

The Unintended Impact of Pretrial Detention on Case Outcomes: Evidence from New York City Arraignments

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Abstract

In the United States, over 400,000 individuals are in jail daily waiting for their criminal cases to be resolved. The majority of detainees are held because they cannot post bail. We estimate the impact of being detained pretrial on the likelihood of being convicted and sentence length using data on nearly a million criminal cases in New York City. Causal effects are identified using variation across arraignment judges in their propensities to detain defendants. We find that being detained increases the probability of conviction by 13 percentage points for felony defendants. Although pretrial detention lowers the probability of rearrest while cases are being adjudicated, this reduction in criminal activity is mostly offset by an increase in recidivism within 2 years after disposition. Higher pretrial detention rates among minority defendants explain 40 percent of the black-white gap in rates of being sentenced to prison and 28 percent of the Hispanic-white gap.

Excessive bail shall not be required, nor excessive fines imposed, nor cruel and unusual punishments inflicted. (US Constitution, Eighth Amendment)

1. Introduction

On any given day in the United States, over 400,000 individuals (or one of every 550 adults) are in jail awaiting the resolution of their criminal cases (Wagner and

We would like to thank Jeffrey Denning, Brigham Frandsen, David Frisvold, Lars Lefgren, Steven Levitt, Jens Ludwig, Justine Olderman, Nathan Petek, Mary Phillips, Joseph Price, Michael Riley, and John Whiston for helpful comments and discussion. We are also grateful for the data provided by the New York State Division of Criminal Justice Services (DCJS). The opinions, findings, and conclusions expressed in this paper are those of the authors and not those of the DCJS. Neither New York State nor the DCJS assumes liability for its contents or use thereof.

[*Journal of Law and Economics*, vol. 60 (August 2017)]

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Sakala 2014) at a large cost to both local and federal governments.¹ The number of individuals detained pretrial is also growing, with an increase of over 20 percent between 2000 and 2014 (Wagner 2015). A large majority of these detainees are prevented from returning home while their cases are adjudicated because they do not have access to the financial resources to post bail. Of the 38 percent of felony defendants who are detained, nine of 10 fail to post bail (Reaves 2013).² Of those held on bail, 81 percent have bail set at less than \$5,000, and 44 percent have bail set at less than \$1,000 (Phillips 2012). Given that the high levels of pretrial detention are driven by failure to post bail, access to financial resources is often a deciding factor in who experiences the effects of pretrial detention.

In this paper, we instrument for pretrial detention status using the degree to which judge-specific detention rates deviate from the average detention rate across judges, which allows us to isolate the causal effect of detention on conviction and other case outcomes. The law allows a judge to set bail or detain a defendant to ensure appearance at court or in the interests of public safety. Pretrial detention is intended to play no further role in legal proceedings. However, detained individuals may lose bargaining power to the prosecutor because of the costs of being detained, and they face stronger incentives to plead guilty, even if they are innocent. Detainees might miss work and therefore forgo income or even lose employment, and they are unable to attend to family responsibilities or access their social support network. For defendants charged with minor offenses, pleading guilty often results in immediate release. Because time spent in jail awaiting the resolution of the case is counted against sentence length, the cost of pleading guilty is lower for detained defendants because they have effectively paid part of the price of conviction in advance. In general, it appears that being detained may shift the bargaining power to the prosecutor because the detained defendant is incurring a cost.

We use data from nearly a million criminal cases in New York City from 2009 to 2013, including information linking defendants with arraignment judges. The arraignment judge's only involvement with the case comes at the arraignment; a different judge is randomly assigned to preside at subsequent hearings. Our objective is to estimate how an increase in the probability of pretrial detention, driven solely by the arraignment judge, affects the probability of being convicted, pleading guilty (a subset of convictions), and sentence length. We present evidence that assignment to an arraignment judge is conditionally random for felony cases.

We find that pretrial detention increases the probability that a felony defendant will be convicted by at least 13 percentage points. We also find significant effects for misdemeanor cases, although these are not the primary focus of our analysis. The increase in conviction rates is driven by detainees accepting plea deals more frequently. We also find evidence that detention increases minimum

¹ The cost to county governments alone is \$9 billion per year (Holder 2011).

² The Bureau of Justice Statistics does not compile national statistics on pretrial detainees in misdemeanor cases.

sentence length. In addition, individuals who are detained pretrial are less likely to obtain a reduction in the severity of the crimes with which they are charged.

One potential benefit of pretrial detention is reducing the crime rate by incapacitating suspected criminals. We find that detention lowers the probability of arrest pretrial by over 10 percentage points. However, most of the reduction in criminal activity is reversed within 2 years after disposition by increases in recidivism. As a result, the net effect of detaining people pretrial on the crime rate is a small improvement at best.

Furthermore, pretrial detention is responsible for 40 percent of the black-white gap in incarceration among individuals charged with a crime and 28 percent of the Hispanic-white gap. Being detained pretrial does not affect blacks or Hispanics differently than whites, but minority defendants fail to make bail at higher rates than their white counterparts and are consequently detained more often. As a result, they are disproportionately affected by pretrial detention. The disparity in the rates at which whites and minorities are detained pretrial is an important factor in explaining why minorities are at least 25 percent more likely to be sent to prison, conditional on being charged with a crime.

The paper proceeds as follows. Section 2 provides a review of relevant literature. Section 3 describes the institutional setting of the New York City arraignment process. Section 4 describes the data used in the analysis. Section 5 describes the instrumental variables (IV) methodology and discusses potential concerns with it. Section 6 presents the main results from our analysis along with suggestive evidence for the underlying mechanism and robustness checks. Section 7 concludes.

2. Literature Review

Numerous studies have established the positive correlation between detention and the probability of being convicted, even after controlling for observable criminal history, case, and defendant characteristics (Ares, Rankin, and Sturz 1963; Rankin 1964; Kellough and Wortley 2002; Williams 2003; Leiber and Fox 2005; Phillips 2008). Although these results are certainly consistent with a causal effect of detention on conviction, they cannot rule out the possibility that systematic, unobserved differences between detained and released defendants are responsible for part or all of the relationship.

One branch of literature uses assignment to a judge (hereafter, judge assignment) in a similar way to analyze postsentence outcomes for convicted criminals: Kling (2006) finds no consistent evidence of adverse effects from serving a longer sentence on employment and earnings. Aizer and Doyle (2015) find that juvenile incarceration decreases the likelihood of high school completion and increases the likelihood of adult recidivism. Mueller-Smith (2014) finds that a 1-year prison term generates \$56,200–\$66,800 in social costs through increased recidivism, decreased employment and wages, and increased dependence on public assistance.

Concurrent work has used the same methodology to explore the impact of pretrial detention. Stevenson (2016) uses variation in the bail-setting patterns of ar-

raignment magistrates in Philadelphia to find that defendants who fail to make bail are more likely to be convicted. Gupta, Hansman, and Frenchman (2016) also use data from Pennsylvania and estimate that being assigned money bail increases the probability of both pleading guilty and reoffending. Dobbie, Goldin, and Yang (2016) use data from Philadelphia and Miami and find that pretrial detention increases convictions and reduces employment. By contrast, our data from New York City, which has the second-largest jail system in the country, allow us to estimate precise causal effects for felony and misdemeanor cases separately using a larger sample of arraignments and arraignment judges. We also explore the impact of pretrial detention on the overrepresentation of minorities in the inmate population and allow judges' behavior to be nonmonotonic with regard to features of the cases and demographic characteristics.

