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The effect of pretrial detention on labor market outcomes

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The effect of pretrial detention on labor market outcomes*

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Abstract

We combine Chilean individual administrative data for criminal cases and labor market outcomes to estimate the effect of pretrial detention on labor outcomes using the Difference-in-differences (DiD) method and an instrumental variables (IV) approach. The IV approach takes advantage of the quasi-random assignment of judges. The IV results show that pretrial detention reduces the probability of having formal employment and the average monthly wage by 39% and 56% during the six months following the final trial verdict. DiD estimation delivers estimates that are between one-third and one-half smaller. The magnitudes of the effects shown continue to be relevant as much as 24 months after the final trial verdict. The results of our analysis suggest that the negative effect of pretrial detention is (at least) driven by the lasting effect of being excluded from the labor market during the trial, the accompanying social stigma, and the impact of pretrial detention on the probability of post-verdict incarceration.

Keywords. Wages and unemployment, Criminal Law, Pretrial detention, Incarceration.

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1 Introduction

At a global level, around one-third of individuals charged with a crime (approximately 2.8 million) are subject to "pretrial detention" (see Walmsley (2016)), a judicial measure to incarcerate accused individuals after they have been arrested and charged until their trail as a precautionary measure or to protect the investigation. Advocates of pretrial detention usually justify this measure based on the concern that the accused will not appear in court, may be a danger to others, or may interfere with the investigation. Detractors hold that pretrial detention is not only a threat to the presumption of innocence, a keystone of all contemporary judicial systems,[1] but could also have moral, social, and economic costs for the accused, including an impact on their posttrial labor market outcomes.[2]

In Chile, the focus of this study, individuals detained pretrial as percentage of the total prison population rose from 21.9% in 2007 to 36% in 2017. The corollary of this trend has been an increase in the number of individuals detained pretrial but who were either found not guilty or whose punishment did not include a custodial sentence. Furthermore, the time accused individuals who are detained pretrial spend in prison is not insignificant: 16.1% spent less than ten days in prison, 49.7% spent between ten days and six months, and 34.2% were incarcerated for more than six months.[3]

To assess the potential impacts of pretrial detention, this paper evaluates its effect on post-verdict labor market outcomes using Chilean data.[4] We use a novel dataset that merges individual administrative data on pretrial detention and labor market outcomes for arrested occurred between 2008 and 2016. The pretrial detention data comes from the Public Defender's Office records and the labor market outcome data comes from the administrative records from the Chilean unemployment insurance office. The labor market outcome data covers employment and wages and includes the monthly individual labor performance for all people who work in the formal private sector.

[1] See Article N° 11 of the United Nation's Universal Declaration of Human Rights.

[2] See Open Society Fundations (2011) and Open Society Fundations (2014).

[3] All statistics on pretrial detention are from the Public Defender's Office (Defensoría Penal Pública, DPP), the Chilean public institution that provides free legal representation for almost all individuals accused in criminal cases.

[4] For general perspective on mas incarceration and labor market see Weiman et al. (2007) and Western (2007).

To obtain causal estimates in this context is challenging. The pretrial detention decision is made by a judge who is likely to observe individual characteristics that we, as econometricians, can only partially observe. Moreover, these characteristics are probably correlated with labor outcomes. We follow two empirical strategies to address this endogeneity problem. First, and since the Chilean setting is characterized by a quasi-random assignment of judges for arraignment hearings at the court-by-time level, we build and use a measure of judge severity as an instrument to estimate a two-stage least squares (2SLS) model, following Dobbie et al. (2018) and Dahl et al. (2014). An important factor for the efficacy of this instrument is that the judges that preside over arraignment hearings determine pretrial detention but not final verdicts. We show suggestive evidence that this instrument meets the conditions of independence, relevance, exclusion, and monotonicity (Imbens and Angrist (1994)). Second, we take advantage of a panel database that begins several months before the person is arrested and ends after the final trial verdict to estimate a panel data DiD model, controlling for individual fixed effects. For reasons that are discussed in the paper, our preferred specification is the 2SLS model.

By estimating the IV and DiD models, we show a short-term (i.e., between one and six months after treatment) negative impact of pretrial detention on the probability of having a formal job that is between 9.8 (IV) and 17.8 (DiD) percentage points (pp). This equates to a reduction of 21% and 39%. With respect to average monthly wages, the negative short-term effect is $-32,227$ and $-92,635$ Chilean pesos (CLP) for the DiD and the IV estimations, or about 50 and 150 US dollars (USD). These point estimates represent a decrease of 20% and 56.7% in an individual's average monthly wage. These results are robust to alternative specifications. Regarding the persistence of these effects, there is a reduction in the point estimates in the case of employment over time, but the trend is less clear for wages. That said the negative effect of pretrial detention on both employment rate and average monthly wages is still relevant 18 to 24 months after treatment.

Using the DiD model we study two dimensions of treatment heterogeneity. First, we show that the negative effects are much larger for those who were detained pretrial for longer [5] by dividing the treatment group into terciles based on the time they spent in

[5] This result is contrary to the findings in Kling (2006) and Landersø (2015), but it is more in line with Ramakers et al. (2014).

pretrial detention. For the short-term effect on employment probability, the effects for the first, second, and third terciles are -4.5, -8.5, and -16.5 pp; for the short-term effect on average monthly wage, the effects are $-17,686$ CLP (26 USD), $-26,810$ CLP (39 USD), and $-52,900$ CLP (78 USD). Second, we estimate the causal effect of pretrial detention on labor outcomes for those accused individuals who were not incarcerated as a result of their final trial verdict. We find that the magnitude of the point estimates for this group is between one-third and one-half lower relative to the full estimation sample.

Regarding mechanisms, we discuss the relevance of three broad explanations for our results. The first potential explanation considers the importance of the fact that being detained pretrial removes the accused from the labor market, which may have lasting consequences on post-verdict employment and wages. This could happen, for example, because individuals are fired during pretrial detention and post-verdict they have problems in finding a new job. We refer to this explanation as the *labor market hypothesis*. The second explanation examines whether or not pretrial detention carries with it additional and specific impacts on labor outcomes beyond simply being unable to work during the detention period. We call this explanation as the *social stigma hypothesis*: in the hiring process previously incarcerated people are discriminated.[6] Finally, we investigate whether the effect of pretrial detention on the post-verdict labor market is due to its positive effect on the probability of post-verdict incarceration. We call this explanation the *labor incapacitation hypothesis*. Our results suggest that all these mechanisms may play a role in explaining the impact of pretrial detention on labor outcomes.

This paper contributes to the literature in two main ways. To the best of our knowledge, it is one of the first papers that estimates a causal effect of pretrial detention on labor outcomes, and certainly the first using data from a developing country, where prison conditions tend to be worse.[7] Given that we find an effect even for individuals whose final verdict does not include a custodial sentence, across both guilty and not guilty verdicts, our findings contrast with the results shown in Harding et al. (2018), who find an effect of prison sentences on employment but mainly as a result of incapacitation. Additionally, by taking advantage of our rich labor market data, we shed some

[6] On how incarceration and a criminal history can generate a stigma in the labor market see Bushway (2004); Finlay (2009); Mesters et al. (2016), Pager (2003), and Pettit and Lyons (2007). A complete discussion on these two mechanisms can be found in Apel and Sweeten (2014).

[7] Other papers have used data from a developing country to estimate the effect of pretrial detention on recidivism, they include Cortés et al. (2019) and Ferraz and Ribeiro (2019).

light on the mechanisms that could be causing the effect of pretrial detention on labor outcomes.

Our paper is related to the growing body of literature that studies the impact of (pre and posttrial) imprisonment on labor market outcomes. This literature has produced mixed results. There are studies that find a negative effect of incarceration on labor outcomes (see Mueller-Smith (2015); Raphael (2007); Western (2006) and Western et al. (2001)) and other studies have shown that the negative effects could be moderate in magnitude and rather short-lived (see Grogger (1995)). Some studies, such as Jung (2011), Lalonde and Cho (2008), Nagin and Waldfogel (1995), and Bhuller et al. (2020) have shown positive effects for incarceration. For example, Bhuller et al. (2020) uses data from Norway to show that posttrial discourages further criminal behavior, which is driven by individuals who were not working prior to incarceration. For them, incarceration increased future employment and earnings. Their findings demonstrate that time spent in prison could potentially be *pro human capital accumulation* for individuals already having issues with the labor market when incarceration focuses on rehabilitation.

Similar research has been carried out by Dobbie et al. (2018), showing that pretrial detention decreases formal sector employment, using data that links over 420,000 criminal defendants from two large, urban counties in the US with administrative court and tax records. The empirical strategy used in Dobbie et al. (2018), as in this paper, exploits exogenous variation in pretrial release given the quasi-random assignment of cases to bail judges.[8] They find that pretrial detention decreases the probability of employment in the formal labor market three to four years after the bail hearing by 9.4 pp (a 24.9% decrease). This result is smaller than the result from our IV estimations (about half of our point estimates), if we consider the first 24 months. The larger point estimates in our setting can be explained by the differences in prison conditions and in labor markets between the two settings, which reinforces the relevance of studying these policies in developing countries.

The rest of the paper proceeds as follows: Section 2 briefly describes the Chilean legal system and the conditions under which pretrial detention is imposed. Section 3 describes the data and presents some stylized facts on labor market outcomes for treatment and

[8]This source of exogenous variation has also been used in Aizer and Doyle (2015); Cortés et al. (2019); Dahl et al. (2014); Di Tella and Schargrodsky (2013); Ferraz and Ribeiro (2019); Green and Winik (2010); Knepper (2018) and Kling (2006).

the control groups that motivate our empirical strategy in Section 4. Section 5 presents our findings on the impact of pretrial detention on the probability of being employed and average monthly wages. Section 6 discusses the possible mechanisms that explain our results. Finally, Section 7 concludes.

2 Pretrial Detention in Chile

The reform of the criminal justice system in Chile was a gradual process that began in 2000 and was finalized in 2005. This broad reform, which included a new criminal code, replaced the inquisitorial model, a written system that had been in place for more than a century, with an oral, public, and adversarial procedure.[9] As part of the reform, new institutions were created, including the Office of the Public Prosecutor (Ministerio Público); the Public Defender's Office (Defensoría Penal Pública, DPP); the Guarantee Court (Juzgados de Garantía), where hearings that decide pretrial detention are undertaken; and the Oral Criminal Trial courts (Tribunales Orales de Juicio Penal). The DPP provides free legal representation for nearly all individuals who have been accused of committing a crime (more than 95%) and records all defendants that use their services, including detailed information on the particular crime in question.

In this new system the criminal process has the following stages. The first stage concerns the arrest of the individual, either because they are caught by the police in *flagrante delicto* (i.e., the commission of the crime) or as the result of an investigation conducted by the Public Prosecutor that culminates in an accusation. This stage concludes with an arraignment hearing at the Guarantee Court, where the detention judge chooses between three possible outcomes: to begin a criminal proceeding; an alternative ending (which may include compensation agreements and the conditional suspension of proceedings); or to simply dismiss the proceedings. It should be noted that most cases are resolved in the Guarantee Court, either by an alternative ending or dismissal. Generally speaking, a criminal proceeding is reserved for severe imputed crimes, which are the focus of this paper.

When the detention judge decides to begin a criminal proceeding, the length of the trial (considering the time required for an investigation) and any precautionary measures

[9] See Blanco et al. (2004) for a detailed description of this reform.

