the departures are different than the factors that explain the size or value of the departures. In the same way, the factors that determine who gets a charge bargain may be different than the factors that determine the value of the charge bargain. What is needed methodologically is a way to measure the consequences of the decision to charge bargain.

In this paper, we present a method, based on work by Smith (1986), designed to measure the distance covered by a charge bargain. We then apply this approach in a case study—the inter-jurisdictional comparison of prosecutorial discretion in two different types of guideline systems, a strict (Washington) and a voluntary system (Maryland). We use available BJS data from county courts in these two states because research on these two states' sentencing systems is an important part of the literature. Other comparisons are also possible using the technique we develop in this paper.

Theory suggests that there will be relatively more prosecutorial discretion in places with stricter guidelines, Washington (Reitz 1998). The application of the method raises another methodological problem—how to compare processes across populations which have very different compositions. We apply a technique from economics, a version of an Oaxaca (1973) decomposition, which allows us to focus on the mechanics of the process rather than population differences. Our estimates contradict Miethe (1987) and are consistent with the Reitz's (1998) hypothesis that strict guidelines lead to substantial displacement of discretion to the prosecutor. Although the rate at which people plea down from a felony charge to a misdemeanor conviction is higher in our Maryland counties, the sentencing consequences of these pleas are greater in King County, Washington (Seattle). This result is a direct demonstration of the importance of both of the methodological contributions of the paper. An emphasis on only the misdemeanor rate would have led to a misleading sense of the relative discretion in the systems. And, a direct comparison of the extent of plea bargaining in each of the two jurisdictions understates the relative amount of plea bargaining in King County, Washington due to differences in the composition of the population of offenders. Holding observable population characteristics constant, we find that a non-trivial proportion of the cases in King County that start at arraignment as felony cases end up (through plea bargains) as misdemeanor cases, which are not sentenced according to the guideline grid.

## Method

In classical charge bargaining, the individual defendant is faced with a charge at arraignment. This is typically viewed as the maximum charge that will be faced if the person goes to trial. A charge bargained plea (in contrast to a fact bargain) would involve a lowering of the charges in return for a guilty plea. To measure the value of this reduction in terms of sentence length, we need to know what would have happened to the person if they did not plea bargain. Obviously, this is unobservable. However, we do have data on what other people who are convicted of these offenses receive at sentencing, and to the extent to which these people are similar to the person who plea bargains, we can use this information to create an estimate of our desired counterfactual.

Smith (1986) encountered a similar problem in trying to understand the value of the so-called “trial penalty.” In his case, he needed to know the sentencing outcome at trial for someone who pled guilty. To create an estimate of this unobservable value, he first estimated regression models for conviction and incarceration for those who went to trial. He then used the coefficient estimates from these models to predict both the probability of conviction at trial and the probability of incarceration for those who plead guilty. The key assumption is that the process of trial would have worked the same way for those who plead versus those who went to trial, conditional on differences in observed characteristics. The trial penalty is then the difference between the observed outcome for the plea bargain and the predicted outcome at trial, discounted for the probability of an acquittal. Smith found that, on average, there was in fact no trial penalty once the discount for an acquittal was taken into account. This result is consistent with a rational choice model.

In what follows, we work only with the data from those who were convicted by plea. In effect we assume that the system is rational, and will assign penalties at plea that are roughly equivalent to the expected value that the person would receive at trial.<sup>3</sup> This assumption will allow our approach to be applied to cases in which information on conviction rates is not available. If in fact there is a trial penalty as defined by Smith, our estimate will be a conservative estimate of the charge bargain’s value. The main piece of information we need is information about the charge at arraignment.

Following Smith, we propose that researchers first estimate a model for the expected sentence length at plea using all available information about the case and the criminal history, including the charge at conviction. Then, using these coefficient estimates, we advocate creation of a predicted sentence for these same individuals, where the only change is that the prediction is made with the charge at arraignment versus the charge at conviction. If there has been no charge bargain, the predicted and actual values will be the same on average. However, if there has been charge bargaining, we expect that the predicted values using the charges at arraignment will be higher than the actual values, on average. The difference for each individual will

<sup>3</sup> In 1998, 94% of all felony convictions in state court were the result of plea bargains (U.S. Department of Justice 2001, Table 10). This restriction allows us to focus on the vast majority of convictions without modeling the selection of cases for trial. Plea bargains are arrived at in negotiations between the defense counsel and the prosecution. It is common to characterize plea bargains as “contracts” (Scott and Stuntz 1992) occurring in the context of asymmetric information (Reinganum 1988). Support for the view that the process responds to the context surrounding it comes from LaCasse and Payne’s (1999) finding that the reputation of the judge assigned to hear a case if negotiations fail influences the outcome of negotiations (see also Tonyr 1996).

be an estimate of the value of the charge bargain in terms of the sentence avoided by pleading guilty. Or alternatively, the difference will be an estimate of the size of the discretion exercised by the prosecutor in assigning the charge.

While this approach is fairly simple conceptually, a number of issues arise in implementation. In what follows therefore, we present an application of this approach to the study of cross-jurisdictional differences, a substantively interesting setting that will allow us to fill in more details.

### **Prosecutorial Discretion in the Presence of Guidelines**

Each system for criminal sentencing includes some role for discretion. Necessarily, actors such as prosecutors and judges are expected to use their own judgments in the course of carrying out their jobs. It is generally believed that the institutional environment affects both the amount and form of discretion, and reforms are often proposed to adjust the use of discretion. The exercise of discretion can carry out the appropriate aims of sentencing policy through the consideration of idiosyncratic case factors, but it can also be the way in which inequitable sentences are imposed.

Reforms toward more determinate sentencing structures were generally promoted as curbing what was perceived to be widespread disparity in sentencing outcomes across similar cases and/or bringing some semblance of order to the sentencing process (Frankel 1972; Tonry 1996). Both of these motivations beg an understanding of how discretion was affected by a change in sentencing regime.

