PONTIFÍCIA UNIVERSIDADE CATÓLICA
DO RIO DE JANEIRO

Beatriz Machado Ribeiro

Pretrial detention and rearrest: evidence from Brazil

Dissertação de Mestrado

Thesis presented to the Programa de Pós–graduação em Economia da PUC-Rio in partial fulfillment of the requirements for the degree of Mestre em Economia.

Advisor: Prof. Claudio Ferraz

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Abstract

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In most legal systems, detaining individuals pretrial is a common practice. Pretrial detention prevents that defendants commit crimes while they wait for their trials, but prison experiences can also encourage future criminal activity. In this paper, we use novel data on detention hearings and *in flagrante delicto* arrests in the state of Rio de Janeiro to assess the effect of pretrial detention on future crime. Since detention assignment is endogenous to defendants' characteristics, we adopt an instrumental variable approach that exploits randomly assigned judges who differ in terms of their idiosyncratic tendencies of ordering pretrial detention. Our findings suggest that pretrial incarceration reduces rearrest in the medium term, and that this effect is entirely driven by incapacitation effects. We also provide evidence that pretrial detention increases the probability and the severity of post-release crime.

Keywords

Resumo

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Na maioria dos sistemas legais, manter réus detidos antes de seus julgamentos é uma prática comum. A prisão provisória evita que os réus cometam crimes enquanto aguardam por seus julgamentos, mas, por outro lado, experiências na prisão podem promover a participação futura em atividades ilícitas. Neste estudo, utilizamos novos dados de audiências de custódia e prisões em flagrante no estado do Rio de Janeiro para avaliar o efeito da prisão provisória em reaprisionamento. Dado que a aplicação da prisão provisória é endógena às características de cada indivíduo, adotamos uma abordagem de variável instrumental que explora a variação das tendências a encarcerar de juízes que são aleatoriamente selecionados para conduzir audiências de custódia. Nossos resultados sugerem que a prisão provisória reduz reaprisionamento no médio prazo, e que esse efeito é completamente explicado por um efeito de incapacitação de curto prazo. Nós também apresentamos evidências de que a prisão provisória aumenta a probabilidade e a severidade de crimes após a saída da prisão.

Palavras-chave

Prisão provisória   Crime   Reaprisionamento   Incapacitação

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1
Introduction

Throughout the world, more than two and a half million people are held in prison awaiting trial (Walmsey, 2017). One of the primary aims of detaining individuals pretrial is to prevent recidivism while case adjudication is ongoing. Nevertheless, prison experiences could also foster post-release crime if, for instance, inmates are negatively influenced by their peers or if prison stigma harms post-release employability. Thus, criminogenic effects of prison raise concerns about the effectiveness of pretrial detention. Furthermore, widespread arbitrary use of pretrial detention underscores the importance of assessing the unintended effects of prison on criminal behavior[1]. Yet, little is know about the causal effects of detention on future crime.

Latin America holds the world's highest rates of pretrial detainees in the population. In Brazil, 40% of the prison population, or nearly 300,000 individuals, are awaiting trial (Infopen, 2017). Over half of these people have been detained for more than six months, and many are placed in overcrowded facilities ruled by powerful prison gangs. This situation has resulted in a number of Brazilian courts adopting an alternative detention hearing system, designed to promote the use of non-custodial measures for untried defendants. This reform has sparked strong criticism, with its detractors arguing that releasing individuals shortly after they have been caught increases incentives for criminality and thus rearrest and crimes rates[2]. In contrast, supporters of the reform claim that the arbitrary and excessive use of pretrial detention disrupts jobs and studies, denies individuals the right to freedom and forces interaction with organized crime.

In this paper, we use data from Brazil to examine the impact of pretrial detention on the probability of rearrest. Estimating this effect is challenging because one of the criteria for ordering pretrial detention is an offender's risk of rearrest, meaning that detained and non-detained populations differ

[1]For example, in the United States, there is an ongoing debate about the use of monetary bail for pretrial release and the implications of this for low-risk defendants without the means to post bail. For insights into this discussion, see Liu et al. (2018). In Brazil, the high number of pretrial detainees released after trial has also raised concerns about the adequacy of the criteria for ordering pretrial detention and the effects of that on defendants.

[2]The idea that detention hearings encourage crime activity led a federal deputy representing the state of São Paulo to draft a bill that prohibited detention hearings in 2016.

in a number of characteristics that are likely to correlate to their propensity of engaging in future crime. We deal with this identification problem by following the estimation strategy first proposed by Kling (2006) and use judges' propensity to incarcerate as an instrument for pretrial detention. Two features of our setting allow us to plausibly interpret our estimates as causal effects. First, judges responsible for ordering pretrial detention differ considerably in their decisions even when faced with similar individuals, which is consistent with our measure of magistrate severity being predictive of prison assignment. Second, defendants are randomly assigned to detention hearing judges, ensuring that judge profile is not correlated with any case characteristic that might affect future crime[3].

Our analysis is based on a case-level dataset of detention hearings held in Rio de Janeiro city. Our dataset combines novel data on defendants and hearings obtained from Rio de Janeiro Public Defenders' Office, detailed police administrative records obtained from Rio de Janeiro's Institute of Public Safety and data on judicial processes publicly available online. This data together yielded a dataset of 5,728 cases initiated between September 2015 and September 2017, with rearrest indicators that cover *in flagrante delicto* arrests up to February 2018. Importantly, our data also includes the characteristics of defendants and charges, thereby allowing for a number of additional estimations.

We find that initially detained individuals are 18.7 percentage points less likely to be rearrested within an average period of one year and five months after the detention hearing as compared to their counterparts who were released on the hearing. This is equivalent to a 66.3% reduction in the rearrest rate of 28.2% among released defendants. We also show that this effect stems entirely from prison reducing rearrest for non-violent crime, which mostly consists of theft and drug dealing episodes. We estimate pretrial detention effects separately by crime severity and find that while detained defendants are 16.1 percentage points less likely to engage in a subsequent event of non-violent crime, the effect on violent crime (mostly robberies) is positive and not statically significant.

These estimated net effects are the combination of incapacitation effects and post-release changes induced by incarceration. To examine the potential mechanisms driving our results, we estimate how pretrial detention affects

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[3]This empirical strategy has also been applied in a number of recent studies. Di Tella and Schargrodsky (2013) use the judge severity instrument to estimate the impact of electronic monitoring in Argentina. Mueller-Smith (2015), Aizer and Doyle (2015) and Dobbie et al. (2018) apply equivalent strategies to assess the impacts of pre- and post-conviction detention in the United States. Bhuller et al. (2019) also uses the same instrument in an analysis of the Norwegian prison system.

rearrest before and after trial, exploiting the fact that a number of initially detained individuals are released after trial. We find that the effect of pretrial detention on pretrial rearrest is a 26.1 percentage points reduction, even larger than that on total rearrest. This result is consistent with incapacitation explaining our estimated negative effect of prison on crime, and is in line with previous research that has provided evidence on the importance of this mechanism for crime prevention (see Buonanno and Raphael 2013, Barbarino and Mastrobuoni 2014, Munyo and Rossi 2015). However, we also find that pretrial detention increases post-trial crime by 7.1 percentage points, implying that post-release effects are likely to partly offset the incapacitation mechanism. This result challenges the idea that prison reduces crime through deterrence or rehabilitation effects.

We proceed to investigate how these effects differ depending on initial crime severity. Incapacitation effects are strong regardless of the severity of initial charges, but post-trial criminogenic effects are fully driven by individuals initially charged for non-violent crimes. Our analyses also detect that individuals who were caught committing a non-violent crime and sent to pretrial detention are more likely to engage in violent crime after trial than their counterparts who were released on the detention hearing. These findings indicate that prison experiences can increase both rearrest and crime severity, and they are consistent with a number of recent papers that study the criminogenic effects of prison. These studies link positive effects on post-release crime to the influence of prison peer effects (Bayer et al. 2009, Ouss 2011), to adverse implications on labor market outcomes (Dobbie et al. 2018, Mueller-Smith 2015) and to harsh prison conditions (Chen and Shapiro, 2007). All of these mechanisms are consistent with our setting. However, note that positive post-trial effects could also be explained by substitution of crime activity over time. Finally, we run additional heterogeneity analyses and show that positive post-trial effects on violent crime are mainly driven by older non-white defendants.

Our work is closely related to the analysis of the Argentinian prison system in Di Tella and Schargrodsky (2013), given the similarities of the two institutional settings. In Di Tella and Schargrodsky (2013), the authors use the judge severity instrumental variable approach to compare the post-release criminal behavior of defendants who were detained to that of those who were treated with electronic monitoring. They show that spending time under electronic monitoring has a large negative causal effect on rearrest. Our analyses differs to theirs in a couple of aspects. First, our sample of released individuals is significantly less constrained than those who were formerly under

electronic monitoring, since they are not under any surveillance system. This is a more usual counterfactual scenario to pretrial detention. Second, in our framework we are able to estimate both pre- and post-release effects, allowing us to provide evidence on incapacitation effects as well.

Our analysis contribute to the empirical literature on the effects of incarceration on recidivism. Three other recent studies assess the effects of prison on future crime in the United States, with similar conclusions. Mueller-Smith (2015) shows that post-conviction incarceration increases both the severity and the frequency of recidivism in the long-run, especially promoting property and drug-related offenses. Aizer and Doyle (2015) look at juvenile detention and find that it significantly increases the likelihood of adult crime. The authors also report negative effects on educational attainment, which is consistent with a human capital deterioration mechanism. Dobbie et al. (2018) analyze bail setting hearings that otherwise result in pretrial detention and conclude that pre-conviction incarceration has no impact on rearrest, with null effects being a combination of very short-run incapacitation effects with medium-term criminogenic effects. In contrast, Bhuller et al. (2019) examine the Norwegian prison system and find that prison spells discourage future crime activity through a labor-market effect, driven by detainee participation on programs inside prison that improve their employability once they are released.

