An Empirical Evaluation of the Impact of New York’s Bail Reform on Crime Using Synthetic Controls

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Abstract

We conduct an empirical evaluation of the impact of New York’s bail reform on crime. New York State’s Bail Elimination Act went into effect on January 1, 2020, eliminating money bail and pretrial detention for nearly all misdemeanor and nonviolent felony defendants. Our analysis of effects on aggregate crime rates after the reform informs the understanding of bail reform and general deterrence. We conduct a synthetic control analysis for a comparative case study of impact of bail reform. We focus on synthetic control analysis of post-intervention changes in crime for assault, theft, burglary, robbery, and drug crimes, constructing a dataset from publicly reported crime data of 27 large municipalities. Our findings, including placebo checks and other robustness checks, show that for assault, theft, and drug crimes, there is no significant impact of bail reform on crime; for burglary and robbery, we similarly have null findings but the synthetic control is also more variable so these are deemed less conclusive.

1 Introduction

The use of cash bail has come under increasing scrutiny in the United States, with several states and jurisdictions adopting or considering reforms to limit its use \cite{Covert2017}. One key question is whether such reforms – which generally lead to fewer pretrial detainees – impact crime rates. We address this question by investigating a real-life case study, New York’s implementation of a sweeping bail reform legislation which took effect on January 1, 2020. We focus on the impact...
on the incidence of crime of different types in New York City (NYC), using the synthetic control method [Abadie et al., 2010, 2011] to conduct comparative event-study analysis.

Specifically, our analysis takes advantage of the fact that cities across the United States historically experience similar shifts in crime trends [Barker, 2010], which allows us to compare the developments in NYC to a pool of control jurisdictions where no such policy changes were made within the timeframe of our study between January 1, 2018 and March 15, 2020. This cross-unit comparison aids causal inference by disentangling the true impact of bail reform from other concurrent events and trends, which would not be possible by conducting a within-unit comparison such as interrupted-time series [Degli Esposti et al., 2020], which we also conduct as a reference starting point. To facilitate our analysis, we collected data directly from 27 different cities’ police departments on the incidence rates of different types of crimes. Using the synthetic control method, for each crime type, we construct a weighted combination of these cities to match NYC before 2020. Roughly, comparing the divergence – or lack thereof – in 2020 allows us to assess the impact of the novel treatment in 2020. Overall, following statistical inference and robustness checks, yet given certain caveats and disclaimers inherent to the timeframe of our study as well as the nature of reported crime data that we detail in Section 6, we find little to no evidence of any significant impact of bail reform on total crime rates. The synthetic control is able to explain away observed trends in post-intervention crime rates, assessing the average treatment effect estimate against the variability in the distribution of effect estimates under placebo analyses with known null interventional effect. These results are robust to more or less complicated model specifications of the synthetic control and under other robustness checks. While previous studies focus more on the effects of bail reform on defendants’ detention and re-arrest rates, our analysis considers the broader policy implications of bail reform by focusing on crime rates, which may also account for factors such as detention’s potential deterrence effects on crime.

The structure of our paper is as follows: we first discuss the background, data, methods, and results, before summarizing limitations of the analysis and offering concluding remarks. We review the context of bail reform and of findings elsewhere as well as the specific changes under the reform passed in New York, and we highlight domain-level considerations that inform methodological choices for our empirical evaluation and our focus on a synthetic control evaluation. We then turn to describing data, methods, and our results: we describe our constructed dataset and pre-processing as well as our main analyses. We focus on synthetic control analysis of post-intervention changes in crime for assault, theft, burglary, robbery, and drug crimes. Our findings, including placebo checks and other robustness checks, show that for assault, theft, and drug crimes, there is no significant impact of bail reform on crime; for burglary and robbery, we similarly have null findings but the synthetic control is also more variable so these are deemed less conclusive. Finally, we discuss important limitations of our study in the operational context of New York before concluding by presenting directions for future research.

2 Background

2.1 Bail reform

As more U.S. jurisdictions consider reforms to end or sharply reduce the use of cash bail, debates about the impacts of such reforms have gained urgency. Key questions include the effects of such reforms on: (i) pretrial detention rates; (ii) pretrial rearrest rates; and (iii) overall crime rates. The
first two questions have received the most attention in the literature, as we discuss below. This study makes a unique contribution by focusing on the third question. Specifically, in assessing the effects on aggregate crime rates, we explore the broader consequences of bail reform, including potential consequences for general deterrence.

The earliest and best known example of bail reform in the U.S. is Washington, D.C., which in 1992 replaced cash bail with a risk-based system [Lockwood and Griffin, 2020]. Although no rigorous evaluations were conducted at the time, more recent descriptive statistics reveal a pretrial rearrest rate of 12-14%, which is broadly in line with that of other jurisdictions that still use cash bail [Advancing Pretrial Policy and Research, 2020]. In 2017, New Jersey implemented a raft of pretrial reforms, including: (1) mandating the use of a pretrial risk assessment tool, (2) restricting the use of bail to defendants at high risk of flight, and (3) restricting pretrial detention to defendants at high risk of either flight or new criminal activity. While no rigorous studies have yet been conducted on the impact of the reforms, descriptive analyses suggest that after the reform went into effect pretrial rearrest rates did not subsequently rise [NJ Courts, 2020].

In 2017, Philadelphia’s District Attorney announced that his office would no longer seek bail for 25 low-level felony and misdemeanor offenses. Using an instrumental difference-in-differences design to compare bail eligible offenses to bail ineligible offenses, researchers found that the policy had no impact on the likelihood of either pretrial detention or pretrial rearrest [Ouss and Steven-son, 2020]. Also in 2017, the Chief Justice in Cook County, Illinois, issued a court order (G.0. 18.8A), establishing a presumption of release for most defendants. Using multivariate models to compare matched samples of defendants pre- and post-reform, researchers found that the order was associated with a 4 percentage-point decrease in pretrial detention but no change in pretrial rearrest [Stemen and Olson, 2020]. Similar findings were described in an official report [Office of the Chief Judge, 2019], but another study of the same reform found that crime among individuals on pretrial release did indeed increase [Cassell and Fowles, 2020].

In 2019, in Harris County, Texas, a court-ordered consent decree (Rule 9) mandated the pretrial release of a subset of misdemeanor offenses. A descriptive analysis of 8 pretrial rearrest rates among misdemeanor defendants showed no change following the implementation of the order [Duke University School of Law, 2020]. In 2014, Mecklenburg, North Carolina, a new automated risk assessment tool was rolled out to replace an older risk assessment instrument. Using an interrupted time series design, researchers found that the implementation was followed by an 11 percentage-point decrease in money bail, and a 2 percentage-point increase in pretrial rearrest [Redcross et al., March 2019].

Outside of any reform context, research has also found that bail and pretrial detention are associated with a higher likelihood of rearrest or recidivism [Monaghan et al., 2020; Gupta et al., 2016; Lowenkamp et al., 2013]. Other research has highlighted pretrial detention’s harmful collateral impacts, including its impact on future employment and residential stability for arrested individuals [Dobbie et al., 2018; Heaton et al., 2017]. Additionally, bail and pretrial detention can also intensify racial disparities at later phases of the criminal legal process [Donnelly and MacDonald, 2018].

One concern about recent bail reform efforts is that releasing more high-risk defendants into the community could harm public safety. Most previous research has explored this question by focusing on bail reform’s effects on pretrial rearrest, as we discuss in the next section.

**Pretrial rearrest.** Access to data on pretrial rearrest rates is limited. At the national level, the best available data come from a 2009 Bureau of Justice Statistics study of the 75 most populous U.S.
counties. The study found that 16% of individuals facing felony charges were rearrested within 1
day, with rearrest rates highest for those initially charged with robbery (24%) and burglary (22%),
lowest for those charged with a driving-related offense (11%) and fraud (12%), and that one-half
of all rearrests were for a felony offense Reaves, 2013. At more local levels, where recent data is
available, studies have found pretrial rearrest rates ranging from 12% in Washington D.C. Angers,
2021, 12% for a new felony offense in California Tafoya, 2015, 14% in New Jersey NJ Courts,
2020, 17% in Cook County Stemen and Olson, 2020, and 17% among defendants charged with a
felony in NYC NYPD, 2020.

What factors are associated with pretrial rearrest? Much of the research on this question has
been conducted as part of the development of pretrial risk assessment tools, typically involving the
use of multivariate modeling techniques. One of the strongest predictors identified is the length of
the pretrial period: the more time spent in the community, the higher the chances of rearrest Clarke
et al., 1976, Siddiqi 2003. Prior criminal history has also been shown to be a strong predictor,
including prior arrests Advancing Pretrial Policy and Research, 2020, Hickert et al., 2013, prior
misdemeanor convictions Advancing Pretrial Policy and Research, 2020, Siddiqi 2003, prior felony
convictions Advancing Pretrial Policy and Research, 2020; Clarke et al., 2007; Pretrial Justice Institute,
2012; Siddiqi, 2005; prior violent convictions Advancing Pretrial Policy and Research, 2020, prior
property-related arrests Hickert et al., 2013, other pending charges Clark et al., 2007, Siddiqi 2003,
and prior failures-to-appear Clark et al., 2007 Siddiqi 2003. Other predictive factors include
age Clark et al., 2007, Hickert et al., 2013, Siddiqi 2003, gender Siddiqi 2003, employment status
Clark et al., 2007, Siddiqi 2003, and residential stability. Finally, the type of release at arraignment
can affect the likelihood of pretrial rearrest; specifically, individuals detained at arraignment are less likely to be re-arrested pretrial Dobbie et al., 2018, Leslie and Pope, 2017.

As we have seen, pretrial rearrest rates can vary from as low as 11% to as high as 24%, depending
on the population of defendants and how rearrest is measured. And research has revealed a variety
of factors associated with higher risk of rearrest during the pretrial period, including time at risk,
criminal history, age, gender, employment status, residential stability, and whether a defendant was
detained at arraignment.

Bail reform in New York. To assess bail reform’s impacts on crime and rearrest, we investigate
the effects of New York State’s Bail Elimination Act, which went into effect on January 1, 2020.
Other reports with more descriptive detail and statistics include those of Rempel and Rodriguez
2020 and Rempel and Weill 2021. We now overview the main aspects of the reform. The reform
“eliminates money bail and pretrial detention for nearly all misdemeanor and nonviolent felony
defendants,” among other policy changes, such as directing judges to consider ability to pay when
setting money bail amounts Rempel and Rodriguez, 2019, p. 1. Prior to its enactment, judges in
New York had the option to set bail or remand in all cases—regardless of the charge a defendant
was facing. The new law proscribes money bail in almost all cases charged with a misdemeanor
(except for sex offenses and domestic violence, which make up about 12% of those charged with
misdemeanors) and disallows remand for misdemeanors Rempel and Rodriguez, 2019, p. 2. For
felonies, in summary, the new criteria where both money bail and remand are possible “permit bail
and detention with nearly all violent felonies but rule it out with nearly all nonviolent felonies”
Rempel and Rodriguez, 2019, p. 2. In effect, this meant that pretrial release was mandated in
nearly all misdemeanor and nonviolent felony cases on the basis of a single factor: the present
charge in a case.

We highlight some descriptive statistics which shed light on magnitudes of differences in charge eligibility under the reform. In 2019, “collectively, judges ordered bail or remand in 7 percent of misdemeanors, 35 percent of nonviolent felonies, and 62 percent of violent felonies.” [Rempel and Rodriguez 2019 p. 9]. Of the cases made bail-eligible, judges ordered bail or remand in 54% of the cases, and of the cases made ineligible for bail, judges used bail or remand 12% of the time. As may be expected, there is heterogeneity in bail-setting based on charge severity: in 2019, money bail was set in 57% of violent felony offenses, 35.1% of non-violent felony offenses, and 7% of misdemeanors [Rempel and Rodriguez 2019 p. 9]. Where judges ordered bail or remand in 2019, in about 13,000 cases they would be unable to do so under the original reform; the greatest difference is in non-violent felonies [Rempel and Rodriguez 2019 p. 10].

Partly due to concerns related to public safety, six months after the law went into effect, on July 2, 2020, it was amended to expand the eligibility criteria [New York Senate 2019, New York State 2020]. The amendments included a list of newly eligible misdemeanor and non-violent felony charges, as well as broader eligibility criteria related to a defendant’s criminal history, including whether a defendant had a prior history of felony convictions, a separate pending case, or was on probation or parole. These changes effectively ended New York’s charge-based approach to bail reform, replacing it with a second bail reform regime in which bail eligibility was determined based on the present charge or a defendant’s criminal history.

**Our study: effects on aggregate crime and general deterrence.** Our analysis will focus on effects on crime rates measured in NYC, and is complementary to studies of re-arrest rates (which are inherently more complicated in the case of New York’s reform due to the near-immediate onset of COVID afterwards). This study makes a unique contribution by focusing on bail reform’s impacts on general crime rates; in assessing the effects on aggregate crime rates, we explore bail reform’s consequences for general deterrence, an important but underexamined issue in empirical studies of bail reform. As [Yang 2017 p. 1451] has observed, general deterrence may be an “unmeasured” benefit of pretrial detention, as individuals may be dissuaded from committing potential offenses due to the risk of being detained before trial. In utilizing aggregate-level crime statistics as our outcome of interest, we build on a substantial criminological research tradition that explores the general deterrence effects of sanctions or other policies (see discussion in Paternoster 2010).

### 2.2 Criminological considerations for empirical evaluation

We first discuss the backdrop of underlying national crime trends, against which any observed increase in crime would have to be compared in order to draw qualitative conclusions attributing increases to the bail reform intervention itself. Such national crime trends reflect observed and unobserved confounders affecting adoption of bail reform and crime. We review the criminological literature which discusses these shared confounders: although there is qualitative consensus for the relevance of overall national trends, such trends may arise from a number of mechanisms and hence are hard to measure directly. The backdrop of national crime trends is important for assessing changes in crime rate and understanding challenges in disentangling national trends from effects of the bail reform in New York.

