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CRIMINAL JUSTICE SYSTEM**

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The Role of Discretion in the Criminal Justice System
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ABSTRACT

Although a substantial body of research suggests that the discretion of actors in the criminal justice system is important, there is disagreement in the existing empirical literature over its specific role. Studies in this literature generally hypothesize that discretion plays one of two roles: either it serves as the means by which changing broad social norms against crime causes changes in sentencing patterns, or it serves as the means by which internal social norms of the criminal justice system prevent the implementation of formal changes in laws.

We reject both of these hypotheses using data on the sentencing of California prisoners before and after the passage of Proposition 8, which provided for sentence enhancements for those convicted of certain “serious” crimes with “qualifying” criminal histories. We find that an increase in the statutory sentence for a given crime can increase sentence length for those who are charged with the crime, and also for those who are charged with factually “similar” crimes, where a “similar” crime is defined as one that has legal elements in common with the given crime. These spillovers are consistent with neither broad social norms nor internal social norms, so we conclude that discretion takes a less-well studied form, which we call “prosecutorial maximization.”

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Discretion is the means by which actors in the criminal justice system substitute their own judgment, interests, or objectives for formally specified statutory punishments in order to influence criminal justice outcomes. In this context, discretion can enable judges, prosecutors, defense attorneys, and/or juries to implement sentencing policies in line with their own views, even if these policies conflict with formal law. Discretion of this sort is frequently vilified as the means by which judges and parole boards treat criminals leniently and are “soft on crime” (e.g., Lipson and Peterson 1980; Casper, Brereton, and Neal 1982); it can also facilitate unwarranted and undesirable disparities on the basis of characteristics such as race (e.g., Waldfoegel 1993). On the other hand, discretion can enable participants to implement policies consistent with broad “social norms” that may or may not reflect formal law. For example, researchers have attributed secular trends in prison commitment rates in the 1970s in California to an increasingly punitive social norm against relatively minor crimes (e.g., Cohen and Tonry 1983).

Because discretion can enable participants in the criminal justice system to nullify legitimately-adopted sentencing laws and to impose inequitable sentences based on irrelevant characteristics of defendants and crimes, both policy makers and scholars have been concerned about the its effects on criminal justice outcomes. In response, Congress and several states’ legislatures have sought to limit discretion in sentencing by replacing indeterminate sentencing laws (ISLs) with sentencing guidelines or determinate sentencing laws (DSLs).¹ For a given criminal offense, DSLs specify statutorily a fixed prison term or a narrow range of acceptable terms among which the judge can choose. In contrast, ISLs specify a much wider range of acceptable prison terms, leaving discretion over the actual sentence that a convict would serve jointly to judges and an entity like a parole board. For example, California’s DSL specifies that

conviction of second-degree burglary (unlawful entry into an uninhabited structure with intent to commit larceny or any felony) carries a prison term of 24 months, although the judge can impose a sentence of 16 or 36 months in the presence of “mitigating” or “aggravating” factors. Before passage of the DSL, California ISL provided that conviction of second-degree burglary carried a prison term of 12 to 168 months.

Despite this policy importance, empirical evidence about the influence of social norms and discretion on sentencing patterns has been mixed. Some studies report that discretion plays a relatively minimal role, with DSLs causing substantial changes in sentencing patterns; some report that participants exercise partial leeway, particularly in the application of DSLs they consider to be unduly harsh; and some report that DSLs have no causal effects, either because the statutory changes would have been implemented regardless or because statutory restrictions on discretion are completely undone by participants’ behavior. These studies generally hypothesize that discretion plays one of two roles: either it serves as the means by which changing broad social norms against crime cause changes in sentencing patterns (with concurrent changes in formal laws reflecting broad social norms but not causing criminal justice outcomes), or it serves as the means by which internal social norms of the criminal justice system prevent the implementation of formal changes in laws.

In this paper, we test the hypotheses that discretion plays one or both of these roles against the hypothesis that discretion takes a less-well-studied form. We suggest that an increase in the statutory sentence for a given crime can increase sentence length for those who are charged with the crime, and also for those who are charged with factually “similar” crimes, where a “similar” crime is defined as one that has legal elements in common with the given crime. In this

case, discretion is the means by which prosecutors seek to maximize punishment, subject to the constraints of laws (e.g., Altschuler 1968). Instead of causing an apparently statutory change in sentencing policy or inhibiting change in sentencing, we hypothesize that discretion can have spillover effects that intensify the effects of changes in laws.

We label this hypothesis "prosecutorial maximization" to distinguish it from the two traditionally-hypothesized roles of discretion discussed above. Although prosecutorial maximization is itself in some sense a social norm, it differs from traditional norm-based theories of discretion in at least two ways. First, it arises out of a formal model of individual behavior, with a maximand that is consistent with the explicit incentives facing actors in the system. Second, it generates different predictions about the impact of discretion on criminal justice outcomes.

We investigate the hypothesis that internal social norms in the criminal justice system inhibit the implementation of laws by estimating empirically the effects on sentencing patterns of California's Proposition 8, a popular referendum, which increased the statutory sentence for defendants with criminal histories who committed one or more certain "qualifying" crimes. We reject this hypothesis. Based on a sample of 10,520 California prison admits who committed crimes from 1981 to 1985, we find that defendants with criminal histories charged with a qualifying crime experienced statistically significant increases in prison sentences as a result of Proposition 8, as did defendants with criminal histories who were charged with a nonqualifying crime that was factually similar to a qualifying crime.

We also specifically investigate the hypothesis that changing broad social norms against crime caused the changes in sentencing patterns that were apparently caused by Proposition 8.

We explore this possibility by examining changes in sentence lengths before and after adoption of Proposition 8 for convicts subject to the new law *relative to* changes in sentence lengths for convicts not subject formally to the new law, but who would have been likely to be affected by well-defined changes in social norms that might have been correlated with the referendum. We also reject this hypothesis. We find that sentence lengths for convicts who were formally subject to the law exhibited statistically significant increases relative to sentences for convicts who were not subject to the law but who might have been affected by changes in norms. Our attempts to disentangle traditional norm-based theories of discretion from the prosecutorial maximization theory depend crucially on our choice of comparison groups; we discuss this at length below.

