Journal of Experimental Criminology
https://doi.org/10.1007/s11292-022-09542-w

Examining the causal effect of pretrial detention on case outcomes: a judge fixed effect instrumental variable approach

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Accepted: 28 October 2022

Abstract

Exploiting quasi-random assignment to NYC arraignment judges with varying propensities to detain, we use a judge fixed effect instrumental variable approach to estimate the impact of pretrial detention on several case outcomes: guilty plea, conviction, and carceral sentence. We find that any period of pretrial detention increases the likelihood of a guilty plea by 23 percentage points, a conviction by 24 percentage points, and a carceral sentence by 35 percentage points. Stratified analyses show differences in the size of the effect by charge severity and race: felony defendants experienced a larger effect on all case outcomes; non-Black defendants experienced a larger effect on guilty pleas and convictions; and Black defendants experienced a larger effect on carceral sentences.

Keywords Instrumental variable · Pretrial · Detention · Guilty plea · Conviction · Carceral sentence

Introduction

Nationwide, roughly 2 in 3 individuals in local jails are detained while awaiting trial (Zeng, 2018), with Black individuals making up a disproportionate share of the pretrial detention population (Arnold et al., 2018; Dobbie et al., 2018; Kutateladze et al., 2014; Schlesinger, 2005; Spohn, 2009). Past research shows that being held while awaiting trial is associated with worse case outcomes for defendants (Dobbie et al., 2018; Gupta et al., 2016; Leslie & Pope, 2017; Phillips, 2007; Stevenson, 2018). Exploiting quasi-random assignment to NYC arraignment judges with varying propensities to detain, we use a judge fixed effect instrumental variable design

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Published online: 23 December 2022

to estimate the impact of pretrial detention on multiple case outcomes: guilty plea, conviction, and carceral sentence. This study builds on prior research by estimating the impact of *any* period of pretrial detention and stratifying our estimates by charge severity and race.

Literature review

In a landmark 1963 study, researchers for the Vera Institute of Justice's Manhattan Bail Project found that pretrial detention was associated with worse case outcomes for defendants (Ares et al., 1963). Specifically, those detained pretrial were more likely to be convicted and receive a carceral sentence. Relying on more advanced statistical techniques, a more recent body of research supports these findings. After controlling for relevant factors such as criminal history, Heaton et al. (2017) found that defendants who experienced pretrial detention were about 25% more likely than similarly situated defendants to plead guilty and 43% more likely to receive a carceral sentence. Comparing samples matched on a range of relevant characteristics, Lee (2019) found that pretrial detention was associated with a 58% increase in the likelihood of conviction. Using near-far matching on a measure of judge propensity to detain, Lum et al. (2017) found that money bail at arraignment increased the likelihood of conviction by 34%. Instrumenting on quasi-random assignment to an arraignment judge, a popular method in economics often referred to as "judge fixed effects" (Frandsen et al., 2019), Dobbie et al. (2018) found that pretrial detention increased the likelihood of conviction by 16 percentage points; Leslie and Pope (2017) found that pretrial detention increased the likelihood of conviction by 14 percentage points for felony defendants; Stevenson (2018) found a 13 percentage-point increase in the likelihood of conviction primarily due to an increase in guilty pleas; and Gupta et al. (2016) found that money bail increased the likelihood of conviction by 12 percentage points.

Despite this growing body of research on the impact of pretrial detention on case outcomes, more research is needed on how these effects differ among defendant populations. Gupta et al.'s (2016) work indicates that the effects of pretrial detention are typically more severe for defendants charged with "minor" crimes like retail theft, suggesting that the impact of pretrial detention may differ by charge severity or crime type. Similarly, research suggests that the impact of detention may be more detrimental for defendants facing lesser charges for whom other costs of detention such as loss of employment or housing may be particularly harmful (Kohler-Hausmann, 2018). This claim is supported by Phillips' (2012) work, showing that pretrial detention had a greater impact on case outcomes in misdemeanor cases compared to felony cases.

Prior research suggests that pretrial detention disproportionately impacts Black defendants (Phillips, 2007), a finding that deserves more in-depth investigation given its potential role in exacerbating racial disparities throughout later stages of criminal justice processing (Donnelly & Macdonald, 2018; Martinez et al., 2019). The most recent national Bureau of Justice Statistics data on the demographics of pretrial detention from 2002 indicate that, among people awaiting trial in US jails,

43% were Black, nearly 20% were Hispanic, and 31% were white (James, 2004). One report found that Black arrestees in San Francisco were nine times more likely to be held in jail than their white peers (Human Rights Watch, 2017). Black individuals are less likely to make bail and thus more likely to be held in pretrial detention (Demuth, 2003; Schlesinger, 2005). Within NYC specifically, Kim et al. (2018) found that younger individuals, males, and African Americans were also *readmitted* to pretrial detention with greater frequency.

