The Effects of Pretrial Detention on Sentencing Outcomes

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Häktning före rättegång är en viktig institution i många länders rättssystem. Det ger domare möjlighet att frihetsberöva åtalade före och under rättegången och är vanligtvis en förebyggande åtgärd som begärs av en åklagare. Domaren häktar oftast av två skäl: För det första om den åtalade skulle kunna påverka rättegången, antingen genom att inte inställa sig i domstol eller genom att vidta åtgärder mot någon av de berörda aktörerna (till exempel åklagare och brottsoffer). För det andra om den åtalade bedöms vara farlig för allmänheten om hen släpps fri innan fällande dom. Trots att användningen av häktning är omfattande, och varje år innebär frihetsberövande för miljontals individer runt om i världen, vet vi mycket lite om dess effekter.

I den här studien undersöker vi om häktning före rättegång påverkar domslut i Chile. För att skatta orsakssamband använder vi oss av att domare i Chile tilldelas fall slumpmässigt och att domare skiljer sig åt i hur ofta de godkänner en häktning. Vi finner att ett beslut om häktning ökar sannolikheten för fällande dom med cirka 23 procentenheter – en ökning med 43 procent. Effekten förklaras i hög grad av en ökad sannolikhet att den åtalade närvarar vid rättegången, eftersom risken för utevaro i stort sett försvinner vid häktning. Den skattade effekten är stor med tanke på att det chilenska rättssystemet ger mycket begränsade möjligheter för åklagare att förhandla med de åtalade under rättegången och innehåller institutionella hinder mot så kallad "plea bargaining".

Efter att ha fastställt ett orsakssamband mellan häktning och sannolikheten för fällande dom undersöker vi en särskild reform av rättssystemet i Chile år 2016 som effektiviserade åtalen mot personer misstänkta för rån. Eftersom reformen endast påverkade åtal för rån och liknande brott, studerar vi hur den påverkade häktningens betydelse för fällande domar för rån, i jämförelse med andra brottstyper som inte påverkades av reformen. Våra resultat tyder på att reformen, genom att bland annat ge åklagare möjlighet att förhandla med de åtalade, ökade effekten av häktning på fällande domar med 29 procentenheter för rånfall, medan andra brott inte påverkades.

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Abstract

Pretrial detention accounts for an important share of prison population around the globe and imposes detrimental consequences on defendants. We examine its impact on sentencing outcomes in Chile's criminal justice system, where plea bargaining is a limited prosecution tool. Pretrial detention increases conviction probability from 53 to 76 percent. We also assess how this effect changed after a reform modified prosecution tools for a specific set of crimes (robberies). The impact of pretrial detention on conviction increases for robbery-related crimes while we observe no relevant changes for others. These findings highlight pretrial detention's role in shaping criminal justice outcomes.

Keywords: Pretrial detention, Sentencing outcomes, Plea bargaining, Judge-IV design

JEL Classification: K42, C26, D01

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1 Introduction

Pretrial detention is a crucial institution of the criminal justice system in many countries. It allows judges to incarcerate defendants during the trial period. It is a precautionary decision requested by a prosecutor and authorized by a judge who can incarcerate defendants prior to a trial conviction. It is usually based on two specific concerns: i. the defendant would affect the course of the trial either by failing to appear in court or adopting actions against some of the relevant actors (prosecutors, victims, etc) or ii. a possibility where letting the defendant out before conviction is likely to put the public at danger. Despite the extensive use of this practice that annually affects millions of people in many countries (Initiative, 2011) there is little analysis on its effects.

In this paper, we study the causal impact of pretrial detention on sentencing outcomes in Chile. The Chilean case offers an interesting setting to study how pretrial detention may affect defendant's outcomes. Similar to other cases, judges can be considered to be allocated in a quasi random fashion, which allows us to asses its causal impact.[1] However, unlike the United States where most of the evidence on the downstream consequences of pretrial detention comes from, the Chilean criminal justice has been designed to restrict the role of plea bargain (Riego, 2017). Previous studies point to a weakening on defendants bargaining position as the main explanation for the large impact of pretrial detention on conviction, thus it may remain unclear how pretrial detention may affect defendants' outcomes in a setting where plea bargain is a more restricted tool.[2] For instance, in Chile around 59% of cases going to trial are solved through a plea bargain procedure, compared to 95% in the United States (Lerman et al., 2022).

We use data for all pretrial detention hearings in Chile during 2005-2020. For the first part of the paper we restrict the sample to the 2005-2016 period, before a prosecutorial reform took place. During this period, we exploit the quasi-random allocation of judges to build a leave-out leniency as an instrumental variable (IV) for pretrial decisions. Following recent recommendations from Frandsen et al. (2023) we discuss the validity of our instrument

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[1]The criminal process consists of several stages. It begins with a formalization hearing, presided over by a randomly assigned judge who -after a formal prosecutor's request- determines whether a defendant is sent to pretrial detention or not. At this stage, alternative exits may also be offered, consisting on conditional suspension of the procedure or reparatory agreements, which avoid trial and conviction. Otherwise, the case proceeds to a formal accusation and is ultimately resolved –either through conviction or absolution– by a different judge in an ordinary, simplified or plea bargain trial.

[2]In general, a plea bargain is "an agreement between the prosecutor and the defendant which the latter pleads through guilty of a crime without a trial" (Silveira, 2017, p.419). Defendants can plead guilty under specific circumstances and only when they are charged for minor offenses.

and observe that our results are robust to different specifications of the instrument.

We find that a pretrial detention decision increases the probability of conviction by roughly 23 percentage points (pp), relative to a baseline of 53 percent. The effect is strongly driven by a change in the probability of attending a sentencing trial. This estimate of the impact of pretrial detention on the likelihood of conviction speaks to similar estimates in the United States once we pool the probability of conviction and plea guilty in a single outcome category. In Philadelphia, Stevenson (2018) documents 6.2 pp in conviction and 4.7 pp in guilty plea, in NYC, Leslie and Pope (2017) find estimates of 13 pp and 10 pp, respectively, while Dobbie et al. (2018) show estimates around 14 pp for 10 pp for being guilty and plead guilty, respectively. From a comparative standpoint, the magnitude of this effect can be assessed as large after considering the specific features of the Chilean system -with a limited ability of prosecutors to negotiate with the defendant during the trial and institutional constraints on prosecutorial plea bargaining.

After establishing a causal link between pretrial detention and the probability of conviction, and given the magnitude of these results, we look at a particular feature introduced in July 2016 to the criminal justice system. With the aim of better-prosecuting defendants charged for robbery, Law 20.931 limited the application of alternative exits and instead encouraged the prosecution of cases through plea bargain trials. Since this reform altered the prosecution of robbery-related crimes, we describe how it changed the impact of pretrial detention on sentencing outcomes for that specific subset of crimes, relative to what we observe for the prosecution of other types of crimes not necessarily affected by the reform. In particular, our results suggest that by, among other things, enhancing prosecutors' plea bargaining to bring defendants to trial, the effect of pretrial detention on the likelihood of conviction increased substantially for robberies after the establishment of the 2016 reform.

Prior to the 2016 reform, we document some degree of heterogeneity in the impact of pretrial detention by type of crime, but in all cases the estimate ranges between 17 to 26 pp –with respect to different baseline levels. After the reform, despite the large change in the impact of pretrial detention for robbery-related crimes, we find no similar variations for crimes that were unaffected by the 2016 reform. We show results for two sets of crimes that were not affected by the 2016 reform: theft and other types of crimes. Again, we find that this change is mainly driven by an increased likelihood of attending a trial.

Our results speak to a set of recent studies on the effect of pretrial detention in the U.S. where the plea bargaining process plays a key role. These studies have focused on criminal justice outcomes such as the probability of conviction (Heaton et al., 2017; Lerman et al., 2022; Dobbie et al., 2018; Leslie and Pope, 2017), harsher sentences (Didwania, 2020; Stevenson, 2018), and future crimes (Heaton et al., 2017; Dobbie et al., 2018). Despite the extensive application of pretrial detention in Latin America, there is even less rigorous evidence on its use and effects. One exception is Grau et al. (2023) who study the effect of pretrial detention of *ex-post* non-guilty people on labor market outcomes in Chile. Importantly, they report that pretrial detention has increased from 63 to 90 individuals per 100,000 inhabitants in the last ten years.[3] In terms of the prison population, pretrial detainees accounted for 22% of the overall population in 2007, up to 36% in 2017.[4]

By examining the 2016 reform we provide new evidence on the relationship between plea bargaining and pretrial detention. Most of the United States studies on the effect of pretrial detention on sentencing outcomes show that the increase in the likelihood of conviction is through an increase in guilty pleas (Stevenson, 2018; Heaton et al., 2017; Dobbie et al., 2018). We further examine this relationship not only by assessing its link in a more restricted setting but also how it became more prominent after expanding prosecutors' ability to negotiate with defendants.[5]

The remainder of the paper proceeds as follows. Section 2 provides an overview of the criminal justice system in Chile. Our data is described in Section 3. Section 4 details our empirical strategy. Section 5 presents the results, Section 6 discusses the mechanisms, and Section 7 concludes.

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[3]Dobbie and Yang (2019) report that this is the second-highest pretrial detention rate per 100,000 individuals, following the United States.
[4]In 2024, Gendarmería reports 20k out of 55k individuals in prison are pretrial detainees which accounts for a rate of 105 individuals per 100,000 inhabitants and 36% of prison population.
[5]For instance, around 58% of cases going to trial are solved through a plea bargain procedure, compared to 95% in the United States (Lerman et al., 2022).

2 Institutional background

2.1 Criminal justice system in Chile

After the revision of a crime report, the Public Ministry decides whether to open an investigation or not. The first step is the formalization hearing.[6] At this stage, a judge decides between two options, depending on the case. One is to offer alternative exits, which are are mechanisms of conflict resolution that look for the parties to find agreements in order to avoid going to trial. There are two possible alternative exits that can be offered by the judge at the formalization hearing: conditional suspension of the procedure and reparatory agreements. The first one consists of offering the defendant to suspend the procedure for a period of 1 to 3 years. During that time, the defendant is given behavior conditions that, if fulfilled, lead to terminate the criminal process. The second alternative exit that might be offered corresponds to a deal between the two parties on which the defendant must repair the victim's damages as agreed. Both of these solutions avoid going to trial, and with it, conviction and sanctions.

The second option is to inform the defendant about the investigation and establish, if necessary, precautionary measures.[7] These apply for as long as the investigation takes place (with a maximum of 2 years) and include, domiciliary arrest, monthly signatures at police station, prohibition to leave the country, and, among others, pretrial detention. Once the investigation ends, the prosecutor may decide not to persevere or dismiss the case.[8] Otherwise, having enough evidence, the defendant will be accused and therefore going to trial.

There are three different trial procedures: i. Ordinary, for serious or complex cases involving disputed facts that take longer (usually for robberies, homicides, rapes), it is a full trial where a panel of three judges (with the pretrial judge excluded from selection) deliberate a verdict and set a sentence based on extensive evidence; ii. Simplified, for less serious crimes and minor offenses (less than 3 years of imprisonment, usually for theft, minor assault, traffic violation), with much quicker resolution and a single judge (strictly distinct from the pretrial judge) makes a decision based on simplified evidence; iii. Plea bargain, also for less severe crimes (less than 5 years of prison,

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[6]Chile's criminal procedure is described in Figure B1 and Appendix section C provides a step-by-step description.
[7]Appendix section D describes all types of precautionary measures.
[8]In case of not finding enough evidence, realizing the person is not guilty or finding someone else is guilty.

usually for burglary, fraud, and since 2016, also for robberies[9]) and to expedite cases where the defendant pleads guilty and agrees a reduced sentence, where a single judge (strictly distinct from the pretrial judge) decides the outcome.[10]

In this paper, we focus on the three possible sentencing outcomes: convicted, absolved and alternative exits. Alternative exits are equivalent to not going to trial. Then, going to trial can be decomposed on convicted and absolved. We also split trial into the three different procedures previously mentioned and compare them to alternative exits, allowing us to better understand of the mechanisms behind the criminal process.

2.2 Pretrial Detention

Pretrial detention is an important institution of the Chilean criminal justice system. It allows judges to incarcerate defendants prior to a trial conviction, while the investigation is being conducted. Until 1976, under the inquisitorial system, the most serious offenses led the legislator to establish that pretrial detention was deemed necessary and the judges were not authorized to lift it. In 1976, a constitutional act opened the possibility to grant provisional release, which was reaffirmed in the 1980 Constitution. Still, no changes were observed until the establishment of a new accusatory system in 2000 through the Criminal Procedure Reform (fully implemented in 2005). These norms are applicable ever since (Duce and Riego, 2015).

The adjudication process of pretrial detention works as follows: prosecutors should request to pretrial detain a particular defendant at the formalization hearing. Judges can accept or reject the prosecutor's request based on specific reasons they need to justify based on two main concerns that should be expressed on their verdicts: i. releasing the defendant could alter the course of the trial or, ii. the releasing the defendant may impose the society to a substantial danger of being victimized.[11]

Every year, there are around 30,000 pretrial detention hearings in Chile. In average, the measure is conceded for 90% of the cases. After the U.S., Chile is the country with the highest rate of pretrial detained (80 per 100,000

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[9]Robbery-related crimes are included (up to 10 years of prison) after the 2016 reform that will be explained in section 2.3.
[10]In our sample, the distribution of trial procedures is: 59% plea bargain, 36% simplified, 5% Ordinary
[11]Appendix E details the requirements to order pretrial detention from the Criminal Procedure Code, Article 140.

individuals) among OECD countries (Dobbie and Yang, 2019). Thus, it accounts for an important share of the prison population (20% in the U.S., 36% in Chile).

Individuals can be held for a maximum of 2 years before trial, which is the maximum time for the investigation. According to our data, the average length of pretrial detention (measured as the time between the formalization hearing and the trial) is 6 months.

