Bail Reform and Public Safety: Evidence from 33 Cities

Methodological Supplement

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This methodological supplement outlines the steps we took to assemble this report, from gathering data sources to applying our research strategy. The full report can be found at https://www.brennancenter.org/our-work/research-reports/bail-reform-and-public-safety.

Data Collection

City Data Collection — Crime Data

We constructed a database of crime data from 33 cities spanning 2015 through 2021. A list of cities was initially devised based on size and geographic diversity, then winnowed according to the availability of crime data.

Data was first collected from raw sources, then trimmed to focus exclusively on six of the eight Part I index offenses tracked by the FBI: murder, robbery, aggravated assault, burglary, motor vehicle theft, and larceny.[1] Narrowing the analysis to only these offenses solved several analytical problems. These are the most serious offenses known to law enforcement, and they are also tracked by almost all police agencies. Additionally, their elements are clearly defined in FBI documentation, providing a baseline that could be used to overcome differences among cities in how offenses are labeled.[2] However, collecting and cleaning the data to focus on these offenses proved challenging and required a different strategy for each data source.

For more than half of the cities, data was obtained from the Crime Open Database (CODE).[3] This resource collects incident-level data on major cities, quality-checks it, aggregates it into yearly files, and makes the results publicly available to researchers. Offenses are listed by name and "offense code," a variable that approximates — but does not precisely mirror — the National Incident-Based Reporting System (NIBRS) codes used by the FBI.[4] Codes and offense descriptions were used to capture only those offenses that would meet the FBI's definitions of the six offenses spotlighted for the study. For example, aggravated assault (offense code 13A in the CODE dataset) is a Part I index crime, but simple assault (offense code 13B) is not. Therefore, only offenses coded with "13A" were included.

For nearly all remaining cities we obtained crime data from public portals that track offenses known to local law enforcement.[5] These databases are regularly updated and generally current through the most recent quarter. However, the data quality and format vary widely among cities. These databases fell into three broad categories:

- **Incident-level lists of Part I index offenses.** Some cities, such as Washington, D.C., listed only Part I index offenses in their dataset. Each row in the dataset is an offense, making importing the data comparatively straightforward.

- **Incident-level lists with NIBRS codes.** Cities in this group maintain a database that lists each offense as a row, with variables for offense name and a NIBRS code. In these cases, offenses were identified and included using NIBRS codes to tag only those offenses meeting the FBI's definitions of Part I index offenses. For example, the NIBRS code "09A" corresponds to murder.

- **Incident-level lists with no codes.** Some cities reported crime data as a list of offenses but with no variable corresponding to NIBRS codes. (New York City, for example, assigns "key codes" and internal police department codes to offenses, but neither correspond to NIBRS codes). In these cases, we manually matched offense descriptions to FBI Part I index offense definitions. To continue with the example of New York City, "larceny" was defined for this report by adding together various theft offenses listed in city data, such as "Petit Larceny" and "Grand Larceny," but excluding those theft crimes that do not fall within the FBI's definition of the crime, such as "Theft-Fraud."

Newark, New Jersey, rounds out the sample — a necessity given the expansive bail reform New Jersey began implementing in 2017. To ensure that the state was represented in this study, we submitted crime data requests or requests for information to city officials in five New Jersey cities: Atlantic City, Hoboken, Jersey City, Newark, and Trenton. Only Newark responded in time to complete the analysis. City officials shared weekly reports of Part I index offenses for 2015 through the first quarter of 2023. These PDFs were transcribed to a spreadsheet and grouped into months, and the few gaps were filled by statistical interpolation.[6]

Several jurisdictions transitioned from the FBI's legacy Summary Reporting System (SRS) to the new NIBRS reporting standard during the study period. This posed a challenge due to the way these systems count offenses. Under the SRS, a criminal incident involving multiple offenses is "flattened," and only the most serious offense is retained and reported. Under NIBRS, all offenses are reported. For example, an assault that escalates to murder would be reported in an SRS jurisdiction as a murder, and in a NIBRS jurisdiction as an assault *and* a murder. Failing to account for this change could risk inflating the number of crimes in NIBRS jurisdictions, and in later years, by 2 to 5 percent.[7]