There are several remaining gaps in the literature. First, much of the prior research on the impact of pretrial detention fails to rule out confounding from unobserved differences between those detained pretrial and those released pretrial, either biasing estimates up or down. For example, judges may be more inclined to impose bail in cases where the evidence against a defendant is strong, biasing estimates upward. Or indigent defendants may be less likely to post bail but more susceptible to pressure exerted by pretrial detention, biasing estimates downward. Second, prior studies have operationalized pretrial detention inconsistently, ranging from a failure to post bail within three days (Dobbie et al., 2018; Stevenson, 2018), to a failure to post bail within seven days (Heaton et al., 2017), to a failure to post bail at any time before disposition (Leslie & Pope, 2017). One challenge this creates is that administrative datasets often lack precise information on pretrial release dates, making such studies difficult to replicate. Another is that such definitions exclude very short periods of pretrial detention, which prior work has shown can significantly affect case outcomes (Lowenkamp, 2022). Third, past research suggests that the impact of pretrial detention may differ by charge severity. Though some studies have looked at the effects of pretrial detention by severity type (Leslie & Pope, 2017; Phillips, 2012), research on pretrial detention's effect is typically lacking in this more fine-grained analysis. Finally, previous research suggests that pretrial detention may play an important role in worsening racial disparities in later stages of the criminal justice process, yet few studies have examined whether pretrial detention's effect differs by race.

Study context

In NYC, an adult's first appearance in Criminal Court following arrest is known as an arraignment. This study focuses on summary arrests (aka in-custody arrest), in which individuals are brought to central booking by the police and held until arraignment. Approximately 20% of summary arrests are disposed at arraignment, which means that the defendant accepted a plea deal, the case was dismissed, or the case was otherwise resolved (Koppel et al., 2021). The remaining 80% of summary cases are continued at arraignment. When a case is continued, an arraignment judge must decide whether a defendant should be released prior to their next court date. Specifically, a judge must decide whether a defendant should be: (1) released on recognizance (ROR); (2) released under supervision; (3) have bail set; or (4) be remanded. During the general time period analyzed in this study, about 28% of summary cases continued at arraignment had bail set, with a further 2% remanded (Ferri & Koppel, 2019).

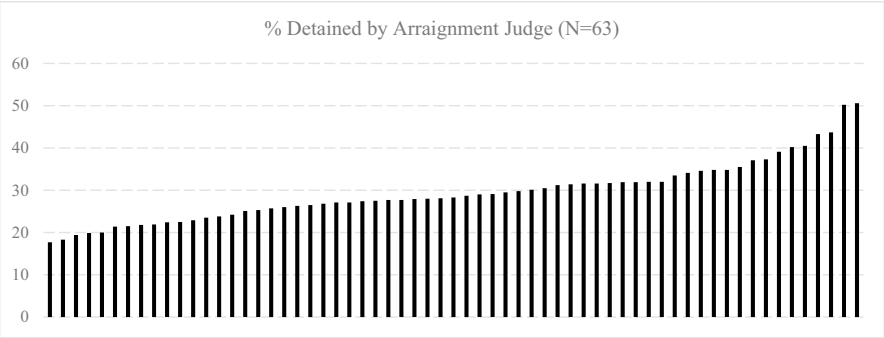

Fig. 1 Percentage of defendants detained by arraignment judge. Note: Data are restricted to arraignment judges who made at least 500 arraignment decisions during the study period.

Data

Dataset inclusion criteria

Our dataset includes only summary arrests that were arraigned in 2016 and were continued at arraignment. Individuals are only represented in our dataset once. If a person had multiple cases with the same release outcome, we selected the earliest case. However, if an individual had multiple cases with different release outcomes (e.g., one released at arraignment and one detained at arraignment), we selected the earliest case in which the person was detained. This was to (1) ensure we included as many people with the exposure of interest as possible, and (2) to avoid a form of contamination whereby a person was classified as 'non-treated' (released at arraignment) but received the treatment (detention) in a parallel case. To further ensure that individuals are not represented twice in the dataset, we exclude cases charged with non-finger-printable offenses for which a unique New York State ID Number (NYSID) was *not* assigned.