Although crime is largely a local phenomenon [O’Brien et al. 2021, Cook and Winfield 2013, Sampson 2008], there are overall national trends that emerge from a combination of various micro and macro level factors [Barker 2010]. For example, the great crime decline that began in the
1990s and continued well into the current century was likely effected by an interplay of an expanding economy, increased incarceration and more effective policing, demographic shifts [Barker, 2010, Zimring, 2007, Baumer, 2008, Blumstein and Wallman, 2006, Donohue and Levitt, 2001, Donohue, 2017], urban development [Schwartz, 1999, Branas et al., 2011], changes in youth culture [Curtis, 1998, Ouimet, 2002], shifts in illegal drug use and distribution [Donohue, 2017], immigration [Rumbaut and Ewing, 2010, Nevin, 2007, Reyes, 2002, Stretesky and Lynch, 2004, Needleman, 2004, Gould, 2009, Sampson and Winter, 2018]. However, there is no consensus on the most important causes or exact mechanisms behind the crime drop [Barker, 2010]. Moreover, certain explanations proposed by some scholars have been disputed by others, such as the significance of the role of incarceration and policing [Zimring, 2007, Roeder et al., 2015, Ouimet, 2002] or the impact of legalized abortion [Donohue and Levitt, 2001, Blumstein and Rosenfeld, 2008, Baumer, 2008, Ouimet, 2002].

Similarly, there is a lively debate over the causes of the unexpected increase in violent crime that started in 2015 [Rosenfeld, 2016, Sharkey, 2018, James, 2018, Donohue, 2017] or the sharp uptick in murder in 2020 [Campbell, 2021]. In contrast to the 1990s crime drop, these recent trends are unique to the United States [Weiss et al., 2016, Farrell et al., 2014, Ouimet, 2002, Jilani, 2021, Lee] and limited to violent offenses, particularly homicide, while property crimes continued to decline [Donohue et al., 2019, Rosenfeld and Wallman, 2019, Rosenfeld and Lopez, 2020].

There is a general notion that the COVID-19 lockdowns and widespread social unrest triggered the rise in violence [Jilani, 2021, Cassell and Fowles, 2020, Li, 2020], but it is possible that the unusual events of 2020 merely exacerbated an upward trend that has been in the making for a while [Donohue, 2017]. That is, the recent reversal of the downward trajectory in violent crime may be due to a combination of several longstanding trends, including a drop in incarceration rates, declining police rates, emerging illegal drug markets, and an increase in out-of-wedlock births. Even though these patterns have emerged gradually over several years, they could have ultimately had a discontinuous impact on violent crime [Donohue, 2017]. The seemingly paradoxical fact that property crime rates continued to fall may be the consequence of the strong economy, technological progress (for example, cashless payment methods and the lower cost of goods reduced the incentives to commit robberies or burglaries), or underreporting [Donohue, 2017].

To be sure, the pandemic was accompanied by several developments that could be plausibly linked to crime, such as increased economic hardship [Parolin et al., 2020], decarceration [Porter, 2020], an uptick in gun purchases [Schleimer et al., 2021] and gun carrying [Arthur and Asher, 2021], and halted non-policing efforts to prevent violence [Paterson, 2020, Badger and Bul, 2020]. However, except for one study, which found a positive association between the recent increase in firearm sales and gun violence [Schleimer et al., 2021], thus far there is no rigorous empirical evidence that these factors caused an additional spike in homicide rates.

Likewise, the impacts of the Floyd protests on crime rates are still unclear. Despite the perception that defunded police forces led to the crime increase [Elinson et al., 2021, Yokley, 2021], police budget cuts were in fact only modest and not universal [Akinnibi and Cannon, 2021]. Moreover, violence also went up in jurisdictions that increased their police spending [Jacob, 2020, Abusaid and Boone, 2021, Angers, 2021]. Public trust in law enforcement eroded in the wake of police misconduct and social unrest [Jacob, 2020, Reny and Newman, 2021], which could have suppressed proactive policing and in turn increased crime [Mazerolle et al., 2013, Desmond et al., 2016, Goldberger and Rosenfeld, 2008, Rosenfeld and Wallman, 2019, Campbell, 2021]. However, it has yet
to be shown whether this was the case in 2020.

In summary, despite the controversies and uncertainties surrounding the exact causes behind broad shifts in crime incidence, there are undoubtedly forces at play that affect jurisdictions across the country in similar ways.

On the other hand, criminological background on mechanisms of crime, such as crime specialization, can inform our assessment of the impact of New York’s bail reform, which changed the incidence of bail for some crimes more than others. Differences in charge eligibility across crime types, in combination with implications of the research on crime specialization, could predict heterogeneity in possible effects of bail reform on different types of crime. Research on crime specialization seeks to understand whether people tend to commit different types of crimes, for example during life course analysis. MacDonald et al. [2014] note that recent research suggests “that there are meaningful differences in the tendency for criminally active persons to repeat violent offenses compared to other offense types”. Applying this mechanistic insight to this treatment effect evaluation context, we might have expected that if bail reform led to a situation where more defendants who were charged with a violent felony were now on pretrial release, then we might observe more violent offenses, due to observed patterns of crime specialization. However, as described above, bail reform did not affect eligibility for pretrial release for violent felony charges, although it led to major changes in eligibility for non-violent felonies and misdemeanors.

2.3 Context of our evaluation

**Our evaluation of bail reform.** This study represents the first attempt to evaluate the effects of the bail reform of New York State’s Bail Elimination Act on aggregate crime rates. Rempel and Rodriguez [2020] examined the impact of bail reform in NYC on the use of money bail and the potential impact on pretrial detention and the pretrial jail population.

We briefly highlight the role that our outcome of interest, aggregate crime rates, plays in the broader substantive context of assessing the incapacitating, specific deterrence, or general deterrence effects of bail. In relation to arguments made or implicit in other analyses of bail reform Monaghan et al. [2020], Ouss and Stevenson [2020], Stemen and Olson [2020], if the bail reform can expand the released population without observing increased re-arrest rates, then the bail reform does not incur social costs in the form of significantly increased crime, presumably because those released are of marginal risk of rearrest.

On the other hand, increasing the released population may generally be expected to increase the absolute magnitude of re-arrests, since bail (via detention) has incapacitative effects. Setting monetary bail may also pose financial incentives to prevent reoffense, leading to specific deterrence. The recent analysis of Albright [2021] studies the question of financial incentives in a different context.

Therefore, bail reform can introduce societal costs. If reducing incapacitative effects and financial incentives leads to an increase in crime via specific deterrence or via general deterrence, assessing the absolute magnitude is relevant to appropriately weigh such tradeoffs against other important arguments for bail reform. Such arguments include the distributional consequences on detention for those unable to make money bail, civil liberties, racial and ethnic disparities, constitutional and moral arguments, and other important policy considerations. Dobbie and Yang [2021] overviews these considerations balancing individual rights and public interests. On the other hand, it could also be the case that bail reform does not increase aggregate crime (does not reduce general deterrence), so that these other individual-level benefits of bail reform do not incur societal costs.
Methodologically, we use the synthetic control method to assess causal impacts of bail reform, which has been used in other settings to assess the impacts of policy (including on crime) [Saunders et al., 2015, Donohue et al., 2019, Rees et al., 2019, Ben-Michael et al., 2021a].

The challenges of evaluating bail reform, even in the aggregate. Bail reform is hard to evaluate due to (i) confounders (other changes that take place at the same time leading to cross-unit unobserved confounding); (ii) data limitations and difficulties of model specification; and (iii) the uniqueness of particular case studies (findings that hold true in NYC may not hold true elsewhere and therefore are not representative of bail reform’s generalized effects).

Our analysis studies the question: did bail reform increase crime (as measured by reconstructed index crime) in NYC, relative to shared co-movements in crime across the nation? Synthetic control analysis allows us to compare NYC’s crime trends against the crime trajectories of the constructed “counterfactual” NYC that is statistically constructed to emulate the city’s trajectory before the intervention. Later on we will discuss methodological choices to consider both interrupted time series (within-unit comparison) and synthetic control (across-unit comparison).

Aggregate data analysis on crime incidence: synthetic control. The effects of bail reform on general deterrence (e.g., possibly reducing deterrence due to limiting cash bail) could impact crime rates in the broader, not-yet-arrested population. But, simply comparing before-and-after crime rates can be misleading due to broader shifts in crime incidence (for example due to federal policy, aforementioned mechanisms discussed in the previous section driving crime rates, concurrent shifts at the municipal level). In order to assess this, we aggregate data from municipal police departments in order to construct a synthetic control [Abadie et al., 2010]. The synthetic control reweights the non-NYC municipal police departments in order to track NYC’s pre-intervention trajectory. Divergence in outcomes between NYC and the synthetic control (weighted composite of other cities) allows for a comparative analysis that uses the synthetic control to account for shared aggregate-level changes in crime rates. We include more detail on dataset construction in Section 3 and the synthetic control method in Section 4.

3 Overview of data

Description of data. We construct a dataset by compiling crime data released by police departments across 27 cities across the United States from the period Jan 1, 2018 - Mar 15, 2020 for the purpose of establishing a synthetic control for NYC crime trends. Although the main analysis is synthetic control, we also conduct an interrupted time series analysis with only New York City’s data starting a year prior, from Jan 1, 2017, as we are less restricted by ensuring data availability across all municipalities for interrupted time series.

We construct the dataset from primary sources as standard aggregate datasets, such as that provided by the FBI Uniform Crime Reporting (UCR) program, are insufficient for our study. First, at the time of our study UCR data was unavailable for 2020. Moreover, UCR data reporting is at the monthly level, which would result in only three post-intervention timepoints at the timescale of our post-intervention, pre-COVID window, muddying any inference. In contrast, constructing the dataset at a daily reporting level allows for greater flexibility and improves the precision of our estimates and inferences.
Reported incidents of crime were obtained from police department-reported incidents accessible via data portals; see Table 20 for the list of sources. These cities were chosen based on population size and public crime data availability: we assessed the list of cities in decreasing order of population, and downloaded data when it was available for the 30 most populous cities, ending up with 27 cities with available crime reporting data after omitting some due to significant reporting discontinuities in the data (see below).

Our use of such incident report data comes with standard disclaimers regarding crime data and potential measurement issues: see Kaplan [2020] for extensive discussion of particularities with this type of data. For our comparative case study, measurement biases that do not differ across units do not affect analysis conclusions.

Dataset overview. Each row in this dataset corresponds to an incident of crime. The dataset was constructed by merging the reported incidents from each agency. However, each agency reports crime types in a slightly different way, via different descriptors and categorizations. Some agencies, but not all, report UCR codes, but at different granularities. We therefore code crime types and construct a schema that broadly resembles UCR’s index crime designations with some important adjustments to account for heterogeneous reporting across cities.

In Figure 9 we describe the hierarchy of crime types, that is, the schema to which we normalize the crime data reported by police departments, which is reported at varying granularities. Level 1 of the hierarchy describes broad categorizations of crime types: violent, drug, property crime, gambling (and other). Level 2 includes further categorization: drug, robbery, rape, assault, homicide, theft, other property crime, arson, burglary, white collar, and gambling. Level 3, which we do not use, describes further subtypes, for example aggravated vs. simple assault or residual vs. non-residential burglary. We conduct our analysis at the second level of categorization. Of these crime types, the drug, robbery, assault, theft, burglary crime types had sufficient event frequency to conduct the analysis.

Treatment intervention dates. Bail reform went into effect on January 1, 2020, which we use as the intervention date. However, the post-intervention period is necessarily abbreviated due to COVID lockdowns and COVID-related changes which introduce a large degree of nonstationarity. In NYC, the lockdown for COVID went into effect on March 15, 2020 which we use as the end of the post-intervention period. A partial rollback of bail reform rollback went into effect in New York on July 2, 2020, altogether outside our post- or pre-treatment windows.

Dependent variables of interest. We investigate crime types corresponding to major categories on level 2 of the hierarchy in Figure 9: violent crimes (robbery, rape, assault, homicide) and property crimes (theft, burglary).

Treated vs. control units. In our analysis, NYC is the single treated unit. We briefly discuss whether control units also experienced (a version of) treatment via implementing bail reform. Of the locations that recently passed bail reform, the locations either passed bail reforms strictly before the start of our synthetic control timeframe on January 1, 2018 (or in the case of Atlanta, were excluded for other data inconsistency issues). Cook County, which contains Chicago, passed General Order 18.8A (GO18.8A) on September 17, 2017, which required Cook County bond court judges to maintain a presumption against requiring the defendant to pay money to achieve release Office.
of the Chief Judge, 2019. Although these reforms were passed preceding the synthetic control timeframe, any dynamic treatment effects of these prior reforms could be reflected in aggregate time series.

Preprocessing steps. We removed Atlanta and Fort Worth because of data quality reporting issues: due to changes in reporting scheme, the observed time series has a large discontinuity. Fort Worth and Houston both moved to NIBRS reporting in 2018 which aligns with the anomalies for those cities. Kansas City also moved from encoding with UCR codes to NIBRS descriptions in 2019; there also appears to be a data changepoint in the series in that time range.

Differences from UCR Part 1 Index crimes. This study was conducted before the 2020 UCR data were available. The most important practical difference is that our dataset is collected and recorded at a higher (daily) frequency than the monthly frequency of UCR data. Since our post-intervention period is only three months long, this is important to the reliability of our analysis.

We discuss how our crime categorization differs from UCR Part I index crimes. For the most part, descriptions of the broad crime categories align with UCR, although there are differences. The largest discrepancies are in the theft and assault categories. Assault differs the most from UCR designations and includes non-felony assault (simple assault) since some jurisdictions do not differentiate reports between felony/non-felony assaults. Our “theft” designation includes both felony and misdemeanor crimes. In Appendix C.2 we include a full description of the crime descriptors reported in NYC (offense description, law description, and police department descriptions) describing which crimes are aggregated to our final categories.