Our work offers some important contributions to this literature. First, it provides some of the only evidence on the specific consequences of pretrial detention. Second, our analysis of the Brazilian system contributes to the discussion on how the characteristics of prison systems influence the effectiveness of incarceration. In particular, we address how prison affects crime in a context of an inefficient judicial system and poor prison conditions. So far, most of the empirical literature has focused on developed countries, using mostly data from the United States. Their setting differs from that of poorer countries in a number of aspects. For instance, in Dobbie et al. (2018), the average length of pretrial detention is estimated at 40 days, while in our setting this estimate is 318 days. Conditions of confinement are also remarkably different. While, US prisons operate just above full capacity on average, Brazilian jails house almost twice as much inmates as their official number of places.

The remainder of this paper proceeds as follows. Section 2 gives a description of the Brazilian prison system and the new criminal-justice procedures for pretrial detention assignment. Section 3 describes our data and the sample used in the estimations. Section 4 discusses our empirical strategy. Section 5 presents the results, and section 6 concludes.

2
Background

This section provides a more extensive description of the institutional setting analyzed in this paper. First, we provide an overview of the Brazilian prison system. We then describe the detention hearing system and discuss the features of this setting that allow us to use an instrumental variable estimation strategy.

2.0.1
Overview of the Brazilian prison system

Between 2000 and 2016, the incarceration rate in Brazil jumped from 137.1 to 352.6 people per 100,000 inhabitants, leading to a prison population of 726,712 individuals, the third largest in the world. Brazilian prisoners can serve their sentences in three types of prison conditions, depending on charge severity and recidivism profile: custodial, semi-custodial and open conditions. Custodial consists of detainees spending most of their time inside prison cells with a limited amount of time spent in the open-air. In the semi-custodial condition, detainees can move around and have to work within prison facilities. Open means that detainees are allowed to work outside the prison during office hours but are required to spend all the remaining time in a detention facility. Currently, 78% of the prison population is detained in the custodial condition, 15% in semi-custodial and 6% in open[1]. The remaining 1% is either hospitalized or under treatment of a health condition.

More than half of the individuals serving a custodial punishment - 40% of the country's detainees - have not yet received a sentence, amounting to a total of 292,450 people in pretrial detention. These people have to wait, on average, more than one year to receive a sentence[2]. While pretrial detention is always served in custodial conditions, the majority of those who are detained pretrial end up either sentenced to softer prison conditions or found to be

[1]In theory, each of these regimes should be in different prisons. However, in practice, the lack of semi custodial and open prison facilities lead to the discretionary application of alternative measures such as home detention, electronic monitoring or even to replacement with harsher prison conditions.

[2]See Infopen (2017) and Conselho Nacional de Justiça (2017). In our data we estimate pretrial detention length at 318 days.

innocent[3]. These figures contribute to the recurrent argument that many pretrial detainees spend an unnecessary time spell in custodial detention.

Figure 2.1: Conditions of confinement in Brazil. Source: Agência RBS/Folhapress

In addition to a slow functioning legal system, Brazilian prisoners also face extremely harsh prison conditions, as it has been exposed by several international organizations[4]. Prison overcrowding is one of the main issues. As of 2016, 78% of prison units operated above full capacity and the average detention centre occupation rate was estimated at 197.4% (Infopen, 2017). Detainees also suffer from critical hygiene problems. The lack of proper lighting and ventilation in prison facilities favors the spread of infectious diseases, which is reflected in the high rates of morbidity among inmates. For instance, the incidence of tuberculosis is estimated to be 30 times higher among detainees than among the non-prison population of Brazil (Ministério da Saúde, 2016). Figure 2.1 illustrates the situation of Brazilian detention centres.

Violence is another major problem inside prisons. Riots with fatal victims are not uncommon and they are the consequence of the strength of prison gangs. In fact, within the first two weeks of 2017 alone, 125 prisoners died due to riots and gang disputes inside Brazilian jails.The presence of criminal organizations within Brazilian prison facilities is a well-know fact, with the largest of them, the *Primeiro Comando da Capital* (PCC), being described

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[3]A recent study has showed that by 2014, more than half of those held in pretrial detention in Rio de Janeiro in 2013 had been sentenced to a noncustodial punishment or were found innocent, and that another 20% were still awaiting trial (Rede de Justiça Crminal, 2015).

[4]For example, in a 2015 article, the organization Human Rights Watch stated that *"Brazil's prisons are a human-rights disaster"*(Acebes (2015)).

as *"likely the most powerful prison gang in the world and the leading case of prison-based criminal governance"* (Lessing and Willis, 2019). By 2018, their estimated number of members reached 30,000 people. Like other Brazilian criminal networks, prison is where the PCC faction mostly seeks new members, with most of their leaders being behind bars as well. This setting leads to Brazilian jails frequently being described as "prison gangs' human resources" or "school of criminals". These features of the prison system in Brazil underscore the importance of assessing the consequences of pretrial detention on individuals.

2.0.2
The detention hearings system

In 2015, in response to above described situation, Brazil's National Council of Justice (*Conselho Nacional de Justiça, CNJ)* launched the *Audiências de Custódia* (Detention Hearings[5]) program. The reform aimed to replace the poorly defined procedures for pretrial detention assignment with a system in which every individual arrested *in flagrante delicto* is taken before a judge within 24 hours of their arrest. The program also aimed to fight the overcrowding of prison facilities by promoting the use of non-custodial measures for untried defendants.

To date, regional courts can choose whether or not to adopt the new system. Initial detention hearings took place in São Paulo city in February 2015. In the state of Rio de Janeiro, the program was first introduced in the capital in September 2015 and then gradually expanded until it reached the whole state in 2018. Although Brazil's largest cities have also adopted the new detention hearing system, it still lacks federal legislation and is yet to be implemented in many smaller municipalities.

Detention hearings consist of a short interaction between defendants and judges that occur in the presence of a defense lawyer and a public prosecutor. Judges have to decide whether to convert the *in flagrante delicto* arrest into pretrial detention based on the legal precedent[6] and the testimonies of those in court. In theory, defendants should not be asked about aspects of the alleged offense and decisions should be based exclusively on the conditions of the arrest

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[5]In this paper, detention hearing is used as a translation to *audiência de custódia*, although the Brazilian proceedings differ in some ways from the American system. For instance, in the US, detention hearings apply to more serious offenses, while in Brazil *audiências de custódia* are used for all types of crimes.

[6]The Brazilian law foresees that pretrial detention is ordered in five situations: if the individual threatens the proceedings of the criminal investigation; if there are strong indications that the alleged offender can influence witnesses or compromise evidence; if there is evidence that the defendant is likely to recidivate; if there is a high risk of absconding; and if the suspect is likely to produce financial harm to others.

and the defendant profile. In practice, however, judges do seem to ask for this information in order to make their decisions.

Every detention hearing analyzed in this paper took place at the Court House in Rio de Janeiro city. Four rooms were available for hearings and defendants were assigned to courtrooms - and therefore judges - as they arrived in court. This procedure ensures that judge assignment is as good as random, as long as there is no correlation between the time that an individual is caught by police - and thus the time that he arrives in court - and the type of judge who is available to conduct a detention hearing. If this is true, the characteristics of cases are orthogonal to the profile of the magistrates, which is one of the assumptions needed for the instrumental variable strategy that we use to identify the effects of pretrial detention.

In total, we observe eighteen judges in our data, and they were responsible for a minimum of 84 and a maximum of 724 cases (the mean is 318 and the median 267)[7]. Detention hearing' judges are regular state judges who are temporarily assigned to preside over detention hearings by the president of the state's Court of Justice. They are switched every four months and can be reassigned to the job in a consecutive or later period. The data we use in this study covers seven of these four-month periods, and five to six different judges are observed in each one of them. Moreover, judges need not to be in court every day and, on average, 2.1 judges conducted hearings on any given day in our sample. These features are important to account for in our model because they give room to the possibility of judge selection, both into the system and across days.

Another important feature of the detention hearing system is that judge profile plays a relevant role in defining the outcome of hearings. This ensures that our indicator of judge stringency is predictive of detention assignment. Indeed, as figure 2.2 shows, judges significantly differ in their detention assignment decisions, with the rate of pretrial assignment per magistrate ranging from 36% to 81%. This is consistent with the existence of significant differences in ideology among judges. Besides differing personal criteria for ordering pretrial detention, two other elements of the detention hearing system contribute to the existence of this variation. First, as explained above, hearings consist of a brief interaction between judges and defendants that occurs shortly after the arrest, and not after a lengthy investigation, which gives ample room to the subjective component of decisions. Second, in the Rio de Janeiro setting, judges that are not originally from a criminal court can also act as a detention

[7]Within period, each judge was responsible for a proportion of 7% to 28% of total hearings, with 90% being responsible for 10% to 25% of cases.

Figure 2.2: Rate of pretrial detention assignment per judge
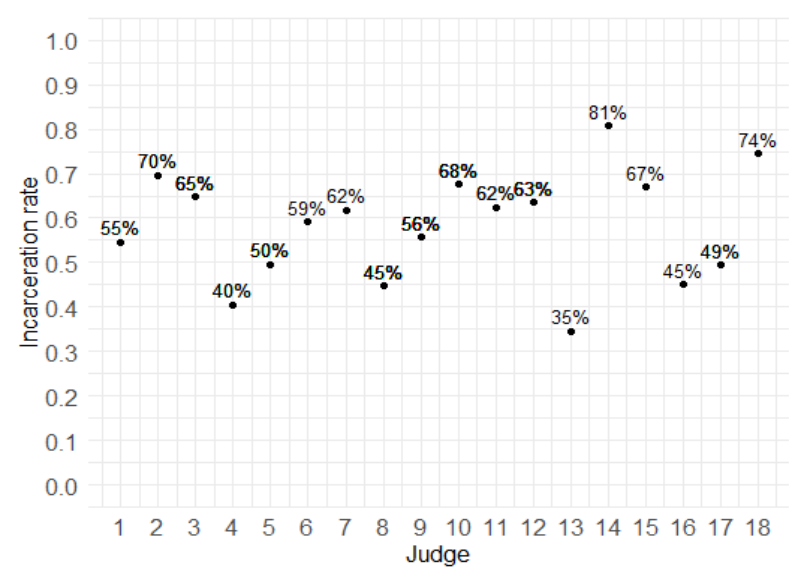
Notes: This figure plots the incarceration rate for each of the eighteen judges present in our sample. Numbers on the horizontal axis represent each judge, and the incarceration rate in the vertical axis is the proportion of cases in which each judge ordered pretrial detention.

hearing judge, which can lead to extra variation in decisions driven by judges' different backgrounds.