Self-reviews by sentencing commissions in virtually all states that adopted sentencing guidelines have concluded that the guidelines increase consistency. Two independent reviews of Minnesota's guidelines (Miethe and Moore 1986; Stolzenberg and D'Alessio 1994) concluded that the guidelines substantially reduced disparity across similar cases by as much as 60%. According to a review by Tonry, "[e]valuations in Minnesota and Pennsylvania (and later in Oregon and Washington) showed that use of sentencing guidelines reduced disparities generally and in particular reduced racial and gender disparities (Tonry 1996, p. 10)."<sup>4</sup>

However, later in the same review, Tonry suggested that these results should be treated with some skepticism. Pre-post studies have internal validity problems associated with variation in other factors during the same time span.<sup>5</sup> More importantly, researchers and practitioners have expressed concern that prosecutors gain more discretion relative to the judges under sentencing guidelines (Alschuler 1978; Coffee and Tonry 1983). Although the guidelines envision all individuals with similar crimes in the same cell on the grid, it is possible for prosecutors to add disparity to the system by fact and charge bargaining similar cases to different parts of the grid. In its most extreme form, displacement of discretion to the prosecutor is complete, commonly referred to as the "hydraulic displacement of discretion."

Despite the concern with hydraulic displacement of discretion, pre-post studies of guidelines, which form the basis for Tonry's conclusion, utilized only conviction data. Unfortunately, information about conviction offense will not reveal the existence or

<sup>4</sup> The message with respect to the Federal guidelines is less clear. A self-review by the Federal Sentencing Commission has found increased consistency, a GAO review cast doubts on these conclusions, and the academic literature is also mixed (Anderson et al. 1999; LaCasse and Payne 1999).

<sup>5</sup> This is particularly problematic for analyzing disparity in the federal system, as the margin between federal and state jurisdiction has moved over time (Beale 1996; Brickey 1995).

extent of charge or fact bargaining. Research that does not consider what takes place prior to conviction systematically misses any change in the operation of bargaining associated with the change in sentencing system. Therefore, while studies using conviction data generally find less variation in sentencing outcomes following introduction of guidelines, the methodology cannot discern whether discretion is merely displaced to the charging stage of the process.<sup>6</sup>

Evidence suggests that prosecutors do change their practices with the introduction of guidelines (Frase 1993; Engen and Steen 2000). For example, in Washington State, 12.4% of the cases fell within the most serious offense types prior to the implementation of the guidelines, but only 7.4% of the cases fell within that same range after the guideline implementation. There was also evidence of a corresponding increase in conviction charges that did not carry the presumption of incarceration (Washington State Sentencing Guidelines Commission 1985). But change in practice after the onset of guidelines does not prove that prosecutors have more discretion or that the additional discretion leads to increasing disparity in outcomes. It could be that the same outcomes occur, just with a modified process (Miethe 1987) perhaps bounded by evidentiary requirements and working relationships with other members of the courthouse “working group.” Studies of changes within a determinate sentencing structure also provide evidence that sentencing institutions affect discretion and outcomes, sometimes in unpredictable ways (Engen and Steen 2000; Kessler and Piehl 1998). As a whole, these findings do not provide a consistent picture of how legal changes alter how discretion functions in practice. They do provide, however, further evidence that considering only conviction data can lead researchers to miss important consequences of changes in the sentencing context.

In his theoretical work outlining how strict guidelines can create prosecutorial discretion, Reitz (1998) identified a clear dependent variable—sentencing outcome—over which disparity should be defined. He defines sentencing outcome of a case as the “sum total of all applied sanctions as experienced by an offender until the legal system relinquishes jurisdiction over him” (Reitz 1998, p. 391). Other scholars, such as Miethe (1987), focused on changes in actual plea bargain practices. But workgroup practices can vary across jurisdictions in subtle and complicated ways (Ulmer and Miller 2002). We believe it is essential to define a measurable outcome of the sentencing process, one with inherent scale and meaning, to make comparisons across jurisdictions. We will apply our empirical approach to develop an estimate of the difference in the sentencing outcome that can be attributed to charge bargaining.<sup>7</sup>

<sup>6</sup> Most researchers take pains to note this possibility even where they cannot directly address it (e.g., Stolzenberg and D’Alessio 1994, p. 308; Engen and Gainey 2000, p. 1221).

<sup>7</sup> The importance of taking an inclusive definition of sentencing outcome becomes readily apparent when comparing jurisdictions. Charge bargaining in the strict guideline state may include moving offenders into misdemeanor categories not covered by the guidelines. Ignoring offenders convicted of misdemeanor offenses will therefore lead to misleading conclusions about the disparity in the system. For example, consider D’Alessio and Stolzenberg’s (1995) analysis of the impact of guidelines in Minnesota on jail populations. Numerous studies (Miethe and Moore 1986; Stolzenberg and D’Alessio 1994; Miethe 1987) have made the argument that Minnesota guidelines decreased disparity without increasing plea bargaining. However, the Minnesota guidelines did not cover incarcerations in jail, and the data sets used in the above analysis did not include information about jail incarcerations. Increased use of jail sentences, which would avoid the guidelines, would be undetected by data on prison incarceration. D’Alessio and Stolzenberg (1995) used aggregate data on jail populations to show that judges and prosecutors did increase their use of jail after the guidelines, particularly when prisons became crowded.

## Estimating the Effect of Charge Bargaining Across Jurisdictions

Our analysis compares the relative amounts of charge bargaining across two jurisdictions with two different guideline systems. We choose two states where researchers have conducted analyses using data from sentencing commissions with conviction data. The two states are Washington, which had presumptive guidelines before they were struck down by the recent US Supreme Court decision *Blakely v. Washington*, and Maryland, which has a voluntary guideline system. Details on the institutional arrangements in the two states can be found in Appendix.