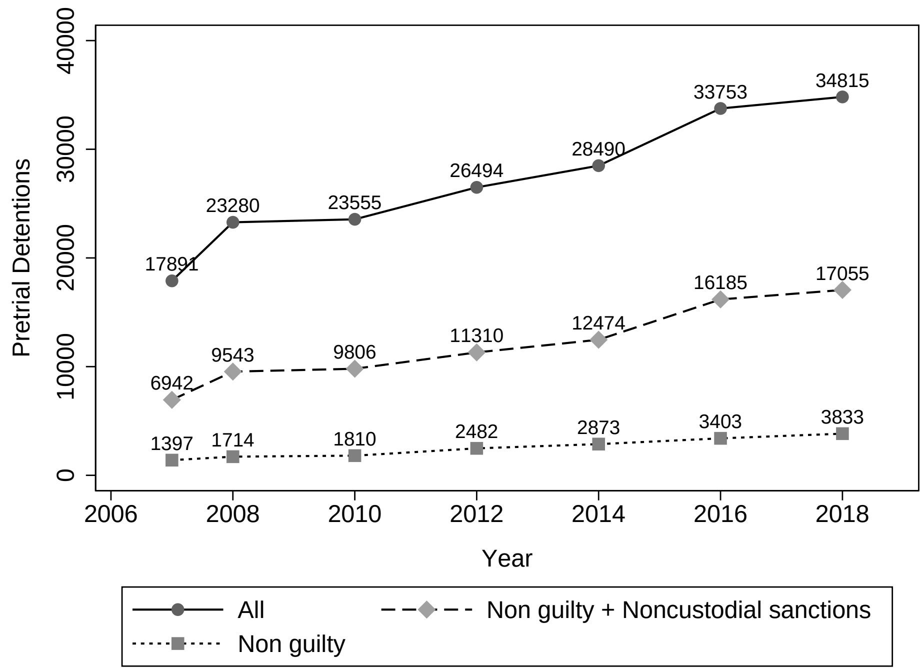
must be stipulated. Pretrial detention is the most severe precautionary measure and it must be requested by the prosecutor. The defendant's attorney is given the opportunity to advocate for their client with regard to the decision. To support the request for pretrial detention, the prosecutor may offer the following legal arguments: that the defendant presents a clear flight risk; that the defendant represents a danger to society; or the imprisonment of the defendant will aid with the investigation (see Riego and Duce (2011)). In theory, the prosecutor must put forward a very strong argument for pretrial detention. Unlike the legal system in the US, defendants in Chile do not have the option of posting bail in order to avoid pretrial detention.

There are several outcomes for a trial, ranging from a not guilty verdict to a conviction and prison sentence. One characteristic of criminal proceedings in Chile, which is particularly relevant to our study, is that the pretrial detention decision is kept separate from other trial outcomes through the use of the two different courts. It is also notable that the judge that presides over the arraignment hearing does not participate in the oral proceedings court trial, which is presided over by three separate judges.

Respect for a defendant's human rights was one of the principal motivations for criminal justice system reform in Chile. As time has passed, however, this original motivation has been somewhat forgotten (see Riego and Duce (2011)). Figure 1 illustrates this, showing that decisions to detain defendants pretrial became more frequent between 2007 (17,891 cases) and 2018 (34,815 cases), which means the proportion of the total cases that include a pretrial detention has increased from 7.3% to 9.6%. To look at it from another angle, the percentage of pretrial detainees in relation to the total prison population rose from 21.9% in 2007 to 36% in 2017, an increase of 64.4%. Figure 1 also shows that the number of individuals who were detained pretrial but who were either found not guilty or whose punishment did not include a prison sentence at all has also increased. Although, following the literature, we study the impact of pretrial detention on labor outcomes regardless of trial outcome, we also show the effect for those individuals who were accused of a crime and detained pretrial but who were not sentenced to prison after the trail itself.

In sum, although the reformed criminal justice system, implemented between 2000 and 2005, is based on principle of the presumption of innocence, Chile still detains many individuals who have yet to be found guilty, a trend that has been increasing over time.

Figure 1: Evolution of Pretrial Detention



Notes: This figure shows the evolution of pretrial detention from 2007 to 2018, using DPP administrative records. The dynamic is shown for three groups: a) All: considers the full sample; b) Not guilty + non-custodial sentence: includes all the individuals whose final verdict resulted in no prison time; c) Not guilty: includes those individuals whose final verdict declared them not guilty.

Finally, and this is formally tested in the empirical strategy section, it is relevant to stress that the assignment of arraignment judges does not depend on the characteristics of the prosecuted individual or the criminal case. Arraignment judges are allocated across days and cases depending on their workloads. Hence, conditional on court and year, the assignment of arraignment judges can be considered random.

3 Data and Stylized Facts

3.1 Data

We assemble an administrative, individual-level dataset from the national unemployment insurance scheme (operated by the Ministry of Labor) and the Public Defender's Office (DPP). As previously described, the DPP provides free legal representation for nearly all individuals who have been accused of committing a crime. For individuals who are not legally represented by a DPP attorney (i.e., those who hired a private attorney), we have data on their alleged crime but not on the final verdict of their trial. Therefore, given our treatment and control group definition, we do not include these cases in our sample. That said, less than 5% of prosecuted individuals are represented by a private attorney without DPP participation. In this paper, we use the DPP's prosecution records between 2006 and 2016.

The information provided by the DPP is very detailed on the crime (i.e., the specific type of crime that we group into broader categories), the court, the time spent in jail during the prosecution (if any), and the final verdict. Using the final verdict, we can determine whether the prosecuted individual was found guilty (as well as the punishment) or not guilty. In addition, the Research Department at Chile's Supreme Court provided us with data on all the arraignment judge who presided over arraignment hearings to decide pretrial detention between 2008 and 2017. This information is useful for our empirical strategy because it gives us exogenous variation in the probability of being subject to pretrial detention.

Our labor market data comes from administrative records held by Chile's national unemployment insurance scheme. As described in Acevedo et al. (2010), the country's unemployment insurance covers all enrolled workers over 18 years who are employed in private sector salaried jobs. Temporary workers are also included in the system, but individuals who have been unemployed or working in the public or informal sector since the unemployment insurance scheme began in 2002, are excluded.[10] Participation in the unemployment insurance scheme is compulsory for all workers who started a new job after October 2002; it is voluntary for those workers who were already in formal employment at that time. Therefore, full implementation of this scheme was only achieved after 2005 as workers were gradually incorporated. The database provides monthly data on wages, type of contract (either full or part time), as well as data on the employer including size of the firm (measured by the number of workers), economic sector, and a specific firm ID. The firm ID is useful because it allows us to see if a given worker keeps the same job over time or switches between jobs. As will become clear in Section 6, the possibility of keeping a job held pretrial is an important mechanism to explain the effect of pretrial detention on labor market outcomes.

[10]We have to exclude 27% of the accused individuals given that for them we do not observe any labor information before arrest.

Because of data availability issues, we focus on individuals whose prosecutions started after 2007. And we only consider first-time offenders (as revealed by the DPP data) because some prosecuted individuals who are not first-time offenders may have low pretreatment labor outcomes as a result of their pretreatment imprisonment. We further restrict the study to those accused of severe crimes -that is, crimes for which more than 10% of cases receive pretrial detention (this restriction is relaxed in the robustness check analysis).[11] Finally, our sample is restricted to those individuals employed for at least one month in the formal sector during the two years before the beginning of their prosecution: individuals who have no formal work experience would not have contributed to the unemployment insurance scheme from which we draw our data.

Accordingly, the treatment group are those who were subject to pretrial detention and the control group are those who were not. Individuals in both the treatment and the control group were prosecuted for the first time for a severe crime (i.e. a crime for which in more than 10% of cases the defendant is detained pretrial).

To illustrate the impact of these sample restrictions, Table 1 shows the differences in a set of variables between the non-restricted sample and the estimation sample. We do not compare the two groups using the labor market information because most of the differences between them are due to the lack of labor market information for individual excluded from the estimation sample. Looking at Table 1 we can observe that the estimation sample has a greater proportion of men, that individuals in this sample are more likely to have been charged with more severe crimes, and, consequently, that they are more likely to be subject to pretrial detention. Additionally, the trials of these individuals tend to be longer. Of course, a tendency for accusation of a more severe crime and a higher chance of pretrial detention for individuals in the estimation sample is deliberate.

To provide a clearer picture of our estimation sample, Table 2 shows the descriptive statistics for the covariates, reporting the information for the control and treatment groups separately. A few elements deserve comment. First, individuals in the treatment group are more likely to be male, and more likely to be of indigenous decent. Second, individuals in the treatment group are accused of more severe crimes and their trials

[11] Plea-bargaining is restricted in Chile to minor crimes. Because those crimes have a very low probability of pretrial detention, they are not considered in our estimation sample.

Table 1: Estimation sample versus population

Variable	All	Est. Sample	Norm. Dif.	p-value
Male	0.79	0.91	-0.358	0.000
Indigenous	0.02	0.01	0.023	0.000
non-Chilean citizen	0.01	0.01	0.047	0.000
Severe crimes	0.07	0.27	-0.553	0.000
Pretrial detention	0.02	0.16	-0.479	0.000
Days in judicial process	100	190	-0.428	0.000
Days in pretrial detention	151	147	0.027	0.002
Observations	1,188,482	142,679		

Notes: In this table, the first column (All) considers all the individuals in the DPP database (between 2007 and 2016) who are 18 years or older. The second column (Est. Sample) considers the sample used in our main estimations. In order to build this sample, we add the following conditions to the sample restrictions that were made to obtain column (1): (a) individuals must have been employed for at least one month in the formal sector during the twenty-four months before the beginning of the prosecution; and (b) individuals must have been accused of crimes where pretrial detention is an distinct possibility (i.e., severe crimes, for which in more than 10% of cases the defendant is detained pretrial. Norm. Dif. denotes the normalized differences in the means.

last longer. Third, individuals in the control group tend to have longer labor contracts although there are no differences in terms of firm size or industry. It is also notable that, even though our control and treatment groups are very different, none of our empirical strategies require them to be equal. We examine this point in more detail in Section 4 .

Table 2: Summary statistics for the covariates by treatment status

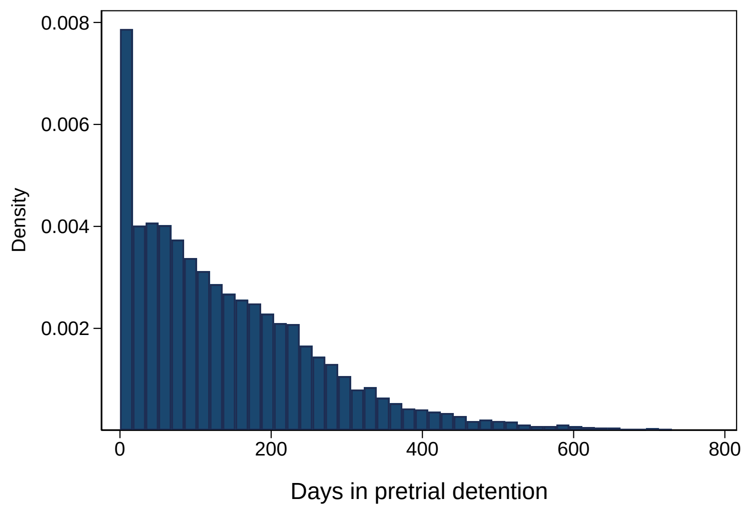
	Treated		Control	
	Mean	S.d	Mean	S.d
Demographic variables				
Male	0.9393	0.2388	0.9096	0.2868
non-Chilean citizen	0.0086	0.0921	0.0050	0.0703
Indigenous	0.0211	0.1438	0.0127	0.1122
Judicial variables				
Severe crimes	0.5473	0.4978	0.2160	0.4115
Days in judicial process	254	222	178	229
Type of contract				
Indefinite term	0.3183	0.0011	0.4242	0.0008
Fixed term	0.6817	0.0015	0.5758	0.0010
Firm size				
Micro	0.1235	0.0010	0.1398	0.0007
Small	0.2356	0.0016	0.2237	0.0009
Medium	0.2242	0.0018	0.2148	0.0007
Big	0.4167	0.0013	0.4217	0.0011
Firm sector				
Agriculture - Silviculture - Industrial Fishing	0.1017	0.0021	0.0921	0.0010
Mining	0.0094	0.0035	0.0117	0.0015
Manufacturing	0.0935	0.0011	0.1045	0.0008
Electricity-Gas-Water	0.0022	0.0076	0.0024	0.0029
Construction	0.3016	0.0012	0.2457	0.0010
Commerce	0.1181	0.0022	0.1367	0.0007
Services	0.2966	0.0014	0.3174	0.0010
Transportation-Communication	0.0730	0.0017	0.0852	0.0009
No information	0.0039	0.0073	0.0044	0.0017
Observations	22,433		120,246	

Notes: This table reports the summary statistics for the covariates considered in our estimated models, comparing control and treatment groups. The table considers the estimation sample – namely, individuals in the DPP database (between 2007 and 2016) who are 18 years or older; who have been employed for at least one month in the formal sector during the two years before the beginning of prosecution; and who have been accused of crimes where pretrial detention is an distinct possibility (i.e., severe crimes, for which in more than 10% of cases the defendant is detained pretrial). S.d denotes standard errors.

