How Much do Mandatory Minimums Matter?*

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Abstract

I estimate the causal increase to sentence lengths created by mandatory minimum (MM) eligibility for federal drug crimes. I utilize a regression discontinuity design with extrapolation to decompose the impacts of MM eligibility into effects driven by the law and effects driven by legal actor decisions. I find that without manipulation, MM eligibility increases sentence length uniformly across case types by about 10 months (18%). However, cases that are manipulated to a higher MM eligibility receive an additional 7 to 11-month increase. I find that manipulation exclusively impacts minority defendants and is at least in part driven by prosecutor discretion. These results suggest that race gaps in MM sentencing are driven by legal actor decision making rather than a systemic aspect of the law.

The federal prison system is overcrowded and extremely costly. The Federal Bureau of Prisons estimated federal prisons operated at 10% overcapacity in 2024, with the average cost of incarceration estimated to be $43,836 per inmate, per year (2023 United States Government Publishing Office). This large number of inmates is primarily comprised of drug offenders, who make up around 45% of all federal prisoners. Drug offenders bring especially high costs due to their long incarceration spells; in 2019, federal drug offenders had sentence lengths 252% higher than non-drug defendants. This was not always the case; both the volume of drug convictions and attached sentence lengths have dramatically increased since the 1980s and 90s (The Pew Charitable Trusts 2015). The cause of this increase is often attributed to legislation that increased punishment schedules for drug offenders. The most commonly cited and controversial aspect of such legislation is mandatory minimum (MM) sentencing. Yet, despite its prominence and controversy in drug reform debate, very little research has been done to estimate the causal impact MM eligibility has on sentence lengths.

Mandatory minimum sentencing restricts a judge's ability to give a sentence length below a specified amount. In practice, over 97% of all trafficking cases are resolved by plea bargain but MM sentencing is still expected to increase sentence lengths by increasing prosecutor bargaining power. MM sentencing applies to several crime types in the federal system, including weapon crimes, sex crimes, certain economic crimes, and most commonly, drug crimes. Federal drug trafficking offenses may be eligible for a 5-year or 10-year MM sentence depending on the drug type and the charged quantity of drugs. Many cases are affected by this practice; in 2019 when looking at the five most common drug types, over 86% of cases were eligible for a MM sentence, with over 69% of cases meeting the threshold for a 10-year MM charge.

Mandatory minimum laws also raise equity concerns. One of the most common complaints over MM legislation is that it is believed to disproportionately impact racial minority defendants. These concerns are supported in empirical research; Rehavi and Starr (2014) attributes significant disparities in sentence lengths to MM charges and Tuttle (2023) finds Blacks are disproportionately targeted for MM sentencing in crack cases. I add to these findings by considering whether eligibility disproportionately increases sentence lengths for minorities.

The primary challenge in estimating the causal impact of MM laws on sentence length comes from selection issues. For most cases, MM eligibility is determined by the drug quantity listed at sentencing, measured by weight. If the weight at sentencing is greater than some threshold value, an individual is eligible to be charged with a 5 or 10-year mandatory minimum. However, this quantity does not need to equal the weight recorded at arrest. Essentially, this means that cases near the MM threshold may have their charged quantity manipulated in such a way that their eligibility is determined in part by legal actors. Tuttle (2023) notes the presence of significant bunching in the number of crack cases right above the 10-year MM threshold weight, evidence of this manipulation. This means that certain cases are systematically moved from 5-year MM eligibility to 10-year MM eligibility, thereby distorting the relationship between charged weight and sentence length.

In this paper, I estimate the causal impact of MM eligibility on federal drug trafficking sentence lengths. I specifically consider the effects of 10-year MM eligibility and its impacts on race disparities in sentencing. Because cases are subject to manipulation, I decompose MM eligibility impacts into effects driven by the law and effects driven by legal actor discretion. This is done by separately fitting the distribution of sentence length over drug weight

using data only in non-manipulated regions on either side of the 10-year eligibility cutoff. I then extrapolate the fitted distributions into the manipulated regions to create sentence length measures under the counterfactual scenario where no manipulation occurs. The discontinuity between these counterfactual distributions at the threshold weight is what I call the legal effect. It is the impact of MM laws for cases without manipulation. The distance between the actual sentence length and the higher counterfactual distribution (the one fit from the right-side of the distribution) is what I call the manipulation effect. It is a combination of two separate effects: (1) selection bias created by manipulation choices, and (2) the impact of being pushed from the 5-year eligibility into 10-year eligibility separate from the legal effect. That is, cases at the bunching point may have high sentence lengths because these cases are fundamentally different than non-manipulated cases, but also because the process of manipulation causes an increase in of itself. I provide evidence that manipulation effects are at least in part driven by non-selection factors at the higher eligibility level.

I find that, across the five most common drug types, 10-year MM eligibility increases sentence lengths by about 10 months (or 18% above the mean) purely through the legal effect. I find that the manipulation effect for the average case is around 17 months. Further, the manipulation effect persists with the inclusion of controls, though the magnitude shrinks to around 7 months in the full-controls specification. This indicates that manipulation occurs most commonly for high-sentence defendants, but also that the manipulation process has a causal impact on increasing sentence lengths outside of selection bias. Taken together, these findings suggest that on average, all cases see an increase to their sentence length if eligible for the 10-year mandatory minimum, but for cases that are manipulated up to the 10-year eligibility the increase is significantly higher.