To build our empirical model, we begin with the traditional model for explaining sentence length using only the factors usually considered to be legitimate factors involved in sentencing, namely case characteristics and criminal history:

$$\text{Sentence length}_i = \alpha + \beta \text{criminal history}_i + \gamma \text{crime severity}_i + \varepsilon_i \quad (1)$$

Note that we do not include the presumptive sentence as a regressor, as recommended by Engen and Gainey (2000) and Bushway and Piehl (2001), because this paper studies prosecutor decision making, not judicial decision making. The presumptive sentence is the outcome of prosecutor decision making, not the starting point. Therefore, for the same reason that it is important for researcher trying to isolate judicial behavior, including the presumptive sentence would be incorrect in this setting.

Our estimating equation includes several modifications to this general approach in Eq. 1. First, we log sentence length to help take into account non-linearities across the sentencing range. The truncation of non-incarcerated offenders can lead to substantial bias in the coefficient estimates in the sentence length regression (Bushway and Piehl 2001). Therefore, our second modification involves specifying a model that includes those who receive terms of incarceration as well as those who do not. We expect the non-incarcerated offenders to capture a disproportionate amount of the unexplained variation, since many who receive probation were eligible for incarceration.<sup>8</sup> The extent of truncation is larger in this paper than in studies of felony convictions because the proportion of cases with a sentence that do not involve incarceration is higher in a sample of felony arraignments.

There are three basic approaches for handling the truncation. The first, adopted by Stolzenberg and D'Alessio (1994), splits the sentencing variation into two parts: incarceration and sentence length for those with terms of incarceration. The drawbacks of this approach are that it divides sentencing disparity into two parts and it raises serious concerns about specification bias in the sentence length regression. The second approach, adopted by Barry and Greer (1981), involves using some type of sentence severity rubric to convert all sentences, including jail, prison, probation, and fine into one scale. The results of this approach can be highly sensitive to the scaling approach chosen. We follow the final approach, adopted by Rhodes (1991) and Albonetti (1997, 1998), which models the sentence length as censored at zero, using a Tobit regression. While parsimonious, this approach carries with it assailable parametric assumptions about the censored variation (the sentences that are zero or below).<sup>9</sup> To test whether

<sup>8</sup> In parts of the sentencing grid, sentencing ranges include probation. By omitting the probation cases, the usual model in the criminology literature essentially truncates the amount of measured discretion, leading to an underestimate of the true unexplained variation in sentence outcomes.

<sup>9</sup> For more detail on the Tobit model see Smith and Brame (2003) and Osgood et al. (2002).

our results are driven by these assumptions, we will also estimate a probit model of the incarceration/non-incarceration decision (see Sect. 5).

With these modifications, our estimating equation becomes the Tobit equation:

$$\begin{aligned} \text{Ln}(\text{sentence length}) = & \alpha + \beta_1 \text{misdem. crime types}_i + \beta_2 \text{felony crime types}_i + \\ & \beta_3 \text{CJ status}_i + \beta_4 \# \text{arrests}_i + \beta_5 \# \text{prison}_i \\ & + \beta_6 \# \text{jail}_i + \varepsilon_i \end{aligned} \quad (2)$$

We enter the five crime types—person, property, drug, public order, and other—as dummy variables which we interact with a misdemeanor dummy to account for the difference between felonies and misdemeanors. The categorization is based on the most serious charge at conviction. Criminal history is represented using four variables measuring active criminal justice status at the time of current offense and numbers of past felony arrests, prison terms and jail terms. We also include sex, race and age. Although these factors are not legitimate sentencing factors, they may be correlated with unobserved factors that are legitimate. Failure to control for these factors could be particularly problematic in our case since our jurisdictions have different case mixes. The results are not sensitive to whether or not these demographic variables are included.

Instead of comparing the proportion of charge bargains across each jurisdiction as in Miethe (1987), we estimate how much difference in sentence length can be attributed to the charge bargain. Using the estimated coefficients from Eq. 2, we form a predicted sentence length. This predicted value represents the systematic component of sentencing. Because of the nature of the State Court Processing Statistics, we also know the most serious crime type for which the person was charged in the arraignment. We use this to create a second predicted value for each individual based on the most serious charge at arraignment. In other words, we create an estimate of a person's *expected sentence at arraignment*, again using the estimated coefficients from Eq. 2.

We then calculate the difference between each person's expected sentence at arraignment and the predicted actual sentence. The difference represents the effect of charge bargaining. This estimate is unbiased as long as the relevant predictors have been included in the models. If there are omitted case factors that make the conditional sentences of those with charge bargains systematically different from those without charge bargains, then the estimate will be biased. This is the same issue faced in Smith (1986). Testing this assumption requires still richer data than is generally available in single-jurisdiction studies, much less available in comparable forms across jurisdictions.

To control for differences in absolute variation in the two jurisdictions, we normalize the estimate by dividing by the average predicted sentence in each jurisdiction. Based on Reitz's hypothesis about the displacement of discretion in the face of presumptive guidelines, we expect to see a larger difference due to charge bargaining (as a proportion of the average sentence) in Washington than in Maryland.<sup>10</sup>

<sup>10</sup> We will show in Sect. 5 that Washington has shorter sentences than Maryland. This in part reflects the elimination of the parole board in Washington (Hughes et al. 2001). Therefore, 1 month of sentence in Washington has greater impact on the sentencing outcome as defined by Reitz (1998) than the same month in Maryland. Measuring change in discretion as a proportion of the average sentence eliminates this potential source of bias (the effect of parole is the same in both the numerator and denominator, and cancels out).



Before proceeding, it is important to note that we are not claiming that this model captures all of the discretion of prosecutors, only that which is associated with bargaining over charges. There may be other forms of bargaining. (For example, bargaining may also take place over which aspect of a defendant's criminal history will be included during adjudication.) Or, what may appear to be bargaining may simply reflect changes in the "facts of the case" produced by investigation. Prosecutorial discretion also causes cases never to appear in the criminal justice system. This "disabling" discretion is not generally observable (Reitz 1998). As a result of these limitations, we cannot draw conclusions about the total amount of prosecutorial discretion in the two systems. However, we feel reasonably certain that these limitations will not affect a comparison of the *relative* amount of prosecutorial discretion in the two systems.