3
Data

For our empirical analysis we merged data from three sources and put together a novel case-level dataset with information on 8,576 detention hearings conducted in Rio de Janeiro between 2015 and 2017. Our data sources are the Rio de Janeiro's Public Defender's Office (DPRJ), the Institute of Public Safety of Rio de Janeiro (ISP) and the website of the Rio de Janeiro's Court of Justice (TJRJ).

3.0.1
Variables and sample construction

Initial information on defendants and detention hearings was obtained from the Research Department of DPRJ and from ISP, which provided detailed information on all hearings held since the implementation of the system in September 2015[1]. Note that this data does not include every *in flagrante delicto* arrest made in the period because the implementation of detention hearings was staggered across the city and only fully completed by the end of 2017. The data from the two sources was matched using either a combination of defendant's name and arrest date or the case's judicial process number[2], resulting in a cross-section of 8,576 cases carried out between September 2015 and September 2017. Besides case identifiers, the resulting dataset also includes the arrest date and charge, the date, and an identifier of the judge who presided over each hearing, and the pretrial detention decision. This dataset also contains the following defendant-level information: *registro geral* (identification) number, race, age, and gender.

Using defendant identification number we can compute our rearrest indicator based on a broader dataset of 90,760 *in flagrante delicto* apprehensions made by the police in Rio de Janeiro state between January 1st 2014 and

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[1]The data provided by DPRJ comes from questionnaires completed by public defenders at the time of the hearings and covers the period from September 2015 to September 2017. The data provided by ISP consists of administrative records of Rio de Janeiro's Civil Police and covers a larger time span, until February 2018.

[2]This number was not available for all observations in the ISP dataset and could only be used for matching arrests involving a single offender. Nearly 80% of the final dataset consists of observations matched using name and arrest date. Note that this procedure should not bias the causal estimate but could affect to some extent the external validity of the results.

February 20st 2018. The sources of this information are daily administrative records obtained from the DPRJ Provisional Detainee Attention Centre combined with police records from ISP, which also give information on the type of the alleged crime in each arrest.

Finally, in order to obtain information on the progress of judicial processes, we used text data available from the TJRJ website. We employed a scraping algorithm that was able to locate and download information on all but 319 processes using the search engine of the TJRJ website and case identifiers (name and arrest date or process number). Then, we applied regular expression matching procedures to extract trial date and process status from the texts. One drawback to computing rearrest before and after trial is that we have to drop an extra 1,046 cases because they were found to be suspended, meaning that no trial was carried out and it is likely never to be[3].

3.0.2
Descriptive statistics

Figure 3.1 presents summary statistics for our final sample of 5,728 cases[4]. We present full sample means in column 1, and in columns 2 and 3 we present summary statistics for the subsamples of individuals sent to pretrial detention and individuals released after the detention hearing. Note that initial detention assignment is based on judges' decision and does not necessarily indicate that the defendant remained detained for the whole course of the investigation until trial date[5]. Conversely, initially released individuals can also be sent for pretrial detention at a later point of the investigation. In our sample, judges ordered initial pretrial detention in 65.3% of cases.

Panel A provides a description of the defendants in our sample. Detained individuals are on average 25.9 years old, are mostly men (96.8%), non-white (78.4%), and 26.9% of them had been previously arrested in the period between January 2014 and the time of the arrest for which we observe a detention hearing. In contrast, released individuals are older (28.9 years old on average), have a higher proportion of women (12.2%) and whites (25.2%), and only 22.7% had a previous record of arrest.

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[3]The aim of splitting our rearrest variable between before and after trial is to assess the relative importance of incapacitation and post-release mechanisms. That said, unlike sentence date, suspension date is not as correlated to release decisions, so that we chose to drop suspended cases. In Appendix D we report our main estimates including these additional processes.

[4]We drop an extra number of observations due to missing information on defendants and charges characteristics.

[5]In fact, one of the legal precedents for defenders to request an *habeas corpus* for their clients is pretrial detention being longer than what the final sentence on the case is likely to be.

Table 3.1: Mean case characteristics

	All (1)	Detained (2)	Released (3)
Number of observations	5,728	3,740	1,988
Panel A. Defendants' characteristics			
Pretrial detention	0.653	1.000	0.000
Age	26.969	25.947	28.892
Woman	0.063	0.032	0.122
White	0.228	0.216	0.252
Previously arrested	0.255	0.269	0.227
Panel A. Charges' characteristics			
Number of charges	1.330	1.408	1.182
Attempted crime	0.096	0.084	0.118
Robbery	0.400	0.549	0.119
Theft	0.231	0.114	0.450
Drug related	0.236	0.239	0.231
Gun related	0.103	0.121	0.067
Receiving stolen goods	0.074	0.065	0.092
Homicide	0.017	0.024	0.004
Injury	0.009	0.009	0.010
Rape	0.004	0.005	0.001
Panel C. Outcomes			
Rearrest	0.158	0.093	0.281
Rearrest for violent crime	0.046	0.033	0.071
Rearrest for non-violent crime	0.086	0.045	0.162
Rearrest before trial	0.112	0.032	0.263
Rearrest after trial	0.046	0.061	0.017

Notes: This table reports mean characteristics for cases which had a detention hearing in the Court House of Rio de Janeiro between September 2015 and September 2017. Information on pretrial detention assignment, ethnicity, rearrest and previous arrest was obtained from DPRJ. Defendants' gender, age and charge characteristics were obtained in ISP records. Rearrest before and after trial is calculated using the data on judicial processes collected from the TJRJ website.

Panel B describes the cases in terms of the arrest charge. Cases can involve more than one charge, and charge types presented in Panel B are non-exclusive. Note that these categories refer to the understanding of the police authority responsible for registering the arrest, and judges can decide for the investigation to proceed with a different charge. Cases that resulted in pretrial detention involved an average of 1.4 charges and in 8.4% of cases this charge corresponded to an attempted crime. Robbery represented 54.9% of these cases, theft 11.4%, drug related crimes 23.6%, gun related crimes

12.1%, receiving stolen goods 6.5%, homicide 2.4%, injury 1%, and rape 0.5%. As for cases with released defendants average 1.18 charges were involved and attempted crimes represented a larger fraction of 11%. Among these cases there was a smaller fraction of robberies (11.9%), drug related crimes (23.1%), gun related crimes (6.7%), homicides (0.4%) and rapes (0.1%), and a larger fraction of thefts (45%), receiving stolen property (9.2%) and injury (1%).

The figures indicate that, although detained and released groups significantly differ in all of the observable characteristics available, there is still a relevant overlap between detained and released individuals' characteristics. This is important for our instrumental variable strategy because the fact that observable traces do not completely determine pretrial detention assignment favors the hypothesis that judge profile is a relevant component in detention decision.

Panel C presents summary statistics for the outcomes of interest to this research. 15.8% of the cases resulted in the defendant being arrested at least once more in the period between his detention hearing and February 2018. Note that this time span corresponds to the average individual being observed for almost one year and five months. This rate significantly differs between detained and released individuals: 9.3% for the first group and 28.1% for the second. In terms of rearrest severity, 3.3% of detained individuals were involved in subsequent violent crime, whereas this proportion reaches 7.1% among released individuals. For non violent crime the proportions are 4.5% and 16.2%, respectively[6].

Concerning the timing of rearrest relative to trial date, 3.2% of detained individuals were rearrested before their trials[7], while 26.3% of released individuals were. 6.1% of initially detained defendants were rearrested after trial, as compared to only 1.7% of released defendants. In our sample, pretrial period length is on average 318 days, but it significantly differs between detained and released individuals, which partly explains why total rearrest splits so differently between pre- and post-trial across the two groups. This period is of 264 days among initially detained individuals and 411 days among initially released ones. The implications of that to our results are discussed in section 5. Further, it can be noted that by February 2018, only 61.6% cases had received a sentence. For cases that had not yet been tried, total rearrest equals pretrial rearrest.

[6]Rearrest for violent and non-violent crime does not sum to total rearrest because we only used the above mentioned categories to build these indicators and they do not capture every case's crime type.

[7]This is possible because defendants can be released before trial if they receive an *habeas corpus*.

Finally, table 3.2 describes our sample in terms of both initial and rearrest crime severity. This table reports the proportion of individuals with an initial violent or non-violent charge (lines) who were rearrested for committing a violent or non-violent crime (columns). In Panel A we report this information for the whole sample. 3.95% of the individuals charged for violent crime were involved in a subsequent episode of violent crime, and 2.78% in one of non-violent crime. As for individuals with a non-violent initial charge, 5.12% were arrested for a subsequent violent charge and 12.74% for a non-violent one. In Panels B and C we show how these patterns differ across detained and released defendants. The proportions reported in these panels indicate that there is a stronger correlation between initial and rearrest crime severity for released individuals, suggesting that prison might affect the criminal behavior of individuals. This is particularly true for rearrest for violent crime (column 1): while the proportion of individuals that initially committed violent and non-violent crime is similar for detained defendants (3.08% and 3.63%), they significantly differ among released individuals (11.03% and 6.50%).

Table 3.2: Initial and rearrest crime severity

Panel A

	All	
	(1)	(2)
Rearrest for crime type:	Violent	Non-violent
Initial crime type:		
Violent	3.95%	2.78%
Non-violent	5.12%	12.74%

Panel B

	Detained	
	(1)	(2)
Rearrest for crime type:	Violent	Non-violent
Initial crime type:		
Violent	3.08%	2.33%
Non-violent	3.63%	7.39%

Panel C

	Released	
	(1)	(2)
Rearrest for crime type:	Violent	Non-violent
Initial crime type:		
Violent	11.03%	6.46%
Non-violent	6.50%	17.70&

Notes: This table reports the proportion of defendants charged with an initial crime type (vertical) rearrested for a crime of type violent or non violent (horizontal).

