Responses to more severe punishment in the courtroom: Evidence from Truth-in-Sentencing laws

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April 2011

Abstract

We investigate the behavioral responses of judges and prosecutors to more severe punishments by analyzing the effects of Truth-in-Sentencing (TIS) laws in a large sample of criminal cases. The TIS laws raised the severity of punishment by requiring offenders to serve at least 85 percent of their imposed sentences in prison. Differences between the U.S. states in the timing of adoption and the types of crimes covered provide a source of identification. The key findings are: (1) The TIS laws reduced the probability that an arrested offender is eventually convicted by 9 percent through an increase in the probability that the case is dismissed, a reduction in the probability that the defendant pleads guilty, and a reduction in the probability that the defendant is convicted at trial. (2) The TIS laws reduced the imposed sentence that a defendant can expect upon arrest by 8 percent. (3) These effects were more pronounced for crimes that were not the primary target of the TIS law, i.e., non-violent crimes.

Keywords: criminal procedure, criminal law, sentencing, Truth-in-Sentencing laws
JEL Codes: K00, K41, K14

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1 Introduction

Laws that impose more severe punishments on criminals sometime bring unexpected consequences. Their direct objective – to deter and incapacitate offenders by keeping them longer in prison – may be mitigated by behavioral responses of judges, jurors, and prosecutors who exercise a certain amount of discretion at various stages of the criminal procedure. Judges and jurors may become more reluctant to convict, judges may impose a shorter sentence, and prosecutors may adjust their plea bargaining tactics. Understanding the character and empirical magnitude of the behavioral responses has important policy implications. Since the legislators cannot fully control the choices of judges, jurors, and prosecutors, they should take the mitigating responses into account when designing sentencing policies. Legislating longer sentences may be undesirable both on the grounds of efficiency as well as fairness if the mitigating responses are large enough.

This paper presents evidence on mitigating responses by evaluating the effects of the so-called Truth-in-Sentencing (TIS) laws on the outcomes of criminal cases. The TIS laws, adopted by many U.S. states during the 1990's, required that convicted offenders must serve at least 85 percent of the imposed prison sentence. This implied a stark increase in the fraction of the sentence served compared to the 1980's and early 1990's when prisoners served 48 percent on average (Ditton and Wilson 1999), mostly due to discretionary early releases by parole officers and prison overcrowding. If the probability of conviction and the imposed sentences did not change after introducing the TIS laws, an offender could spend 70 percent more time in prison than previously expected.

Several states and the federal government imposed TIS-type requirements prior to the 1990's (U.S. Department of Justice 1993). The Federal Violent Crime Control and Law Enforcement Act of 1994[1] encouraged more states to adopt such provisions by introducing the so-called Violent Offenders Incarceration and Truth-in-Sentencing Incentive Grant Program. To be eligible for the TIS grant, a state had to implement a TIS law that required offenders convicted of a Part I violent crime[2] to serve no less than 85 percent of the sentence imposed, or a similar law that effectively resulted in such offenders serving on average at least 85 percent of the sentence.[3]

The timing of adoption of the TIS laws by individual states varied (see Table 1). While only two states (plus the District of Columbia) had TIS-type provisions in the early 1990's, eleven other states adopted the TIS laws within one year of the passage of the Violent Crime Control and Law Enforcement Act of 1994. Twenty seven states and the District of Columbia met the eligibility criteria by 1998.[4] The states also varied in the scope of coverage of the TIS laws; the 85 percent requirement applied to Part I violent felonies in all adopting states, but in some states it applied to other crimes as well.

[1] Public Law 103-322, Sept. 13, 1994 (the "1994 Crime Act").

[2] Part I violent crime includes murder, rape, robbery, and assault.

[3] For more detail of criteria, see U.S. Department of Justice (2005).

[4] These states received \$2.7 billion in total during 1996-2001 through the VOI/TIS grant program (U.S. Department of Justice 2005).

The variation among the states in the timing of adoption and the types of crime covered allows identifying the effects of the TIS laws on case outcomes by a difference-in-differences-in-differences estimator. The data set - State Court Processing Statistics (SCPS) - consists of a large sample of felony cases from the most populous counties of the United States and allows controlling for many observable characteristics of each case.

The paper contributes to the empirical literature on behavioral responses in criminal procedure in several ways. First, it captures the various margins of responses in the criminal justice process in two simple summary measures. One measure is the change in the probability that an arrested offender is eventually convicted, irrespective of whether at trial or by pleading guilty. Indeed we find that it fell by 9 percent. The other measure is the change in the imposed sentence that an arrested offender receives at the final disposition of the case, which is either the actual sentence imposed on a convicted defendant or zero sentence imposed on an offender that is not convicted. It gives a particularly useful summary of the behavioral responses as the changes in the probability of dismissal, guilty plea, conviction at trial, and the sentence imposed upon conviction are reflected into the sentence that is ultimately imposed. It can also be interpreted as a change in the sentence that an offender can expect conditional on arrest. The TIS laws reduced the imposed sentence conditional on arrest by 8 percent according to our most preferred specifications.

The behavioral responses mitigated the intended effect of the TIS laws to impose more severe punishment. In the absence of behavioral responses, the sentence actually served, conditional on arrest, would have increased by 70 percent on average. As the sentence imposed, conditional on arrest, fell by 8 percent, the sentence that an arrested offender can expect to actually serve increased not by 70 percent but by "only" 56 percent.[5] Therefore, the unintended behavioral responses removed about one-fifth of the intended increase in the severity of punishment. The mitigating responses are empirically relevant to be taken into account in the design of sentencing policies.

Second, the paper provides one of the first empirical tests of Andreoni's (1991) proposition that more severe punishment should lead to a lower probability of conviction. While the proposition is widely accepted as theory, empirical evidence has been scant at best. We identified only two empirical studies using data on actual cases. Snyder (1990) finds a reduction in the probability of conviction in antitrust cases as the level of charges for certain antitrust violations was raised from misdemeanor to felony. Bjerk (2005), who explores primarily the response of prosecutors to the three-strikes laws, also tests whether offenders qualifying for a third-strike offense face lower probability of conviction at trial, but does not find any significant effect.[6] We do find a significant decrease in the overall

[5] The expected sentence actually served was 50 percent of the sentence imposed upon conviction times the probability of being convicted prior to the adoption of the TIS laws. In the absence of behavioral responses, it would rise to 85 percent, a 70-percent increase. The behavioral responses reduced the product of the sentence and the probability by 8 percent. Hence the new sentence actually served, conditional on arrest, increased to 78 percent (92 percent of 85), which is 56 percent higher than the pre-TIS law level.

[6] Bjerk's result may plausibly be explained by sample selection. The three strikes laws made it more likely that

probability that an arrested defendant is convicted. Further, when investigating the particular channels behind this overall effect, we find that TIS laws reduced the probability of conviction through a higher probability that the case is dismissed, lower probability that the defendant pleads guilty, and, to a lesser extent, through a lower probability of conviction at trial.

Third, the paper adds new results to the empirical literature on the behavioral responses of prosecutors. One line of the literature finds that prosecutors "exploit" enhanced statutory sentences, consistently with models of the prosecutors that maximize the total punishment imposed. Kuziemko (2006) shows that defendants in murder cases in New York were accepting plea bargains with harsher terms after the state reintroduced the death penalty in 1995, while the likelihood that the defendant would plead guilty did not change. Kessler and Piehl (1998) find that California's Proposition 8, a popular initiative that mandated enhanced sentences for offenders with certain criminal histories caused an increase in sentences for those crimes that were subject Proposition 8 as well as for crimes that were factually similar but were not subject to Proposition 8.

Another line of the literature instead finds that the prosecutors mitigate enhanced statutory sentences, which is rather consistent with the view prosecutors that use their discretion to apply broader social norms of justice and fairness in punishment. Bjerk (2005) studies the impact of the three-strikes laws which dramatically enhanced prison sentences to criminals with at least two prior violent felony convictions. The prosecutors became more likely to reduce the charge from felony to misdemeanor when the defendant was at risk of receiving a three-strike sentence. Walsh (2004) documents that between 25-45 percent of offenders eligible for a three-strike sentence in urban counties in California have their prior strikes dismissed.[7]

According to our findings, the probability that the defendant would plead guilty decreased by 10 percent and the probability that the prosecutors would reduce charges from felony to misdemeanor decreased by 4 percent. Pleading guilty apparently became a less favorable alternative to trial; these findings rather support the "exploiting" view of the prosecutors.