Under the Chilean adversarial criminal justice system, different judges handle different stages of the processes. In particular, a first judge handles formalization hearings which includes pretrial detention decision among other precautionary measures. Then, a judge conducts the trial and imposes the sentence.[12] This judge is different from the first judge, to ensure impartiality.[13] At the formalization courts, cases are distributed among judges on a rotating basis. The rotation mechanism is designed to ensure equal treatment and fairness by preventing judges from selecting their own cases and preventing parties from choosing specific judges. This creates a quasi-random allocation process. At trial courts, cases are similarly assigned on a rotation basis, but judges cannot have been involved in the pretrial phase of the same case. This is a key feature that will be exploited for quasi-experimental variation in the allocation of pretrial detention in section 4.

2.3   The 2016 Reform: Modifying prosecutorial tools

In July 2016, Law 20.931 was passed in Chile. It aimed to facilitate the effective application of established penalties for robberies and few other related crime-categories, and improving the criminal prosecution on those crimes (The law reads as follows: *"Facilita la aplicación efectiva de las penas establecidas para los delitos de robo, hurto y receptación y mejora la persecución penal en dichos delitos"* (Law 20.931, 2016). Broadly, the new law contained two main changes: the first modification was specifically aimed to reduce robbery related crimes and consisted of two channels: i. an increase of the expected sanctions by setting longer sentencing times in the criminal code[14] and ii. enhancing

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[12] This is the case for plea bargain and simplified trials, whereas for ordinary procedures, there is panel of judges, usually three, with one president of the court.

[13] The courts are separated, but since they are part of the same judicial system, we cannot ensure that judges from different roles never interact in some way. Nevertheless, they do not overlap in their functions for the same case.

[14] For example, the minimum sentence for robberies with violence or intimidation increased from 10 to 15 years.

prosecution capacities by limiting the application of the principle of opportunity, limiting the regime of conditional suspension of the procedure, and allowing robberies to be resolved through plea bargain trials rather than exclusively through ordinary trials. In practice, these changes reduced the incentives to offer alternative exits and made it easier for prosecutors to get defendants on trial (e.g. through plea bargaining procedures). These changes implied modifications to the Criminal Code (increasing sanctions) and the Criminal Procedure Code (trial procedures).[15] In addition, the 2016 reform also introduced preventive identity controls, which allow police to control the identity of any person over 18 years of age on public roads, public places and private places accessible to the public.[16]

The second part of the paper aims to focus on describing the changes after July 2016 when the reform was passed. Since all changes were implemented at once, we are unable to fully disentangle which component of the reform is driving the observed changes. In this section, we argue that these effects can be mainly attributed to the modifications looking to reduce robbery related crimes (changes in the Criminal Code and the Criminal Procedure Code) and not necessarily to changes in police behavior on citizen control –which were not only targeted at robbery-related crimes. Of course, preventive identity controls may have also affected the set of cases in court making hard to disentangle the observed effects. We will later examine this issue on detail by comparing several features of the set of criminal cases over time. During the 2016 reform, we observe no major changes in either the types of crimes prosecuted or the cases where prosecutors requested pretrial detention. This suggests that the observed changes in pretrial detention outcomes stem from changes in prosecution methods rather than other upstream factors in the process.

3 Data

This study leverages administrative data from a highly centralized agency, *Poder Judicial* (PJ). PJ oversees all courts and the judiciary system in Chile. We collected information on all criminal cases between January 2005 and March

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[15]See Riego (2017).

[16]Prior to the 2016 reform, police was allowed to perform identity controls under court warrant. After the 2016 reform, police was able to conduct this procedure without the need of court warrant, but still respecting equality of treatment and non-arbitrary discrimination. We do not have data on the source of the arrest for each criminal case but in section 6.1 we examine several features of the criminal cases over time and discuss whether it substantially increased the number of cases where prosecutors asked for pretrial detention.

2020. The data has information at the defendant-case level of all court hearings, including pretrial detention and sentencing hearings. For each hearing, both from pretrial and sentencing hearing, we have information on the defendant-case ID, judge ID, date, court, region, type of crime, type of procedure, and verdict. Additionally, we use the number of crimes known to police reported monthly by the Center for the Study and Analysis of Crime (CEAD).

Pretrial detention data accounts for our base dataset and includes 432,904 hearings (27,000 a year). For cases with more than one pretrial detention hearings related to the same sentencing case, we keep only the first pretrial detention hearing, as it states the initial decision taken by the judge (which can be then appealed and changed). We also collected information on 1,707,245 cases (107,000 per year) in the sentencing data. We merge both bases using the defendant-case ID. We only keep cases with pretrial detention hearing (dropping those with sentence but not pretrial detention hearing). We also drop error observations such as sentencing date prior to pretrial hearing and missing judge ID (1,671 observations). Moreover, we restrict the sample to cases in which the judge presided over at least 50 cases (827 judges). Therefore, the main dataset, prior to the establishment of the 2016 reform (2005-2016), includes 307,875 defendant-case observations with a pretrial detention hearing and a sentencing trial verdict. For the second part of the paper, we also consider cases after the introduction of the 2016 reform, completing the 420,202 observations (2005-2020).

Table 1 summarizes our main dataset. Column (1) reveals information about cases on which pretrial detention was conceded, column (2) for those who did not go to pretrial detention and column (3) includes all observations. From Panel A we can infer that cases conceding pretrial detention are composed by a higher proportion of robbery-related crimes compared to cases without the precautionary measure, which is compensated by the last group having a higher proportion of other crimes. From Panel B, we observe that cases with pretrial detention tend to be more severe, with a higher proportion of felonies and lower proportion of misdemeanor and infractions. Then, Panel C reveals that cases in pretrial detention are solved faster (6 months) compared to those without pretrial detention (9 months). This might be related to trial procedures: cases with pretrial detention are more likely to attend trial, and in particular, plea bargain trials (that are shorter), compared to those without pretrial detention. Finally, Panel

D describes that pretrial detained cases are more likely to be convicted and less likely to be absolved or offered an alternative exit. Table A1 also includes this information the post-reform period and reports similar trends.

4 Research Design

4.1 Empirical Strategy

To measure the effects of pretrial detention on our three sentencing outcomes, consider a model that relates sentence verdict to an indicator for whether the defendant was ordered be pretrial detained, PD_i:

$$Y_i = \beta_0 + \beta_1 PD_i + \beta_2 X_i + e_i, \tag{1}$$

where Y_i is the outcome of the case for defendant i, X_i is a set of control variables and e_i is an error term. The OLS estimates of equation (1) are likely to be biased given that pretrial detention is correlated to unobserved defendant characteristics that have correlation with the outcomes. In particular, one can imagine that defendants accused for more severe crimes are more likely to be found guilty, thus judges assign pretrial detention. In this case, OLS estimates will be biased toward finding that pretrial detention lowers the likelihood of conviction.

To isolate the effect of pretrial detention, we exploit the quasi-random allocation of judges. The judge-IV design has been widely used by researchers as a credible source of exogenous variation in different settings (Kling, 2006; Aizer and Doyle Jr, 2015; Mueller-Smith, 2015; Bhuller et al., 2020; Dobbie et al., 2018; Dobbie and Yang, 2021a,b; Leslie and Pope, 2017). For example, Di Tella and Schargrodsky (2013) leveraged the quasi-random allocation of judges to measure the recidivism rate of the participants of an electronic monitoring program in Buenos Aires, Argentina. The intuition behind this instrument is that after controlling for court characteristics, the judge allocation is plausibly random among defendants. In a given court, judges make turns and must have no control on what cases they get assign to. Thus, a researcher can exploit the variation of being "treated" (e.g. sent to pretrial detention) by different type of judges. For example, if judges differ by the degree of leniency or stringency, one could observe that a

more lenient judge is less likely to send a defendant to prison relative to a more stringent judge.[17] After showing that the allocation is random or at least balanced along a set of observable characteristics, we can plausibly estimate the causal effect of pretrial detention on sentencing outcomes.

Then, the idea is to *instrument* the pretrial detention assignment by the leniency of the judge allocated to the case. In practice, this leniency index is built using the judge leave-out mean by using all pretrial detention hearings with sentencing trial verdicts. More precisely, we closely follow Grau et al. (2023), for defendant-case i matched with judge j (who works at court c), we estimate the average pretrial detention rate using every other case handled by judge j after adjusting for court-by-year fixed effects. Formally, we first estimate the residual from the following regression:

$$PD_i = \alpha_0 + \alpha_1'(court_c * year_{jc}) + \nu_i \tag{2}$$

Then, we calculate the judge leniency (stringency) score variable, $Z_{j(i)}^{jugde}$, with $j(i)$ as the judge from the pretrial detention hearing for defendant-case i:

$$Z_{j(i)}^{jugde} = \frac{1}{N_j - 1} \sum_{k \neq i}^{N_j - 1} \nu_k' \tag{3}$$

The judge leniency score measures the propensity that a given judge has of sending any given individual to pretrial detention. Given this instrument, we can estimate the causal effect of pretrial detention on judges decisions at sentencing hearings in a 2SLS regression by considering the following two equations:

$$PD_i = \lambda_0 + \lambda_1 Z_{j(i)}^{jugde} + \lambda_2 X_i + \theta_{ct} + \epsilon_i \tag{4}$$

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[17]Other researchers have used a similar idea by exploiting the quasi-random allocation of other actors in the justice systems such as prosecutors of public defendants. A salient case is Doyle Jr (2008), who uses the placement frequency of child protection investigators as an instrument to identify causal effects of foster care placement on adult arrests, convictions, and imprisonment rates in Illinois.

$$Y_i = \beta_0 + \beta_1^{iv} PD_i + \beta_2 X_i + \theta_{ct}^{iv} + \epsilon_i^{iv} \tag{5}$$

Where X_i is a set of controls including prior pretrial detention and prior sentence, and θ_{ct} denotes court-by-year fixed effects. Equation 4 describes the first stage measuring the effect of judge leniency on pretrial detention decisions and Equation 5 shows the second stage, which calculates the effect of pretrial detention on sentencing outcomes. The IV estimation identifies the causal effect of pretrial detention decision for individuals on the margin of being detained before trial, i.e., the compliers, therefore corresponds to the local average treatment effect (LATE).[18]

We will examine whether the results are robust to the instrument specification by displaying several tables in the Appendix section. Also, following Frandsen et al. (2023), we check the validity of the instrument by testing whether it predicts pretrial detention, the random allocation of judges and the monotonicity of the instrument for different sub-samples.

In the second section of results we build on the 2SLS specification to examine whether the impact of pretrial detention changed after the prosecution reform. Of course, isolating the effect of the reform is challenging. However, considering its institutional details one can compare changes across different types of crimes to isolate the specific changes that could be attributed to the reform. For example, we should expect no major changes in crimes whose prosecution rules were not altered, and contrasts those estimates to the changes in robbery-related crimes whose prosecution rules were modified. More specifically, the group of crimes whose prosecution rules were not modified during that period may offer a potential placebo for what we should have observe in the absence of the reform. In particular, we plan to examine the extent to which the prosecution reform altered the effect of pretrial detention by comparing the change on this effect over time and across crime categories. Thus, we will measure the impact of pretrial detention for each type of crime (robberies, theft and other crimes) before and after the 2016 reform, separately. Since this idea resembles a difference-in-difference strategy we will first present the instrumental variable

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[18] Appendix Table A2 shows the share of compliers. Then, following Bald et al. (2022), Appendix Table A3 compares the main sample with the compliers, in terms of their observables' distribution.

difference-in-differences (IV-DiD) using the binary definition of judge leniency as an instrument.[19] In particular, we run the following IV-DiD regression specification:[20]

$$
\begin{aligned}
Y_i = \ &\gamma_0 + \gamma_1 Z_{j(i)} + \gamma_2 Z_{j(i)} \times Robbery_i + \gamma_3 Z_{j(i)} \times Theft_i + \gamma_4 Z_{j(i)} \times Post_i \\
&+\ \gamma_5 Z_{j(i)} \times Robbery_i \times Post_i + \gamma_6 Z_{j(i)} \times Theft_i \times Post_i \\
&+\ \gamma_7 X_i + \theta_{ct}^{iv} + \epsilon_i^{iv}
\end{aligned}
\tag{6}
$$

Where γ_1, γ_2 and γ_3 capture the baseline effect of pretrial detention on sentencing outcomes before the reform, for other crimes, robberies and theft, respectively. γ_4, γ_5 and γ_6 denote the effect of pretrial detention for each type of crime after the reform, and represent the main coefficients of interest in this regression.

4.2 Instrument validity

Figure B2 shows a graphical depiction of the first stage relationship between our residualized measure of judge leniency and the likelihood of being pretrial detained. Based on the specific judge allocated to a case, defendants chances of being pretrial detained varies substantially. The figure includes a local linear regression fit –with the respective 95 percent confidence intervals– for the average residualized rate of pretrial detention for each group of judge-cases. When comparing the extremes of the horizontal axis, the figure suggests the difference in terms of the probability of being pretrial detained between the most and least lenient group of judges is roughly 20 percentage points.[21] We also plot the histogram of the judge leniency measure, which suggests that judges' leniency follows a roughly normal distribution, centered at zero, as expected.

Table 2 shows the main set of results measuring the impact of pretrial detention on sentencing outcomes, including the first stage regression. Consistent with Figure B2, it shows that judge leniency is highly and significantly

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[19]This variable takes the value 1 for judge leniency scores higher than the sample's average and 0 for scores under the leniency mean of the sample. Table A4 shows consistent results using this instrument.

[20]In the appendix section, we also include separate IV regressions by type of crime and period of study. Results are available in Table A8.

[21]We show similar results when using only the period prior to the 2016 reform (Appendix Figure B3) and only the post-reform period (Appendix Figure B4).

predictive of pretrial detention ([Montiel Olea and Pflueger](2013) robust F-Stat for weak instruments of 5,536).[22] This strong relationship validates the use of judge leniency as an instrument given that it confirms to be relevant to predict the endogenous variable.

Judge-IV design has been recently subject to criticism for different reasons ([Frandsen et al., 2023]). For one, judges are not necessarily allocated following a specific random protocol as one performs in a traditional randomized controlled trial. In this case, the *quasi*-random allocation process reside on the characteristics of the job shifts and by virtue of the equity principle which ensure defendants (or their counterparts) play no role on deciding the judge of a case. We examine this issue by performing two standard checks in the literature ([Dobbie et al., 2018]; [Frandsen et al., 2023]), and include the larger set of covariates that were available in the dataset.