3. New York City's Arraignment Process

In New York City, after an individual is arrested, he is taken to the local police precinct to be booked and fingerprinted. Prosecutors are assigned immediately after arrest and are responsible for the case until disposition by trial or plea. Assignment to a prosecutor (hereafter, prosecutor assignment) is not linked to judge assignment in New York City, unlike some jurisdictions, and prosecutors have no influence over judge assignment. The fingerprints and booking information taken at the precinct are processed by the state and are used to provide a criminal history of the defendant for the arraignment judge. The individual is then moved to a holding cell in the county courthouse corresponding to the county of arrest (Bronx, Kings, New York, Queens, or Richmond) to await arraignment. About a fourth of misdemeanor arrestees are given desk-appearance tickets by the police in the booking precinct and are not held prior to arraignment (Barry 2014). At the courthouse, the Criminal Justice Agency interviews the defendant and provides a bail recommendation for the arraignment judge. The defendant is then allowed to meet with his defense attorney, who may be private or assigned, before the arraignment. Anyone who does not have a private defense attorney present is represented by a public defender. The indigent defense organizations have their own criteria for determining whether individuals are eligible for their services at subsequent hearings. The lawyers interview defendants before their arraignment hearings to assess eligibility.³

The process of judge assignment is central to our identification strategy. If individuals are sorted among arraignment judges on the basis of unobservables that are correlated with case outcomes (for example, the strength of evidence against them), then judge-level variation in propensity to detain (through denial of bail or systematically setting higher bail amounts) will not be a valid instrument. Arraignments in New York City take place every day of the year in two sessions: day

³ Conversations with public defenders and arraignment clerks revealed that the population of criminal defendants in much of New York City is almost exclusively low income. For example, in the Bronx nearly everyone awaiting arraignment is poor enough to qualify for public defense services.

(9 a.m. to 5 p.m.) and night (5 p.m. to 1 a.m.). There is a supervising judge in each county who determines the schedule assigning judges to preside at each arraignment shift. The supervising judges create schedules for 4 weeks at a time. The same judge presides over day sessions (or night sessions) during an entire business week (Monday through Friday). In a session, a judge has an average of 6 minutes per arraignment. Most felony defendants are arraigned within 1 to 2 days of their arrest, during which time they are kept in holding cells.⁴ Before our sample period, the New York City criminal courts received a mandate from the Court of Appeals to arraign within 24 hours. The average time from arrest to arraignment in our sample period fell from 25.39 hours in 2009 to 21.44 hours in 2013 (Barry 2014). Given the short window between arrest and arraignment, police officers are unlikely to manipulate which judge a defendant will see at arraignment. Each defendant's arraignment is scheduled by an arraignment coordinator, whose primary objective if multiple courtrooms are operating is maintaining balance in the workloads across courtrooms, subject to the constraint of ensuring that cases are arraigned within 24 hours of arrest. Arraignment coordinators do not assign any other court actors to cases. Some intentional sorting across arraignment shifts does occur. Defendants with desk-appearance tickets, who are generally charged with relatively minor offenses, are grouped so that everyone in a borough who receives a desk-appearance ticket on the same day will be arraigned in the same session. Beyond the grouping of defendants with desk-appearance tickets, there is no institutionalized sorting. Below we test for conditionally random judge assignment.

The arraignment is the first time the defendant appears before a judge, where he is formally informed of the charges being brought against him. In felony cases, the judge releases the defendant on his own recognizance, sets a bail amount, or orders the defendant held without bail (Phillips 2012).⁵ Misdemeanor cases may be adjudicated at arraignment if the defendant pleads guilty or the case is dismissed. In practice, defendants are generally not allowed to plead guilty at arraignment if there is a complaining witness (for example, the victim in an assault case). In determining the terms of a guilty plea at arraignment, the judge may not offer reduced charges but can offer specific punishments for the existing charges. There is less room for judicial discretion in determining whether a case will be dismissed, because judges may dismiss a case at arraignment only when a specific legal defect exists. Because some misdemeanor cases are disposed of at arraignment, and we do not observe which of those defendants would have been detained if their cases had not been immediately resolved, we interpret estimates from the misdemeanor subsample with more caution than those from the felony subsample.

⁴ Individuals facing a charge in the least serious category, nonviolent class E felonies, may be released awaiting an arraignment. In this situation, the arraignment is scheduled weeks or months ahead of time.

⁵ With the exception of a pilot program in Queens, supervised release was not available in New York City during our sample period.

In New York county courts, aside from being prohibited from denying bail in misdemeanor cases or for the sake of public safety, the judge has complete discretion in the bail decision (Crim. Proc., sec. 530.20). Judges may set different amounts for different types of bail; for example, a defendant could be required to either post \$1,000 in cash or secure a \$2,000 bond. Following Phillips (2012), we use the amount of cash required for release. If a cash alternative is not set, we use the bond amount. Defendants who fail to make bail are detained until their cases are adjudicated. Nonmonetary conditions are not used in conjunction with bail in New York City. Almost all pretrial detainees are housed in jails on Rikers Island, with the remainder kept in county jails throughout the city. Cases may be resolved at trial with a conviction or acquittal, but they may also be resolved through plea deals and dismissals. Trials are far less common than plea deals; only .7 percent of the cases in our sample went to trial, whereas 64.3 percent were disposed of through plea deals. In a large majority of the remaining cases, the charges are dismissed. A different judge is randomly assigned to the case after the arraignment, so the arraignment judge can influence the case's outcomes only through pretrial status assignment (NY Codes R. & Regs. tit. 22, sec. 200.11[c]). We are interested in how the detention status resulting from the arraignment hearing affects cases' outcomes, including guilty verdicts, guilty pleas, the relationship between arraignment charges and disposition charges, and sentencing.

4. Data

Our data cover all felony and misdemeanor criminal cases in the New York City criminal courts between 2009 and 2013. To have a precise measure of a judge's severity, we restrict the sample to cases in which the arraignment was before a judge who saw at least 500 arraignments in our sample. This eliminates 24,679 cases (2.5 percent of the original sample). Along with pretrial status, bail amount, demographics, criminal history, and case outcomes, the data include the arraignment shift, courthouse, and judge's identification code. Arraignments in our sample were presided over by 212 judges, all of whom presided over arraignments for both felony and misdemeanor cases. The average number of arraignments per judge per year is 1,523. Of these, 291 are felony cases. We treat a defendant as detained pretrial if he was remanded without bail or bail was set by the arraignment judge but never posted by the defendant. We observe whether, but not when, bail was posted and therefore have no way of determining how many defendants in our sample were detained for only part of the period between arraignment and case disposition. Therefore, defendants who posted bail after a period of pretrial detention are categorized as released for our analysis. This could bias results toward 0, because some defendants in the control group may have experienced the treatment. Figure 1 presents histograms of pretrial detention lengths among detainees. The bumps in the distributions around 180 days for felonies and 90 days for misdemeanors are a result of laws meant to protect the right to a speedy trial: the state is generally required to bring cases to trial within