Perhaps the most interesting finding in this analysis is that racial disparities in MM sentencing are almost entirely driven by manipulation. I find that the legal effect is nearly identical across race groups and is fairly similar across drug type and criminal history level. However, the manipulation effect is entirely driven by racial minorities, with effects mostly local to cocaine and heroin cases. This means that the legal impact of MM eligibility has similar effects for most cases, but Black and Hispanic defendants with weights near the cutoff receive significantly higher sentences due to legal actor discretion. Manipulation effects are also present for low criminal history offenders, suggesting that policies such as the safety valve provision may not adequately shield low level offenders from MM effects.

This paper contributes to literature on the welfare impacts of sentencing structure, legal actor discretion, and how each of these may drive racial disparities. Many papers suggest legal actors use discretion to disproportionately target or punish racial minorities with worse court outcomes (Arnold et al. 2018; Rehavi and Starr 2014; Sloan 2022; Tuttle 2023; Yang 2016), though recent literature has suggested prosecutor discretion may actually limit racial disparities, specifically in connection with enforcement decisions (Shaffer and Harrington 2017; Shaffer 2023). Findings from this paper support previous results; that race disparities are largely driven by discretion and are likely tied to prosecutor choices. On a smaller scale, this paper also adds to the drug crime, MM sentencing literature. Findings among these studies vary, with some papers downplaying the significance of mandatory minimums (Bjerk 2005; Bjerk 2017a; Bjerk 2017b; Fischman and Schanzenbach 2012) and others finding significant impacts on sentencing generally and in contributing to racial disparities (Didwania 2020; Didwania 2025; Rehavi and Starr 2014; Tuttle 2023). This paper uses discontinuity techniques similar to Diamond and Persson (2017), leniency measures similar to Goncalves

and Mello (2021), and exploits the same bunching point as seen in Tuttle (2023).

Bjerk (2017b) is the paper most closely related to this work. It provides a descriptive analysis of the sentence length impacts of being convicted at particular charging weights for fiscal years 2011 and 2012. The main analysis shows that MM eligibility rates are similar across drug types, the safety valve provision effectively prevents low-level offenders from receiving large sentence length increases, and that effects for crack are minimal.[1] I contribute to this paper in four ways: (1) by identifying the causal impact of MM laws on sentence length, (2) by disentangling causal effects into legal and manipulation effects, (3) by assessing the degree to which low criminal history defendants are protected from MM effects, and (4) by giving a more comprehensive view of MM impacts on racial and drug type disparities. To my knowledge, this is the first paper to causally estimate the impacts of MM eligibility on sentence length and the first to decompose MM race gaps by systemic and discretionary channels.

II. Background

II.A. Mandatory Minimum Sentencing of Federal Drug Cases

The main criteria for MM eligibility is drug quantity.[2] In order for the mandatory minimum to apply, the weight at sentencing for one drug type must meet or exceed the set

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[1]It's worth noting that in a brief appendix analysis, Tuttle (2023) finds that crack does indeed exhibit some sentence length effects if other years are included.

[2]A small subset of cases are eligible for MM penalty without meeting quantity thresholds. This can occur in two ways: if the crime involves death or serious injury, or if the defendant has committed a serious prior drug offense. Including, excluding, or controlling for this subset does not substantially change results or significance of findings.

threshold weight. Weights cannot be added across drug types in regard to MM eligibility, meaning there must be a large quantity of at least one drug type. There are two separate thresholds, one for a 5-year and one for a 10-year mandatory minimum. In this paper, all analysis considers only the higher threshold, which has stronger bunching, more severe punishment increases for eligible cases, and for which 70% of all federal trafficking cases are eligible.

Cases with drug quantities at or above the MM threshold weight may not necessarily be charged with a mandatory minimum. But being charged at an eligible weight opens the possibility for the prosecutor to impose a MM charge, meaning hitting the threshold weight significantly increases prosecutor bargaining power. Some cases have initial charges that apply the MM filing, but through plea negotiations, do not carry a mandatory minimum in the final charges. In my data, I only observe final charges. For this reason, I do not focus on the MM charges themselves but only consider drug quantity and the sentence length. This ensures I do not leave out cases that do not show MM charges in the final charge data, but were still substantially impacted by MM eligibility during the plea bargaining process. Furthermore, MM sentences are often non-binding, with many convictions receiving sentences above or below the minimum specified sentence length. Sentences below the mandatory minimum can occur if a defendant provides "substantial assistance" in the prosecution or investigation of a another offender,[3] if the the offender is eligible for the safety valve provision, or if a lower sentence is negotiated through plea bargain. Defense can apply for the safety valve provision if the defendant in question has a sufficiently low criminal history record. I directly test for MM effects for cases filed under the safety valve provision.

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[3]See USSG § 5K1.1

II.B.   Drug Weight Manipulation

Discrepancies between seized, charged, and sentencing drug weights can technically occur for several reasons. However, the primary source of drug weight manipulation occurs due to changes in relevant evidence. This may come by way of additional testimony or connecting a defendant to other traffickers or cases (Lynch 2016). The impetus for this additional evidence comes from prosecutors, who can decide whether to pursue additional evidence in building a case. If prosecutors can connect a defendant to other offenders or larger organizations, they may increase the available evidence of drugs to charge a person with.