3.2 Stylized Facts

Figure 2 is a histogram that shows days spent in pretrial detention for individuals in our estimation sample. We observe that, although a large proportion of prosecuted individuals only spent a few days in prison, many were detained for months. Indeed, the average length of pretrial detention for individuals that meet our restriction criteria is 111 days; the median is 72 days, the 25th percentile is 23, and the 75th percentile is 163 days.

Figure 2: Days in Pretrial Detention for the Estimation Sample



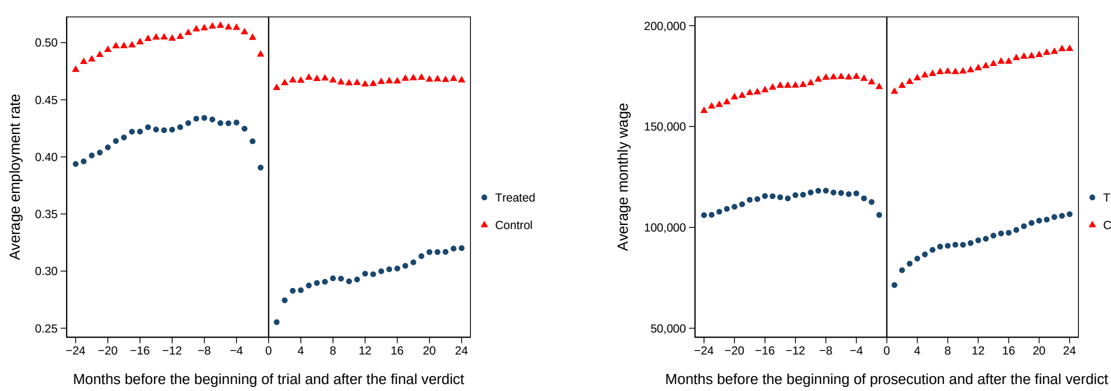
Notes: This figure shows a histogram for days spent in pretrial detention, considering the estimation sample of this paper –namely, individuals in DPP database (between 2007 and 2016) who are 18 years or older, who have been employed for at least one month in the formal sector during the two years before the beginning of the prosecution, and whose imputed crime category is one for which the defendant is detained pretrial in more than 10% of cases.

Before we present our empirical strategy and discuss our estimation results, we provide visual evidence to show the impact of pretrial detention on labor outcomes, using two different types of graphs that illustrate the dynamics of our dependent variables: monthly average wages and employment rate. Accordingly, panel (a) in Figure 3 shows the employment rate and panel (b) shows the average monthly wage for the period before the prosecution begins and the period after the final verdict. To be able to compare the labor dynamics of individuals whose prosecutions began in different months and years, we group individuals together by the number of months before the beginning of their prosecution and by the number of months after their final verdict in order to calculate the average monthly wage and average employment rate for the individuals in the treatment and control groups. For months before the beginning of the trial the value on the horizontal axis is negative; for months after the final verdict the value on the horizontal axis is positive. This implies that the time between -1 and 1 is equal to the duration of the trial, a variable that is heterogeneous across individuals. The average length of trial for the treatment group is equal to 254 days with a standard deviation equal to 243. And the average length of trail for the control group is equal to days 167 with a standard deviation equal to 231.

The second type of graphs present the differences in the employment rate or average monthly wage between the treatment and the control group. They are presented in Figure 4.

Figure 3: Employment and Wages Dynamics for the Treatment and Control Groups

(a) Employment rate (b) Average wages

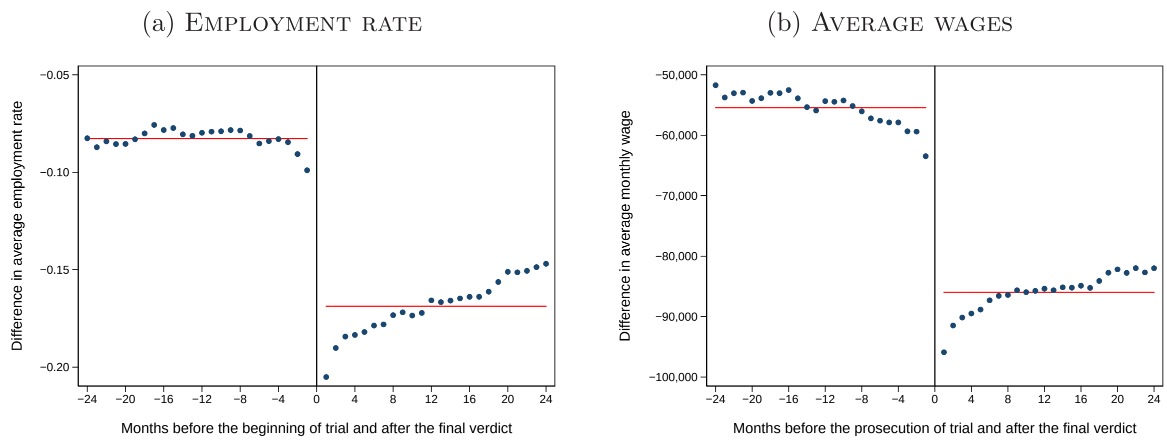


Notes: These figures show the dynamics for the employment rate (panel (a)) and average wages (panel (b)), pre- and post-prosecution, for the treatment (triangles) and control groups (circles). Each dot represents the average value of employment or monthly wages, for a particular month, X months before the beginning of the prosecution (when X has a negative value on the horizontal axes) or X months after the final verdict (when X has a positive value on the horizontal axes). Note that X equals zero refers to a period that is a different length for different individuals, lasting from the beginning of prosecution to the end of trial, including the final verdict.

These plots illuminate a few aspects in the data. First, both before and after treatment, individuals in the control group perform better in the labor market compared to individuals in the treatment group. During the months before prosecution, control group members have a higher probability of being employed, between 8 pp and 12 pp more on average. Moreover, their average monthly wages are about 70,000 CLP (or 108 USD) higher, which represents 37% of the average monthly wage of the control group.[12] Second, the parallel trends condition appears to be satisfied. This can be directly observed in Figure 3 and is also evidenced by the consistency of the pretreatment dynamics in Figure 4. It is formally tested in Section 5.2. Third, there is a clear, discrete change after an individual has been subject to pretrial detention in the differences between the treatment and control groups in both employment rates and average monthly wages, which supports this paper's main result. As panel (a) in Figure 4 shows, in the case of employment rates this change is about 4 pp on average. Panel (b) shows that for average monthly wages the change is around $18,000$ CLP. Finally, Figure 4 shows that the increase in the differences in labor outcomes between the treatment and control groups are more severe immediately after the final verdict, and it also shows that the increase in these differences does not fade over a period of two years after the end of the trial.

[12]The treatment group is partially comprised of individuals who make less than the minimum wage. There are three main reasons for this: the probability of being employed is low (between 44% and 51%) and the average wage calculation considers unemployment as zero wages; treatment group members are likely to have low productivity (and low bargaining power); and, finally, many could be employed in the informal labor market and therefore their wages are not observable.

Figure 4: Dynamics for the differences in employment and wages between treatment and control groups



Notes: These figures show the dynamic for the employment rate (panel (a)) and average monthly wages (panel (b)), pre- and post-prosecution. Each dot represents the difference in the average value of employment or wages between the treatment and control groups for a particular month, X months before the beginning of the prosecution (when X has a negative value on the horizontal axes) and X months after the final verdict (when X has a positive value on the horizontal axes). Note that X equals zero refers to a period that is a different length for different individuals, lasting from the beginning of prosecution to the end of trial, including the final verdict.

4 Empirical Strategy

We use two empirical strategies to estimate the effect of pretrial detention on labor market outcomes: a panel DiD approach (described in Section 4.1), where we control for individual fixed effects; and an IV approach, where we take advantage of the quasi-random assignment of arraignment judge (Section 4.2). We also present the OLS results as a reference point. As we note in Section 5, all the estimation procedures deliver similar results in qualitative terms.

4.1 Difference-in-Differences Empirical Approach

The DiD estimation uses the discrete change between pretrial and posttrial outcomes as a source of identification, a discrete change that is observed in both panels of Figure 4. To describe the OLS and DiD models, we use the notation outlined below.

Let Y_{it} be the outcome of interest for individual i at time $t \in \{0, 1\}$, which can be the average monthly wage or the employment rate, during the M months before the beginning of the trial (when $t = 0$) and during the M months after the final verdict (when $t = 1$). In our empirical implementation M is equal to 6 or 24 months. Treated individuals ($PreTrial_i = 1$) were incarcerated during the criminal proceedings (or for at least part of them) and the individuals in the control group ($PreTrial_i = 0$) were not incarcerated during the criminal proceedings.

The DiD and OLS models have different sets of covariates X. The OLS set of covariates (X_{i1}^{ols}) includes dummies for gender, Chilean citizenship, ethnicity, pretreatment average monthly wage, pretreatment employment rate (6 or 24 months before prosecution), location of the court (by region), type of crime, trial duration, and the year and month of the sentence. The panel DiD set of covariates (X_{it}^{p}) include the year and month when the prosecution began (when $t = 0$), the year and month of the sentence (when $t = 1$), and ω_i, which is the fixed effect for individual i.

The parameters of interest are denoted by β. For example, $\widehat{\beta^p}$ is the estimated effect of pretrial detention using the panel DiD approach (Eq. (2)). The specifications of the estimated models set out below.[13]

OLS:

$$Y_{i,1} = \alpha + \beta^{ols} PreTrial_i + \gamma' X_{i1}^{ols} + \epsilon_{i1}. \tag{1}$$

Difference-in-differences, panel data:

$$Y_{i,t} = \alpha \mathbb{1}[t = 1] + \beta^p PreTrial_i * \mathbb{1}[t = 1] + \gamma' X_{it}^{p} + \omega_i + \epsilon_{it},\ t \in \ \{0, 1\}. \tag{2}$$

The identification of β^p requires that there is no variable that varies between $t = 0$ and $t = 1$ and that influences both the probability of being incarcerated during the prosecution and labor market outcomes. This assumption is indirectly tested by presenting the labor outcome trends for the treated and control groups before treatment (Subsection 4.1.1).[14]

In Appendix D.1 we also show the results from a cross-sectional DiD and a DiD matching estimator. The results are very stable across different DiD specifications.

[13] $\mathbb{1}[A]$ is an indicator function that takes the value of one when A is true and zero otherwise.

[14] A good discussion on the importance of a proper understanding of the reasons why individuals from the treated and control groups have different pretreatment outcomes can be found in Kahn-Lang and Lang (2019).

4.1.1 General Difference-in-Differences Approach

Following Duflo (2001), we generalize the panel DiD identification strategy to an interaction terms analysis by allowing for period-by-period contrasts. In the literature, this is commonly referred to as an "event study" approach. This approach is suitable for presenting the effect of pretrial detention on labor outcomes, a test of parallel trends, and the potential fading out of the estimated effect, all in the same figure.

By using the month as the time unit, where the omitted category is the month that occurred 24 months before the beginning of prosecution, we run the following model:

$$Y_{it} = \sum_{n=-23 \wedge n \neq 0}^{24} \beta_n PreTrial_i * \mathbb{1}[t = n] + \sum_{n=-23 \wedge n \neq 0}^{24} \delta_n * \mathbb{1}[t = n] + \omega_i + \epsilon_{it}, \tag{3}$$

where i is the individual subindex, t is the period subindex, and ω_i is the fixed effect for individual i. Given this model, we are interested in the point estimates of β_n (and their confidence intervals), where each coefficient β_n can be interpreted as an estimate of the impact of the pretrial detention on period n relative to the first period ($n = -24$, 24 months before treatment), which is the omitted category.[15] Because the first 24 periods (including period -24, the reference category) are pretreatment, the identification strategy is suitable for causal interpretation to the extent that the point estimates of β_n, where $n < 0$, are close to zero. Conditional on the point estimates of β_n being close to zero, which is a simple way to test parallel trends, the point estimates for β_n, where $n > 0$, can be interpreted as the effects of pretrial detention after n month post treatment.