Fourth, the paper provides interesting results on the heterogeneity of the responses. The TIS laws were designed primarily to punish violent criminals more severely, although about one third of the states extended them to non-violent crimes as well. The behavioral responses to the TIS laws were more pronounced for non-violent crimes, i.e., those crimes at which the laws were not primarily

a felony defendant with two prior strikes would have charges reduced to a misdemeanor (resulting in cases with relatively stronger evidence being prosecuted as felonies) and that he would not accept the plea bargain (resulting in cases with relatively stronger evidence being continued to trial). The shift in the distribution of cases reaching trial shifts the probability of conviction upward, offsetting the predicted behavioral response.

[7] The findings by Bjerk (2005) and Walsh (2004) can alternatively be rationalized as an optimal response by prosecutors who maximize the average sentence or number of convictions at trial subject to the resource constraint. Realizing that the judge or jury will be very reluctant to convict a defendant with two prior strikes when the punishment for the third-strike offense is deemed too severe (typically a situation when the third strike is a relatively petty crime), the prosecutor anticipates that winning the case would require substantial resources that would no longer be available for other cases. Offering "softer" terms to the defendant is then optimal even for a prosecutor who maximizes the average sentence and need not necessarily indicate an intentional objective to mitigate very long sentences.

targeted. Judges and prosecutors appear to respond more strongly when the actual content of the law deviates from its stated objectives.[8]

Last, the paper also provides several policy-relevant findings about the effects of the TIS laws themselves. So far, Shepherd (2002) analyzed their deterrence effects. Using a county-level panel, she estimates the effect of the TIS laws on crime rates, arrest rates, and the median prison sentences. She finds that the arrest rates increased with the introduction of TIS laws as the states that introduced the TIS laws tended to adopt a "tough on crime" attitude and the police made more effort to arrest. Similarly she finds an increase in the imposed prison sentences. Her estimates can be interpreted as evidence of judges and prosecutors not offsetting an increase in the severity of punishment; alternatively they can be interpreted as evidence of other "tough on crime" policies that were correlated with the adoption of the TIS laws. Our empirical strategy differs from that of Shepherd; we use case-level as opposed to county-level data and our "difference-in-differences-in-differences" estimator allows controlling for the unobservable "tough on crime" policies. In addition, we provide new findings of a substantial reduction in the probability of conviction and an overall reduction in the sentence imposed conditional on arrest. Our other findings, namely the reduction in the plea rate and an overall increase in the sentences imposed upon conviction, generally concur with those of Shepherd. Owens (2010), using the same data set as we do, detects a particular response to the TIS laws in the criminal procedure - people who were arrested for an offense covered by the TIS law, but pleaded guilty to a misdemeanor (to which the TIS requirement does not apply) were punished with relatively harsher sentences. Our paper instead evaluates the impact of the TIS laws on the overall punitiveness of by the criminal justice process and on a broader range of case outcomes.

2 Theoretical predictions

This section discusses the behavioral responses to the TIS laws predicted by the theoretical literature. Simple expressions of measurable case outcomes organize our thinking:

$$S^S = S^C \cdot f \tag{1}$$

$$E[S^S|arrest] = p \cdot S^C \cdot f = \big(p \cdot S^C + (1-p) \cdot 0\big) \cdot f = S^A \cdot f \tag{2}$$

The punishment suffered by a convicted defendant is the sentence actually served in prison S^S which is a product of the sentence imposed upon conviction S^C and the fraction of the sentence that is

[8] Such a selective response is presumably possible only if the judges and prosecutors share the stated objective of the legislation, which apparently was the case of the TIS laws (Shepherd 2002).

actually served f. The expected punishment facing an arrested defendant is the expected sentence actually served in prison $E[S^S|arrest]$ which in turn is the product of the probability p that he is convicted (conditional on arrest), the sentence imposed if convicted, and the fraction actually served. The sentence if not convicted is of course zero. Adding the outcome under non-conviction to the expression in equation 2 shows straightforwardly that the expected sentence actually served in prison can also be stated as the expected sentence imposed (conditional on arrest) S^A multiplied by the fraction of the sentence served. The variable S^A summarizes adjustments in the probability and the sentence into a single measure of punishment that is produced as an "output" of the criminal procedure.

TIS laws exogenously shifted the fraction f upwards by a certain percentage and they would have, *ceteris paribus*, mechanically increased the expected sentence actually served $E[S^S|arrest]$ by that same percentage. However, the probability of conviction and the sentence upon conviction are determined endogenously and as a result, $E[S^S|arrest]$ may have increased by less than the mechanical change. We estimate how the variables that are determined inside the courtroom, p, S^C, and S^A, respond to a change in f. (We unfortunately cannot estimate the effect of the TIS laws on $E[S^S|arrest]$ since the data on prison releases do not cover enough years after the adoption of the laws.)

The predicted effect of the TIS laws on the probability of conviction follows a well-known model by Andreoni (1991). As the sentence actually served in prison increases, the social cost of convicting an innocent defendant also increases. The judge or jury who cares about the social costs of wrongful conviction then requires a higher standard of proof to convict a defendant.[9] The conviction rate among the cases resolved at trial should therefore fall. A similar trade-off may operate at other stages of the criminal procedure, such as the decision whether to dismiss a case.

In the plea bargaining process, changes in case outcomes reflect behavioral responses of the prosecutor (the terms of the plea bargain he offers) and the defendant (willingness to accept the terms). The predicted effects also depend on a particular model of the prosecutor, where the existing literature offers two broad views: According to one, the prosecutors are maximizing a well-defined deterrence objective, such as the total punishment imposed.[10] According to another, they pursue broader objectives of justice and fairness and apply punishment in accordance with these objectives.[11] Even though some prediction are ambiguous, certain observed effects of the TIS laws allow discriminating between these alternative views. A reduction in the plea rate is predicted by the "maximizing" view of the prosecutors while an increase is possible under both views. A decrease in the probability that the prosecutor reduces charges is predicted by the "maximizing" view and an increase by the "justice-pursuing" view.

[9] Ezra and Wickelgren (2005) reach the same prediction in an alternative model where the population of defendants is endogenous.

[10] Landes (1971), Easterbrook (1983), Reinganum (1988, 2000).

[11] (Miceli 1996).

In the "maximizing" models of the prosecutorial behavior, the prosecutor typically offers a sentence that makes the defendant indifferent between accepting the plea or going to trial.[12] If the TIS law applies irrespective of whether the defendant pleads guilty or is convicted at trial, the sentence to be actually served S^S rises mechanically as f increases for both trial and plea convictions. The prosecutor then need not adjust the imposed sentence S^C to make the defendant indifferent.[13] However, pleading guilty frequently implies being convicted of less severe charges compared to a potential conviction at trial. In such situations the TIS laws may apply under the trial conviction but need not not apply under the plea conviction. A maximizing prosecutor should then offer a longer sentence S^C or be less likely to reduce the charges. The prosecutor essentially "exploits" the increased gap between the actual sentence served under trial and under the plea, and offers the defendant less favorable terms in the plea bargain.

The predicted impact on the defendants' plea choice is theoretically ambiguous. On one hand, they would be more likely to plead guilty if the TIS law applies only to the trial sentence. However, if the prosecutors offer tougher bargains because of the TIS laws, the plea rate may fall. Likewise, the defendants would be less likely to accept the plea if they take into account that the probability of conviction at trial decreased.

In the "justice-pursuing" view of the prosecutors, the prosecutors may regard the increase in f as a departure from the prevailing norms of justice and use their discretion to mitigate its impact. They would then offer shorter sentence S^C and be more likely to reduce charges. As a result, the defendants should be more likely to accept plea bargains.