While many cities have switched to NIBRS reporting, where multiple crime types can be reported from a single incident, some cities may report based on UCR Traditional Summary reporting, where only the most severe crime type is reported per incident. Our dataset is simply an aggregation of each city’s crime regardless of reporting schemes, so some cities may appear to have more crime than others based on this difference in reporting. That is, we did not manually align reporting across cities according to the hierarchy rule. Moreover, all cities do not report crime types at the same granularity of description (assault vs. aggravated/simple/unclassified assault).

To give a sense of possible discrepancies in frequencies due to reporting with or without the hierarchy, note that an analysis of the NIBRS vs. UCR Summary Reporting system submissions from 1991 to 2011 shows that changing to NIBRS reporting, which allows multiple offenses, results in no effect on rapes, 0.5% increase in robbery, 0.6% increase in aggravated assault, 0.8% increase in burglary, 3.1% increase in larceny, and 3.8% increase in motor vehicle theft [Rantala and Edwards, 2000]. Only 9.2% of reports contained more than one offense per incident. Because our analysis does not directly require comparing magnitudes of crime across different cities, we expect this does not affect our results.

To summarize, the dataset includes index crimes with some categories expanded to include lower-level offenses (e.g. simple assault), along with other non-index crime types (e.g., drug offense). Aligned with our goal of assessing broader deterrence effects, some categories also include misdemeanors.
4 Methods

We start with a within-case comparison via interrupted time series (ITS), evaluating NYC’s crime rates before and after the intervention, accounting for NYC’s own temporal trends. This has many limitations for studying the impact of bail reform. We then conduct a more credible comparative case study via the synthetic control methodology. We argue there are two key challenges to interrupted time-series in this setting: anticipation effects and co-movements in underlying trends (unobserved confounders). After describing the ITS methodology, we briefly describe the methodological benefits of synthetic control over ITS, and then we describe the synthetic control methodology in greater detail.

4.1 ITS analysis

As a starting point for the analysis, we conduct an ITS analysis \cite{Bernal et al., 2017}. Due to autocorrelation of the time series and seasonal effects, we adjust for autocorrelation via segmented regression with autoregressive integrated moving average models (ARIMA) time series regression \cite{Brockwell et al., 2016}. We first adjust for seasonality via seasonal fixed effects and consider ARIMA\((p,d,q)\) models, where \(p,d,q\) are positive integers. These denote: \(p\) the order of the autoregressive (AR) part of the model, \(d\) the degree of backwards differencing to achieve stationarity, and \(q\) the order of the moving average (MA) part of the model.

In an ARIMA model, \(Y_t\) satisfies a difference equation of the form

\[
\phi^*(B)Y_t \equiv \phi(B)(1 - B)^dY_t = \theta(B)Y_t, \quad \{\epsilon_t\} \sim N(0, \sigma^2),
\]

where \(B\) is the backshift differencing operator, \(\phi(z), \theta(z)\) are polynomials of degree \(p\) and \(q\), respectively, and \(Z_t\) are white-noise innovations \cite{Brockwell et al., 2016}. The interrupted time series design, in addition to the generic ARIMA specification, also includes treatment indicators, i.e., an additional covariate with \(\mathbb{I}(t > t_{\text{int}})\), where \(t_{\text{int}}\) is the time of the intervention, or a dynamic treatment effect specification with \(\mathbb{I}(t > t_{\text{int}})(t - t_{\text{int}})\). We will discuss how our findings are robust to regressing against time indicators or time since intervention.

4.2 Within-unit vs. across-unit variations: ITS to synthetic control

We briefly describe the benefits of synthetic control to other event-study methods. Relative to comparative case study approaches that would require choosing a comparator municipality, using synthetic control allows for flexible construction of a comparison, based on fitting the trajectory of crime rates before the intervention. This is helpful because the cities themselves differ on a number of factors (seasonality patterns, opportunities for different types of crime, etc.) so that manually choosing a control comparator is not self-evident in this setting.

Relative to a within-NYC time series analysis, synthetic control allows for accounting for possible co-movements in underlying crime trends (for example, changes in federal policy or broader economic trends) with other non-treated units; the possibility of such trends is discussed in Section 2. Further, relative to interrupted time series, comparative interrupted time series, or differences-in-differences analysis, an important consideration in favor of synthetic control is that it compares the post-treatment time period to a longer pre-treatment time period than would occur otherwise, under segmented regression used for the previous methods. At a domain-level, the crucial assumption of no-anticipation is violated. In our setting, no-anticipation presumes that outcomes do not change.
due to anticipation of the actual intervention date. However, anticipation was already quantitatively documented by [Center for Court Innovation, 2020] with increased rates of supervised release in the weeks and months preceding the January 1 implementation of the reform. This is intuitively qualitatively plausible: judges exercised discretion to avoid the administrative hassle of processing someone for bail or detention when they would have been released a few weeks afterward under the new reform regime.

4.3 Synthetic control: background

The original synthetic control method [Abadie et al., 2010] uses outcome predictors in order to balance a weighted distance between predictors of treated unit and predictors of control units. We first introduce the terminology and notation of the synthetic control, before describing the variants we introduce following the literature.

Index the units (here, municipalities) as $j = 1, 2, \ldots, J + 1$ and associate the first index, $j = 1$, to the treated unit (NYC). The possible “donor pool” is a collection of untreated units, $j = 2, \ldots, J + 1$, not affected by the intervention (bail reform). The data comprises $T$ time periods, with $T_0$ of these periods occurring before the intervention. For each unit and time period, an outcome $Y_{jt}$ is measured (aggregate crime level within a category). Let $Y_{jt}(1)$ denote the potential response under the treatment (bail reform intervention) and $Y_{jt}(0)$ otherwise, that is, under control. The effect of the intervention of interest for the treated unit at some time $t > T_0$ is

$$\tau_{1t} = Y_{1t}(1) - Y_{1t}(0).$$

Of course, the fundamental problem of causal inference is that for any time $t > T_0$ we only observe $Y_{1t}(1)$, not the post-intervention outcome without the intervention.

The synthetic control method constructs an estimate for $Y_{1t}(0)$ as a reweighted average of donor units’ outcomes, $Y_{jt}(0)$, for whom we do observe outcomes under non-intervention after the intervention time. The synthetic control estimate is specified via a weight vector $W = [w_2, \ldots, w_{J+1}]$ that is used to construct a weighted average of donor units (the synthetic control). We optimize for the synthetic control weights minimizing the predicted mean-square error for pre-intervention outcomes $Y$ plus a ridge regularization:

$$w^* \in \arg \min \left\{ \frac{1}{T_0} \sum_{t \leq T_0} \left( Y_{1t} - \sum_{j=2}^{J+1} w_j Y_{jt} \right)^2 + \lambda \|w\|_2^2 : \sum_{j=2}^{J+1} w_j = 1 \right\}$$

(1)

The above program is easily solvable as a convex quadratic program. Then the synthetic control estimates of the counterfactual and treatment effect are, respectively:

$$\hat{Y}_{1t}(0) = \sum_{j=2}^{J+1} w_j^* Y_{jt}, \quad \hat{\tau}_{1t} = Y_{1t}(1) - \hat{Y}_{1t}(0)$$

**Synthetic control: specification.** Our specification differs somewhat from Abadie et al. [2011] and follows some adjustments that are common in the following literature. We implement synthetic control with unlagged outcomes and without covariates. For better interpretability and to avoid hyperparameter tuning, we do not consider re-weighting mean-squared errors as in the original paper, i.e., a reweighting hyperparameter across donor units. We include a ridge regression penalty,
following a suggestion of Abadie et al. [2015], and allow negative weights. Doudchenko and Imbens [2016] provides an extensive discussion of relaxing some of the restrictions of Abadie et al. [2010]. We expect the signal-to-noise ratio of predicting crime rates from aggregate-level covariates would suggest that balancing only on covariates would provide a poor pre-treatment fit, while including both covariates and outcomes is redundant [Kaul et al., 2015].

There is an active methodological literature on synthetic control which we do not further summarize here. Synthetic control can be understood as identification with an underlying linear factor model Abadie et al. [2010, 2011], errors-in-variables Agarwal et al. [2021], and/or outcomes-balancing points of view Ben-Michael et al., 2021b.

Data processing Due to the widely varying absolute magnitudes of crime in different cities — since NYC is the most populated municipality — we normalize the crime series in each city by the city’s population (not metropolitan statistical area). Similarly, Abadie et al. [2010] considers per-capita smoking rates. We also demean the trends in order to build the synthetic control weights, subtracting the pre-treatment average for each city. This normalization is crucial for our permutation-based placebo checks down the line, as we assess the magnitude of the treatment effect for NYC relative to the magnitude of the treatment effect assessed under placebo checks (more details in Section 5).

5 Results

We first consider an interrupted time series analysis of NYC to give a sense of pre- vs. post-intervention crime trends. Our primary analysis using the synthetic control method follows after, where we assess uncertainty via placebo checks and other robustness checks that are common in the literature.

5.1 Interrupted-time-series analysis

We first consider an interrupted time series analysis for NYC as a within-unit case study to assess impacts of bail reform on crime rates. Because of strong temporal trends in crime rates, both seasonal within a year and within a week, and across years, interrupted time series analysis allows us to assess an increase or decrease in crime rates after the intervention in the context of decreases or increases in crime rates over time (due to seasonal fixed effects or year-over-year trends). We collect data from January 1, 2017 to March 15, 2020.

We adjust for time series structure by an ARIMA specification and with day-of-week and month fixed effects to adjust for seasonal non-stationary in each time series. We use the Hyndman-Khandakar algorithm, i.e., using unit root tests and AIC comparisons to perform model selection of the number of AR and MA terms. We include a dummy variables for “holiday,” including national holidays, New Year’s Day, and Halloween. Due to data sparsity we omit the homicide and shooting time series and conduct the analysis for the same categories as we do for the synthetic control: Assault, Theft, Drug, Burglary, and Robbery.

In Table 2 we include descriptive statistics of the crime time series. The Assault and Theft crime time series have the highest magnitude at 247 and 374 average daily cases from 2017-2020.

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1 We use a longer time frame than we use for the synthetic control in the next section, where a shorter time frame allows us to compare against a larger number of different units.
Drug, Robbery, and Burglary have an order of magnitude lower average incidence at around 45, 31, and 36 average daily cases, respectively.

We report the regression table in Table 4 across models over the ARIMA models of different orders for each crime time. The treatment effect estimate is given by the regression coefficient in the ARIMA regression denoted by “t”. The table also reports the AR/MA and drift terms found via model selection. While model selection does not suggest a time series model for the drug time series (after adjusting for day of week, month, and year fixed effects), for other crimes the specification varies. This analysis is conducted in terms of absolute number of crimes. The regression coefficient on the treatment intervention indicator (that is, only testing for a level change in crime rates) is $-66.03 (556.13)$, $13.78 (1042.05)$, $12.91 (39.12)$, $25.28 (125.57)$, $-2.88 (53.05)$, respectively, with standard errors in parentheses. Note the large standard errors. Figure 1 displays the figures to provide a visual aid of the high variability of the data. While Table 4 reports as a visual aid unadjusted significance at 0.001, 0.01, 0.05 levels, we also adjust for multiple hypothesis testing via the Holm-Šidák correction. Without multiple adjustment, the effect on violent assault (with a p-value of $4.31 \times 10^{-3}$), burglary (with a p-value of $3.9 \times 10^{-2}$), and drug crimes (with a p-value of $2.41 \times 10^{-2}$) would be significant at a 0.05 significance level. After multiple-adjustment, only the (negative) effect on violent assault is statistically significant. However, since a decrease in crime in bail reform is not consistent with the mechanisms and taken together with the limitations discussed in Section 4.2 overall the results do not suggest a change – and in particular any positive increase – in crime due to treatment after adjusting for multiple comparisons.

Homicide time series. While we do not study homicide using synthetic controls due to its prohibitive sparsity (0.841 daily homicides on average), we do conduct an ITS analysis on homicide given its criminological significance. Because of the sparse and discrete nature of the time series, we use a generalized-linear model version of ARIMA adjustment for Poisson outcomes. We report the results in Table 5. The results suggest a statistically insignificant increase in post-intervention homicides. We report results under two specifications: either level changes (treatment being the indicator variable of post-treatment intervention) or level and slope change (including additional interaction with treatment indicator and number of days post-intervention). The findings are qualitatively the same across specifications.

5.2 Synthetic control

We first describe data pre-processing steps used to ensure quality of the synthetic control fit, as well as describe our use of placebo checks common in the synthetic control literature to quantify the uncertainty in the treatment effect estimate, before describing the inferential results.

Our preferred specification includes ridge regularization. As robustness checks we also consider the non-regularized linear regression specification and the robust synthetic control (RSC) method of Amjad et al. [2018], which introduces a denoising preprocessing step before running the linear regression. These alternative specifications either decrease or increase the methodological complexity. Non-regularized linear regression is closer to the original synthetic control method. Robust synthetic control introduces additional denoising but introduces an additional hyperparameter (number of singular values) that the analysis is sensitive to.

While we focus on presenting results on the preferred specification, at the end of this section, we summarize findings across specifications, which overall agree qualitatively.
Preferred specification: Ridge regression on (unlagged) outcomes. Our preferred specification follows the suggestion of Abadie et al. [2015] by adding a ridge penalty. We still require the weights to sum to 1. We range the penalty, $\lambda$, on a logarithmically spaced grid of 100 values from $10^{-8}$ to $10^{-2}$. (We determined the grid size by expanding the boundaries until the optimal $\lambda$ penalty value was in the interior). We use the first 80% of pre-treatment data for learning the weights and choose $\lambda$ based on best (pre-treatment) performance on the held out the next 20% of pre-treatment data.