The dataset only includes people who were 18 or older at the time of arrest. Cases charged with violations are also excluded because only 5% were detained for the entire time between arraignment and disposition. Additionally, only cases disposed as of 2019 with non-missing release-status data are included (as cases without release-status data cannot be used to analyze the effect of pretrial detention on case outcomes). These restrictions yielded roughly 98,000 individuals. Consistent with Leslie and Pope (2017), we then further restricted the data to cases where the judge made at least 500 arraignment decisions for the year to ensure a sufficiently robust measure of propensity to detain. Sixty-three arraignment judges met this criterion, with rates of detention imposed ranging from a low of 17.7% to a high of 50.6% (Fig. 1). Overall, these restrictions yielded a sample with about 82,000 unique defendants. The study protocol was approved by the Institutional Review Board at the Center for Court Innovation.

Dependent variables

Our models explore three different dependent variables:

- *Guilty plea.* Guilty plea based on the final disposition in the case.
- *Conviction*. Convicted based on the final disposition in the case.
- *Carceral sentence*. Sentenced to prison or jail based on the most severe sentence imposed in Criminal or Supreme Court.[1] (A jail or prison sentence is the most severe sentence category in New York since there is no death penalty.) Non-carceral sentences such as credit for time served are excluded.

Control variables

- *Race.* Black, non-Black.
- *Sex.* Male, female.
- *Age.* Age at arrest.
- *Prior misdemeanor convictions.* Number of prior misdemeanor convictions.
- *Prior felony convictions.* Number of prior felony convictions.
- *Most serious arraignment charge category.* Harm to person only, harm to property only, harm to person and property, weapon, drug, sex, theft intangible, vehicle and traffic, misconduct, obstructing justice, or local law.

Defining pretrial detention

We operationalized pretrial detention based on whether a person was detained at arraignment for either failing to post money bail or was remanded. With this measure, we compare individuals who experienced any period of pretrial detention with those released at arraignment.[2]

Methods

A key challenge in estimating the causal effect of pretrial detention on case outcomes is overcoming the problem of endogeneity (Bushway & Apel, 2010): the possibility that any differences seen in case outcomes might be due not to pretrial detention but

[1] In combining jail and prison sentences into a single dependent variable, we are mindful that these two sentence types may be qualitatively different, with defendant and case characteristics predicting assignment to each (Holleran & Spohn, 2004). To address this concern, we follow the approach of Kutateladze et al. (2014) and stratify charges by severity. Such stratification allows us to separate carceral sentences into jail (i.e., misdemeanors) and prison (i.e., felony) sentences (Holleran and Spohn, 2004; Wang and Mears, 2010; Wang et al., 2013).

[2] Our dataset is not able to reliably capture situations in which an individual was released at arraignment and then detained at a later point (e.g., they were re-arrested and had bail set in that case and were unable to pay). Thus, a small number of defendants in our "released at arraignment" comparison group may in fact have experienced some amount of detention, potentially biasing our estimates downward.

pre-existing differences between those detained and not detained. And while classical regression techniques can be useful in adjusting for observed differences, it is unlikely that all potential sources of variation are observed in the available data. To address this possible source of bias from unobserved differences, we use a two-stage least-squares instrumental variable (IV) approach. As Angrist (2006) points out in his influential introduction of instrumental variables to the field of criminology, this strategy represents a promising quasi-experimental research design which can provide a "credible substitute" for random assignment.

Instrumental variable analysis requires a plausible 'instrument' to serve as a source of exogenous variation in the independent (endogenous) variable of interest (Bushway & Apel, 2010); in our case, a source of exogenous variation in pretrial detention. Like a large number of previous studies (Dobbie et al., 2018; Gupta et al., 2016; Leslie & Pope, 2017; Stevenson, 2018), we employ what is commonly referred to as a judge fixed effect design (Frandsen et al., 2019), which involves instrumenting on assignment to arraignment judge with varying propensities to detain. In NYC, arraignment coordinators are responsible for assigning cases to an arraignment judge with the goal of balancing workloads across court parts. This creates an opportunity to solve the endogeneity problem by only entering into our model the arguably exogenous variation in judge propensity to detain (for empirical evidence that assignment to arraignment judge in NYC is conditionally random, see Leslie & Pope, 2017). Formally, this means that in our first-stage model we regress X (pretrial detention) on instrument Z (propensity to detain), and then in our second-stage model we regress our outcome of interest on the variation in X that can be predicted by instrument Z (propensity to detain), as such:

$$X_{\mathrm{i}} = \delta + \gamma Z_{\mathrm{i}} + \nu_{\mathrm{i}}$$

$$Y_{\mathrm{i}} = \alpha + \beta X_{\mathrm{i}} + \varepsilon_{\mathrm{i}}$$

Our analysis is presented as follows. First, we estimate a naïve linear probability by regressing our case outcomes of interest on pretrial detention. Second, we add to the model our full set of control variables described above along with arraignment-shift fixed effects.[3] Third, we use a judge fixed effect IV approach to estimate the impact of pretrial detention. By identifying the presiding judge at every arraignment in our sample, we were able to construct an instrument based on an arraignment judge's propensity to detain (Dobbie et al., 2018; Gupta et al., 2016; Leslie & Pope, 2017; Stevenson, 2018). To adjust for possible confounders, the instrument controls for defendant characteristics, arraignment shift, and crime-type category (Dobbie et al., 2018). A jackknife leave-one-out (JIVE) estimation approach was used with the following steps: calculate the residuals of detention status regressed on the full set of control variables and arraignment-shift fixed effects; use those residuals to calculate a leave-out mean for each judge within each crime-type category;