In the sentencing stage, the judges may offset a higher fraction of the sentence actually served in prison simply by imposing shorter sentences. This would be particularly the case if they regard the mandated increase in the fraction of the sentence served as unjust.[14]

The preceding discussion of the particular behavioral responses implies predictions of the sign of our summary measures. The overall probability p that an arrested offender is convicted (by pleading guilty or at trial) is most likely expected to fall, although there is a theoretical possibility that it could rise if the prosecutors are mitigating the increased actual sentences and defendants become sufficiently more likely to accept plea bargains. The expected imposed sentence conditional on arrest S^A should most likely decrease as the probability of conviction decreases and the judges also reduce the sentences; however, there is a theoretical possibility that it may rise if the prosecutors

[12] If the offenders are of different types (e.g., when they have imperfect information about the strength of evidence against them) and the prosecutor cannot distinguish their type, the optimal sentence offered involves only a marginal defendant being indifferent between the plea and trial: while defendants who think the case against them is weak strictly prefer a trial, those who think the case against them is strong strictly prefer pleading guilty.

[13] Whether he would optimally adjust the offered sentence upward or downward depends on the details of the model. For example, the very basic version of the Landes (1971) model with risk-neutral defendants and positive costs of trial predicts that the prosecutor should reduce the maximum sentence offered.

[14] The legal literature has been concerned with the sentencing implications of parole releases (see Genego et al. [1975] for an early example). The empirical evidence on the relationship between sentences imposed by judges and the anticipation that the offender will be released early is, to our best knowledge, missing.

offer sufficiently harsher sentences in plea bargaining.

3 Data and empirical strategy

We use the "*State Court Processing Statistics: Felony Defendants in Large Urban Counties (SCPS),*" an individual level data set on approximately 100,000 criminal cases in state courts.[15] The sample covers 45 counties selected from 75 percent of the most populous counties in the United States. It tracks cases that were filed in May of every even year from 1990 till 2002. The universe of the data set is cases initiated by a felony arrest.[16] Due to missing values for relevant variables in some observations, the sample used in regressions has over 83,000 observations.

The SCPS data set contains rich information on each case: offender characteristics such as age, sex, and detailed prior record, information about the procedural aspects of the case (pretrial detention, type of attorney), and the final disposition of the cases including the length of the maximum jail or prison sentence, if applicable. The offenses are divided into 16 categories - violent crimes (murder, rape, robbery, assault, other violent) and non-violent crimes (burglary, larceny-theft, motor vehicle theft, fraud, other property crime, drug sales, other drug crimes, and four other minor categories). The data is summarized in the first column of Table 2.

The empirical strategy is based on a "quasi natural experiment" which compares the treatment cases (those covered by the TIS laws) with appropriately chosen control cases. We adopt two alternative "difference-in-differences-in-differences" (D-i-D-i-D) estimators, formally stated as

$$Y_{icst} = f(TIS_{icst}, TISstate_{st}, X_{icst}, \lambda_{ct}, \lambda_{av}, \epsilon_{icst}) \quad (3)$$

$$Y_{icst} = f(TIS_{icst}, TISstate_{st} \times violent_{icst}, X_{icst}, \lambda_{ct}, \lambda_{av}, \epsilon_{icst}), \quad (4)$$

where i, c, s, and t denote the individual case, offense type, state, and year, respectively. Additionally a denotes county and v denotes violent crime. Y_{icst} stands for the outcome variable and TIS_{icst} is a dummy variable indicating whether the individual case is covered by the TIS law.[17] $TISstate_{st}$ is a dummy variable equal to one if a state has the TIS law in force. $TISstate_{st} \times violent_{icst}$ is a dummy variable equal to one if a state has adopted the TIS laws and a given offense is a violent felony. X_{icst} is a vector of individual characteristics of the offender and the case.[18] Finally, we

[15] The data is collected by the Bureau of Justice Statistics. ICPSR study #2038.

[16] About 15 % of cases end up adjudicated as misdemeanors.

[17] The TIS case dummy may change for a given case during the criminal process. For example, the person may be arrested for a violent felony, and if convicted for a violent felony, the TIS law would apply. However, he may be convicted for a misdemeanor, and the TIS law would no longer apply. In the regressions we set the TIS law according to the offense type that the offender is charged with at the relevant stage of the criminal process.

[18] Prior felony convictions (measured by dummies for 1, 2, and 3 or more prior convictions), number of prior misdemeanor convictions, log age, log age interacted with the prior conviction dummies, gender dummy, race/origin

include offense-year fixed effects λ_{ct}, and county-violent crime fixed effects λ_{av}.[19] The offense-year fixed effects control for unobserved heterogeneity at the level of each offense and year. Compared to commonly used offense and year fixed effects, they impose less restrictive assumptions on the structure of of the unobservables and allow, for example, separate national trends in the outcomes of criminal cases for each offense. The county-violent crime fixed effects control for unobserved heterogeneity at the county level, further disaggregated for violent and non-violent crimes. In alternative specifications, we include state-offense fixed effects instead.[20] ϵ_{icst} is an error term.

We use the D-i-D-i-D estimator, as opposed to the more conventional difference-in-differences (D-i-D) estimator since the identifying assumption for the latter is unlikely to hold. It would require that there was no differential change between the adopting and non-adopting states in the unobservables that affect outcomes in the offenses covered by the TIS laws after the adopting states implemented them. However, the states adopting the TIS laws may have adopted other "tough on crime" policies precisely because the objective of the laws was to punish certain crimes more severely. If that was the case, the error term may be correlated with the TIS_{icst} case dummy variable.

Our first specification (equation 3) therefore includes a TIS state control (variable $TISstate_{st}$). It captures the effect of state-specific unobservable variables that are potentially correlated with the adoption of the TIS laws and affect all crimes equally. The effect of the TIS laws is estimated from a within-state comparison of the change in the outcome for the crimes covered by the TIS laws with the crimes that are not covered. It is identified under the assumption that *within a state* the unobservable characteristics of TIS offenses and other offenses follow the same trend, even though they may not follow the same trend in the adopting and non-adopting states. In other words, the adopting states may have gotten "tougher on crime" than the non-adopting states, but then did so equally for all crimes.

The second specification (equation 4) exploits the fact that violent felonies are covered by the TIS laws in all states that adopted them while property, drug, and other non-violent crimes are covered only in some states. It includes a TIS state–violent crime interaction (variable $TISstate_{st} \times violent_{icst}$,) which captures the effect of unobservables that are correlated with the adoption of the TIS laws and affect violent crimes only. The effect of the TIS laws is estimated from a between-state comparison of the change in the outcome for non-violent crimes in the states that imposed the TIS requirement on both violent and non-violent crimes with the states that imposed the TIS laws on violent crimes only. The estimates are identified under the assumption the adopting states may have gotten "tougher" on violent than on non-violent crimes but must have gotten proportionately

dummies (white non-hispanic, black non-hispanic, hispanic, and other), and type of attorney (public, private, assigned, *pro se*, and other) are included in the X vector.

[19] Represented by interactions of county dummies with a dummy variable equal to one for violent offense and zero for other offenses.

[20] Ideally, we would include the county-offense fixed effects. However, there are too few observations for many county-offense combinations which prevents a meaningful estimation. The county-violent crime fixed effects or state-offense fixed effects are therefore workable compromises, still superior to a specification with only county or state fixed effects which assumes away any differences in unobserved heterogeneity between offense types within states.

tougher on violent crimes irrespective of whether they imposed the TIS laws on all crimes or just violent crimes.[21]

A possible change in the sample composition poses a concern. The TIS laws were accompanied by more intensive policing (Shepherd 2002). As the police arrests a larger fraction of offenders, it is possible that it also arrests a different sample of offenders; namely the marginal offenders now being apprehended are likely to be those who are more difficult to identify. The evidence against such offenders is likely to be weaker and they are less likely to be convicted. As a result, the average probability of conviction may fall even in the absence of any behavioral response. The importance of this problem can be checked by comparing the observable characteristics of cases before and after the adoption of the TIS laws; presumably, should there be a change in the sample composition of observables, it is quite likely that the unobservables changed as well. Table 2 show the sample means for the observable characteristics of cases in the last year in the SCPS data set before the TIS laws were adopted, and in the first year after the adoption.[22] The table does not show discernible changes in the observable characteristics. The only exception is the share of defendants who use a public defender, which rose by 10 percentage points in violent and by 11 percentage points in non-violent crime cases. This may have indeed reflect a change in the strength of evidence but the bias would rather go against the predicted effects (public defenders tending to represent in less defensible cases). We further address the sample composition issue in two robustness checks (section 4.7) with little effect on the results.

