The Safety Valve Lottery: Judge Discretion, the First Step Act, and Racial Equity in Federal Drug Sentencing

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Abstract

Federal mandatory minimum sentences bind mechanically—but the statutory safety valve lets judges sentence below the floor when defendants meet eligibility criteria. The First Step Act of 2018 expanded safety valve eligibility from defendants with 0–1 criminal history points to those with up to 4 points, newly covering roughly 1,400 drug trafficking defendants annually. Using USSC individual sentencing data (FY2016–FY2024) and a difference-in-differences design comparing newly eligible to already-eligible defendants, I estimate the effect on sentence length, safety valve utilization, and racial disparities. The FSA expansion increased safety valve usage among newly eligible defendants and reduced their sentences. I examine whether these reductions were uniform across race and across districts with differing pre-reform judicial cultures. The *Pulsifer v. United States* (2024) decision, which narrowed eligibility, provides a natural validity check.

JEL Codes: K14, K42, J15

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1. Introduction

Mandatory minimum sentences are the most debated feature of the American criminal justice system. With over 1.8 million people incarcerated in the United States, the sentences that federal judges impose—and the constraints that limit their discretion—have consequences that cascade through families, labor markets, and communities for decades. The First Step Act (FSA) of 2018, one of the rare bipartisan criminal justice reforms in a generation, expanded the statutory "safety valve" that permits judges to sentence below mandatory minimums for certain drug trafficking defendants. But expanding judicial discretion creates a fundamental tension: if judges use the new latitude to correct unfairly harsh mandatories, racial sentencing gaps should narrow; if judicial bias compounds with expanded discretion, those gaps could widen. This paper asks which force dominates.

The federal safety valve, codified at 18 U.S.C. §3553(f), was originally enacted in 1994 to provide relief for low-level, nonviolent drug offenders facing mandatory minimums. Before the FSA, eligibility required a defendant to have no more than 1 criminal history point—effectively limiting relief to first-time offenders. The FSA expanded eligibility to defendants with up to 4 criminal history points, a seemingly modest statutory change that brought roughly 1,400 additional drug trafficking defendants per year into the zone of judicial discretion. This expansion provides the identifying variation for my analysis.

I exploit this expansion using a difference-in-differences design. Newly eligible defendants—those with 2–4 criminal history points who gained safety valve eligibility only after December 2018—constitute the treatment group. Already-eligible defendants with 0–1 points, who could access the safety valve both before and after the FSA, serve as the comparison group. The key identifying assumption is that, absent the eligibility expansion, sentencing trends for these two groups would have evolved in parallel. I provide event-study evidence supporting this assumption and discuss potential threats at length.

The central finding is that the FSA's safety valve expansion meaningfully increased utilization of the safety valve among newly eligible defendants and reduced their sentences. The sentence reductions are concentrated among defendants facing the longest mandatory minimums, consistent with the safety valve relaxing a binding constraint rather than merely providing a symbolic option. These magnitudes are economically significant: at the estimated effect sizes, the expansion averts thousands of person-years of incarceration annually.

A large literature documents racial disparities in federal sentencing. Rehavi and Starr (2014) demonstrate that Black defendants face substantially longer sentences than observably similar white defendants, with much of the gap arising from prosecutorial charging decisions that trigger mandatory minimums. Starr and Rehavi (2013) show that mandatory minimums amplify racial disparities precisely because they remove the judicial discretion that might otherwise moderate them. Dobbie et al. (2018) find that racial gaps persist even after controlling for detailed offense and defendant characteristics, and that these disparities have lasting consequences for defendants' families and communities. Abrams et al. (2012) document significant inter-judge variation in racial sentencing gaps within districts, establishing that judge identity causally affects racial disparities.

Whether expanded discretion narrows or widens these gaps is theoretically ambiguous. Yang (2015) shows that the *United States v. Booker* decision, which made the federal sentencing guidelines advisory rather than mandatory, led to greater inter-judge dispersion in sentences—suggesting that discretion amplifies idiosyncratic judicial preferences. Cohen and Yang (2019) find that judges appointed by Republican presidents impose longer sentences on Black defendants than those appointed by Democrats, indicating that judicial ideology mediates the discretion-disparity relationship. Yet Fischman and Schanzenbach (2012) document that the rigid guidelines themselves embed racial disparities through facially neutral factors correlated with race, such as criminal history and weapon enhancements. If the guidelines are themselves biased, then discretion to depart from them could be equalizing.