4.2 Instrumental Variable

The instrumental variable considered in this paper is a leave-out mean that captures how the (quasi-random) assignment of the arraignment judge impacts the probability of an individual being subject to pretrial detention.

To construct this instrumental variable we use a dataset that includes all criminal records, with and without data on labor outcomes. More specifically, this is the sample described in the first column of Table 1. From this starting point, we only consider courts

[15] Note that in the estimation we do not consider $n = 0$.

with an average of more than three cases per day. For individual 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:

$$\text{PreTrial}_{jc} = \alpha_0 + \alpha_1'(\text{court}_c \times \text{year}_{jc}) + \xi_{jc}. \tag{4}$$

We then proceed by calculating the judge severity score variable, denoted by $Z^{\text{judge}}_{j(i)}$, where $j(i)$ is the arraignment judge that made the decision on pretrial detention for individual i:

$$Z^{\text{judge}}_{j(i)} = \frac{1}{N_j - 1} \sum_{k \neq i}^{N_j - 1} \hat{\xi}_{kc}. \tag{5}$$

The judge severity score measures the propensity that a given arraignment judge has of detaining any given individual pretrial. As previous research has noted, this procedure is numerically equivalent to the judge fixed effect in a jackknife regression of pretrial detention estimated over all years. Our 2SLS estimators, therefore, are essentially jackknife instrumental variables estimators (JIVE), which are recommended when fixed effects are used to construct the instrument (see Stock et al. (2002) and Kolesár et al. (2015)). It should be noted that the leave-out mean is customary to avoid having an artificial strong first stage given by the direct linkage between an individual's own endogenous outcome and the instrument.

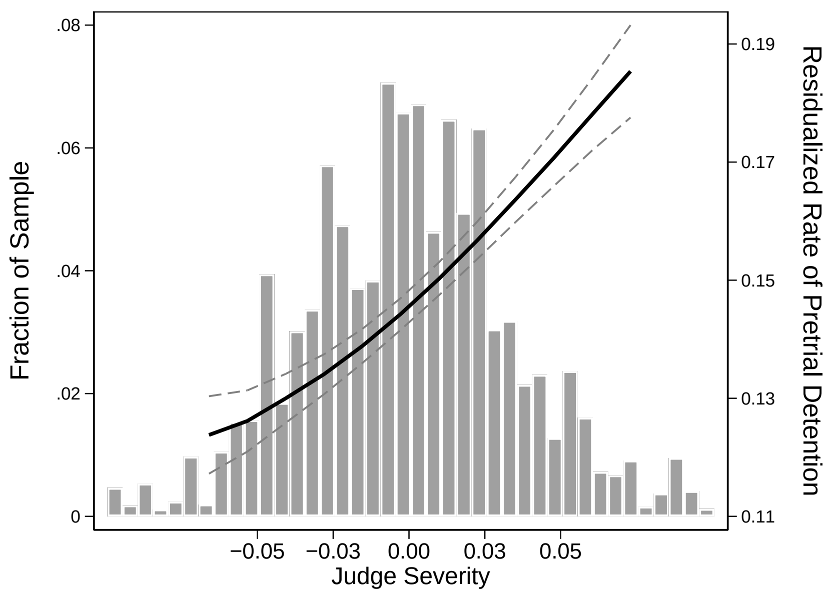
Given this instrument, we can estimate the effect of pretrial detention on labor outcomes in a 2SLS regression by considering the following two equations:

$$\begin{aligned} Y_{i1} &= \alpha^1 + \beta^{iv} PreTrial_i + \gamma^1 X_{i1} + \theta^1 \text{court}_c \times \text{year}_{jc} + \epsilon^1_{i1} \\ PreTrial_i &= \alpha^2 + \beta^2 Z^{\text{judge}}_{j(i)} + \gamma^2 X_{i1} + \theta^2 \text{court}_c \times \text{year}_{jc} + \epsilon^2_{i1}. \end{aligned} \tag{6}$$

IV Variation

In Figure 5 we present the distribution of the instrumental variable. The sample used to construct the instruments consists of 907 judges. The average judge handles 367 cases per year. In the estimation sample, the mean of the severity score variable is -0.00001 with a standard deviation of 0.0414. The severity measure ranges from -0.060 (5th percentile) to 0.065 (95th percentile), which implies that moving from a less severe to a more severe judge is associated with a 5 pp increase in being detained pretrial.

Figure 5: Distribution of the Judge Severity Instrument and the Non-parametric First Stage



Note: This figure reports the distribution of the judge severity measure that is estimated following the procedure described above. It also shows the non-parametric estimation of the relationship between the judge severity score and the residualized rate of pretrial detention.

4.2.1 IV validity

In order to interpret the 2SLS estimates as a local average treatment effect, the following four conditions must be met (Imbens and Angrist (1994)): (i) a non-trivial first stage, (ii) independence of the instrument, (iii) exclusion of the instrument, and (iv) monotonicity. In what follows, we will discuss each of these conditions.

A Non-trivial First Stage

Figure 5 shows the effect of the judge severity score on whether or not an individual is detained pretrial, estimated via a local linear regression of the former against the latter after controlling for court-by-year fixed effects (i.e., the residualized rate). The pretrial detention status varies monotonically along the judge severity score in a fairly linear fashion.

The first stage estimation is presented in Table 3. It shows that our instrumental variable is a good predictor of whether an individual is detained pretrial. We present two specifications: (1) controlling for employment outcomes during the 6 months before the prosecution and (2) controlling for employment outcomes during the 24 months before the prosecution. Both specifications deliver similar results. The magnitudes suggest that if an accused individual is assigned to a judge who is 10 pp more likely to give pretrial detention, then that individual is between 3.7 pp and 4.2 pp more likely to be detained pretrial.

Table 3: First stage: judge severity score

	(1)	(2)
Judge severity IV	0.373***	0.417***
	(0.036)	(0.037)
Six months of wages	-0.000***	
	(0.000)	
Six months of employment	-0.034***	
	(0.004)	
Two years of wages		-0.000***
		(0.000)
Two years of employment		-0.049***
		(0.005)
Male	0.046***	0.045***
	(0.004)	(0.004)
non-Chilean citizen	0.043*	0.028
	(0.022)	(0.023)
Indigenous	0.010	0.015
	(0.017)	(0.018)
Days in judicial process	0.000***	0.000***
	(0.000)	(0.000)
Crime severity	0.009***	0.009***
	(0.000)	(0.000)
Fixed term contract	0.013***	0.005
	(0.003)	(0.003)
Sector = Mining	-0.006	-0.006
	(0.014)	(0.014)
Sector = Manufacturing	-0.015**	-0.016**
	(0.006)	(0.006)
Sector = Electricity-Gas-Water	-0.042	-0.040
	(0.028)	(0.031)
Sector = Construction	0.013**	0.016***
	(0.005)	(0.006)
Sector = Commerce	-0.017***	-0.017***
	(0.006)	(0.006)
Sector = Services	-0.004	-0.005
	(0.005)	(0.005)
Sector = Transportation-Communication	-0.008	-0.007
	(0.007)	(0.007)
Firm size = Small	0.006	0.007
	(0.004)	(0.005)
Firm size = Medium	0.000	0.001
	(0.005)	(0.005)
Firm size = Big	-0.004	-0.001
	(0.004)	(0.004)
Constant	-0.130***	-0.113***
	(0.009)	(0.009)
F test	106.84	128.19
Observations	68,814	61,479
Court-by-time fixed effects	Yes	Yes

Notes: This table reports first-stage results for the linear IV model that estimates the effect of pretrial detention on labor outcomes. The IV is the judge severity measure, which is estimated following the procedure described in Subsection 4.2. In Column (1) we control for employment outcomes during the 6 months before the prosecution begins, and in Column (2) we control for employment outcomes during the 24 months before the prosecution begins. The model is estimated on the sample described in the notes for Table 2. Regression includes year interacted with court fixed effects. Robust standard errors are clustered at the judge level in parentheses. Statistical significance at 1%, 5%, and 10% is indicated by ***, **, and *, respectively.

Independence

The independence of the instrument is a key condition to validate the IV approach. In order for this condition to be met, the instrument must be as good as random assignment. To verify that this assumption holds in our context, Table 4 presents the same kind of analysis that would be performed in an actual experiment to assess the quality of its randomization. The first column displays the coefficients of a regression of the endogenous variable (i.e., pretrial detention) against the covariates described in the rows. The second column shows the same regression but with the instrumental variable (i.e., judge severity) as the dependent variable. In both models we control for year interacted with court fixed effects (the level at which judges are quasi-randomly assigned). That said, the null hypothesis of all parameters equal to zero, tested using the F-test, does not consider these court-by-year covariates. In Table 4 we note that gender, pretreatment labor outcomes, crime severity, and employer sector dummies are highly predictive of being subject to pretrial detention, whereas almost none of these variables seem to predict the severity of the assigned judge. This is corroborated further by the p-value given by the joint significance test, which is not able to reject the null hypothesis that all the coefficients are equal to zero (p-value = 0.35).

It is the combination of the two regressions presented in Table 4 that makes this test a convincing approach. In this table, the first column shows that these covariates are very relevant for predicting the endogenous variable, and the second column shows that the relevant covariates are not correlated with the instrumental variable, in a similar fashion to how one would test the validity of a randomized controlled trial.

Table 4: RANDOMIZATION TEST FOR JUDGE SEVERITY SCORE

	Pretrial Detention	Judge severity IV
Male	0.045***	-0.000
	(0.004)	(0.001)
non-Chilean citizen	0.027	-0.004**
	(0.023)	(0.002)
Indigenous	0.015	0.000
	(0.018)	(0.001)
Days in judicial process	0.000***	-0.000
	(0.000)	(0.000)
Two years of wages	-0.000***	-0.000
	(0.000)	(0.000)
Two years of employment	-0.049***	0.000
	(0.005)	(0.001)
Crime severity	0.009***	0.000
	(0.000)	(0.000)
Fixed term contract	0.005	0.000
	(0.003)	(0.000)
Sector = Mining	-0.006	-0.001
	(0.014)	(0.002)
Sector = Manufacturing	-0.016***	-0.000
	(0.006)	(0.001)
Sector = Electricity-Gas-Water	-0.039	0.002
	(0.031)	(0.004)
Sector = Construction	0.016***	0.000
	(0.006)	(0.001)
Sector = Commerce	-0.017***	0.000
	(0.006)	(0.001)
Sector = Services	-0.005	-0.000
	(0.005)	(0.001)
Sector = Transportation-Communication	-0.007	-0.000
	(0.007)	(0.001)
Firm size = Small	0.007	-0.000
	(0.005)	(0.000)
Firm size = Medium	0.001	-0.001*
	(0.005)	(0.001)
Firm size = Big	-0.001	-0.001*
	(0.004)	(0.000)
Constant	-0.113***	0.000
	(0.009)	(0.002)
Joint Test	0.0000	0.3533
Observations	61,479	61,479
Court-by-time fixed effects	Yes	Yes

Notes: This table reports the reduced form results that test the random assignment of cases to arraignment judges. The judge severity measure is estimated following the procedure described in Subsection 4.2. Column 1 presents estimates from an OLS regression of pretrial detention on the variables listed and year interacted with court fixed effects. Column 2 reports estimates from an OLS regression of the judge severity IV on the variables listed and year interacted with court fixed effects. The p-value reported at the bottom of columns 1 and 2 (named Joint Test) is for an F-test of the joint significance of the variables listed in the rows with the standard errors clustered at the judge level. Therefore, the F-test does not include the year interacted with court fixed effects in the null hypothesis. Robust standard errors clustered at the judge level are in parentheses. Statistical significance at 1%, 5%, and 10% is indicated by ***, **, and *, respectively.