4 Results

This section presents the results in two steps: Firstly we present the summary measures: the reduction in overall probability of conviction conditional on arrest and the decrease in length of sentence imposed given the arrest. Then we investigate specific channels behind the two summary findings[23] and the heterogeneity of behavioral responses across different offense categories.

[21] Admittedly, the estimates are not identified if states imposed the TIS laws on certain crimes and targeted other "tough on crime" policies on the same crimes. Unfortunately, there is no case-level variation within a particular crime (which would be the case if the TIS laws applied only to offenders with certain characteristics, for example).

[22] The data set records arrests made in May of an even year. For the two states that adopted the TIS laws in the first few months of an even year, we use the observations two years after the adoption to allow the effect of TIS laws to be fully realized for the purpose of this before-after comparison.

[23] Two of the specific channels (the probability of conviction at trial and the length of sentence upon conviction)) are estimated on subsamples of cases at different stages of the criminal procedure. The natural concern is that results for those channels are possibly affected by sample selection. The TIS laws may have changed the distribution of unobservable characteristics of cases that result in conviction or that proceed to trial. For example, if the TIS laws reduce the fraction of cases settled in plea bargaining, the marginal offenders now proceeding to trial would face longer potential sentences than the average offender previously proceeding to trial. Unfortunately we do not have instruments that would be correlated with the likelihood that the case proceeds to the subsequent stage and at the same time would not be correlated with the error term in the outcome equation in that stage. We still think it is preferable to present such results as tentative evidence and interpret them with caution. Majority of the channels (the probability that the case is dismissed, the probability of pleading guilty, and the probability that the prosecutor reduces charges) are estimated on the full sample and hence are not affected by sample selection.

4.1 Probability of conviction conditional on arrest

Our first summary measure of the effects of the TIS laws is the change in the probability that an arrested offender is eventually convicted, irrespective of whether via plea bargaining or conviction at trial. The marginal effects from probit estimates are presented in Table 3. They imply a reduction in the probability of conviction by 9 percent. This result is robust to alternative specifications – controlling for the TIS state or the TIS state–violent crime interaction (columns 1 and 2) or for replacing the county-violent crime fixed effects with state-offense fixed effects (columns 3 and 4).[24] In all specification the marginal effects of the TIS case dummy are significant at 1 percent level.

We also report the marginal effects of the TIS state and the TIS state–violent crime controls to demonstrate the appropriateness of the D-i-D-i-D estimator.[25] The coefficients of these two controls imply that the introduction of the TIS laws was associated with an overall increase in the probability of conviction, including the cases that were not subject to the TIS laws, on the order of 4 to 11 percent. Correspondingly, our estimates are different from the simple D-i-D estimates; when we exclude the $TISstate_{st}$ or the $TISstate_{st} \times violent_{icst}$ controls such that the specification is reduced to D-i-D, the marginal effect of the TIS_{icst} dummy becomes smaller in magnitude (-0.069). Even though these regressions do not directly estimate the choices by judges and juries, they nevertheless provide strong support for Andreoni's prediction in the sense that the criminal justice system convicts less if the sentences to be actually served are raised.

4.2 Sentence imposed conditional on arrest

The second summary measure of the behavioral responses to the TIS laws is the change in the sentence imposed conditional on arrest S^A. It is obtained by estimating equations 3 and 4 on the full sample of arrests, the dependent variable being the logarithm of the maximum prison or jail sentence imposed (in months). If the defendant was not convicted, the sentence in the regressions is set to zero.[26]

We estimate Tobit and quantile regressions instead of the conventional OLS for several reasons. The observed sentences are naturally censored at zero. They should also be censored at very high sentence length, since the requirement to serve 85 percent out of a 70-year maximum sentence may be of little practical significance. We therefore run Tobit regressions with the lower bound set at zero and the upper bound at 55 years.[27] Also, we expect the impact of the TIS laws to be more

[24] We also estimated alternative specifications which included a dummy variable for the presence of sentencing guidelines in a state and its interaction with the TIS case dummy; the key findings of the effects of the TIS laws were unaffected.

[25] The marginal effects on the two controls are 0.041 and 0.07 in the specification with county-violent crime fixed effects, and 0.1 and 0.11 in the specification with the state-offense fixed effects.

[26] The sentence is set to zero if the defendant was convicted but was punished with a fine instead of a prison or jail sentence. To deal with the logarithm of zero, we add one month to each sentence.

[27] As an alternative, we estimated the Tobit model with the lower bound equal to 4.5 months - treating non-convictions and convictions with short sentences as equivalent outcomes, with little effect on the results.

pronounced the longer is the potential sentence since the difference between serving, say 5 weeks or 8.5 weeks out of a 10-week maximum sentence may not be of such a concern to the judge than the difference between serving, say 5 years or 8.5 years out of a 10-year maximum sentence. The natural tool to address this issue is a quantile regression estimated at several quantiles. It predicts a change in a given quantile of the distribution of the dependant variable due to a change in the independent variable.

Table 4 shows the Tobit estimates. In the specifications with the offense-year and county-violent crime fixed effects, the marginal effect of the TIS case dummy is -0.114 when the TIS state control is included (column 1) and -0.097 when the TIS state-violent crime control is included (column 2). Both are significant at 1 percent level. In the specifications with offense-year and state-offense fixed effects, the marginal effects are smaller in magnitude (-0.083 and -0.039 for the respective controls (columns 3 and 4), and significant at the level of 1 and 10 percent.[28]

The estimates of the quantile regressions for the 75th and 90th quantiles are shown in Table 5.[29] They demonstrate that the behavioral response leading to shorter expected sentences was concentrated on the longest sentences, conditional on other factors. The marginal effects of the TIS case dummy are several times smaller in magnitude at the 75th quantile (columns 1 and 3) than at the 90th quantile, although all of them are statistically significant at 1 percent level.

Both sets of regressions show fairly consistently that offenders overed by the TIS laws experienced a reduction in the sentence that they can expect at the time of arrest, compared to offenders not covered. The reduction was not trivial; we regard the average of the four Tobit estimates (8.3 percent), as the most preferred "summary" result.

4.3 Probability of conviction disentangled

The TIS laws may have reduced the likelihood of eventual conviction through three channels: a lower probability of conviction at trial, a higher probability that the case is dismissed before reaching a verdict on merits, or a lower probability that the offender accepts a plea bargain. The first two columns of Table 6 estimate the effect of the TIS laws on the probability of conviction at trial. They show a statistically significant reduction (by 9.8 percent) in the specification with the TIS state control and smaller and insignificant (5.2 percent) reduction in the specification with the TIS state – violent crime interaction.[30]

[28] The marginal effects of the TIS state and TIS state–violent crime interaction controls are positive as expected and significant at 1 percent level. The unobserved factors that they capture increased the expected sentence by between 10 to 23 percent, depending on specification.

[29] The quantile regressions are estimated at 75th and 90th percentiles only. They could not be estimated at lower quantiles since zero sentence represents most observations for the 50th or lower quantiles, leaving almost no variation in the dependant variable.

[30] The results have to be interpreted with caution since the trial cases consist of highly selected sample. The selection, however, rather induces an upward bias. As the TIS laws induced fewer cases to be resolved through plea bargaining, the marginal defendants who would have plead guilty now proceed to trial. However, the evidence against such defendants would be stronger than the average defendants who proceed to trial, implying an increase in the

Columns 3 and 4 of Table 6 estimate the magnitude of the second channel by probit regressions with a dependant variable equal to one if the case was dismissed. The marginal effects of the TIS case dummy are 0.051 and 0.035 in the two basic specifications, and both are significant at the 1 percent level.[31] The tendency to convict less apparently applies to other stages of adjudication and not just to conviction/acquittal verdicts at trial. Unfortunately we cannot say to what extent the higher probability of a dismissal is due to more dismissals by the judges during the pre-trial reviews and preliminary hearings or by the prosecutors since both are theoretically plausible.