## Data and Empirical Results

To compare these two jurisdictions, we use data from the publicly available State Court Processing Statistics. The data are collected by the Pretrial Services Resource Center (PSRC) under contract with the Bureau of Justice Statistics and are intended to represent the population of all felony cases brought in May 1998 in the 75 largest counties in the United States. PSRC follows cases from filing to disposition or for 1 year, whichever occurs first. PSRC collects data on arrest charges, demographic characteristics, criminal history, pretrial release and detention, adjudication and sentencing from administrative records. PSRC makes some attempt to make sure the information collected is consistent across jurisdictions, but it is limited by the information available in the administrative records.

The PSRC does the sampling in two stages. In the first stage, they divide the 75 largest counties in the United States into four strata based on population size. From these strata, they chose 40 counties. In the second stage, they chose days in May, choosing more days for the smaller counties. For each of the selected counties, the sample includes all felony defendants indicted. They provide weights for inference at the county and "largest 75 counties" level. They also provide information to correct standard errors for clustering and stratification. Throughout the paper, we present results properly weighted to provide correct inference across counties and adjust standard errors to reflect the multi-stage sampling.

In this paper, we are interested only in data from Washington State and Maryland. We were initially interested in more states, but Washington State is the only state that has a presumptive or strict guideline system and also has at least one county in the sampling frame of the SCPS dataset. The other three states with strict or presumptive guidelines are North Carolina, Kansas, and Minnesota.

We looked at data from the 1996, 1998 and 2000 versions of SCPS. In 2000, data were not collected in any Washington counties. In 1996 and 1998, data were collected on King County Washington (Seattle) and three counties in Maryland (Montgomery—a wealthy suburb of Washington, DC; Baltimore County, a suburb of Baltimore City; and Baltimore City). To code the differences in charge at indictment and at adjudication, we use the actual statute listed in the data, and tracked it back to its status as a felony or a misdemeanor. These data are missing in the 1996 version of SCPS, which forced us to limit our analysis to the 1998 data, which eliminated Baltimore County from consideration. Because we analyze only plea-bargained

cases, we omitted all cases that were pending after 1 year and those settled at trial. We are left with a case study of 497 total cases, which limits our ability to generalize to these states, let alone jurisdictions more generally. In Maryland, we have 187 observations from Baltimore City and only 53 observations from Montgomery County, which means the main results will be driven by Baltimore City.

Descriptive statistics, presented in Table 1, reveal substantial differences between the data from the two states. Among felony arraignments that are resolved by a plea bargain within 1 year, 82% included a prison sentence in our two Maryland counties compared to 71% in King County, Washington. Sentences were also longer in the Maryland counties: 27 months in the Maryland counties among those with many prison time and close to 17 months in King County. The differences in time served, or effective sentence length, will not be this large, as Maryland has discretionary release whereas Washington does not.

The case characteristics, not surprisingly, also differ somewhat across the sites. Over half of the cases in the Maryland counties were drug convictions. In King County, drug offenses were also the most common type, representing 37% of the convictions. In both locations, over 20% were convicted of person and property offenses.<sup>11</sup> There is a stark difference between the rate at which these arraignments were resolved by a plea to a lesser, misdemeanor charge. In the Maryland counties, over 30% of the cases were settled with a misdemeanor as the most serious offense, over twice the rate in King County (15%).

The criminal history profiles, on average, look somewhat different also. Convicted offenders in King County are less likely to have had an open case. On average, the King County sample has more past felony convictions, but fewer terms in both jail and prison. This suggests that incarceration is used less heavily in King County, a fact that is consistent with their low incarceration rates measured against overall population.<sup>12</sup> Finally, the demographic profile is similar in terms of age and the proportion female. The two Maryland counties have a much higher proportion African American (over 85% versus 44%); King County has a much higher proportion of European descent (43% versus 9%).

Table 2 presents a general picture of case processing. Note that these numbers do not control for demographics or case characteristics (which will be done in later tables). Here, we characterize cases by the crime type of the original (most serious) felony charge. Among each crime type, the percentage of cases in which the most serious charge of conviction is the same as the most serious arraignment type is 90% across the board. The final column for each site shows the percentage convicted of a misdemeanor. In all cases, we find that the Maryland counties have a higher rate of misdemeanors. Within state, the largest percentage misdemeanor is among those arrested for person offenses. Bargaining of this type is less common among drug and property arrestees.

We now turn to the multivariate results. Table 3 presents the basic models. Our goal is to use this model to isolate systematic variation in sentencing from idiosyncratic variation. Including demographic controls might help for two reasons. First,

<sup>11</sup> Note that these values are not comparable to published statistics from the SCPS. The data file considers “misdemeanor” and the crime type categories to be mutually exclusive. We recoded the offense information to create a true representation of crime type independent of the felony/misdemeanor dimension.

<sup>12</sup> In 2001, 422 people per 100,000 population were incarcerated in Maryland. The comparable figure for Washington was 249 (U.S. Department of Justice 2002b).