To correct for this risk, in every jurisdiction that reported multiple offenses per incident, we ran a code dropping all but the most serious offense. Essentially, this translated NIBRS-style data into SRS-style data. To identify the most serious offense in each incident, the code was written to follow the FBI's original "hierarchy rule." While this process may be imperfect, it is unlikely that any offense inflation was large enough to distort our findings. If anything, offense inflation related to the NIBRS transition would risk *overstating* post-reform increases in crime rates.

Last, for the purposes of analysis, data was reorganized from the offense level to the *monthly* level, making each observation a count of a specific offense, in a specific city, in a specific month, in a specific year. This grouping allowed us to increase the analysis's statistical power, yielding at the maximum 15,934 observations. Table D6 presents descriptive statistics of monthly rates for offenses in our database.

Bail Policies

Next, we identified which of the cities in the report's sample had enacted a bail reform policy — that is, some form of policy change designed to reduce detention or the use of money bail — during the study period. Any such change was classified according to the branch of government through which the policy was enacted: prosecutorial, court-enforced, or legislative. Jurisdictions were then coded as "treated" — that is, as a bail reform jurisdiction — and by policy type. Where multiple bail reforms went into effect during the study period, we performed this coding based on the first reform to go into effect. The sole exception to this rule was New York City. Though New York experienced pre-2020 prosecutorial bail reforms, these were borough-based, not citywide. Additionally, we believed that coding the city according to pre-2020 prosecutorial bail reform would risk understating the impact of the statewide 2020 legislative reforms.

Some jurisdictions were then coded as having "major" reforms if existing research demonstrated a significant change in post-reform bail-setting behavior. This subset was constructed to address the possibility that some bail policies studied here may have failed to achieve their desired effect due to implementation or political challenges.[8] Focusing the analysis on "major" reforms enabled us to evaluate both the effect of bail reform writ large and those reforms with clear, demonstrable impacts on bail outcomes and decision-making.

Table M1 provides a summary of the crime database and coding decisions.

Table M1. Cities Studied, by Data Source and Reforms Enacted (2015–21)

City	Years of Data	Data Source	Bail Reform
Atlanta, GA	2015–2021	Data portal[9]	Legislative (2018)
Austin, TX	2015–2021	CODE	Courts (2020) Prosecutorial (2021)
Baltimore, MD	2015–2021	Data portal[10]	Courts (2017)
Boston, MA	2015–2021	CODE	Prosecutorial (2019)
Buffalo, NY	2015–2021	Data portal[11]	Legislative (2020)*
Chicago, IL	**2015–2021**	**CODE**	Prosecutorial, **Courts (2017)*** Legislative (2018)
Cincinnati, OH	2015–2021	Data portal[12]	Courts (2020)
Colorado Springs, CO	2016–2021	CODE	Legislative (2019)
Dallas, TX	2015–2021	Data portal[13]	Prosecutorial (2019)
Denver, CO	2017–2021	Data portal[14]	Legislative (2019)
Detroit, MI	2015–2021	CODE	—
Houston, TX	**2015–2021**	**Data portal[15]**	**Courts (2017)***
Kansas City, MO	2015–2021	CODE	Courts (2019)
Los Angeles, CA	2015–2021	CODE	Prosecutorial (2020) Courts (2021)
Louisville, KY	2015–2021	CODE	Legislative (2011) *Prior to study period*
Memphis, TN	2015–2021	CODE	—
Mesa, AZ	2016–2021	CODE	—
Milwaukee, WI	2015–2021	Data portal[16]	—
Nashville, TN	2015–2021	CODE	—
New Orleans, LA	2015–2021	Data portal[17]	—
New York, NY	**2015–2021**	**CODE**	**Legislative (2020)*** Prosecutorial (2017, 2018)
Newark, NJ	**2015–2021**	**Records request**	**Legislative (2017)***
Philadelphia, PA	2015–2021	Data portal[18]	Prosecutorial (2018)
Phoenix, AZ	2015–2021	Data portal[19]	—