4.4 Plea bargaining

The next set of probit regressions estimates the effect of the TIS laws on the likelihood that the case outcome is a guilty plea (columns 5 and 6). The estimates show a 9.5 percent reduction in the the specification with the the TIS state control and a 11 percent reduction in the specification with the TIS state–violent crime interaction.

The reduction in guilty pleas did not come about mechanically from the fact that more cases were dismissed and therefore fewer cases were left to be potentially resolved through plea bargaining. When the regressions are re-estimated on a subsample of cases that were resolved either through plea bargaining or at trial, the marginal effects of the TIS case dummy are statistically significant at the 1 percent level, although somewhat smaller in magnitude (-4.1 and -7.2 percent in the two alternative specifications).[32]

As the data does not record the exact terms that the defendants were offered in the plea bargaining process, we can only partially infer whether the reduced probability of accepting a plea bargain is due to the defendants being less willing to plead guilty holding the terms of the plea bargain constant or due to the prosecutors offering relatively worse terms. The SCPS data allows us to check two channels through which the prosecutors can make the bargains less generous: by being less likely to reduce the charge from felony to misdemeanor (while all defendants in the data set were initially arrested with a felony charge) or by being less likely to reduce the charge to a felony which carries a shorter sentence. Results from a probit dependant variable equal to one if the case was adjudicated as a misdemeanor (columns 1 and 2) of Table 7 results show a significant reduction in the likelihood that the charges would be reduced to a misdemeanor (by 4 or 2.7 percent, respectively, depending on the controls). The next two columns report marginal effects from a probit regressions where the dependant variable is equal to one if the predicted sentence for the offense for which the case was adjudicated is shorter than the predicted sentence for the offense for which the defendant was arrested.[33] The sample is restricted to cases that were adjudicated as

probability of conviction. The relatively small sample size (4363 cases) inevitably limits the statistical significance of the results.

[31] The coefficients on the TIS state and TIS state - violent crime interaction controls are negative, again indicating a presence of other "tough on crime" factors that tended to reduce dismissals.

[32] Detailed results are available upon request.

[33] The dependant variable was constructed as follows: First, we regressed the logarithm of the sentence as a function

felonies (to isolate the reductions to a misdemeanor which we already estimated) and that resulted in conviction, since only for conviction cases is the adjudication offense recorded in the SCPS data set. The results show a reduction in the likelihood of reducing charges by 2.3 percent when the TIS state control is included and a smaller (and insignificant) reduction when the TIS state–violent crime control is included.

These findings are qualitatively similar to Kessler and Piehl (1998) and tend to support the "maximizing" view of the prosecutors as opposed to the "justice-pursuing" view of the prosecutors. The prosecutors appear to have "exploited" the increase in the severity of punishment by the TIS laws by offering the defendants harsher terms which they in turn became less likely to accept. The contrast to Bjerk's (2005) finding that the prosecutors got "softer" in response to the three-strikes laws warrants further discussion. The difference in results can hardly be attributed to the differences in empirical methodology, as Bjerk (2005) adopts a very similar D-i-D-i-D empirical strategy, uses the same data, but estimates the prosecutors' response to a different punishment-enhancing policy. We instead hypothesize that the responses of prosecutors (and other enforcement agents in general) to enhanced legislated sentences are inevitably context-specific. If they prosecutors regard more severe sentences as unjust, the tendency to "purse justice" would dominate and their actions would mitigate the increased severity. On the other hand, if more severe sentences conform to the prosecutors' norms of justice (in a given context), the desire to mitigate is absent and we observe responses consistent with narrow maximization objectives. The prosecutors apparently shared the objectives of the TIS legislation (Shepherd 2002) which possibly explains why their observed responses are consistent with the prosecutorial maximization in the context of the TIS laws but not in other contexts.

4.5 Length of sentence imposed upon conviction

The last two columns of Table 6 shows the effects of the TIS laws in the last stage of the criminal procedure, i.e., sentencing of the defendants who were convicted.[34] Additional control variables are introduced: The plea dummy captures the difference between the sentence in plea and trial cases while its interaction with the TIS case dummy allows us to see whether the TIS laws had a differential impact on sentencing in plea cases vis-à-vis the trial cases. The marginal effects of the TIS case dummy are positive and significant at the 1 percent level (0.223 and 0.260). The marginal effects of the plea-TIS interactions are negative, but small and insignificant -0.053 and -0.058)

of offense dummies, year dummies, and county-violent crime dummies in a sample of cases that resulted in a conviction via plea bargaining. Second, we use the coefficients from this regression to predict, for each case in the sample, the sentence for which the defendant was arrested and the sentence for which the case was adjudicated. Third, if the latter predicted sentence is shorter than the former, the variable categorizing whether charges were reduced is equal to one. Across the sample, 11 percent of defendants who are convicted of a felony are convicted of a felony with a shorted sentence than for which they were arrested.

[34]The Tobit regressions are equivalent to those estimating the sentence conditional on arrest except that we add a dummy variable for whether the defendant pleaded guilty and an interaction of the plea dummy with the TIS case dummy.

implying that the TIS laws did not have a discernibly differential effect on the sentence length in cases resolved through plea bargaining or trial. The positive coefficient on the TIS case dummy was obtained also when we experimented with alternative specifications.[35]

These results do not support the prediction that the judges would mitigate a higher fraction of the sentence served by imposing shorter sentences.[36] One explanation is that our TIS case dummy is still partially correlated with other "tough on crime" policies even after controlling for the presence of the TIS law in the state, and the resulting upward bias is greater than the behavioral response. The second explanation comes from sample selection for which we were unable to correct for. As the cases covered by the TIS laws are more likely to be dismissed, the relatively weaker cases that would have received relatively shorter sentences drop out of the sample. Also, defendants covered by the TIS laws are more likely to reject the plea bargain and go to trial. All else equal (including a sentence received if pleading guilty), the marginal defendant who was indifferent between a guilty plea and a trial expects to receive a longer sentence at trial than an inframarginal defendant who strictly preferred going to trial. If the TIS laws shift the marginal defendant to choose to go to trial, the average sentence at trial would then rise, and the average sentence in plea bargains would fall, as the results suggest.

4.6 Offense-specific effects

We also estimate the impacts of the TIS laws specific to individual crime categories: murder, violent crime (other than murder), property, drug, and other crime.[37] Table 8 reports the main results from regressions that are equivalent to those in Tables 3-7, except that the single TIS dummy variable is replaced by interactions of the TIS dummy with the dummies indicating the five offense categories.[38]

The TIS laws affected the two main outcomes of interest, the probability of conviction conditional on arrest and the sentence imposed conditional on arrest, predominantly among non-violent crimes. The probability of conviction declined by 13.6, 6.9, and 14.5 percent for property, drug, and other crimes, respectively; the sentence conditional on arrest declined by 15.3, 9.9, and 14.3 percent. The estimated effects are significant at 1 percent level. For violent crimes (other than murder), the results indicate a smaller (5 percent) reduction in the probability of conviction but no significant

[35] Such as including a dummy variables for the presence of the sentencing guidelines in the state, its interaction with the TIS case dummy, or including state-offense fixed effects instead of county-violent crime fixed effects.

[36] The only rather weak indicator of the offsetting behavior are the offense-specific effects of the TIS laws (Table 8). For violent crimes, there is indeed a large and negative effect on the sentence length.

[37] The "violent crime" (other than murder) category includes rape, robbery, assault, and other violent crime; "property crime" category includes burglary, larceny-theft, motor vehicle theft, forgery, fraud, and other property crime; "drug crime" category includes drug sales and other drug offenses; "other crime" category includes weapons-related offenses, driving-related offenses, and other offenses.

[38] It is impossible to estimate the specification with the $TISstate_{st} \times violent_{icst}$ interaction variable because all states that adopted the TIS laws covered all violent crimes. The effects on violent crimes overall and sub-categories of violent crimes cannot be separated.

effect on the sentence imposed conditional on arrest. Almost no estimates are significant for murder.