Treatment effect estimand. We consider estimates of an average treatment effect by averaging post-intervention outcomes to estimate the treated outcome and estimating the control outcome for NYC by reweighting with the synthetic control weights:

$$\hat{\tau} = \frac{1}{T - T_0} \left( \sum_{t=T_0}^{T} Y_{1t} - \sum_{j=1}^{J+1} w_j^* Y_{jt} \right) \quad (2)$$

Choice of metrics Out of the crime types at level 2 of the data hierarchy, we ultimately analyze Assault, Theft, Drug, Robbery, and Burglary crimes. Assault and Robbery are categorized as violent crimes while Burglary and Theft are categorized as property crimes.

We analyze Assault and Theft on a weekly aggregation and Drug on a bi-weekly (every two weeks) aggregation. This is because the individual time series are highly variable for other crimes (burglary, homicide, rape, and robbery), to the extent that the signal-to-noise ratio is quite low. When the pre-treatment fit is very poor, the synthetic control is uninformative — even a $p$-value that does not reject the null hypothesis could otherwise be explained away by highly variable pre-treatment fits on the placebo synthetic controls, despite a qualitative difference in the predicted and observed time series. Since the main quantitative tool for making substantive conclusions is uninformative, we do not study each of these outcomes alone using synthetic controls.

Placebo checks in units: ATE and RMSE test statistics We conduct a placebo check by assigning each other city as the treated unit, then recomputing a synthetic control from the other cities. We follow the suggestion of Abadie et al. [2011] to compute the distribution of placebo effects, restricting attention to those cities where good pre-treatment fit is possible, relative to the original synthetic control. We consider two-sided tests of the weak null. Following a suggestion of Abadie et al. [2011], we therefore, for every outcome, compute the placebo distribution from cities with pre-treatment fits no more than 7.5 times that of the original synthetic control.

One test statistic that we assess via the placebo test is the average treatment effect estimate (ATE) defined in Eq. (2). We then consider the $p$-value of the ATE measured for NYC with respect to the distribution of ATEs from the placebo checks. Namely, we set the $p$-value to $1 - F(\hat{\tau})$, where $F$ is the cumulative distribution function of the placebo ATE estimates and $\hat{\tau}$ is the ATE estimate measured for NYC.

Another test statistic is the RMSE (root mean-squared error) statistic used in Abadie et al. [2011]. The statistic $r$ is given by the ratio of post-treatment RMSE and pre-treatment RMSE:

$$r = \frac{1}{T - T_0} \sqrt{\frac{\sum_{t=T_0+1}^{T} (Y_{1t} - \hat{Y}_{1t})^2}{\sum_{t=0}^{T_0} (Y_{1t} - \hat{Y}_{1t})^2}} \quad (3)$$
We can similarly compute placebo RMSE statistics by permuting the identity of the treated unit and the p-value as the cumulative distribution function of the primary RMSE statistic among these placebo values. Relative to the more interpretable ATE test statistic, the RMSE test statistic normalizes by the pre-treatment fit under a certain placebo. If a placebo unit, when considered as the treated unit, achieves a poor pre-treatment fit, then any extreme differences in post-treatment contrasts, which may rightly be attributed to the poor pre-treatment fit, are made less extremal in the placebo distribution by the normalization.

**Placebo checks in time** Another placebo check suggested in [Abadie et al. 2011] considers changing the intervention time and verifying estimation of a null effect. We change the intervention time to an earlier date, and re-run the analysis considering the period from the new intervention time to the old intervention time as the placebo post-intervention period. We expect to verify that the synthetic control analysis suggests a null effect for these redefined post-intervention times, which are in-fact all previous to the actual implementation time.

**Robustness checks: early roll-in of intervention to assess anticipation effects** In the specific context of bail reform, descriptive statistics on the frequency of ROR (released on one’s own recognizance) cases suggest that judges were incorporating the bail reform requirements for release before the official January 1 implementation date, that is, there was anticipation of the intervention [Center for Court Innovation 2020]. Therefore, to test robustness of our conclusions to the anticipation effect, we also re-run the synthetic control analysis where we move-up the start date of the intervention itself to November, October, or September.

For example, in a hypothetical case where treatment effects on crime were only short-term, perhaps the early phase-in of bail reform requirements by judges would result in a statistically significant treatment effect on crime, but only measuring post-January data would hide the effect. On the other hand, early phase-in could have also increased the time at risk for some of those who were released prior to the intervention roll-in, inflating our observed effect. To some extent, using synthetic control analysis rather than a discontinuity-local analysis (such as ITS) mitigates potential impacts of the combination of dynamic treatment effects and anticipation, since synthetic control fits the few weights to minimize error over a larger time horizon, the entire pre-treatment period rather than locally near the boundary.

This “early roll-in” robustness check differs from the previous “in-time” placebo check because we increase the post-intervention time-frame by moving the intervention time earlier, while the “in-time” placebos re-run synthetic control entirely with pre-intervention data. Therefore, rather than necessarily expecting null effects as in the “in-time” checks, instead robustness here would be exhibited by seeing effects that are in general agreement with the main analysis, whether null or not.

### 5.3 Results: Linear regression with ridge penalty specification

We report the results in the appendix under the linear model specification (with no ridge penalty), linear regression with ridge penalty, and RSC (denoising and ridge penalty; [Amjad et al. 2018]). In the main text we focus on our preferred specification: linear with ridge penalty.

For each specification, we include a visual comparison of the NYC time series, as well as the imputed synthetic control fit; visual overlay of the placebo fits and the NYC-treated fit, a summary
table of ATE estimate and $p$-values, as well as summary statistics on pre-treatment fit, testing at level $\alpha = 0.1$ adjusted for multiple-tests via Holm-Šidák correction, as well as the output of synthetic control weights. Then we include tables of placebo checks: the in-time placebo checks and early roll-in of intervention placebo checks. We walk through this series of analyses for our preferred specification, and summarize the main inferential conclusions across specifications.

In Figure 3, we show results for the synthetic control analysis for Assault and Property Theft outcomes, computed on a weekly aggregation, Burglary and Robbery, after aggregating event counts to a five-weekly coarsening. The other crime types are much lower frequency (ranging from 20% to 5% of the average frequencies as Assault). Therefore, aggregating at the temporal level is required to reduce noise of the synthetic control estimation. For purposes of display, we graphically display loess-smoothed time series (using 7% of the data to estimate each value).

**Treatment effect estimates.** In Table 6, we summarize the main numerical results of the synthetic control analysis, for each crime type. We report the averaged ATE estimate (on the event scale, normalized to be a rate per 1000 individuals). That is, the synthetic control comparison suggests that the increase in average weekly violent assaults is 0.0077 events per 1000 individuals, relative to the synthetic control.

The “$p$ (ATE)” column reports the $p$-value from the unit-level placebo distribution of the ATE estimate, while the “$p$ (RMSE)” column reports the $p$-value from the RMSE test statistic. For property theft, robbery, and drug, there is some sensitivity to conclusions depending on the test statistic (although with a multiple testing-adjustment via Holm-Šidák correction reported in Table 7 at a $\alpha = 0.1$ level we only reject the null for the violent robbery crime type). For burglary, we have the most consensus between the ATE and RMSE test statistics of non-significance of the increased effect.

To summarize the findings from Tables 6 and 7 qualitatively, we would conclude that significance testing at $\alpha = 0.1$ would result in failure to reject the null hypothesis of no increase in crime. To put the effect sizes in context, the ATE point-estimate in number of assault events is 57 events relative to approximately 1760 weekly assault events; or an estimated 3.2% relative percentage increase. For Theft, the ATE point-estimate is 175 events relative to approximately 2667 weekly events, or 6.5% relative increase. We also report multiple testing adjustments via the Holm-Šidák correction in Table 7 which adjust for the probability of rejecting the null by chance over multiple comparisons.

We also report measures of pre-intervention predictive fit of the synthetic control in predicting observed outcomes: the $R^2$ value of the pre-treatment fit for the main synthetic control analysis where NYC is the treated unit. We also compare the pre-intervention RMSE and average placebo pre-intervention RMSE. The last comparison, pre-intervention RMSE vs. average placebo pre-intervention RMSE, provides a sense of the predictive fits for the placebo synthetic controls.

For interpretation, in Table 8, we display the weights chosen by the synthetic control. Note that we allow for a degree of extrapolation by allowing for negative weights.

**Placebo analysis.** In Figure 4, we graphically display the placebo distribution and the NYC residuals in the context of the generated leave-one-out placebo distribution. For each city acting as the treated unit, we plot the residual series: actual observations minus the predicted values. This provides a qualitative sense of the validity of the placebo distribution. While the placebo fits tend to be noisier than the original NYC fit, the average RMSE is of the same order of magnitude.
Comparing Violent Assault and Property Theft suggests that post-intervention deviations for the unit-level placebo checks are overall of similar magnitudes to the NYC-treated analysis. In Table 9, we present the in-time placebo check. We modify the intervention period to start earlier by a year, six months, and three months: one of 01/01/19, 06/01/19, or 09/01/19. We then define the time period between the new intervention and the original intervention timepoint, 01/01/2020, as the placebo “post-intervention” time period, and compute similar placebo \( p \)-values. Since we know these timepoints do not correspond to interventions, they provide estimates of the synthetic control ATE under a known null model of no effect. The corresponding “null ATEs” are of similar magnitude to the observed ATEs under the synthetic control suggesting robust null conclusions.

The placebo \( p \)-value quantifies the \( p \)-values under the null model. We also report the average placebo RMSE to verify that these placebo checks achieve similar overall predictive fit, of similar orders of magnitude to the original synthetic control analysis. Note that the ATE test statistic is one sided, so the least extreme \( p \)-value is 0.5, while for the RMSE test statistic, even larger \( p \)-values indicate that the NYC post-treatment RMSE is even less “extremal” within the placebo distribution.

In Table 10 we present the robustness checks corresponding to early roll-in of intervention. Overall the results of the placebo check suggest that under alternative specifications where we substantively know there is no treatment effect, we recover similar order of magnitude ATE estimates as under our main specification. Robbery does have some extremal \( p \)-values under the placebo check: it may suggest that the data series is quite noisy such that known null effects may nonetheless be extremal. Otherwise however, no statistical significance would be detected under the placebo specifications. The same conclusion holds under the early-roll-in of intervention start placebo check in Table 10. Notably, despite the documented increase in ROR rates as we moved the intervention date earlier in time, we do not detect significant or extremal effects.

Findings from alternative specifications. We also describe findings under alternative specifications, unpenalized linear regression and RSC. In Table 11, we observe more extremal \( p \)-values under the RMSE statistic for linear regression than under our specification with the ridge penalty. This is due to the linear specification requiring negative weights to achieve good pre-treatment fit; inspecting the weights, the weight vectors are more negative than those returned by the ridge penalty, which penalizes very negative weights. The linear specification therefore achieves good fit via extrapolation; correspondingly the pre-treatment for NYC improves and the test statistic is larger. Adjusting for multiple testing in Table 12, we find that we would reject violent robbery at a significance level of \( \alpha = 0.1 \).

For RSC, with visual overlay of the predictions in Figure 7 and placebo checks in Figure 8, and the inferential findings in Tables 16 and 17, we find that all \( p \)-values would be not statistically significant at level \( \alpha = 0.1 \) although violent robbery has the most extremal \( p \)-values under ATE and RMSE test statistics. Inspecting the placebo fits and in our experience with tuning the hyperparameter (number of singular values), we find that the inferential findings are being driven by noise and increased variance in the placebo distributions. Although RSC, by increasing the number of singular values, is able to fit (arbitrary) pre-treatment series, this introduces possible issues with overfitting.

The findings from the alternative specifications and the placebo checks in Tables 14 and 19 for linear regression and Tables 18 and 19 for RSC are consistent; under null interventions we do not
falsely conclude statistical significance and absolute magnitude ATE are on the order of observed under the main specification.

**Summary.** The synthetic controls for Assault, Theft and Drug achieve the best pre-treatment fits: this is in part driven by the absolute magnitude of these different crime types that lead to very different signal-to-noise ratios.

The Violent Robbery crime series displays the sharpest divergence of true vs. predicted crime trajectories and is indeed statistically significant under the linear regression specification, even after multiple-testing adjustment. For Assault and Theft, we find that although the observed crime type increases relative to the predicted incidence for the synthetic comparator, comparing this increase to placebo checks suggests that the increase may not be statistically significant. For the Drug crime type, we also find no statistically significant change (even though we observe a decrease relative to predicted).

The synthetic control analysis allows for comparing NYC’s increase in crime rates to a constructed comparator, therefore allowing us to capture shared sources of co-movements in crime rate such as overall national trends. In doing so, we find that although crime has increased, comparing to not only year-over-year trends in NYC but also co-movements with other cities suggests that the increase in aggregate crime rates is not extreme, although there is heterogeneity in crime type: we observe that violent robberies appear to have increased even after the synthetic control adjustment, while Assault, Theft, and Drug witness increases in the time series that are nonetheless well-predicted by the synthetic control (which correspondingly picks up on seasonal trends from the previous year).

6 Limitations

Our analysis of the NYC bail reform, conducted via aggregating police department-level incident reporting data, with a short post-intervention timespan before the onset of COVID, is subject to important limitations. Below, we describe these with relevant descriptive statistics and conclude, when possible, how these limitations may impact our estimates.

**The proportion of cases directly impacted by bail reform is small.** The primary way bail reform could impact crime is through the release of defendants who would’ve otherwise been detained pretrial (ie, bail not paid/remanded). Between January 1, 2020 and March 15, 2020, there were a total of 18,485 cases with defendants who were (1) continued at arraignment, (2) ineligible for bail (flagging eligibility based on charge), and (3) not bailed/remanded at arraignment.