Our empirical results have several implications. Broadly consistent with the previous literature, we find evidence of discretion in the criminal justice system, although the form and extent of this discretion differs from the findings of the previous empirical literature. Participants in the criminal justice system do not have sufficient discretion to completely undo changes in laws. Statutory increases in sentences result in increased punishments. Furthermore, we do not find evidence that broad social norms, rather than changes in formal law, are the true cause of changes in sentencing patterns. Instead, we find results consistent with discretion and a model of prosecutorial maximization: increases in statutory sentences result in more punishment, not less punishment, than the simple statement of the laws would suggest.

Our paper has four sections. Section I discusses the previous literature on discretion and its implications for the impact of increases in determinate sentences on the criminal justice system. Section II discusses the details of California's Proposition 8 and our experiment. Section III presents the data on prisoners that we use and our empirical results, and section IV concludes.

I. DISCRETION AND THE IMPLICATIONS OF INCREASING DETERMINATE SENTENCES

There is substantial anecdotal evidence that judges and juries can exercise some discretion in the litigation process to not implement the letter of the law. Both judicial and juridical nullification in the face of restrictions on choice of punishment is a tradition of common-law systems dating from medieval England (Lachman 1981). Most recently, the Supreme Court of California upheld a lower court's decision not to implement California's "Three Strikes and You're Out" law specifying long prison terms for repeat offenders (*People v. Superior Court of San Diego County (Jesus Romero)*, 1996 Cal. LEXIS 3025 (June 20, 1996)).

Potentially more important, however, is the extent to which the entire community of actors in the criminal justice system can use discretion in settled cases to nullify the law. Virtually all defendants' cases are settled by plea-bargain, and therefore are never subject explicitly to an application of formal law.² Research in sociology and criminology suggests that this may enable actors in the system to resolve the vast majority of cases according to their own internal social norms rather than statutory rules. Plea negotiation, according to this reasoning, is carried out by court professionals who must dispose of more cases than could possibly be tried to a verdict, given constraints on resources. To handle this volume of cases, the participants in the system develop rules of thumb, or social norms, through which they categorize cases into different punishments. Once defense attorneys, prosecutors, and judges have agreed on categorization rules, they become resistant to changing them. In fact, these informal rules may take on a life of their own, whereby court participants working within a "subculture of justice" develop a "going rate" prison sentence for crimes that reflects their own independent view of justice (Rosett and Cressey 1976).

An extensive empirical literature has investigated the validity of this hypothesized role of discretion, examining the impact on sentence lengths and the behavior of participants in the criminal justice system of statutory restrictions on discretion and increases in sentences. As Tonry (1988) observes, the results reported in this literature have been mixed, both because of differences in methods and because of unmodeled differences in the studied statutes. The vein of this literature most closely related to our analysis investigates the impact on sentencing patterns of the imposition of and changes in DSLs. Although many studies in this vein have found that DSLs have a causal effect on criminal justice system outcomes,³ several have found that discretion plays an important role.⁴ Disagreement in the literature extends to evaluations of the effects on justice-system outcomes of other types of statutory restrictions and to evaluations of the deterrent effects on crime of mandatory penalties. Some researchers have found that restrictions on plea-bargaining have a substantial impact on bargaining behavior,⁵ while others report the opposite.⁶ There are similar differences in findings regarding the deterrent effects of DSLs and other forms of mandatory punishments.⁷ However, in those studies that find evidence of discretion, it is hypothesized to play one of two roles: either as a device to enable participants in the system to implement changes in broad social norms that lead changes in formal law, or as a device to provide participants with varying degrees of leeway to mitigate sentences that they find to be unjust, according to the internal social norms of the criminal justice system.

II. SENTENCE ENHANCEMENTS, PROPOSITION 8, AND OUR EXPERIMENT

We test these two traditionally-hypothesized roles of discretion against a model of discretion based on prosecutorial maximization of sentences. Proposition 8 provides us with an ideal experiment for this purpose since it altered sentencing patterns in a way that is distinguishable from well-defined changes in broad social norms.

Prior to the passage of Proposition 8, California DSL provided for sentence enhancements that could increase defendants' prison sentences. For example, defendants who used a firearm in connection with a felony were eligible for a sentence enhancement, as were defendants with criminal histories and defendants who caused great bodily injury to their victims. Section 667.5 of the Penal Code specified the sentence enhancements for offenders with criminal histories. In litigated cases, history enhancements could only be imposed if 1) the defendant were eligible (e.g., the defendant had a qualifying criminal record); 2) the prosecutor charged the defendant with the enhancement; 3) the prosecutor proved that the defendant qualified for the enhancement to the jury; and 4) the judge chose to impose the enhancement (Tillman and McCoy 1987). "Violent" felony offenders (defined in Section 667.5(c)), none of which we analyze in this paper, received a three-year enhancement for each prior prison term served for a "violent" felony offense or a one-year enhancement for each prior prison term served for any offense, whichever was greater. "Nonviolent" felony offenders received an additional one-year sentence for each prior separate felony prison term.

Proposition 8

Proposition 8 was passed directly by California voters through the initiative process on June 8, 1982, and went into effect the next day. By adding Sections 667(a) and 1192.7(c) to the California Penal Code, Proposition 8 sought to increase the punishment given to repeat “serious” felony offenders.⁸ After the change in law, “serious” felony offenders under Section 1192.7(c) received a five-year enhancement for each prior conviction of a “serious” felony offense, or a one-year enhancement for each prior prison term served for any offense, whichever was greater. (Section 1192.7(c) “serious” felonies include all Section 667.5 “violent” felonies and some nonviolent felonies). In addition, Proposition 8 eliminated the statute of limitations in Section 667.5 that only considered a offender’s record for at most the past 10 years; prohibited judges from sentencing defendants to serve their enhancements concurrently with their base sentence, requiring that the enhancements be served consecutively; and eliminated the requirement that only considered prior felonies for which a prison term had been served in the calculation of enhancements.