[3] These are dummy variables grouping together defendants arraigned in the same borough, month, day of the week, and shift (day vs night).

and, finally, use the judge leave-out mean to instrument for detention (Angrist et al., 1999). The resulting instrument was used to estimate the local average treatment effect (LATE) on marginal defendants for whom arraignment-judge assignment might have impacted the outcome of a case (Aronow & Carnegie, 2013). To evaluate instrument strength, we use estimates of model fit from the first-stage regression. Applying Staiger and Stock's (1994) "rule of thumb," a first-stage regression *F*-statistic below ten is treated as indication of a weak instrument. As recommended, we use standard linear models to estimate the IV models even where our dependent variable is binary (Angrist & Pischke, 2008; Angrist, 2006).[4] Overall, our judge fixed effect IV strategy most closely resembles those of Leslie and Pope (2017) and Dobbie et al. (2018). Finally, we ran stratified analyses to examine whether the impact of pretrial detention differed by charge severity or race. All analyses were conducted with *Stata 15*.

Results

Table 1 below shows descriptive statistics for our data. Substantial differences can be seen across individuals who were either released at arraignment or experienced any detention. For example, males were 77% of those released at arraignment compared to 91% of those who experienced any period of pretrial detention. Black defendants made up 45% of those who experienced no detention compared to 53% of those who experienced any detention. The mean number of prior misdemeanor convictions for individuals who experienced no detention was 1.2 compared to 5.6 for individuals who experienced any detention.

Table 2 shows comparative models of the impact of pretrial detention on three case outcomes: (1) likelihood of guilty plea, (2) likelihood of conviction, and (3) likelihood of a carceral sentence. Model A is a naïve linear probability model where each case outcome is regressed on pretrial detention alone. Any period of pretrial detention increased the likelihood of a *guilty plea* by 24 percentage points ($b = 0.24$, s.e. = 0.00, $p < 0.001$), a *conviction* by 25 percentage points ($b = 0.25$, s.e. = 0.00, $p < 0.001$), and a *carceral sentence* by 38 percentage points ($b = 0.38$, s.e. = 0.00, $p < 0.001$). However, the concern with these estimates is that they fail to account for factors associated with *both* pretrial detention and case outcomes. One simple way to address this is to control for observed differences. Model B is the same model with our full set of control variables and arraignment-shift fixed effects. For each outcome, the estimated impact of pretrial detention dropped substantially. After adjusting for possible observed confounders, including demographic characteristics,

[4] While less common in some disciplines, our use of linear models with a binary dependent variable is standard practice in economics, and in particular was used in the economics papers that most inform our research design to generate baseline estimates (see Heaton, Mayson, & Stevenson, 2017; Leslie & Pope, 2017; Dobbie et al., 2018). One important reason for its use is the fact that logit and probit models are forbidden in the first stage of IV estimation with 2SLS (the so-called forbidden regression) (Wooldridge, 2002). So regardless of whether a dependent variable is binary or continuous, IV estimation with 2SLS is simply an extension of a 1-stage OLS model.

Table 1 Descriptive statistics

Variable	Released Mean/proportion	Any detention Mean/proportion
Demographics		
Race		
Black	.45	.53
Non-Black	.55	.47
Sex		
Male	.77	.91
Female	.23	.09
Age (mean)	33.8	34.8
Criminal history		
Prior misdemeanor convictions	1.2	5.6
Prior felony convictions	.3	1.2
Most serious arraignment charge category		
Harm to person only	.38	.28
Harm to property only	.13	.13
Harm to person and property	.02	.11
Weapon	.03	.07
Drug	.11	.19
Sex	.02	.01
Theft intangible	.07	.05
Misconduct	.05	.04
Obstructing justice	.04	.07
Local law	.02	.01
Case outcome		
Pled guilty	.49	.74
Convicted	.50	.75
Carceral sentence	.02	.40
N	57,090	23,093

criminal history, and arraignment-shift fixed effects, pretrial detention increased the likelihood of a *guilty plea* by 18 percentage points ($b = 0.18$, s.e. $= 0.00$, $p < 0.001$), a *conviction* by 19 percentage points ($b = 0.19$, s.e. $= 0.00$, $p < 0.001$), and a *carceral sentence* by 30 percentage points ($b = 0.30$, s.e. $= 0.04$, $p < 0.01$).