Similar patterns apply to the particular channels behind the summary measures. The estimated effects of the TIS laws on the increase in the probability that a case is dismissed, the reduction in the probability that the defendant accepts the plea bargain, and the reduction in the probability that charges are reduced to misdemeanor are all larger in magnitude and have smaller standard errors for non-violent crimes than for the violent crimes. On the contrary, the estimates for the sentence imposed upon conviction they show large reductions in the sentence for violent crimes but are not significant and have different signs for other crimes.

4.7 Robustness checks

Our main results are generally robust to alternative specifications. The first set of robustness checks addresses the concern that the TIS laws altered the distribution of unobserved characteristics of arrests. If the police makes more arrests and the marginal arrests tend to be cases with weaker evidence than the average cases, the probability of conviction would fall. This mechanism may explain the observed increase in the probability that the case is dismissed as the judges and prosecutor "weed out" some of the marginal arrests with particularly weak evidence. If, however, the judges and prosecutors apply the same standard for dismissing the case, the distribution of the strength of evidence in the subsample of cases that proceed beyond dismissal should remain constant. Our first robustness check exploits this plausible assumption by re-estimating the model on a subsample of cases that were not dismissed.[39] The estimated marginal effect of the TIS cases dummy on the probability of conviction are -0.05 and -0.059, depending on the specification (columns 1 and 2) of Table 9). They are somewhat smaller than the estimates obtained from the full sample[40], but remain highly statistically significant. Interestingly, the effect of the TIS state and TIS state–violent controls vanishes. Likewise for the sentence conditional on arrest, the marginal effects of the TIS case dummy are somewhat smaller than the full sample estimates (-0.095 and -0.082) but they are not different in the statistical sense.

The second robustness check exploits information about the pretrial phase of the case. The defendant is more likely to be released on bail and the terms of the pretrial release tend to be more favorable if the evidence is weak or the case is less serious. Should the judges apply the same standards in the pretrial release decisions under the TIS laws as they did before, the information about pretrial release is a relevant control for the strength and seriousness of the cases. The SCPS data contains information about the type of pretrial release granted,[41] the amount of bail set, and

[39] As a result, the sample is reduced to approximately 62,000 observations

[40] The confidence intervals of the marginal effects obtained from the full sample do not overlap with the confidence intervals of the marginal effects obtain from the sample excluding the dismissed cases.

[41] The types of pretrial release are categorized as follows: financial release, nonfinancial release, emergency release, held on bail, denied bail, release conditions unknown, detained but reasons unknown.

the behavior of the defendant during the pretrial phase.[42] In columns 5-8 we reestimate the model with dummy variables for each release type, the amount of bail set, and a dummy variable equal to one if the defendant failed to appear.[43] Including these controls has essentially no effect on the estimates in the probability of conviction regressions. In the sentence conditional on arrest regressions, the marginal effect of the TIS case dummy is the same (0.114) when the TIS state control is included, and slightly smaller (0.077) when the TIS state x violent crime interaction is included.

The third robustness check addresses the concern that the TIS state and the TIS state–violent crime control may not adequately capture the unobservables affecting the outcomes of violent crimes. We therefore estimate the model on a subsample of non-violent crimes only, reducing the estimator to a simple D-i-D. It comes at a cost of dropping the crimes for which the TIS laws were designed but at a benefit of keeping the crimes for which any confounding effects are likely to be less serious. The estimated effects (-0.098 for the probability of conviction and -0.129 for the sentence conditional on arrest) are similar to those obtained in the full sample and to the offense-specific effects reported for non-violent crimes in Table 8.

The last set of checks exploits the variation in the intensity of the TIS laws. There are two sources of such variation. First, while most states followed the federal law and required offenders to serve 85 percent of the sentence, 3 states in our sample opted for 100 percent[44] and 2 states for 50 percent only.[45] Second, the fraction of the time actually served had varied among states and offenses prior to the adoption of the TIS laws. We expect the TIS laws to "bite" more if the offenders had previously served a shorter fraction of the sentence. We ran the same set of regressions where we replaced the TIS dummy variable (and all interactions) with a continuous variable equal to the predicted fraction of the sentence served.

The predicted fraction is constructed as follows: for cases not covered by the TIS laws, it is computed from the National Corrections Reporting Program (NCRP) data series, individual level data on approximately 2.9 million prisoners released from prison between 1989 and 2002.[46] The data was collected at the time of release and contain information on the individual characteristics of prisoners, the offense for which they were sentenced, the maximum and minimum sentence to which they were sentenced and the time served under the current admission. The predicted fraction of the sentence served is calculated by dividing the time served by the maximum sentence for each offender and then taking the average for each state-year-offense combination. The information about the time of admission to prison allows distinguishing which prisoners were sentenced under the TIS laws and which were not. The number of observations for some states[47] is too small to allow predicting the

[42] Whether he failed to appear, became fugitive, or was re-arrested.
[43] The failure to appear is likely a good indicator of strength of the evidence and eventual conviction.
[44] Georgia, Pennsylvania, Virginia.
[45] Indiana, Maryland.
[46] The data is available at http://www.icpsr.umich.edu/cocoon/ICPSR/SERIES/00038.xml.
[47] Arizona, Connecticut, District of Columbia, Indiana, Pennsylvania.

fraction for each state-year-offense. These states were dropped, reducing the number of observations used in the regressions by 7 percent. For cases covered by the TIS laws, we set the predicted fraction to the minimum fraction required by the TIS legislation in the respective state for the respective offense.[48]

The results are presented in Table 10.[49] They are qualitatively and quantitatively similar for the following outcomes of interest: probability of conviction conditional on arrest, probability of conviction at trial, and the probability of reducing charges to a misdemeanor. For example, the marginal effect of the predicted fraction on the probability of conviction conditional on arrest is -0.074 which implies approximately a 2.5 percent reduction in that probability.[50] The marginal effect on the probability of conviction at trial implies a 12 percent reduction in that probability.

Qualitatively the same but quantitatively different estimates are found for probability of a guilty plea - the effect is also negative but very small and statistically insignificant. For three outcomes the specification with the expected fraction implies qualitatively different results than the TIS dummy: the effects on the sentence conditional on arrest and the probability that the case would be dismissed are statistically insignificant and have the opposite sign. The effect on the sentence imposed upon conviction is negative, statistically significant, and large in magnitude. The last result is at least consistent with the theoretical prediction that judges should respond to the TIS laws by imposing shorter sentences, which was not confirmed in the main regressions (Table 6).

5 Conclusions

Our evaluation of the impacts of the Truth-in-Sentencing laws produced consistent evidence on several channels of behavioral responses to more severe punishment in the criminal justice process. Requiring offenders to serve a higher fraction of their sentence in prison significantly reduced the probability that an arrested offender is convicted. This result represents one of the first empirical tests of the popular Andreoni (1991) model. Moreover, the magnitude of the reduction (9 percent) is empirically relevant and suggests that this line of behavioral response should be seriously considered

[48] Ideally, we would like to use the predicted fraction served for cases covered by the TIS laws as well. However, we have two reasons why we prefer the legislated rather than predicted fraction. First, the predicted fraction is likely to be downward-biased for the cases covered by the TIS laws. New admissions to prison covered by the TIS laws occur only after the TIS laws are in force (1994 or later in most states). The NCRP data set therefore cannot record releases of prisoners who served 8 or more years post-TIS (and actually more than mere 2 years for those admitted to prison in 2000). Missing observations for releases after 2002 induces a downward bias in the estimate of the fraction since we are more likely to observe prisoners who were released early. Due to this limitation we are also unable to observe post-TIS fraction of the sentence served for very long maximum sentences. Second, it may be more plausible to assume that agents in the criminal process acted upon the expectation that the post-TIS offenders would serve the legislated minimum fraction rather than the ex-post realizations of the fraction.

[49] Due to space limitations, only the coefficients on the expected fraction served and their standard errors are reported. Full results are available upon request.