First, we evaluate the characteristics of the allocation of judges. Table [A6] displays results for two outcomes. We begin by assessing whether crime severeness increases or not the chances of being pretrial detained in our main sample. As expected, we observe that misdemeanors and felonies are significantly predictive of the likelihood of being pretrial detained (compared to infractions). A similar result is found for individuals with pretrial detention or sentence history. There is no significant coefficient for robberies. This reflects the fact that judges consider the characteristics of the case on their decision and tend to impose pretrial detention on defendants that can pose a more serious risk to society. Then, we run the same specification using judge leniency as the dependent variable. A statistically significant coefficient in this case suggests that the characteristics of the case influence in the assignment of the judge by leniency, disproving the plausible claim that judges were allocated in a *quasi* random fashion. Instead, as opposed to the previous case, type of crime and the history of the case are not significant. Also, the joint significance F-test suggest that the model explains too little variation and that judge allocation can be considered *as good as random*.

We also examine the monotonicity assumption in this setting, which is a necessary condition to interpret the coefficient of these IV regression as representative of the compliers population -the group of individuals that would be treated only if they receive a strict judge but not otherwise. Although, this is essentially an untestable assumption,

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[22]We also estimate our main results using slightly different instrument specifications in Appendix Tables [A4] and [A5].

a potential test consists on examining whether the impact of a judge's appointment on the probability of pretrial detention is *monotonic* across defendants (e.g. it moves in the same direction). In our setting, this assumption requires that individuals released by a lenient judge must also be released by a strict judge and vice versa. A testable implication of this assumption is that the first-stage estimates should be positive for all subsamples. In practice, this consists on splitting the sample by type of crime and crime severity, and comparing the coefficients. In Table A7 we see that in each of the panels, and for the different specifications, the residualized measure of judges' leniency is consistently positive, for all subsamples and both periods.

5 Results

5.1 The impact of pretrial detention on sentencing outcomes

Table 2 displays the impact of pretrial detention on different outcomes indicated on each panel of results: the likelihood that the defendant-case was convicted, absolved, or ended on an alternative exit. The first column shows the OLS coefficients of a regression of pretrial detention on each of the outcomes and can be contrasted with the IV coefficients in column (3). Although OLS coefficients point toward the right direction we can observe the presence of bias likely due to selection. In column (2), we can observe that there is a strong first stage reaffirming the relationship between judge leniency and pretrial detention.

The main takeaway of Table 2 is the strong effect of pretrial detention on sentencing outcomes. Pretrial detention increases conviction probability from 53 to 76 percent. As we discuss later, the increase in conviction is closely related to the increase in the likelihood of the case being resolved by a trial. We examine the robustness of these findings by including different alternatives of the instrument in the Appendix Tables A4 and A5.

There are different ways to asses the magnitude of these estimates. One is to compare the rough numbers with similar estimates found in different context. For example, Dobbie et al. (2018) following a similar strategy and data from Philadelphia and Miami-Dade County find that a marginal released defendant is 16 pp less likely to be found guilty, and 12 pp less likely to plead guilty. Similarly, Leslie and Pope (2017) by using data from New York

city find that being detained increases the probability of conviction by 13 pp for felony defendants. Relative to these estimates our findings from the Chilean system can be located at the higher range. However, we have to be careful when comparing estimates from different contexts. One important distinction is the prosecutor's ability to negotiate with the defendant which is a salient feature of the United States justice system but a very limited tool to Chilean prosecutors. This is a crucial institutional difference since Dobbie et al. (2018) highlight that their findings "are consistent with the theory that pretrial release improves a defendant's bargaining position in plea negotiations" (Dobbie et al., 2018, p.20).

Considering this important institutional distinction, Chilean estimates can be seen as large. Given the magnitude of these results appears to be puzzling, we exploit the establishment of a law that increased the prosecution capacities for robbery-related crimes.

5.2 The impact of the 2016 reform

Estimates of Table 2 suggest a strong impact of pretrial detention. In this section, we examine the change in the effect of pretrial detention on sentencing outcomes induced by the 2016 reform, which mainly modifies the possibility to plea bargaining during the process as described in the institutional background (Section 2).

Table 3 reports the estimates from the IV-DiD model described in Equation 6. We report the triple interacted terms that aim to capture a differential impact of pretrial detention for robbery- and theft-related crimes during the post-reform period, relative to a change in the baseline crime category (other-crimes). The coefficients suggest that there is no increase in the effect of pretrial detention for thefts after the passage of the 2016 reform. In the case of robberies, the crimes that were supposed to be affected by the law, the effect of pretrial detention on the likelihood of conviction for robberies increased by 29 pp after the reform.

To better understand the magnitude of the estimates in Table 3, we include Table A8, which shows separate IV regressions, for each period and type of crime. We estimate the impact of pretrial detention on final conviction by type of crime and period of study. For each type of crime and period we report the *naive* OLS model, the first stage and the IV coefficient. Panel A shows the 2SLS procedure for robbery-related crimes, Panels B and C

display similar results for theft-related and other crimes, respectively. Before the promulgation of the 2016 reform, a pretrial detention increased the chance of conviction by 17 pp, for robberies. After the reform, the impact of pretrial detention on the likelihood of conviction is 36 pp. Meanwhile, for both theft and other crimes, the impact of pretrial detention on conviction remained remarkably similar across periods: it changed from 26 to 28 pp for theft and from 20 to 19 pp for other crimes.

To complement the IV results presented above, we examine the evolution of conviction likelihood by crime type and date of pretrial detention hearing. Consistent with the findings in Table 3, Figure 1 shows a sharp increase in the monthly average conviction rate for robberies following the 2016 reform. Each figure displays the monthly average evolution of conviction rate for a set of crime categories, based on how likely they were impacted by the 2016 reform. It also includes a predicted trend of conviction rate calibrated with pre-reform information of judges and types of crimes. Based on those coefficients –calibrated during the pre-reform period– we project the expected monthly average trend of conviction rates before and after the 2016 reform period. We exclude from this analysis cases supervised by judges with no observed cases during the pre-reform period. As expected, the trend fits very well the evolution of conviction rate over time during the pre-2016 reform period. For the post-2016 reform period, the projected trend can be interpreted as a simple counterfactual of what would have been the evolution of the monthly average probability of conviction, holding pre-reform regression coefficient estimates for crime and judges constant. We observe that there is a sharp increase in the likelihood of conviction that only took place in robbery-types of crimes and no similar reactions can be detected for other crimes that were not affected by the reform.[23] This suggests that the post-reform increase in conviction rates can hardly be attributed to the characteristics of judges or a change in the volume or composition of cases, but rather to the new set of rules in place.

Consistently, Figure B5 shows the evolution of the likelihood of attending trial across the three types of crime categories. We can observe that there is a sharp discontinuity for robberies after the passage of the reform. Given the low probability of absolution in the Chilean judicial system, that change is closely related to an abrupt

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[23]For the purposes of visualization, we exclude from the predicted lines observations between Jan 1, 2016 and July 5, 2016 when the reform was officially in place. Criminal cases that entered the system during this period are likely to be treated by the change in prosecution rules –which applies *ipso facto*– if they were not finished on trial before July, 2016. Conversely, changes in expected sanctions are related to the time the crime took place (e.g. before or after the reform).

increase in the probability of conviction we observed in Figure 1.[24] To get a sense of the magnitude of the change, a simple regression discontinuity design suggests that the likelihood of attending trial increased by 20 pp in July 2016, when the new law was in place.

Collectively, these sets of results confirm a large change in the impact of pretrial detention on the likelihood of conviction for robbery-related cases, and no major changes for theft and other crimes, implying that the 2016 reform substantially altered the consequences of this institutional feature for robberies.

6   Discussion

To better assess the implications of the 2016 reform, we analyze the evolution of several features of the criminal system over time. First, we examine variation at upstream stages of the criminal justice system, focusing on observable factors that may influence how pretrial detention affects sentencing outcomes, including caseload volume and crime type composition. Then, by exploiting several characteristics of our dataset, we dig into key aspects of the reform and whether they translated into effective changes or not. A key aspect of this analysis is comparing robbery-related cases, where the reform's primary effects should be observed, with other criminal cases, where we would not expect to see substantial changes.

6.1   Upstream stages

Of course, one cannot simply interpret pre *vs.* post comparisons as a result of a single reform. If the 2016 reform modified the number of cases or induced some type of selection on pretrial detention cases, then we could hardly attribute the observed differences over time to the reform. To address this concern, we examine whether the evolution in the number or the composition of criminal cases changed around the time of promulgation. Observing no major variation in this area would suggest that the observed change during the 2016 reform can be attributed to the characteristics of the new regime. Among other key features, we examine the use of the pretrial detention measure as well as judges' leniency towards certain crimes.

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[24]See Figure B6 in the Appendix.

We first examine the evolution of the number of reported crimes, by type of crime. Figure B7 displays time series with the number of crimes reported to the police. Police reports provide the principal input for criminal investigations, which may then be included in our primary dataset. Relative to thefts and other-crime, robbery represents a smaller share of total crimes. For each time series, the volume of incidents remains constant over time, even around the time that the 2016 reform passed.

Figure B8 describes a similar picture. It shows the evolution of pretrial hearings by crime category, and aims to examine potential changes in the number of crimes that enters our dataset. There are several reasons why the magnitude of cases is substantially smaller than the number of crimes reported in figure B7. While figure B7 shows monthly crimes reported to the police, figure B8 displays monthly cases where prosecutors found grounds for investigation, a defendant was formally charged, and the prosecutor requested pretrial detention. Still, in both cases, robbery represents a smaller share of cases.

In Figure B8, we observe no important variation around the 2016 reform's passage across any of the three crime categories. Moreover, the composition of crimes in pretrial hearings appears unchanged after the reform. This reinforces the conclusion that neither the total number nor the composition of crimes shifted around the time of the 2016 reform.

Additionally, Figure B9 shows no significant variation in terms of the outcomes of pretrial hearings (e.g. whether the judge accepts or rejects the request made by the prosecutor). The average proportion of pretrial detention remains remarkably stable over time. Before the reform, the likelihood of accepting pretrial detention was 88.2%, 90.4% and 89.9% for other crimes, theft and robberies, respectively. After the reform, the proportions were 87.3%, 89.8% and 90.2%, respectively.

But pretrial detention hearing outcomes can be affected by a set of characteristics. For instance, due to a change in judges' willingness to pretrial detain defendants or by a change in the composition of crimes. We examine this issue by comparing the average evolution of judge's behavior to accept pretrial detention requests. Based on the first stage of our main IV estimation, Figure B10 compares the monthly evolution of the average judicial leniency

estimates calculated across cases. Again, we observe no significant change around the passage of the 2016 reform. As expected, overall leniency is centered around zero during the entire period, and the leniency measure does not change more than 0.01 for any type of crime. Before the reform, the leniency measure was 0.0003, -0.003 and -0.0002 for others, theft and robberies, respectively, and after the reform, the values of leniency are -0.003, 0.002 and 0.0005, respectively.

In sum, this set of results strongly suggest that neither upstream crime activity nor other concurrent factors that could affect the comparison of cases around the reform's passage changed fundamentally during that period. Consequently, these factors can hardly explain the magnitude of the change in how pretrial detention affects robbery conviction rates.

6.2 New prosecution tools: increasing plea bargaining

The main motivation of the 2016 reform was to improve the criminal prosecution of robbery-related crimes. It established two main modifications: increasing the expected sanctions and expanding the bargaining power of the prosecutors. In this section we focus on the second component. As described in section 2, prosecutors can bring cases to trial through one of three procedures: plea bargaining, simplified trial, or ordinary trial. The 2016 reform introduced an extraordinary incentive for the defendant, in robbery cases, to agree to waive their right to an ordinary trial and opt for a plea bargain or accept responsibility in a simplified procedure (Riego, 2017).

Figure B11 exhibits the evolution of the number of criminal cases according to the type of procedure followed, for each type of crime. The figure reveals a marked increase in the use of plea bargaining for resolving robbery-related offenses following the 2016 reform. In contrast, the use of simplified and ordinary trial procedures for these crimes remains stable over the same period, which suggests that the increase in plea bargaining was driven by a reduction in alternative exits. On the other hand, in the case of theft and other crimes, the distribution of procedural types appears unchanged, indicating that the reform's impact was concentrated primarily on robbery cases. This trend suggests that the new prosecutorial tools introduced by the reform facilitated and incentivized the use of plea bargains in these cases.

6.3 How pretrial detention and the 2016 reform impacted trial procedures

Finally, to better understand the mechanisms underlying our findings, we explore how specific aspects of the criminal process were altered by the 2016 reform. The increased effect of pretrial detention on conviction rates could stem from changes in absolution patterns, alternative case resolutions, or other procedural pathways. Through a series of targeted analyses, we investigate the channels through which this reform shaped case outcomes.

In Table 2, we observe that the increasing effect of pretrial detention on the likelihood of conviction coincides with a decrease in the likelihood to get an alternative exit. This relationship is almost mechanical since in our sample, 97% of the cases going to trial end up with a conviction. Table A9 in the Appendix presents estimates of the effect of pretrial detention on the likelihood of going to trial (instead of conviction), revealing results that are nearly identical to those reported in Appendix Table A8. Thus, the effects on conviction are strongly driven by the likelihood of going to trial. The reverse is true for alternative exit: Appendix Table A10 replicates Table 3 for absolved and alternative exit, confirming that the increase of the effect of pretrial detention on conviction coincides with a decrease on alternative exit.

The 2016 reform opened new possibilities for plea bargaining in robbery-related cases. This could have affected cases that were previously ending as alternative exists, but also those that ended up in simplified or ordinary trials. To understand how the reform affected trial procedures, we examine the evolution of the effect of pretrial detention on these different types of trial procedures following the 2016 reform.

Table 4 shows how pretrial detention affects the likelihood of different procedures for robbery cases, based on the available options: Plea bargain, Simplified, Ordinary and Alternative exit. The first three correspond to forms of trial, while the latter reflects avoiding trial altogether. Before the reform, pretrial detention increased the probability of a plea bargain by 16 percentage points and decreased the likelihood of an alternative exit by 17 pp, with no significant effect on simplified (-2 pp) and ordinary trials (3 pp). After the 2016 reform, pretrial detention increased the likelihood of resolving the case through a plea bargain (24 pp) and simplified trial (11.5 pp), while reducing the probability of an alternative exit by 36 pp.[25] This pattern is consistent with prosecutors gaining greater

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[25] Tables Appendix Tables A11 and A12 show the same estimates for thefts and other crimes, respectively. For those crimes, we observe

bargaining power to steer cases away from alternative exits and into trial proceedings.