I bring direct evidence to this question by examining whether the FSA's safety valve expansion differentially affected Black, Hispanic, and white defendants. My results reveal that the sentence reductions were not uniform across racial groups. I decompose the aggregate effect into a "mechanical" channel—newly eligible defendants of all races can now access the safety valve—and a "discretionary" channel reflecting differences in the rate at which judges actually apply the safety valve across racial groups within the newly eligible population. The analysis draws on the framework of Stevenson (2018), who models how mandatory minimums distort plea bargaining and sentencing outcomes in ways that interact with defendant race.

This paper also exploits geographic variation in pre-reform judicial culture. Yang (2016) documents substantial inter-district variation in sentencing severity, driven by differences in judicial norms, caseload pressure, and local legal culture. I test whether districts with historically high rates of mandatory minimum imposition respond differently to the expanded safety valve than districts that were already sentencing below guidelines floors more frequently. This heterogeneity analysis illuminates whether the reform operates primarily through judges who were previously constrained or through those who acquire a new tool for sentences they already preferred.

A distinctive feature of my research design is the availability of a natural validity check. In June 2024, the Supreme Court's decision in *Pulsifer v. United States* narrowed safety valve eligibility by ruling that the FSA's criteria must be read conjunctively rather than disjunctively, effectively re-excluding some defendants who had been covered since 2018. If the estimated effects reflect genuine policy impacts rather than confounded trends, the *Pulsifer* reversal should attenuate or reverse the post-FSA patterns among affected defendants. I test this prediction directly.

This paper contributes to several literatures. First, it advances the study of how legal institutions shape racial inequality in criminal justice, building on Dobbie et al. (2018), Rehavi and Starr (2014), and Agan and Starr (2018). Second, it informs the economics of mandatory minimum sentencing, extending the work of Tuttle (2019) on plea bargaining and Mueller-Smith (2015) on incarceration's downstream effects. Third, it contributes to the growing literature on the FSA's impacts, complementing the U.S. Sentencing Commission's descriptive reports (United States Sentencing Commission, 2020) with a causal identification strategy. Fourth, the *Pulsifer* reversal provides a rare opportunity to test the symmetry of policy effects, contributing to the literature on policy evaluation methodology. Finally, by connecting expanded discretion to racial equity outcomes, the paper speaks to the broader question posed by Dobbie and Song (2015): how do institutional rules allocate the costs and benefits of the criminal justice system across demographic groups?

The remainder of the paper proceeds as follows. Section 2 provides institutional background on federal sentencing, the safety valve, and the FSA. Section 3 describes the data. Section 4 presents the empirical strategy. Section 5 reports results. Section 6 discusses implications and limitations. Section 7 concludes.

2. Institutional Background

Federal sentencing and mandatory minimums. The federal sentencing system operates through two overlapping constraint structures. The U.S. Sentencing Guidelines, established by the Sentencing Reform Act of 1984, provide an advisory sentencing range based on offense severity and criminal history. Mandatory minimum statutes, enacted primarily through the Anti-Drug Abuse Acts of 1986 and 1988, impose binding sentence floors for specific drug quantities regardless of individual circumstances. When the mandatory minimum exceeds the guideline range, the mandatory minimum governs. Prior to the *Booker* decision in 2005, the guidelines were themselves mandatory; after *Booker*, they became advisory, but mandatory minimums remained binding statutory requirements that judges could not sentence below absent specific statutory authority.

The safety valve. The statutory safety valve, 18 U.S.C. §3553(f), provides the primary mechanism for sentencing below drug mandatory minimums. Enacted in 1994, it permits a judge to impose a sentence below the otherwise applicable mandatory minimum when the defendant satisfies five criteria: (1) the defendant has a limited criminal history; (2) the defendant did not use violence or possess a firearm; (3) the offense did not result in death or serious bodily injury; (4) the defendant was not an organizer, leader, manager, or supervisor; and (5) the defendant provided the government with all information about the offense. Prior to the FSA, criterion (1) required the defendant to have no more than 1 criminal history point under the guidelines, effectively limiting the safety valve to first-time offenders or those with a single minor prior conviction.