Portland, OR	2015–2021	Data portal[20]	*After study period*
Raleigh, NC	2015–2021	Data portal[21]	—
Sacramento, CA	2015–2021	Data portal[22]	Courts (2021)
San Francisco, CA	2015–2021	CODE	Courts (2021)
Seattle, WA	2015–2021	CODE	—
St. Louis, MO	2015–2020	CODE	Prosecutorial (2017) Courts (2019)
Tucson, AZ	2015–2020	CODE	—
Virginia Beach, VA	2015–2021	CODE	Legislative (2021)
Washington, DC	2015–2021	Data portal[23]	Legislative (1992) *Prior to study period*

Source: Brennan Center analysis. Jurisdictions with "major" reforms are in bold type, and the specific reform is indicated with an asterisk.

Difference-in-Differences Analysis Strategy

Crime outcomes are defined as index crime rates, violent crime rates, property crime rates, and larceny rates. The term *rate* refers to the number of offenses per 100,000 people in each jurisdiction. Results represent the estimated change in crime rate per type of crime monthly (e.g., ±1 larceny per 100,000 residents per month).

To measure the impact of bail reform on crime outcomes, this paper employs a difference-in-differences (DD) strategy. This is a quantitative tool used to measure the outcome difference between treatment and comparison groups before and after the implementation of a reform. In the bail reform context, therefore, the DD strategy measures the outcome difference between cities with bail reform and cities without, before and after the implementation of the reform. If certain conditions are met, this crime-outcome difference can be interpreted as a causal impact of bail reform.

This strategy permits us to distinguish, ideally, between the impact of bail reform and the impact of other factors. For instance, the effective date of some bail reforms (such as New York City's) coincided with the onset of the Covid-19 pandemic. Therefore, to separate the impact of bail reform from that of the pandemic and other factors, the DD strategy controls for:

- City-level lockdown dates
- State-level median household income
- Racial composition of the cities
- State-level indicator for Democratic majority in most recent presidential election
- City-level indicator for above-average number of police officers per capita
- City- and year-specific fixed effects

By adding these control variables to the DD regressions, the report's model seeks to account for any effects these variables might have on crime outcomes separate from the effect of bail reform. Attempts were made to control for the implementation of other criminal justice reform policies that overlap with bail reform — specifically, policies in which prosecutors have systematically declined to prosecute specific types of lower-level offenses. Unfortunately, adding these controls introduced a collinearity problem, and they had to be removed.

One important condition that must hold for the DD strategy to succeed is that trends in the outcome variable must be parallel in treatment and comparison groups before the reform is implemented. This is to ensure that no other factors conflated with bail reform are working latently to change crime outcomes. If this condition holds, any change in crime outcomes thereafter *may* be attributed to bail reform (rather than to some other factor).

Because the implementation dates of bail reform are staggered across cities, a basic DD strategy would inadvertently compare some cities that are treated earlier in the sample to cities that are treated later in the sample. To correct for this problem, we used the Chaisemartin-d'Haultfoeuille (C-H) procedure to check the sensitivity of the DD estimates to any bias induced by staggered implementation.[24]

Tables M2 through M5 show C-H estimates of the average treatment on the treated (ATT) effects for 12 months post-reform and whether the pre-reform parallel trend condition is satisfied. Table M2 Column (1) shows that bail reform raises index crime rates by about one offense per 100,000 people in a given month. Although this estimate maintains parallel outcome trends in pre-reform years, it is not statistically different from zero. The C-H procedure also reveals that prosecutorial, court-enforced, and legislative bail reforms do not change index crime rates to a degree of statistical significance, despite parallel pre-reform outcome trends.