Still, these estimates may be biased due to unobserved differences between those who experienced detention and those released at arraignment. To address the possibility of residual confounding from unmeasured differences, we use a judge fixed effect IV approach that involves instrumenting on variation in judge propensity to detain. Model C is the first-stage IV regression where pretrial detention is regressed on our excluded instrument (propensity to detain) along with the full set of control variables and arraignment-shift fixed effects. The model fits

Table 2 Comparative models of the effect of pretrial detention on case outcomes ($N = 80{,}183$)

	Model A: OLS	Model B: OLS with controls/FE	Model C: IV:1st stage	Model D: IV 2SLS
Pled guilty				
Dependent variable	Pled guilty	Pled guilty	Any detention	Pled guilty
Pretrial detention	.24(.00)***	.18(.00)***		.23(.04)***
Propensity to detain			.59(.02)***	
Instrument diagnostics				
F-test			771.5	
R-square	.05	.23	.27	
Convicted				
Dependent variable	Convicted	Convicted	Any detention	Convicted
Pretrial detention	.25(.00)***	.19(.00)***		.24(.04)***
Propensity to detain			.59(.02)***	
Instrument diagnostics				
F-test			771.5	
R-square	.05	.24	.27	
Carceral sentence				
Dependent variable	Carceral sentence	Carceral sentence	Any detention	Carceral sentence
Pretrial detention	.38(.00)***	.30(.04)**		.35(.03)***
Propensity to detain			.59(.02)***	
Instrument diagnostics				
F-test			771.5	
R-square	.26	.31	.27	

*$p < .05$, **$p < .01$; ***$p < .001$

well with an *F*-statistic of 771.5 and an *R*-square of 0.27, and the excluded instrument's coefficient is positive and statistically significant ($b = 0.59$, s.e. $= 0.02$, $p < 0.001$). This suggests that judge propensity to detain has a strong impact on detention at arraignment. Model D is the second-stage of the IV analysis where our outcome is regressed on the variation in detention predicted by our instrument. Findings from these main IV specifications show that any period of pretrial detention significantly increased the probability of a *guilty plea* by 23 percentage points ($b = 0.23$, s.e. $= 0.04$, $p < 0.001$), a *conviction* by 24 percentage points ($b = 0.24$, s.e. $= 0.04$, $p < 0.001$), and a *carceral sentence* by 35 percentage points ($b = 0.35$, s.e. $= 0.04$, $p < 0.001$). Note that the IV estimates for all three outcomes (Model D) are somewhat great than the OLS with controls estimates (Model B),

Table 3 Comparative models of the effect of pretrial detention on guilty pleas by subgroup

	Model A: OLS	Model B: OLS with controls/FE	Model C: IV:1st stage	Model D: IV 2SLS
Felony ($N=27{,}509$)				
Dependent variable	Pled guilty	Pled guilty	Any detention	Pled guilty
Pretrial detention	.14(.01)***	.16(.01)***		.21(.06)***
Propensity to detain			.63(.03)***	
Instrument diagnostics				
F-test			326.1	
R-square	.02	.16	.20	
Misdemeanor ($N=52{,}674$)				
Dependent variable	Pled guilty	Pled guilty	Any detention	Pled guilty
Pretrial detention	.24(.01)***	.21(.01)***		.18(.07)*
Propensity to detain			.46(.02)***	
Instrument diagnostics				
F-test			328.6	
R-square	.03	.25	.16	
Black ($N=38{,}186$)				
Dependent variable	Pled guilty	Pled guilty	Any detention	Pled guilty
Pretrial detention	.24(.01)***	.19(.04)***		.10(.05)
Propensity to detain			.63(.03)***	
Instrument diagnostics				
F-test			441.8	
R-square	.05	.23	.28	
Non-Black ($N=41{,}997$)				
Dependent variable	Pled guilty	Pled guilty	Any detention	Pled guilty
Pretrial detention	.25(.01)***	.17(.01)***		.33(.06)***
Propensity to detain			.53(.03)***	
Instrument diagnostics				
F-test			323.8	
R-square	.05	.23	.26	

*$p<.05$, **$p<.01$; ***$p<.001$

suggesting that alternative estimation strategies underestimate the true magnitude of pretrial detention's effect.