Exclusion

The quasi-random assignment of cases to judges is sufficient for a causal interpretation of the reduced form impact of being assigned to a more severe arraignment judge (see reduced form estimates in Appendix A). The interpretation of the IV estimates as the impact of pretrial detention on labor outcomes, however, also requires that the arraignment judge assignment only impacts individuals' outcomes through the probability of being subject to pretrial detention and not through any other channel. Conditional on deciding to begin a criminal proceeding, this is likely to be the case in our setting because the only role of these judges is to prescribe precautionary measures. Recall that the verdict is determined by three different judges in an oral proceedings court.[16]

That said, we follow Bald et al. (2019) by presenting suggestive evidence that the exclusion restriction is met in our setting. In Table 5, we show the results of our test to see whether judge severity is correlated with other trial outcomes for the subgroup of accused individuals who were detained pretrial. More specifically, we test the correlation between our measure of judge severity and the duration of the pretrial detention, which ends when the final trial verdict is given (whether a custodial sentence is passed or not). As Table 5 shows, we find no evidence of a correlation between these variables, which is consistent with the exclusion assumption.

[16] A arraignment judge may also give a sentence in a case if it is *simple* enough and takes place in an abbreviated trial process for non-severe crimes only. In general, it is defined during the arraignment hearing at the guarantee court. Given that our focus is on more severe crimes, we do not consider theses cases.

Table 5: Exclusion restriction test: the impact of judge severity on sentencing outcomes

	Days in pretrial detention	Custodial sanction
Judge severity IV	30.44	-0.15
	(26.47)	(0.13)
Two years of wages	-0.00	-0.00
	(0.00)	(0.00)
Two years of employment	-11.38	0.00
	(7.12)	(0.03)
Male	7.99	-0.05**
	(5.00)	(0.02)
non-Chilean citizen	11.97	0.06
	(12.04)	(0.08)
Indigenous	11.29	0.07
	(12.14)	(0.05)
Days in judicial process	0.48***	0.00***
	(0.03)	(0.00)
Crime severity	0.98***	-0.00
	(0.11)	(0.00)
Fixed term contract	-0.55	-0.01
	(2.98)	(0.02)
Sector = Mining	22.19*	0.11
	(12.61)	(0.07)
Sector = Manufacturing	0.26	0.00
	(5.99)	(0.03)
Sector = Electricity-Gas-Water	-16.17	0.04
	(18.04)	(0.17)
Sector = Construction	-2.28	0.01
	(5.20)	(0.02)
Sector = Commerce	-1.18	0.03
	(5.63)	(0.03)
Sector = Services	-0.29	0.03
	(5.00)	(0.02)
Sector = Transportation-Communication	0.51	0.05*
	(7.20)	(0.03)
Firm size = Small	-3.74	-0.01
	(4.33)	(0.02)
Firm size = Medium	-0.67	-0.00
	(4.33)	(0.02)
Firm size = Big	-3.19	-0.00
	(3.93)	(0.02)
Constant	16.66*	0.62***
	(9.58)	(0.04)
Observations	8,854	8,854
Court-by-time fixed effects	Yes	Yes

Notes: This table presents the correlation, for those who were detained pretrial, between judge severity and the duration of the pretrial detention, which ends when the final trial verdict is given (whether a custodial sentence is passed or not), controlling for a set of relevant covariates (including court-by-time fixed effects). Robust standard errors are clustered at the judge level in parentheses. Statistical significance at 1%, 5%, and 10% is indicated by ***, **, and *, respectively.

Monotonicity

The monotonicity assumption requires that an accused individual who is detained pretrial by a lenient judge could have also been detained pretrial by a more severe judge and viceversa. In the literature, as discussed by both Dobbie et al. (2018) and Bhuller et al. (2020), a common approach to (indirectly) test this assumption is to estimate the first-stage regression for different subgroups and obtain point estimates for the instrument that are all of the same sign and have similar magnitudes. The result of this approach for our study is illustrated in panel A of Table 6, where we present the first-stage estimations for different subgroups of imputed crimes, by individuals' pretreatment average monthly wages (below and above the median), the size of the individual's pretreatment employer (big companies versus the rest), and crime severity (below and above the median). The table shows that in all cases but one (below median crime severity), the point estimates for the first stage are very similar across different groups. Even in the one case where the point estimate is different, We do not observe a flip in the sign of the estimated coefficient. These results suggest that the monotonicity assumption holds in our setting.

Table 6: Monotonicity test: First-stage estimations for different groups

Panel A: *First-Stage Impact of Judge severity, by Subgroup*

	above median wages	below median wages	big and medium companies	small and micro companies	above median crime severity	below median crime severity
Judge severity IV	0.330***	0.439***	0.386***	0.400***	0.453***	0.263***
	(0.046)	(0.055)	(0.046)	(0.061)	(0.058)	(0.041)
Observations	35,426	33,514	45,236	23,704	36,970	31,970

Panel B: *First-Stage Impact of Judge severity, by Reverse Sample Calculation for Subgroups*

	above median wages	below median wages	big and medium companies	small and micro companies	above median crime severity	below median crime severity
Judge severity IV	0.160***	0.301***	0.154***	0.229***	0.081	0.029
	(0.032)	(0.054)	(0.032)	(0.050)	(0.053)	(0.027)
Observations	35,352	33,456	45,057	23,680	36,825	31,892

Notes: In panel A, this table reports first-stage results for the linear IV model that estimates the effect of pretrial detention on labor outcomes by crime severity. Panel B shows the estimations of the first-stage models, where the leave-out instrument is recalculated for different subgroups using all cases outside of these subgroups. The regressions in both panels are estimated using the sample as described in the notes of Table 2. The regressions include year interacted with court fixed effects. Robust standard errors clustered at the judge level in parentheses. Statistical significance at 1%, 5%, and 10% is indicated by ***, **, and *, respectively.

To provide more indicative evidence to support the monotonicity assumption, we follow Bald et al. (2019) by estimating the first-stage models and recalculating the leave-out instrument for different subgroups using all cases outside of these subgroups. We present this analysis in panel B of Table 6. We observe that the point estimates are consistently positive and always statistically different from zero. The results from this approach also suggest that the monotonicity assumption holds in our setting. In Table 13 (Appendix B) we replicate these two tests but grouping the data by type of crime. As in the case of Table 6, both tests present evidence supporting monotonicity.

5 Results

In this section we present the results of the panel DiD and IV approaches. We also provide the OLS estimates as a point of reference.

5.1 Main results

For the short-term impact of pretrial detention on employment, the dependent variable is calculated in $t = 0$ as the employment rate during the six months before treatment and in $t = 1$ as the employment rate during the six months after treatment. Regarding the timing of these effects, note that *before treatment* refers to the period before the beginning of prosecution and *after treatment* refers to the period after the verdict. Table 7 shows that the short-term impact of pretrial detention on the employment rate is -9.8 pp in the case of the DiD estimation (column 2) and -17.8 pp in the case of the IV specification (column 3). These effects represent a decrease in the likelihood of being employed of between 21.1% (9.8/46.5) and 39.5% (17.8/45). Regarding the mid-term effect, in which case the dependent variable is calculated in $t = 0$ as the employment rate during the 24 months before treatment and in $t = 1$ as the employment rate during the 24 months after treatment, the impact of pretrial detention on the employment rate is -8.5 pp and -21.0 pp for the DiD estimation and the IV estimation, respectively. These point estimates represent a decrease in the likelihood of being employed of between 18.4% (8.5/46.3) and 46.2% (21.0/45.5). All these point estimates are statistically significant at the 1% level.

For the impact of pretrial detention on the average monthly wage, Table 8 reports

Table 7: Effect of pretrial detention on employment rate

	OLS (1)	DiD Panel (2)	IV (3)
Six months of employment			
Petrial Detention	-0.120***	-0.098***	-0.178**
	(0.003)	(0.003)	(0.084)
R^2	0.30	0.76	0.32
Observations	142,419	285,358	68,814
Mean of dependent variable	0.437	0.465	0.450
Two years of employment			
Petrial Detention	-0.112***	-0.085***	-0.210***
	(0.003)	(0.003)	(0.075)
R^2	0.29	0.75	0.29
Observations	126,547	253,550	61,479
Mean of dependent variable	0.441	0.463	0.455

Notes: This table presents the results for the impact of pretrial detention on employment rate, considering the panel DiD and the IV models (using the sample described in the note for Table 2). The results are presented for two separate time horizons: the average for the six months after verdict and the average for the two years after verdict. In order to group the results of a number of specifications into one table, only the point estimates and standard errors (in parentheses) for the parameter of interest are presented. Column (1) presents the OLS results (Eq. 1) as a reference point. Column (2) presents the panel DiD model (Eq. 2) and Column (3) presents the results from the IV model (Eq. 6). The mean of the dependent variable is calculated only considering the control group. Statistical significance at 1%, 5%, and 10% is indicated by ***, **, and *, respectively.

a short-term negative effect of pretrial detention of $-32,227$ CLP and $-92,635$ CLP (about 50 USD and 150 USD) for the DiD estimation (Column 2) and the IV estimation (Column 3), respectively. These point estimates represent a decrease in an individual's average monthly wage of 20.0% ($32,227/161,095$) and 56.7% ($92,635/163,293$). In terms of mid-term effects, the impact of pretrial detention on the average monthly wage is $-32,176$ CLP (DiD model) and $-90,647$ CLP (IV model), which represents a decrease of 20% ($32,176/160,170$) and 53.1% ($90,647/170,560$). All these point estimates are statistically significant at the 1% level.

Unlike the employment dynamics, in the case of monthly average wage there is evidence of a non-parallel trend in pretreatment evolution (see Figure 7).[17] To address this threat to DiD identification strategy, in Table 14 (Appendix C) we present the panel DiD estimation controlling for individual specific linear trends. We present the results of this alternative specification for employment and monthly wages. As expected, there is no

[17] A discussion on the relevance of the pretreatment employment evolution can be found in Loeffler (2018).

relevant difference when comparing the point estimates of the employment models with and without these individuals' trends as a covariates. On the contrary, there is a clear difference when comparing the point estimates of the monthly wage models with and without these individuals' trends as a covariate. In particular, we see that this violation of parallel trend could explain 25% of the point estimate that we find in column 2 (Table 8). That said, this issue does not affect the identification strategy for the IV estimation.

Two more aspects illustrated by these tables are worth highlighting. First, the magnitudes are very similar if we compare short-term to mid-term effects. The mid-term effect estimation, however, requires more months of data posttreatment. We therefore have a smaller sample and the magnitudes for mid-term effects must be taken with a note of caution. Indeed, as shown in Section 5.2, when we use the same sample – and specification – to estimate the effect of pretrial detention for different periods after treatment, we find a clear reduction in the effect on employment over time in the point estimates. This is less clear in the case of wages. Second, in the cases of both employment and wages, OLS estimations present larger point estimates relative to DiD estimations . This difference in magnitude is consistent with the fact that, unlike the DiD models, the OLS estimation does not control for unobserved variables that are stable through time and affect labor market outcomes. The comparison between the OLS estimation and the IV estimation is less clear because the IV estimation is based on a different sample (as a result of the restrictions required to use the judge severity instrument) and identifies the causal effect for the compliers (i.e., the LATE).

Table 8: Effect of pretrial detention on average wage

	OLS (1)	DiD Panel (2)	IV (3)
Six months of wages			
Petrial Detention	-42,380***	-32,227***	-92,635**
	(1,178)	(1,337)	(38,019)
R^2	0.56	0.87	0.58
Observations	142,419	285,358	68,814
Mean of dependent variable	158,304	161,095	163,293
Two years of wages			
Petrial Detention	-41,315***	-32,176***	-90,647**
	(1,230)	(1,385)	(46,093)
R^2	0.51	0.85	0.52
Observations	126,547	253,550	61,479
Mean of dependent variable	164,986	160,170	170,560

Notes: This table presents the results for the impact of pretrial detention on monthly wages, considering the panel DiD and the IV models (using the sample described in Table 2). The results are presented for two time horizons: the average for the six months after verdict, and the average for the two years after verdict. In order to group the results of a number of specifications in one table, only the point estimates and standard errors (in parentheses) for the parameter of interest are presented. Column (1) presents the OLS results (Eq. 1), which is a useful reference point. Column (2) presents the panel DiD model (Eq. 2) and Column (3) presents the results from the IV model (Eq. 6). The mean of the dependent variable is calculated only considering the control group. Statistical significance at 1%, 5%, and 10% is indicated by ***, **, and *, respectively.