Applying the C-H procedure to the evaluation of violent crime rates, property crime rates, and larceny rates also confirm the results from the general model: bail reform does not change crime rates statistically significantly (see Tables M2–M5, below). For these findings, our model yielded pre-reform parallel trends for all outcomes except violent crime rates. We acknowledge that this is a limitation of our findings; however, violent offenses are rarer than other offenses in our sample. (Larceny is the most common.) It is possible that our inability to establish pre-reform parallel trends may stem from this characteristic of our dataset.

Sensitivity Checks

To reinforce the main results from the C-H procedure, our study also evaluates the relationship between bail reform and crime rates using the Callaway-Sant'Anna (C-S) procedure to rectify the weighting bias problem that emerges from staggered implementation.[25] We used the C-S procedure to compare bail reform cities with other cities that are not yet treated.

The C-S model did not yield parallel pre-reform trends. However, the average treatment on the treated (ATT) estimates from this analysis showed statistically significant effects in only one case. In that case, the ATT estimate showed that legislative bail reform *lowers* violent crime rates at the 5 percent statistical significance level. However, given that the parallel trends condition does not hold for this result either — and that it is the only statistically significant outcome in our study, under either estimator — we conclude that this is likely a spurious finding.

We performed several other checks. For one, some jurisdictions (especially New York City) enacted bail reform around the time of the Covid-19 pandemic, the effects of which may be conflated with the post-reform effects of bail policies. Given this, we restricted the analysis sample to the period before March 2020. This sample restriction did not change the main findings. We also sought to ensure that no one city or state drove the results of our analyses. To check the sensitivity of the findings to anomalous cities or states, we dropped each city and each state in succession from the analysis sample. Again, this did not alter our findings that bail reform does not change crime outcomes in a statistically significant fashion.

Limitations

This report's findings represent a step forward in our understanding of bail policy but also point to directions for future research. The models used here do not account for some metrics, like arrests and court caseloads, that may be affected by bail reform and could help explain changes in crime rates. Data limitations are partly to blame. Caseload data can be very hard to find, especially for a sample of cities as large as the one used here. Similarly, while some jurisdictions have begun to post data on arrests, that information is not available for anything close to the full sample of 33 cities. Future researchers could consider ways to find and incorporate this data, such as through data sharing agreements with police agencies and state court systems.

As a result, this report also cannot fully account for the possibility that bail reform policies face challenges in implementation or even fail to reduce the use of money bail altogether. That possibility is very real. Other Brennan Center research has documented at least two cases where court-led bail reform has had limited or counterintuitive impacts on bail setting.[26] However, this report's focus on "major" reforms should theoretically address this limitation by focusing on jurisdictions (such as Harris County, Texas) where reform is known to have reduced the use of money bail. More data, and more jurisdiction-by-jurisdiction research on where bail reforms have accomplished their goals or fallen short, would improve this approach.

Relatedly, crime data is challenging to work with due to varying definitions of offenses and differing data-reporting practices among cities. Thankfully, the quality of crime data appears to be improving. More cities post incident-level crime data online today than did so just a few short years ago, and the FBI is in the middle of a transition to a new crime reporting system that will provide detailed, incident-level information on crime.[27] Future research may benefit from these ongoing improvements.

It is also conceivable that bail reform may affect trends in lower-level offenses not tracked by this study. But that is unlikely, for two reasons. First, other researchers have concluded that misdemeanor bail reform in Harris County, Texas, did not lead to an increase in lower-level offenses.[28] Second, while most offenses studied for this report are likely to be felonies under local law, the FBI's definition of larceny includes nearly all thefts, regardless of the dollar value involved, and some of these are almost certainly misdemeanors.[29] And this report uncovered no statistically significant change in larceny rates after bail reform. Indeed, that finding held even when we tested whether specific *types* of bail reform might influence larceny rates.