**Table 1** Descriptive statistics: felony cases disposed by plea within 1 year of arraignment (SCPS 1998)

	Maryland	Washington
Sentence outcomes		
Prison	0.822 (0.018)	0.712 (0.016)
Sentence (months)	22.12 (1.323)	11.82 (0.752)
Sentence, if prison (months)	26.93 (1.528)	16.60 (0.898)
Conviction offense		
Person	0.207 (0.023)	0.241 (0.020)
Drug	0.524 (0.039)	0.370 (0.130)
Property	0.247 (0.021)	0.253 (0.028)
Public order	0	0.070 (0.030)
Other	0.022 (0.006)	0.062 (0.011)
Misdemeanor	0.311 (0.020)	0.148 (0.019)
Criminal history		
CJ status time of offense	0.406 (0.022)	0.195 (0.020)
Number of prior felonies	0.777 (0.067)	1.319 (0.080)
Number of prior jail terms	1.435 (0.116)	0.875 (0.058)
Number of prior prison terms	1.061 (0.101)	0.319 (0.030)
Demographics		
Female	0.132 (0.012)	0.140 (0.015)
White	0.090 (0.012)	0.432 (0.033)
Black	0.866 (0.018)	0.436 (0.037)
Other race	0.044 (0.015)	0.132 (0.014)
Age	30.44 (0.377)	31.54 (0.207)
N	240	257

**Table 2** Case processing by arrest offense

	Maryland		Washington	
	Arrest offense	% Misdemeanor	Arrest offense	% Misdemeanor
Person	22.3%	40.7%	25.7%	18.2%
Drug	53.6%	25.3%	37.7%	14.4%
Property	24.1%	35.1%	25.7%	13.6%
Public order	0		6.2%	12.5%
Other	0		4.7%	8.3%

Cells are blank where there are fewer than ten cases. The first column for each site shows the distribution of offense type, and sums to one. The second column reports the percent of each offense type that ends as a misdemeanor

while we have extensive information on criminal histories, our specification has somewhat limited information, relative to guideline datasets, about the current crime. Second, if the demographic variables are systematically related to sentence outcomes (whether or not this is appropriate and/or legal) they are part of the systematic variation. As such, this variation should be absorbed with the variation systematically related to case characteristics.

The findings of our sentence length models are in accord with the literature. In the Maryland counties, those convicted of person offenses have longer sentence lengths, on average, than those convicted of drug offenses. In both jurisdictions, the coefficient on “misdemeanor” is negative and strongly significant, as is expected; longer criminal histories are associated with longer sentences; and women tend to have

**Table 3** Tobit regressions of Ln(sentence length)

	Maryland	Washington
Person	0.800* (0.119)	-0.380 (0.223)
Property	-0.574 (0.290)	-1.467* (0.197)
Public order	N/A	-0.789* (0.285)
Other	1.125* (0.300)	-0.423 (0.337)
Misdemeanor	-1.451* (0.192)	-2.904* (0.304)
Person × misdemeanor	-0.075 (0.376)	1.311* (0.537)
Property × misdemeanor	0.185 (0.396)	1.649* (0.506)
Public order × misdemeanor	N/A	0.799 (0.369)
Other × misdemeanor	-1.748* (0.400)	-4.739* (0.535)
CJ status at time of offense	0.348* (0.097)	-0.203 (0.180)
Prior felonies	0.207* (0.073)	0.223* (0.068)
Prior jail terms	0.055 (0.049)	0.117 (0.082)
Prior prison terms	0.096 (0.060)	0.405* (0.104)
Female	-0.936* (0.252)	-0.302 (0.224)
White	-0.064 (0.328)	-0.037 (0.109)
Race missing	-0.003 (0.263)	-0.789* (0.132)
Age	0.099* (0.044)	-0.024 (0.032)
Age squared	-0.0015* (0.0007)	-0.0001 (0.0004)
N	240	257

Drugs is the omitted offense category

\* Indicates the coefficient is significant at the 5% significance level

shorter sentences controlling for other factors (though there is a small sample so it is not precisely measured).<sup>13</sup>

We now turn to the main results of the paper starting in Table 4. As described earlier, our statistical approach is most appropriate when used to compare the relative amounts of variation within jurisdictions. Table 4 contains predicted sentence length from Tobit models for the counties from the two states. First, focus on the first three rows in the Maryland column. The first entry contains the predicted sentence length from the model reported in Table 3 for the Maryland counties. The second row contains the prediction from applying the estimated coefficients of this model to the arraignment characteristics of each individual. Thus, it contains an estimate of the systematic part of sentencing that would occur if there were no charge bargaining (and nothing else changed in sentencing practice). In the first column, as expected, the predicted sentence length is higher using the second method. If people in the two Maryland counties were sentenced based on their arraigned crime using the sentence magnitudes associated with convictions offenses, the average sentence would be longer by 3.08 months (row 3), or a decrease of 22.2% from the sentence expected at arraignment. That is, sentences are 22.2% shorter due to charge bargaining (most of which takes place through the reduction to a misdemeanor, as shown in Table 2). To see a similar set of results for King County, Washington, consider the last three rows in the second column. Relative to the Maryland counties, average predicted

<sup>13</sup> As a robustness check, we re-ran all models without demographic controls and found no qualitative difference. In response to an anonymous referee, we also ran a model with age, race and sex interactions to more accurately reflect the literature. Because of the missing data on race, and the small sample sizes (in particular for female), these models led to instability in the main coefficients of interest. We focus on the results from the model without the interactions because the focus of this article is not on the extra legal effects of demographic characteristics.

**Table 4** The role of charge bargaining on sentence length: Tobits Maryland and Washington

	Sample from the two counties in Maryland	Sample from King County, Washington
<i>Predictions based on coefficient estimates from Maryland</i>		
Predicted sentence using convicted offense	10.80	12.61
Predicted sentence using arrested offense	13.88	13.78
Change as a result of charge bargaining	-3.08 (-22.2%)	-1.17 (-8.5%)
<i>Predictions based on coefficient estimates from King County, Washington</i>		
Predicted sentence using convicted offense	12.41	7.59
Predicted sentence using arrested offense	26.91	8.51
Change as a result of charge bargaining	-14.50 (-53.9%)	-0.92 (-10.8%)

sentences are shorter by 0.92 months as a result of charge bargaining, and the impact of charge bargaining is less—on the order of 10.8%.