5.2 General Approach (event study)

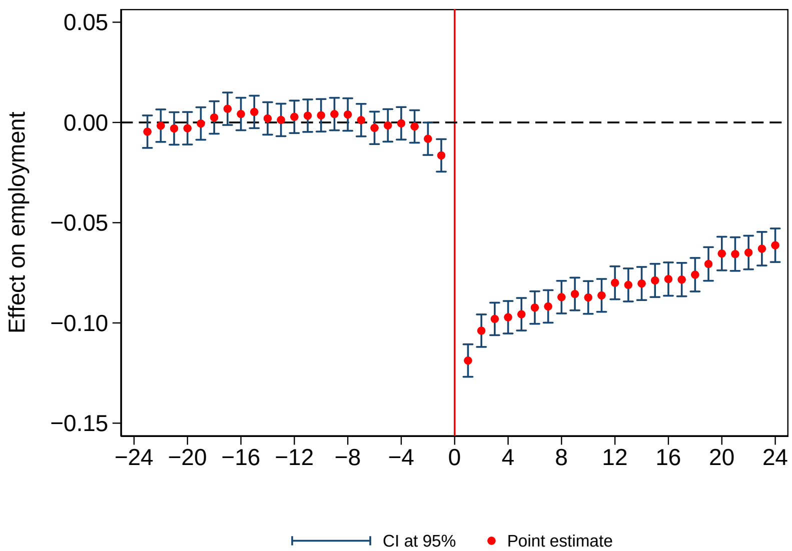
Figures 6 and 7 present the estimations of equation (3) – that is, the panel DiD model that allows us to estimate an effect for each period for the impact of pretrial detention on employment rates and average monthly wages. The dynamics of these two plots can be summarized in three findings.

First, for employment rates the empirical strategy does not find a statistically significant effect before treatment,[18] which is consistent with the parallel trend condition. Because pretrial detention occurs at different times for different individuals, it is unlikely that another treatment (i.e., policy) occurs at the same time as pretrial detention for all individuals. And as the relevant test to ensure the identification of the causal parameter in the DiD model is the verification of the pretreatment parallel trends assumption, Figure 6 supports the idea that the DiD estimates can be interpreted as a causal effect. For average monthly wages, there is clear evidence that the parallel trend condition is not

[18] There is a point estimate that is statistically significant but quantitatively irrelevant in the last period before treatment

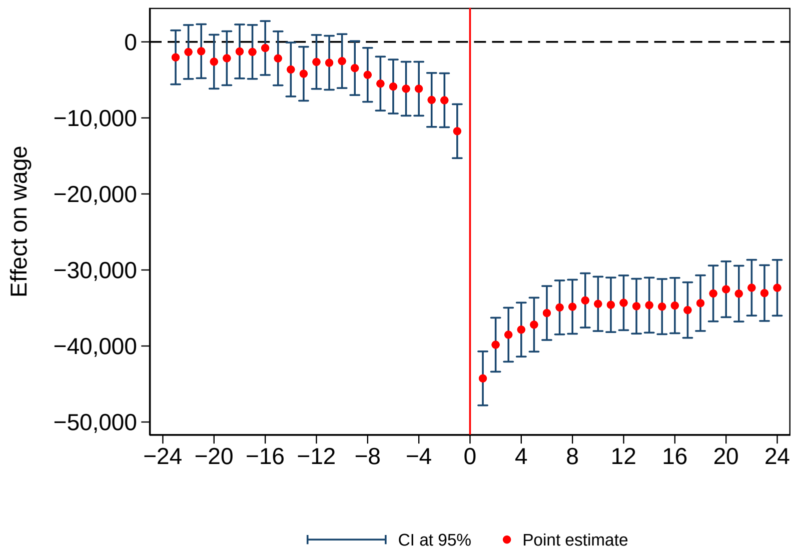
met (Figure 7). For this reason, we also estimate that model including a linear trend for treated as a covariate. Second, there is a statistically significant effect posttreatment, the magnitudes of which are in line with the point estimates presented in Tables 7 and 8 at the beginning of this section. Third, and finally, there is a clear reduction in the point estimates in the case of employment over time, but the trend is less clear for wages. That said, for both measures, the effect is still present in the average outcome measured 18 to 24 months after treatment.

Figure 6: Impact of Pretrial Detention on Employment Rate (General Approach)



Notes: This figure shows the point estimates and their 95% confidence intervals for the effect of pretrial detention on employment rate by estimating Equation 3, considering the estimation sample described in Table 2.

Figure 7: Impact of Pretrial Detention on Average Monthly Wage (General Approach)



Notes: This figure shows the point estimates and their 95% confidence intervals for the effect of pretrial detention on average monthly wage by estimating Equation 3, considering the estimation sample described in Table 2.

5.3 Robustness Analysis

In Appendix D we present a set of results to study the robustness of our main conclusions to alternative specifications. We begin our robustness checks by estimating alternative DiD models – namely, a cross-sectional DiD model and a matching DiD model(Heckman et al. (1997)). Tables 15 and 16 (in Appendix D.1) show that our point estimates are very stable across the alternative specifications for DiD models.

Our second robustness analysis is to estimate our models with a different estimation sample. Here, we consider crimes for which more than 5% of cases receive pretrial detention rather than the 10% threshold used in our main specification. As shown in Tables 17 and 18 (in Appendix D.2), the sample size is increased because of the way it is constructed, but the point estimates are very similar to the values presented in Tables 7 and 8.

Our final robustness analysis is to estimate the DiD model considering the estimation sample used for the IV estimation. Here, given that the instrument construction requires that only courts with more than three cases per day on average are considered, we cannot use the full estimation sample. This exercise is presented in Tables 19 and 20 (in Appendix D.3). The tables clearly show the difference s between the IV and the DiD point estimates are not due to sample differences. Thus, they should be because of the heterogeneous impact of the treatment and the local nature of the causal effect that is shown by the IV estimation.

5.3.1 Heterogeneity

In this subsection we explore two dimensions for the heterogeneous impact of pretrial detention on labor outcomes: how the impact of pretrial detention depends on its length, and the effect of pretrial detention by restricting the sample to those individuals who did not receive a custodial sentence as a result of the verdict of their trial. In our main specification, we do not restrict the sample based on trial verdict.

The impact by time imprisoned

Using a panel DiD model, similar to the one used to estimate the average effect, we can study whether the effect of pretrial detention on labor outcomes increases with the length of the pretrial detention. To study this heterogeneity, we extend the model presented in Equation (2), defining terciles ($T_i \in \{1, 2, 3\}$) for the distribution of days detained pretrial. Note that the amount of time spent in pretrial detention varies vastly over terciles. The first tercile are imprisoned for 35 days on average, whereas it rises to 122 for the second tercile and 296 for the third. Then, the estimated model is as follows:

$$\begin{aligned} Y_{i,t} = \alpha \mathbb{1}[t=1] + \beta_1^p PreTrial_i * \mathbb{1}[T_i = 1] * \mathbb{1}[t=1] + \beta_2^p PreTrial_i * \mathbb{1}[T_i = 2] * \mathbb{1}[t=1] + \\ \beta_3^p PreTrial_i * \mathbb{1}[T_i = 3] * \mathbb{1}[t=1] + \gamma' X_{it}^p + \omega_i + \epsilon_{it},\ t \in \{0, 1\}. \quad (7) \end{aligned}$$

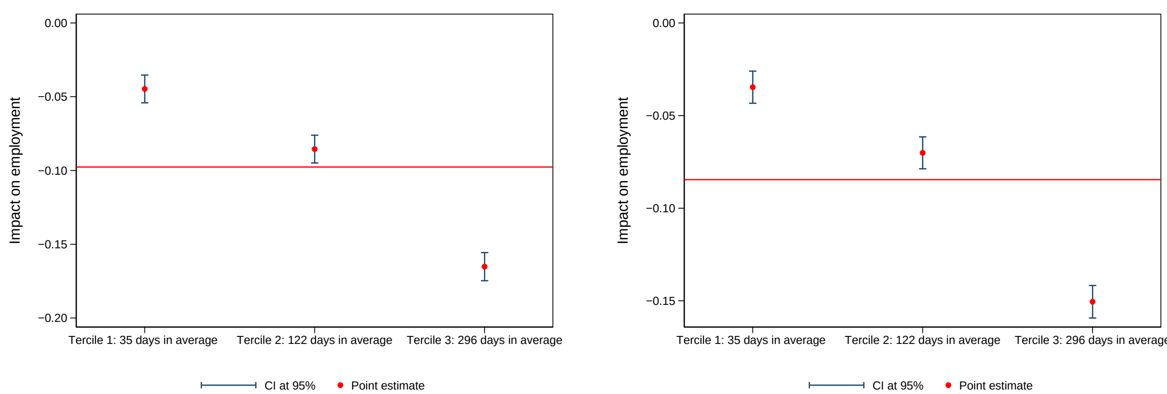
The estimated parameters (β_1^p, β_2^p, and β_3^p) are presented in Figures 8 and 9. The figures show the average treatment effect for the treated individuals by tercile. As a reference point, we also include the average treatment effect for the entire estimated sample (shown by the red line), and the point estimates presented in Tables 7 and 8. In all cases, for both the employment rate and average monthly wage, for both short and mid term, there is a clear step gradient pattern for the magnitudes of the treatment effect as individuals spend more time in pretrial detention. That said, for the three terciles defined for this approach the effect is statistically significant.

Specifically, the short-term effects on employment rate for the first, second, and third terciles are -4.5 pp, -8.5 pp, and -16.5 pp, respectively. Regarding the mid-term effects, the numbers are -3.5 pp, -7.0 pp, and -15.1 pp. Thus, the mid-term effect for the third tercile represents a decrease of 33% (15.1/46.3). For average monthly wages, the corresponding short-term effects are $-17,686$ CLP, $-26,810$ CLP, and $-52,900$ CLP. These numbers are equal to $-17,650$, $-25,213$, and $-54,511$ in the mid term. The effect on the third tercile represents a decrease of 34% ($54,511/160,170$).

Figure 8: Effect of Pretrial Detention on Employment Rate by the Duration of Pretrial Detention

(a) short-term effect: 6 months

(b) mid-term effect: 24 months



Notes: These figures show the point estimates and their 95% confidence intervals for the effect of pretrial detention on employment rate by estimating model for the three terciles of pretrial detention duration (Equation 7), considering the estimation sample described in Table 2.

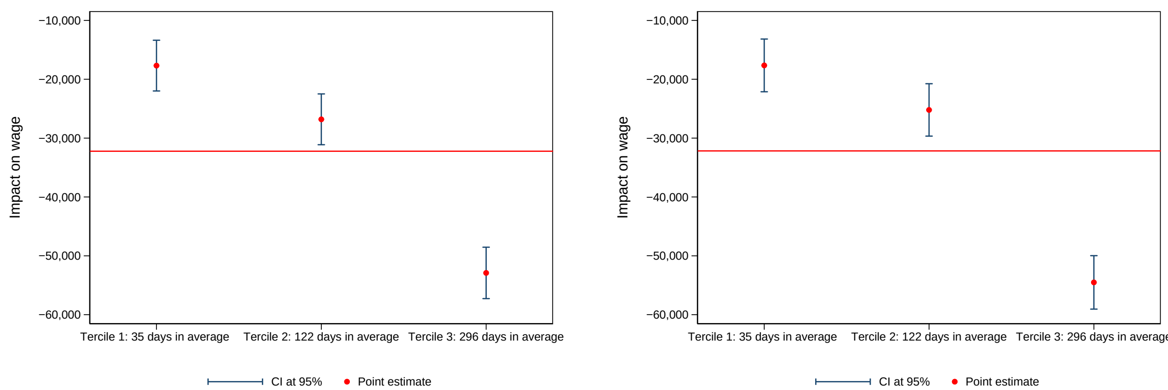
The impact for individuals released after the final trial verdict

Given that pretrial detention may affect the probability of post-verdict incarceration (discussed in the Section 6), it is more challenging to estimate the causal effect of pretrial detention on labor outcomes for individuals not incarcerated as a result of their final trial verdict. That said, in Appendix E.1 we show that this potential source of bias can be overcome by the panel DiD approach under reasonable assumptions, which is not the

Figure 9: EFFECT OF PRETRIAL DETENTION ON AVERAGE MONTHLY WAGE BY THE DURATION OF PRETRIAL DETENTION

(a) SHORT-TERM EFFECT: 6 MONTHS

(b) MID-TERM EFFECT: 24 MONTHS



Notes: These figures show the point estimates and their 95% confidence intervals for the effect of pretrial detention on average monthly wage by estimating model for the three terciles of pretrial detention duration (Equation 7), considering the estimation sample described in Table 2.

case for the IV estimation. Thus, to estimate the effect for this specific group, we only consider the panel DiD estimation.