This report's national perspective provides an important new way to consider the effects of bail reform. The lack of evidence for a relationship between bail reform and crime certainly undercuts politicized attempts to link bail reform with national trends in violence. But this aggregate approach also obscures the experience of individual jurisdictions. Some mainstream studies of individual jurisdictions, as discussed in appendix B, do find a limited connection between bail reform and crime — in both directions. Nothing in this report necessarily contradicts those findings.

Last, this report covers a relatively small period — encompassing just the first two years after the onset of the Covid-19 pandemic. In some cases, such as in New York City, this means that there are a relatively small number of post-reform observations in the sample. It is possible that bail reform affects crime rates, positively or negatively, over the long term, such as on the order of five years or a decade. Data to test that possibility across a similar number of jurisdictions does not yet exist, but researchers should continue to study the issue.

Regression Tables

Table M2. The Effect of Bail Reform on Index Crime Rates

	(1)	(2)	(3)	(4)	(5)
Variables	General	Prosecutor	Court	Legislative	"Major" Reforms
		Chaisemartin-d'Haultfoeuille (C-H) Estimates			
Reform*Post	0.009	–0.865	–0.809	–0.321	–2.960
	(2.167)	(6.145)	(1.695)	(1.860)	(6.099)
Pre-reform p-value	0.10	0.32	0.42	0.59	0.20

Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Data sources: Crime Open Database (CODE) [2015–2021]; other city-level open data sources.

Notes: The Chaisemartin-d'Haultfoeuille (C-H) regressions include median household income, percent Black, and percent Hispanic; starting month of Covid lockdown and lockdown period; indicator for Democratic majority in most recent presidential election; indicator for above-average officers per capita; crime type; and city- and year-month fixed effects. Statistically significant pre-reform p-values indicate that the parallel trends assumption is violated.

Table M3. The Effect of Bail Reform on Violent Crime Rates

	(1)	(2)	(3)	(4)	(5)
Variables	General	Prosecutor	Court	Legislative	“Major” Reforms
	Chaisemartin- d’Haultfoeuille (C-H) Estimates				
Reform*Post	0.348	1.167	–0.702	–0.333	–1.656
	(0.855)	(2.010)	(1.330)	(1.350)	(2.079)
Pre-reform p-value	0.00***	0.07*	0.35	0.01**	0.17

Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Data sources: Crime Open Database (CODE) [2015–2021]; other city-level open data sources.

Notes: The Chaisemartin-d’Haultfoeuille (C-H) regressions include median household income, percent Black, and percent Hispanic; starting month of Covid lockdown and lockdown period; indicator for Democratic majority in most recent presidential election; indicator for above-average officers per capita; crime type; and city- and year-month fixed effects. Statistically significant pre-reform p-values indicate that the parallel trends assumption is violated.

Table M4. The Effect of Bail Reform on Property Crime Rates

	(1)	(2)	(3)	(4)	(5)
Variables	General	Prosecutor	Court	Legislative	“Major” Reforms
		Chaisemartin- d’Haultfoeuille (C-H) Estimates			
Reform*Post	–1.052	–2.624	–3.233	–0.653	–6.178
	(4.265)	(12.309)	(2.483)	(3.364)	(13.94)
Pre-reform p-value	0.74	0.27	0.95	0.81	0.33

Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Data sources: Crime Open Database (CODE) [2015–2021]; other city-level open data sources.

Notes: The Chaisemartin-d’Haultfoeuille (C-H) regressions include median household income, percent Black, and percent Hispanic; starting month of covid lockdown and lockdown period; indicator for Democratic majority in most recent presidential election; indicator for above-average officers per capita; crime type; and city- and year-month fixed effects. Statistically significant pre-reform p-values indicate that the parallel trends assumption is violated.