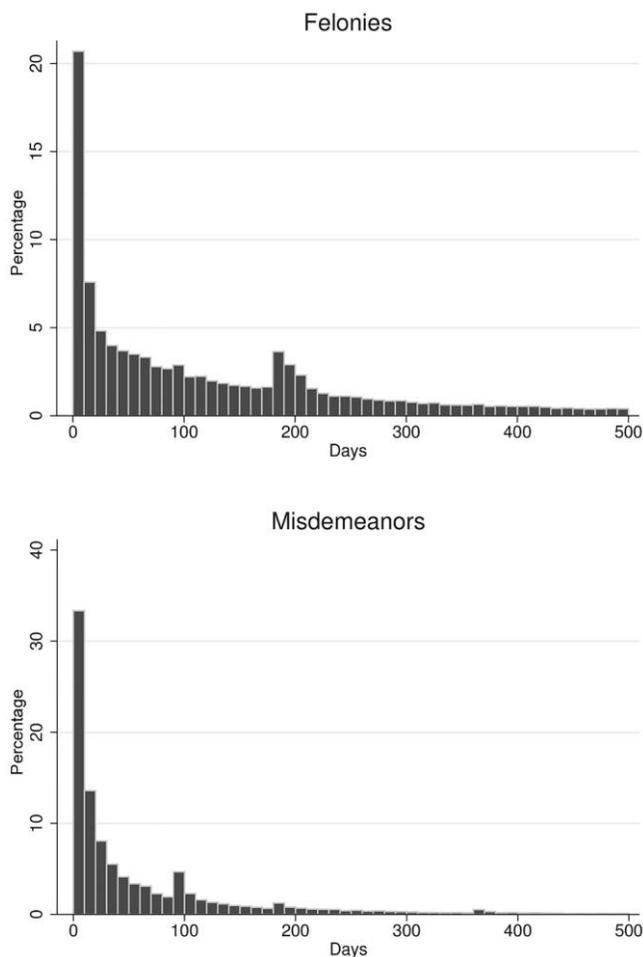


Figure 1. Length of detention if detained

6 months for felony cases or 90 days for misdemeanor cases, although prosecutors can successfully move for an extension in a variety of circumstances (Crim. Proc., sec. 30.30). Figure A1 shows how the sample is distributed across the most common types of offenses.⁶ Table 1 displays summary statistics. Detention rates are higher for defendants in felony cases compared with misdemeanor cases (43 percent versus 12 percent). In both groups, the systematically worse outcomes for detainees are apparent: almost two of three released individuals are convicted, compared with close to four of five detainees. Figure 2 shows the distribution of bail amounts for misdemeanor and felony defendants and illustrates that, re-

⁶ All appendix tables and figures are available in the online appendix.

Table 1
Arraignments in New York City: Summary Statistics

	Felonies		Misdemeanors		All Arraignments
	Released	Detained	Released	Detained	
Demographics:					
Age	31.0	32.5	31.6	35.4	31.9
Female	.20	.10	.19	.13	.17
White	.13	.08	.14	.10	.13
Black	.46	.56	.46	.59	.48
Hispanic	.35	.34	.34	.29	.34
Criminal history:					
First-time offender	.68	.36	.66	.24	.59
Prior felony arrest	2.2	5.5	2.2	6.8	2.9
Prior misdemeanor arrest	3.0	7.6	3.8	13.2	5.0
Prior felony conviction	.4	1.2	.4	1.3	.6
Prior misdemeanor conviction	1.4	5.2	1.8	10.1	2.9
Sex offender	.01	.02	.01	.02	.01
Arraignment information:					
Detained pretrial	.00	1.00	.00	1.00	.20
Released on own recognizance	.72	.00	.94	.00	.72
Released on bail	.28	.00	.06	.00	.08
Held on bail	.00	.93	.00	.95	.19
Remanded	.00	.07	.00	.05	.01
Counts	1.4	1.2	1.1	1.1	1.1
Case outcome:					
Convicted	.64	.79	.65	.81	.68
Pled guilty	.59	.70	.63	.77	.64
Dismissed	.33	.18	.34	.19	.31
Went to trial	.01	.02	.00	.00	.01
Sentenced to incarceration	.11	.53	.04	.50	.15
Reduction in charges	.84	.72	.79	.33	.74
Minimum sentence length (days)	71	400	15	35	66
Maximum sentence length (days)	85	532	15	37	83
<i>N</i>	139,801	105,259	639,141	89,614	973,815

Note. Values are for arraignments in the New York City Division of Criminal Justice Services sample. Released individuals were released any time after arraignment. Detained individuals were detained between arraignment and the resolution of their cases.

gardless of the bail amount, defendants are more likely to be held on bail than to post bail. Defendants who were detained pretrial had failed to post bail; defendants who were released had posted bail. The summary statistics in Table 1 also make clear that, with few exceptions, cases end in either conviction or dismissal. Thus, although we do not estimate the effects on dismissal directly, the impact of detention on dismissal would be equal and opposite to the estimates we find for conviction.

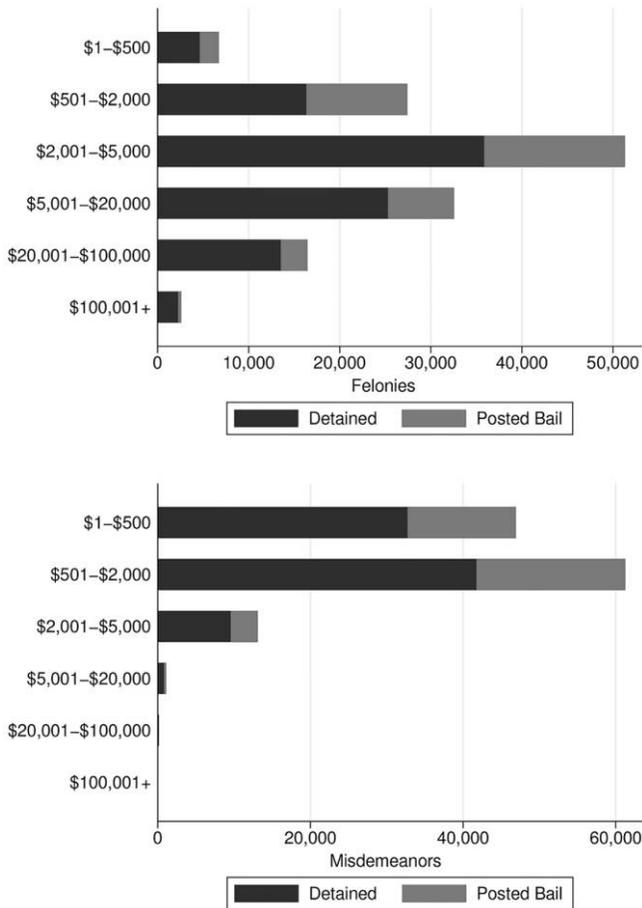


Figure 2. Bail amounts by crime type

5. Methodology

Our baseline ordinary least squares (OLS) results come from estimating the following equation:

$$y_i = \beta_0 + \beta_1 \text{Detained}_i + \beta_2 \mathbf{X}_i + \beta_3 \mathbf{C}_i + \beta_4 \mathbf{T}_i + \varepsilon_i, \tag{1}$$

where y_i is a dummy for whether defendant i was convicted, a dummy for whether he pled guilty, or the minimum sentence length. The variable *Detained* is a dummy for being detained pretrial. The term \mathbf{X}_i is a vector of demographic characteristics including race, a cubic in age, gender, and a police-precinct fixed effect. The term \mathbf{C}_i is a vector of criminal history features including dummies for first-time offender, sex-offender status, whether the current charge is an at-

tempt charge, whether the charge is a violent felony, whether a firearm was involved, whether a weapon was involved, whether a child was involved, whether the charge is a hate crime, whether the charge is a drug crime, the class of the most serious offense charge (for example, class B felony), Uniform Crime Reporting (UCR) crime-type code for the most serious offense charge (for example, aggravated assault or driving under the influence [DUI]), and cubics in the number of prior felony arrests, prior misdemeanor arrests, prior felony convictions, prior misdemeanor convictions, and the number of counts in the current case. The term T_i is a vector of fixed effects for year, month, day of week, and courtroom and shift (for example, the Tuesday night shift in courtroom A of the Bronx courthouse in July 2012), hereafter referred to as time-by-shift fixed effects. Standard errors are clustered at both the defendant and time-by-shift level.⁷

Our objective is to estimate the causal effect of being detained on case outcomes, which we do by instrumenting for Detained in regression equation (1) using judge-level variation in the propensity to detain pretrial for a given crime type.⁸ We calculate a leave-out mean measuring the degree to which each judge deviates from the full-sample average detention rate for each UCR crime-type code. We construct a residualized leave-out mean to account for sorting of cases by time and place. For example, as described above, defendants with desk-appearance tickets are often grouped in the same arraignment session. If some arraignment judges are scheduled to preside over those sessions more often, the simple leave-out mean would be a biased measure of judges' tendencies. To control for systematic differences across shifts, we use the residual from regressing Detained on time-by-shift fixed effects, $\overline{\text{Detained}}$, in constructing the leave-out mean instrument. By using the residualized detention measure, we are comparing judges' propensities to release defendants relative to other cases arraigned during the same year and month, on the same day of the week, in the same courtroom, and during the same shift. Our instrument is similar to the approach used in Dobbie, Goldin, and Yang (2016), but our version is calculated within crime types:

$$z_{jc} = \frac{1}{n_{jc} - 1} (\sum_k \overline{\text{Detained}}_k - \overline{\text{Detained}}_i) - \frac{1}{n_c - 1} (\sum_m \overline{\text{Detained}}_m - \overline{\text{Detained}}_i),$$

where the first term divides the total number of people charged with crime type c and detained by judge j (indexed by k and excluding individual i) by the total number of defendants charged with crime type c who appeared before judge j (n_{jc}), excluding individual i , and the second term is the total number of people in our sample who were charged with crime type c and detained (indexed by m and excluding individual i), divided by the total number of people charged with crime type c in the sample (n_c), excluding individual i . Positive values of z_{jc} correspond with judges who detain more than average for the sample, and negative values indicate lower-than-average detention rates.