Overall, the 2016 reform provided the prosecutors with greater attributions to solve robbery cases through plea bargaining. Prior to the reform, pretrial detention increased the likelihood of conviction, largely due to a higher probability of attending trial. This effect increased after the reform, but only for robbery offenses. Notably, during the same period, a similar increase is observed in the effect of pretrial detention on the likelihood of resolving a case via plea bargaining. These findings suggest that the 2016 reform amplified the impact of pretrial detention on conviction by providing prosecutors an alternative to negotiate with defendants.

7   Conclusion

This paper examines the effect of pretrial detention on criminal case outcomes over the last decade in Chile. We find that pretrial detention significantly increases the probability of conviction. The magnitude of this effect is substantial and exceeds comparable findings from recent U.S. studies. This result is particularly salient given Chile's restricted plea bargaining system.

We further explore how prosecutorial tools interact with pretrial detention's impact by exploiting specific features of a reform that expanded prosecutors' negotiating ability. Given that the reform only altered the prosecutorial incentives in robbery-related crimes, we were able to isolate its effect by comparing similar trajectories in other crime categories. We found that the reform substantially amplified pretrial detention's effect on conviction likelihood, suggesting that institutional design shapes these outcomes.

Our findings underscore that pretrial detention, while used extensively, carries profound consequences for defendants —a critical concern given its intended function as a precautionary measure rather than a mechanism for distributing justice. The 2016 reform, though potentially beneficial in facilitating plea negotiations and reducing trial burdens, may have inadvertently amplified pretrial detention's coercive power, transforming what should remain a neutral procedural tool into a more influential determinant of case outcomes. Future research should examine

minimal changes in the composition of procedures, suggesting that this shift is specific to robberies.

how criminal justice reforms can enhance system effectiveness while safeguarding against unintended consequences that compromise fairness and undermine public trust. The challenge lies in designing institutional frameworks that serve justice without allowing precautionary tools to become instruments that predetermine outcomes.

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Tables

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Table 1: Descriptive Statistics

Pretrial Detention Decision	(1) Conceded	(2) Rejected	(3) All
Panel A: Type of Crime			
Robbery	0.226	0.192	0.223
Theft	0.296	0.265	0.293
Other	0.478	0.543	0.485
Panel B: Severity of Crime			
Felony	0.588	0.477	0.576
Misdemeanor	0.388	0.480	0.398
Infraction	0.024	0.043	0.026
Panel C: Trial Procedures			
Days to Case Decision	178.04	276.78	187.15
Trial	0.549	0.463	0.540
Plea bargain	0.329	0.202	0.315
Simplified	0.192	0.238	0.197
Ordinary	0.026	0.021	0.026
Panel D: Outcomes			
Conviction	0.539	0.448	0.529
Absolved	0.011	0.015	0.011
Alternative exit	0.451	0.537	0.460
Observations	274,725	33,150	307,875

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Note: The table reports descriptive statistics for the main database described in section 3 prior to the 2016 reform (January 2005 to July 2016).

Table 2: The impact of pretrial detention on sentencing outcomes

	(1) OLS	(2) First Stage	(3) IV
Panel A: Convicted			
Pretrial Detention	0.1038***		0.2254***
	(0.0055)		(0.0370)
Judge Leniency		0.9815***	
		(0.0132)	
R^2	0.024	0.076	0.019
Mean DV	0.53	0.89	0.53
Panel B: Absolve			
Pretrial Detention	-0.0053***		-0.0234***
	(0.0010)		(0.0065)
Judge Leniency		0.9815***	
		(0.0132)	
R^2	0.020	0.076	0.017
Mean DV	0.01	0.89	0.01
Panel C: Alternative exit			
Pretrial Detention	-0.0985***		-0.2019***
	(0.0055)		(0.0370)
Judge Leniency		0.9815***	
		(0.0132)	
R^2	0.023	0.076	0.019
Mean DV	0.46	0.89	0.46
Observations	307,875	307,875	307,875
F-stat		5536.19	

Note: The table reports estimates for pretrial detention on sentencing outcomes. Each panel shows results for separate set of regressions, modifying the dependent variable. Panel A, B and C report coefficients for the likelihood of conviction, absolution and alternative exit, respectively. Columns (1), (2) and (3) show the effect of pretrial detention in different outcomes (OLS as in Equation 1), the effect of judge leniency in pretrial detention (IV First Stage as in Equation 4) and the effect of judge leniency in different outcomes (IV Second Stage as in Equation 5) respectively. All coefficients are estimated using data for the pre-reform period and include court-by-year fixed effects. Standard errors clustered at judge level in parentheses. Montiel Olea and Pflueger (2013) robust F-stat is reported. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: IV-DiD coefficients on the probability of conviction

	(1) Convicted	(2) Convicted	(3) Convicted
Pretrial Detention × Theft × Post	0.0149	0.0112	0.0132
	(0.0096)	(0.0089)	(0.0089)
Pretrial Detention × Robbery × Post	0.2787***	0.2867***	0.2960***
	(0.0119)	(0.0110)	(0.0110)
Observations	420,202	420,202	417,935
R^2	0.030	0.055	0.039
Mean DV	0.53	0.53	0.53
Court-by-year FE	No	Yes	Yes
Crime FE	No	No	Yes

Note: The table shows the effect of pretrial detention on the likelihood of conviction using an IV difference-in-differences approach as described in Equation 6. Estimates can be interpreted as relative to the baseline category "Other crimes" during the pre-reform period. Column (1) has no controls, column (2) has court-by-year fixed effects and column (3) adds court-by-year and crime fixed effects. Standard errors clustered at judge level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Effects of the 2016 reform: different case procedures for Robbery

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A		Pre 2016 reform			Post 2016 reform	
Plea bargain	OLS	First Stage	IV	OLS	First Stage	IV
Pretrial Detention	0.1173***		0.1596***	0.2418***		0.2431***
	(0.0079)		(0.0514)	(0.0141)		(0.0853)
Mean DV	0.28	0.91	0.28	0.49	0.90	0.49
Panel B: Simplified		Pre 2016 reform			Post 2016 reform	
Pretrial Detention	-0.0372***		-0.0246	-0.0103		0.1153***
	(0.0066)		(0.0428)	(0.0074)		(0.0434)
Mean DV	0.08	0.91	0.08	0.06	0.90	0.06
Panel C: Ordinary		Pre 2016 reform			Post 2016 reform	
Pretrial Detention	0.0003		0.0317**	0.0011		-0.0016
	(0.0021)		(0.0154)	(0.0036)		(0.0227)
Mean DV	0.02	0.91	0.02	0.02	0.90	0.02
Panel D: Alternative exit		Pre 2016 reform			Post 2016 reform	
Pretrial Detention	-0.0811***		-0.1662***	-0.2330***		-0.3564***
	(0.0092)		(0.0638)	(0.0150)		(0.0830)
Mean DV	0.62	0.91	0.62	0.43	0.90	0.43
Judge Leniency		0.8217***			0.8271***	
		(0.0337)			(0.0571)	
Observations	68,558	68,558	68,558	22,779	22,779	22,782
F-stat		593.55			209.60	

Note: The table reports estimates for pretrial detention on different case procedures for robberies. Each panel shows results for separate set of regressions, modifying the dependent variable. Panel A, B, C show results for different trial procedures: plea bargain, simplified, ordinary, respectively, and Panel D for Alternative exit. Columns (1), (2) and (3) show the effect of pretrial detention on each procedure (OLS as in Equation 1), the effect of judge leniency on pretrial detention (IV First Stage as in Equation 4) and the effect of judge leniency on each procedure (IV Second Stage as in Equation 5), respectively, before the 2016 reform. Columns (4), (5) and (6) show the same effects post-reform. All coefficients include court-by-year fixed effects. Standard errors clustered at judge level in parentheses. Montiel Olea and Pflueger (2013) robust F-stat is reported. *** p<0.01, ** p<0.05, * p<0.1.

Figures

Figure 1: Evolution of Likelihood of Conviction, including prediction lines
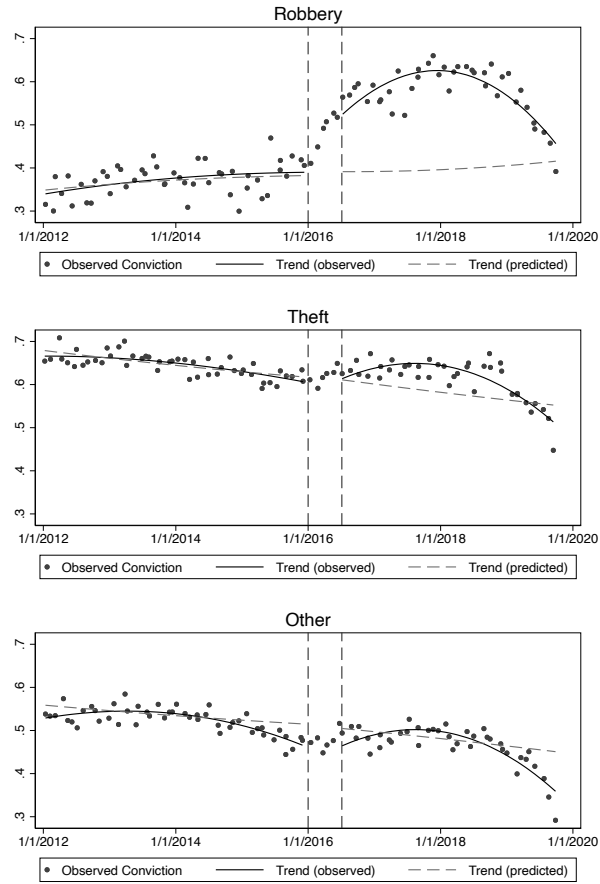
Note: This figure reports the monthly average likelihood of conviction by type of crime (robbery, theft and other crimes) for the period January 2012 - September 2019. The horizontal axis aggregates incidents monthly, using the date of pretrial detention hearing. The black lines correspond to the fractional-polynomial plots of this likelihood. Dashed lines include the predicted fractional-polynomial plots, based on the likelihood of conviction prior to the implementation of the 2016 reform, controlling by crime, month and judge fixed effects. We exclude from the predicted lines observations between January 1, 2016 and July 5, 2016 when the reform was officially in place –indicated by the respective vertical lines. Criminal cases that entered the system during this period are likely to be treated by the change in prosecution rules –which applies *ipso facto*– if they were not finished on trial before July, 2016.

Appendix

A   Tables

Table A1: Descriptive Statistics

Pretrial Detention Decision	Before 2016 reform			After 2016 reform		
	(1) Conceded	(2) Rejected	(3) All	(4) Conceded	(5) Rejected	(6) All
Panel A: Type of Crime						
Robbery	0.226	0.192	0.223	0.206	0.175	0.203
Theft	0.296	0.265	0.293	0.329	0.298	0.326
Other	0.478	0.543	0.485	0.465	0.527	0.472
Panel B: Severity of Crime						
Felony	0.588	0.477	0.576	0.551	0.477	0.542
Misdemeanor	0.388	0.480	0.398	0.428	0.493	0.435
Infraction	0.024	0.043	0.026	0.021	0.030	0.022
Panel C: Trial Procedures						
Days to Case Decision	178.04	276.78	187.15	173.34	235.24	178.35
Trial	0.549	0.463	0.540	0.582	0.399	0.561
Plea bargain	0.329	0.202	0.315	0.360	0.193	0.341
Simplified	0.192	0.238	0.197	0.198	0.182	0.196
Ordinary	0.026	0.021	0.026	0.023	0.023	0.023
Panel D: Outcomes						
Conviction	0.539	0.448	0.529	0.550	0.373	0.530
Absolved	0.011	0.015	0.011	0.032	0.025	0.031
Alternative exit	0.451	0.537	0.460	0.418	0.601	0.439
Observations	274,725	33,150	307,875	99,511	12,816	112,327

Note: The table reports descriptive statistics for the main database described in section 3 prior and after the introduction of the 2016 reform (January 2005 to July 2016 and July 2016 to October 2019, respectively).

Table A2: Sample Share by Compliance Type

	(1)	(2)	(3)
Leniency Cutoff	1%	2%	3%
Compliers (π_c)	0.28	0.24	0.22
Never Takers (π_n)	0.61	0.65	0.67
Always Takers (π_a)	0.11	0.11	0.11

Note: The table reports the share of compliers, never takers and always takers of the sample for different leniency cutoffs. We define compliers as defendants whose pretrial decision would have been different had their case been assigned to the most lenient ($\underline{z}$ instead of the most stringent judge ($\bar{z}$). Never takers are those who would never be detained before trial regardless of the judge assigned. Finally, always takers those who would always be detained before trial despite of the judge:

$\pi_c = P(Detention_i = 1 | Z_i = \bar{z}) - P(Detention_i = 1 | Z_i = \underline{z}) = P(Detention_i(\bar{z}) > Detention_i(\underline{z}))$

$\pi_a = P(Detention_i = 1 | Z_i = \bar{z}) = P(Detention_i(\bar{z}) = Detention_i(\underline{z}) = 1)$

$\pi_n = P(Detention_i = 0 | Z_i = \bar{z}) = P(Detention_i(\bar{z}) = Detention_i(\underline{z}) = 0)$

Table A3: Characteristics of Compliers

	(1) $P[X=x]$	(2) $P[X=x\|complier]$	(3) $\frac{P[X=x\|complier]}{P[X=x]}$
Robbery	0.223	0.190	0.855
Theft	0.293	0.277	0.947
Other	0.485	0.533	1.101
Felony	0.576	0.496	0.862
Misdemeanor	0.398	0.467	1.175
Infraction	0.026	0.039	1.477
Trial	0.540	0.506	0.938
Plea bargain	0.315	0.232	0.736
Simplified	0.197	0.245	1.246
Ordinary	0.026	0.024	0.940
Conviction	0.529	0.494	0.933
Absolved	0.011	0.012	1.042
Alternative exit	0.460	0.487	1.059

Note: The table presents the sample distribution, complier distribution, and relative likelihood for different subgroups prior the introduction of the 2016 reform (January 2005 to July 2016).