Tables 21 and 22 in Appendix E present the results of this DiD estimation. As the tables show, the magnitude of the point estimates is between one-third and one-half lower when we restrict the sample to those individuals who were released from pretrial detention after the final verdict. These differences can be explained by the heterogeneity in the treatment effect (given that they use different samples) or by the fact that the treatment is different between the two empirical settings. In the main specification we allow for an impact of pretrial detention through its effect on increasing the probability of a post-verdict custodial sentence. This point will be discussed in the next section.

6 Mechanisms

There are several different mechanisms that may explain our results, we focus on three. Firstly, it could be that the impact of pretrial detention on labor outcomes is the general and natural effect of spending time outside of the labor market during the trial, and its lasting effect on future (post-verdict) labor market performance. We call this explanation the *labor market hypothesis*; the effect here is explained, for example, because individuals are fired during pretrial detention and post-verdict they have problems in finding a new job, as it were the case for any poor individual who loses his job. In this case, the pretrial detention only impacts future labor outcome through its impact on contemporaneous (during trial) employment participation. Secondly, it could be that pretrial detention carries an extra and specific impact on labor outcomes due to incarceration itself. For example, in the hiring process previously incarcerated people are discriminated. We call this explanation the *social stigma hypothesis*: the reason for the individuals absence from the labor market is relevant, not just their non-participation. Finally, it could be that the effect of pretrial detention on post-verdict labor market outcomes is due to its positive effect on the probability of post-verdict incarceration, and we call this explanation the *labor incapacitation hypothesis*.

6.1 The relevance of the labor market hypothesis

We assess the quantitative relevance of the labor market hypothesis in different ways. First, we compare the effect of pretrial detention to the effect of losing a job due to a negative shock beyond the individual's control. Specifically, we compare our pretrial detention impacts to the effect of losing a job due to a firm bankruptcy. To estimate the effect of a firm bankruptcy on wages and employment we use both the treatment and control group samples are used, but we only consider labor outcome data before prosecution (i.e. before our treatment). We divide the period before the beginning of prosecution into three subperiods: between 17 and 24 months before prosecution (treating this as the period before the firm bankruptcy), between 9 and 16 months before prosecution (treating this as the period during which the firm would go bankrupt), and between 1 and 8 months before prosecution (treating this as the period after the potential bankruptcy, i.e., after the individual's job loss). In this context, treated individuals are those who were working at firm j for 17 months before prosecution, and firm j went bankrupt between 9 and 16 months before the prosecution. Naturally, the control group is composed of individuals who were working at firm j for 17 months before prosecution, but firm j did not go bankrupt between 9 and 16 months before the prosecution. As in our panel DiD specification, we use data on wages and employment pre- and posttreatment –namely, labor outcome averages between 17 and 24 months before prosecution and between 1 and 8 months before prosecution. In order to generate a fair comparison, we replicate our panel DiD estimation on the effect of pretrial detention on labor outcomes, considering the six months before and after the prosecution, but we restrict the sample to those individuals who were working one month before the beginning of prosecution, as we did in the firm bankruptcy estimation.

Tables 9 and 10 present the results of this exercise for employment rate and average monthly wages. In the case of the employment rate, Table 9 shows that the firm bankruptcy impact is about one-half of the pretrial detention effects. Table 10 shows that the magnitude of the pretrial detention effect is much more relevant than the magnitudes of the impact of firm bankruptcy, which is not statistically significant. It should be noted that these point estimates are different from the estimates reported in Tables 7 and 8 because we are attempting to compare the effects of these two treatments and we therefore restrict the sample to those individuals who were working the month before the beginning of prosecution.

Our second approach to assessing the quantitative relevance of the labor market mechanism hypothesis is to take advantage of the fact that we have a firm ID. We can therefore compare the monthly probability of remaining employed at the same firm pre- and posttreatment between control and treatment groups. We present these probabilities in Figure 10, where the triangles represent the probabilities for the treated group and the circles represent the probabilities for the control. Note that almost all of the points in figure 10 show the probability of remaining employed at the firm where the individual was working during the previous month. The first triangle and circle after verdict, however, cannot refer to the firm where the individual was employed the previous month because they were being detained pretrial. In fact, they represent the probability of an individual keeping the job they held immediately before prosecution. As this figure shows, the dynamics of these probabilities are very similar pre-prosecution and posttrial, with the exception of the first value after the trial. This means that, conditional on having a job the month after the verdict (i.e., after being detained pretrial for many of the treated individuals), the difference in job stability between the control and treatment groups is very similar after and before the judicial process.

This is indirect evidence that the labor market mechanism hypothesis is playing a relevant role. It does not seem plausible that the negative consequences related to

Table 9: A COMPARISON BETWEEN THE EFFECT ON THE EMPLOYMENT RATE OF A FIRM BANKRUPTCY VERSUS THE EFFECT OF PRETRIAL DETENTION

	OLS (1)	DiD Panel (2)
Pretrial Detention	-0.178***	-0.164***
	(0.005)	(0.005)
R^2	0.17	0.68
Observations	67,533	135,274
Mean of dependent variable	0.644	0.725
Firm bankruptcy	-0.102***	-0.083***
	(0.027)	(0.027)
R^2	0.16	0.68
Observations	69,139	138,646
Mean of dependent variable	0.627	0.696

Notes: This table shows the point estimates and standard errors (in parentheses) for the effect of pretrial detention and firm bankruptcy on employment rate by estimating OLS and panel DiD models. To estimate the effect of a firm bankruptcy, we use data for both the treatment and control groups from the period before the beginning of the prosecution. More specifically, we divide this period into three subperiods: between 17 and 24 months before prosecution (treating this as the period before the firm bankruptcy), between 9 and 16 months before prosecution (treating this as the period during which the firm would go bankrupt), and between 1 and 8 months before prosecution (treating this as the period after the potential bankruptcy, i.e., after the individualâ€™s job loss). Treated individuals are therefore those who were working at firm j for 17 months before the prosecution, and firm j went bankrupt between 9 and 16 months before the prosecution. Naturally, the control group is composed of individuals who were working at firm j for 17 months before prosecution, but firm j did not go bankrupt between 9 and 16 months before the prosecution. To have comparable results between the two models (with different treatments), in the case of pretrial detention we also restrict the sample to the individuals who were working the month before treatment. Statistical significance at 1%, 5%, and 10% is indicated by ***, **, and *, respectively.

pretrial detention disappear one month after leaving prison. Note, however, that this is conditional on being employed after leaving prison due to pretrial detention.

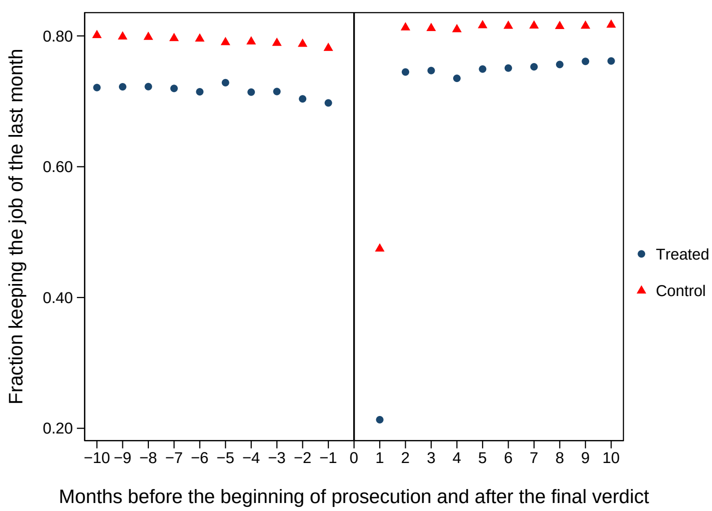
Taken together, the results of these two exercises suggest that being forced out of the labor market for a certain period plays a relevant role in explaining the impact of pretrial detention on post-verdict labor outcomes for accused individuals.

Table 10: A comparison between the effect on monthly wages of a firm bankruptcy versus the effect of pretrial detention

	OLS (1)	DiD Panel (2)
Pretrial Detention	-66,900***	-54,222***
	(2,373)	(2,497)
R^2	0.58	0.87
Observations	67,533	135,274
Mean of dependent variable	259,100	275,451
Firm bankruptcy	-29,386**	-13,227
	(12,417)	(15,192)
R^2	0.54	0.86
Observations	69,139	138,646
Mean of dependent variable	238,968	253,750

Notes: This table shows the point estimates and standard errors (in parentheses) for the effect of pretrial detention and firm bankruptcy on average monthly wages by estimating OLS and panel DiD models. To estimate the effect of a firm bankruptcy we use data for both the treatment and control group from the period before the beginning of the prosecution. More specifically, we divide this period into three subperiods: between 17 and 24 months before prosecution (treating this as the period before the firm bankruptcy), between 9 and 16 months before prosecution (treating this as the period during which the firm would go bankrupt), and between 1 and 8 months before prosecution (treating this as the period after the potential bankruptcy, i.e., after the individualâ€™s job loss). Treated individuals are therefore those who where working at firm j for 17 months before prosecution, and firm j went bankrupt between 9 and 16 months before the prosecution. Naturally, the control group is composed by individuals who where working at firm j for 17 months before prosecution, and firm j did not go bankrupt between 9 and 16 months before the prosecution. To have comparable results between the two models (with different treatments) in the case of pretrial detention we also restrict the sample to the individuals who were working the month before treatment. Statistical significance at 1%, 5%, and 10% is indicated by ***, **, and *, respectively.

Figure 10: Probability of Retaining Previous Employment after Pretrial Detention



Notes: This figure shows the probability of remaining employed at the same firm as the previous month, for the control and treatment groups. Thus, it is only calculated for those individuals who were working the previous month. When the horizontal axis is negative it represents the months before the beginning of the prosecution, and when the horizontal axis is positive it represents the months after the final verdict. When the horizontal axis equals zero it refers to a period that is a different length for different individuals, lasting from the beginning of prosecution to the end of trial, including the final verdict. The values of the dots in period "1" is equal to the probability of retaining employment in the same firm as the one in which the individual was employed one month before the beginning of prosecution.

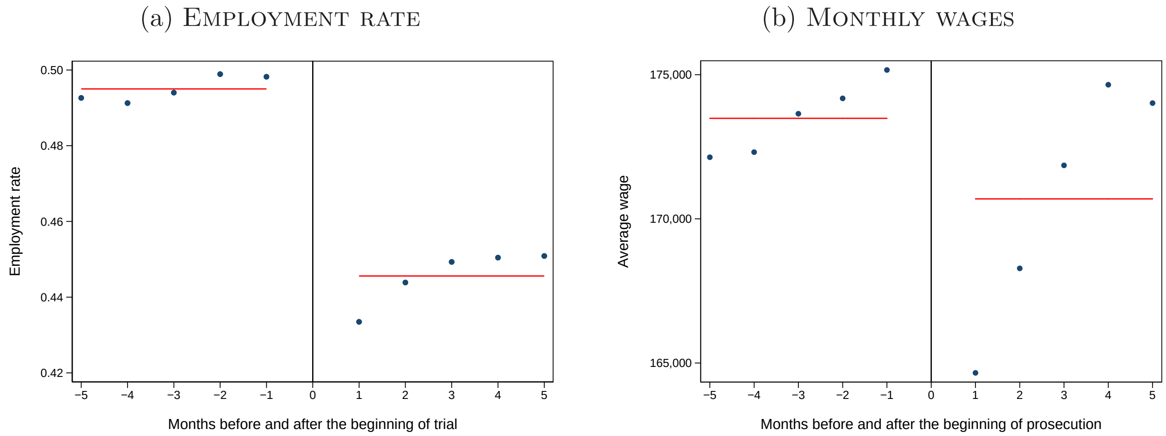
6.2 The relevance of the social stigma hypothesis

We do not have data that allows us to directly test the relevance of social stigma mechanism hypothesis; however, using our database we can empirically test whether being prosecuted (without pretrial detention) has some direct effect on labor market outcomes. Because both the control and treatment groups are prosecuted, this exercise by definition cannot explain the effect of pretrial detention on labor outcomes. It is, however, an indirect way to study whether the social stigma that may arise because of prosecution (or incarceration) plays a relevant role in the determination of labor outcomes.