⁷ The results are robust to clustering by judge, by courthouse, and time by arraignment shift.

⁸ In theory, using judge fixed effects as our instruments would introduce bias. In practice, we find that the results are qualitatively similar in our context.

One concern with standard IV in this case is that the monotonicity assumption may be violated. This assumption requires that if defendants on average are more likely to be detained by judge A than judge B, anyone who would be detained by judge B would also be detained by judge A (Imbens and Angrist 1994). If arraignment judges vary their treatment on the basis of defendants' characteristics, the standard monotonicity assumption will not hold. Monotonicity violations result in bias if there is treatment-effect heterogeneity along the same dimensions as the monotonicity-assumption violations. For example, suppose the only violation of the monotonicity assumption is that judge A has a relatively high detention rate on average but is lenient with Hispanic defendants. As long as the effect of pretrial detention is homogenous across Hispanics and non-Hispanics, regular IV estimates using average judge detention rates will be unbiased. If detention affects Hispanics differently, however, then the standard estimates will be biased. The extent of the bias depends on the degree to which monotonicity is violated and the treatment effect for defiers.⁹ Figure A2 presents evidence that the judges in our sample display different leniency orderings depending on crime type and defendant characteristics. This is consistent with both monotonicity violations and sampling error. In the absence of a formal test of monotonicity, we instead compare results that rely on a relaxed monotonicity assumption with those that require a higher degree of monotonicity. The instrument defined above allows for nonmonotonicity across crime types, because it measures judges' deviations from caseload averages within crime types. To further relax the monotonicity assumption, we follow Mueller-Smith (2014) in constructing a set of candidate instruments that measure the degree to which judges deviate from average trends across the full sample in their tendency to detain people in certain groups. These groups are defined by criminal history features (a dummy for first-time offender and sex offender and the numbers of prior felony arrests, prior misdemeanor arrests, prior felony convictions, prior misdemeanor convictions, and counts in the current case), most serious charge (UCR crime-type code), race, and gender and interactions between every pairwise combination of these characteristics. We use a least absolute shrinkage and selection operator (Lasso) procedure described in Belloni et al. (2012) to reduce noise and avoid the many-instruments problem by selecting only the most informative of these instruments. This process leaves us with 27 instruments for the felony subsample and 23 for the misdemeanor subsample; the five most powerful instruments for each sample are listed in Table A1. The new, less restrictive, monotonicity assumption for these specifications is that judges' detention decisions are monotonic within groups of defendants with similar characteristics. The estimates based on the more relaxed monotonicity assumption (instrumental variables nonmonotonicity, or IVNM) tend to be fairly similar to those using the instrument measuring judges' tendencies within crime types. They are usually, though not always, slightly closer to 0. We cannot account for possible monotonicity violations across unobserved characteristics, but the similarity of the IV and IVNM results suggests that bias from violations

⁹ See the appendix of Mueller-Smith (2014) for a detailed example and discussion of bias from monotonicity violations in a similar context.

across observables is minimal. If the results were highly sensitive to allowing for nonmonotonicity along all observable dimensions, then we would be concerned that unobservable dimensions were also an important factor. The stability of our results is reassuring.

The randomness of arraignment-judge assignment is essential to the validity of our estimation strategy. To test for conditionally random judge assignment, we regress the measure of judge severity within crime types on demographic and criminal history characteristics, conditioning on time by time-by-shift fixed effects. We then test for the joint significance of the defendant's characteristics. This approach allows us to test for the random assignment of cases to judges within, for example, daytime arraignment shifts in Queens courtroom 2 on Tuesdays in July 2012. The demographic characteristics included are age, sex, and race and ethnicity dummies. The criminal history features are indicators for whether the defendant is a sex offender or a first-time offender, the number of counts in the current case, the number of previous felony arrests and convictions, and the number of previous misdemeanor arrests and convictions. Table 2 presents the results. The p -value of .216 associated with the felony subsample indicates that we cannot reject the null hypothesis that the defendant's characteristics are not significant factors in predicting a judge's leniency. This affirms that the process of felony case assignment to arraignment judges is conditionally random. The results for the misdemeanor subsample suggest that there is significant sorting across judges: the p -value on the test of joint significance is less than .001.

To estimate the degree to which nonrandom case assignment could bias our results, we use a methodology developed in Oster (forthcoming), which extends the work of Altonji, Elder, and Taber (2005). We estimate the reduced-form effect of the within-crime-type instrument described above on the probability of conviction. Following Oster (forthcoming), we then determine the level of selection on unobservables that would imply a true treatment effect of 0.¹⁰ We find that the level of selection on unobservables would need to be 1.35 times larger than the level of selection on observables for the true effect of arraignment-judges' severity on conviction to be equal to 0. As Altonji, Elder, and Taber (2005) discuss, if we observe the most important factors that influence judges' decision making, then the true correlation with the unobservables will be smaller than the correlation with the observables. We conclude that the bias introduced by the sorting of misdemeanor cases is unlikely to account for the entire effect of arraignment judges' tendencies on case outcomes. Therefore, we include results for the misdemeanor subsample but emphasize that they should be interpreted with caution.

In the online appendix, we provide evidence that the arraignment judge is a stronger predictor of the defendant's arraignment outcome than of the defendant's observable characteristics going into arraignment. We first estimate the ex ante probability that each defendant would be detained on the basis of his demographic and criminal history characteristics and time-by-shift fixed effects. We then regress these predicted probabilities on a full set of judge dummy variables,

¹⁰ We use Oster's recommended approach to assign an upper bound to the R^2 term from a hypothetical regression that includes both observables and unobservables.

Table 2
Test for Random Assignment to Judges by Judges' Severity

Variable	Felonies	Misdemeanors
Demographics:		
Age	.000 (.000)	.000 (.000)
Female	-.013 (.006)	.003 (.005)
White	.027 (.015)	.009 (.006)
Black	.048 (.014)	.018 (.006)
Hispanic	.042 (.014)	.012 (.006)
Criminal history:		
Sex offender	.028 (.026)	.005 (.015)
First-time offender	.007 (.006)	.015 (.004)
Prior felony arrest	-.001 (.001)	.004 (.001)
Prior misdemeanor arrest	-.002 (.001)	.000 (.000)
Prior felony conviction	.003 (.002)	-.007 (.002)
Prior misdemeanor conviction	.001 (.001)	.001 (.001)
Counts	.000 (.000)	.000 (.000)
<i>F</i> -statistic	1.152	9.016
<i>p</i> -Value	.216	.000

Note. Values are conditional on time-by-shift fixed effects. *F*-statistics are for the joint significance of defendants' characteristics. Standard errors are in parentheses.

controlling for time-by-shift fixed effects, and test for the joint significance of the judge effects. Table A4 compares the *F*-statistics from these regressions with the *F*-statistics from testing the joint significance of judge effects in a regression using actual pretrial detention status, controlling for time-by-shift fixed effects. The *F*-statistics from the tests using predicted pretrial status probabilities are all less than 2 for the felony subsample. These reflect relatively small differences in the ex ante characteristics of defendants who appear before different judges. By contrast, the tests using actual pretrial status yield high *F*-statistics, which confirms that judge assignment matters substantially in terms of a defendant's arraignment outcome. The same general pattern exists for misdemeanor cases, although the higher *F*-statistic associated with the predicted probability of being released on one's own recognizance reflects the higher degree of sorting we documented for this subsample. Thus, arraignment judges see fairly comparable groups of defendants ex ante, but the arraignment outcomes vary substantially across judges.