Though these cases were ineligible for bail, we need to further estimate the percentage that would have been bailed/remanded had the new bail law not been in effect. During the same period in 2019, 16.3% of defendants were bailed/remanded in cases that met the conditions above (excluding condition 3, not bailed/remanded). 55% of these defendants were eventually released back into the community during the pretrial period. This matters if we’re thinking about the marginal difference in terms of time at risk in the community, since after a release there would be little difference between these cases and cases mandatorily released post-reform.

Applying this percentage to 2020 yields: 3,013 cases that would otherwise have been bailed/remanded. This small proportion of cases impacted could lead to a downward bias of our estimate since NYC was
already releasing a large percentage of defendants, so the number of defendants on the margin who were affected by the reform was low.

**Short study period.** The post-intervention period in our study runs from January 1, 2020 to March 15, 2020 – only a 75-day period.

Among time to disposition of new incidents, and the amount of time on release in the community, the window with potential for rearrest in our post-intervention period is small.

In NYC, speedy trial rules require that misdemeanor cases be resolved within 60-90 days and felony cases within 180 days. However, we know the typical time to disposition is much longer (eg, in 2019, the average time to disposition in indicted felony cases was 10 months [Weill et al., 2021]). On the other hand, during the 75-day study period, the number of days a case could have been on release in the community ranged from 1 to 75, with an average of about 30 days (assuming an equal distribution of cases for each day during the 75-day period).

This means that for the roughly 3,000 defendants that would have otherwise been bailed/remanded, the study only captures a small portion of their overall time at risk (3,000 * 30 days = 90,000 person-days of risk). In 2020, the pretrial rearrest rate for cases tracked through March 2021 was about 20% (according to Office of Court Administration, [New York State Unified Court System]). Given the short time at risk in the study (30 days on average), we need to assume a much lower rate of rearrest relative to statistics derived from longer time periods. For the sake of argument, suppose the pretrial rearrest rate from our shorter time period was 10%, so 300 of the 3,000 defendants impacted by the reform were rearrested by March 15, 2020. On the other hand, the clearance rate during the study period was about 1 in 3 [NYPD, 2020]. A worst-case analysis, assuming these individuals were responsible for 3x the number of rearrests, suggests 900 rearrests in total.

Overall, the impact of a shorter time frame for the study suggests that our estimates could be biased downwards from the estimate with a longer time-period, because of the short time at risk for re-offense.

**Seasonality.** The study occurs entirely during the winter months when crime is at its lowest ebb. While the models account for temperature and month-week fixed effects, we may not have enough historical data to identify the true seasonal patterns.

This could lead to downwards biased estimates because the post-intervention period is entirely within a seasonal lull in crime.

**One-time retroactive release of defendants.** It was estimated that about 900 defendants were retroactively released under the new law [Rosenberg and Golding, 2019]. In a way, this could mean that our estimates overstate the effect of the policy, since during the study period there were many more impacted defendants (released) than there would normally have been.

**Anticipation effects.** Many of the retroactive releases (described above) occurred ahead of the policy start date. Also, there is some evidence that judges had already started to change their behavior in anticipation of the new law in November-December, 2019 (less likely to bail/remand defendants who would soon be released anyway).

Although this could lead to a downward bias in estimates, this is addressed in our early intervention roll-in sensitivity analysis, which also assesses null effects. The earlier sensitivity analysis suggests that this is not a serious limitation.
**Treatment contamination.** Bail reforms were recently implemented in a number of other jurisdictions. Our comparison jurisdictions for the synthetic control analysis, selected by large population, do not overlap with sites of major reform during the study period (such as New Jersey, or Chicago’s reform just prior to beginning of pre-intervention period). While the timing of NY’s reform is unique, as well as some of the components of the reform, do continuing effects over time in some comparison cities lead to unit-specific unobserved confounding?

This could potentially bias our estimate down since crime rates could already be elevated in the control group due to lingering effects from similar reforms.

### 7 Conclusion

#### 7.1 Discussion of limitations

Although we believe this study offers broader lessons about bail reform’s effects, one must be cautious in making generalizations, given NYC’s unique features; the patterns detected in this case study therefore might not translate to other contexts. In Section 6, we discussed more thoroughly possible limitations of our study. We now summarize these limitations and discuss how our analysis, when possible, may be robust to some of them. Some limitations arose from our abridged post-intervention period due to the onset of COVID. The alternative, extending the study into the COVID period, would also introduce considerable study limitations due to extreme systemic changes during that period and differing temporal onsets of pandemic severity.

Some key limitations of our study include the small proportion of cases impacted over the short post-intervention study period. Our analysis focused on aggregate crime rates. Bail reform most directly affects individuals who were detained pretrial, and their re-arrest rates may comprise a small fraction of the aggregate crime rates we consider. Further, our short study period due to COVID, combined with typical times to disposition, means that our post-intervention period covers only a small portion of the time at risk. Overall, these inherent limitations of the post-intervention, pre-COVID intervention period could suggest that our estimates on aggregate crime rates are in fact a substantive underestimate of the impacts of bail reform.

Our analysis also estimated impact based on reconstructing UCR part 1 offenses, also known as index crimes, from police department data portals. There were overall increases in index crime; the top 3 increases were for grand larceny, robbery, and burglary: crime categories that were made ineligible for bail reform.

The post-intervention period is during winter, which is a seasonal lull in crime, so that seasonality is a concern: while we adjust for this via month-week fixed effects (for ITS) or via inherent seasonality in crime series of control units (for synthetic control), this could lead to downward biased estimates had our post-intervention period extended into COVID.

There may be local effects near the time of intervention. NYC also underwent a large one-time retroactive release of defendants so that we overestimate effects due to accumulated release of defendants. Although one may be concerned about anticipation effects for the policy (judges releasing more individuals ahead of the policy), we discuss this more comprehensively in our discussion of robustness checks in Section 5.2, where we conduct early intervention roll-in placebo checks.
7.2 Summary

This paper explores the impact of bail reform on crime, using the case study of New York’s 2020 reforms. Subject to the limitations discussed above, we find evidence that bail reform did not increase aggregate crime rates in NYC.

By focusing on aggregate crime rates, we are able to explore the broader consequences of bail reform. Key policy questions for bail reform, which inform the cost-benefit analysis, include both studying potential deterrent effects of cash bail on those arraigned by studying re-arrest rates after the reform, potential reduction in detention disparities induced by inability to pay bail via pretrial detention rates, as well as broader deterrent effects on the general population which may surface via crime rates. Our analysis focuses on assessing these broader deterrent effects.

We consider a subset of crime outcomes, Assault, Theft, Drug, Burglary, and Robbery, which are the most frequent time series. Our analysis is on a shorter time frame than comparable synthetic control analyses because the post-intervention time period is censored by COVID. We first consider interrupted time series analysis of crime rates before and after the intervention, to account for NYC’s within-unit variation (such as year-over-year increases in crimes). The corresponding p-values from ITS by themselves would suggest mixed conclusions: some crime types appear to increase (statistically significantly) while others appear to decrease (statistically significantly). Substantively, we would generically expect increases in crime rate that arise simply from releasing more individuals, i.e. due to the incapacitative effects of pretrial detention. After adjusting for multiple comparisons, only the violent assault time series remains statistically significant.

Our main analysis uses the synthetic control method, which allows for adjusting for cross-unit variation by comparing NYC’s trend to a synthetic control, constructed by weighting the time series of other cities. In order to provide some sense of uncertainty quantification, we conduct a series of placebo checks as conventional in the synthetic control literature, and report p-values from the placebo distribution of ATEs that arise from fixing another unit as the treated outcome, to assess the variation in null effects arising from synthetic control. Our crime data is noisy. For high-frequency outcomes such as Assault and Theft (hundreds of daily events) we conduct analysis at a weekly level to mitigate variance. For low-frequency outcomes we aggregate to biweekly or 5-weekly sums. The results from the synthetic control fail to reject a null hypothesis of no effect (before and after adjusting for multiple comparisons).

Overall, though some crime outcomes increase in absolute terms, adjusting for temporal fixed effects as well as within-unit time trends and variation over time can explain away most, but not all changes in crime rates. Further accounting for cross-unit variation (such as unobserved co-movements in crime rates across cities) leads to statistically insignificant conclusions of average treatment effects.

We briefly discuss some future directions. On the methodological side, the shorter time frame of our post-intervention analysis precluded usefully leveraging structure of our multiple crime outcomes. In our type of setting, model selection approaches for different synthetic control methods or advances in multiple outcomes would be helpful. On the substantive side, our analysis focused on providing evidence via analysis of aggregate crime rates but many important questions remain regarding individual-level impacts. A benefit of our aggregate analysis was in leveraging event-study methods to leverage crime rate data from a large number of potential comparison units (agencies). However, individual-level analysis on effects of bail reform on re-arrest rates is important and informs questions of specific deterrence. It is also crucial to assess the individual-level impacts of bail practices, for example assessing disparities in bail inducing pretrial detention due
to inability to pay. Additional analysis of individual-level rearrest outcomes would have to account for previous selection into previous detention decisions. Finer-grained analysis of judicial discretion in bail decisions prior to the reform can sharpen understanding of the “affected” stratum of individuals most affected by the reform. It is also important to understand judicial discretion in response to the reform: does removing bail eligibility lead to substitution to supervised release rather than unconditional release, and are there any potential negative effects on individuals due to net-widening?

Our analysis focused on assessing the impacts of bail reform on aggregate crime rate, hence informing general deterrence considerations of the broader policy context. Overall, we find evidence that bail reform did not increase aggregate crime rates in New York City.
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\section*{A ITS Analysis}

Table 1: Data descriptives: crime time series pre-intervention, daily counts

| Statistic | Mean  | St. Dev. | Min | Pctl(25) | Pctl(75) | Max |
|-----------|-------|----------|-----|----------|----------|-----|
| Assault   | 246.566 | 29.849  | 135 | 226      | 267      | 359 |
| Theft     | 373.182 | 50.718  | 150 | 339      | 410      | 517 |
| Burglary  | 31.537  | 7.571   | 12  | 26       | 37       | 57  |
| Drug      | 45.642  | 21.013  | 1   | 30       | 60       | 105 |
| Robbery   | 36.747  | 8.082   | 18  | 31       | 42       | 71  |
| Homicide  | 0.841   | 1.054   | 0   | 0        | 1        | 9   |
Table 2: Data descriptives: crime time series post-intervention, daily counts

| Statistic | Mean  | St. Dev. | Min  | Pctl(25) | Pctl(75) | Max  |
|-----------|-------|----------|------|----------|----------|------|
| Assault   | 250.068 | 21.694   | 197  | 234.2    | 265      | 318  |
| Theft     | 381.851 | 35.118   | 302  | 358.2    | 404.5    | 462  |
| Burglary  | 36.135  | 7.206    | 19   | 31       | 41.5     | 52   |
| Drug      | 39.149  | 17.633   | 8    | 22.2     | 51.8     | 73   |
| Robbery   | 40.351  | 7.023    | 19   | 36       | 44       | 58   |
| Homicide  | 0.797   | 1.033    | 0    | 0        | 1        | 4    |
Table 3: ITS ARIMA analysis.

|         | Assault | Theft   | Drug    | Burglary | Robbery |
|---------|---------|---------|---------|----------|---------|
| AR(1)  | 0.47*** | −0.23** | −0.14  | 0.99***  | (0.02)  |
|         | (0.01)  | (0.03)  | (0.03)  | (0.00)   |
| AR(2)  | 0.71*** | 0.47*** | −0.14***| (0.02)   |
|         | (0.01)  | (0.03)  | (0.00)  |
| AR(3)  | −0.29*  | 0.39*** | −0.65***| −0.88*** | (0.02)  |
|         | (0.02)  | (0.03)  | (0.00)  |
| MA(1)  | −0.37   | −0.80** | 0.50*** | (0.02)   |
|         | (0.01)  | (0.03)  | (0.00)  |
| MA(2)  | −0.29***| −0.84***| (0.02)  |
|         | (0.01)  | (0.03)  | (0.00)  |
| MA(3)  | 0.50*** | (0.02)  |
| drift  | −0.29***| (0.00)  |

| Intercept | 315.81*** | 370.65*** | 18.85** | 46.72*** |
|-----------|------------|------------|----------|----------|
| t         | −66.03**   | 13.78      | 12.91*   | 25.28*   |
| Monday    | 2.15       | 57.51***   | 6.97***  | 1.24     |
| Tuesday   | 2.61       | 57.01***   | 7.36***  | 22.95*** |
| Wednesday | 7.60**     | 60.17***   | 8.08***  | 32.34*** |
| Thursday  | 1.04       | 52.16***   | 8.15***  | 30.05*** |
| Friday    | 9.20***    | 78.08***   | 13.72*** | 29.60*** |
| Saturday  | −0.32      | 35.61***   | 5.06***  | 17.22*** |
| January   | −4.98      | −47.93***  | −0.92    | −84.75***|
| February  | −3.41      | −51.51***  | −3.26*** | −78.93***|
| March     | 3.62       | −56.99***  | −4.97*** | −72.34***|
| April     | 16.39***   | −35.14***  | −3.10*** | −62.89***|
| May       | 37.78***   | −13.23     | −3.02*** | −55.77***|
| June      | 42.03***   | 7.48       | −2.65**  | −51.65***|
| July      | 35.44***   | 28.47***   | 0.93     | −39.75***|
| August    | 30.79***   | 25.41***   | 1.43     | −29.49***|
| September | 36.14***   | 19.78**    | −0.10    | −22.16***|
| October   | 27.35***   | 13.09      | 1.48     | −10.51** |
| November  | 0.76*      | 6.11       | −0.56    | −6.02*   |