Estimation Strategy

We analyze sentences of convicts resulting from both litigated and plea-bargained cases⁹ with a base charge of one and only one of three crimes committed between 1981 and 1985: robbery (denoted by superscript ‘r’),¹⁰ grand theft (denoted by superscript ‘t’),¹¹ and possession of controlled substances (CS) (denoted by superscript ‘c’).¹² We specifically examine convicts charged with one and only one of these three offenses to isolate the spillover effects of statutory changes from the direct effects as much as possible. For example, an increase in the sentence for

robbery could affect directly a convict charged with both robbery and theft to the extent that the robbery charge accurately reflected the convict's true offense severity; to the extent that the theft charge accurately reflected the convict's true offense severity, any spillovers due to an increase in the sentence for robbery would affect the same convict. We also exclude convicts charged with one of the studied offenses plus some other offense to avoid conflating the estimated effect of changes in the statutory sentence for a studied offense with the effects of changes in the statutory sentences for other offenses.

Robbery and grand theft are statutorily different but factually similar crimes; CS possession is statutorily and factually different from robbery and grand theft. Neither robbery, grand theft, nor CS possession experienced a statutory change in sentence length from 1981 to 1985,¹³ but defendants charged with robbery with serious felony criminal histories did experience a statutory change in sentence as a result of Proposition 8.

Thus, Proposition 8 provides an experiment that enables us to test whether internal social norms in the criminal justice system inhibit the implementation of laws. If this hypothesis were true, then there would be change in neither the absolute magnitude of sentences nor the relative magnitude of sentences received in response to Proposition 8 by individuals charged with the three crimes above.

Proposition 8 also provides an experiment that enables us to test whether both the direct and the spillover changes in sentences that occurred contemporaneously with Proposition 8 were due to broad shifts in social norms rather than the change in law. First, Proposition 8 enables us to test whether the change in law had a direct causal effect on those individuals formally subject

to the law, against several null hypotheses that any such changes in sentencing patterns were due to changing broad social norms. We specify the null hypotheses h_0^1 , h_0^2 , h_0^3 as follows:

h_0^1 : Changing social norms against crime generally caused the changes in sentences that occurred contemporaneous with Proposition 8. Thus, even if there were a change in absolute sentence lengths contemporaneous with Proposition 8, there would be no change in relative sentence lengths across different types of crimes.

h_0^2 : Changing social norms against violence caused the changes in sentences that occurred contemporaneous with Proposition 8. Thus, even if there were a change in sentence lengths for individuals charged with robbery relative to individuals charged with a nonviolent offense, there would be no change in sentence lengths for individuals without criminal histories charged with robbery relative to individuals with criminal histories charged with robbery.

h_0^3 : Changing social norms against repeat offenders caused the changes in sentences that occurred contemporaneous with Proposition 8. Thus, even if there were a change in sentence lengths for individuals with criminal histories relative to individuals without criminal histories, there would be no change in sentence lengths for individuals with criminal histories charged with robbery relative to individuals with criminal histories charged with nonviolent crimes.

However, to the extent that Proposition 8 represented a change in social norms specifically directed at violent criminals with criminal histories, we cannot distinguish the direct causal effects of Proposition 8 from the effects of traditionally-hypothesized social norms.

Second, Proposition 8 enables us to test whether the change in law had spillover causal effects on the sentences of individuals with criminal histories charged with grand theft, which also would be inconsistent with each of the three null hypotheses discussed above. However, to the extent that Proposition 8 represented a change in social norms directed at individuals with criminal histories charged with (violent) robbery or (nonviolent) grand theft but not at individuals charged with CS, we cannot distinguish the spillover causal effects of Proposition 8 from the effects of traditionally-hypothesized social norms.

We test these hypotheses using two alternative sets of treatment and control groups. First, under the assumption that unobserved differences in offender and crime characteristics over time between history and no-history defendants with the same base charge are constant, no-history defendants provide a control group for estimating the effect of Proposition 8 on history defendants charged with robbery, theft, and CS. Second, under the assumption that unobserved differences in offender and crime characteristics over time between history defendants with a base charge of robbery or theft and history defendants with a base charge of CS are constant, history defendants charged with CS provide a control group for estimating the effect of Proposition 8 on history defendants charged with robbery or theft.

Table 1 presents our parameterization of $\ln(\text{sentence length})$ for defendants with and without criminal histories, before and after Proposition 8, by base charge. The coefficients in Table 1 come from the following regression of $\ln(\text{sentence length})$ on a set of time fixed effects, defendant demographic characteristics, and binary variables denoting base charge, criminal history, and post-Proposition 8 date of crime:

$$(1) \quad \ln(\text{sentence}) = \gamma * \text{time} + \delta * \text{demographics} + \beta_1 * \text{base charge} + \beta_2 * \text{base charge} * \text{history} + \beta_3 * \text{base charge} * \text{post-Prop. 8} + \beta_4 * \text{base charge} * \text{history} * \text{post-Prop. 8} + \epsilon,$$

where $\beta_i = [\beta_i^r \mid \beta_i^t \mid \beta_i^c]$, and $\beta_1^c = \beta_3^c = 0$ to identify γ . We parameterize the effect of Proposition 8 on $\ln(\text{sentence})$ rather than the level of sentence, although we discuss results from models of the level of sentence.

The estimator that uses the first control group is based on differences within base-charge groups over time between history and no-history defendants. According to the prosecutorial maximization model of discretion in determinate sentencing, increasing the statutory sentence for history defendants charged with robbery (as Proposition 8 did) will result in $\beta_4^r, \beta_4^t > 0$ and $\beta_4^c = 0$. Put another way, pre-post Proposition 8, both defendants charged with robbery and with grand theft will experience increasing history enhancements; however, defendants charged with CS possession will experience no change in their history enhancement pre-post Proposition 8, because Proposition 8 did not affect CS defendants statutorily, and CS possession is not a “similar” crime to either robbery or grand theft. In terms of regression model (1), the null hypothesis that internal social norms in the criminal justice system inhibit the implementation of laws implies $\beta_3 = \beta_4 = 0$. Regarding the tests of the effects of external social norms, h_0^1 : implies $\beta_3 = \beta_4 = 0$; h_0^2 : implies $\beta_4 = 0$; and h_0^3 : implies $\beta_4^r = \beta_4^t = \beta_4^c$.