These results suggest two things. First, charge bargaining to a misdemeanor contributes a substantial amount of variation in sentencing outcomes. Second, there is more “value” to a charge bargain in the Maryland counties relative to King County, contrary to the prediction of the theory. However, as noted earlier, differences in the criminal populations can make direct comparisons problematic. The apparent finding that King County, Washington has less discretion at the plea bargaining stage may be because the distribution of offenders in King County, Washington is much more compressed (toward zero) because of the lower use of incarceration (both through fewer sentences involving prison and the shorter sentences conditional upon incarceration). If the initial charge will generally lead to a probation sentence or very short jail term, then we will be unable to see movement in outcomes due to plea bargaining because all non-prison sentences are treated as equivalent. It is not until we consider cases that include the possibility of a substantial incarceration sentence that we are able to see the effect of plea bargaining on outcomes.

To deal with this problem, we isolate the differences in the sentencing practices from the differences in population characteristics using the Oaxaca (1973) decomposition first developed in economics. To hold population constant, we in effect want to ask what would happen if Maryland offenders were sentenced in King County and vice versa. With this exercise, we can compare sentencing practices the Maryland counties with sentencing practices in King County on the same group of people. It is well known that the quantities resulting from these measurements may differ substantially depending upon which population is used for the comparisons so we adopt the standard practice of reporting both sets of calculations.

Because the estimated coefficients from the regressions are measures of the sentencing practices, we can perform this decomposition by simply applying the King County coefficient estimates to the Maryland data:

$$\hat{Y}_i^{MW} = F\left(X_i^M \hat{\beta}^W\right) \tag{3}$$

where  $X_i^M$  is a vector of characteristics from an offender in Maryland and  $\hat{\beta}^W$  are the estimated coefficients from the regression using King County data. The bottom three

rows of column (1) reports predicted sentences using this approach. In other words, we ask what would happen if the population of arrestees from Maryland were charged and sentenced in King County, rather than in the two Maryland counties. Because Maryland arrestees tend to have more serious offenses and criminal histories, this decomposition has the practical effect of measuring the King County, Washington sentencing practices using a higher part of the distribution of offenders. In this scenario, the difference between the sentence length using the adjudication and arraignment charges is 14.5 months, or a 53% decline. Comparing across the columns for the last row of the table, we see a larger effect of charge bargaining using the King County estimated coefficients on the Maryland data (53.9% decline in the predicted sentence) than we found using these same coefficient estimates on the King County, Washington data (10.8% decline in the predicted sentence), suggesting that the concentration of low-level offenses in the Washington population leads to compression in the final estimate of the impact of plea bargaining on sentencing outcomes.

We perform the same exercise in reverse by applying Maryland county coefficient estimates to the King County data in the first three rows of the second column.

$$\hat{Y}_i^{WM} = F\left(X_i^W \hat{\beta}^M\right) \quad (4)$$

In effect, we are asking what would happen if the arrestees from King County were prosecuted in our two Maryland counties. We find that there is a difference between charge at adjudication and arraignment of 1.17 months, or an 8.5% decline. Comparing the third row of the table across columns, we find that there is a smaller measured effect of charge bargaining using the Maryland county coefficient estimates on the King County data (−8.5%) than on the data from the two Maryland counties (−22.2%).

Having performed the decomposition, we are now ready to compare the impact of sentencing practices in our two Maryland counties and King County, Washington on the same populations. Recall that we predicted that the plea bargaining in Washington will lead to a bigger impact on outcomes than plea bargaining in Maryland, holding population constant, because of the different types of guidelines. This final comparison can be done by comparing the third row of the first column with the sixth row of the first column, and the third row of the second column with the sixth row of second column. The first comparison is of Maryland and Washington sentencing practices (at least as evident in our three counties) on the population from our two Maryland counties, and the second comparison is of Maryland and Washington sentencing practices on King County's population. If Reitz (1998) is correct, we should find more discretion at the charge bargaining stage in Washington than in Maryland.

In the first case with the Maryland county data, we see that there is a 22.2% decline in sentence length from the arrest to conviction using the estimated Maryland coefficients (third row, first column), but a 53.9% decline in sentence length from the arrest to conviction using the estimated King County coefficients (sixth row, first column). In the second case with King County data, we find that there is a 8.5% decline in sentence length from the arrest to conviction using the estimated Maryland coefficients (third row, second column), but there is a 10.8% decline in sentence length from the arrest to conviction using the estimated King County

coefficient estimates (sixth row, second column). Regardless of which population of offenders we use, the estimated King County coefficients yield a larger estimate of the impact of charge bargaining on sentencing outcomes. This is consistent with the prediction of Reitz (1998) that more structured sentencing will lead to increased discretion by the prosecutors.

Finally, to explore further the issue of data compression in our results, we ran probit models of the incarceration/non-incarceration margin. These results are reported in Table 5. Using each state's own estimated coefficients to form predictions, we found effects of charge bargaining on the likelihood of receiving a prison sentence similar to those for sentence length. In the Maryland counties, the effect is about 6.8% points, or a 7.7% difference in the probability of a sentence including incarceration (see third row, first column). In King County, the plea bargaining effect is 7.4% points, which is proportionately 9.5% (sixth row, second column). The results are clearer in the decomposition. Using the Maryland population, the decline in the probability of an incarceration for Maryland estimated coefficients is 7.7%, but the decline is 13.8% using the King County coefficient estimates (sixth row, first column). With the King County population, the decline in the probability of incarceration is 4.6% using Maryland coefficient estimates, but the decline is 9.5% using the King County coefficient estimates (third row, second column). In each case, we get results consistent with the earlier interpretation: applying Washington's estimated coefficients leads to higher estimates of the effect of charge bargaining in both states.

Our findings are clearly limited by the small sample and the use of only three counties in two states.<sup>14</sup> It is clearly possible that the results would be different if we had access to data from the all counties in each state. In addition, there are numerous other differences between Washington and Maryland which could account for these differences besides sentencing guidelines. Nevertheless, these results are consistent with the predictions of Reitz (1998) that charge bargaining is more important under presumptive guidelines than it is under an advisory guidelines system. The results are also consistent with those of a Washington State case study of drug prosecutions (Engen and Steen 2000), which found that conviction patterns were different after a change in the law increasing drug sentences. They are also consistent with the findings of D'Alessio and Stolzenberg (1995) on the increased use of jail sentences following the introduction of guidelines in Minnesota.