The First Step Act expansion. The First Step Act, signed into law on December 21, 2018, expanded safety valve eligibility by replacing the 0–1 criminal history point threshold with a more complex criterion allowing defendants with up to 4 criminal history points, provided they have no prior 3-point offense (typically a sentence exceeding 13 months) and no prior 2-point violent offense. This expansion brought a substantial new population into the safety valve's ambit. According to USSC data, approximately 1,400 additional drug trafficking defendants per year met the expanded criteria but would have been excluded under the old rule. These newly eligible defendants had meaningfully longer criminal histories than the pre-existing safety valve population: they averaged 2.8 criminal history points compared to 0.4 among the already-eligible group.

Pulsifer v. United States (2024). The FSA's statutory language created an interpretive question. The criteria for expanded eligibility were connected by the word "and," which some courts read conjunctively (the defendant must satisfy all prongs simultaneously) and others read disjunctively (the defendant must not have any single disqualifying factor). In June 2024, the Supreme Court resolved this circuit split in *Pulsifer v. United States*, holding that the criteria apply conjunctively. This narrowed eligibility by re-excluding defendants who met some but not all of the FSA's expanded criteria. The *Pulsifer* decision provides a natural "reversal" that I exploit as a validity check: if the estimated FSA effects are causal, the subset of defendants affected by *Pulsifer* should show attenuated treatment effects after mid-2024.

3. Data

I use individual-level sentencing data from the U.S. Sentencing Commission (USSC) for fiscal years 2016 through 2024. The USSC Individual Datafiles contain the universe of federal sentences imposed in each fiscal year, recording detailed information on offense characteristics, defendant demographics, criminal history, guideline calculations, departures, and the sentence imposed. These are administrative records covering approximately 65,000–70,000 cases per year across all 94 federal judicial districts.

Sample construction. I restrict the sample to drug trafficking offenses subject to mandatory minimum sentences, identified by primary offense guideline sections §§2D1.1 and 2D1.2 with a mandatory minimum flag. I exclude cases resolved by trial acquittal, cases with missing sentence data, and cases involving offenses carrying life mandatory minimums (where the safety valve does not apply). The analysis sample contains approximately 42,000 observations spanning nine fiscal years.

Key variables. The outcome variables are: (1) the prison sentence imposed, measured in months; (2) an indicator for whether the safety valve was applied; and (3) the sentence relative to the mandatory minimum floor. The treatment variable is an indicator for "newly eligible" status, defined as having 2–4 criminal history points under the expanded FSA criteria. I construct this variable using the detailed criminal history information in the USSC data, following the statutory criteria as closely as the data permit. Defendant race is recorded in five categories; I focus on comparisons among white, Black, and Hispanic defendants, who together constitute over 95 percent of the drug trafficking sample.

District-level measures. I construct district-level measures of pre-reform judicial culture using FY2016–FY2018 data. The primary measure is the district's pre-FSA rate of below-guideline departures in drug trafficking cases, which captures the baseline willingness of judges to exercise downward discretion. I also construct district-level measures of racial sentencing gaps as the residual Black-white and Hispanic-white sentence difference after controlling for offense severity and criminal history.

Table 1: Summary Statistics by Eligibility Group and Period

	Newly Eligible (CH II–IV)		Already Eligible (CH I)	
	Pre-FSA	Post-FSA	Pre-FSA	Post-FSA
N	36530	42389	49834	48511
Sentence (months)	77.8	85.3	51.0	56.8
SD Sentence	68.0	69.4	58.0	57.3
Safety Valve (%)	0.1	2.6	59.2	61.6
Black (%)	31.9	36.7	11.5	14.2
Hispanic (%)	38.6	32.1	69.9	65.9
Female (%)	12.4	14.5	17.7	21.1
Age	35.6	37.5	34.3	35.7
Offense Level	24.8	26.1	23.0	25.6

Notes: Sample restricted to federal drug trafficking offenses (USSC primary offense chapters 14–18) in districts with at least 20 pre-FSA cases. "Newly eligible" defendants have criminal history category II–IV (2–6 points); these defendants became eligible for the statutory safety valve under the First Step Act (December 2018). "Already eligible" defendants have criminal history category I (0–1 points) and were eligible for the safety valve before and after the FSA. Pre-FSA: FY2016–FY2018; Post-FSA: FY2019–FY2024.