The estimator that uses the second control group is based on differences among history defendants between base-charge groups. According to the prosecutorial maximization model of discretion in determinate sentencing, increasing the statutory sentence for history defendants charged with robbery (as Proposition 8 did) will result in $(\beta_3^r + \beta_4^r) - (\beta_3^c + \beta_4^c), (\beta_3^t + \beta_4^t) - (\beta_3^c + \beta_4^c) > 0$. In other words, pre-post Proposition 8, history defendants charged both with robbery and with grand theft will experience increasing sentences relative to CS defendants. Proposition 8 did not affect CS defendants statutorily, and CS possession is not a “similar” crime to either robbery or grand theft. We investigate this implication of our alternative hypothesis with estimates based only on defendants with criminal histories; defendants without criminal histories are omitted from the model. These estimates are from a regression that specifies $\ln(\text{sentence}$

length) as a function of time fixed effects, demographic characteristics, and binary variables denoting base charge and post-Proposition 8 date of crime:

$$(2) \ln(\text{sentence}) = \gamma * \text{time} + \delta * \text{demographics} + \beta_2 * \text{base charge} + \beta_4 * \text{base charge} * \text{post-Prop. 8} + \epsilon,$$

where $\beta_i = [\beta_i^r \mid \beta_i^t \mid \beta_i^c]$, $\beta_1 = \beta_3 = 0$ to identify β_2 and β_4 , and $\beta_1^c = \beta_2^c = 0$ to identify γ . In terms of regression model (2), the null hypothesis that internal social norms in the criminal justice system inhibit the implementation of laws implies $\beta_4 = 0$. Regarding the tests of the effect of external social norms, h_0^1 : implies $\beta_4 = 0$; h_0^2 : is not testable, because defendants without criminal histories are omitted from the model; and h_0^3 : implies $\beta_4^r = \beta_4^t = \beta_4^c$.

III. DATA AND RESULTS

We analyze data collected by the California Board of Prison Terms (BOPT) on new commitments to California state prisons based on crimes committed between 1981 and 1985. The attraction of the BOPT data for our purposes is the richness of information collected. In particular, the BOPT data includes information on the defendant, the crime, and the charged offenses as well as the convicted offenses and the sentence given. Information about charged offenses and sentence length is both rarely collected and essential to our approach, since we measure spillovers by changes in sentences conditional on charged offense.

We analyze new commitments rather than admissions, since many admissions result from revocations of probation or parole, not new sentences. We also limit our analysis to offenses committed 1981-1985 due to concerns about the introduction of crack cocaine, which led to

many changes in the crime and sentencing environment, although our results are not sensitive to this choice.

Although the number of cases coded by the BOPT is relatively constant over the 1980's, the proportion of cases coded falls off steadily. In order to weight the data to accurately reflect the increasing scale of California's prison system, we use information on the number of new prison admissions according to Offender-Based Transaction Statistics (OBTS), which are reported by California courts to the U.S. Department of Justice. While the OBTS figures are known to underestimate the true numbers because of under-reporting (Bureau of Criminal Statistics 1984, p.94), they are an improvement over the BOPT data for tracking admissions over time. We use the ratio of the OBTS number to the BOPT number as a weight in order to compensate for the fall off in the proportion of cases coded by the BOPT over time, under the assumption that the BOPT did not systematically select cases to code.¹⁴

Descriptive Statistics

Table 2 reports descriptive statistics for the groups of offenders that we analyze, and previews the results of our regression analysis. Table 2 separates offenders into 12 categories: by base charge, by criminal history, and by before/after Proposition 8. Defendants who committed offenses after June 8, 1982, are classified as post-Proposition 8. Ideally, we would separate defendants into those who had no criminal histories, those who had criminal histories that qualified for a Section 667.5 enhancement, and those who had criminal histories that qualified for a Section 1192.7(c) enhancement under Proposition 8. However, the most detailed criminal history information in the data records only the number of prior convictions for a “same or similar” offense. Thus, we are not able to identify whether a defendant has a criminal history that

would qualify for enhancement under Proposition 8. In particular, we are not able to identify whether defendants charged with robbery and grand theft who had been convicted of a “same or similar” offense had been convicted a “Proposition 8-qualifying” offense (such as robbery) or a Proposition 8-nonqualifying offense (such as grand theft). For purposes of our analysis, we define a defendant as having a criminal history conservatively as those who both had prior convictions for a same or similar offense and had served a prison term for prior offenses. First, counting only those who had served prison terms limits the criminal history category to defendants more likely to have been convicted of a (more serious) Proposition 8-qualifying offense such as robbery. Second, counting only those who had served prison terms limits our analysis of the impact of Proposition 8 to its provisions regarding sentence length. Proposition 8 statutorily expanded the set of defendants who qualified for a history enhancement to include certain defendants who had not served prison terms as well as statutorily increasing the sentence enhancement for defendants convicted of robbery with “serious” felony criminal histories.

The six “no-history” columns in the first row of the table suggest that individuals sentenced to prison who do not have criminal histories, on average, receive the “middle term” statutory punishment associated with their base charge. Both before and after Proposition 8, defendants with a base charge of robbery received an average sentence of almost exactly 48 months. Defendants with a base charge of theft received an average sentence of 24.9 months before and after Proposition 8, and defendants with a base charge of CS possession received an average sentence of either 21.7 or 22.5 months.

Also in line with statutory sentences, individuals charged with a given crime with criminal histories received greater sentences than individuals without them. Before the passage

of Proposition 8, robbery defendants with histories received an average sentence of 74.3 months (robbery defendants without histories received an average sentence of 48 months); theft defendants with histories received an average sentence of 26.6 months (theft defendants without criminal histories received an average sentence of 24.9 months); and CS defendants with histories received an average sentence of 26.6 months (CS defendants without histories received an average sentence of 22.5 months).