[50] The TIS laws raised the expected fraction of the sentence served from approximately 50% to 85%, i.e., by approximately 0.35. The coefficients on the fraction served should therefore be divided by 1/0.35 (approximately 3) to obtain estimates comparable to those on the TIS dummy variable.

in the design of sentencing policies.

The overall effect of the TIS laws was a reduction in the imposed sentence expected upon arrest. The stated intention of the TIS laws to increase criminal punishment was therefore mitigated by the behavioral responses on several margins. The magnitude of the mitigating effect is empirically relevant as well. In the absence of the behavioral responses, the increase in the fraction of the sentence served to 85 percent would have increased the expected sentence actually served by 70 percent on average. The behavioral responses reduced the expected imposed sentence conditional on arrest by 8 percent, which implies that the expected sentence actually served rose by "only" 56 percent.[51] The behavioral responses have therefore undone about one-fifth of the intended direct effect of the TIS laws. Also, they inevitably increased the disparities in punishment. Because of the TIS laws, a higher fraction of defendants walk away with no punishment at all while a smaller fraction of those who are convicted are punished much more severely.

Last, the results give an interesting perspective on the behavioral responses of the judges and prosecutors. The behavioral responses were most pronounced for non-violent crimes but small or insignificant for violent crimes. The primary goal of the TIS laws was to punish violent offenders more heavily. If the judges and prosecutors share that goal, they may not apply any offsetting behavior in violent crime cases. But they may have as well regarded the TIS laws as unnecessarily overreaching when they were applied to non-violent crimes; the offsetting behavior is then a logical reaction. A similar conclusion can be drawn when comparing our finding that the prosecutor got "tougher" in plea bargaining in response to the TIS laws with Bjerk's (2005) finding that the prosecutors instead got "softer" in response to the three-strikes laws. Judges and prosecutors do respond to more severe sentences but they do so selectively. Alternative models of judicial and prosecutorial behavior need not be, after all, mutually exclusive but may correctly characterize the behavior of even the same individual judges and prosecutors depending on the context of the particular legislation.

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[51] The expected sentence actually served was $pS^C \cdot 0.5 = S^A \cdot 0.5$ in the absence of the TIS laws (0.5 being the average fraction of the sentence served). In the absence of behavioral responses, it would rise to $S^A \cdot 0.85$, a 70-percent increase. The behavioral responses reduced S^A by 8 percent. Hence the new sentence actually served, conditional on arrest, increased to $S^A \cdot 0.92 \cdot 0.85$, which is 56 percent higher than the pre-TIS law level.

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Table 1: Adoption of the TIS laws

State	Year of introduction	Requirement(%)	Type of crime covered
Alabama	NA		
Arizona	1994	85	all
California	1994	85	violent felony
Connecticut	1996	85	violent felony
District of Columbia	1989	85	violent felony
Florida	1995	85	all
Georgia	1995	85	violent felony
Hawaii	NA		
Illinois	1995	85	all
Indiana	NA		
Kentucky	1998	85	violent felony
Massachusetts	NA		
Maryland	NA		
Michigan	1994	85	part I violent
Missouri	1994	85	repeat or dangerous felony
New Jersey	1997	85	violent felony
New York	1995	85	violent felony
Ohio	1996	85	felony
Pennsylvania	1911	100	part I violent
Tennessee	1995	85	violent felony
Texas	1993	50	aggravated
Utah	1985	85	all
Virginia	1995	100	felony
Washington	1990	85	part I violent
Wisconsin	1999	100	felony

Sources:
United States General Accounting Office: Truth In Sentencing: Availability of Federal Funds Influenced Laws in Some States, Report to Congressional Requesters, February 1998.
Chen, Elsa: Impact of Three Strikes and Truth in Sentencing on the Volume and Composition of Correctional Populations, Report Submitted to the National Institute of Justice, March 2000.
Table includes only the states covered in the SCPS data set.

Table 2: Summary Statistics

	Mean	violent crime		non-violent crime	
Case Outcomes	all	last year before TIS adoption	first year after TIS adoption	last year before TIS adoption	first year after TIS adoption
% convicted cases/arrest	67.31	64.24	60.06	75.59	72.33
	(46.90)	(47.94)	(48.99)	(42.96)	(44.74)
sentence/arrest in months	15.06	29.17	24.23	15.19	11.66
	(69.19)	(117.58)	(114.15)	(71.44)	(54.96)
% convicted cases/trial	80.23	77.01	75.25	76.30	78.59
	(39.83)	(42.19)	(43.26)	(42.62)	(41.08)
% dismissed or acquitted	25.71	32.01	36.62	18.03	22.30
	(43.70)	(46.66)	(48.19)	(38.44)	(41.63)
% pleaded guilty	63.01	58.24	53.80	73.48	68.96
	(48.28)	(49.33)	(49.87)	(44.15)	(46.27)
plea sentence/plea conviction	18.32	31.73	27.98	18.17	14.66
in months	(59.79)	(83.27)	(88.01)	(72.29)	(59.31)
trial sentence/trial conviction	82.32	178.36	149.08	87.40	46.29
in months	(229.83)	(369.52)	(354.35)	(228.13)	(123.49)
Individual Characteristics					
age	29.99	28.32	29.68	29.56	30.137
	(10.30)	(10.31)	(11.25)	(9.54)	(10.05)
% black	36.30	41.97	37.55	34.07	34.11
	(48.09)	(49.36)	(48.43)	(47.40)	(47.41)
% hispanic	21.07	21.40	22.26	21.81	21.65
	(40.78)	(41.02)	(41.61)	(41.30)	(41.19)
% women	16.72	10.99	13.10	16.41	18.07
	(37.31)	(31.28)	(33.75)	(37.04)	(38.49)
prior felony convictions	1.07	0.86	0.84	1.05	1.03
	(1.91)	(1.63)	(1.60)	(1.85)	(1.76)
prior misdemeanor convictions	1.61	1.45	1.50	1.75	1.67
	(2.58)	(2.53)	(2.51)	(2.76)	(2.67)
public defender (%)	40.35	40.17	53.04	42.37	54.83
	(49.06)	(49.04)	(49.92)	(49.42)	(49.77)
private attorney (%)	13.12	14.74	12.26	14.11	13.17
	(33.77)	(35.46)	(32.81)	(34.82)	(33.81)
assigned attorney (%)	11.09	11.87	13.94	12.22	10.80
	(31.40)	(32.34)	(34.65)	(32.75)	(31.04)
# Observations/arrest	83506	2402	2381	7628	7937
# Observations/ trial	4482	187	198	211	341
# Observations/trial conviction	3567	144	146	161	267
# Observations/ plea conviction	52387	1395	1281	5578	5470

Standard errors in parentheses.
Only states that eventually adopted the TIS laws are included in the summary statistics for comparison between before and after TIS. To calculate the overall means of the variables, additional states that did not introduce TIS (Alabama, Indiana, Hawaii, Massachusetts, Maryland, Texas) are also included.

Table 3: Probit Estimates, Probability of Conviction Conditional on Arrest

	1	2	3	4
TIS case	-0.094***	-0.088***	-0.093***	-0.061***
	(0.010)	(0.010)	(0.010)	(0.009)
TISstate	0.042***		0.105***	
	(0.011)		(0.010)	
TISstate x violent		0.070***		0.108***
		(0.017)		(0.015)
offense x year dummies	Yes	Yes	Yes	Yes
county x violent dummies	Yes	Yes	No	No
state x offense dummies	No	No	Yes	Yes
# observations	83,506	83,506	83,437	83,437
pseudo R^2	0.153	0.153	0.140	0.139

* significant at 10%; ** significant at 5%; *** significant at 1%
Marginal effects on the probability and their standard errors (in parentheses) are reported.
All regressions include the individual characteristics of the offender and the case (age, sex, race, prior convictions, type of attorneys).