Table A4: The impact of pretrial detention on sentencing outcomes using alternative instrument construction

	(1) Judge	(2) Year	(3) Judge$_{100}$	(4) Raw	(5) Raw$_{100}$	(6) $\mathbf{1}_{Z \geq \text{Mean}}$	(7) $\mathbf{1}_{Z \geq \text{Median}}$
Conviction	0.2254***	0.1678***	0.2290***	0.2085***	0.2080***	0.2851***	0.2932***
	(0.0370)	(0.0258)	(0.0378)	(0.0378)	(0.0386)	(0.0545)	(0.0536)
Absolved	-0.0234***	-0.0146***	-0.0234***	-0.0220***	-0.0227***	-0.0238**	-0.0104
	(0.0065)	(0.0048)	(0.0067)	(0.0071)	(0.0072)	(0.0096)	(0.0098)
Alternative exit	-0.2019***	-0.1532***	-0.2055***	-0.1865***	-0.1853***	-0.2613***	-0.2828***
	(0.0370)	(0.0258)	(0.0377)	(0.0374)	(0.0382)	(0.0552)	(0.0553)
First Stage	0.9815***	0.8465***	0.9859***	0.9237***	0.9293***	0.0980***	0.1017***
	(0.0132)	(0.0088)	(0.0134)	(0.0149)	(0.0151)	(0.0055)	(0.0064)
F-stat	5,536.19	9,172.85	5,389.96	3,819.38	3,798.65	322.70	249.60
Court-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	307,875	307,747	299,315	297,768	291,218	307,875	307,875

Note: The table reports robustness checks of our two-stage least squares results (as in Equation 5) using alternative instruments for our three dependent variables: conviction, absolved and alternative exit. We also include the First Stage estimate (Equation 4) for each instrument. Column (1) uses the main instrument allowing variation at the judge level. Column (2) also allows for variation at the year level. Column (3) builds the main instrument allowing variation at the judge level, but restricting the sample to judges with at least 100 cases. Column (4) uses the raw instrument, which is build without residualizing and therefore consists of the average likelihood of conceding pretrial detention for each judge. Column (5) uses the raw instrument, but restricting to judges with at least 100 cases. Column (6) shows the binary definition with respect to the mean of the sample's judge leniency score and column (7) with respect to the median. All coefficients are estimated using data for the pre-reform period and include court-by-year fixed effects. Standard errors clustered at judge level in parentheses. Montiel Olea and Pflueger (2013) robust F-stat is reported. *** p<0.01, ** p<0.05, * p<0.1.

Table A5: The impact of pretrial detention on sentencing outcomes using consistent judges

		Leniency			Pretrial Detention		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Main	Mean	10%	20%	Mean	10%	20%
Conviction	0.2254***	0.2271***	0.2252***	0.2253***	0.2638***	0.2653***	0.2298***
	(0.0370)	(0.0428)	(0.0425)	(0.0424)	(0.0708)	(0.0489)	(0.0462)
Absolved	-0.0234***	-0.0236***	-0.0238***	-0.0237***	-0.0142	-0.0244***	-0.0193**
	(0.0065)	(0.0078)	(0.0077)	(0.0077)	(0.0114)	(0.0090)	(0.0083)
Alternative exit	-0.2019***	-0.2034***	-0.2014***	-0.2016***	-0.2496***	-0.2408***	-0.2105***
	(0.0370)	(0.0424)	(0.0420)	(0.0420)	(0.0692)	(0.0494)	(0.0460)
First Stage	0.9815***	0.9916***	0.9902***	0.9898***	1.0651***	1.0000***	1.0013***
	(0.0132)	(0.0164)	(0.0163)	(0.0162)	(0.0258)	(0.0129)	(0.0138)
F-stat	5,536.19	3,673.05	3,704.67	3,715.66	1,710.12	6,025.42	5,239.74
Court-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	307,875	248,348	246,360	246,762	43,706	211,888	241,867

Note: The table reports robustness checks of our two-stage least squares results (as in Equation 5) using alternative instruments for three dependent variables: conviction, absolved and alternative exit. We also include the First Stage estimate (Equation 4) for each instrument. Column (1) uses the main instrument allowing variation at the judge level. Column (2) only uses judges whose average leniency changes less than the average leniency of all judges before and after the 2016 reform. Columns (3) and (4) only use judges whose average leniency changes less than 10% and 20%, respectively, after the introduction of the 2016 reform. Columns (5), (6) and (7) replicate the restrictions of columns (2), (3) and (4) but with respect to judges' likelihood of conceding pretrial detention instead of the leniency. All coefficients are estimated using data for the pre-reform period and include court-by-year fixed effects. Standard errors clustered at judge level in parentheses. Montiel Olea and Pflueger (2013) robust F-stat is reported. *** p<0.01, ** p<0.05, * p<0.1.

Table A6: Test of Randomization

	(1)	(2)	(3)	(4)
	Pretrial Detention		Judge Leniency	
Felony	0.0903***	0.0911***	0.0006	0.0007
	(0.0082)	(0.0082)	(0.0014)	(0.0014)
Misdemeanor	0.0486***	0.0491***	0.0004	0.0005
	(0.0071)	(0.0071)	(0.0013)	(0.0013)
Robbery	0.0033	0.0022	-0.0015*	-0.0016*
	(0.0028)	(0.0028)	(0.0009)	(0.0009)
Prior Pretrial Detention	0.1513***	0.1522***	0.0084	0.0084
	(0.0145)	(0.0158)	(0.0162)	(0.0162)
Prior Sentence	0.0198*	0.0200*	-0.0006	-0.0005
	(0.0111)	(0.0111)	(0.0037)	(0.0037)
Joint significance F-stat	55.2780	53.8201	0.8719	1.0034
Joint significance p-value	0.0000	0.0000	0.4994	0.4146
Mean DV	0.8923	0.8923	0.0005	0.0005
Observations	307,875	307,875	307,875	307,875
Court-by-year FE	Yes	Yes	Yes	Yes
Month FE	No	Yes	No	Yes
Day of the week FE	No	Yes	No	Yes

Note: The table presents the results that justify the random assignment of cases to judges prior to the introduction of the 2016 reform. Column (1) reports estimates from an OLS regression of pretrial detention on the variables listed and court-by-year fixed effects. Column (3) presents estimates from an OLS regression of judge leniency on the variables listed and court-by-year fixed effects. Columns (2) and (4) add month and day-of-the-week fixed effects. The F-stat is reported at the bottom showing joint significance of the variables listed in columns (1) and (2) but not for columns (3) and (4). If the type of crime is more serious, the propensity to be sent to pretrial detention increases, while these characteristics have no impact on the judge's leniency. Standard errors clustered at the judge level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

	Full sample	Crime type			Crime Severity		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	Robbery	Theft	Other	Felony	Misdemeanor	Infraction
Panel A: Before 2016 reform							
Judge Leniency	0.9815***	0.8217***	0.9336***	1.0868***	0.8416***	1.1654***	1.4810***
	(0.0132)	(0.0337)	(0.0285)	(0.0239)	(0.0208)	(0.0273)	(0.1024)
Mean Pretrial Detention	0.8923	0.9074	0.9024	0.8793	0.9109	0.8699	0.8238
Observations	307,875	68,558	90,118	149,199	177,371	122,440	8,038
Panel B: After 2016 reform							
Judge Leniency	0.9745***	0.8271***	0.8849***	1.0912***	0.9075***	1.0547***	1.2320***
	(0.0305)	(0.0571)	(0.0468)	(0.0431)	(0.0375)	(0.0470)	(0.1693)
Mean Pretrial Detention	0.8859	0.9014	0.8957	0.8725	0.8997	0.8709	0.8452
Observations	112,326	22,779	36,561	52,981	60,913	48,881	2,506

Note: The table reports estimates of the OLS regression of pretrial detention status on judge leniency as described in Equation 4. Column (1) shows the results for the full sample. Columns (2), (3) and (4) correspond to the estimates by type of crime: robbery, theft and other crimes respectively, and columns (5), (6) and (7) estimate by severity of crime: felony, misdemeanor and infraction respectively. Panels A and B report results prior and posterior to the introduction of the 2016 reform, respectively. Under a lenient or strict judge the result on pretrial detention does not change. Standard errors clustered at the judge level with court-by-year fixed effects. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A8: Effects of the 2016 reform: Conviction

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A	Pre 2016 reform			Post 2016 reform		
Robbery	OLS	First Stage	IV	OLS	First Stage	IV
Pretrial Detention	0.0830***		0.1654***	0.2336***		0.3570***
	(0.0092)		(0.0634)	(0.0152)		(0.0833)
Judge Leniency		0.8217***			0.8271***	
		(0.0337)			(0.0571)	
Observations	68,558	68,558	68,558	22,779	22,779	22,782
R^2	0.053	0.070	0.050	0.060	0.062	0.055
F-stat		593.55			209.60	
Mean DV	0.37	0.91	0.37	0.57	0.90	0.57
Panel B	Pre 2016 reform			Post 2016 reform		
Theft	OLS	First Stage	IV	OLS	First Stage	IV
Pretrial Detention	0.1075***		0.2648***	0.1821***		0.2800***
	(0.0079)		(0.0560)	(0.0118)		(0.0644)
Judge Leniency		0.9336***			0.8849***	
		(0.0285)			(0.0468)	
Observations	90,118	90,118	90,118	36,561	36,561	36,563
R^2	0.032	0.083	0.023	0.058	0.071	0.054
F-stat		1,070.30			358.13	
Mean DV	0.65	0.90	0.65	0.61	0.90	0.61
Panel C	Pre 2016 reform			Post 2016 reform		
Other	OLS	First Stage	IV	OLS	First Stage	IV
Pretrial Detention	0.1209***		0.1957***	0.1211***		0.1854***
	(0.0066)		(0.0322)	(0.0099)		(0.0580)
Judge Leniency		1.0868***			1.0912***	
		(0.0239)			(0.0431)	
Observations	149,199	149,199	149,199	52,981	52,981	52,982
R^2	0.037	0.085	0.034	0.048	0.086	0.046
F-stat		2,074.58			642.12	
Mean DV	0.52	0.88	0.52	0.46	0.87	0.46

Note: The table reports estimates for pretrial detention on the likelihood of conviction. Each panel shows results for separate set of regressions, modifying the subsample by type of crime. Panel A, B and C report coefficients for robbery, theft and other crimes respectively. Columns (1), (2) and (3) show the effect of pretrial detention in the likelihood of conviction (OLS as in Equation 1), the effect of judge leniency in pretrial detention (IV First Stage as in Equation 4) and the effect of judge leniency in the likelihood of conviction (IV Second Stage as in Equation 5) respectively, prior to the introduction of the 2016 reform. Columns (4),(5) and (6) report the same respective effects post-reform. All coefficients include court-by-year fixed effects. Standard errors clustered at judge level in parentheses. Montiel Olea and Pflueger (2013) robust F-stat is reported. *** p<0.01, ** p<0.05, * p<0.1.

Table A9: Effects of the 2016 reform: Attending Trial

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A		Pre 2016 reform			Post 2016 reform	
Robbery	OLS	First Stage	IV	OLS	First Stage	IV
Pretrial Detention	0.0852***		0.1625***	0.2318***		0.3352***
	(0.0089)		(0.0479)	(0.0137)		(0.0753)
Judge Leniency		0.8267***			0.8443***	
		(0.0309)			(0.0528)	
Observations	68,557	68,557	68,558	22,779	22,779	22,782
R^2	0.054	0.071	0.052	0.071	0.065	0.068
F-stat		713.79			256.06	
Mean DV	0.38	0.91	0.38	0.57	0.90	0.57
Panel B		Pre 2016 reform			Post 2016 reform	
Theft	OLS	First Stage	IV	OLS	First Stage	IV
Pretrial Detention	0.1238***		0.2408***	0.2032***		0.2484***
	(0.0069)		(0.0351)	(0.0109)		(0.0500)
Judge Leniency		0.9344***			0.8779***	
		(0.0269)			(0.0412)	
Observations	90,118	90,118	90,118	36,560	36,560	36,563
R^2	0.075	0.088	0.070	0.070	0.073	0.069
F-stat		1,209.81			454.96	
Mean DV	0.67	0.90	0.67	0.65	0.90	0.65
Panel C		Pre 2016 reform			Post 2016 reform	
Other	OLS	First Stage	IV	OLS	First Stage	IV
Pretrial Detention	0.1486***		0.1655***	0.1481***		0.1676***
	(0.0051)		(0.0241)	(0.0084)		(0.0401)
Judge Leniency		1.0719***			1.1005***	
		(0.0247)			(0.0375)	
Observations	147,516	147,516	147,521	52,374	52,374	52,391
R^2	0.106	0.109	0.106	0.139	0.113	0.139
F-stat		1,877.30			859.15	
Mean DV	0.54	0.88	0.54	0.50	0.87	0.50

Note: The table reports estimates for pretrial detention on the likelihood of attending trial. Each panel shows results for separate set of regressions, modifying the subsample by type of crime. Panel A, B and C report coefficients for robbery, theft and other crimes respectively. Columns (1), (2) and (3) show the effect of pretrial detention in the likelihood of attending trial (OLS as in Equation 1), the effect of judge leniency in pretrial detention (IV First Stage as in Equation 4) and the effect of judge leniency in the likelihood of attending trial (IV Second Stage as in Equation 5) respectively, before the reform. Columns (4),(5) and (6) report the same respective effects post-reform. All coefficients include court-by-year fixed effects. Standard errors clustered at judge level in parentheses. Montiel Olea and Pflueger (2013) robust F-stat is reported. *** p<0.01, ** p<0.05, * p<0.1.