One indication of this evidentiary channel is in conspiracy charges. Qualifications for a conspiracy charge are broad, with drug conspiracy generally defined as two or more individuals agreeing to transport, manufacture, or sell illegal substances. Thus, if prosecutors seek to connect defendants to other offenders or an organization, they are more likely to charge them with conspiracy. This appears true in my data; I find that 54% of cases that are not at the bunching point carry a conspiracy charge. However, at the MM threshold weight, 83% carry a conspiracy charge. It's also worth noting that conspiracy charge rates are similar across race, with White defendants having slightly more conspiracy charges than non-White defendants.

III.   Data

The primary drug case data is provided by the United States Sentencing Commission (USSC) and includes all federal drug trafficking cases from 2010 to 2021. Data is at the case-individual level. I restrict the data to the five most prevalent substances subject to MM

sentencing: powder cocaine, crack, heroin, methamphetamine, and marijuana.[4] I also restrict the data to the primary racial groups of study: White, Black, and Hispanic. For the intensive margin analysis, the data is further restricted to include only cases with primary drug weights at 20% to 180% of the threshold weight. This gives enough data to fit distributions on but excludes the 5-year threshold and extremely high weight cases that are less similar to cases near the 10-year threshold. This gives a total of 44,626 observations.

The USSC data provides a rich set of defendant and litigation details. Information on the defendant's sex, race, age, education, and citizenship is included. The data also contains specific statutes that are charged, the drug weight given in grams or an approximation of the weight, any factors that increased or decreased the sentence length above or below the prescribed guideline amount, whether the case was plead or not, and sentence length the defendant received.

Each of the five drug types have a sizable number of cases, with heroin cases making up the fewest percent of cases at 14.3% and cocaine making up the most with 26.1%. Drug type is highly correlated with race, suggesting primary specifications for race heterogeneity should control for the primary charging drug. The table also shows that Black and White defendants have very similar criminal history points, while Hispanic defendants have much lower criminal history on average. I also consider conspiracy charges as these are a primary mechanism by which drug weight manipulation occurs (Lynch 2016, Tuttle 2023, Cooper 2023). Across all three racial groups, the proportion of cases with a conspiracy charge is nearly identical. This helps alleviate concerns of unequal opportunity for manipulation across

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[4]The other three substances subject to MM law are PCP, LSD, and fentanyl. These had too few observations for any meaningful analysis.

races. These and other summary statistics are shown in Online Appendix Table A.1.

Additional data are used for robustness checks and a supplementary analysis in the Online Appendix. These include the National Incident-Based Reporting System (NIBRS) from the FBI's Uniform Crime Reporting program and a hand-collected data set of US Attorneys. Details of these data and their use are contained in the online appendix.

IV. Empirical Strategy

To accurately estimate the causal effects of MM sentencing, I need to know what the discontinuity looks like without any manipulation. Thus, my strategy is to create counterfactual distributions of sentence lengths over charged drug weight by estimating fits over the distribution of unmanipulated regions. These fits are then extrapolated into the regions near the eligibility cutoff where manipulation is present. This technique is a regression discontinuity design using extrapolation, often referred to as a "donut RD". I fit two separate distributions on either side of the cutoff. The fitted distributions are determined only by cases that are assumed to be unmanipulated. I then look at the extrapolated points right at the threshold weight. The distance between these two points gives the legal effect - the causal effect of MM law in absence of manipulation activity.

I also consider how these fitted distributions compare against the actual sentence lengths. Specifically, I compare the point extrapolated from the right-hand side at the threshold weight against the actual sentence length at the threshold weight. Essentially, I am comparing the counterfactual sentence length at the threshold weight versus the observed sentence length at this point. The distance between these two points gives the manipulation effect, which is comprised of bias and non-bias caused increases. Selection bias occurs because pros-