In addition to identifying the possible hydraulic displacement of discretion to prosecutors, we have also developed a way to quantify the magnitude of these shifts in practice. By measuring the average change in expected sentence due to charge bargaining, we reveal the relevance of prosecutorial practice, not merely its existence. The gap in expected sentence is a magnitude that can be assessed by both defendants and policy makers, as it is expressed in a metric relevant to both.

<sup>14</sup> An anonymous referee suggested we attempt to do the analysis described below for the two counties in Maryland separately to help build intuition about the extent to which the observed differences were truly state or jurisdictional differences, and not differences only at the county level. Because of the small sample size of Montgomery County, this exercise does not have the power we would prefer. Nonetheless, the pattern reported below holds for both counties relative to King County. The effect of charge bargaining is substantially larger in absolute terms in Montgomery County than in Baltimore City (8.24 months versus 2.52 months).

**Table 5** The role of charge bargaining on the probability of incarceration: probits Maryland and Washington

	Sample from the two counties in Maryland	Sample from King County, Washington
<i>Predictions based on coefficient estimates from Maryland</i>		
Predicted probability, convicted offense	0.818	0.812
Predicted probability, arrested offense	0.886	0.851
Change as a result of charge bargaining	-0.068 (-7.7%)	-0.039 (-4.6%)
<i>Predictions based on coefficient estimates from King County, Washington</i>		
Predicted probability, convicted offense	0.722	0.705
Predicted probability, arrested offense	0.838	0.779
Change as a result of charge bargaining	-0.116 (-13.8%)	-0.074 (-9.5%)

### Discussion of Substantive Findings

Perhaps the most powerful implication of this work is the caution it provides with regard to interpreting results from single-jurisdiction analyses. Although the empirical models are quite standardized in the literature on sentencing disparity, differential charge bargaining across jurisdictions suggests that identically parameterized models may be picking up different variation in different jurisdictions. For example, consider two recent estimates of the correlation between race and sentencing outcomes reported by Engen and Gainey (2000) and Bushway and Piehl (2001). These two papers estimated similar models on guideline conviction data in Washington State and Maryland, respectively, finding quite different estimates of the relationship between race and sentencing outcomes. Engen and Gainey estimated that African Americans have sentences that differ little from those of whites, while Bushway and Piehl found a substantial and statistically significant relationship. It is possible that small differences in modeling account for the divergent results or that the states have very different levels of racial disparity in sentencing outcomes. Yet it is also possible that the “meaning” of the empirical models varies with the institutional context. That is, more of the discretion has occurred in Washington prior to the sentencing stage when compared with Maryland.

This third interpretation is supported by the small amount of unexplained variation in the model of Engen and Gainey after controlling for the case characteristics at conviction. One approach to measuring the total amount of sentencing disparity, taken by Barry and Greer (1981), Carrow et al. (1985), Stolzenberg and D’Alessio (1994), and Rhodes (1991), is to define unsystematic (or unwarranted) disparity as the residual variation from a model in which sentence length is explained only by the factors usually considered to be legitimate in sentencing. An easy way to implement this idea is to use the  $R^2$  from a regression of the form of Eq. 1 as a measure of the proportion of the variation that is systematic.

In the SCPS data, we estimated Eq. 1 for log sentence length by ordinary least squares for several samples in both jurisdictions. For the full sample of arraignments, the  $R^2$  is substantially higher in King County (0.3436) than in the two Maryland counties (0.1887). This ranking continues to hold as the sample is restricted to just those convicted of felonies (0.3660 versus 0.2266) and to those sentenced to terms of



incarceration (0.3996 versus 0.1758). This simple exercise suggests that King County has less unexplained variation than does the Maryland counties.<sup>15</sup> Consistent with the results from Engen and Gainey (2000) and Bushway and Piehl (2001), in the SCPS data, race has a larger impact in the two Maryland counties than it does in King County, Washington.

The results of this paper suggest that the differences in the two jurisdictions matter for the interpretation of variation at different stages of the sentencing process. Jurisdictions with “tight” sentencing schemes such as Washington State are characterized by narrow, presumptive sentencing ranges. Studies in these jurisdictions using conviction data tend to find relatively small amounts of unexplained variation. On the surface, such findings suggest that the guidelines are working to constrain discretion. However, if discretion is located elsewhere in the system, these measures may understate sentencing disparity. Moreover, under such circumstances it would not be surprising to find that illegitimate factors are not strongly correlated with variation in sentences after controlling for the conviction characteristics. But this says nothing about whether these illegitimate factors account for variation in the plea bargaining process.

To tentatively explore this characterization with our limited data on sentencing, we estimated a simple linear regression model of the decision to plea bargain to a misdemeanor using the same array of legitimate factors used above for the SCPS data from the Maryland and Washington counties. The  $R^2$  for these two models are 0.0403 for Maryland and 0.0292 for Washington. We then re-ran the same models with the addition of race, sex, age and age squared. The  $R^2$  in these two models increased to 0.0514 in the Maryland county regression and 0.0801 in the King County, Washington regression. The larger relative increase in explanatory power in King County, Washington versus the two Maryland counties is consistent with the hypothesis that the discretion at the sentencing phase in the strict guideline state has simply been displaced to the plea bargaining stage.

The ultimate ability of this exercise to determine causality is limited because of parsimonious descriptions of crime severity and criminal history in the SCPS data and the use of only three counties in two states. Nonetheless, we do feel comfortable concluding that stricter sentencing structures are more likely to have discretion in sentencing take place before the point at which researchers generally assess discretion with conviction data. This is a troubling reality: rules to control discretion may shift the discretion so that it is invisible to the researcher. Because the shift is likely to be greater in cases where the rules are stricter, the result is the appearance of success in increasing consistency. Without going so far as to recommend dramatic new data collection efforts, data collection in SCPS can and should be improved by (1) developing an inclusive measure of sentencing outcome for a set of cases not selected on the basis of that outcome and (2) collecting more information from earlier stages, such as the charging information used in standard guideline datasets.