Raw pre-post differences-in-differences between history and no-history defendants in sample average sentences suggest that the prosecutorial maximization model of discretion explains the justice system's reaction to Proposition 8. Proposition 8 increased history enhancements for individuals charged with robbery and with grand theft, but not for individuals charged with CS; in fact, over the 1981-1985 period, history enhancements for CS defendants actually declined. Robbery defendants' average enhancement increased from 26.4 months to 37.6 months, and theft defendants' average enhancement increased from 2.6 months to 3 months, whereas CS defendants average enhancement declined from 4.1 months to 2.2 months. In other words, participants in the criminal justice system do not have sufficient discretion to completely undo changes in laws: statutory increases in sentences result in increased punishments. Furthermore, to the extent that participants do have discretion, increases in statutory sentences result in increases in sentences for offenders with "similar" crimes not affected statutorily by the change in law.

Table 2 also presents means of other variables used in our analysis, principally offender demographic characteristics. Although defendants with criminal histories are on average older, their average age was relatively constant within offense and history category before and after

Proposition 8. The share of defendants who received their sentence as a result of litigation rather than plea-bargain declined over the 1981-1985 period. However, the litigation rate declined most for defendants charged with CS, and declined least for defendants charged with robbery. Except for defendants charged with CS possession pre-Proposition 8, defendants with an adult criminal history were more likely to have a juvenile criminal history also, replicating the common result that recidivists begin to accrue a criminal history as a juvenile. The distribution of educational attainment is also roughly similar across the groups, with more than half of defendants of all types (except history defendants charged with theft pre-Proposition 8) reporting less than a high-school education.

Regression Results

Table 3 presents regression estimates of the effect of Proposition 8 on the history enhancement received by convicted defendants, by base charge. The first two columns of Table 3 report estimates from regression equation (1); the second two columns report estimates from regression equation (1) with the pre-post change in sentence for no-history defendants constrained to be equal across groups (since there was no statutory change in the relative pre-post growth rate in sentence across no-history groups), e.g., $\beta_3^r = \beta_3^t = 0$. The estimates in Table 3 verify what the raw differences-in-differences showed. Holding defendant demographic characteristics constant, Proposition 8 increased the history enhancement received by defendants with a base charge of robbery (by approximately 14 percent) and the history enhancement received by defendants with a base charge of grand theft (by 6 - 10 percent). Put another way, Proposition 8 increased the sentence received by history defendants more than it increased the

sentence of no-history defendants. Both for defendants charged with robbery and for defendants charged with theft, the difference-in-difference is statistically significantly different from zero in both the constrained and the unconstrained model. Proposition 8 increased the statutory sentence for robbery defendants with criminal histories, judges and juries implemented this legislative change, and plea-bargains reflected this decision to implement. In addition, because robbery and grand theft are similar crimes, spillover effects consistent with a model of prosecutorial maximization led to increased sentences for history defendants charged with grand theft. The T-statistics on the coefficients in the first and second rows of Table 3 reject the hypotheses that Proposition 8 was completely undone by actors in the criminal justice system, and reject the null hypotheses h_0^1 and h_0^2 that changing social norms against crime or against violence caused the changes in sentence lengths observed contemporaneous with Proposition 8.

Table 3 also shows that the history enhancement received by defendants charged with CS possession did not change significantly pre-post Proposition 8, as a model that depended on changing social norms against repeat offenders would suggest. The effect of Proposition 8 on history enhancement does not reflect an increase in history enhancements for all crimes, just for those crimes specified in the statute and for similar crimes. Table 4 tests this hypotheses explicitly with estimates from regression equation (2). Table 4 presents estimates of the effect of Proposition 8 on the difference between the sentence received by history defendants with a base charge of robbery or theft and the sentence received by history defendants with a base charge of CS. The results from Table 4 validate the results from Table 3. Instead of using no-history defendants as a control group, equation (2) uses history defendants charged with CS -- equation (2) is estimated on history defendants only. The first two rows of Table 4 indicate that, among

defendants with criminal histories, both defendants charged with robbery and defendants charged with theft experienced an increase in sentence relative to defendants charged with CS, whether or not defendant demographic characteristics are held constant. The third row of Table 4 shows that history defendants charged with robbery had approximately 2.5 times ($= \exp(0.917)$) higher sentences, on average, than did history defendants charged with CS before the passage of Proposition 8 -- this is not surprising in light of the fact that the middle term DSL sentence for robbery was twice as great as the middle term sentence for CS. The last row of Table 4 shows the sentence for history defendants charged with theft was not statistically different from the sentence for history defendants charged with CS before the passage of Proposition 8. This also supports the view that the effect of Proposition 8 was to increase the sentence for the crime(s) specified in the legislation and for similar crimes. Before the passage of Proposition 8, the sentence length of history defendants charged with theft and CS were not distinguishable statistically, but after Proposition 8, the sentence length of history defendants charged with theft rose statistically significantly compared to history defendants charged with CS.

Robustness

Because both prosecutors' charging behavior and prison admission may affect selection into our sample(s) and be correlated with law-induced changes in sentence length, we investigated whether our results were due to two types of sample selection. First, if the increased punishments conditional on prison admission that we observe for history defendants charged with robbery were accompanied by a decreased probability of receiving a prison term, then expected punishment may not have increased as much as we report that it has, weakening our principal conclusion. Second, if the increase punishments that we observe for history defendants

charged with robbery are due to prosecutors' reclassification of the least-serious of these offenders as history defendants charged with theft, then our finding of spillovers may be partially due to changes in the relationship between offense severity and base charge.

However, there is no evidence that either selection effect is biasing our results. First, expressed as a share of all felony robbery arrests, the imprisonment rate increased dramatically for robbery defendants in our sample, but the imprisonment rate for theft defendants remained virtually unchanged; this is inconsistent with the sample-selection hypothesis under which increases in statutory sentences lead to decreases in conviction probability for the convicts in our sample.¹⁵ Second, according to the last row of Table 2, the number of history defendants charged with robbery who were admitted to prison annually increased post-Proposition 8; the number of history defendants charged with theft who were admitted to prison annually decreased post-Proposition 8 (relative to the number of no-history defendants who were admitted to prison annually, the number of history defendants charged with robbery decreased less than the number of history defendants charged with theft). In addition, as measured by the share of convicts with juvenile histories, there is no evidence of reclassification of the least-serious of history defendants charged with robbery as history defendants charged with theft.