Stratified analyses

Tables 3, 4, and 5 show findings from the analysis described above stratified by arraignment charge severity (felony vs misdemeanor) and race (Black vs non-Black). With regard to severity, the main results show that pretrial detention had a greater impact in felony cases compared to misdemeanor cases for all three outcomes: on a *guilty plea*, it was 21 percentage points in felony cases ($b=0.21$, s.e.$=0.06$, $p<0.001$) vs 18 percentage points in misdemeanor cases ($b=0.18$, s.e.$=0.07$,

Table 4 Comparative models of the effect of pretrial detention on convictions by subgroup

	Model A: OLS	Model B: OLS with controls/ FE	Model C: IV:1st stage	Model D: IV 2SLS
Felony ($N=27{,}509$)				
Dependent variable	Convicted	Convicted	Any detention	Convicted
Pretrial detention	.16(.01)	.16(.01)***		.22(.06)***
Propensity to detain			.63(.03)***	
Instrument diagnostics				
F-test			326.1	
R-square	.03	.17	.20	
Misdemeanor ($N=52{,}674$)				
Dependent variable	Convicted	Convicted	Any detention	Convicted
Pretrial detention	.24(.01)***	.22(.01)***		.18(.07)*
Propensity to detain			.46(.02)***	
Instrument diagnostics				
F-test			328.6	
R-square	.03	.25	.16	
Black ($N=38{,}186$)				
Dependent variable	Convicted	Convicted	Any detention	Convicted
Pretrial detention	.25(.01)***	.20(.01)***		.12(.05)*
Propensity to detain			.63(.03)***	
Instrument diagnostics				
F-test for instrument			441.8	
R-square for instrument	.06	.24	.28	
Non-Black ($N=41{,}997$)				
Dependent variable	Convicted	Convicted	Any detention	Convicted
Pretrial detention	.26(.01)***	.18(.01)***		.33(.06)***
Propensity to detain			.53(.03)***	
Instrument diagnostics				
F-test for instrument			323.8	
R-square	.05	.23	.26	

*$p<.05$, **$p<.01$; ***$p<.001$

$p<0.05$); on a *conviction*, it was 22 percentage points in felony cases ($b=0.22$, s.e.$=0.06$, $p<0.001$) vs 18 percentage points in misdemeanor cases ($b=0.18$, s.e.$=0.07$, $p<0.05$); and on a *carceral sentence*, it was 33 percentage points in felony cases ($b=0.33$, s.e.$=0.05$, $p<0.001$) vs 21 percentage points in misdemeanor cases ($b=0.21$, s.e.$=0.03$, $p<0.001$). Compared to the other estimation strategies, in felony cases, the IV estimates were higher for a guilty plea and conviction but lower for a carceral sentence; in misdemeanor cases, the IV estimates were lower for all three outcomes. With regard to race, the main findings were mixed. Pretrial

Table 5 Comparative models of the effect of pretrial detention on carceral sentences by subgroup

	Model A: OLS	Model B: OLS with controls/FE	Model C: IV:1st stage	Model D: IV 2SLS
Felony ($N=27{,}509$)				
Dependent variable	Carceral sentence	Carceral sentence	Any detention	Carceral sentence
Pretrial detention	.41(.00)***	.35(.01)***		.33(.05)***
Propensity to detain			.63(.03)***	
Instrument diagnostics				
F-test			326.1	
R-square	.18	.27	.20	
Misdemeanor ($N=52{,}674$)				
Dependent variable	Carceral sentence	Carceral sentence	Any detention	Carceral sentence
Pretrial detention	.29(.00)***	.24(.01)***		.21(.03)***
Propensity to detain			.46(.02)***	
Instrument diagnostics				
F-test			328.6	
R-square	.15	.23	.16	
Black ($N=38{,}186$)				
Dependent variable	Carceral sentence	Carceral sentence	Any detention	Carceral sentence
Pretrial detention	.39(.00)***	.30(.01)***		.36(.04)***
Propensity to detain			.63(.03)***	
Instrument diagnostics				
F-test			441.8	
R-square	.21	.29	.28	
Non-Black ($N=41{,}997$)				
Dependent variable	Carceral sentence	Carceral sentence	Any detention	Carceral sentence
Pretrial detention	.37(.00)***	.29(.01)***		.35(.04)***
Propensity to detain			.53(.03)***	
Instrument diagnostics				
F-test			323.8	
R-square	.20	.27	.26	

*$p<.05$, **$p<.01$; ***$p<.001$

detention had a greater impact for non-Blacks compared to Blacks for a guilty plea and conviction: on a *guilty plea*, it was 33 percentage points for non-Blacks ($b=0.33$, s.e. $=0.06$, $p<0.001$) vs 10 percentage points in Blacks ($b=0.10$, s.e. $=0.05$, n.s.); on a *conviction*, it was 33 percentage points for non-Blacks ($b=0.33$, s.e. $=0.06$,

$p<0.001$) vs 12 percentage points in Blacks ($b=0.12$, s.e.$=0.05$, $p<0.05$). On the other hand, the impact of pretrial detention on the likelihood of a carceral sentence was slightly higher for Blacks vs non-Blacks: 36 percentage points for Blacks ($b=0.36$, s.e.$=0.04$, $p<0.001$) vs 35 percentage points for non-Blacks ($b=0.35$, s.e.$=0.04$, $p<0.001$). Compared to the other estimation strategies, in cases with a Black defendant, the IV estimates were lower for a guilty plea and conviction, but higher for a carceral sentence; for non-Blacks, the IV estimates were higher for all three outcomes.