4
Empirical Strategy

For each case c in our dataset, consider the following regression model relating an initial pretrial detention assignment indicator (PD_c) to a rearrest indicator (R_c):

$$R_c = \beta_0 + \beta_1 PD_c + \beta_2 X_c + u_c, \tag{4-1}$$

where X_c is a vector containing both defendant and charge characteristics presented in table 3.1 and u_c is an idiosyncratic error term. Note that we observe more than one case for some individuals. The issue with estimating equation 4-1 by ordinary least squares (OLS) is that pretrial detention assignment is endogenous to the risk profile of defendants, which might lead to a correlation between PD_c and unobservable error components that correlate to the outcome variable R_c. Hence, estimating 4-1 by OLS yields an inconsistent estimate of the the parameter of interest β_1.

To address this identification problem, we use an instrumental variable (IV) strategy where we exploit judges' estimated severity as an instrument for PD_c. Formally, we estimate the following model by two-stage least squares:

$$R_{ctd} = \beta_0 + \beta_1 \hat{PD}_{ctd} + \beta_2 X_c + \delta_{td} + u_{ctd} \tag{4-2}$$

$$PD_{ctd} = \gamma_0 + \gamma_1 SS_c + \gamma_2 X_c + \eta_{td} + v_{ctd} \tag{4-3}$$

where R_{ctd} is a rearrest outcome for case c defendant, whose hearing was carried out in period t and weekday d, SS_c is the constructed severity score for the judge responsible for case c, and δ_{td} and η_{td} represent four-month period-by-day of the week fixed effects. Estimated standard errors are two-way clustered at the individual and judge level.

4.0.1
Instrument construction

Our stringency instrument is estimated from the judge decisions observed in the dataset. Precisely, for each case c seen by judge j in period t, the severity score (SS_c) corresponds to the share of all other cases from judge j in period t that resulted in pretrial detention assignment. Equation 4-4 outlines this formula:

$$SS_{cijt} = \left(\frac{1}{n_{jt} - n_{ijt}}\right) \left(\sum_{c=1}^{n_{jt}} PD_{cjt} - \sum_{c=1}^{n_{ijt}} PD_{cijt}\right), \tag{4-4}$$

with n_{jt} denoting the number of cases seen by judge j in period t and n_{ijt} denoting number of cases from defendant i seen by judge j in period t. This formula is a modification of the instrument used in Aizer and Doyle (2015) that allows judges' propensities to vary over time. We allow our instrument to vary across periods because judges' decision are likely to have evolved over time, especially considering that the detention hearing system was new and expanding during our sample period.

Our measure of judge severity is algebraically equivalent to the judge fixed effect estimated from a leave-out regression estimated in each four-month period. Thus, our resulting instrumental variable estimator is equivalent to the Jackknfie instrumental variable estimator (JIVE), which is recommended for models in which the number of instruments - in our case, the number of judges - is likely to increase with sample size (Stock et al. 2002, Kolesár et al. 2015). In appendix B we present results from a traditional JIVE estimation that uses a full set of judge dummies as instruments[1]. In this Appendix, we also present results using a residualized measure of judge severity, as suggested in Dobbie et al. (2018). This alternative measure accounts for the period-by-weekday fixed effects in the construction of the instrument as well.

4.0.2
First stage

For our instrumental variable approach to identify the causal effect of pretrial detention on rearrest, our instrument SS_c must be correlated to our endogenous variable PD_c. In support of this hypothesis, we first present a visual description of the variation in judge tendency present in our dataset. Then, we display our first stage estimates.

In Figure 4.1, white bars plot the distribution of our judge stringency measure. Our severity score ranges from 0.325 to 0.803, has a mean of 0.583 with a standard deviation of 0.116. Black bars plot a residualized version of our instrument, that means, the residuals from a regression of SS_c in all controls listed in table 3.1 and controlling for our full set of period-by-weekday fixed effects, added by median severity. This residualized measure ranges from 0.310 to 0.830, has a mean of 0.596 with a standard deviation of 0.077. Note that there is still considerable variation in our instrument when we account for the

[1]Note that this procedure yields the same coefficients as applying our estimation procedure using a judge severity instrument that is constant across periods.

Figure 4.1: Instrument variation
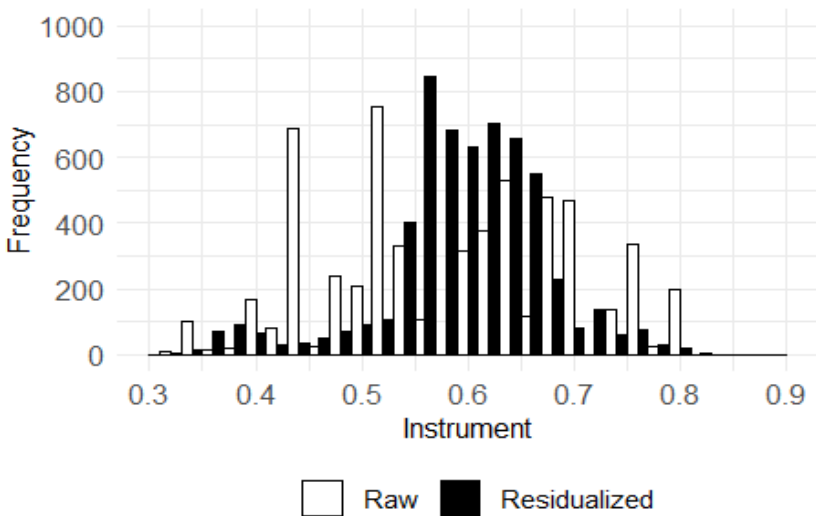
Notes: This figure presents a histogram of the instrument variable used in the regressions. White bars represent a raw measure, which is the result of calculating 4-4 for each observation in the dataset. Black bar plots a residualized measure of SS_c, which means the residuals from a regression of the raw instrument in all the variables presented in table 3.1 and the fixed effects, added by median rearrest.

variation in severity that is explained by the date of the hearing and case characteristics.

Table 4.1: First stage estimates

	Dependent variable: pretrial detention		
	(1)	(2)	(3)
Severity score	0.886***	0.838***	0.619***
	(0.038)	(0.063)	(0.038)
Controls	No	Yes	Yes
Fixed effects	No	No	Yes
F-statistic	278.532	356.28	86.968
Adjusted R^2	0.046	0.338	0.342
Observations	5,728	5,728	5,728

Notes: Fixed effects refer to four-month period-by-day of the day fixed effects. Controls include all variables listed in Table 1. Column 1 reports the mean and standard deviation from the dependant variable. In columns 2 and 3, standard errors are presented in parenthesis and they are clustered at the individual and judge levels. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

In table 4.1 we report the results from estimating equation 4-3 using a linear probability model. In column 1 we estimate the raw correlation

between SS_c and the pretrial detention indicator, which yields an estimate of 0.886, statistically significant at the 1% level. In column 2 we add case controls and the estimated correlation reduces to 0.838 and remains significant. Finally, in column 3 we also include period-by-weekday fixed effects in the first stage regression and the correlation between pretrial detention and judge severity score is estimated at 0.619, remaining statistically significant, with an associated F-statistic of 86.968. It should be noted that our fist stage estimates are fairly stable across the three specifications. According the estimate reported in column 3, going from the first quartile of judge severity to the third quartile raises the probability of incarceration by 10.8 percentage points, which corresponds to a 16.5% increase over the sample average of 65.3%.

4.0.3
Exclusion restriction

Another assumption needed for the instrumental variable approach to be valid is that our instrument only affects the outcome variables through its relationship with the endogenous variable. In our setting, this means that the severity of judges should influence rearrest exclusively by increasing the likelihood that defendants are detained pretrial. We take a few steps to address the validity of this assumption.

First, we include four-month period dummies interacted with day of the week fixed effects in our model. Random judge assignment ensures that the exclusion restriction is not violated by judges' profile correlating with defendants' propensity of being rearrested. However, our model must account for the possibility of judge selection across periods and weekdays. Since the pool of judges who enter the system is not randomly chosen, one could worry that the severity of judges correlates with crime patterns over time. Analogously, if judges choose which days of the week to work based on any expected pattern of cases on those days, the exclusion restriction would also be violated. The inclusion of fixed effects means that only the variation coming from individuals who had a detention hearing on the same day of the week and within the same four-month period is used in the estimation. In other words, we assume that judge assignment is random conditional on period and day of the week. We interact the two set of fixed effects as to allow the influence of the days of the week to vary freely across periods.

Second, although there should be no room for judge selection in addition to the above explained, we perform a balancing test in order to verify if defendants were actually randomly allocated among judges. For this test, we follow Aizer and Doyle (2015) and Di Tella and Schargrodsky (2013). First, we

assign each observation in our dataset to one of two groups of judge stringency (above and below median), based on a within period ranking of the judge severity score. Second, we test if the resulting groups are comparable in terms of all of the observable characteristics available in our dataset.

Results of the balancing test are presented in table 4.2. In columns 1 and 2 we present summary statistics for each group of judge stringency. In columns 3 and 4 we present the p-value of a mean comparison test between the severity groups, with column 5 p-values accounting for the four-month period-by-day of the week fixed effects. Since reported p-values are high and all but one is significant at the 10% level in column 5, these results corroborate the random assignment assumption.