Table A10: IV-DiD coefficients on the probability of absolved and alternative exit

	(1) Absolved	(2) Absolved	(3) Absolved	(4) Alternative Exit	(5) Alternative Exit	(6) Alternative Exit
PD × Theft × Post	0.0135***	0.0112***	0.0099***	-0.0294***	-0.0233***	-0.0232***
	(0.0027)	(0.0024)	(0.0025)	(0.0077)	(0.0075)	(0.0076)
PD × Robbery × Post	-0.0200***	-0.0218***	-0.0202***	-0.2577***	-0.2636***	-0.2742***
	(0.0025)	(0.0025)	(0.0024)	(0.0095)	(0.0092)	(0.0092)
Observations	420,200	420,200	417,933	420,200	420,200	417,933
R^2	0.010	0.044	0.076	0.030	0.055	0.088
Mean DV	0.02	0.02	0.02	0.45	0.45	0.45
Court-by-year FE	No	Yes	Yes	No	Yes	Yes
Crime FE	No	No	Yes	No	No	Yes

Note: The table shows the effect of pretrial detention (PD) on the likelihood of being absolved (columns (1)-(3)) and the likelihood of having an alternative exit (columns (4)-(6)) using a IV difference-in-differences approach, as described in Equation 6. Estimates can be interpreted as relative to the baseline category "Other Crimes" during the pre-reform period. Columns (1) and (4) have no fixed effects, columns (2) and (5) have court-by-year fixed effects and columns (3) and (6) add court-by-year and crime fixed effects. Standard errors clustered at judge level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A11: Effects of the 2016 reform: different trial procedures for Theft

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A		Pre 2016 reform			Post 2016 reform	
Plea bargain	OLS	First Stage	IV	OLS	First Stage	IV
Pretrial Detention	0.1632***		0.1493***	0.1686***		0.1952**
	(0.0069)		(0.0485)	(0.0097)		(0.0782)
Mean DV	0.31	0.90	0.31	0.30	0.90	0.30
Panel B: Simplified		Pre 2016 reform			Post 2016 reform	
Pretrial Detention	-0.0605***		0.0579	0.0310***		0.0257
	(0.0081)		(0.0509)	(0.0112)		(0.0850)
Mean DV	0.32	0.90	0.32	0.30	0.90	0.30
Panel C: Ordinary		Pre 2016 reform			Post 2016 reform	
Pretrial Detention	-0.0002		0.0227	0.0013		0.0229
	(0.0022)		(0.0145)	(0.0032)		(0.0176)
Mean DV	0.03	0.90	0.03	0.02	0.90	0.02
Panel D: Alternative exit		Pre 2016 reform			Post 2016 reform	
Pretrial Detention	-0.1020***		-0.2296***	-0.2010***		-0.2446***
	(0.0080)		(0.0552)	(0.0115)		(0.0558)
Mean DV	0.33	0.90	0.33	0.35	0.90	0.35
Judge Leniency		0.9336***			0.8849***	
		(0.0285)			(0.0468)	
Observations	90,118	90,118	90,118	36,561	36,561	36,563
F-stat		1,070.30			358.13	

Note: The table reports estimates for pretrial detention on different case procedures for thefts. Each panel shows results for separate set of regressions, modifying the dependent variable. Panel A, B, C show results for different trial procedures: plea bargain, simplified, ordinary, respectively, and Panel D for Alternative exit. Columns (1), (2) and (3) show the effect of pretrial detention on each procedure (OLS as in Equation 1), the effect of judge leniency on pretrial detention (IV First Stage as in Equation 4) and the effect of judge leniency on each procedure (IV Second Stage as in Equation 5), respectively, before the 2016 reform. Columns (4), (5) and (6) show the same effects post-reform. All coefficients include court-by-year fixed effects. Standard errors clustered at judge level in parentheses. Montiel Olea and Pflueger (2013) robust F-stat is reported. *** p<0.01, ** p<0.05, * p<0.1.

Table A12: Effects of the 2016 reform: different trial procedures for Other crimes

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A		Pre 2016 reform			Post 2016 reform	
Plea bargain	OLS	First Stage	IV	OLS	First Stage	IV
Pretrial Detention	0.1383***		0.1625***	0.1144***		0.0818
	(0.0054)		(0.0335)	(0.0094)		(0.0509)
Mean DV	0.33	0.88	0.33	0.30	0.87	0.30
Panel B: Simplified		Pre 2016 reform			Post 2016 reform	
Pretrial Detention	-0.0258***		-0.0067	0.0115		0.0769*
	(0.0053)		(0.0304)	(0.0074)		(0.0432)
Mean DV	0.18	0.88	0.18	0.15	0.87	0.15
Panel C: Ordinary		Pre 2016 reform			Post 2016 reform	
Pretrial Detention	0.0039**		0.0138	-0.0016		0.0156
	(0.0016)		(0.0099)	(0.0027)		(0.0124)
Mean DV	0.03	0.88	0.03	0.02	0.87	0.02
Panel D: Alternative exit		Pre 2016 reform			Post 2016 reform	
Pretrial Detention	-0.1156***		-0.1697***	-0.1237***		-0.1728***
	(0.0069)		(0.0326)	(0.0098)		(0.0590)
Mean DV	0.46	0.88	0.46	0.51	0.87	0.51
Judge Leniency		1.0868***			1.0912***	
		(0.0239)			(0.0431)	
Observations	149,199	149,199	149,199	52,981	52,981	52,982
F-stat		2,074.58			642.12	

Note: The table reports estimates for pretrial detention on different case procedures for other crimes. Each panel shows results for separate set of regressions, modifying the dependent variable. Panel A, B, C show results for different trial procedures: plea bargain, simplified, ordinary, respectively, and Panel D for Alternative exit. Columns (1), (2) and (3) show the effect of pretrial detention on each procedure (OLS as in Equation 1), the effect of judge leniency on pretrial detention (IV First Stage as in Equation 4) and the effect of judge leniency on each procedure (IV Second Stage as in Equation 5), respectively, before the 2016 reform. Columns (4), (5) and (6) show the same effects post-reform. All coefficients include court-by-year fixed effects as described in Equation 5. Standard errors clustered at judge level in parentheses. Montiel Olea and Pflueger (2013) robust F-stat is reported. *** p<0.01, ** p<0.05, * p<0.1.
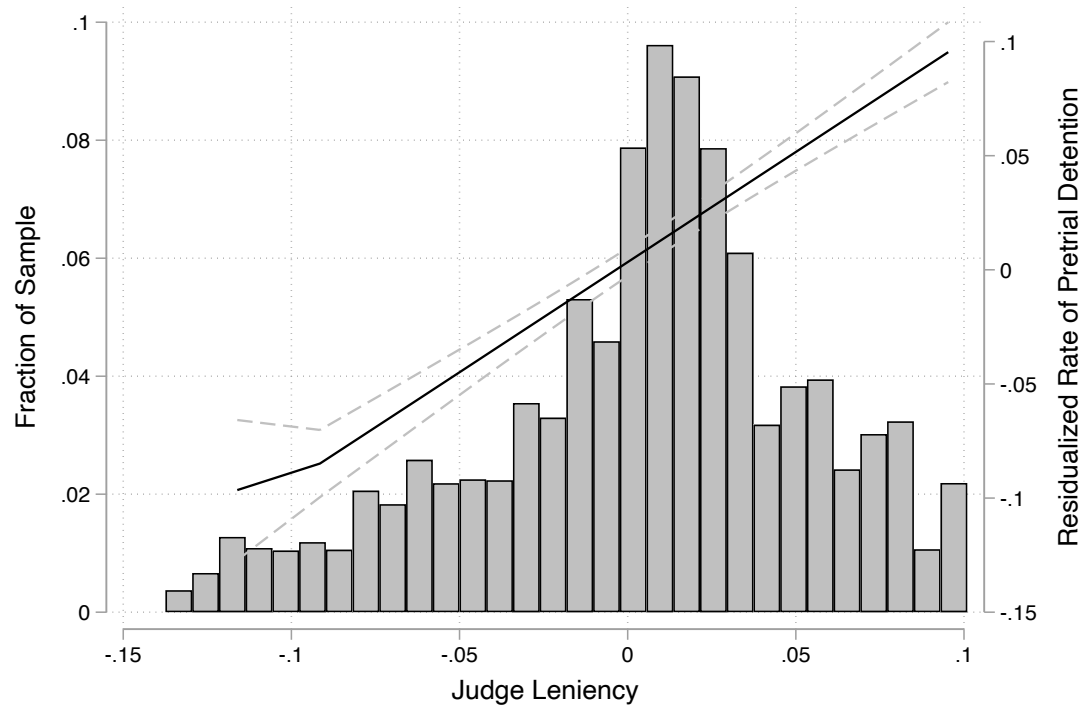
B Figures

Figure B1: Criminal justice system in Chile
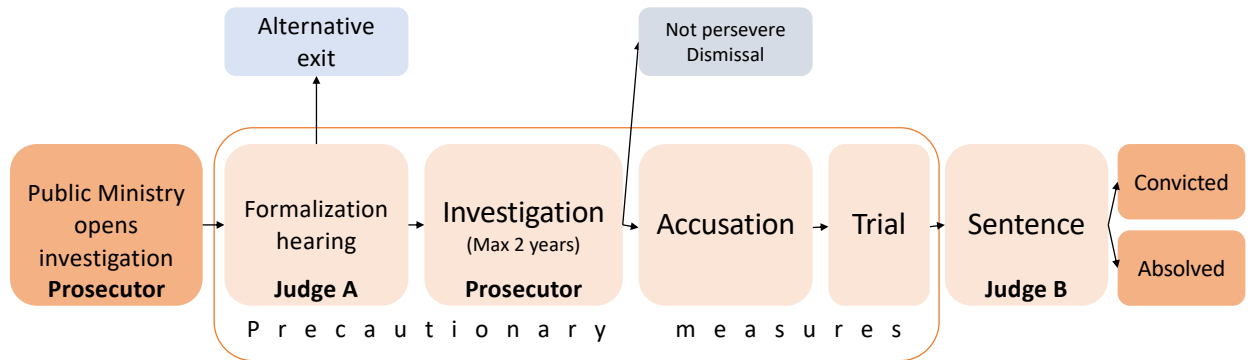
Note: This figure describes the criminal justice system in Chile. From the moment the Public Ministry opens an investigation until the sentence is given. All outcomes of the paper can be observed: alternative exit, convicted and absolved. The figure also describes the role of prosecutors during investigation and the different judges during formalization and sentence hearings.

Figure B2: Distribution of the Judge Leniency Instrument and First Stage

Note: This figure reports the distribution of the judge leniency instrument. It also shows the fractional-polynomial estimation of the relationship between the instrument and the likelihood of being pretrial detained.

Figure B3: Distribution of the Judge Leniency Instrument and First Stage
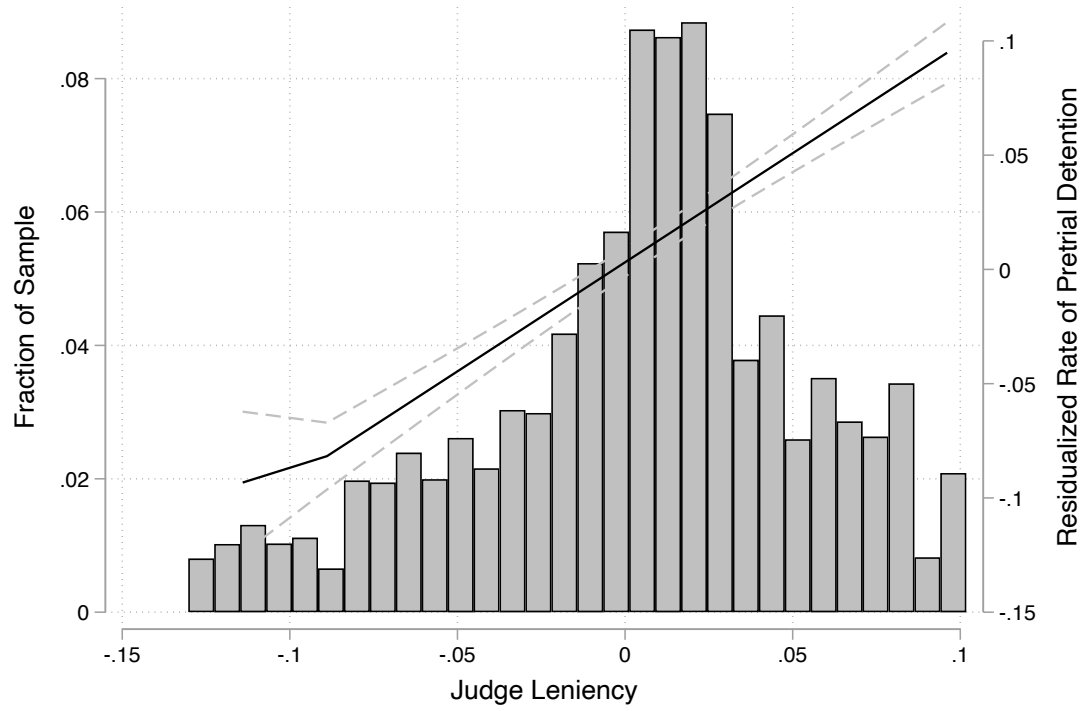
Note: This figure reports the distribution of the judge leniency instrument before the 2016 reform. It also shows the fractional-polynomial estimation of the relationship between the instrument and the likelihood of being pretrial detained.