We estimated a variety of other models to explore the robustness of our principal results and to investigate further the hypothesized effects of increases in legislative sentence lengths proposed in previous theoretical research. First, estimates from models that disaggregate the simple before/after effect of Proposition 8 into year-by-year effects indicate that the relative sentences for history defendants charged with robbery and history defendants charged with grand theft rose in the middle of 1982 and remained constant thereafter, suggesting that the enactment

of Proposition 8 was indeed driving the change in sentences. Second, estimates from models of the effect of Proposition 8 on the level rather than the log of sentence length also show that Proposition 8 increased the sentence for history defendants charged with robbery and grand theft, but not for history defendants charged with CS. In all of the four specifications of regression equation (1), defendants with a base charge of robbery showed a statistically significant (at the 1 percent level) increase in the level of history enhancement as a result of Proposition 8 of between 10.7 and 11.6 months (depending on the specification). In three of the four specifications of equation (1), defendants with a base charge of grand theft showed a statistically significant (at the 10 percent level) increase in the level of history enhancement as a result of Proposition 8. For example, holding demographic characteristics constant, estimates from the constrained model (imposing $\beta_3^t = \beta_3^t = 0$) show that defendants charged with grand theft experienced a one to two-month increase in their history enhancement (depending on the specification). Defendants with a base charge of CS showed smaller and statistically insignificant changes in the level of sentence enhancement before and after Proposition 8.

We also estimated regression equations (1) and (2) using only plea-bargained cases to investigate further the possibility that statutory increases in sentence lengths were undone for settled but not for litigated cases. If juries imposed the higher statutory sentences on convicted defendants but convicted a decreasing share of defendants, the effect of an increase in statutory sentence on the average sentence length of convicted defendants in litigated and plea-bargained cases pooled together might overstate actual changes in punishment. On the other hand, the change in response to an increase in statutory sentence of the average sentence length of convicted defendants in plea-bargained cases only might understate actual changes in

punishment, if the higher statutory sentence led to a shift in the composition of litigated cases towards relatively higher-sentence cases (Since Table 2 showed that the decline in litigation over time was greatest for the lowest-sentence (no-history CS) defendants and least for the highest-sentence defendants (history robbery) defendants, this could be an important concern).

Unfortunately, disentangling the direct impact on sentence length of changes in statutory sentences from the indirect impact through statutory-sentence-induced changes in litigation behavior requires an instrument that affects litigation decisions but not sentence length, which our data do not provide.

Estimates of equations (1) and (2) based on plea-bargained cases reinforce the findings from Tables 3 and 4: increases in the statutory sentence for a given crime increase punishments both for the given crime and for similar crimes. Estimates from regression equation (1) based on plea-bargained cases only ($N = 9,502$) show that Proposition 8 statistically significantly increased the history enhancement for defendants charged with robbery in all specifications (e.g., holding demographic characteristics constant, estimated effect from unconstrained model of .117, standard error 0.070), although the effect for defendants charged with grand theft was insignificant. In addition, estimates from regression equation (2) based on plea-bargained cases only ($N = 2,169$) show that Proposition 8 statistically significantly increased the difference in sentence both between history defendants charged with robbery and history defendants charged with CS (e.g., holding demographic characteristics constant, estimated effect of .156, standard error 0.054) and between history defendants charged with grand theft and history defendants charged with CS (e.g., holding demographic characteristics constant, estimated effect of .110, standard error .043).

IV. CONCLUSION

Although a substantial body of research suggests that the discretion of actors in the criminal justice system is important, there is disagreement in the existing empirical literature over the specific role of discretion and the extent to which discretion influences criminal justice outcomes. Studies in this literature generally hypothesize that discretion plays one of two roles: either it serves as the means by which changing broad social norms against crime cause changes in sentencing patterns (with concurrent changes in formal laws reflecting broad social norms but not causing criminal justice outcomes), or it serves as the means by which internal social norms of the criminal justice system prevent the implementation of formal changes in laws.

In this paper, we test the hypotheses that discretion plays one or both of these roles against the hypothesis that discretion takes a less-well-studied form that we describe as “prosecutorial maximization.” We suggest that an increase in the statutory sentence for a given crime can increase sentence length for those who are charged with the crime, and also for those who are charged with factually “similar” crimes, where a “similar” crime is defined as one that has legal elements in common with the given crime. In this case, discretion is the means by which prosecutors seek to maximize punishment, subject to the constraints of laws. Instead of causing an apparently statutory change in sentencing policy or inhibiting change in sentencing, we hypothesize that discretion can have spillover effects that intensify the effects of changes in laws.

We investigate these hypotheses by estimating empirically the effects of California’s Proposition 8, a popular referendum, which increased the statutory sentence for defendants with criminal histories who committed one or more certain “qualifying” crimes. We reject both of the

generally-hypothesized roles of discretion in favor of prosecutorial maximization. We reject the null hypothesis that actors in the criminal justice system seek to undo changes in laws, because we find that defendants with criminal histories charged with a qualifying crime experienced statistically significant increases in jail sentences as a result of Proposition 8, as did defendants with criminal histories who were charged with a nonqualifying crime that was factually similar to a qualifying crime.

We also reject the hypothesis that changing broad social norms against crime caused the changes in sentencing patterns that were apparently caused by Proposition 8. Indeed, because Proposition 8 was a popular referendum, this finding is especially striking. Specifically, we reject the hypotheses 1) that changing social norms against crime generally caused the changes in sentences that occurred contemporaneous with Proposition 8; 2) that changing social norms against violence caused the changes in sentences that occurred contemporaneous with Proposition 8; and 3) that changing social norms against repeat offenders caused the changes in sentences that occurred contemporaneous with Proposition 8. We reject these three hypotheses because we find statistically significant changes in relative sentence lengths between convicts who would have been affected similarly by each of these changes in social norms, but who were affected differently by the formal change in law imposed by Proposition 8. Furthermore, these results are robust to choice of specification, to the definition of the sample, and to two forms of possible selection bias.

Our empirical results have several implications. Broadly consistent with the previous literature, we find evidence of discretion in the criminal justice system, although the form and extent of this discretion differs from the findings of the previous empirical literature.