Table M5. The Effect of Bail Reform on Larceny Rates

	(1)	(2)	(3)	(4)	(5)
Variables	General	Prosecutor	Court	Legislative	“Major” Reforms
			Chaisemartin- d’Haultfoeuille (C-H) Estimates		
Reform*Post	–7.612	–15.260	–9.049	–5.217	–38.955
	(11.824)	(33.472)	(7.629)	(7.469)	(46.159)
Pre-reform p-value	0.85	0.67	0.99	0.37	0.06*

Clustered standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Data sources: Crime Open Database (CODE) [2015–2021]; other city-level open data sources.

Notes: The Chaisemartin-d’Haultfoeuille (C-H) regressions include median household income, percent Black, and percent Hispanic; starting month of covid lockdown and lockdown period; indicator for Democratic majority in most recent presidential election; indicator for above-average officers per capita; crime type; and city- and year-month fixed effects. Statistically significant pre-reform p-values indicate that the parallel trends assumption is violated.

Table M6. Descriptive Statistics

Variable	Mean	Standard Deviation
Index Crime Rate	396.55	249.67
Violent Crime Rate	80.54	87.92
Property Crime Rate	319.64	248.53
Larceny Rate	194.34	113.81

Data sources: Crime Open Database (CODE) [2015–2021]; other city-level open data sources.

Endnotes

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[1] Federal Bureau of Investigation (hereinafter FBI), "Offense Definitions," *Crime in the United States — 2019*, accessed October 24, 2023, https://ucr.fbi.gov/crime-in-the-u.s/2019/crime-in-the-u.s.-2019/topic-pages/offense-definitions. Arson is technically also a Part I offense, but this report follows other scholars in omitting it from our study. We would prefer to include rape, which the FBI classifies as a Part I violent offense. However, a surprising number of cities failed to report data on this serious offense. The offense was dropped from this analysis accordingly to minimize variation among cities.

[2] See FBI, *Summary Reporting System (SRS) User Manual* 47, 2013 (on file with the authors).

[3] OSFHome, "Crime Open Database," last accessed October 27, 2023, https://osf.io/zyaqn/wiki/3.%20Get%20the%20data/.

[4] FBI, "NIBRS 2011: NIBRS Offense Codes, 2011, https://ucr.fbi.gov/nibrs/2011/resources/nibrs-offense-codes. For example, theft from a motor vehicle is coded as "23F," matching NIBRS coding precisely. But assaults are coded as "13A," "13B," and "13C" rather than "130."

[5] See, e.g., NYC OpenData, "NYPD Complaint Data Current (Year to Date)," last accessed October 27, 2023, https://data.cityofnewyork.us/Public-Safety/NYPD-Complaint-Data-Current-Year-To-Date-/5uac-w243/.

[6] For example, in several cases a weekly report contained blanks for one or more offenses. But the Newark Police Department's weekly reports contained data on the current week and the *previous* week, presumably as a basis for comparison. Therefore, we were able to backfill gaps in one week from the following week's report.

[7] Ames Grawert and Noah Kim, "Understanding the FBI's 2021 Crime Data," Brennan Center for Justice, October 27, 2023, https://www.brennancenter.org/our-work/research-reports/understanding-fbis-2021-crime-data (referencing a since-deleted FBI white paper on the effect of the NIBRS transition).

[8] For a discussion of the difficulty of translating policy change to behavioral change in pretrial justice systems, see generally Stephanie Wylie and Ames Grawert, *Challenges to Advancing Bail Reform*, Brennan Center for Justice, April 10, 2024, https://www.brennancenter.org/our-work/research-reports/challenges-advancing-bail-reform.

[9] Atlanta Police Department, "Open Data Portal," last accessed November 22, 2023, http://opendata.atlantapd.org/Crimedata/Default.aspx.

[10] Baltimore Police Department, "Open Data," last accessed October 27, 2023, https://www.baltimorepolice.org/crime-stats/open-data.

[11] OpenData Buffalo, "Crime Incidents," last accessed October 27, 2023, https://data.buffalony.gov/Public-Safety/Crime-Incidents/d6g9-xbgu.