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ecutors do not randomly choose who they manipulate. But increases also may occur beyond selection effects - recall that the primary manipulation mechanism is additional evidence gathering by the prosecution. Evidence of more drugs and conspiracy connections are likely to increase sentence lengths beyond the effects of a higher weight. If the manipulation effect survives the inclusion of controls, it suggests the manipulation effect is not purely driven by bias, but is also impacted by these evidentiary effects.
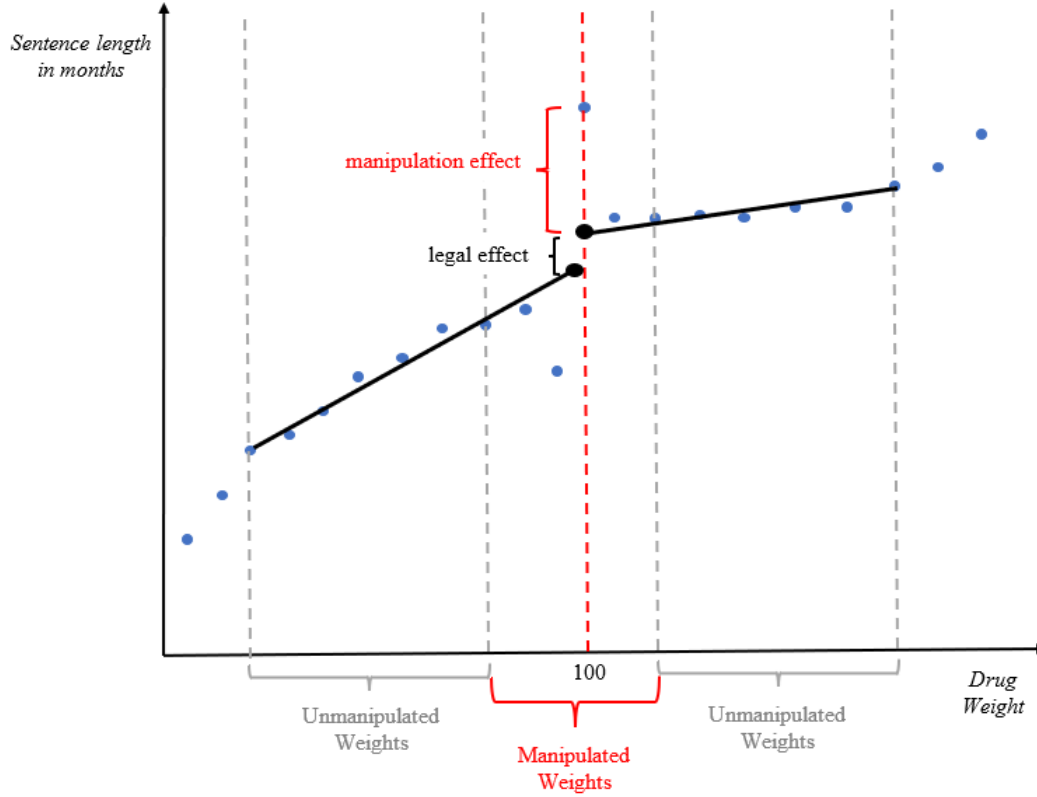
Finally, I look for a discontinuous drop in sentence length for cases just before the cutoff. If prosecutors manipulate cases that are already likely to have higher sentences, the "most severe cases," then the average sentence where they are manipulated from will be lower than the rest of the trend. Note that this is analogous to a missing mass argument. This missing sentence amount is not formally estimated since the number of cases varies drastically at the bunching point compared to at other charging weights.[5] However, I note its presence as evidence that prosecutors are indeed manipulating cases upward to increase sentence lengths for certain defendants.

Figure 1 illustrates the idea of this strategy. The blue points represent the actual sentence length distribution for the group being considered. Dashed vertical lines show the manipulated and unmanipulated regions. The black lines give the fitted distributions which are extrapolated into manipulation regions. The large black points give the discontinuity absent manipulation. The distance between the top point and actual sentence length at the threshold gives the manipulation effect. I do not label the missing sentence amount since it

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[5]Ideally, I would multiply the missing sentence amounts by the missing mass in cases and compare this value against the product of the manipulation effect and the excess mass at the threshold weight. I refrain from doing this since missing masses in cases left of the threshold weight cannot be precisely estimated.

is not estimated, but it can be observed as the distance between the fitted sentence just to the left of the threshold and the actual sentence lengths near this weight.

Figure 1: Empirical strategy illustration

Notes: This figure depicts the empirical strategy used to estimate the MM impacts and decompose them into legal and manipulation effects. The black lines represent the fit over the unmanipulated weights and extrapolated into the manipulated weights. The discontinuity between these fits at the threshold is the legal effect. The gap between the predicted sentence from the right-hand side of the distribution and the actual sentence length is the manipulation effect. Note that the legal effect is interpreted as causal while the manipulation effect is likely biased upward in its raw form.

To implement this empirical strategy, I must first determine which regions are manipulated and which are not. There are two primary ways in which manipulation regions have been detected and determined in past literature. The first is a formal test developed in Frandsen (2017) and practiced in Goncalves and Mello (2021) used to detect changes in the distribution attributed to manipulation. This method is unlikely to work well in this setting because small scale manipulation likely occurs as prosecutors or law enforcement round to

whole numbers. The second method is simply using visual inspection, as done in Saez (2010), Chetty et al. (2011), and Kleven and Waseem (2013). This approach works when there is a clear and obvious missing mass in the distribution that is supplying the observations at the bunching point. Where the missing mass begins is assumed to be the beginning of the manipulation region. To tease out the missing mass area, I fit a fifth order polynomial with fixed effects for each 10-percentage point round figure over the main analysis weights: 20% to 180% of the threshold weight. Online Appendix Figure A.1 shows this fit distribution in comparison to the actual case density. Missing mass appears following the 70% value and continues up until cases just before the MM threshold at 100%.

One common way to check whether the manipulation region is correctly specified is to compare the excess and missing mass amounts, which should be equal. The excess mass at the bunching point is just under 2,385 cases. The missing mass from 70% to 99% is about 1,129. The smaller missing mass may be generated by two sources. First, when looking at each drug individually, meth has a manipulation range that is clearly wider than the other drug types (closer to 60%). Second, the missing mass estimation does not include cases at 70%, 80%, or 90%. These round numbers are omitted as rounding bunching occurs at these points as well. Significant case reduction may occur from cases at the 80 percentage point cases.