We estimate effects for the misdemeanor subsample but emphasize that they



Figure 3. Guilty pleas and dismissals at arraignment by crime type

should be viewed with caution. In addition to the sorting across arraignment judges, sample construction is more problematic for misdemeanors than felonies. Unlike felony cases, misdemeanor cases may be disposed of at arraignment if a case is dismissed or the defendant chooses to plead guilty. The fraction adjudicated at arraignment by crime type is shown in Figure 3. As discussed in more detail below, the results are somewhat sensitive to the inclusion or exclusion of cases that are adjudicated at arraignment.

6. Results

6.1. Instrumental Variables Results

Figures 4 and 5 display evidence of the causal relationship between detention and case outcomes. These graphs plot judge-specific outcome residuals by judge-specific detention residuals. Each point represents the average outcome residual and detention residual for an individual judge, after regressing on demographic, criminal history, and time and courtroom fixed effects. Average outcome residuals reflect the ultimate outcomes of cases that appeared before the judge at arraignment (not those over which the judge presided at trial). The slope of the fitted linear-regression lines confirms that simply appearing before a harsher arraignment judge increases a defendant's likelihood of being convicted and pleading guilty.

These effects are estimated formally for our sample of felony defendants in Table 3.¹¹ The IV estimates on the subsample that is convicted are similar to the OLS estimates. The IVNM specification produces coefficients that are slightly smaller than the standard IV estimates. The estimates are significant, both statistically and economically: all specifications indicate that pretrial detention increases the probability of conviction by over 10 percentage points. In our preferred specification, in which we relax the monotonicity assumption, being detained increases the probability of conviction by 13 percentage points and the probability of pleading guilty by 10 percentage points. The estimated effect of detention on pleading guilty is only about 25 percent smaller than the effect on conviction, which suggests that detention primarily affects conviction by inducing some individuals who would not have pled guilty if released to plead guilty after they are detained. Felony defendants without a criminal record are more responsive to pretrial detention. Table A10 displays estimates by first-time offender status using the IVNM specification. In felony cases, being detained increases the probability of conviction for people without a criminal record by 15.2 percentage points, compared with 11.5 percentage points for people with prior convictions.

The marginal defendant in this context is one for whom the assignment of arraignment judge influences whether he is detained. Some defendants would always be detained and some would always be released, regardless of which judge presided at the arraignment. We are estimating a local average treatment effect for defendants who could have been assigned a different pretrial status if they had come before a different arraignment judge. We use the approach described in Bhuller et al. (2016) to characterize the compliers in Tables A8 and A9. Within the felony and misdemeanor subsamples, we split the defendants into 12 mutually exclusive and collectively exhaustive subgroups based on quartiles of the predicted probability of pretrial detention and race. We estimate the share of compliers for each subgroup. Within each subgroup, there are fewer compliers among misdemeanor cases than felony cases. Blacks and Hispanics have roughly

¹¹ Coefficients with fewer controls are similar (see Tables A5–A7). The results are also robust to using only the judges in the top and bottom quartiles of our severity measure (see Table A23).

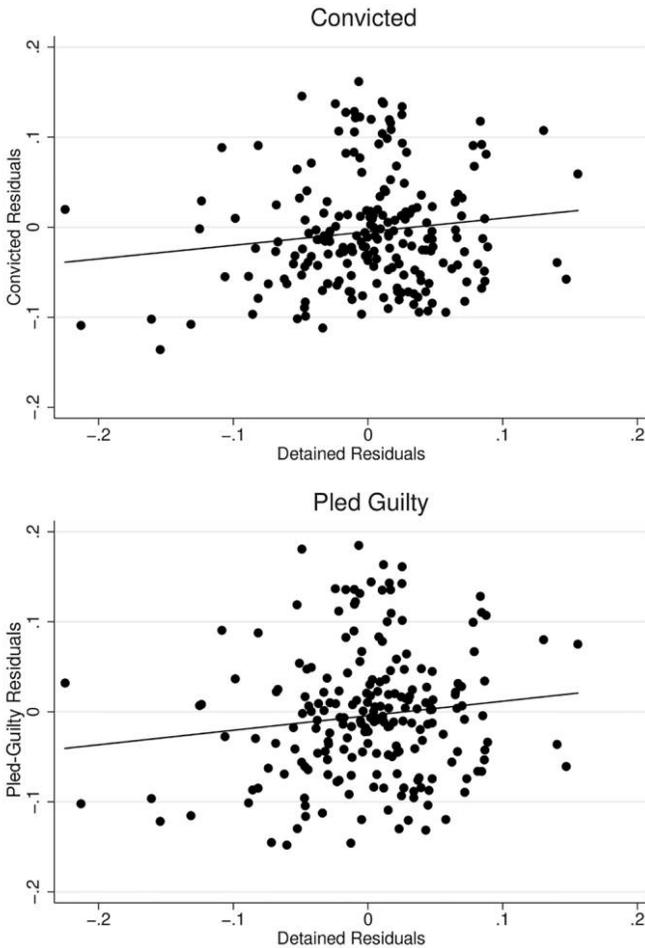


Figure 4. Instrumental variables scatterplots: felonies

equal probabilities of being compliers, but whites at each quartile of the predicted pretrial detention status are more likely to be compliers. In both subsamples, the share of compliers is lowest for the lowest quartile of the predicted probability of detention and increases through at least the third quartile, which indicates that arraignment-judge assignment is more likely to matter for defendants who are more likely to be detained.

Over 90 percent of detainees are held on bail, which means that they failed to put up the amount of money set by the arraignment judge. Although judges may differ in the frequency with which they set bail and the level at which they set it, the decision to set it or not appears to be more important in this setting. In our sample, 71 percent of felony defendants and 68 percent of misdemeanor defen-

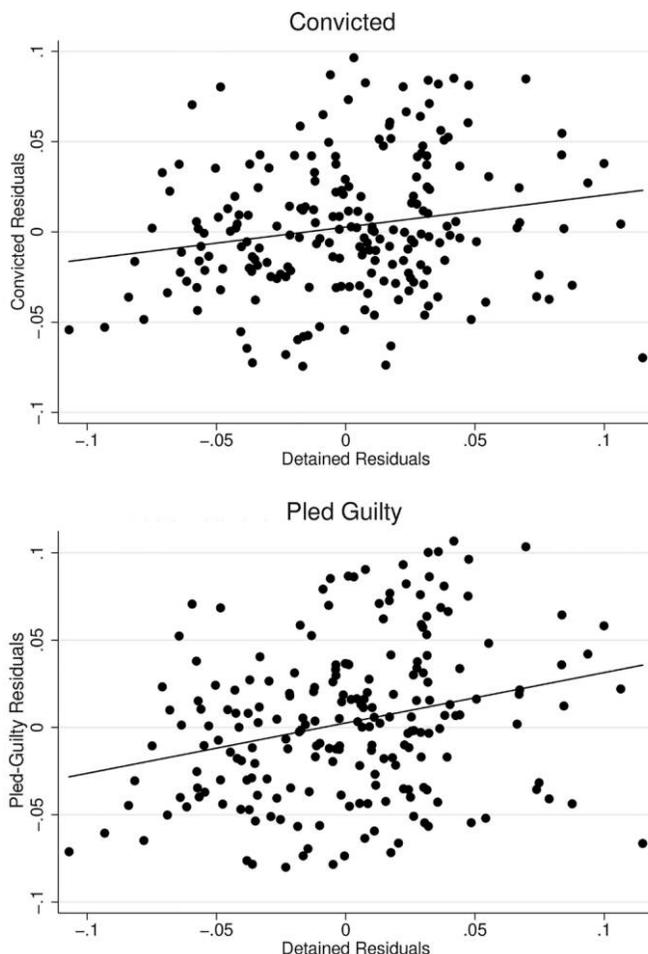


Figure 5. Instrumental variables scatterplots: misdemeanors

dants for whom bail is set are unable to post bail to secure their release. Most of the bail amounts standing between these defendants and their release are low: around 15 percent of defendants in felony cases that are held on bail would need less than \$2,000, and the majority have bail set at less than \$5,000. For the misdemeanor sample, well over half of individuals held on bail need less than \$2,000, and over a fourth need only a few hundred dollars. We do not observe individual or household income or other individual-level indicators of socioeconomic status, so we cannot directly measure the extent to which people from different income levels are affected by pretrial detention. The size of the bail amounts assigned to detainees suggests a group that is poor, with limited access to financial resources.