Figure B4: Distribution of the Judge Leniency Instrument and First Stage
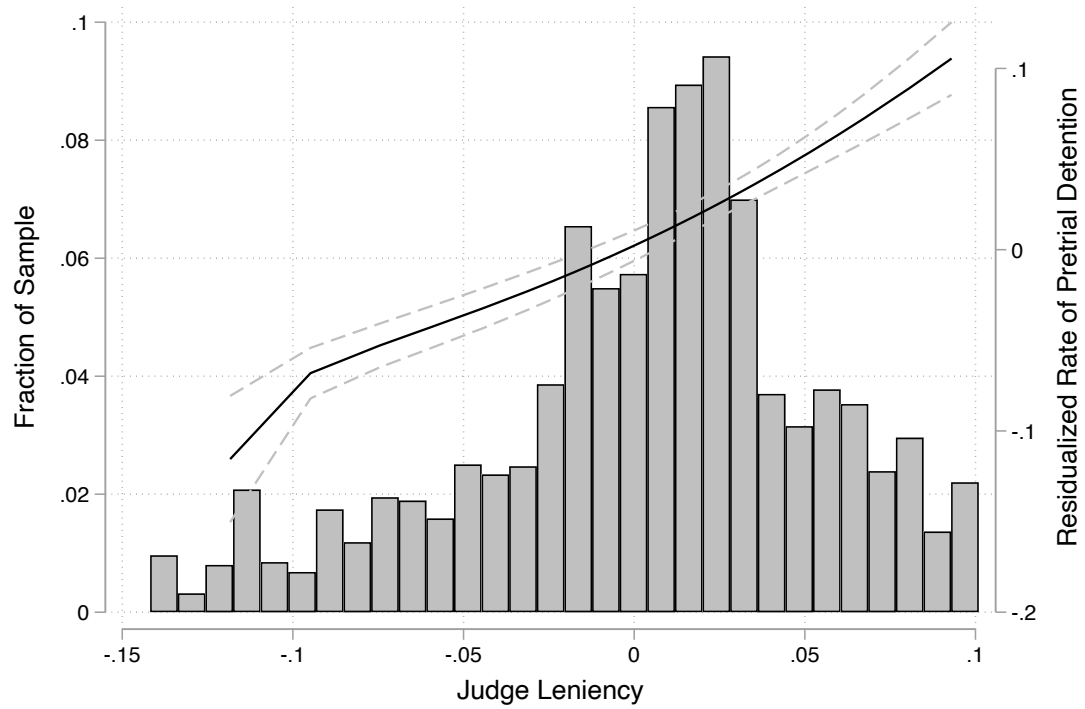
Note: This figure reports the distribution of the judge leniency instrument after the 2016 reform. It also shows the fractional-polynomial estimation of the relationship between the instrument and the likelihood of being pretrial detained.

Figure B5: Evolution of Likelihood of Attending Trial
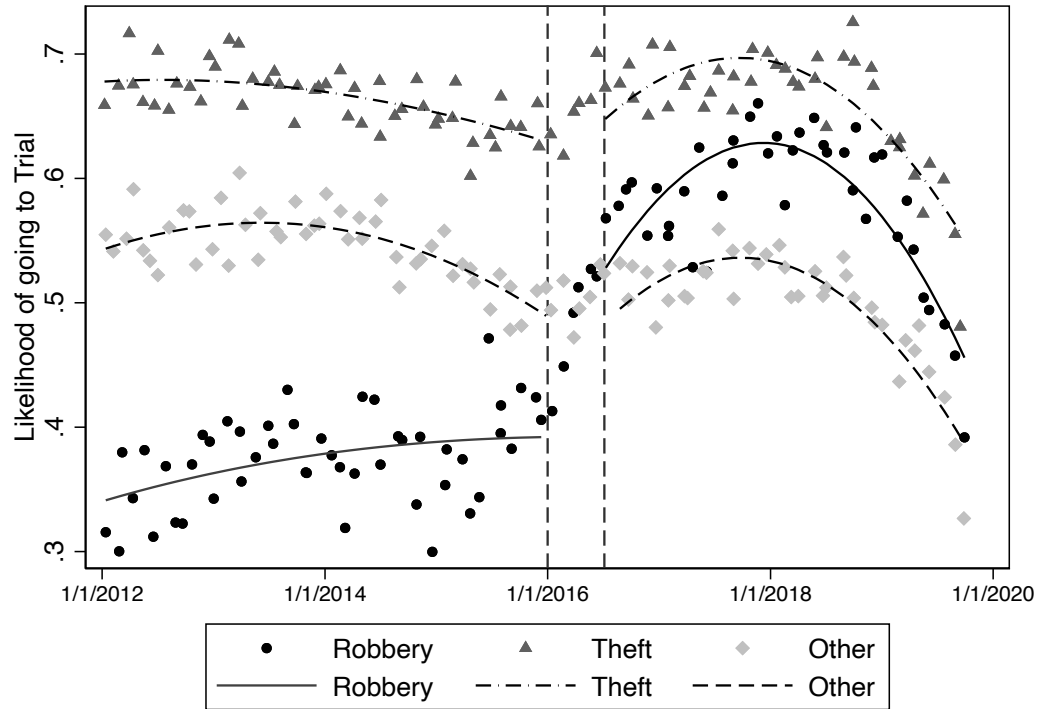
Note: This figure reports the monthly average likelihood of attending trial by type of crime (robbery, theft and other crimes) for the period January 2012 - September 2019. It corresponds to the number of cases resolved through trial divided by the total number of cases in a given month. The horizontal axis aggregates incidents monthly, using the date of pretrial detention hearing. We adjusted a fractional-polynomial line to each serie, and exclude from the predicted lines observations between January 1, 2016 and July 5, 2016 when the reform was officially in place –indicated by the respective vertical lines. Criminal cases that entered the system during this period are likely to be treated by the change in prosecution rules –which applies *ipso facto*– if they were not finished on trial before July, 2016.

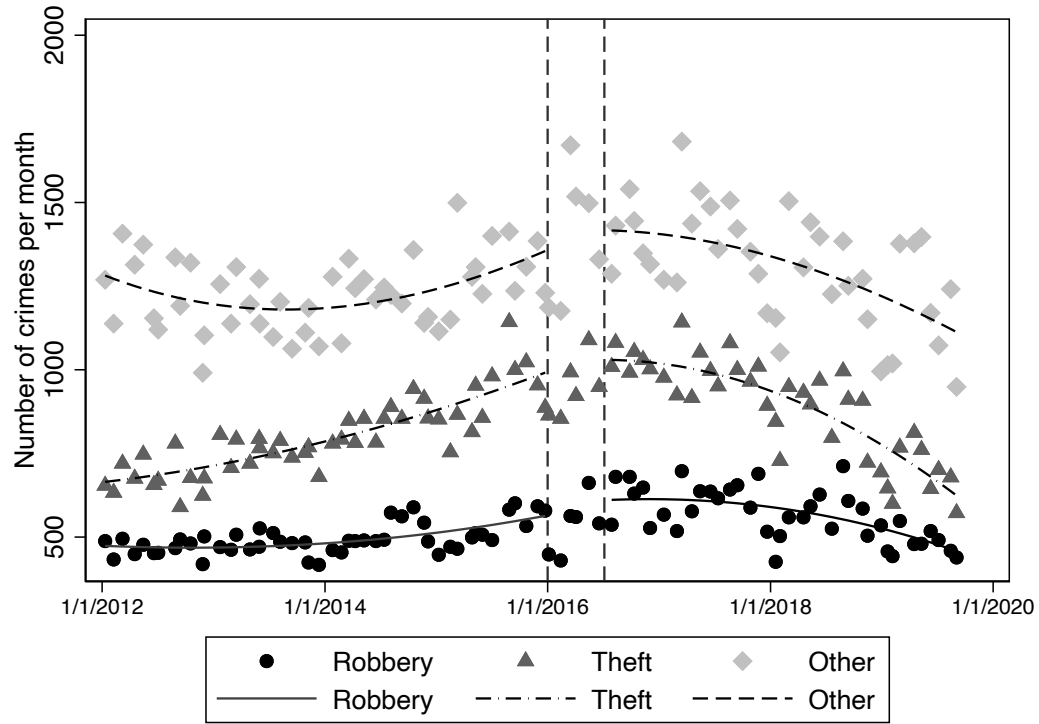Figure B6: Evolution of Likelihood of Conviction
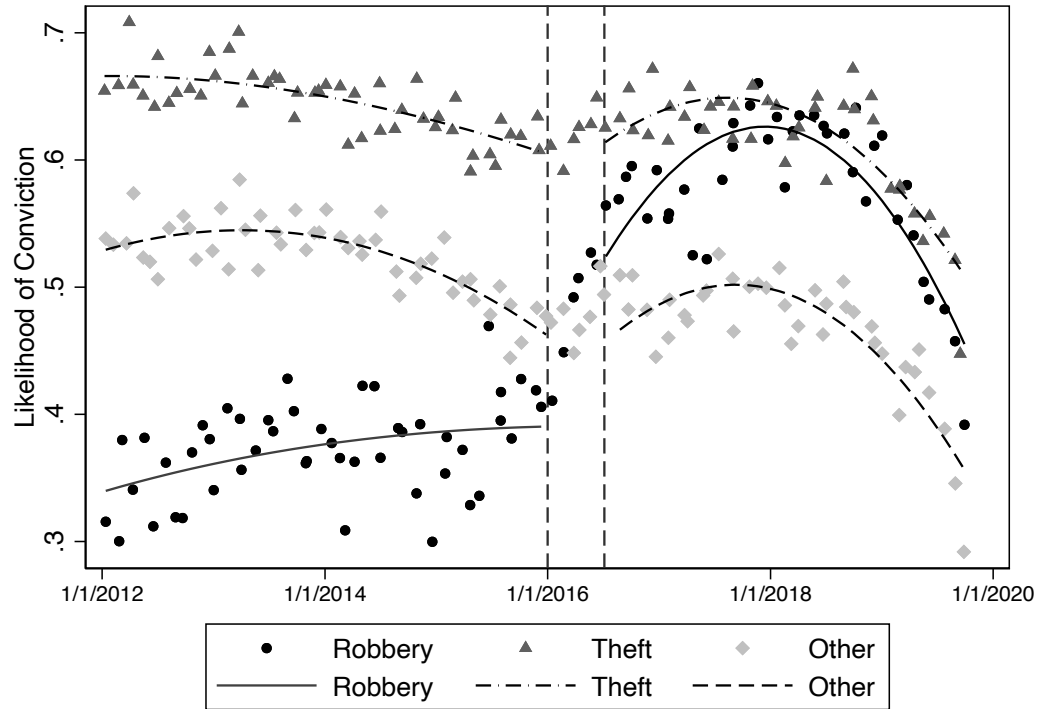
Note: This figure reports the monthly average likelihood of conviction by type of crime (robbery, theft and other crimes) for the period January 2012 - September 2019. It corresponds to the number of cases that resulted in a conviction divided by the total number of cases in a given month. The horizontal axis aggregates incidents monthly, using the date of pretrial detention hearing. We adjusted a fractional-polynomial line to each serie, and exclude from the predicted lines observations between January 1, 2016 and July 5, 2016 when the reform was officially in place –indicated by the respective vertical lines. Criminal cases that entered the system during this period are likely to be treated by the change in prosecution rules –which applies *ipso facto*– if they were not finished on trial before July, 2016.
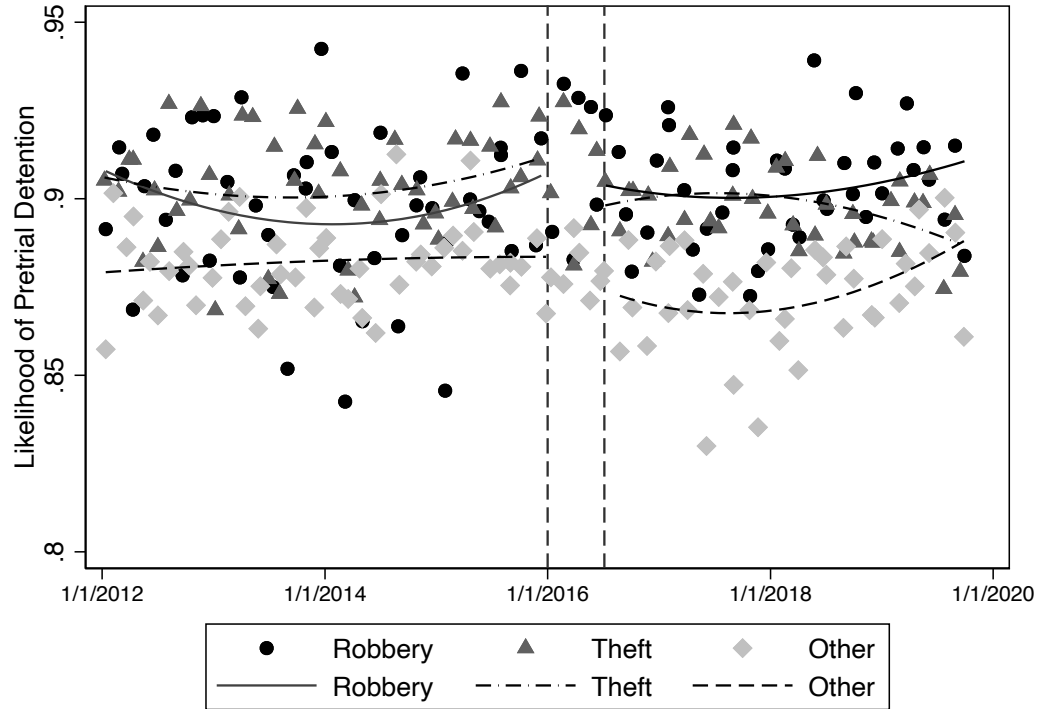
Figure B7: Evolution of crimes
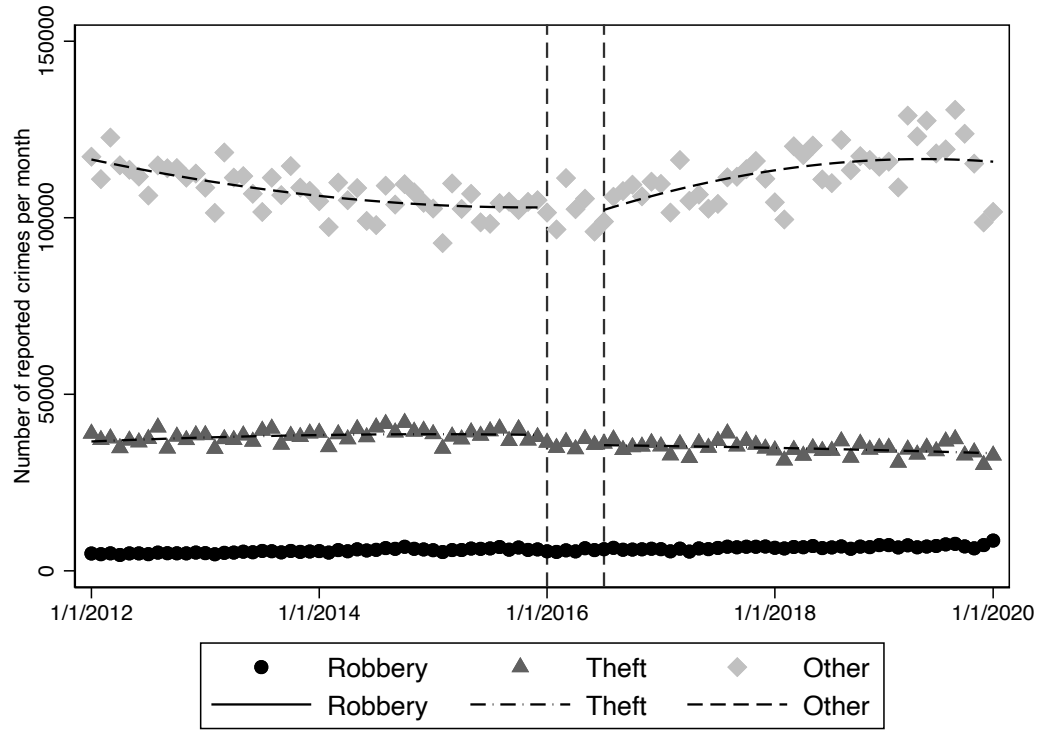
Note: This figure reports the monthly number of reported crimes (robbery, theft and other crimes) for the period January 2012 - December 2019. It corresponds to the *crimes known to the police* that were reported in a month by CEAD. The horizontal axis aggregates incidents monthly, using the date of the report. We adjusted a fractional-polynomial line to each serie, and exclude from the predicted lines observations between January 1, 2016 and July 5, 2016 when the reform was officially in place –indicated by the respective vertical lines. Criminal cases that entered the system during this period are likely to be treated by the change in prosecution rules –which applies *ipso facto*– if they were not finished on trial before July, 2016.