Based on the comparison between the fit and actual case distribution, I consider cases between 70% and 105% to be within manipulation ranges for my main analysis. This means the left-side regression is fit using cases between 20% and 70% of the threshold weight, while the right-side regression is fit using cases between 105% and 180%. Each of these fit predictions are then extrapolated into the manipulation region to create counterfactual distributions, or

the trend of sentence length absent manipulation. In my robustness checks I consider the results under many specifications with different cutoffs for the left-side manipulation region. There is a tradeoff in setting the manipulation region cutoff; cutoffs further from the threshold are less likely to be biased since they are less likely to accidentally include cases that are manipulated, while windows closer to the threshold have more data to fit on and are thus likely to be more precise. For this reason, it is important to show that results are similar across many different set cutoffs. This is analogous to testing a regression discontinuity with different bandwidth sizes.

Following Gelman and Imbens (2019), my analysis assumes a functional form that is linear, though I include some quadratic fit predictions in my robustness checks. I use standard errors of the prediction to create confidence intervals for each fitted value to assess inference. The legal effects are considered statistically significant if the two confidence intervals do not overlap. The manipulation effect is considered statistically significant if the actual sentence length falls outside of the confidence interval from the right-side fitted regression.

The key identifying assumption of this empirical design is that without manipulation, the distribution of sentence lengths for cases in manipulation regions would have followed the counterfactual distributions fit using unmanipulated cases. Note that this assumption is standard in a traditional bunching design, only there it is typically involving the variable being bunched, which in this context would be cases. Now I assume that the relationship between sentence length and charging weight can be predicted using the polynomial coefficients among unmanipulated cases.

However, the main methodological concern is that prosecutors may manipulate cases from lower weight amounts (from 0%-50% of the threshold weight) up to the bunching point.

I argue such manipulation is unlikely for three reasons: first, drug weight manipulation primarily occurs from additional fact finding on the part of the prosecutor (Lynch, 2016; Tuttle, 2023). From the Online Appendix D of Tuttle (2022), costs to evidence gathering are assumed to increase as the manipulation quantity increases. Thus, if the distribution of available evidence used to manipulate weights is relatively uniform across seized drug weights, the probability of manipulation should be highest for cases just below the threshold weight. This manipulation probability will decrease for lower seized weights, indicating manipulation is more likely to occur for cases near the threshold. Two, Cooper (2023) assesses decreases bunching caused by a policy and finds increased mass appear primarily for cases near the bunching point. This indicates these marginal cases were mostly manipulated near the threshold weight. Three, missing sentence lengths appear at weights just below the threshold. In addition to these points, I also check whether the legal effect holds in districts with very low bunching. These can serve as an additional sort of counterfactual in the scenario where manipulation is far less prevalent.
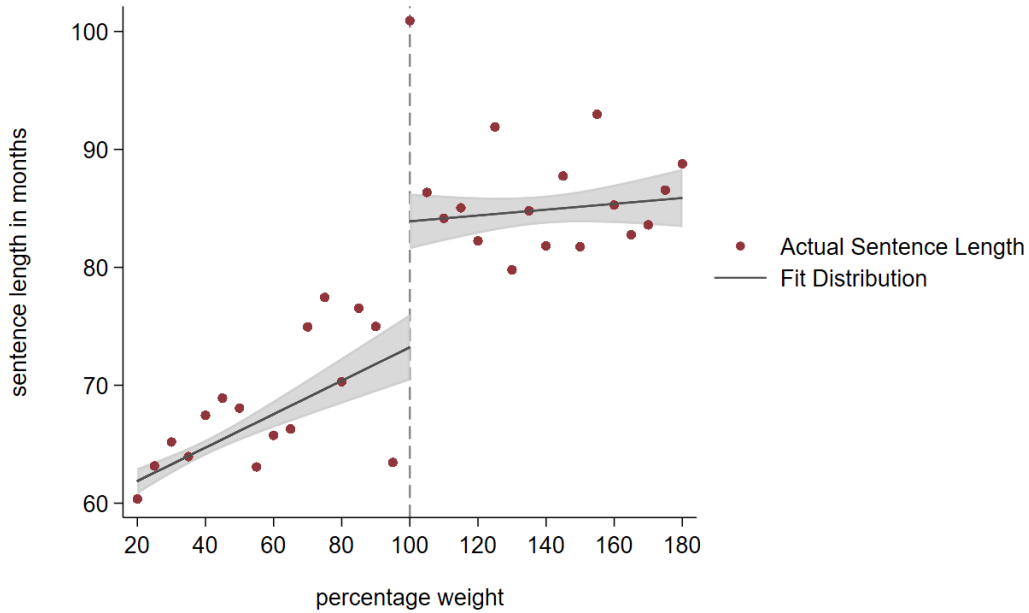
V. Results

Overall Effects

Figure 2 shows the discontinuity aggregated across all cases. The figure highlights four important findings. First, the fit values approximate sentence lengths quite closely within the manipulation range except for cases closest to the threshold weight. Second, the legal effect of mandatory minimums is significant, with an increase of 10.63 months. This is an 18.04% increase over the mean at 99% threshold weight. Third, the figure shows a large manipulation effect with the actual sentence length at the bunching point far higher than

the fit value. I estimate a difference of 17.01 months between these points. And fourth, the missing sentence appears to be local to cases just left of the threshold weight. The presence of missing sentence lengths gives evidence that prosecutors are manipulating cases up to the threshold weight.