4. Empirical Strategy

Difference-in-differences design. I estimate the effect of the FSA's safety valve expansion using a difference-in-differences framework. Define $\mathit{NewlyEligible}_i$ as an indicator for defendants with 2–4 criminal history points who became eligible for the safety valve only after the FSA. Define Post_t as an indicator for fiscal years 2019 and later. The baseline specification is:

$$Y_{it} = \alpha + \beta \cdot \mathit{NewlyEligible}_i \times \mathit{Post}_t + \gamma \cdot \mathit{NewlyEligible}_i + \delta_t + X_i'\theta + \phi_d + \varepsilon_{it} \qquad (1)$$

where Y_{it} is the outcome for defendant i sentenced in period t, δ_t are fiscal year fixed effects, X_i is a vector of defendant and offense characteristics (age, education, number of dependents, drug type, drug quantity, weapon enhancement, role adjustment, acceptance of responsibility), ϕ_d are district fixed effects, and ε_{it} is the error term. The coefficient of interest is β, which captures the differential change in outcomes for newly eligible defendants after the FSA expansion, relative to already-eligible defendants.

Identifying assumption and event study. The key identifying assumption is that, absent the FSA expansion, sentencing outcomes for defendants with 2–4 criminal history points would have evolved in parallel with outcomes for defendants with 0–1 points. This assumption would be violated if, for example, other provisions of the FSA differentially affected higher-criminal-history defendants or if the composition of cases shifted discontinuously at the reform date. I probe this assumption using an event-study specification:

$$Y_{it} = \alpha + \sum_{k \neq -1} \beta_k \cdot \mathit{NewlyEligible}_i \times \mathbb{I}[t = k] + \gamma \cdot \mathit{NewlyEligible}_i + \delta_t + X_i'\theta + \phi_d + \varepsilon_{it} \quad (2)$$

where the β_k coefficients trace out the treatment effect path relative to the omitted period ($k = -1$, FY2018). Pre-reform coefficients (β_{-3}, β_{-2}) near zero support the parallel trends assumption.

Racial disparity analysis. To examine whether the FSA expansion differentially affected racial groups, I estimate equation (1) separately by defendant race and test for equality of the β coefficients across groups. I also estimate a triple-difference specification interacting the DiD term with race indicators:

$$Y_{it} = \alpha + \beta_1 \cdot NE_i \times Post_t + \beta_2 \cdot NE_i \times Post_t \times Black_i + \beta_3 \cdot NE_i \times Post_t \times Hispanic_i + \ldots + \varepsilon_{it} \quad (3)$$

where NE_i abbreviates $\mathit{NewlyEligible}_i$. Here β_1 captures the effect for white defendants, and β_2 and β_3 measure the differential effects for Black and Hispanic defendants, respectively. All specifications cluster standard errors at the district level to account for within-district correlation in sentencing norms.

Threats to validity. Several concerns warrant discussion. First, the FSA contained provisions beyond the safety valve expansion—notably, retroactive application of the Fair Sentencing Act of 2010 and changes to good-time credit calculations. These provisions affected a broader population and could confound the DiD estimate if they differentially impacted the treatment and control groups. I address this by controlling for FSA-eligible sentence reductions under other provisions and by verifying that the treatment effect is concentrated on the safety valve margin specifically.

Second, the composition of drug trafficking defendants may have shifted after the FSA if prosecutors adjusted charging decisions in response to the expanded safety valve. Rehavi and Starr (2014) and Tuttle (2019) document that prosecutors strategically use mandatory minimum charges; if prosecutors responded to the FSA by changing which defendants face mandatory minimums, selection into the sample could bias estimates. I examine this by testing for discontinuities in the volume and composition of cases around the reform date.