Discussion

Prior research has shown that pretrial detention is associated with worse case outcomes for defendants, such as a guilty plea, conviction, and carceral sentence (Heaton et al., 2017; Lee, 2019; Lum et al., 2017; Phillips, 2007; Sacks & Ackerman, 2014). However, much of this earlier research has relied on standard regression techniques that fail to account for possible confounding from unobserved differences between those detained and not detained. Using a popular method in economics, often referred to as judge fixed effects, a more recent series of studies attempts to tackle this problem by instrumenting on arraignment judge assignment (Dobbie et al., 2018; Gupta et al., 2016; Leslie & Pope, 2017; Stevenson, 2018). By exploiting effective randomization to judges with different propensities to detain, these researchers have been able to demonstrate a causal link between pretrial detention and worse case outcomes for defendants. However, prior judge fixed effect studies have several important limitations, including definitions of pretrial detention that exclude short periods of detention, and a lack of analysis stratified by factors such as case severity and defendant race.

To address these limitations, we use a judge fixed effect IV design to estimate the effect of any pretrial detention on multiple case outcomes. The analysis was then stratified by arraignment charge severity and race. Consistent with prior work, in our main IV specification, we find that any period of pretrial detention significantly increases the probability of a *guilty plea* by 23 percentage points, a *conviction* by 24 percentage points, and a *carceral sentence* by 35 percentage points. By arraignment charge severity, the analyses show that pretrial detention had a greater impact in felony cases for all three outcomes: on a *guilty plea*, it was 21 percentage points in felony cases compared to 18 percentage points in misdemeanor cases; on a *conviction*, it was 22 percentage points in felony cases compared to 18 percentage points in misdemeanor cases; and on a *carceral sentence*, it was 33 percentage points in felony cases compared to 21 percentage points in misdemeanor cases. By race, the analyses show that pretrial detention had a greater impact for non-Blacks compared to Blacks for a guilty plea and conviction: on a *guilty plea*, it was 33 percentage points for non-Blacks compared to 10 percentage points in Blacks; on a *conviction*, it was 33 percentage points for non-Blacks compared to 12 percentage points in Blacks. On the other hand, the impact of pretrial detention on the likelihood of a carceral sentence was slightly higher for Blacks (36 percentage points) compared to non-Blacks (35 percentage points).

Overall, our findings contribute to the literature on the effect of pretrial detention in several ways. First, this study adds to a growing number of judge fixed effect studies showing a relationship between pretrial detention and worse case outcomes for defendants (Dobbie et al., 2018; Gupta et al., 2016; Leslie & Pope, 2017; Stevenson, 2018). Following this empirical strategy, we largely replicate the findings from previous judge fixed effect studies. Given the advantages of this approach, particularly the ability to account for observed and unobserved differences, our findings provide further evidence for a *causal* connection between pretrial detention and worse case outcomes for defendants.

Second, previous judge fixed effects studies have limited the definition of pretrial detention based on detention length, ranging from at least several days all the way up to the final disposition in a case. This means that a significant share of defendants in the comparison group of these studies may have experienced some period of pretrial detention, potentially biasing the estimate of pretrial detention's effect downward. By contrast, we define pretrial detention broadly to include any pretrial detention, as past research has shown that even a short stay in detention can impact case outcomes (Lowenkamp, 2022). Notably, the use of this more expansive definition may account for several of our somewhat higher estimates of pretrial detention's effect; for example, for felony defendants, we find that pretrial detention increases the likelihood of pleading guilty by 21 percentage points and a conviction by 22 percentage points, while Leslie and Pope's (2017) found an increase of 11 percentage points and 14 percentage points, respectively.

Third, in our data, we found that about 98% of convictions were secured through a guilty plea. It was unsurprising, then, that we also found that pretrial detention had nearly the same effect on the likelihood of a guilty plea (23 percentage point increase) as on the likelihood of a conviction (24 percentage point increase). The similarity of these findings lends support to the idea that pleading guilty—which can serve as a way for defendants to get released sooner or transferred from a pretrial detention facility to a jail or prison with more favorable conditions—is one of the main mechanisms of pretrial detention's effect on case outcomes (Dobbie et al., 2018; Heaton et al., 2017, Peterson, 2020).