Figure 2: Discontinuity analysis for all cases

Notes: This figure gives the main discontinuity analysis for all cases with linear fits through points before 70% of the threshold weight and extrapolated past 70% to the threshold from either side. Sentence lengths are binned by 5% except at the threshold weight, which only reports average sentences for cases charged with exactly the threshold weight.

I also consider these results with three different control schemes. Baseline controls include criminal history points, drug type, racial group, the defendant's age, their education, and whether they are female. The fixed effects are at the district and year level. The prosecutor decision controls include the a binary for gun use, the number of drug types an individual is charged with, and whether the case went to trial. It's worth noting these prosecutor controls may be decisions made in conjunction with the manipulation decision. Thus controlling for them may reduce effect sizes lower than the true causal effect. Still, I include the specification
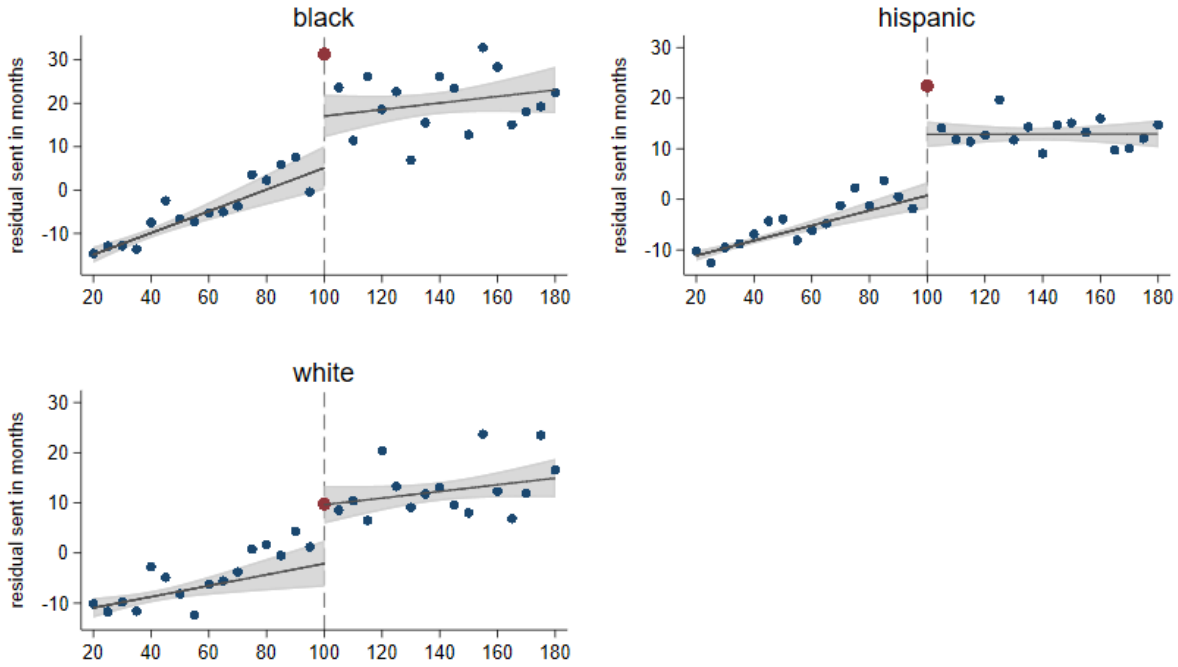
with these prosecutor decisions to try and reduce any possible bias from the manipulation effect.

The size of the manipulation effect decreases to 11.24 when controlling for baseline and fixed effects, and to 7.05 when including prosecutor decisions. This means that even with the full set of controls, there is still a sizable sentence premium that cannot be accounted for by the included observables. If this estimate were to be interpreted as the manipulation effect free from selection, it would imply that cases that are manipulated to the 10-year mandatory minimum threshold receive a causal increase to their sentence length of 16.70 months (this is the legal + manipulation effects after controls). Statistical significance persists through each specification. These results are shown in Online Appendix Table A.2, with robustness to other fit values shown in Online Appendix Table A.3

Racial Disparities

Figure 3 illustrates the discontinuity analysis separated by race with sentence lengths residualized on the baseline controls, which in this case are criminal history points, drug type, defendant sex, education, age, and whether they are an illegal alien. These regression results are also reported in Table 1. There are two key results presented here. First, the legal effect is nearly identical across each race group. It's worth noting that these are level effects and if considered by percentage of mean sentence left of the cutoff, the effects vary more, with Hispanic and White defendants experiencing a significantly larger impact from the legal effect compared to Black defendants. However, the results still imply that, absent any manipulation, the causal impact of mandatory minimum eligibility is effectively equal across race in terms of level sentence lengths.