Table 3
Effect of Pretrial Detention on Felony Case Outcomes

Variable	OLS	IV	IVNM
Conviction	.149** (.002)	.142** (.020)	.130** (.006)
Pled guilty	.126** (.003)	.113** (.021)	.102** (.006)
Minimum sentence (days)	159** (3)	73** (26)	157** (8)
F-statistic		2,195	1,176
Instruments		1	27
N	245,060	243,375	242,241

Note. Values are estimates from ordinary least squares (OLS) and instrumental variables (IV) regression. The IV regressions use judge-level variation in the propensity to detain pretrial as an instrument. The instrumental variables nonmonotonicity (IVNM) regressions use instruments selected using a least absolute shrinkage and selection procedure. All specifications include demographic and criminal history controls and time and courtroom fixed effects. Standard errors clustered at the defendant and shift level are in parentheses.

** Significant at the 1% level.

Table 3 also shows evidence that the impact of detention extends to minimum sentence length. The coefficients are less stable, but all suggest a strong, positive effect of being detained. Because we are estimating effects for the full sample, including people who were not convicted, these estimates reflect the combined effect on conviction (a precondition to receiving a sentence) and sentence length once convicted. Our preferred specification suggests that being detained pretrial increases the minimum sentence length for felony defendants by over 150 days.

Our estimated effects are qualitatively comparable to those in concurrent work on pretrial detention and case outcomes. The results in Dobbie, Goldin, and Yang (2016) are closest to our own: they find that pretrial release decreases the probability of conviction by 14 percentage points. Gupta, Hansman, and Frenchman (2016) find that being assigned money bail increases the probability of conviction by 6 percentage points, and Stevenson (2016) finds that being detained pretrial increases the probability of conviction by 5 percentage points. The majority of cases in these papers are from Philadelphia, where a higher fraction of defendants have bail set than in New York. It is possible that differences in magnitudes are because the marginal defendant is different in a setting where nearly two-thirds of cases have money bail set (as in Philadelphia) compared with a setting where fewer than one-third are asked to post bail (as in New York City). All three papers pool felony and misdemeanor cases for their main results. We treat them separately, both because of the sample selection and randomization issues in the misdemeanor subsample and because it is plausible that pretrial detention affects the two groups of defendants through different mechanisms.

Table 4
Effect of Pretrial Detention on Misdemeanor Case Outcomes

Variable	OLS	IV	IVNM
Conviction	.100** (.002)	.077** (.020)	.074** (.005)
Pled guilty	.096** (.002)	.058** (.020)	.071** (.005)
Minimum sentence (days)	11** (0)	35** (4)	17** (1)
<i>F</i> -statistic		3,215	1,388
Instruments		1	23
<i>N</i>	728,750	726,167	725,480

Note. Values are estimates from ordinary least squares (OLS) and instrumental variables (IV) regression. The IV regressions use judge-level variation in the propensity to detain pretrial as an instrument. The instrumental variables nonmonotonicity (IVNM) regressions use instruments selected using a least absolute shrinkage and selection procedure. All specifications include demographic and criminal history controls and time and courtroom fixed effects. Standard errors clustered at the defendant and shift level are in parentheses.

** Significant at the 1% level.

6.2. Misdemeanor Cases

Because of the larger variety of outcomes possible at a misdemeanor arraignment and the higher degree of sorting across judges, we are more cautious about our analysis of this subsample. Table 4 shows the OLS estimate of the effect of detention on conviction for people charged with misdemeanors. Overall, the estimates are similar to those from the felony subsample. Our preferred IV specification, IVNM, indicates that being detained increases the probability of conviction by 7.4 percentage points and the probability of pleading guilty by 7.1 percentage points. Table A11 presents results from a subset of our specifications using various sample constructions that include or exclude cases that were dismissed or in which the defendant pled guilty at arraignment. Adding individuals who were not detained or convicted (cases dismissed at arraignment) results in higher point estimates, because it adds people to the control group who did not experience the outcome of interest. Adding individuals who were not detained and were convicted (cases with guilty pleas entered at arraignment) results in lower point estimates, because it adds people to the control group who did experience the outcome of interest. Including both of these groups—that is, using the full sample—produces almost identical results to those reported in Table 4. Despite this sensitivity, almost all specifications indicate a positive and statistically significant effect of detention on the probability of conviction and pleading guilty. As an additional check, we analyze a subset of misdemeanor cases that are the least likely to be adjudicated at arraignment. Figure 3 shows the fraction of cases adjudicated at arraignment by most serious offense charge. Simple assault, aggravated

assault, and DUI cases are the least likely to be disposed of at the arraignment hearing, especially through a guilty plea. Table A12 shows results from running our analysis on this subset of misdemeanor cases. Coefficients for crimes with low levels of adjudication at arraignment are substantially higher than for the full misdemeanor sample. Of course, the defendants in this group are a nonrandom sample, but the large effects provide evidence that the sample-selection issues created by arraignment adjudications are not generating the large and statistically significant effects we estimate for the misdemeanor sample. Thus, although we are less confident in the magnitude of the effect for misdemeanors, we find strong evidence that pretrial detention influences case outcomes unfavorably for misdemeanor cases. In Table A10, we find that, unlike the felony subsample, first-time-offender status is not a source of substantial heterogeneity. If anything, individuals without a criminal record are less affected by pretrial detention.

6.3. *Effects on Reductions in Charges*

Not only are detainees more likely to plead guilty, but the plea deals they accept are less favorable. Using information on crimes with which an individual is charged at arraignment (arraignment charges) and crimes of which he is ultimately convicted (disposition charges), we determine whether the most serious disposition charge belongs to a lower class than the most serious arraignment charge. Crimes are organized into ordered classes based on seriousness. The crimes in our sample fall into eight classes (class A–E felonies and class A, class B, or classless misdemeanors). Table A13 reports estimates of the effect of detention on charge reduction conditional on being convicted. We find evidence of large negative effects on reductions in charges, which means that detainees are less likely to be convicted of less serious crimes than the ones with which they were charged at arraignment. Individuals charged with felonies are 10 percentage points less likely to receive a reduction in charge class if they are detained. This effect could translate into more severe punishments for individuals who are subsequently arrested and charged again, because criminal history is an important dimension of sentencing guidelines. Many people who interact with the criminal justice system do so multiple times during the course of their lives. In our sample, only 59 percent of defendants were first-time offenders, with defendants averaging 3.5 prior felony and misdemeanor convictions. As individuals repeatedly face criminal charges, the negative consequences of being detained even once will continue to accrue.