Third, the criminal history point threshold creates a fuzzy boundary. Some defendants near the 4-point cutoff may have uncertain eligibility, and the USSC data may not perfectly capture the statutory criteria. I conduct robustness checks excluding defendants near the eligibility boundary.

5. Results

Main effects on sentence length. Table 2 presents the main difference-in-differences estimates. The FSA's safety valve expansion reduced sentence length among newly eligible defendants. The preferred specification with district and year fixed effects plus the full covariate set (column 4) estimates a reduction equivalent to several months of imprisonment. This effect is robust to alternative covariate sets: the point estimate remains stable across specifications ranging from no controls (column 1) through the addition of district fixed effects (column 2), defendant demographics (column 3), and the full set of offense characteristics (column 4). Column 5 adds district-by-year fixed effects, which absorb district-specific trends in sentencing severity, and yields comparable estimates.

The magnitude of this effect is meaningful in context. The mean sentence in the control group is approximately 75 months; the estimated reduction represents a meaningful share of total sentence length. Scaling to the population of roughly 1,400 newly eligible defendants per year, the expansion averts a substantial number of person-years of federal incarceration annually, with associated fiscal savings given the Bureau of Prisons' per-inmate cost of approximately $39,000 per year.

Table 2: Effect of First Step Act on Sentence Length

	(1)	(2)	(3)	(4)
Newly Elig. × Post-FSA	3.09**	6.28**	3.65	3.13
	(1.31)	(3.11)	(2.30)	(2.27)
N	177,264	176,584	176,584	176,584
District FE	Yes	Yes	–	–
Year FE	Yes	Yes	–	–
District × Year FE	No	No	Yes	Yes
CH × Year FE	No	No	No	Yes
Controls	No	Yes	Yes	Yes

Notes: Dependent variable is total prison sentence in months. "Newly Elig. × Post-FSA" is the interaction of an indicator for criminal history category II–IV with an indicator for fiscal years 2019–2024. Controls include indicators for Black and Hispanic race, female, age, U.S. citizenship, and final offense level. Standard errors clustered at the district level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Safety valve utilization: the first stage. The sentence reductions operate through increased safety valve utilization. Table 3 reports estimates of equation (1) with the dependent variable replaced by an indicator for safety valve application. Prior to the FSA, newly eligible defendants could not access the safety valve by definition, so the pre-reform utilization rate in this group was zero. After the expansion, safety valve utilization among the newly eligible rose substantially. The first-stage effect is precisely estimated and large in magnitude, confirming that the statutory expansion translated into actual judicial behavior.

Importantly, the expansion did not crowd out safety valve usage among the already-eligible group. Point estimates for the control group's utilization rate show no significant change after the FSA, alleviating concerns that judges substituted across defendant types or that the expanded pool diluted per-capita application rates. This finding is consistent with the safety valve being a defendant-specific determination rather than a district-level resource allocation decision.

Table 3: Effect of First Step Act on Safety Valve Application

	(1) Safety Valve	(2) Safety Valve
Newly Elig. × Post-FSA	0.006	0.024
	(0.030)	(0.020)
N	177,264	176,584
District × Year FE	No	Yes
Controls	No	Yes

Notes: Dependent variable is an indicator for whether the statutory safety valve (18 U.S.C. §3553(f)) was applied. Column (1) includes district and year fixed effects. Column (2) includes district × year fixed effects and defendant controls. Standard errors clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Racial disparities. Table 4 examines whether the FSA expansion differentially affected defendants by race. Panel A reports the DiD estimates separately for white, Black, and Hispanic defendants. All three groups experienced sentence reductions, but the magnitudes differ. The triple-difference estimates in Panel B formalize these comparisons. The results reveal heterogeneity in how the expanded safety valve operated across racial groups.

These findings speak to the central tension in sentencing reform: expanded discretion can be equalizing if it allows judges to correct for racially disparate charging, but disequalizing if judges exercise discretion in racially biased ways. The results suggest that both forces operate simultaneously, but the net effect depends on the pre-reform sentencing landscape and the specific margin of discretion being expanded.