Table 4.2: Case characteristics and judge severity

| | Severity Score | | P-value | |
Variable	< median (1)	≥ *median* (2)	All (3)	Adjusted (4)
Severity score	0.52	0.63	0.00	0.00
Panel A. Defendants' characteristics				
Age	26.85	27.07	0.37	0.38
Woman	0.07	0.06	0.02	0.01
White	0.23	0.23	0.67	0.22
Previous arrest	0.24	0.26	0.10	0.25
Panel B. Charges' characteristics				
Number of charges	1.33	1.33	0.91	0.23
Attempted crime	0.10	0.09	0.49	0.18
Drug related	0.23	0.24	0.36	0.23
Theft	0.24	0.23	0.40	0.14
Robbery	0.41	0.39	0.20	0.17
Gun related	0.1	0.11	0.26	0.26
Homicide	0.01	0.02	0.31	0.27
Injury	0.01	0.01	0.99	0.62
Receiving stolen goods	0.07	0.08	0.42	0.14
Rape	0.00	0.00	0.35	0.88
P-score	0.64	0.66	0.01	0.30

Notes: This table presents the result of a balancing test. Sample has been split by halves of judge severity. Columns 1 and 2 report mean characteristics for each group. Columns 4 and 5 present the p-values of mean comparison tests, and in column 5 p-values are adjusted for period-by-day of the week fixed effects. P-score is the probability of prison assignment estimated from a linear probability model of pretrial detention assignment in all cases characteristics listed in table 3.1 and age dummies.

Another setting that would lead to a violation of the exclusion restriction is whether judges can influence defendants' behavior through other channels other than the assignment (or not) of pretrial detention. In our case, one

important feature of the detention system is that judges' decisions are not actually binary, since release can be combined with the assignment to a non-custodial measure, such as a travel restriction or a curfew. If judges' propensity to incarcerate correlates with the assignment to a non-custodial measure, and such measures somehow affect defendants' criminal behavior, the instrumental variable strategy does not identify the causal effects of prison. However, anecdotal evidence suggest that judges do not make frequent use of stricter alternative measures and that the enforcement of such measures is also poor.

4.0.4
Monotonicity

In the presence of heterogeneous treatment effects, an additional monotonicity assumption is needed to interpret our results as the local average treatment effect (LATE) of pretrial detention on future crime. In our setting, this assumption requires that the impact of judge stringency on the probability of pretrial detention assignment is monotonic across defendants. In other words, that means individuals whose detention was ordered by a strict judge would also have been assigned to prison by a stricter judge, and that defendants who were released by a lenient judge would also have been released by a more lenient magistrate. This assumption ensures that our estimated effect can be interpret as a well defined local average treatment effect (Angrist et al., 1996).

To provide evidence that this assumption is not violated in our setting, we follow Dobbie et al. (2018) and present additional first-stage estimates by subsamples in Appendix A tables A.1 and A.2. We report estimates by crime type and severity and defendant age, gender, race and arrest history. We show that these estimates are non-negative and significant in every subsample, which is consistent with the monotonicity assumption. In Appendix A figure A.1, we also plot the correlation between judge severity estimated from different subsamples. The intuition behind this test is that monotonicity is violated if judges are strict with one type of defendant and lenient with others, which is true if, for instance, there is strong race or gender bias in their decisions. Thus, the positive and steep slopes reported in figure A.1 are consistent with the monotonicity assumption because they indicate that judges who are more (less) lenient with one subgroup are also more (less) lenient with the second subgroup of defendants[2].

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[2]As noted by Frandsen et al. (2019), this approach is actually only a weak test of monotonicity because strict monotonicity requires the additional hypothesis that subgroup-specific judge propensities monotonically increase with each judge's overall propensity

to incarcerate. However, it does test an implication of the weaker average monotonicity assumption, which still ensures that our estimate converges to a proper weighted average of treatment effects.

5
Results

In this section, we present the results for the estimated effects of pretrial detention on rearrest, following the IV strategy described in section 4. We begin by estimating the overall effect of pretrial detention on rearrest and crime severity. We then turn to investigating the role of potential mechanisms by looking at the impact of pretrial detention on rearrest that happens before and after trial. Finally, we perform heterogeneity analyses based on defendant age and race.

5.0.1
Rearrest and crime severity

Table 5.1: Pretrial detention and rearrest

	Released mean	*Outcome*: rearrest			
		OLS estimates		IV estimates	
		(1)	(2)	(3)	(4)
Panel A: sample with controls					
Pretrial detention	0.280 (0.449)	−0.193*** (0.013)	−0.146*** (0.017)	−0.176** (0.077)	−0.161* (0.092)
Observations	5,728	5,728	5,728	5,728	5,728
Panel B: sample without controls					
Pretrial detention	0.282 (0.450)	−0.191*** (0.012)		−0.187** (0.072)	
Observations	6,347	6,347		6,347	
Fixed effects	-	Yes	Yes	Yes	Yes
Controls	-	No	Yes	No	Yes

Notes: Fixed effects refer to four-month period-by-day of the week fixed effects. Controls include all variables listed in table 3.1. Column 1 present the mean of the dependent variable and the standard deviation in parenthesis. Standard errors are presented in parenthesis in columns 2-4 and they are clustered at the individual and judge levels. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

Table 5.1 presents our main findings. In panel A we use the sample of 5,728 individuals for which we observe all controls listed in table 3.1. In panel

B we use a larger sample that includes individuals with missing information for any control variable. As a benchmark, the rearrest rate among released individuals in these samples is 28.0% and 28.2%, respectively. In columns 1 and 2 we display estimated coefficients from a ordinary least squares regression which relates pretrial detention to the probability of rearrest. In column 1, only four-month period-by-weekday fixed effects are included in the regressions, and pretrial detention is related to a 19.1 to 19.3 percentage points drop in rearrest. In column 2 we add defendant and charge characteristics as controls, and the coefficient in panel A reduces to 14.6 percentage points. In columns 3 and 4 we present the results for the instrumental variable estimation. As expected from the random assignment assumption, results do not alter with the inclusion of controls in panel A column 4. Our main result is that pretrial detention reduces rearrest in between 16.1 and 18.7 percentage points, which is equivalent to a reduction of 57.5% to 66%.

We then investigate how pretrial detention affects the severity of future crime. Since we have shown that the inclusion of controls does not significantly alter our results, from here we use the sample in panel B from table 5.1 and report the results from regressions that do not include cases' controls[1]. In table 5.2, we estimate separately the effect of pretrial detention on rearrest for violent crime (columns 1 and 2) and non-violent crime (columns 3 and 4). Panel A shows that although there is a negative correlation of -0.040 between pretrial detention and rearrest for violent crime, the IV estimate in column 2 is positive and not statistically significant. That means that not only is violent crime not responsive to pretrial detention spells but also that OLS estimates are misleading for this type of crime. Conversely, column 3 shows that there is a negative correlation of -0.124 between pretrial detention and subsequent non-violent crime, but the IV estimate remains negative and statistically significant, meaning that the estimated overall reduction in rearrest is actually a reduction in rearrest for non-violent crime.

In panels B and C we further investigate how prison affects the criminal behavior of detainees. In panel B we examine how individuals initially charged with a violent crime are affected by pretrial detention, and find negative coefficients in both types of crimes, although neither are statistically significant. In contrast, in panel C we show that for individuals initially charged exclusively with non-violent crimes there is a positive (but not significant) effect on the probability of rearrest for violent crime and a large, negative and significant decrease in the probability of engaging in non-violent crime. These results indicate that pretrial detention reduces criminal activity but it is not as effective in

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[1]Results with controls are displayed in appendix C.

decreasing the severity of future crime, given the finding that some defendants engage in crimes of increased severity due to time spent in prison.

Table 5.2: Pretrial detention and crime severity

	Outcome: rearrest for crime type			
	Violent		Non-violent	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Panel A: full sample				
Pretrial detention	−0.040***	0.017	−0.124***	−0.161***
	(0.006)	(0.035)	(0.009)	(0.045)
Observations	6,347	6,347	6,347	6,347
Panel B: charged with violent crime				
Pretrial detention	−0.083***	−0.062	−0.048***	−0.065
	(0.020)	(0.047)	(0.011)	(0.061)
Observations	2,622	2,622	2,622	2,622
Panel C: charged only with non-violent crime				
Pretrial detention	−0.031***	0.042	−0.106***	−0.187***
	(0.008)	(0.034)	(0.011)	(0.070)
Observations	3,725	3,725	3,725	3,725
Fixed effects	Yes	Yes	Yes	Yes
Covariates	No	No	No	No

Notes: Fixed effects refer to four-month period and day of the week fixed effects. Controls include all variables listed in table 3.1. Standard errors are presented in parenthesis and they are clustered at the individual and judge levels. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

The adoption of an instrumental variable approach in the assessment of the effects of prison on crime is motivated by the possible existence of positive selection bias in ordinary least square estimates. This concern derives from the intuition that judges tend to incarcerate prisoners with higher idiosyncratic propensities to rearrest, so that any estimated correlation between detention and rearrest (positive or negative) will overstate the true causal effect of prison on future crime. If this is true, since IV estimates reveal the causal effect of prison, they should be lower than OLS estimates. However, the coefficients in columns 1 and 3 in table 5.1 are not statistically different from each other, and our findings contradict this intuition. We discuss some alternative explanations to selection bias that are consistent with our findings.

One first possible interpretation of our coefficients is that there is actually no selection bias in pretrial detention assignment. This could be driven by failure of judges in correctly detecting defendants' risk of engaging in future crime, combined with judges' sharp differences in decision patterns. That said,

given that detained and released individuals significantly differ in a number of observable characteristics, as presented in table 3.1, this would imply that these traces are not predictive of future crime.

A second and more plausible explanation to our findings is related to the concept of local treatment effect (LATE). In the presence of heterogeneous treatments effects, IV estimations identify a weighted average of the causal effect among a subsample of individuals, namely the compliers group. In our setting, this group corresponds to defendants who would have had a different decision on their cases had their hearings been conducted by a different judge. This means that in the presence of heterogeneous treatment effects, directly comparing OLS and IV estimates is not appropriate to determine the signal of selection bias, because these estimates apply to different pools of individuals, and this could also explain our results.

Third, based on the results in table 5.2, we suggest another interpretation to our findings that is consistent with constant treatment effects. In panel A, columns 1 to 4 show different patterns of bias across the estimation of the effect of pretrial detention on violent and non-violent rearrest. While OLS is greater than IV for violent crime, it is lower than IV for non-violent rearrest[2]. This pattern is consistent with judges acting as expected towards non-violent crime, but failing to detect the probability that a defendant will commit violent crime, leading to a prison population that is actually less likely to commit future violent crime. This is consistent with rearrest for violent crime being harder to detect, or even with judges' preferences being biased towards assessing defendants' risk of engaging in non-violent crime, such as theft of drug dealing.