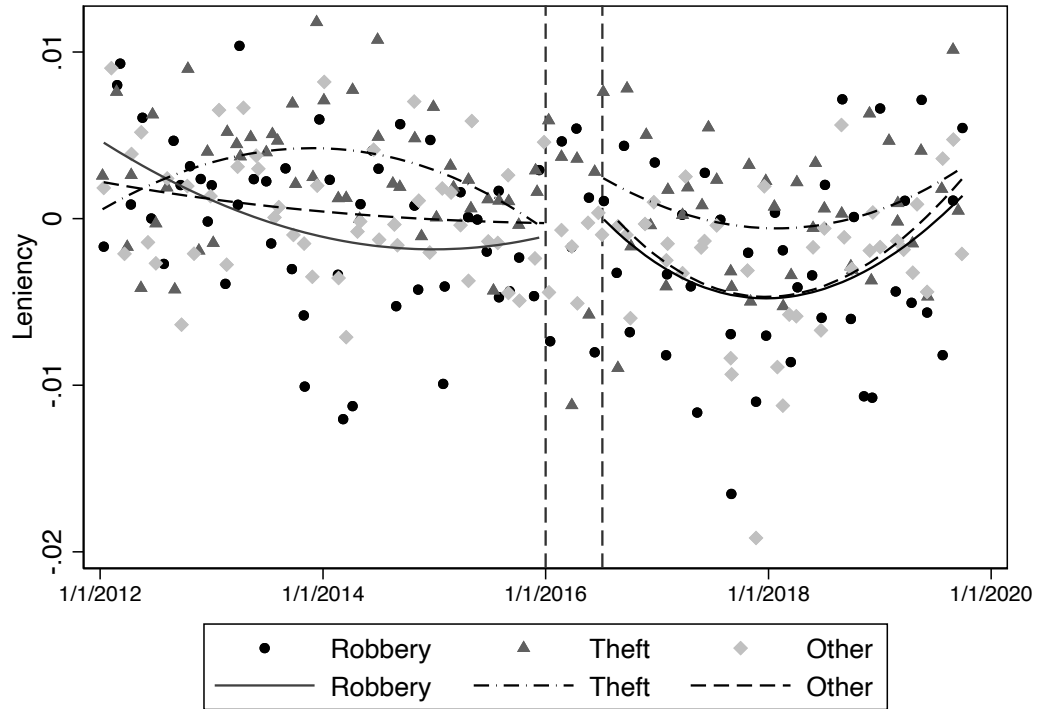Figure B8: Evolution of the number of pretrial detention hearings

Note: This figure reports the monthly sum of pretrial detention hearings by type of crime (robbery, theft and other crimes) for the period January 2012 - September 2019. It corresponds to the number of cases in which a prosecutor requested pretrial detention in a given month. The horizontal axis aggregates incidents monthly, using the date of pretrial detention hearing. We adjusted a fractional-polynomial line to each serie, and exclude from the predicted lines observations between January 1, 2016 and July 5, 2016 when the reform was officially in place –indicated by the respective vertical lines. Criminal cases that entered the system during this period are likely to be treated by the change in prosecution rules –which applies *ipso facto*– if they were not finished on trial before July, 2016.

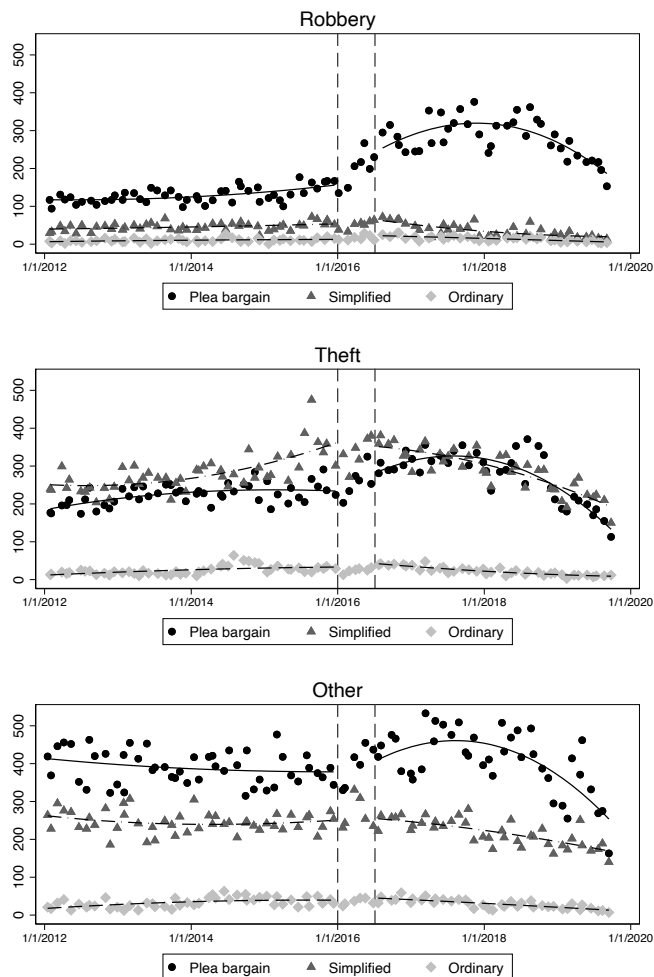Figure B9: Evolution of Likelihood of getting Pretrial detention

Note: This figure reports the monthly average likelihood of pretrial detention by type of crime (robbery, theft and other crimes) for the period January 2012 - September 2019. It corresponds to the number of cases where the judge conceded pretrial detention over the sum of pretrial detention hearings in a given month. The horizontal axis aggregates incidents monthly, using the date of pretrial detention hearing. We adjusted a fractional-polynomial line to each serie, and exclude from the predicted lines observations between January 1, 2016 and July 5, 2016 when the reform was officially in place –indicated by the respective vertical lines. Criminal cases that entered the system during this period are likely to be treated by the change in prosecution rules –which applies *ipso facto*– if they were not finished on trial before July, 2016.

Figure B10: Evolution of the Leniency of the judges

Note: This figure reports the monthly average leniency of the judges by type of crime (robbery, theft and other crimes) for the period January 2012 - September 2019. It corresponds to the propensity that a given judge has of sending any given individual to pretrial detention in a given month (see section 4.1). The horizontal axis aggregates incidents monthly, using the date of pretrial detention hearing. We adjusted a fractional-polynomial line to each serie, and exclude from the predicted lines observations between January 1, 2016 and July 5, 2016 when the reform was officially in place –indicated by the respective vertical lines. Criminal cases that entered the system during this period are likely to be treated by the change in prosecution rules –which applies *ipso facto*– if they were not finished on trial before July, 2016.

Figure B11: Evolution of Procedures by type of crime

Note: This figure reports the monthly sum of cases solved by type of procedure (plea bargain, simplified, ordinary), for each type of crime (robbery, theft and other crimes) for the period January 2012 - September 2019. It corresponds to the number of cases that were solved by each procedure in a given month. The horizontal axis aggregates incidents monthly, using the date of pretrial detention hearing. We adjusted a fractional-polynomial line to each serie, and exclude from the predicted lines observations between January 1, 2016 and July 5, 2016 when the reform was officially in place –indicated by the respective vertical lines. Criminal cases that entered the system during this period are likely to be treated by the change in prosecution rules –which applies *ipso facto*– if they were not finished on trial before July, 2016.

C Stages of Criminal Process

This appendix section describes the complete criminal proceeding:

1. The Public Ministry receives information about a report and decides whether it constitutes a crime and if there is enough evidence to open an investigation

2. If the investigation is open, a Formalization hearing will take place. There, the defendant will be informed that there is an investigation against him and that a lawyer from the Public Criminal Defense Office (*Defensoría Penal Pública*) will be provided, if needed.

3. In some cases, during the formalization hearing, alternative exits can be offered, after agreement with the prosecutor and the victim (when the defendant has no prior sentence and the crime is not severe).

4. In absence of alternative exit, the judge (*Juez de Garantía*) will set up a period for the investigation (that will not be longer that 2 years). Also, if necessary, the judge will order precautionary measures.

5. During the investigation, both the prosecutor and the defense must collect evidence to prove their cases.

6. Once the investigation concludes, the prosecutor informs the judge whether there is enough evidence for an accusation. Otherwise, the case will be dismissed or the Public Ministry decides not to persevere.

7. If accused, simplified or plea bargaining procedures can be offered, when the potential sentence is less than 3 and 5 years, respectively. Also for a maximum of 10 years for robberies after the 2016 reform.

8. Otherwise, the case follows to the regular trial procedure, where evidence is presented to prove the existence of crime and the degree of responsibility. For the trial, a set of 3 judges deliberates. These judges are different and come from different tribunals than the formalization judge that can order pretrial detention.

9. Finally, a sentence is given: conviction or absolution.

D   Precautionary measures

According to the Criminal Procedure Code, these are the precautionary measures that a judge to ensure the correct procedure of the investigation.

- Citation: The defendant must attend the court at any time his presence is needed;

- Detention: Deprivation of liberty for a maximum of 4 days with the purpose of making the defendant available to the prosecutor;

- Pretrial detention: Deprivation of liberty for as long as the investigation takes (maximum 2 years). Applied when the other precautionary measures are insufficient to ensure the correct procedure;

- The deprivation of liberty, whether total or partial, at home or in another residence designated by the accused, if the latter is located outside the city where the court is based;

- Submission to the supervision of a specific person or institution, which shall report periodically to the judge;

- The obligation to report periodically to the judge or to an authority designated by the judge;

- The prohibition from leaving the country, the locality of residence, or the territorial area determined by the court;

- The prohibition from attending certain meetings, venues, or public events, or from visiting specific places;

- The prohibition from communicating with certain individuals, provided that this does not infringe on the right to a defense;

- The prohibition from approaching the victim or their family and, if applicable, the obligation to leave the home shared with them;

- The prohibition from owning, possessing, or carrying firearms, ammunition, or cartridges; and

- The obligation of the accused to vacate a specific property.

E   Criminal Procedure Code, Article 140: Requirements to order pretrial detention

Once the investigation has been formalized, the court, at the request of the Public Prosecutor's Office or the complainant, may order the pretrial detention of the accused, provided that the requesting party proves that the following requirements are met:

- There is evidence justifying the existence of the crime under investigation;

- There is evidence that allows a well-founded presumption that the accused has participated in the crime as a perpetrator, accomplice, or accessory, and

- There is qualified evidence that allows the court to consider that pretrial detention is indispensable for the success of specific and determined investigative procedures, or that the freedom of the accused poses a danger to the security of society or the victim, or that there is a risk of flight, in accordance with the provisions of the following paragraphs.

Pretrial detention shall be deemed indispensable for the success of the investigation when there is a serious and well-founded suspicion that the accused may obstruct the investigation by destroying, altering, concealing, or falsifying evidence; or when they may induce co-defendants, witnesses, experts, or third parties to provide false information or act in an untruthful or uncooperative manner.

To assess whether the defendant's freedom constitutes a danger to society, the court must consider, in particular, the following circumstances: the seriousness of the offense; the severity of the penalty assigned to the crime; the number and nature of the crimes attributed to the accused; the existence of pending legal proceedings; and whether the accused acted as part of a group, gang, or organization.

The defendant's freedom shall be considered a danger to the security of society, especially when the crimes charged carry a felony-level penalty under the applicable law; when the accused has been previously convicted of a crime carrying an equal or greater penalty, whether or not they have served their sentence; when the crimes involve

attacks against the life or physical integrity of members of Chilean Police (*Carabineros de Chile*), the Investigative Police of Chile, Armed Forces personnel, or prison officers due to their position or in the course of their duties, and the assigned penalty is equal to or greater than the maximum degree of minor imprisonment; when the accused has used a firearm or weapons listed in Article 3 of Law No. 17.798 on Arms Control; when the accused is already subject to personal precautionary measures, such as a pending arrest warrant, parole, or alternative sentencing measures; or when, in the past two years, the accused has repeatedly been placed under detention, pretrial detention, or the personal precautionary measure described in Article 155(1)(a), if these measures were ordered for crimes carrying an afflictive penalty.

The victim's security shall be deemed at risk due to the release of the accused when there is qualified evidence allowing a presumption that the accused will commit attacks against the victim, their family, or their property.

The risk of the accused fleeing shall be presumed particularly when their identity is unknown, they lack proper identification documents, they refuse to provide such documents, or they use false or altered identification.

For the purposes of the fourth paragraph, only pending arrest warrants issued for the purpose of appearing before a court as a defendant shall be considered.