Participants in the criminal justice system do not have sufficient discretion to completely undo changes in laws. Statutory increases in sentences result in increased punishments. Furthermore, we do not find evidence that broad social norms, rather than changes in formal law, are the true cause of changes in sentencing patterns. Instead, we find results consistent with discretion and a model of prosecutorial maximization: increases in statutory sentences result in more punishment, not less punishment, than the simple statement of the laws would suggest.

However, we cannot reject absolutely that discretion serves as the means through which social norms influence criminal justice outcomes. To the extent that Proposition 8 represented a change in social norms directed specifically at those individuals affected by the formal change in law, we cannot distinguish the direct causal effects of Proposition 8 from the effects of traditionally- hypothesized social norms; we can only reject that broad shifts in norms caused the change in sentencing patterns associated with Proposition 8. Future research might investigate further the role of norms in the sentencing process. In addition, future research might investigate further the validity of the prosecutorial maximization hypothesis, either by examining whether other changes in laws create spillover effects, or by testing the validity of other implications of such a model.

Table 1: Parameterization of $\ln(\text{Sentence Length})$ for Defendants
With and Without Criminal Histories, Before and After Proposition 8, by Base Charge

	Base charge of robbery	Base charge of grand theft	Base charge of CS possession
with history, pre Proposition 8	$\gamma_1 + \beta_1^r + \beta_2^r$	$\gamma_1 + \beta_1^t + \beta_2^t$	$\gamma_1 + \beta_1^c + \beta_2^c$
with history, post Proposition 8	$\gamma_2 + \beta_1^r + \beta_2^r + \beta_3^r + \beta_4^r$	$\gamma_2 + \beta_1^t + \beta_2^t + \beta_3^t + \beta_4^t$	$\gamma_2 + \beta_1^c + \beta_2^c + \beta_3^c + \beta_4^c$
Difference, w/history, post-pre Prop. 8	$\beta_3^r + \beta_4^r + \gamma_2 - \gamma_1$	$\beta_3^t + \beta_4^t + \gamma_2 - \gamma_1$	$\beta_3^c + \beta_4^c + \gamma_2 - \gamma_1$
without history, pre Proposition 8	$\gamma_1 + \beta_1^r$	$\gamma_1 + \beta_1^t$	$\gamma_1 + \beta_1^c$
without history, post Proposition 8	$\gamma_2 + \beta_1^r + \beta_3^r$	$\gamma_2 + \beta_1^t + \beta_3^t$	$\gamma_2 + \beta_1^c + \beta_3^c$
Difference, w/o history, post-pre Prop. 8	$\beta_3^r + \gamma_2 - \gamma_1$	$\beta_3^t + \gamma_2 - \gamma_1$	$\beta_3^c + \gamma_2 - \gamma_1$
Difference between w/history and w/o history differences	β_4^r	β_4^t	β_4^c

Table 2: Means and (Standard Deviations) of Variables Used in Analysis, Before and After Proposition 8, by Base Charge and Criminal History

	BASE CHARGE OF ROBBERY		BASE CHARGE OF THEFT		BASE CHARGE OF DRUG POSSESSION							
	Pre Prop 8 no history	Post Prop 8 history	Pre Prop 8 no history	Post Prop 8 history	Pre Prop 8 no history	Post Prop 8 history						
Sentence in months	48.017 (28.569)	74.357 (56.122)	47.850 (32.392)	85.450 (64.941)	24.934 (9.900)	26.557 (12.371)	24.955 (10.110)	28.030 (13.470)	22.460 (6.943)	26.625 (9.594)	21.738 (6.702)	23.911 (8.179)
Age in years	24.347 (5.618)	30.303 (7.222)	24.374 (6.053)	30.948 (7.104)	27.862 (8.037)	30.446 (8.381)	27.680 (8.060)	32.204 (8.80)	27.683 (6.70)	35.843 (8.412)	26.850 (6.543)	34.244 (8.440)
Litigated	.134	.164	.116	.163	.082	.063	.056	.055	.112	.140	.019	.054
Juv. history	.446	.585	.371	.479	.341	.480	.282	.366	.329	.281	.255	.352
White	.276	.310	.224	.273	.469	.427	.460	.441	.236	.359	.204	.229
Black	.435	.475	.474	.502	.310	.465	.300	.391	.304	.344	.330	.295
Hispanic	.263	.200	.260	.207	.191	.092	.195	.147	.453	.265	.424	.457
Other race	.026	.014	.041	.017	.030	.016	.047	.020	.006	.031	.040	.019
College grad	.001	.007	.004	.014	.032	.019	.033	.007	.012	.031	.004	0
Some college	.129	.121	.124	.165	.182	.190	.182	.183	.087	.203	.109	.111
HS grad	.263	.361	.272	.316	.246	.332	.251	.305	.217	.25	.254	.260
Some hs	.385	.257	.335	.221	.300	.244	.262	.189	.341	.140	.347	.260
Primary sch	.220	.253	.263	.282	.240	.215	.272	.314	.341	.375	.284	.368
Educ missing	.112	.146	.197	.228	.170	.164	.209	.267	.211	.156	.214	.270
N	1012	280	4139	803	471	316	1200	544	161	64	1215	315
N per year	703	194	1163	226	290	219	337	153	112	44	341	89

Table 3: Estimated Change in and Level of History Enhancements,
 Before and After Proposition 8, by Base Charge
 (Heteroscedasticity-Consistent Standard Errors in Parentheses)

	-----Specification-----			
Pre-post change in history enhancement, base charge robbery [β_4^r]	0.139** (0.048)	0.134** (0.047)	0.145** (0.046)	0.143** (0.044)
Pre-post change in history enhancement, base charge theft [β_4^t]	0.076** (0.035)	0.063* (0.034)	0.096** (0.032)	0.083** (0.031)
Pre-post change in history enhancement, base charge CS [β_4^c]	-0.057 (0.049)	-0.045 (0.051)	-0.071 (0.047)	-0.062 (0.050)
Pre Proposition 8 history enhancement, robbery [β_2^r]	0.377** (0.041)	0.326** (0.040)	0.372** (0.039)	0.319** (0.038)
Pre Proposition 8 history enhancement, theft [β_2^t]	0.028 (0.029)	0.036 (0.024)	0.013 (0.026)	-0.005 (0.026)
Pre Proposition 8 history enhancement, CS [β_2^c]	0.148** (0.046)	0.082* (0.049)	0.162** (0.044)	0.098** (0.048)
Controls for defendant demographic characteristics	no	yes	no	yes
Pre-post change in sentence for no-history defendants constrained to be equal across charges [$\beta_3^r = \beta_3^t = 0$]	no	no	yes	yes
Adjusted R2	0.390	0.405	0.390	0.405