5.0.2
Incapacitation and post-release effects

As described in section 3, our arrest data covers the period from January 2014 to February 2018, and the cases we study initiated between September 2015 and September 2017, meaning that we can follow individuals for at least five months and at most two years and five months after they have gone trough a detention hearing. On average, this time spell is approximately one year and five months. That means that our sample of initially detained individuals is likely to have remained in prison during most of the time we are able to observe them, not only because pretrial detention in Brazil is lengthy but also because a fraction of them were convicted and sentenced to prison, so

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[2]Although these differences are still not statistically meaningful, their p-values are close to 20% and are much smaller than the one for the IV-OLS difference in coefficients for total rearrest.

Table 5.3: Pretrial detention effects by rearrest date

	Outcome: rearrest relative to trial date			
	Before trial		After trial	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Panel A: full sample				
Pretrial detention	−0.231***	−0.261***	0.040***	0.071**
	(0.010)	(0.052)	(0.007)	(0.034)
Observations	6,347	6,347	6,347	6,347
Panel B: charged with violent crime				
Pretrial detention	−0.181***	−0.197***	0.035***	−0.041
	(0.028)	(0.068)	(0.013)	(0.049)
Observations	2,622	2,622	2,622	2,622
Panel C: charged only with non-violent crime				
Pretrial detention	−0.219***	−0.287***	0.055***	0.117***
	(0.012)	(0.061)	(0.008)	(0.030)
Observations	3,725	3,725	3,725	3,725
Fixed effects	Yes	Yes	Yes	Yes
Controls	No	No	No	No

Notes: Fixed effects refer to four-month period and day of the week fixed effects. Controls include all variables listed in table 3.1. Standard errors are presented in parenthesis and they are clustered at the individual and judge levels. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

that they remained detained after trial. Since pretrial detention affects future crime trough a combination of incapacitation and after-release effects, these facts motivate us to investigate whether our negative results are purely driven by the incapacitation mechanism or if, alternatively, we can also detect specific deterrence or rehabilitation effects of prison.

In practice, in order to distinguish between the mechanical effect of prison in reducing crime and the post-release behavior mechanism, we exploit the fact that a number of initially incarcerated defendants are released sometime after trial. We use trial date to split our outcome variable between rearrest before and after trial and estimate the effect of pretrial detention on these variables separately. We argue that incapacitation is the main component of the effect on pretrial rearrest and we relate the estimated effect of pretrial detention on post-trial rearrest to the post-release mechanism. In our sample, an average trial occurred 318 days after the detention hearing. Given that we observe defendants for an average period of 506 days, this leads to an average post-trial period of 188 days, or slightly over six months.

Table 5.3 presents the results of this exercise. In panel A columns 2 and 4 show that the roughly 19 percentage points drop in total rearrest due to pretrial detention decomposes into a 26.1 percentage points decrease on pretrial rearrest and a 7.1 percentage point increase in post-trial rearrest, both coefficients being statistically significant. Indeed, these numbers indicate that the main driving force of the estimated reduction in rearrest is likely to be incapacitation effects. Moreover, they suggest that post-release effects not only do not contribute to the reduction in future crime, but also seem to partly offset the incapacitation mechanism, which is consistent with the existence of criminogenic effects in the Brazilian prison system or with inmates substituting criminal activity over time due to incapacitation.

In panels B and C we perform the same exercise as in table 5.2 and split our sample according to the severity of initial charges. Column 2 indicates that the incapacitation mechanism is an important driver of the reduction in both groups of defendants' probability of rearrest. However, column 4 shows sizable differences in the coefficients that relate pretrial detention to post-trial crime: it is negative and not significant for individuals with a violent crime charge, and positive and significant for individuals charged with non-violent crime. These results suggest that the estimated post-release criminogenic effects stem exclusively from individuals initially charged with a less serious crime. This result is of particular interest given the concern with how pretrial detention affects low risk defendants[3].

One drawback to this estimation is that the amount of time until trial is carried out differs significantly between released and detained individuals, as previously noted. In this sample, on average, trial occurred 265 days after the detention hearing for individuals who remained detained, and 418 days after for released defendants. Moreover, we show in appendix E that there is an estimated causal effect of pretrial detention on trial date of -219 days. This means that released and detained individuals are observed for time periods of different lengths before and after trial, and this could be mechanically driving negative pretrial effects and positive post-trial effects. However, to test this hypothesis, in appendix F we repeat the exercise in table 5.2 replacing actual trial date for median trial date among detained individuals (223 days after the detention hearing), and find similar results.

Finally, in table F.2 we combine the exercises in tables 5.2 and 5.3 to

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[3]An alternative explanation for the differences in the effect on post-trial crime is that a larger fraction of individuals with a non-violent charge were released from prison during the sample period. However, this is not obvious given that the Brazilian penal system is known for being particular hard on drug trafficking charges, a non-violent crime. In appendix G we present results split between initial crimes of theft, robbery and drug-related charges, the three main charges our sample.

further investigate the effects of pretrial detention across crime severity and time relative to trial. First, there are strong incapacitation effects from pretrial detention reflected in all coefficients being negative in columns 1 and 2. This effect is particularly salient for non-violent crime. Conversely, all coefficients in columns 2 and 4 are positive, suggesting that pretrial detention increases the probability of rearrest once detainees start to be released. Importantly, offsetting effects are driven mostly by individuals initially charged with a non-violent crime who not only increase their probability of rearrest after trial but also are more likely to engage in an episode of violent crime.

Table 5.4: Pretrial detention effects by rearrest date and crime severity, IV estimates

	Outcome: rearrest for crime type and relative to trial date			
	Violent		Non-violent	
	Before trial	After trial	Before trial	After trial
	(1)	(2)	(3)	(4)
Panel A: full sample				
Pretrial detention	−0.035	0.049***	−0.200***	0.038
	(0.027)	(0.016)	(0.050)	(0.025)
Observations	6,347	6,347	6,347	6,347
Panel B: charged with violent crime				
Pretrial detention	−0.069	0.007	−0.092***	0.027
	(0.051)	(0.042)	(0.027)	(0.052)
Observations	2,622	2,622	2,622	2,622
Panel C: charged only with non-violent crime				
Pretrial detention	−0.026	0.067***	−0.232***	0.040
	(0.025)	(0.023)	(0.067)	(0.033)
Observations	3,725	3,725	3,725	3,725
Fixed effects	Yes	Yes	Yes	Yes
Covariates	No	No	No	No

Notes: Fixed effects refer to four-month period-by-weekday fixed effects. Controls include all variables listed in table 3.1. Standard errors are presented in parenthesis and they are clustered at the individual and judge levels. Significance levels: *$p<0.1$; **$p<0.05$; ***$p<0.01$.

5.0.3
Effects by age and race

In table 5.5 we present heterogeneity analyses based on defendants' age and race. Since here we have to use the smaller sample with controls, full sample results are also reported in panel A, which are very similar to those presented in table F.2 panel A.

Table 5.5: Pretrial detention effects by age and race, IV estimates

	Outcome: rearrest for crime type and relative to trial date			
	Violent		Non-violent	
	Before trial	After trial	Before trial	After trial
	(1)	(2)	(3)	(4)
Panel A: full sample				
Pretrial detention	−0.050**	0.066***	−0.177***	0.034
	(0.023)	(0.016)	(0.052)	(0.022)
Observations	5,728	5,728	5,728	5,728
Panel B: 24 year old or younger				
Pretrial detention	0.008	0.012	−0.185***	0.020
	(0.045)	(0.029)	(0.068)	(0.037)
Observations	2,984	2,984	2,984	2,984
Panel C: older than 24				
Pretrial detention	−0.111***	0.110***	−0.160**	0.030
	(0.017)	(0.015)	(0.067)	(0.032)
Observations	2,744	2,744	2,744	2,744
Panel D: white				
Pretrial detention	−0.038	−0.020	−0.430***	0.149*
	(0.041)	(0.034)	(0.085)	(0.090)
Observations	1,308	1,308	1,308	1,308
Panel E: non-white				
Pretrial detention	−0.050	0.096***	−0.111*	0.001
	(0.032)	(0.018)	(0.057)	(0.031)
Observations	4,420	4,420	4,420	4,420
Fixed effects	Yes	Yes	Yes	Yes
Covariates	No	No	No	No

Notes: Fixed effects refer to four-month period and day of the week fixed effects. Controls include all variables listed in table 3.1. Standard errors are presented in parenthesis and they are clustered at the individual and judge levels. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

According to these estimates, pretrial detention affects similarly younger and older defendants, although the effect on rearrest for violent crime operates differently across the two groups. For older individuals, there are strong incapacitation effects offset by large criminogenic post-trial effects. This is not driven by a positive correlation between violent crime engagement and age: individuals with a violent charge are actually younger, on average, than those charged with non-violent ones - they are 25.5 years old on average, as compared to an average age of 28 years old among defendants with non-violent charges.

Differences across white and non-white defendants are more salient.

Criminogenic effects are present in both groups, although they operate exclusively on rearrest for non-violent crime for whites and exclusively on rearrest for violent crime for non-whites. This could be partly explained by a correlation between race and crime severity: there is a 24.6% chance that a defendant charged with a non-violent offense is white, as compared to a 20% chance if the charge is a violent one. That said, incapacitation effects are similar across the two groups for violent crime but differ significantly for non-violent crime, being much larger among whites.

Together with previous described results, these estimates indicate that the large incapacitation effects found in this study are largely driven by a reduction in non-violent crime committed by all types of defendants in our sample, regardless of age, race and the severity of initial charges. We also find that the offsetting post-release criminogenic effects of prison that we detect are largely driven by an increase in violent crime committed especially by older non-white individuals initially charged for non-violent crimes.