[12] City of Cincinnati, "PDI (Police Data Initiative) Crime Incidents," last accessed October 27, 2023, https://data.cincinnati-oh.gov/Safety/PDI-Police-Data-Initiative-Crime-Incidents/k59e-2pvf.

[13] City of Dallas Open Data, "Police Incidents," last accessed October 27, 2023, https://www.dallasopendata.com/Public-Safety/Police-Incidents/qv6i-rri7 ().

[14] City of Denver, "Open Data Catalog: Crime," last accessed October 27, 2023, https://www.denvergov.org/opendata/dataset/city-and-county-of-denver-crime.

[15] Houston Police Department, "Monthly Crime Data by Street and Police Beat," last accessed October 27, 2023, https://www.houstontx.gov/police/cs/Monthly_Crime_Data_by_Street_and_Police_Beat.htm.

[16] City of Milwaukee, "Milwaukee Open Data," last accessed November 22, 2023, https://data.milwaukee.gov/dataset/wibr.

[17] See, e.g., City of New Orleans Open Data, "Call for Service 2020," last accessed October 27, 2023, https://data.nola.gov/Public-Safety-and-Preparedness/Call-for-Service-2020/hp7u-i9hf.

[18] OpenDataPhilly, "Crime Incidents," last accessed November 22, 2023, https://www.opendataphilly.org/dataset/crime-incidents.

[19] City of Phoenix Open Data, "Crime Data," last accessed October 27, 2023, https://www.phoenixopendata.com/dataset/crime-data/resource/0ce3411a-2fc6-4302-a33f-167f68608a20.

[20] Portland.gov, "Portland Crime Statistics," last accessed October 27, 2023, https://www.portlandoregon.gov/police/71978.

[21] Raleigh Open Data, "Raleigh Police Incidents (NIBRS)," last accessed October 27, 2023, https://data-ral.opendata.arcgis.com/datasets/ral::raleigh-police-incidents-nibrs/about.

[22] Data used for this report has since been deleted due to an update in the city's crime reporting system. See City of Sacramento, "Sacramento Open Data," last accessed July 31, 2024, https://data.cityofsacramento.org/.

[23] Metropolitan [D.C.] Police Department, "crimecards.dc.gov," last accessed October 27, 2023, https://crimecards.dc.gov/all:crimes/all:weapons/8:years/citywide:heat.

[24] Brantly Callaway and Pedro H. C. Sant'Anna, "Difference-in-Differences with Multiple Time Periods," *Journal of Econometrics* 225, no. 2 (December 2021): 200–230, https://www.sciencedirect.com/science/article/abs/pii/S0304407620303948.

[25] Callaway and Sant'Anna, "Difference-in-Differences with Multiple Time Periods."

[26] Wylie and Grawert, *Challenges to Advancing Bail Reform*, 12.

[27] For more on the FBI's transition to the new National Incident Based Reporting System, see Weihua Li and Jasmyne Ricard, "4 Reasons We Should Worry About Missing Crime Data," *Marshall Project*, July 13, 2023, https://www.themarshallproject.org/2023/07/13/fbi-crime-rates-data-gap-nibrs; and Ames Grawert, "Analyzing the FBI's National Crime Data on 2022 — with an Eye Toward 2023 Trends," Brennan Center for Justice, October 18, 2023, https://www.brennancenter.org/our-work/analysis-opinion/analyzing-fbis-national-crime-data-2022-eye-toward-2023-trends.

[28] Paul Heaton, *The Effects of Misdemeanor Bail Reform*, Quattrone Center for the Fair Administration of Justice, August 16, 2022, https://www.law.upenn.edu/institutes/quattronecenter/reports/bailreform/#/.

[29] FBI, *Uniform Crime Reporting Handbook*, 2004, 46, https://perma.cc/8YZY-C3S7. For example, petit larceny, entailing theft of under $1,000, is a misdemeanor under New York law. N.Y. Penal Law § 155.25.