Notes: Results from regressions across convicts of
 $\ln(\text{sentence}) = \gamma \cdot \text{time} + \delta \cdot \text{demographics} + \beta_1 \cdot \text{base charge} + \beta_2 \cdot \text{base charge} \cdot \text{history} + \beta_3 \cdot \text{base charge} \cdot \text{post-Prop. 8} + \beta_4 \cdot \text{base charge} \cdot \text{history} \cdot \text{post-Prop. 8} + \epsilon$
 Number of observations 10,520.

Table 4: Estimated Difference in Difference in Sentences between History Defendants Charged With Robbery or Grand Theft and Defendants Charged with CS, Before and After Proposition 8 (Heteroscedasticity-Consistent Standard Errors in Parentheses)

	-----Specification-----	
Pre-post change in difference in sentence for history defendants, robbery - CS [[$\beta_3^r + \beta_4^r$] - ($\beta_3^c + \beta_4^c$)]	0.182** (0.055)	0.187** (0.053)
Pre-post change in difference in sentence for history defendants, theft - CS [[$\beta_3^t + \beta_4^t$] - ($\beta_3^c + \beta_4^c$)]	0.134** (0.044)	0.116** (0.043)
Pre-Prop. 8 difference in sentence for history defendants, robbery - CS [[$\beta_1^r + \beta_2^r$] - ($\beta_1^c + \beta_2^c$)]	0.917** (0.051)	0.950** (0.050)
Pre-Prop. 8 difference in sentence for history defendants, theft - CS [[$\beta_1^t + \beta_2^t$] - ($\beta_1^c + \beta_2^c$)]	-0.008 (0.040)	0.023 (0.040)
Controls for defendant demographic characteristics	no	yes
Adjusted R2	0.489	0.507

Notes: Results from regressions across convicts with criminal histories of $\ln(\text{sentence}) = \gamma \cdot \text{time} + \delta \cdot \text{demographics} + \beta_2 \cdot \text{base charge} + \beta_4 \cdot \text{base charge} \cdot \text{post-Prop. 8} + \epsilon$
Number of observations 2,322.

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Endnotes

1. We follow Tonry (1988) and treat determinate sentencing laws, like California's DSL, and mandatory sentencing guidelines similarly in the following discussion.
2. Nationwide, approximately 90 percent of all convictions are through a guilty plea (Wishingrad 1991).
3. See, e.g., Lipson and Peterson 1980, Casper, Brereton, and Neal 1982, and Cohen and Tonry 1983, analyzing the imposition of the California DSL; Clarke 1984, analyzing the imposition of the North Carolina DSL; Miethe 1987 and Frase 1991, 1993 analyzing the creation of the Minnesota sentencing commission and DSL; United States Sentencing Commission 1991, analyzing the imposition of the Federal sentencing guidelines; and Boerner 1993, analyzing the imposition of and changes in the Washington DSL.
4. See, e.g., Cohen and Tonry 1983 for a detailed discussion of research on the California DSL; Loftin, Heumann, and McDowall 1983, analyzing mandatory punishments for firearm use; McCoy and Tillman 1986 and Tillman and McCoy 1987, analyzing the effects of Proposition 8; Snyder 1990, analyzing changes in penalties for Federal antitrust violations; and Nagel and Schulhofer 1992, analyzing the Federal sentencing guidelines. There are several possible explanations for the null effect of Proposition 8 found by McCoy and Tillman, which is in contrast to the findings we present in this paper. Most importantly, they did not use information on the criminal histories for their sample of convicted felons, which is essential to analyzing the effect of the law (since the law increased only the penalty associated with past convictions). Second, they report results controlling for conviction offense. We argue that conviction offense is endogenous; indeed, in a determinate sentencing system, choice of conviction offense is likely

to be the mechanism through which discretion operates. Third, they use data from only three counties, whereas we analyze information from the entire state.

5. See, e.g., Cohen and Tonry 1983, analyzing the plea-bargaining ban in Alaska.

6. See, e.g., Church 1976 and Loftin, Heumann, and McDowall 1983, analyzing bans on bargaining over charges; McCoy and Tillman 1986 and Tillman and McCoy 1987, analyzing the plea-bargaining restrictions associated with Proposition 8.

7. McDowall, Loftin, and Wiersema 1992 find evidence that statutory penalties for firearm use reduce some forms of violent crimes, but see Tonry 1996 and research cited there for the opposite view.

8. In addition, Proposition 8 included several broad pronouncements amending the State constitution.

9. Ideally, we would analyze separately defendants with litigated and plea-bargained cases. However, since the decision to litigate is endogenous, identifying separate models of sentence length for litigated and settled cases would require an instrument that affects the decision to litigate but not sentence length. Since our data do not provide us with such an instrument, we analyze settled and litigated cases together and discuss results based on settled cases only as a validity check.

10. Includes California Penal Code Section 211 and Section 213.5.

11. Includes California Penal Code Section 487 and Vehicle Code Section 10851 (auto theft).

12. Includes California Health & Safety Code Sections 11350, 11357, and 11377.

13. The middle-term sentence for robbery is 48 months, with a lower term of 36 months and an upper term of 60 months. The middle-term sentence for grand theft and CS possession is 24 months, with a lower term of 16 months and an upper term of 36 months.

14. Phone interviews with current and past employees of the BOPT support this view.
15. Based on the total number of felony arrests reported in Bureau of Criminal Statistics (1981, 1983, 1984). We approximated the pre-Proposition 8 number of annual arrests with 1981 arrests and the post-Proposition 8 number of annual arrests with the 1983-84 average number of arrests because historical counts of arrests by crime are not available for intervening years.