In sum, our results indicate that the net effect of pretrial detention on defendants is a reduction in the probability of engaging in future crime. However, our results also show that this reduction is only observed on rearrest for non-violent crime, that is, mostly theft and drug dealing episodes, and it is not significant for future violent crime (mostly robberies). Moreover, negative results on crime apparently stem exclusively from the incapacitating effects of prison, with post-release effects on future crime likely being positive. Importantly, we also detect adverse effects on crime severity: those initially caught committing non-violent crime and that were sent to pretrial detention are more likely to engage in violent crime after prison than their counterparts who were released on the detention hearing. These individuals are also mostly non-white and older than 24. These findings suggest that prison might promote changes that increase one's engagement in crime, such as behavioural alterations or the acquisition of prison stigma. Alternatively, individuals might substitute crime over time and thus compensate for the time they were incapacitated once they leave prison.

6
Conclusions

One out of every three people imprisoned around the world has not yet been trialed (Schönteich, 2014), but whether pretrial detention reduces crime remains an open question. Arbitrary and excessive use of pretrial detention has raised concerns about the fairness of legal systems, while debates about the effectiveness of such practices are playing out across the world. In this paper, we use novel case-level data from detention hearings held in Rio de Janeiro to investigate the effects of pretrial detention on rearrest. In order to obtain causal estimates, we apply an instrumental variable approach exploiting randomly assigned judges who differ in terms of their idiosyncratic propensities to send individuals to prison.

Our main finding is that pretrial detention reduces crime by 18.7 percentage points in an average time horizon of one year and five months, equivalent to a 66.3% drop in the rearrest rate among released defendants. Second, we show that this effect is entirely driven by pretrial detention reducing engagement in non-violent crime. Third, we investigate the mechanisms underlying our results and show that they are likely to be fully driven by the incapacitation effects of prison. In this analysis we also detect the presence of criminogenic effects of pretrial detention, especially on violent crime. Finally, we show that prison also affects the severity of rearrest, because positive post-trial effects on violent crime are completely driven by the behavior of individuals who entered prison for non-violent charges. We also show that these individuals are mostly non-white and aged above 24 years old.

These findings are in line with recent studies on criminal deterrence, which find little evidence that prison reduces crime by affecting the post-release behavior of former inmates[1]. Disentangling incapacitation from deterrence is important for assessing the cost-effectiveness of incarceration policies, especially because incapacitating individuals can be an expensive way of reducing crime compared to policies that prevent individuals from engaging in a criminal activity in the first place. For instance, there is evidence that education (Deming, 2011) and labor market (Agan and Makowsky, 2018) policies can prevent the engagement in crime and thus can be thought as alternatives. As

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[1]See the recent literature review by Chalfin and McCrary (2017).

of 2014, monthly costs for each inmate in the Rio de Janeiro prison system were estimated at $1,708$ Brazilian Reais, more than twice the state's minimum wage that year. In this paper, we provide an important parameter for this analysis, the causal effect of prison on rearrest. However, it can be noted that our framework only allows for a partial equilibrium analysis, and our estimates account for deterrence effects on former inmates but not on the general population, which should also be taken into account.

Our results are also related to a recent literature that assesses how prison can promote post-release crime. In Dobbie et al. (2018) and Mueller-Smith (2015), the authors find that prison lowers wages, employment and increases dependence on public assistance. This could happen because employers discriminate against former inmates or because there is a depreciation in human capital due to time spent in prison. Similarly, in Aizer and Doyle (2015), juvenile prison is found to reduce educational attainment. Prison also promotes crime when it works as a "school of crime". Inmates can be negatively influenced by their peers by gaining skills and setting up criminal networks, as shown in Ouss (2011) and Bayer et al. (2009). In Chen and Shapiro (2007), prison harsh conditions are found to increase post-release crime, which could be the result of prison stimulating a negative perspective of society and thus violent behavior. All of these mechanisms are consistent with the situation of the Brazilian prison system and the heterogeneity analyses we run. Thus, further research remains to be done in order to understand how these mechanisms operate and provide guidance for policy intervention. Moreover, one alternative explanation for positive post-release effects that cannot be ruled out in our scenario is that individuals are simply substituting criminal activity over time.

We conclude that pretrial detention reduces crime activity trough an incapacitation mechanism. Given that we are able to detect positive post-release effects even in our limited sample period, covering only slightly over six months of post-trial period, our study also provides evidence that there are offsetting effects hindering the effectiveness of prison in fighting crime. These effects are particularly worrisome because they also reveal that prison is likely to increase the severity of crimes committed by former inmates, which can cost lives and increase the feeling of insecurity in the population. Hence, our results underscore the importance of running a cost-benefit analysis of the Brazilian prison system and of other similar systems. Our study also highlights the need for a deeper understanding of how prison affects individuals, so that governments can improve the effectiveness of prison policies by targeting the criminogenic mechanisms of incarceration.

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A
Monotonicity tests

Table A.1: First stage and charge characteristics

| | Dependent variable: pretrial detention | | | | |
| | Crime severity | | Crime type | | |
| | Non-violent | Violent | Robbery | Theft | Drug related |
	(1)	(2)	(3)	(4)	(5)
Severity score	0.698***	0.493***	0.492***	0.963***	0.571***
	(0.060)	(0.065)	(0.071)	(0.204)	(0.184)
Observations	3,321	2,407	2,289	1,321	1,351
Adjusted R^2	0.249	0.108	0.088	0.200	0.178
Fixed effects	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Notes: Fixed effects refer to four-month period-by-weekday fixed effects. Controls include all variables listed in table 3.1. Standard errors are presented in parenthesis and they are clustered at the individual and judge levels. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

Table A.2: First stage and defendants characteristics

	Gender		Age		Race		Previously arrested	
	Men	Woman	Below 24	Over 24	White	Non-white	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Dependent variable: pretrial detention					
Severity score	0.610***	0.728***	0.570***	0.671***	0.601***	0.626***	0.611***	0.651***
	(0.039)	(0.159)	(0.072)	(0.113)	(0.070)	(0.045)	(0.067)	(0.160)
Observations	5,368	360	2,984	2,744	1,308	4,420	4,270	1,458
Adjusted R^2	0.329	0.249	0.334	0.339	0.316	0.352	0.365	0.287
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Fixed effects refer to four-month period-by-weekday fixed effects. Controls include all variables listed in table 3.1. Standard errors are presented in parenthesis and they are clustered at the individual and judge levels. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

Figure A.1: Correlation between estimated instruments

(A.1(a)) Gender

(A.1(b)) Race

(A.1(c)) Age

(A.1(d)) Arrest history

(A.1(e)) Crime severity
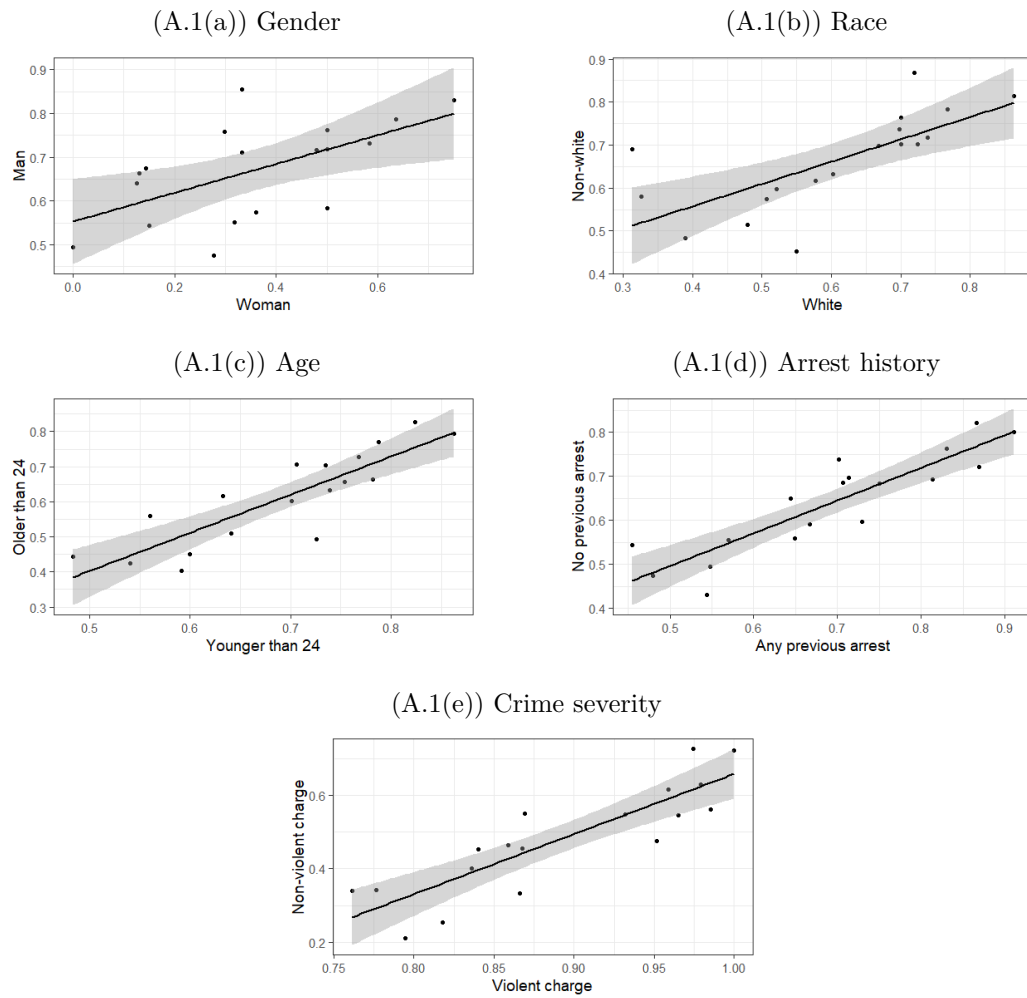
Notes: In this figure each panel plots the estimated relationship between the judge severity instrument calculated from the each of the two subsamples indicated in the axes. Each dot represent one judge. The black line represents the fitted linear model that relates the two type of instruments and shaded areas correspond to 95% confidence intervals.