6.4. *Effects on Rearrest*

One possible benefit of pretrial detention is that it prevents high-risk individuals from reoffending while their cases are being adjudicated. Table 5 presents estimates of the effect of being detained on recidivism. We find that being detained reduces the probability of being rearrested before disposition by 12.2 percentage points for felony defendants and 10.6 percentage points for misdemeanor defen-

Table 5
Effect of Detention on Recidivism

Variable	Felonies		Misdemeanors	
	OLS	IV	OLS	IV
Rearrested before detention	-.100** (.003)	-.188** (.020)	-.122** (.007)	-.062** (.018)
Rearrested within 2 years	.067** (.003)	.073** (.025)	.075** (.008)	.105** (.024)
F-statistic		1,036		1,901
Instruments		1		1
N	111,754	110,871	110,337	374,375
			375,791	374,006

Note. Values are estimates of the effect of pretrial detention on a binary variable equal to one if the defendant is rearrested. Ordinary least squares (OLS) specifications include all baseline controls. Instrumental variables (IV) estimates use judge-level variation in the propensity to detain pretrial as an instrument. Instrumental variables with nonmonotonicity (IVNM) estimates use instruments selected using a least absolute shrinkage and selection procedure. Standard errors clustered at the defendant and shift level are in parentheses.

** Significant at the 1% level.

dants. The reduction in pretrial rearrests highlights the meaningful incapacitation effect of keeping suspected potential criminals behind bars. Most of these gains are reversed, as individuals who were detained pretrial are more likely to be rearrested after their cases are resolved. Table 5 also provides estimates of the impact of being detained on the probability of being rearrested within 2 years after disposition. The effects are positive and large enough to offset most of the reductions during the pretrial period for felony defendants and all of the pretrial reductions for misdemeanor defendants. Pretrial detention increases the probability of being rearrested within 2 years by 7.5 percentage points for the felony subsample and by 11.8 percentage points for the misdemeanor subsample. The smaller effect for felony cases is consistent with higher levels and duration of incarceration, and therefore incapacitation, during the 2 years immediately following disposition. Dobbie, Goldin, and Yang (2016) also find pretrial and postdisposition rearrest effects that offset each other in their sample from Philadelphia and Miami-Dade County. Pretrial detention could plausibly increase recidivism rates indirectly, for example by damaging employability because of higher conviction rates and therefore worse criminal records. There could also be a direct effect on recidivism if pretrial detention results in the loss of jobs, housing, and so on. Whatever the mechanism, the net effect of detaining people pretrial on criminal activity is a small reduction at best.

6.5. Implications for Outcome Disparities by Race and Ethnicity

Minorities are overrepresented among the prison population in the United States. At the end of 2013, nearly 3 percent of black males and 1 percent of Hispanic males in the United States were imprisoned, compared with .5 percent of white males (Carson 2014). A variety of mechanisms could be behind the disparities in incarceration rates. Much of the gap is generated before crimes are adjudicated. Blacks and Hispanics are far more likely than whites to be charged with crimes in the first place, as evidenced by our data: at the time of the 2010 census, blacks and Hispanics made up about 23 percent and 29 percent, respectively, of the population of New York City but 48 percent and 34 percent of our sample. Even conditional on being charged with a crime, minority individuals are still more likely to be sentenced to prison, which means that their punishment includes at least a year at a prison facility created for long-term stays. In our sample, blacks charged with felonies are 3.7 percentage points more likely and Hispanics are 3.1 percentage points more likely to be sentenced to prison than their white counterparts after controlling for demographic and criminal history characteristics. These gaps are large relative to the fraction of people in the felony subsample who are sentenced to prison: 12.3 percent. We do not find evidence that pretrial detention has a differential effect on conviction by race or ethnicity for felony defendants (see Table A14). However, minority defendants are more likely to be detained pretrial. The gaps are shown in Figures A4 and A5. Among the felony subsample, blacks are 14 percentage points more likely to be detained

than whites, and Hispanics are 9 percentage points more likely to be detained than whites.

To determine how much of the disparity in sentencing outcomes can be explained by the gap in pretrial detention rates, we estimate the impact of detention on the likelihood of being sentenced to prison using the IVNM model. The coefficient of .101 on detention implies that the observed pretrial detention gap of 14 percentage points between blacks and whites would result in blacks being sentenced to prison at a rate 1.4 percentage points higher than whites. This explains 37.8 percent of the observed black-white gap in outcomes. Analogously, pretrial detention explains 29 percent of the gap in Hispanic-white sentencing outcomes. Pretrial detention explains even more of the gaps in the rates at which different racial and ethnic groups are sentenced to any incarceration, in either a prison or a jail, in part because of people who are sentenced to time served, which means that the time they spent detained pretrial fulfills their jail sentences. Pretrial detention increases the probability of being sentenced to any incarceration by 33.7 percentage points. For the felony subsample, there is a 7.9-percentage-point gap between blacks and whites in their rates of being sentenced to incarceration, 59.9 percent of which is explained by the difference in pretrial detention rates. The Hispanic-white gap is 5.5 percentage points, 52.7 percent of which is explained by pretrial detention. Gaps for the misdemeanor subsample are smaller: 4.9 points for blacks and 1.7 points for Hispanics relative to whites. Pretrial detention explains 55.1 percent of the black-white gap and 64.7 percent of the Hispanic-white gap for misdemeanor cases. Thus, while being detained does not appear to impact individuals differently by race or ethnicity, it is a contributing factor in the disparities observed among different groups because detention rates vary across groups.

6.6. Possible Mechanisms

In all specifications, the estimated effect of detention on pleading guilty is similar to the effect on conviction. This finding supports the hypothesis that detention influences case outcomes primarily by causing detainees to accept plea bargains more often. Detainees might be more likely to plead guilty for a variety of reasons.

First, some defendants may be offered plea deals that would allow them to go home sooner, potentially without serving any additional time. This is widely accepted among people who work in and write about pretrial detention as a central channel through which detention induces guilty pleas. To investigate whether defendants are pleading guilty to get home sooner, we estimate effects by predicted incarceration length if convicted. To do so, we first restrict the sample to cases that ended in conviction. Then we regress sentence length on the vector of observable characteristics described in Section 5. Using the coefficients from this regression, we predict sentence length conditional on conviction for the entire sample. Tables A16 and A17 present estimated effects of detention on conviction by predicted incarceration length. The coefficients on the group with an expected

sentence length of 0 days are no larger than for groups with positive predicted incarceration lengths.

Thus, although eagerness to get home sooner is a compelling story, it cannot fully explain our findings. Whether we analyze effects by predicted sentence length or by most serious offense charge (Tables A18 and A19), we find strong effects for individuals who would almost certainly face additional time in jail or prison as part of any plea deal. If an individual is not presented with a plea bargain that allows him to go home, why would being detained increase his incentive to accept it? One relevant feature of the criminal justice system is that detainees who are ultimately convicted and sentenced to serve time have the time they spent awaiting adjudication counted against their sentences. This policy lowers the cost of pleading guilty for detainees relative to released defendants because detainees have paid part of the price of conviction already.

Another possible motivation could be to get moved to a different facility. Pre-trial detainees are kept in jail with convicts serving relatively short sentences (generally no longer than 1 year). Prisons are reserved for convicts serving longer sentences and are designed with long-term residence in mind. Some jails have no yard, no employment for inmates, and more limited visiting opportunities. In New York City, the majority of detainees are kept in jails located on Rikers Island, the site of 10 jails with a combined capacity of up to 15,000.¹² Rikers has gained notoriety in recent years for abuse and neglect of prisoners. If defendants perceive their detention facility to be worse than wherever they might serve out their sentences, they might opt to plead guilty rather than stay where they are longer than necessary.