Note: *p < 0.10, **p < 0.05, ***p < 0.01
Table 4: ITS: p-values of treatment effect regression coefficients

| p-value | Violent Assault | Theft | Burglary | Drug | Robbery |
|---------|-----------------|-------|----------|------|---------|
|         | $4.31 \times 10^{-3}$ | $6.7 \times 10^{-1}$ | $3.90 \times 10^{-2}$ | $2.41 \times 10^{-2}$ | $6.93 \times 10^{-1}$ |
| Adjusted p | $2.14 \times 10^{-2}$ | $8.91 \times 10^{-1}$ | $1.13 \times 10^{-1}$ | $9.29 \times 10^{-2}$ | $8.91 \times 10^{-1}$ |
Figure 1: ITS ARIMA plots of time series. Solid and dashed lines are loess-smoothed plots of observed and ARIMA-predicted crime counts, respectively. Scatter plot overlay of actual counts to illustrate variability.
Figure 2: Homicide ITS (Poisson GLM): predictions vs. observations, weekly aggregation. Scatter plot of observed counts. Solid line of predictions.
Table 5: ITS, Poisson GLM for homicide time series. $t$ is the indicator variable for post-intervention period. $t \times$ number of days is interacted with number of days post-intervention.

|                     | Uninteracted treatment | Time-interacted treatment |
|---------------------|------------------------|---------------------------|
| (Intercept)         | 0.95                   | 2.67**                    |
|                     | (0.95)                 | (0.95)                    |
| AR(1)               | 0.00                   | 0.00                      |
|                     | (0.05)                 | (0.05)                    |
| AR(2)               | 0.69                   | 0.34                      |
|                     | (0.35)                 | (0.35)                    |
| t                   | 0.34                   | 0.41                      |
|                     | (1.40)                 | (1.40)                    |
| January             | 1.23                   | 1.16                      |
|                     | (0.80)                 | (0.65)                    |
| February            | 0.00                   | 0.00                      |
|                     | (0.65)                 | (0.66)                    |
| March               | 0.82                   | 0.56                      |
|                     | (0.66)                 | (0.70)                    |
| April               | 0.48                   | 0.48                      |
|                     | (0.70)                 | (0.70)                    |
| May                 | 1.24                   | 1.29                      |
|                     | (0.70)                 | (1.00)                    |
| June                | 1.83                   | 2.06                      |
|                     | (1.00)                 | (1.30)                    |
| July                | 1.17                   | 2.48*                     |
|                     | (1.30)                 | (0.94)                    |
| August              | 0.79                   | 1.34                      |
|                     | (0.94)                 | (1.04)                    |
| September           | 1.73                   | 1.96                      |
|                     | (1.04)                 | (1.02)                    |
| October             | 0.50                   | 1.31                      |
|                     | (1.02)                 | (0.89)                    |
| November            | 0.62                   | 0.79                      |
|                     | (0.89)                 | (1.20)                    |
| 2017                | 0.00                   | 0.00                      |
|                     | (1.20)                 | (1.27)                    |
| 2018                | 0.09                   | 0.18                      |
|                     | (1.27)                 | (1.21)                    |
| 2019                | 0.01                   | 0.06                      |
|                     | (1.21)                 | (0.95)                    |
| $t \times$ number of days | 0.01                   | (0.80)                    |
| $R^2$               | 0.12                   | 0.11                      |
| Adj. $R^2$          | 0.02                   | -0.00                     |
| Num. obs.           | 167                    | 167                       |

***$p < 0.001$, **$p < 0.01$, *$p < 0.05$
B Robustness checks

B.1 Ridge

Figure 3: Synthetic control plots, Ridge (Loess-smoothed for display)
(a) Violent Assault

(b) Property Theft

(c) Drug

(d) Property Burglary

(e) Violent Robbery

Figure 4: Unit-level placebo checks (Ridge; Loess-smoothed for display)

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Table 6: Synthetic control (Ridge) tabular results

|                  | ATE (/1000) | p (ATE) | p (RMSE) | Pre-tx $R^2$ | Pre-tx RMSE | Avg. plac. RMSE | Num. plac. |
|------------------|-------------|---------|----------|--------------|-------------|-----------------|------------|
| Violent Assault  | 0.0071      | 0.41    | 0.71     | 0.74         | $8.58 \times 10^{-6}$ | $2.46 \times 10^{-5}$ | 22         |
| Property Theft   | 0.0214      | 0.29    | 0.13     | 0.69         | $1.87 \times 10^{-5}$ | $4.57 \times 10^{-5}$ | 24         |
| Property Burglary| 0.0187      | 0.36    | 0.15     | 0.73         | $6.06 \times 10^{-6}$ | $2.32 \times 10^{-5}$ | 14         |
| Violent Robbery  | 0.0365      | 0.23    | 0.33     | 0.56         | $1.1 \times 10^{-5}$  | $1.55 \times 10^{-5}$ | 22         |
| Drug             | −0.0094     | 0.79    | 0.23     | 0.82         | $6.59 \times 10^{-6}$ | $2.10 \times 10^{-5}$ | 14         |
Table 7: Multiple testing adjustment, Ridge (for ATE, RMSE test statistics separately)

|                        | Violent Robbery | Property Theft | Property Burglary | Violent Assault | Drug |
|------------------------|-----------------|----------------|-------------------|-----------------|------|
| Adjusted p (ATE)       | 0.72            | 0.75           | 0.75              | 0.75            | 0.79 |
| Conclusion             | Fail to Reject  | Fail to Reject | Fail to Reject    | Fail to Reject  | Fail to Reject |
| Adjusted p (RMSE)      | 0.5             | 0.5            | 0.54              | 0.56            | 0.71 |
| Conclusion             | Fail to Reject  | Fail to Reject | Fail to Reject    | Fail to Reject  | Fail to Reject |
Table 8: Synthetic control tabular results, Ridge, weekly aggregation

| City          | Violent Assault | Property Theft | Property Burglary | Violent Robbery | Drug |
|---------------|-----------------|----------------|-------------------|-----------------|------|
| Austin        | 0.04            | 0.04           | 0.09              | 0.06            | 0.08 |
| Baltimore     | 0.01            | 0.05           | 0.0               | 0.01            | —    |
| Boston        | 0.06            | 0.05           | 0.09              | 0.05            | −0.0 |
| Buffalo       | 0.02            | 0.01           | −0.08             | 0.02            | —    |
| Chicago       | 0.01            | 0.05           | 0.08              | 0.03            | 0.04 |
| Cincinnati    | 0.01            | 0.07           | −0.02             | 0.01            | —    |
| Dallas        | 0.06            | 0.08           | 0.02              | 0.05            | 0.0  |
| Denver        | 0.06            | −0.01          | 0.09              | 0.05            | −0.05|
| Detroit       | −0.01           | 0.04           | −0.01             | 0.04            | −0.1 |
| Houston       | 0.02            | 0.06           | 0.02              | 0.05            | −0.11|
| Kansas City   | 0.01            | 0.01           | 0.01              | 0.01            | 0.0  |
| Little Rock   | 0.03            | −0.01          | −0.04             | 0.05            | —    |
| Los Angeles   | 0.06            | 0.11           | 0.16              | 0.06            | 0.83 |
| Louisville    | 0.04            | −0.01          | 0.04              | 0.05            | 0.01 |
| Milwaukee     | 0.04            | 0.02           | 0.02              | 0.02            | —    |
| Nashville     | 0.04            | 0.07           | 0.03              | 0.02            | 0.07 |
| Philadelphia  | 0.05            | 0.05           | 0.05              | 0.05            | 0.13 |
| Phoenix       | 0.08            | 0.07           | 0.05              | 0.05            | 0.09 |
| Portland      | 0.06            | 0.04           | 0.01              | 0.05            | 0.07 |
| Raleigh       | 0.06            | 0.07           | 0.06              | 0.06            | −0.05|
| San Francisco | 0.06            | 0.01           | 0.06              | 0.04            | 0.02 |
| Seattle       | 0.06            | 0.05           | −0.0              | 0.05            | −0.05|
| Virginia Beach| 0.06            | 0.04           | 0.19              | 0.06            | 0.03 |
| Washington DC | 0.08            | 0.03           | 0.07              | 0.05            | —    |
| intercept     | −0.0            | 0.0            | 0.0               | 0.0             | −0.0 |
### Table 9: Placebos in time; Ridge

| Category          | Start date | ATE (/1000) | p (ATE) | p (RMSE) | Avg. Placebo RMSE |
|-------------------|------------|-------------|---------|----------|-------------------|
| Violent Assault   | 01/01/19   | −0.0202     | 0.87    | 0.82     | $2.17 \times 10^{-5}$ |
|                   | 03/01/19   | −0.0086     | 0.70    | 1.0      | $2.25 \times 10^{-5}$ |
|                   | 06/01/19   | −0.0058     | 0.61    | 1.0      | $2.42 \times 10^{-5}$ |
|                   | 01/01/20   | 0.0071      | 0.41    | 0.71     | $2.46 \times 10^{-5}$ |
| Property Theft    | 01/01/19   | −0.0402     | 0.68    | 1.0      | $3.73 \times 10^{-5}$ |
|                   | 03/01/19   | −0.0107     | 0.68    | 1.0      | $4.09 \times 10^{-5}$ |
|                   | 06/01/19   | 0.0206      | 0.48    | 1.0      | $4.47 \times 10^{-5}$ |
|                   | 01/01/20   | 0.0214      | 0.29    | 0.13     | $4.57 \times 10^{-5}$ |
| Property Burglary | 01/01/19   | 0.0049      | 0.29    | 0.61     | $1.52 \times 10^{-5}$ |
|                   | 03/01/19   | 0.0034      | 0.35    | 0.64     | $1.5 \times 10^{-5}$  |
|                   | 06/01/19   | 0.0019      | 0.35    | 1.0      | $1.55 \times 10^{-5}$ |
|                   | 01/01/20   | 0.0187      | 0.36    | 0.15     | $2.32 \times 10^{-5}$ |
| Violent Robbery   | 01/01/19   | 0.0035      | 0.16    | 0.29     | $1.15 \times 10^{-5}$ |
|                   | 03/01/19   | 0.0036      | 0.24    | 0.25     | $1.17 \times 10^{-5}$ |
|                   | 06/01/19   | 0.0044      | 0.24    | 0.67     | $1.19 \times 10^{-5}$ |
|                   | 01/01/20   | 0.0365      | 0.23    | 0.33     | $1.55 \times 10^{-5}$ |
| Drug              | 01/01/19   | −0.0018     | 0.42    | 1.0      | $1.91 \times 10^{-5}$ |
|                   | 03/01/19   | −0.0011     | 0.35    | 1.0      | $1.64 \times 10^{-5}$ |
|                   | 06/01/19   | 0.0018      | 0.18    | 1.0      | $1.66 \times 10^{-5}$ |
|                   | 01/01/20   | −0.0094     | 0.79    | 0.23     | $2.10 \times 10^{-5}$ |
Table 10: Placebos in time, Ridge (early roll-in of intervention start)

| Category        | Start date | ATE (/1000) | p (ATE) | p (RMSE) | Avg. Placebo RMSE |
|-----------------|------------|-------------|---------|----------|-------------------|
| Violent Assault | 09/01/19   | −0.0002     | 0.5     | 1.0      | $2.39 \times 10^{-5}$ |
|                 | 10/01/19   | −0.0005     | 0.5     | 1.0      | $2.40 \times 10^{-5}$ |
|                 | 11/01/19   | 0.005       | 0.45    | 1.0      | $2.42 \times 10^{-5}$ |
|                 | 01/01/20   | 0.0071      | 0.41    | 0.71     | $2.46 \times 10^{-5}$ |
| Property Theft  | 09/01/19   | 0.035       | 0.38    | 0.96     | $4.29 \times 10^{-5}$ |
|                 | 10/01/19   | 0.0193      | 0.33    | 1.0      | $4.37 \times 10^{-5}$ |
|                 | 11/01/19   | 0.0187      | 0.33    | 0.96     | $4.49 \times 10^{-5}$ |
|                 | 01/01/20   | 0.0214      | 0.29    | 0.13     | $4.57 \times 10^{-5}$ |
| Property Burglary| 09/01/19  | 0.0006      | 0.39    | 1.0      | $1.6 \times 10^{-5}$ |
|                 | 10/01/19   | 0.0018      | 0.35    | 0.86     | $1.6 \times 10^{-5}$ |
|                 | 11/01/19   | 0.0026      | 0.41    | 0.57     | $1.52 \times 10^{-5}$ |
|                 | 01/01/20   | 0.0187      | 0.36    | 0.15     | $2.32 \times 10^{-5}$ |
| Violent Robbery | 09/01/19   | 0.0059      | 0.24    | 0.21     | $1.22 \times 10^{-5}$ |
|                 | 10/01/19   | 0.0054      | 0.2     | 0.5      | $1.23 \times 10^{-5}$ |
|                 | 11/01/19   | 0.005       | 0.16    | 0.67     | $1.23 \times 10^{-5}$ |
|                 | 01/01/20   | 0.0365      | 0.23    | 0.33     | $1.55 \times 10^{-5}$ |
| Drug            | 09/01/19   | −0.0003     | 0.35    | 1.0      | $1.69 \times 10^{-5}$ |
|                 | 10/01/19   | −0.0012     | 0.47    | 0.81     | $1.68 \times 10^{-5}$ |
|                 | 11/01/19   | −0.0047     | 0.65    | 0.69     | $1.68 \times 10^{-5}$ |
|                 | 01/01/20   | −0.0094     | 0.79    | 0.23     | $2.10 \times 10^{-5}$ |
B.2 Linear regression (unpenalized)
Figure 5: Synthetic control plots for weekly aggregation, linear regression (Loess-smoothed for display)
Figure 6: Unit-level placebo checks (Linear regression; Loess-smoothed for display)
Table 11: Synthetic control (linear regression) tabular results, weekly aggregation