B
Alternative instruments

Table B.1: Pretrial detention effects using alternative instruments

	OLS estimates		IV estimates	
Outcome: any rearrest				
	(1)	(2)	(3)	(4)
Panel A: residualized instrument				
Pretrial detention	−0.193***	−0.146***	−0.198***	−0.197**
	(0.013)	(0.017)	(0.075)	(0.085)
Panel B: JIVE				
Pretrial detention	−0.193***	−0.146***	−0.136*	−0.119
	(0.013)	(0.017)	(0.077)	(0.093)
Observations	5,728	5,728	5,728	5,728
Fixed effects	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes

Notes: Fixed effects refer to four-month period-by-day of the day fixed effects. Covariates include all variables listed in table 3.1. Standard errors are presented in parenthesis and they are clustered at the individual and judge levels. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

C
Results with controls

Table C.1: Pretrial detention and crime severity, estimates with controls

	Outcome: rearrest for crime type			
	Violent		Non-violent	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Panel A: full sample				
Pretrial detention	−0.036***	0.028	−0.074***	−0.124**
	(0.009)	(0.030)	(0.011)	(0.059)
Observations	5,728	5,728	5,728	5,728
Panel B: charged with violent crime				
Pretrial detention	−0.079***	−0.020	−0.043***	−0.002
	(0.022)	(0.059)	(0.013)	(0.054)
Observations	2,407	2,407	2,407	2,407
Panel C: charged only with non-violent crime				
Pretrial detention	−0.020**	0.046	−0.089***	−0.183**
	(0.009)	(0.031)	(0.015)	(0.087)
Observations	3,321	3,321	3,321	3,321
Fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: Fixed effects refer to four-month period and day of the day fixed effects. Controls include all variables listed in table 3.1. Standard errors are presented in parenthesis and they are clustered at the individual and judge levels. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

Table C.2: Pretrial detention effects by rearrest date, estimates with controls

	Outcome: rearrest relative to trial date			
	Before trial		After trial	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Panel A: full sample				
Pretrial detention	−0.200***	−0.262***	0.055***	0.096**
	(0.013)	(0.069)	(0.008)	(0.040)
Observations	5,728	5,728	5,728	5,728
Panel B: charged with violent crime				
Pretrial detention	−0.177***	−0.167*	0.040***	0.014
	(0.030)	(0.089)	(0.015)	(0.069)
Observations	2,407	2,407	2,407	2,407
Panel C: charged only with non-violent crime				
Pretrial detention	−0.211***	−0.311***	0.060***	0.122***
	(0.014)	(0.086)	(0.010)	(0.037)
Observations	3,321	3,321	3,321	3,321
Fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: Fixed effects refer to four-month period and day of the day fixed effects. Controls include all variables listed in table 3.1. Standard errors are presented in parenthesis and they are clustered at the individual and judge levels. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

Table C.3: Pretrial detention effects by rearrest date and crime severity, IV estimates with controls

	Outcome: rearrest for crime type and relative to trial date			
	Violent		Non-violent	
	Before trial	After trial	Before trial	After trial
	(1)	(2)	(3)	(4)
Panel A: full sample				
Pretrial detention	−0.052*	0.080***	−0.170***	0.041*
	(0.027)	(0.020)	(0.061)	(0.025)
Observations	5,728	5,728	5,728	5,728
Panel B: charged for violent crime				
Pretrial detention	−0.079	0.058	−0.045	0.043
	(0.066)	(0.064)	(0.027)	(0.056)
Observations	2,407	2,407	2,407	2,407
Panel C: charged only for non-violent crime				
Pretrial detention	−0.043	0.089***	−0.220**	0.029
	(0.026)	(0.026)	(0.086)	(0.039)
Observations	3,321	3,321	3,321	3,321
Fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: Fixed effects refer to four-month period-by-weekday fixed effects. Controls include all variables listed in table 3.1. Standard errors are presented in parenthesis and they are clustered at the individual and judge levels. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

D
Results including suspended processes

Table D.1: Pretrial detention and rearrest, including suspended processes

| | | *Outcome*: any rearrest | | | |
| | | OLS estimates | | IV estimates | |
	Released mean	(1)	(2)	(3)	(4)
Pretrial detention	0.244	−0.156***	−0.117***	−0.171**	−0.171*
	(0.429)	(0.011)	(0.018)	(0.079)	(0.093)
Observations	6,665	6,665	6,665	6,665	6,665
Fixed effects	-	Yes	Yes	Yes	Yes
Controls	-	No	Yes	No	Yes

Notes: Fixed effects refer to four-month period-by-day of the day fixed effects. Controls include all variables listed in table 3.1. Standard errors are presented in parenthesis and they are clustered at the individual and judge levels. Significance levels: *p$<$0.1; **p$<$0.05; ***p$<$0.01.

Table D.2: Pretrial detention and crime severity, including suspended processes

	Outcome: rearrest for crime type			
	Violent		Non-violent	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Panel A: full sample				
Pretrial detention	−0.031***	0.023	−0.056***	−0.138**
	(0.009)	(0.034)	(0.010)	(0.061)
Observations	6,665	6,665	6,665	6,665
Panel B: charged with violent crime				
Pretrial detention	−0.070***	−0.029	−0.038***	−0.014
	(0.018)	(0.057)	(0.012)	(0.043)
Observations	2,556	2,556	2,556	2,556
Panel C: charged only with non-violent crime				
Pretrial detention	−0.015*	0.037	−0.068***	−0.195**
	(0.008)	(0.035)	(0.013)	(0.092)
Observations	4,109	4,109	4,109	4,109
Fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: Fixed effects refer to four-month period and day of the day fixed effects. Controls include all variables listed in table 3.1. Standard errors are presented in parenthesis and they are clustered at the individual and judge levels. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

E
Pretrial detention and trial date

Table E.1: Pretrial detention and trial date

	Released mean	OLS estimates		IV estimates	
		Outcome: any rearrest			
		(1)	(2)	(3)	(4)
Pretrial detention	411.170	−155.552***	−146.102***	−208.070***	−219.347***
	(177.681)	(14.394)	(13.853)	(27.787)	(31.144)
Observations	5,728	5,728	5,728	5,728	5,728
Fixed effects	-	Yes	Yes	Yes	Yes
Controls	-	No	Yes	No	Yes

Notes: Fixed effects refer to four-month period-by-day of the day fixed effects. Controls include all variables listed in table 3.1. Standard errors are presented in parenthesis and they are clustered at the individual and judge levels. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

F
Pretrial detention effects by rearrest date using median trial date

Table F.1: Pretrial detention effects by rearrest date using median trial date

	Outcome: rearrest relative to trial date			
	Before trial		After trial	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Panel A: full sample				
Pretrial detention	−0.169***	−0.271***	−0.022***	0.084
	(0.010)	(0.033)	(0.007)	(0.052)
Observations	6,347	6,347	6,347	6,347
Panel B: charged with violent crime				
Pretrial detention	−0.102***	−0.281***	−0.044**	0.043
	(0.024)	(0.055)	(0.017)	(0.083)
Observations	2,622	2,622	2,622	2,622
Panel C: charged only with non-violent crime				
Pretrial detention	−0.159***	−0.286***	−0.005	0.120***
	(0.013)	(0.046)	(0.008)	(0.044)
Observations	3,725	3,725	3,725	3,725
Fixed effects	Yes	Yes	Yes	Yes
Controls	No	No	No	No

Notes: Fixed effects refer to four-month period and day of the day fixed effects. Controls include all variables listed in table 3.1. Standard errors are presented in parenthesis and they are clustered at the individual and judge levels. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

Table F.2: Pretrial detention effects by rearrest date and crime severity using median trial date

	Outcome: rearrest for crime type and relative to trial date			
	Violent		Non-violent	
	Before trial	After trial	Before trial	After trial
	(1)	(2)	(3)	(4)
Panel A: full sample				
Pretrial detention	−0.054***	0.069***	−0.174***	0.015
	(0.021)	(0.023)	(0.033)	(0.023)
Observations	6,347	6,347	6,347	6,347
Panel B: charged with violent crime				
Pretrial detention	−0.127***	0.065	−0.079**	0.014
	(0.034)	(0.041)	(0.032)	(0.063)
Observations	2,622	2,622	2,622	2,622
Panel C: charged only with non-violent crime				
Pretrial detention	−0.028	0.070**	−0.219***	0.032
	(0.024)	(0.028)	(0.043)	(0.032)
Observations	3,725	3,725	3,725	3,725
Fixed effects	Yes	Yes	Yes	Yes
Controls	No	No	No	No

Notes: Fixed effects refer to four-month period-by-weekday fixed effects. Controls include all variables listed in table 3.1. Standard errors are presented in parenthesis and they are clustered at the individual and judge levels. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

G
Results by crime type

Table G.1: Pretrial detention effects by crime type

| | Theft | | Drug | | Robbery | |
| | Before | After | Before | After | Before | After |
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: charged for theft						
Pretrial detention	−0.320***	0.136**	0.025	−0.016	0.029	0.114***
	(0.084)	(0.059)	(0.015)	(0.021)	(0.029)	(0.031)
N	1,482	1,482	1,482	1,482	1,482	1,482
Panel B: charged for drug related crime						
Pretrial detention	−0.006	−0.013	−0.136***	0.014	−0.018	0.064**
	(0.019)	(0.018)	(0.043)	(0.050)	(0.019)	(0.029)
N	1,508	1,508	1,508	1,508	1,508	1,508
Panel C: charged for robbery						
Pretrial detention	−0.096***	0.001	−0.026	0.028*	−0.065	0.011
	(0.031)	(0.058)	(0.016)	(0.016)	(0.058)	(0.039)
N	2,493	2,493	2,493	2,493	2,493	2,493
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	No	No	No	No	No

Outcome: rearrest for crime type and relative to trial date

Notes: Fixed effects refer to four-month period-by-weekday fixed effects. Controls include all variables listed in table 3.1. Standard errors are presented in parenthesis and they are clustered at the individual and judge levels. Significance levels: *p<0.1; **p<0.05; ***p<0.01.