This option increases in its relative appeal only if detainees whose cases go to trial are disadvantaged once they get there. Collecting evidence or recruiting witnesses to support one's defense might be more challenging from behind bars. Relatively more detainees end up taking their cases to trial, but the fraction of cases that go to trial is tiny for both groups: only about 1 percent of cases in the entire sample were adjudicated at trial. By contrast, 34 percent of released defendants' cases were dismissed, compared with 19 percent of detainees' cases. These statistics suggest that being detained affects outcomes by causing people whose cases would ultimately have been dismissed to plead guilty rather than by causing people to plead guilty who would have been acquitted at trial if they had been released. It does not follow that expectations about trial outcomes are irrelevant to strategic decision making by defendants (or their attorneys). The criminal justice system moves slowly and unpredictably: for detainees whose cases go to trial, the median time between arraignment and sentencing is 513 days for felonies and 138 days for misdemeanors, with the middle 80 percent ranging from 226 to 971 days for felonies and 45 to 428 days for misdemeanors. Dismissals often take months: conditional on being dismissed, the median time to dismissal is 188 days for felony cases and 196 days for misdemeanor cases. By comparison, for de-

¹² Brooklyn, the Bronx, Queens, and Manhattan each have a smaller borough jail, but together these facilities can hold up to only 3,000 inmates.

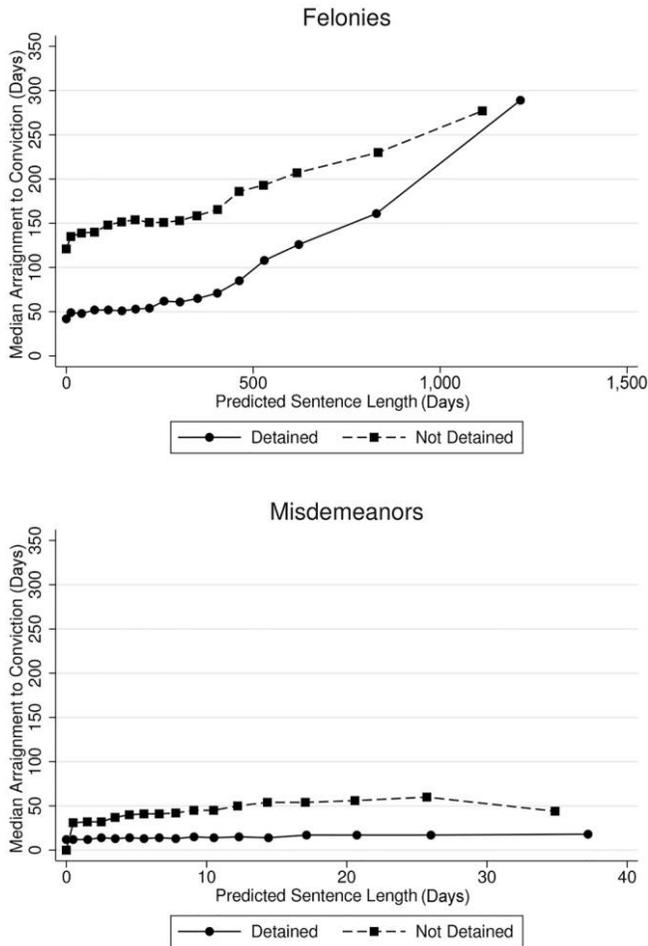


Figure 6. Guilty-plea times to conviction by detainment status

tainees who plead guilty, the median time between arraignment and sentencing is 80 days for felonies and 15 days for misdemeanors. Over time, the prospect of attaching an end date to the period of incarceration may become very attractive in contrast to the alternative of waiting an indeterminate amount of time behind bars for the process to play itself out, risking a trial at which the consequence of losing is a more severe punishment than any plea deal. Conversely, defendants who are released do not have the option of pleading time served, so if incarceration is required as a punishment for the crime of which they are accused, a guilty plea means going from being free to being incarcerated.

Figure 6 shows the relationship between predicted sentence length and the median amount of time between arraignment and conviction for defendants who

ended up pleading guilty. One thing that is immediately clear from these graphs is that detainees plead guilty much earlier than defendants who are released pretrial. Detainees accused of felonies wait more than twice as long as those charged with misdemeanors before pleading guilty, which is consistent with a more gradual process of becoming discouraged. For the misdemeanor subsample, the median time between arraignment and disposition is less than 3 weeks at every predicted sentence length. By contrast, the median gap between arraignment and disposition is almost 50 days for felony defendants in the lowest predicted sentence-length bin and grows larger for groups with longer predicted sentence lengths. We estimate the effect of detention on time to disposition across all cases in Table A20. The coefficients quantify the differences apparent in Figure 6. For felony cases, being detained leads to cases being resolved about 2 months (64 days) faster. Misdemeanor cases are resolved 48 days faster.

Pretrial detention might affect some defendants more adversely than others. For individuals with dependents, detention could also upend their children's lives, possibly putting them into the foster-care system. This scenario could create a powerful incentive for defendants to avoid detention. We find suggestive evidence supporting this possibility. Because felonies are, by definition, crimes that are punishable by at least a year in prison, we would not expect defendants charged with felonies to have the option of pleading guilty and returning home right away. However, many misdemeanor defendants would have access to a plea deal that requires little to no additional time in jail. Women are also more likely to be single parents than men: about three-quarters of all single-parent households are headed by women (Livingston 2013). If defendants with dependents are even more likely to plead guilty if doing so secures their immediate release, we would expect to see a larger disparity between coefficients for men and women with low expected incarceration lengths. In Table A22 we analyze effects for the misdemeanor sample by gender and predicted sentence length. For defendants with a predicted sentence length of 0 days, effects are noticeably stronger for women. The difference disappears for individuals with longer predicted sentences. Although not conclusive, these estimates are consistent with the hypothesis that low-income defendants with children are especially disadvantaged by the bail system.

7. Conclusion

Taken together, our results indicate a strong causal relationship between pretrial detention and case outcomes. Across specifications and subgroups, we see consistent evidence that detainees plead guilty more often to more serious offenses and some evidence that they serve longer sentences. The financially disadvantaged bear the brunt of these effects because the majority of detainees are held after failing to post bail at relatively low levels. The impact of pretrial detention on case outcomes explains a large portion of the systematically worse case

outcomes of minority defendants, who are more likely than whites to be detained pretrial.

Our paper provides several of the inputs to a careful cost-benefit analysis. The most obvious benefit of detaining suspected criminals is ensuring their appearance at trial and incapacitating them so that they do not commit additional crimes. The positive postsentencing impact of detention on recidivism mostly offsets the pretrial reductions within 2 years, so lower crime rates should not be tallied as a benefit of pretrial detention. We demonstrate several costs, including worse case outcomes for detainees, that are an important contributing factor to the unbalanced demographics of the prison population. Several costs are beyond the scope of this study to estimate, such as the impact on labor-market outcomes, household spillover effects, and the total cost of housing, feeding, and guarding detainees. A possible benefit of pretrial detention could be keeping caseloads lower than they would otherwise be because detainees resolve their cases more quickly.

There are several policy avenues for reforming pretrial services. Getting rid of money bail entirely would eliminate the disadvantage for individuals who are unable to post bail. This reform could, in theory, be implemented without changing the fraction of defendants who are detained pretrial, but policy makers should seriously consider whether the costs of the current detention rate outweigh the benefits. The main legal motivations for pretrial detention are to ensure appearance at court and public safety. The optimal detention rate is unlikely to be 0 but may be much lower than the status quo. Some parts of the country have already implemented bail-reform measures. For example, the District of Columbia has completely eliminated money bail in favor of an in-or-out court system and enhanced pretrial services. Only about 15 percent of accused persons are detained. Of those released, about 12 percent are rearrested pretrial, but fewer than 1 percent of these persons are alleged to have committed violent crimes. About 88 percent return to court (Keenan 2013). Avoiding costs associated with rearrests and failures to appear is an important benefit of pretrial detention, particularly for high-risk individuals, but our analysis suggests that policy makers will severely underestimate the costs of detention if they do not figure in the effect on case outcomes.

Although we explore the mechanism behind the effects, additional work is needed to more accurately pinpoint the relative importance of the different channels. Another open question is how the effects of pretrial detention on case outcomes vary as the marginal defendant changes. Our results are local effects based on current detention practices in New York City. If the criminal justice system were to relax or tighten its criteria for pretrial detention, estimated coefficients would pick up effects for a different set of marginal defendants. Understanding how effects vary across the distribution of defendants would be an important component in determining optimal detention practices.

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