Table 4: Tobit Estimates, Imposed Sentence Conditional on Arrest (all cases)

	1	2	3	4
TIS case	-0.114***	-0.097***	-0.083***	-0.040
	(0.026)	(0.025)	(0.026)	(0.025)
TISstate	0.106***		0.185***	
	(0.028)		(0.032)	
TISstate x violent		0.172***		0.233***
		(0.058)		(0.061)
offense x year dummies	Yes	Yes	Yes	Yes
county x violent dummies	Yes	Yes	No	No
state x offense dummies	No	No	Yes	Yes
# observations	83,244	83,244	83,244	83,244
pseudo R^2	0.095	0.095	0.093	0.093

* significant at 10%; ** significant at 5%; *** significant at 1%
Marginal effects on the sentence and their standard erros (in parentheses) are reported.
All regressions include the individual characteristics of the offender and the case (age, sex, race, prior convictions, types of attorneys).

Table 5: Quantile Estimates, Imposed Sentence Conditional on Arrest

	1	2	3	4
TIS case	-0.111***	-0.0161***	-0.394***	-0.183***
	(0.000)	(0.000)	(0.045)	(0.046)
TISstate	0.421***		0.513***	
	(0.000)		(0.048)	
TISstate x violent		0.258***		0.215**
		(0.000)		(0.097)
offense x year dummies	Yes	Yes	Yes	Yes
county x violent dummies	Yes	Yes	Yes	Yes
quantile	75%	75%	90%	90%
# observations	83,244	83,244	83,244	83,244
pseudo R^2	0.236	0.236	0.194	0.193

Standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
The reported coefficients denote the marginal effects on the probability.
All regressions include the individual characteristics of the offender and the case (age, sex, race, prior convictions, types of attorneys).

Table 6: Probablity of Specific Case Outcomes and Length of Sentence upon Conviction

	1	2	3	4	5	6	7	8
Dependent Variable	Conviction at trial		Dismissed		Plea guilty		Sentence upon conviction	
	Probit		Probit		Probit		Tobit	
TIS case	-0.098**	-0.052	0.051***	0.036***	-0.095***	-0.110***	0.223***	0.260***
	(0.049)	(0.044)	(0.009)	(0.009)	(0.011)	(0.010)	(0.071)	(0.072)
TISstate	0.075**		-0.048***		0.005		0.025	
	(0.038)		(0.009)		(0.011)		(0.034)	
TISstate x violent		0.035		-0.044***		0.071***		-0.133*
		(0.051)		(0.015)		(0.018)		(0.079)
Plea							-0.379***	-0.379***
							(0.034)	(0.034)
Plea x TIS case							-0.053	-0.059
							(0.063)	(0.063)
county x violent dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
offense x year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# observations	4,363	4,363	83,506	83,506	83,506	83,506	55,954	55,954
pseudo R^2	0.185	0.184	0.166	0.166	0.129	0.129	0.112	0.112

* significant at 10%; ** significant at 5%; *** significant at 1%
Marginal effects and standard errors (in parenthesis) are reported.
All regressions include the individual characteristics of the offender and the case (age, sex, race, prior convictions, type of attorneys).

Table 7: Probit Estimates, Probability of Reducing Charges

	1	2	3	4
Dependent Variable	Misdemeanor		Felony with shorter sentence	
TIS case	-0.040***	-0.027***	-0.023***	-0.011
	(0.006)	(0.005)	(0.008)	(0.008)
TIS state	0.030***		0.018**	
	(0.007)		(0.009)	
TISstate x violent		0.011		-0.011
		(0.013)		(0.015)
offense x year dummies	Yes	Yes	Yes	Yes
county x violent dummies	Yes	Yes	Yes	Yes
# observations	83,245	83,245	36,851	36,851
pseudo R^2	0.194	0.194	0.118	0.118

* significant at 10%; ** significant at 5%; *** significant at 1%
Marginal effects on the probability and standard errors (in parentheses) are reported.
All regressions include the individual characteristics of the offender and the case (age, sex, race, prior convictions, type of attorneys).

Table 8: Offense-Specific Effects

Dependent Variable	Sample	Offense Categories				
		murder	other violent	property	drug	other
Probability of conviction	all	0.066	-0.050**	-0.136***	-0.070***	-0.145***
		(0.057)	(0.020)	(0.014)	(0.013)	(0.020)
Expected imposed sentence	all	0.138	-0.027	-0.153***	-0.100***	-0.144***
		(0.177)	(0.054)	(0.030)	(0.029)	(0.041)
Maximum sentence imposed	convicted	0.449*	-0.580**	0.041	-0.087	0.289*
		(0.241)	(0.262)	(0.129)	(0.155)	(0.164)
Probability of conviction	trial	0.114***	-0.114	-0.190**	-0.049	-0.031
		(0.042)	(0.074)	(0.080)	(0.065)	(0.082)
Probability of a guilty plea	all	-0.009	-0.023	-0.139***	-0.071***	-0.157***
		(0.067)	(0.021)	(0.014)	(0.013)	(0.020)
Probability of dismissed	all	-0.045	0.036*	0.065***	0.047***	0.060***
		(0.051)	(0.019)	(0.012)	(0.012)	(0.017)
Probability of reducing charges	all	-0.012	-0.043***	-0.029***	-0.047***	-0.028***
		(0.070)	(0.010)	(0.006)	(0.005)	(0.009)

* significant at 10%; ** significant at 5%; *** significant at 1%
Marginal effects and their standard errors (in parentheses) are reported.
All regressions include the individual characteristics of the offender and the case (age, sex, race, prior convictions, type of attorneys), offense - year dummies, county dummies interacted with violent crime dummies, and interaction term of TIS dummy and each crime category type.

Table 9: Robustness Checks

	1	2	3	4	5	6	7	8	9	10
	Dismissed cases excluded				Pre-trial covariates included				Non-violent crimes only	
	Length of Sentence/arrest		Conviction/arrest		Length of Sentence/arrest		Conviction/arrest		Sentence	Conviction
TIS case	-0.096***	-0.082***	-0.050***	-0.060***	-0.115***	-0.077***	-0.092***	-0.083***	-0.126***	-0.098***
	(0.033)	(0.032)	(0.007)	(0.007)	(0.025)	(0.024)	(0.011)	(0.010)	(0.025)	(0.011)
TISstate	0.081**		0.000		0.148***		0.048***		0.088***	0.029***
	(0.035)		(0.004)		(0.027)		(0.011)		(0.028)	(0.011)
TISstate x violent		0.129*		0.017***		0.173***		0.070***		
		(0.072)		(0.005)		(0.056)		(0.017)		
offense x year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
county x violent dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# observations	61,773	61,773	61,231	61,231	81,796	81,796	82,053	82,053	62,572	62,767

* significant at 10%; ** significant at 5%; *** significant at 1%
Marginal effects and standard errors (in parentheses) are reported.
All regressions include the individual characteristics of the offender and the case (age, sex, race, prior convictions, type of attorneys).

Table 10: Estimates of the TIS Effect Using the Predicted Fraction of the Sentence Served

Dependent Variable	Sample	Regression	Specification	
			TISstate	TISstate x violent
Probability of conviction	all cases	probit	-0.075***	-0.078***
			(0.021)	(0.021)
Expected imposed sentence	all cases	tobit	0.098	0.110*
			(0.060)	(0.060)
Expected imposed sentence	convicted cases	tobit	-0.297*	-0.298*
			(0.161)	(0.158)
Probability of conviction	trial cases	probit	-0.360***	-0.285***
			(0.090)	(0.083)
Probability of a guilty plea	all cases	probit	-0.003	-0.033
			(0.022)	(0.022)
Probability of dismissal	all cases	probit	-0.027	-0.036*
			(0.019)	(0.0190)
Probability of reducing charges	all cases	probit	-0.059***	-0.049***
to misdemeanor			(0.015)	(0.015)

* significant at 10%; ** significant at 5%; *** significant at 1%
The table reports marginal effect and standard errors (in parenthese) on the fraction of the predicted sentence served in regressions that are equivalent to regressions in Tables 3 through 7 except that the TIS case dummy is replaced with the fraction of the expected sentence served.
Specification "TISstate" denotes regressions controlling for the presence of the TIS law in the state (equation 3). Specification "TIS state x violent" denotes regressions controlling for an interaction of the TIS state dummy and a violent crime dummy (equation 4).
All regressions include the individual characteristics of the offender and the case (age, sex, race, prior convictions, type of attorneys), offense-year dummies, and county dummies interacted with violent crime dummies.