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Figure 3: Race comparison - discontinuity using residualized sentence lengths

Notes: This figure gives the residualized sentence discontinuity analysis by racial group with linear fits through points before 70% of the threshold weight and extrapolated past 70% to the threshold from either side. Residual sentence lengths are binned by 5% except at the threshold weight, which only reports average sentences for cases charged with exactly the threshold weight.

The second key finding is that the manipulation effects are driven exclusively by racial minority defendant cases. Black defendants at the weight threshold experience sentence lengths 14.28 months higher than cases just to the right of the threshold, even after controlling for criminal history and drug type. Similarly, Hispanic cases have a manipulation effect of 9.59 months in this specification. But White cases have a manipulation effect of essentially zero. These effects shrink under the full control scheme, with manipulation effects of 6.27 months and 4.73 months for Black and Hispanic defendants, respectively. This provides evidence that prosecutors choose to manipulate cases more frequently for minority cases than White ones, corroborating findings from Tuttle (2023). These results also indicate that minority defendants who are manipulated receive a sentence premium, meaning the causal impact of

mandatory minimum eligibility is higher for minority cases than White ones in the presence of manipulation.

Table 1: MM effects by race

	(1) black	(2) hispanic	(3) white
legal effect	11.87	12.03	11.71
left side 95% CI	[-0.04, 9.94]	[-1.91, 3.25]	[-6.74, 2.36]
right side 95% CI	[12.28, 21.96]	[10.32, 15.38]	[5.98, 13.38]
manip. effect	14.28	9.59	0.11
fit value	70%	70%	70%
N of obs fit on	11205	15296	7190

Notes: The dependant variable is sentence length residualized against criminal history points, primary drug type, defendat education, age, sex, and whether they are an illegal immigrant . All specifications are discontinuities based on local linear fits. 95% confidence intervals are presented for each fit regression on either side of the cutoff. These are calculated using the standard error of the predicted expected value, and significance is determined as no overlaps between these two intervals. Fit value represents the cutoff for where extrapolation begins. In this case, all specifications have left-hand regressions fit on cases with weights between 20% and 70% of the threshold weight.

The results can also be considered at each drug-race intersection. However, these samples become smaller and noisier, leading to less precise estimates. The overall story is the same though - legal effects are similar across groups, though estimates tend to be strongest for whichever race has the most cases for a drug type. In extreme cases, one or two race groups make up almost all of the observations within a drug type, such as Black defendants for crack cases. In these settings, legal effects are only present for the high case group. Manipulation effects are driven primarily by racial minorities in cocaine and heroin cases, though some smaller effects exist for other minority race-drug combinations. These results still highlight that the law effects defendants similarly, but manipulation decisions only penalize Black and Hispanic defendants.

Drug Type and Criminal History

I now consider heterogeneity across drug type. The legal effect is estimated to be between 9 months and 14 months for each drug type besides for meth, which has a large legal effect of 20 months. The manipulation effects are driven almost entirely by only two drug types: cocaine and heroin. It's noteworthy that crack cases do not exhibit bunching, a result that corroborates findings from Bjerk (2017b). This may come from the fact that overall sentence lengths are much higher for crack cases than any other drug type. So while mandatory minimums seem to have less impact on crack compared to other drugs, all crack cases are still punished at higher levels. Estimates are displayed in the Online Appendix in Figure A.3 and Table A.4.

To measure low-level offender effects, I consider effects for two subgroups: defendants who have had no criminal history contact of any sort, and those who have had contact but have 0 or 1 criminal point, which relates to crimes with sentences less than 60 days. Because there are policies designed to protect first-time and low-level offenders from MM sentences, the expected effect of MM eligibility may be low for these two low history groups. Bjerk (2017b) reports this expected result of almost no effects for low level defendants.

I find that individuals with low criminal histories are still subject to both legal and manipulation effects. Online Appendix Figure A.4 shows the regression discontinuity graphs for these two criminal history groups. Individuals with no prior criminal justice interactions experience a legal effect increase of 6.721 months and a manipulation effect of 11.49 months. Defendants with 0 or 1 points experience a legal effect of 10.18 months and a manipulation effect of 10.03 months. Both groups exhibit a relatively small missing sentence amount, suggesting that selection may not be a primary driver of the manipulation effect. These results suggest that legal protections do not fully shield low history offenders from higher