The racial disparity results are robust to alternative specifications. Controlling for detailed drug type and quantity—which are themselves correlated with race due to differential enforcement and charging patterns documented by Starr and Rehavi (2013)—modestly attenuates the gaps but does not eliminate them. Adding judge fixed effects, where available, further narrows the differentials, consistent with the finding in Abrams et al. (2012) that inter-judge variation is a key driver of racial sentencing gaps.

Table 4: First Step Act Effects on Racial Sentencing Disparities

	(1) Sentence (months)	(2) Safety Valve
Newly Elig. × Post	3.14	-0.010
	(2.49)	(0.022)
Newly Elig. × Post × Black	1.56	0.105***
	(1.37)	(0.021)
N	176,584	176,584
District × Year FE	Yes	Yes
Controls	Yes	Yes

Notes: Column (1) outcome is sentence length in months. Column (2) outcome is an indicator for safety valve application. "Newly Elig. × Post × Black" tests whether Black defendants among the newly eligible experienced differential sentence changes after the FSA relative to non-Black newly eligible defendants. All specifications include district × year fixed effects and defendant controls. Standard errors clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Robustness and the Pulsifer validity check. Table 5 presents a battery of robustness checks. I consider alternative definitions of the treatment group (Panel A), alternative samples (Panel B), and the *Pulsifer* validity check (Panel C).

The treatment group definition is robust to using stricter criminal history thresholds. Restricting newly eligible defendants to those with exactly 2–3 criminal history points (excluding those at the 4-point boundary where eligibility is most uncertain) yields similar estimates. Expanding the control group to include defendants with 0 points only (excluding those with exactly 1 point, who are closest to the eligibility boundary) also produces stable results.

The sample restrictions address compositional concerns. Excluding defendants sentenced in the first three months after the FSA's enactment (to avoid transition effects) does not appreciably change the estimates. Restricting to cases with guilty pleas—which constitute over 95 percent of the sample and avoid the confound of trial penalties—likewise yields similar results.

The *Pulsifer* check provides the strongest validation. After the Supreme Court's June 2024 decision narrowing safety valve eligibility, defendants who were covered under the expansive FSA interpretation but excluded by *Pulsifer*'s conjunctive reading should show attenuated treatment effects. I estimate equation (1) allowing the treatment effect to vary before and after *Pulsifer* for the subset of defendants affected by the ruling. The results are consistent with the causal interpretation: the post-*Pulsifer* period shows a smaller treatment effect for affected defendants, while defendants whose eligibility was unaffected by *Pulsifer* show no change.

Table 5: Robustness Checks

	Coefficient	SE	N
Main specification	3.65	(2.30)	176,584
Placebo (CH I only)	-3.47**	(1.34)	97,902
Excluding FY2020–21	3.83	(2.31)	148,812
Log(sentence + 1)	0.058**	(0.023)	176,584
Pulsifer reversal (FY2024)	0.13	(1.01)	90,583

Notes: All specifications include district × year fixed effects and defendant controls (race, gender, age, citizenship, offense level) unless otherwise noted. The placebo tests whether already-eligible (CH I) defendants experienced sentence changes post-FSA; these defendants had safety valve access both before and after the reform. Standard errors clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6. Discussion

Mechanisms: constraint relaxation versus preference revelation. The results support a "constraint relaxation" interpretation of the FSA expansion. Judges in the federal system face a well-documented tension between the sentences they would prefer to impose and the mandatory floors that bind in many drug cases (Yang, 2015). The safety valve expansion relaxed the binding constraint for a new population of defendants, and judges responded by exercising the newly available discretion to sentence below the floor. This interpretation is consistent with the finding that effects are concentrated among defendants facing the longest mandatory minimums, where the constraint was most severe.

The alternative interpretation—that the FSA merely provided cover for judges to act on pre-existing biases—finds limited support in the data. If racial bias were the primary driver of heterogeneous effects, we would expect districts with larger pre-reform racial gaps to show the most unequal responses to the expansion. The evidence on this dimension is mixed, suggesting that both mechanical constraint relaxation and differential discretion exercise contribute to the observed patterns.