Fourth, like Heaton et al. (2017), we find that the estimated effect of pretrial detention was greater in IV models than in OLS models, suggesting that the true size of the effect was masked by unmeasured confounding (Angrist, 2006). For example, an unmeasured variable such as indigency may have made it less likely for some individuals to post bail, while at the same time making them more susceptible to pressure exerted by pretrial detention to accept a worse outcome. If so, the absence of such a confounder would have resulted in an underestimate of pretrial detention's effect in OLS but not in IV.

Fifth, we present findings from our analysis stratified by arraignment charge severity. This allowed us to separate charges eligible for jail sentences (misdemeanors) from charges eligible for prison sentences (felonies), which past research has shown can impact case outcomes (Holleran & Spohn, 2004; Kutateladze et al., 2014). In general, we find that the effect of pretrial detention tended to be slightly higher for individuals charged with a felony compared to a misdemeanor. These findings are important as much of the scholarship in this area has focused on the

impact of pretrial detention in lower-level misdemeanor cases (Baughman, 2018; Heaton et al., 2017; Human Rights Watch, 2010).

Finally, we present findings from our analysis stratified by race. The results were mixed. For carceral sentences, we found that the effect of pretrial detention was roughly similar across racial groups, with a slightly higher effect among Black defendants. For guilty pleas and convictions, we found that the effect of pretrial detention was substantially greater among non-Black defendants. Put another way, among non-Black defendants being released into the community at arraignment had a greater impact on a person's chances of avoiding conviction. More research is needed to better understand the factors that might explain this differential effect. It is also worth noting that our main findings show that pretrial detention increases the likelihood of worse case outcomes among *all* defendants, and that this effect is more likely to be experienced by Black defendants as they are disproportionately detained (Arnold et al., 2018; Dobbie et al., 2018; Kutateladze et al., 2014; Schlesinger, 2005; Spohn, 2009).

Limitations

Several limitations of this paper are worth noting. First, the validity of our judge fixed effect design relies on the assumption that assignment to arraignment judge influences whether a defendant is subject to pretrial detention, but not the ultimate outcomes in a case (for a discussion of IV assumptions, see Rassen et al., 2009). However, this so-called exclusion restriction is likely satisfied here as arraignment judges in NYC are responsible for deciding whether to detain a defendant, and afterward that person's case is handed off to a different judge (Dobbie et al., 2018). Second, with our data, we were able to capture whether a person was detained pretrial, but not the *precise* length of detention. Comparing the effect of varying lengths of detention may reveal important differences in the effect of pretrial detention on case outcomes. Third, while we were able stratify our analysis by race (Black vs non-Black), the size of our sample did not support a more fine-grained analysis that accounts for other racial and ethnic categories. Fourth, though we were able to estimate the impact of pretrial detention on whether a defendant received a carceral sentence, among the smaller subset of cases with a carceral sentence, our instrument was not strong enough to estimate sentence length. Finally, though we examine the effect of pretrial detention on several important case-related outcomes, other possible ramifications of pretrial detention were not explored—like the effect of pretrial detention on housing, employment, and provision of childcare. Future research is needed on these other possible collateral consequences of pretrial detention.

Conclusion

We used a judge fixed effect instrumental variable design to estimate the effect of any period of pretrial detention on multiple case outcomes. Consistent with prior research, our results show that pretrial detention is causally associated with an

increased likelihood of a guilty plea, conviction, and a carceral sentence. We find effect sizes that are somewhat larger than in previous studies, possibly because unlike these studies, our comparison group excludes individuals who experience short periods of detention. Stratified analyses show differences in the size of the effect by charge severity and race, with felony defendants experiencing a larger effect on all case outcomes; non-Black defendants experiencing a larger effect on guilty pleas and convictions; and Black defendants experiencing a larger effect on carceral sentences. In future research, the judge fixed effect IV design will be useful for estimating other causal effects of pretrial detention.

Acknowledgements We thank Nathan Kallus, Rick Peterson, Freda Solomon, Diana Spahia, Dalia Sharps, David Topel, and Anisa Stechert, as well as Brenda Velazquez and other members of the NYC Mayor's Office of Criminal Justice (MOCJ) research staff, for comments and helpful suggestions. We also thank Aubrey Fox, Evan Misshula, and Wayne Nehwadowich for their contributions to this project. Any errors or omissions are the responsibility of the authors.

Funding This project was supported by Arnold Ventures. The content is the responsibility of the authors and does not necessarily represent the views of Arnold Ventures or the New York City Criminal Justice Agency.

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