<sup>15</sup> A related test is reported in Bushway and Piehl (2001, p. 759). Both Engen and Gainey (2000) and Bushway and Piehl (2001) argued that the presumptive sentence is a better measure of how legitimate factors should influence sentencing outcomes than the traditional approach of controlling for the scores that determine the presumptive sentence. The substitution of the presumptive sentence for the criminal history and offense scores increases the explanatory power in Washington and leads to a reduction in explanatory power in Maryland, again suggesting that there is less unsystematic variation in Washington.

## Conclusion

In this paper, we have outlined a procedure that can measure the difference in sentencing outcomes caused by plea bargain. We have then applied it to compare prosecutorial discretion across two different jurisdictions. Our approach requires data on the charges earlier in the process, not just at conviction. This requirement is not often met in the conviction data sets currently used in the literature. While not perfect, the BJS-funded SCPS dataset has enough of the necessary information to allow us to at least demonstrate the utility of our approach.

We have demonstrated that charge bargaining plays an important role in determining sentencing outcomes. Furthermore, we have demonstrated that measuring the difference in months of prison time determined by the charge bargain may provide a very different estimate of the discretion than is given by the rate of bargaining. Although the *rate* of charge bargaining was higher in the voluntary guidelines state, its *impact* on sentences was greater in the presumptive guidelines jurisdiction, as predicted. The dramatic difference in predictions that occurred from shifting the case characteristics cautions that distributional differences (either due to the underlying criminal activity or due to the overall level of severity of punishment) can easily obscure the inferences necessary for understanding the operation of the systems. While it is clearly more informative to study the value of the charge bargain than its existence, measuring that value from a reference point requires strong assumptions to form the counterfactual. Future research should work to test the appropriateness of these, and alternative, assumptions.

The finding of differential charge bargaining in these two jurisdictions should provide a caution when comparing the results of studies of disparity in sentencing across jurisdiction types, as the conviction information in more structured systems may represent systematic movement from the arraignment charge. *Ceteris paribus* we expect to find less measured disparity in studies of highly structured systems with conviction data than in more loosely structured systems even if both systems contain similar amounts of total disparity. Judges are not the only actors in the system with discretion—they are merely the most obvious and easily measured source of disparity. The *Blakely v. Washington* decision makes it clear that the exercise of discretion within different sentencing structures is not solely of academic interest but is of broad policy interest. Research that can speak to the differential use of discretion within different sentencing structures can help inform legislatures throughout the country as they struggle to respond to the demands of *Blakely v. Washington*. At the same time, changes in sentencing structures in response to recent Supreme Court decisions should also provide new opportunities to investigate the allocation of discretion within the criminal justice system.<sup>16</sup>

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## Appendix

### Institutional features of Maryland and Washington

In May 1983 Maryland's Judicial Conference, a body of all Maryland judges, voted to apply guidelines statewide beginning July 1, 1983.<sup>17</sup> The judges took this action voluntarily, without legislative mandate. These guidelines were administered by a sentencing guidelines board of 14 justices representing each of Maryland's judicial circuits. The guidelines had four explicit goals: (1) increasing equity in sentencing, (2) articulating an explicit sentencing policy with a regular basis for review and change, (3) providing information for new judges and (4) promoting increased visibility and understanding of the sentencing process.

The guidelines apply to circuit courts only, with separate guideline grids for crimes against the person, crimes against property and controlled dangerous substance crimes. The sentences are based on the seriousness of the primary offense and the offender's prior criminal history record. The guidelines board placed all crimes into five seriousness categories. Offense seriousness for person offenses also takes victim injury, weapon usage and special vulnerability of the victim into account. The "prior criminal history" score is based on prior adult criminal record, prior adult parole and probation violations, prior juvenile record (for offenders age 25 or younger), and relationship to criminal justice system at the time of the offense. The sentencing board considered but ultimately rejected the formal use of mitigating factors. Mitigating factors such as "providing substantial assistance" are to be taken into account by the judge with no formal advice from the guidelines board. No official list of proscribed factors is provided. The guidelines themselves are voluntary, with no penalty for sentencing outside of the range provided by the guidelines. The sentence ranges attached to the grid are broad and deliberately descriptive, rather than prescriptive. In 2000, Maryland became a legislatively mandated guideline state, although the guidelines remain descriptive and voluntary.

In contrast, the guidelines in Washington have been legislatively mandated from their inception in 1984. Guidelines apply to all felony cases, and all cases are sentenced on one 10 × 15 grid. The mission statement for the commission has at least five goals: (1) impose a sentence structure that ensures that the punishment for an offense is proportionate to the seriousness of the offense and an individual's criminal history, (2) increase consistency in sentencing, (3) protect the public, (4) provide the offender with an opportunity to improve him- or herself and (5) make frugal use of the state's resources.<sup>18</sup> Thus, the commission did not intend to remove discretion from sentencing, but did intend to "structure" that discretion so that the goals were met. For example, judges are allowed and even encouraged to use alternative/intermediate sentences for non-violent offenders sentenced to 1 year or less in prison.

The crime seriousness level is determined by the classification of the offense of conviction. The criminal history score is calculated using a points system based on the number of prior felony criminal convictions, the relationship between prior offenses and the current offense, the presence of multiple prior or current convictions, and the custody status of the offender when the crime was committed. Sentences are determinate and the guidelines are presumptive, with departures

<sup>17</sup> This section draws heavily upon Administrative Office of the Courts (1993, Unpublished manuscript).

<sup>18</sup> A goal was for the reform not to lead to an increase in incarceration.

allowed only for substantial and compelling reasons. Justification of a departure from the guidelines must include a written explanation of findings of fact and conclusions of law leading to the departure sentence. Departure sentences are subject to appeal by either the defendant or the prosecutor.

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