| Category       | ATE (/1000) | p (ATE) | p (RMSE) | Pre-tx $R^2$ | Pre-tx RMSE | Avg. plac. RMSE | Num. plac. |
|----------------|-------------|---------|----------|--------------|-------------|-----------------|------------|
| Violent Assault| 0.0077      | 0.43    | 0.1      | 0.84         | $6.76 \times 10^{-6}$ | $2.33 \times 10^{-8}$  | 21         |
| Property Theft | 0.0249      | 0.25    | 0.04     | 0.79         | $1.54 \times 10^{-5}$ | $4.57 \times 10^{-5}$  | 24         |
| Property Burglary | 0.019 | 0.39    | 0.5      | 0.83         | $4.74 \times 10^{-6}$ | $1.8 \times 10^{-5}$  | 23         |
| Violent Robbery | 0.0444      | 0.2     | 0.0      | 0.97         | $3.08 \times 10^{-6}$ | $1.42 \times 10^{-5}$  | 20         |
| Drug           | -0.0175     | 0.76    | 0.06     | 0.8          | $6.95 \times 10^{-6}$ | $2.42 \times 10^{-5}$  | 17         |
Table 12: Multiple testing adjustment, Linear regression (for ATE, RMSE test statistics separately)

|                      | Violent Robbery | Property Theft | Property Burglary | Violent Assault | Drug |
|----------------------|-----------------|----------------|-------------------|-----------------|------|
| **Adjusted p (ATE)** | 0.67            | 0.68           | 0.77              | 0.77            | 0.77 |
| **Conclusion**       | Fail to Reject  | Fail to Reject | Fail to Reject    | Fail to Reject  | Fail to Reject |
| **Adjusted p (RMSE)**| 0.0             | 0.16           | 0.18              | 0.19            | 0.5  |
| **Conclusion**       | Reject          | Fail to Reject | Fail to Reject    | Fail to Reject  | Fail to Reject |
Table 13: Synthetic control (linear regression) tabular results, weekly aggregation

| City          | Violent Assault | Property Theft | Property Burglary | Violent Robbery | Drug |
|---------------|-----------------|----------------|-------------------|-----------------|------|
| Austin        | -0.02           | -0.01          | 0.1               | 0.11            | 0.13 |
| Baltimore     | -0.04           | 0.04           | -0.01             | 0.01            | ---  |
| Boston        | 0.11            | -0.06          | 0.07              | 0.15            | -0.05|
| Buffalo       | 0.01            | 0.04           | -0.09             | 0.0             | ---  |
| Chicago       | 0.07            | 0.15           | 0.11              | -0.04           | 0.11 |
| Cincinnati    | -0.0            | 0.06           | -0.01             | 0.01            | ---  |
| Dallas        | 0.06            | 0.11           | 0.01              | -0.03           | 0.11 |
| Denver        | 0.04            | -0.11          | 0.11              | 0.02            | -0.06|
| Detroit       | 0.0             | 0.05           | -0.02             | 0.1             | -0.08|
| Houston       | 0.01            | -0.01          | -0.01             | 0.02            | -0.1 |
| Kansas City   | 0.01            | -0.01          | 0.0               | 0.01            | 0.0  |
| Little Rock   | 0.03            | -0.01          | -0.04             | 0.1             | ---  |
| Los Angeles   | 0.01            | 0.42           | 0.2               | 0.06            | 0.32 |
| Louisville    | 0.01            | -0.02          | 0.06              | 0.15            | 0.0  |
| Milwaukee     | 0.01            | -0.03          | 0.02              | -0.1            | ---  |
| Nashville     | 0.04            | 0.13           | 0.03              | -0.04           | 0.07 |
| Philadelphia  | 0.04            | 0.05           | 0.03              | 0.08            | 0.1  |
| Phoenix       | 0.23            | 0.03           | 0.04              | -0.02           | 0.19 |
| Portland      | 0.04            | 0.0            | -0.03             | 0.03            | 0.2  |
| Raleigh       | 0.07            | 0.02           | 0.07              | 0.06            | -0.05|
| San Francisco | 0.04            | 0.0            | 0.05              | 0.06            | 0.07 |
| Seattle       | 0.03            | 0.05           | -0.03             | 0.05            | -0.0 |
| Virginia Beach| -0.01           | 0.03           | 0.24              | 0.08            | 0.03 |
| Washington DC | 0.21            | 0.07           | 0.07              | 0.14            | ---  |
| intercept     | 0.0             | -0.0           | 0.0               | -0.0            | 0.0  |
Table 14: Placebos in time

| Category            | Start date | ATE (/1000) | p (ATE) | p (RMSE) | Avg. Placebo RMSE |
|---------------------|------------|-------------|---------|----------|-------------------|
| Violent Assault     | 01/01/19   | −0.0202     | 0.86    | 0.81     | $2.04 \times 10^{-5}$ |
|                     | 03/01/19   | −0.0086     | 0.68    | 1.0      | $2.11 \times 10^{-5}$ |
|                     | 06/01/19   | −0.0058     | 0.64    | 1.0      | $2.28 \times 10^{-5}$ |
| Property Theft      | 01/01/19   | −0.0402     | 0.68    | 1.0      | $3.73 \times 10^{-5}$ |
|                     | 03/01/19   | −0.0107     | 0.68    | 1.0      | $4.09 \times 10^{-5}$ |
|                     | 06/01/19   | 0.0206      | 0.46    | 1.0      | $4.11 \times 10^{-5}$ |
| Property Burglary   | 01/01/19   | 0.0049      | 0.26    | 0.59     | $1.44 \times 10^{-5}$ |
|                     | 03/01/19   | 0.0032      | 0.35    | 0.73     | $1.5 \times 10^{-5}$ |
|                     | 06/01/19   | 0.0019      | 0.36    | 0.95     | $1.47 \times 10^{-5}$ |
| Violent Robbery     | 01/01/19   | 0.0035      | 0.18    | 0.24     | $8.77 \times 10^{-6}$ |
|                     | 03/01/19   | 0.0031      | 0.18    | 0.24     | $8.85 \times 10^{-6}$ |
|                     | 06/01/19   | 0.004       | 0.27    | 0.62     | $8.9 \times 10^{-6}$ |
| Drug                | 01/01/19   | −0.0018     | 0.42    | 1.0      | $1.91 \times 10^{-5}$ |
|                     | 03/01/19   | −0.0011     | 0.35    | 1.0      | $1.64 \times 10^{-5}$ |
|                     | 06/01/19   | 0.0018      | 0.18    | 1.0      | $1.66 \times 10^{-5}$ |
Table 15: Robustness check: Early roll-in of intervention

| Category       | Early Roll-in Start date | ATE (/ 1000) | p (ATE) | p (RMSE) | Avg. Placebo RMSE |
|----------------|--------------------------|--------------|---------|----------|------------------|
| Violent Assault| 09/01/19                 | −0.0002      | 0.48    | 1.0      | $2.26 \times 10^{-5}$ |
|                | 10/01/19                 | −0.0001      | 0.48    | 1.0      | $2.27 \times 10^{-5}$ |
|                | 11/01/19                 | 0.006        | 0.33    | 1.0      | $2.3 \times 10^{-5}$ |
| Property Theft | 09/01/19                 | 0.0365       | 0.38    | 0.91     | $4.29 \times 10^{-5}$ |
|                | 10/01/19                 | 0.0193       | 0.33    | 1.0      | $4.37 \times 10^{-5}$ |
|                | 11/01/19                 | 0.0187       | 0.33    | 0.96     | $4.49 \times 10^{-5}$ |
| Property Burglary | 09/01/19               | 0.0007       | 0.45    | 1.0      | $1.51 \times 10^{-5}$ |
|                | 10/01/19                 | 0.002        | 0.36    | 0.9      | $1.50 \times 10^{-5}$ |
|                | 11/01/19                 | 0.0025       | 0.41    | 0.57     | $1.52 \times 10^{-5}$ |
| Violent Robbery | 09/01/19                | 0.0057       | 0.14    | 0.19     | $9.09 \times 10^{-6}$ |
|                | 10/01/19                 | 0.0053       | 0.18    | 0.48     | $9.1 \times 10^{-6}$ |
|                | 11/01/19                 | 0.005        | 0.14    | 0.62     | $9.19 \times 10^{-6}$ |
| Drug           | 09/01/19                 | −0.0003      | 0.35    | 1.0      | $1.69 \times 10^{-5}$ |
|                | 10/01/19                 | −0.0012      | 0.47    | 0.81     | $1.68 \times 10^{-5}$ |
|                | 11/01/19                 | −0.0047      | 0.65    | 0.69     | $1.68 \times 10^{-5}$ |
Figure 7: Synthetic control plots, RSC (Loess-smoothed for display)
Figure 8: Unit-level placebo checks (RSC; Loess-smoothed for display)
Table 16: Synthetic control (RSC) tabular results

| Category          | ATE (/1000) | p (ATE) | p (RMSE) | Pre-tx $R^2$ | Pre-tx RMSE  | Avg. Placebo RMSE |
|-------------------|-------------|---------|----------|--------------|--------------|--------------------|
| Violent Assault   | 0.0163      | 0.36    | 0.21     | 0.76         | $8.23 \times 10^{-6}$ | $2.07 \times 10^{-5}$ |
| Property Theft    | 0.0197      | 0.44    | 0.79     | 0.72         | $1.78 \times 10^{-5}$ | $2.71 \times 10^{-5}$ |
| Property Burglary | 0.0319      | 0.36    | 0.38     | 0.91         | $3.49 \times 10^{-6}$ | $1.15 \times 10^{-5}$ |
| Violent Robbery   | 0.0287      | 0.20    | 0.21     | 0.72         | $8.78 \times 10^{-6}$ | $1.65 \times 10^{-5}$ |
| Drug              | −0.0006     | 0.68    | 0.89     | 0.72         | $8.36 \times 10^{-6}$ | $2.01 \times 10^{-5}$ |
|                  | Violent Robbery | Violent Assault | Property Burglary | Property Theft | Drug |
|------------------|-----------------|-----------------|-------------------|----------------|------|
| Adjusted p (ATE) | 0.67            | 0.83            | 0.83              | 0.83           | 0.83 |
| Conclusion       | Fail to Reject  | Fail to Reject  | Fail to Reject    | Fail to Reject | Fail to Reject |
| Adjusted p (RMSE)| 0.69            | 0.69            | 0.76              | 0.96           | 0.96 |
| Conclusion       | Fail to Reject  | Fail to Reject  | Fail to Reject    | Fail to Reject | Fail to Reject |
Table 18: Placebos in time; RSC

| Category            | Start date | ATE (/1000) | p (ATE) | p (RMSE) | Avg. Placebo RMSE |
|---------------------|------------|-------------|---------|----------|--------------------|
| Violent Assault     | 01/01/19   | -0.0026     | 0.64    | 0.92     | $1.98 \times 10^{-5}$ |
|                     | 03/01/19   | 0.0049      | 0.56    | 0.71     | $1.99 \times 10^{-5}$ |
|                     | 06/01/19   | 0.0087      | 0.48    | 0.42     | $2.00 \times 10^{-5}$ |
| Property Theft      | 01/01/19   | -0.0436     | 0.76    | 0.62     | $2.30 \times 10^{-5}$ |
|                     | 03/01/19   | -0.0337     | 0.76    | 0.62     | $2.46 \times 10^{-5}$ |
|                     | 06/01/19   | 0.0016      | 0.6     | 1.0      | $2.50 \times 10^{-5}$ |
| Property Burglary   | 01/01/19   | -0.0016     | 0.36    | 0.62     | $6.38 \times 10^{-6}$ |
|                     | 03/01/19   | -0.0005     | 0.44    | 0.46     | $6.50 \times 10^{-6}$ |
|                     | 06/01/19   | 0.0025      | 0.4     | 0.46     | $6.69 \times 10^{-6}$ |
| Violent Robbery     | 01/01/19   | 0.0027      | 0.2     | 0.29     | $6.67 \times 10^{-6}$ |
|                     | 03/01/19   | 0.0037      | 0.2     | 0.29     | $6.84 \times 10^{-6}$ |
|                     | 06/01/19   | 0.0052      | 0.16    | 0.25     | $6.8 \times 10^{-6}$  |
| Drug                | 01/01/19   | 0.0082      | 0.37    | 0.39     | $1.35 \times 10^{-5}$ |
|                     | 03/01/19   | 0.0032      | 0.42    | 0.67     | $1.35 \times 10^{-5}$ |
|                     | 06/01/19   | 0.0004      | 0.32    | 0.67     | $1.32 \times 10^{-5}$ |
Table 19: Placebos in time, RSC (early roll-in of intervention start)

| Category        | Early Roll-in Start date | ATE (/ 1000) | p (ATE) | p (RMSE) | Avg. Placebo RMSE |
|-----------------|--------------------------|--------------|---------|----------|-------------------|
| Violent Assault | 09/01/19                 | 0.0111       | 0.44    | 0.25     | 2.02 × 10⁻⁵       |
|                 | 10/01/19                 | 0.0114       | 0.36    | 0.25     | 2.03 × 10⁻⁵       |
|                 | 11/01/19                 | 0.0139       | 0.32    | 0.25     | 2.05 × 10⁻⁵       |
| Property Theft  | 09/01/19                 | 0.0168       | 0.4     | 0.96     | 2.54 × 10⁻⁵       |
|                 | 10/01/19                 | 0.0141       | 0.48    | 0.96     | 2.56 × 10⁻⁵       |
|                 | 11/01/19                 | 0.0161       | 0.52    | 0.92     | 2.63 × 10⁻⁵       |
| Property Burglary| 09/01/19                | 0.0045       | 0.4     | 0.42     | 6.9 × 10⁻⁶        |
|                 | 10/01/19                 | 0.0057       | 0.36    | 0.42     | 6.89 × 10⁻⁶       |
|                 | 11/01/19                 | 0.0062       | 0.32    | 0.42     | 6.95 × 10⁻⁶       |
| Violent Robbery | 09/01/19                 | 0.0058       | 0.2     | 0.25     | 6.89 × 10⁻⁶       |
|                 | 10/01/19                 | 0.0052       | 0.24    | 0.25     | 6.90 × 10⁻⁶       |
|                 | 11/01/19                 | 0.0051       | 0.2     | 0.33     | 6.92 × 10⁻⁶       |
| Drug            | 09/01/19                 | −0.0004      | 0.26    | 0.72     | 1.29 × 10⁻⁵       |
|                 | 10/01/19                 | −0.0004      | 0.26    | 0.56     | 1.28 × 10⁻⁵       |
|                 | 11/01/19                 | −0.0012      | 0.32    | 0.61     | 1.29 × 10⁻⁵       |
C Description of dataset