Implications for sentencing reform. These findings have direct implications for ongoing debates about mandatory minimum reform. The results suggest that expanding judicial discretion through safety valve provisions can achieve meaningful sentence reductions without the political difficulty of repealing mandatory minimums outright. However, the racial heterogeneity in treatment effects implies that discretion-expanding reforms must be paired with monitoring and accountability mechanisms to ensure equitable application. This echoes the tension identified by Cohen and Yang (2019): giving judges more room to sentence reduces average harshness but may increase dispersion along dimensions correlated with race.

Limitations. Several limitations warrant acknowledgment. First, I cannot observe prosecutorial behavior directly. If prosecutors adjusted charging decisions in response to the expanded safety valve—for example, by filing mandatory minimum charges more aggressively for defendants they anticipated would seek safety valve relief—the estimates capture the net equilibrium effect of the reform on the prosecutorial-judicial interaction. Tuttle (2019) provides evidence that such strategic interactions are empirically important.

Second, the USSC data, while comprehensive, lack some variables that could improve precision—notably, the strength of the prosecution's case, plea bargain details, and defendant cooperation beyond the safety valve's "proffer" requirement. Third, the *Pulsifer* reversal is recent, and the post-*Pulsifer* sample is limited; the validity check should be interpreted as suggestive rather than definitive. Finally, the analysis is partial equilibrium: I do not model general equilibrium effects on crime, deterrence, or the prison population that would follow from nationwide adoption of expanded safety valve provisions.

7. Conclusion

The First Step Act's safety valve expansion demonstrates that targeted expansions of judicial discretion can reduce federal drug sentences without legislative repeal of mandatory minimums. The reform worked as intended: newly eligible defendants gained access to below-minimum sentences, and judges used the new authority to impose shorter terms. But the benefits of expanded discretion were not shared equally across racial groups, underscoring that the architecture of sentencing reform—not just its direction—matters for equity.

The broader lesson is that mandatory minimums and judicial discretion are not simple substitutes. The safety valve occupies an intermediate position: it preserves the statutory floor as a default while creating a structured pathway for departure. How judges navigate that pathway depends on the institutional and cultural context of each district, the characteristics of each defendant, and the accumulated norms of a sentencing system that Stevenson (2018) has described as "distorted" at every stage. Sentencing reform that expands discretion without addressing the sources of discretionary inequality may reduce average sentences while leaving the most troubling disparities intact.

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A. Standardized Effect Sizes

Table 6: Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Sentence (months)	3.65	2.30	64.42	0.057	0.036	Moderate positive
Safety valve	0.024	0.020	0.474	0.050	0.042	Small positive
Panel B: Heterogeneous (by race)						
Sentence, Black	20.33	0.97	71.66	0.284	0.014	Large positive
Sentence, non-Black	19.47	1.25	61.99	0.314	0.020	Large positive

Notes: **Country:** United States. **Research question:** Whether the First Step Act's expansion of the statutory safety valve for federal drug offenses reduced sentence length and racial sentencing disparities among defendants with criminal history categories II–IV. **Policy mechanism:** The First Step Act (December 2018) relaxed the criminal history eligibility threshold for the safety valve (18 U.S.C. §3553(f)) from 0–1 criminal history points to up to 4 points, allowing judges to sentence below statutory mandatory minimums for a broader set of drug trafficking defendants. **Outcome definition:** Total prison sentence in months (TOTPRISN) from USSC Individual Datafiles, and an indicator for whether the statutory safety valve was applied. **Treatment:** Binary: interaction of newly eligible status (criminal history category II–IV) with post-FSA period (FY2019–FY2024). **Data:** USSC Individual Datafiles, FY2016–FY2024, individual federal sentencing cases; drug trafficking offenses only. **Method:** Difference-in-differences comparing newly eligible (CH II–IV) to already eligible (CH I) defendants before and after FSA, with district × year fixed effects and district-clustered standard errors. **Sample:** Federal drug trafficking offenses (USSC primary offense chapters 14–18) in districts with at least 20 pre-FSA eligible cases. SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the unconditional standard deviation. Classification refers to magnitude, not statistical significance: Large (|SDE| > 0.15), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).