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sentences, nor do they protect them from effects driven by case manipulation in non-safety valve cases. See Online Appendix Table A.5.
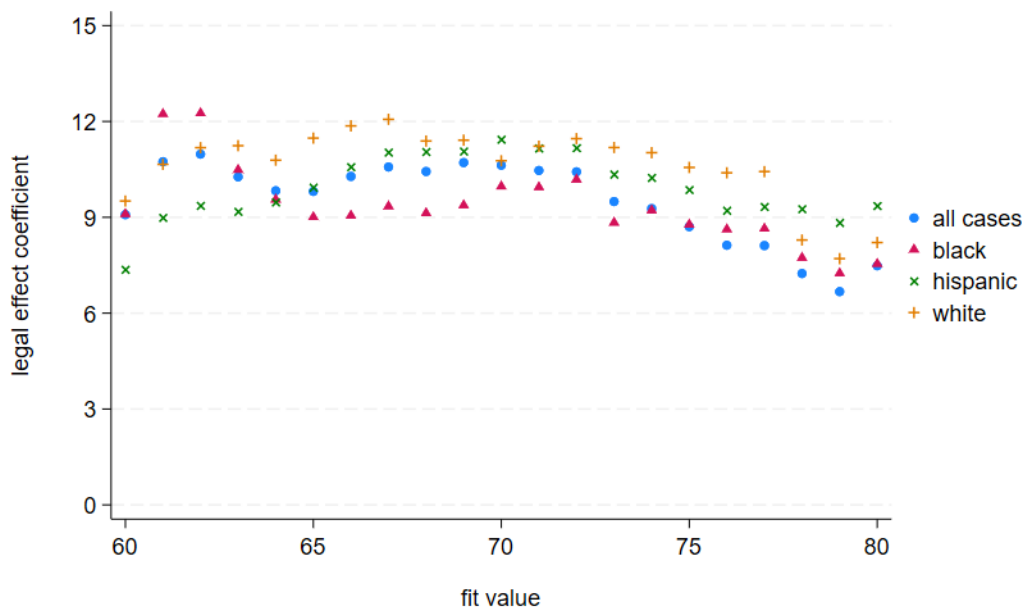
V.A. Robustness

One of the primary concerns for the main results is that using the 70% cutoff for fitting over non-manipulated cases either creates bias (if 70% is too low) or doesn't capture all the variation in the data well (if 70% is too high). To check that results and magnitudes are not driven by one specific fit, I repeat the analyses for the overall results and the heterogeneous results using a variety of different fit cutoffs. For each group, I run the main regression discontinuity again for every cutoff between weights 60% and 90% of the threshold for a total of 30 additional specifications (in addition to the 70% analysis).

The heterogeneous effects presented in the main results are strongly consistent across different fit values. Groups with noisier data, like crack cases, tend to have higher variability of results by fit. Even still, these results tell a similar story. I present the legal effect coefficients across fit by race and for all cases in Figure 4. This graph shows the result for all 21 regressions for each group. The legal effect is consistent across these specifications, suggesting results are not driven by specific cutoff choices.

I also show that the legal effects are legitimate by comparing them against districts with low bunching levels. Districts without bunching represent jurisdictions with low or without any manipulation. I compare the main results against a series of district subsets with low bunching amounts. I find the legal effect in these subsets is consistent with the main results. These are presented in Online Appendix Table A.6.

I also check that the main results hold using a quadratic fit. For this check, I only use a

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Figure 4: Legal effect robustness - all cases and by race

Notes: Each point gives the magnitude of the legal effect regression under a specific fit value. A fit value designates which area is considered manipulation region and which is considered unmanipulated. For example, at a fit value of 65, the regression is fit using all cases with weights between 20% and 65% of the threshold weight. The regression is extrapolated from 65% up to the 10-year MM cutoff, where it is compared against the right-hand regression to estimate the 65% legal effect coefficient.

cutoff value for the manipulation region of 80% of the threshold weight. I use a higher cutoff because at 70%, the quadratic polynomial tends to overfit the data for certain groups. Thus, to assure a reasonable fit I increase the amount of data to fit on and decrease the amount of extrapolation needed. The quadratic fits do not largely change the results, only slightly increasing or decreasing effects.

In the main analysis, I control for the number of other drug types a defendant is charged with outside of the primary drug type. Readers may be concerned that the quantity of other drugs is driving results, especially if other drug type quantities is correlated with certain racial groups. I consider the legal and manipulation effects for the full sample and across racial groups again, now controlling for the quantity of other drug types rather than just the

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number. These results are presented in Online Appendix Table A.7. I find effect sizes and statistical significance very close to those listed above, though with smaller manipulation effects.

Another concern may be that limiting the sample to cases with precise weights biases results if imprecise measures are strongly correlated with sentence length. I now rerun the full sample analysis including cases that are charged with a range of weights rather than one precise count. I use three different measures to do this - the minimum, median, and maximum of the range. These range cases are combined with the precise weight cases for a combined samples of 51,975 cases when using the maximum or median, and 56,189 when using the minimum measure. Regression results for this sample are presented in Online Appendix Table A.8. Across all three measures, the legal and manipulation effects have similar magnitudes to main results and remain statistically significant.

VI. Conclusion

In this paper, I show that mandatory minimum eligibility for federal drug cases affects sentence length in two ways: through a legal effect and a manipulation effect. The main analysis shows that without manipulation, MM eligibility still leads to significant increases in sentence length. The average legal effect of eligibility is a 10.63 month increase in sentence length, which is an 18% increase over the counterfactual mean. This effect is consistent across racial groups. However, for a subset of specific types of cases that are close to the 10-year eligibility threshold weight, manipulation effects can lead to even larger increases in sentence length. When controlling for criminal history, drug type and defendant charac-teristics, I find that Black defendants receive an additional 14.28 months to their sentence

due to manipulation. Similarly, Hispanic cases receive an additional 9.59 months from manipulation. Thus, racial disparities in sentence lengths caused by mandatory minimums are driven almost entirely by manipulation of legal actors. Manipulation effects shrink when including prosecutor decision controls, but remain statistically significant. This indicates that at least part of the manipulation effect can be interpreted as increases from additional evidence against the defendant.

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