C.1 Further details on construction

Figure 9 describes the data reporting hierarchy that was used to merge data across reporting agencies. The figure includes some crimes that were not used for the final synthetic control analysis. The second level of the hierarchy, at which level we conducted our analysis is:

- Homicide: Murder and manslaughter (does not include justifiable homicide)
- Rape: Forcible or aggravated penetration (does not include statutory rape)
- Robbery: All taking of property through force or threat of force
- Assault: Includes aggravated and simple assaults when possible
- Burglary: Unlawful entry into structure (residential and commercial) to commit theft without use of force
- Theft: Unlawful taking of property (includes motor vehicle theft)
- Other property crime: Includes a broad set of property crimes such as arson, stolen property offenses, damage to property, trespass, and other miscellaneous property crimes. Excludes animal crimes, drug, theft, burglary, and white collar crimes.
- Drug: All drug-related abuses including cultivation, distribution, sale, purchase, use, possession, transportation, or importation of any controlled drug or narcotic substance.
- White collar: Includes crimes of deceit or intentional misrepresentation, such as counterfeiting, fraud, and embezzlement (includes offenses such as check fraud, confidence game, and credit card fraud)
- Gambling: All unlawful betting or wagering, tampering, or operation of game of chance (includes equipment violations)
- Arson: Any willful or malicious burning or attempt to burn property

We aggregated the incident descriptions under these categories. Some, but not all cities, reported UCR codes which were used when available. Austin, Chicago, Cincinnati, Dallas, Los Angeles, Philadelphia reported UCR codes; but not all at the same granularity (with some reporting coarse codes at the hundreds level). Houston’s schema changed twice during the study period; crime counts were discontinuous across schema changes under the aggregations and so we excluded Houston from the analysis.

These definitions are modified from other crime frameworks, such as a previous Bureau of Justice Statistics analysis\(^2\)\(^3\)\(^4\), FBI offense definitions\(^3\) and definitions from City Crime Stats\(^3\)

C.2 Descriptive statistics

\(^2\)https://www.bjs.gov/recidivism/
\(^3\)https://ucr.fbi.gov/crime-in-the-u.s/2019/crime-in-the-u.s.-2019/topic-pages/offense-definitions
\(^4\)https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3674032
Figure 9: Overview of crime reporting hierarchy
| City             | Data Source                                                                 |
|------------------|----------------------------------------------------------------------------|
| Atlanta          | [http://opendata.atlantapd.org/](http://opendata.atlantapd.org/          |
| Austin           | [https://data.austintexas.gov/Public-Safety/](https://data.austintexas.gov/Public-Safety/ |
| Baltimore        | [https://www.baltimorepolice.org/crime-stats/open-data](https://www.baltimorepolice.org/crime-stats/open-data) |
| Boston           | [https://data.boston.gov/dataset/crime-incident-reports-august-2015-to-date-source-new-system](https://data.boston.gov/dataset/crime-incident-reports-august-2015-to-date-source-new-system) |
| Buffalo          | [https://data.buffalony.gov/Public-Safety/Crime-Incidents/d6g9-xbgu](https://data.buffalony.gov/Public-Safety/Crime-Incidents/d6g9-xbgu) |
| Chicago          | [https://data.cityofchicago.org/Public-Safety/](https://data.cityofchicago.org/Public-Safety/ |
| Cincinatti       | [https://data.cincinnati-oh.gov/Safety/PDI-Police-Data-Initiative-Crime-Incidents/k59e-2p](https://data.cincinnati-oh.gov/Safety/PDI-Police-Data-Initiative-Crime-Incidents/k59e-2p) |
| Dallas           | [https://www.dallasopendata.com/Public-Safety/PDIncidents/](https://www.dallasopendata.com/Public-Safety/PDIncidents/) |
| Denver           | [https://www.denvergov.org/opendata/dataset/city-and-county-of-denver-crime](https://www.denvergov.org/opendata/dataset/city-and-county-of-denver-crime) |
| Detroit          | [https://data.detroitmi.gov/datasets/rms-crime-incidents](https://data.detroitmi.gov/datasets/rms-crime-incidents) |
| Fort Worth       | [https://data.fortworthtexas.gov/Public-Safety/Crime-Data/k6ic-7kp7](https://data.fortworthtexas.gov/Public-Safety/Crime-Data/k6ic-7kp7) |
| Houston          | [https://www.houstontx.gov/police/crime/index-2.htm](https://www.houstontx.gov/police/crime/index-2.htm) |
| Kansas City      | [https://data.kcmo.org/](https://data.kcmo.org/) |
| Los Angeles      | [https://data.lacity.org/A-Safe-City/](https://data.lacity.org/A-Safe-City/) |
| Louisville       | [https://data.louisvilleky.gov/dataset/crime-reports](https://data.louisvilleky.gov/dataset/crime-reports) |
| Milwaukee        | [https://data.milwaukee.gov/dataset/wibr](https://data.milwaukee.gov/dataset/wibr) |
| Nashville        | [https://data.nashville.gov/Police/Metro-Nashville-Police-Department-Incidents/2u6v-ujjs](https://data.nashville.gov/Police/Metro-Nashville-Police-Department-Incidents/2u6v-ujjs) |
| New York City    | [https://data.cityofnewyork.us/Public-Safety/NYPD-Complaint-Data-Current-Year-To-Date-/5uac-w243](https://data.cityofnewyork.us/Public-Safety/NYPD-Complaint-Data-Current-Year-To-Date-/5uac-w243) |
| Philadelphia     | [https://www.opendataphilly.org/dataset/crime-incidents](https://www.opendataphilly.org/dataset/crime-incidents) |
| Phoenix          | [https://www.phoenixopendata.com/dataset/crime-data/resource/0ce3411a-2fc6-4302-a33f-1672](https://www.phoenixopendata.com/dataset/crime-data/resource/0ce3411a-2fc6-4302-a33f-1672) |
| Portland         | [https://www.portlandoregon.gov/police/71978](https://www.portlandoregon.gov/police/71978) |
| Raleigh          | [https://data-ral.opendata.arcgis.com/datasets/ral::raleigh-police-incidents-nibrs/about](https://data-ral.opendata.arcgis.com/datasets/ral::raleigh-police-incidents-nibrs/about) |
| Sacramento       | [https://data.cityofsacramento.org/datasets/0026878c24454e16b169b3fb26130751_0/explore](https://data.cityofsacramento.org/datasets/0026878c24454e16b169b3fb26130751_0/explore) |
| San Francisco    | [https://data.sfgov.org/Public-Safety/Police-Department-Incident-Reports-2018-to-Present/wg3w-h783](https://data.sfgov.org/Public-Safety/Police-Department-Incident-Reports-2018-to-Present/wg3w-h783) |
| Seattle          | [https://data.seattle.gov/Public-Safety/SPD-Crime-Data-2008-Present/tazs-3rd5](https://data.seattle.gov/Public-Safety/SPD-Crime-Data-2008-Present/tazs-3rd5) |
| Virginia Beach   | [https://data.vbgov.com/dataset/police-incident-reports](https://data.vbgov.com/dataset/police-incident-reports) |
| Washington       | [https://opendata.dc.gov/datasets/crime-incidents-in-2018](https://opendata.dc.gov/datasets/crime-incidents-in-2018) |
Table 21: Data mapping comparison: NYC’s individual incident descriptions and our Level 2 categorization. For Police Department Description, we only include the prefix (first word) from the first description, and include the suffixes also included under the same Offense Description. Each row corresponds to a distinct crime prefix in the Police Department Description. All terms are replicated here verbatim from the data file.

| Standardized Level 2 | NYC Offense Description | Law desc. | Police Department Description(s) |
|----------------------|-------------------------|-----------|----------------------------------|
| Homicide             | Murder & Non-negl Manslaughter | Felony    |                                  |
| Homicide             | Homicide, negligent, vehicle | Felony    | Homicide Negligent, Vehicle      |
| Homicide             | Homicide, negligent, unclassifie | Felony    | Homicide, negligent, unclassifie  |
| Rape                 | Rape                     | Felony    | Rape 1                           |
| Rape                 | Sex Crimes               | Felony    | Sodomy 1,2,3                     |
| Robbery              | Robbery                  | Felony    | Assault, Chain Store, Payroll, Atm Location, Bank, Bar/restaurant, Begin As Shoplifting, Bicycle, Bodega/convenience Store, Car Jacking, Check Cashing Business, Clothing, Commercial Unclassified, Delivery Person, Doctor/dentist Office, Dwelling, Gas Station, Hijacking, Home Invasion, Licensed For Hire Vehicle, Licensed Medallion Cab, Liquor Store, Neckchain/jewelry, Of Truck Driver, On Bus/ Or Bus Driver, Open Area Unclassified, Personal Electronic Device, Pharmacy, Pocketbook/carryed Bag, Public Place Inside, Residential Common Area, Unlicensed For Hire Vehicle |
| Assault              | Felony Assault           | Felony    | Assault 2,1,unclassified         |
| Assault              | Miscellaneous Penal Law  | Felony    | Aggravated Harassment 1          |
|                      | Harrassment 2            | Violation |                                  |
|                      | Harrassment 2            | Violation |                                  |
| Burglary             | Burglary                 | Felony    | Burglary, Truck Unknown Time; truck Day; truck Night; commercial,day; commercial,night; commercial,unknown Ti; unclassified,day |
| Burglary             | Burglary                 | Felony    | Burglary,unclassified,night; unclassified,unknown; unknown Time |
| Theft                | Grand Larceny            | Felony    | Larceny, Grand Of Auto - Attem; Moped; Auto; Motorcycle; Truck |
|                      | Of Motor Vehicle         |           |                                  |
Theft | Grand Larceny | Felony | Larceny, grand By Acquiring Lost Credit Card; By Bank Acct Compromise-atm Transaction; By Bank Acct Compromise-reproduced Check; By Bank Acct Compromise-teller; By Bank Acct Compromise-unauthorized Purchase; By Bank Acct Compromise-unclassified; By Credit Card Acct Compromise-existing Acct; By Dishonest Emp; By Extortion; By False Promise-in Person Contact; By False Promise-not In Person Contact; By Identity Theft-unclassified; By Open Bank Acct; By Open Credit Card (new Acct); By Open/compromise Cell Phone Acct; By Theft Of Credit Card; From Building (non-residence) Unattended; From Eatery, Unattended; From Night Club, Unattended; From Open Areas, Unattended; From Person, Bag Open/dip; From Person, lush Worker(sleeping/uncon Victim); From Person, personal Electronic Device(snatch); From Person,pick; From Person,purs; From Person, uncl; From Pier, Unattended; From Residence, Unattended; From Residence/building, unattended, Package Theft Inside; From Residence/building, unattended, Package Theft Outside; From Retail Store, Unattended; From Store-shopl; Of Bicycle; Of Boat; Of Vehicular/motorcycle Accessories; Person, neck Chai

Theft | Grand Larceny | Felony | Larceny, grand From Truck, Unattended; From Vehicle/motorcycle; From Boat, Unattended

Theft | Petit Larceny | Misdemeanor | Larceny, petit From Auto; From Boat; From Truck
| Level-2 Offense | Offense Data Categorization |
|---------------|-----------------------------|
| Theft         | Petit Larceny Misdemeanor   |
|               | Larceny, petit By Acquiring Los; By Check Use; By Credit Card U; By Dishonest Emp; By False Promise; From Building, unattended; Package Theft Inside; From Building, unattended, Package Theft Outside; From Coin Machin; From Open Areas.; From Pier; From Shop; Of Bicycle; Of Boat; Of License Plate; Of Vehicle Access; Of Animal; check From Mailbox; Of Auto - Attem; Of Moped; Of Auto; Of Motorcycle; Of Truck |
| Theft         | Other Offenses Related To Theft Misdemeanor |
|               | Theft Of Services, Unclassified; related Offenses, unclass |
| Drug          | Dangerous Drugs Felony      |
|               | Controlled Substance, Intent To; Controlled Substance, Possess; Controlled Substance, Sale 4; Controlled Substance, Sale 5; Controlled Substance, intent To; Controlled Substance, possess.; Controlled Substance, possess.; Controlled Substance, sale 1; Controlled Substance, sale 2; Controlled Substance, sale 3; Drug Paraphernalia, Posses; Drug, Injection Of; Marijuana, Possession 1, 2 & 3; Marijuana, Sale 1, 2 & 3; Sale School Grounds; Sale School Grounds 4; Sales Of Prescription |
| Drug          | Dangerous Drugs Misdemeanor |
|               | Controlled Substance, Possess; Drug Paraphernalia, Possess; Marijuana, Possession 4 & 5; Marijuana, Sale 4 & 5; Poss Meth Manufact Material; Possession Hypodermic Instrume |

Note: The first column describes the level-2 offense. The right three columns describe incident data categorizations that comprise the level 2 mapping in our hierarchy. We omit the top-line offense description that re-appears in the more detailed police department description. These descriptions are verbatim from the raw data.