The nature of our data means that we can only test the effect of prosecution using our control group sample because it is only individuals in the control group who face a *pure effect* of prosecution on labor outcome, as they are not incarcerated during the prosecution. This pure effect is what can be attributed to social stigma. Therefore, by construction and because the treatment group is not included in this exercise, the result cannot explain the differences in outcomes between control and treatment groups that we find due to pretrial detention.

That said, we study the impact of the start of the criminal prosecution on the employment rate and average monthly wages for the control group. As stated, we focus on control group because we want to isolate the effect of positive or negative signals relating to criminal culpability from the effect of being incarcerated. This is not possible if we consider individuals who spent time in prison due to a pretrial detention. Panel (a) in Figure 11 presents the employment rate for the months before and after the beginning of the prosecution. It shows a reduction of between 4 pp and 5 pp in the employment rate right after the prosecution begins. Likewise, panel (b) in Figure 11 presents the average monthly wage for the months before and after the beginning of the prosecution. This panel shows that there is a reduction of about $7,000$ CLP in the average monthly wage immediately after the prosecution begins. Given that in these two cases incarceration was not a factor, it is reasonable to attribute the observed effects to factors that are specific to a criminal prosecution, and probably to the stigma related with being accused of a crime, rather than a forced absence from the labor market.

Figure 11: Impact of Judicial Decisions on Employment Rate (Pre- and post-prosecution beginning)



Notes: These figures show the effect of the beginning of the prosecution (panel a) and the final trial verdict (panel b) on the probability of being employed. These probabilities are only calculated for the control group. When the horizontal axis is negative, it represents the months before the beginning of the prosecution (panel a) or the months before the final trial verdict (panel b), and when the horizontal axis is positive, it represents the months after the beginning of prosecution (panel a) or the months after the final trial verdict (panel b).

6.3 The relevance of the labor incapacitation hypothesis

As shown in Dobbie et al. (2018) , Leslie and Pope (2017), and Stevenson (2018), pretrial detention may increase the probability of a guilty sentence. This mechanism may explain our results to the extent that pretrial detention can lead to post-verdict incarceration meaning a higher proportion of the treated group, relative to the control group, are unable to participate in a post-verdict labor market.

We therefore examine whether pretrial detention increases the probability of being given a custodial sentence, focusing on a Chilean context. Because we cannot implement the DiD approach in this case (as the dependent variable does not have a pretreatment value) and given the validity of our IV model, we address this question by estimating an IV linear model, where the dependent variable is an indicator function that takes the value of one when there is a custodial sentence, the endogenous variable is pretrial detention, and the instrument is the judge severity score.

The results of this exercise are presented in Table 11. As the table shows, our estimates are in line with previous literature in the sense that pretrial detention increases the probability of being given a custodial sentence, although the estimation of the effect of pretrial detention is not statistically significant (the p-value is 0.167). Regarding the magnitude, pretrial detention increases the probability of a custodial sentence by 32 pp. This very large magnitude suggests that the reason our estimates are not statistically significant is because of statistical power.

Table 11: The impact of pretrial detention on custodial sentence

	Custodial sanction
Pretrial detention	0.323
	(0.234)
Male	-0.016**
	(0.006)
non-Chilean citizen	-0.004
	(0.023)
Indigenous	0.020
	(0.021)
Crimes special laws	0.030**
	(0.012)
Drug law crimes	0.216***
	(0.008)
Homicides	0.197***
	(0.026)
Nonviolent thefts	0.150***
	(0.007)
Sex crimes	0.128***
	(0.008)
Thefts	0.132***
	(0.029)
Constant	-0.338
	(0.370)
Observations	68,940
Court-by-time fixed effects	Yes

Notes: This table reports the results for a 2SLS model that estimates the effect of pretrial detention on the probability of a final verdict that results in a custodial sentence. The instrumental variable used in this estimation procedure is the judge severity score described in Section 4.2. Robust standard errors are clustered at the judge level in parentheses. Statistical significance at 1%, 5%, and 10% is indicated by ***, **, and *, respectively.

Overall, the discussion on the possible explanations for our results suggests that all three mechanisms may play a role. This is consistent with the results shown in Figures 8 and 9. On the one hand, the effect of pretrial detention on labor outcomes increases with the amount of time spent incarcerated. If the social stigma due to incarceration and increase in the probability of a custodial sentence were the only relevant mechanisms, the actual time of incarceration would be less important. On the other hand, even in the cases of individuals who only spend a few days incarcerated due to pretrial detention (remember the first tercile spends an average of 296 days in pretrial detention), the effect of pretrial detention on labor outcomes has a relevant magnitude and is statistically significant. If stigma and the increase in the probability of a custodial sentence were not factors, then the effect of a few days of pretrial detention should be close to zero.

7 Conclusion

Pretrial detention may have a negative impact on many dimensions – psychological, socio-emotional, and economic – for both the individual and for society. Indeed, Ahumada et al. (2010) showed that the fiscal cost of pretrial detention in Chile in 2007 was 92.48 million USD. In this paper, we contribute to this discussion by estimating the negative impact of pretrial detention on labor market outcomes. One important limitation of our analysis is that – due to data limitations – we need to restrict our sample to those individuals who (pre-arrest) worked in the private formal sector.

The effects found on the employment rate and on average monthly wages are considerable and they last for at least two years, which is the maximum horizon we can study given our empirical strategy and data. Regarding mechanisms, our results suggest that the negative effect of pretrial detention is (at least) driven by the lasting effect of being absent from the labor market during the trial, the social stigma related to being accused of a crime, and the impact of pretrial detention on the probability of post-verdict incarceration.

A comprehensive evaluation of the impact that pretrial detention has on society is a highly complex endeavor that is beyond the scope of this paper. Because the justification of pretrial detention is generally to protect the criminal investigation and trial, and because society values these outcomes, the evidence presented in this paper is not enough to conclude that pretrial detention should not exist. There may very well be a benefit to society, related to pretrial detention protecting the progress of the criminal investigation, for example. Accordingly, one step further towards a comprehensive evaluation of pretrial detention would be to estimate the effect of eliminating pretrial detention on the investigative and prosecution processes.[19]

[19] An interesting take in this regard is the study of the impact of alternative precautionary measures, undertaken by Di Tella and Schargrodsky (2013).

That said, the large negative impact of pretrial detention that we find in this paper, which exists even for those who are eventually found innocent or whose crimes are not deemed worthy of a custodial sentence, invites us to consider two possible paths to facilitate further progress. On the one hand, we need better evidence on the real benefits of pretrial detention in order to compare these benefits with the costs identified in this paper. Only then can we make a judgment on whether the increase in the pretrial detention rate that we observed over the last decade is justified. On the other hand, we need to formulate effective policies that attenuate the negative effect on pretrial detention on post-verdict labor market outcomes. In this regard, our findings suggest that both the control and treatment groups keep the same labor dynamics pre- and posttrial if they return to work immediately after the conclusion of the trial (see Figure 10).[20] A possible avenue to attenuate the negative effect of pretrial detention on labor outcomes, therefore, would be to design public policies that support access to the labor market and finding an employment position immediately following the conclusion of trial proceedings. [21]

[20] This is in line with Engelhardt (2010) who points out the relevance of the duration of the job search after being released from prison. Also see Rauma and Berk (1987), who points out the relevance of post-prison unemployment insurance as a way to reduce the probability of recidivism.

[21] Visher and Kachnowski (2007) show that employment before prison, participation in job training during prison, strong family relationships, and few health problems are associated with finding employment after release.

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Appendix

A Reduced form estimations

Table 12: Reduced form estimations

	Wage		Employment	
	Six months	Two years	Six months	Two years
Judge severity IV	-34,549**	-37,795**	-0.066**	-0.088***
	(13,832)	(18,962.073)	(0.030)	(0.031)
Six months of wages	0.831***		0.000***	
	(0.010)		(0.000)	
Six months of employment	-67,244***		0.464***	
	(3,417)		(0.005)	
Two years of wages		0.883***		0.000***
		(0.014)		(0.000)
Two years of employment		-53,571***		0.531***
		(4,896)		(0.005)
Male	14,566***	18,529***	0.037***	0.044***
	(2,209)	(2,754)	(0.005)	(0.005)
non-Chilean citizen	-19,260*	-27,970**	-0.046**	-0.068***
	(9,949)	(11,145)	(0.021)	(0.020)
Indigenous	-9,553	-7,973	-0.009	-0.002
	(7,814)	(8,434)	(0.016)	(0.015)
Days in judicial process	18***	6	-0.000***	-0.000***
	(4.656)	(5)	(0.000)	(0.000)
Crime severity	-602***	-293***	-0.001***	-0.001***
	(60)	(68)	(0.000)	(0.000)
Fixed term contract	-20,737***	3,826*	-0.056***	-0.002
	(1,783)	(2,043)	(0.003)	(0.003)
Sector = Mining	51,996***	35,743	-0.023	-0.018
	(18,830)	(23,457)	(0.018)	(0.017)
Sector = Manufacturing	9,307***	9,020***	0.020***	0.020***
	(2,869)	(3,308)	(0.007)	(0.007)
Sector = Electricity-Gas-Water	59,901***	34,888*	0.073**	0.034
	(20,553)	(19,879)	(0.031)	(0.028)
Sector = Construction	-1,079	-7,519***	-0.005	-0.021***
	(2,327)	(2,441)	(0.006)	(0.005)
Sector = Commerce	10,209***	18,184***	0.016***	0.031***
	(2,548)	(2,857)	(0.006)	(0.006)
Sector = Services	9,465***	13,167***	0.012**	0.012**
	(2,280)	(2,452)	(0.006)	(0.005)
Sector = Transportation-Communication	5,235*	9,719***	0.013*	0.012*
	(3,158)	(3,396)	(0.007)	(0.007)
Firm size = Small	6,719***	3,267	0.008	0.007
	(2,265)	(2,443)	(0.005)	(0.005)
Firm size = Medium	7,559***	2,881	0.013***	0.011**
	(2,408)	(2,509)	(0.005)	(0.005)
Firm size = Big	9,671***	3,243	0.022***	0.021***
	(2,133)	(2,256)	(0.005)	(0.004)
Constant	57,120***	33,501***	0.206***	0.136***
	(3,729)	(4,449)	(0.009)	(0.009)
Observations	68,814	61,479	68,814	61,479
Court-by-time fixed effects	Yes	Yes	Yes	Yes
Cluster at judge level	Yes	Yes	Yes	Yes

Notes: This table reports the reduced form estimation, namely, the effect of judge severity on labor outcomes. The IV is the judge severity measure, which is estimated following the procedure described in Subsection 4.2. In Column (1) we control for employment outcomes during the 6 months before the prosecution begins, and in Column (2) we control for employment outcomes during the 24 months before the prosecution begins. The model is estimated on the sample described in the notes for Table 2. Regression includes year interacted with court fixed effects. Robust standard errors are clustered at the judge level in parentheses. Statistical significance at 1%, 5%, and 10% is indicated by ***, **, and *, respectively.

B Monotonicity test

Table 13: Monotonicity test: First-stage estimations for different crime types

Panel A: *First-Stage Impact of Judge severity, by Crime*

	Special laws	Against public faith	Nonviolent thefts	Sex crimes	Drug law	Homicides & Thefts
Judge severity IV	0.092**	0.457***	0.375***	0.437***	0.184***	0.730***
	(0.047)	(0.076)	(0.081)	(0.126)	(0.061)	(0.106)
Observations	9,725	11,365	11,540	5,822	17,550	12,938

Panel B: *First-Stage Impact of Judge severity, by Reverse Sample Calculation for Crimes*

	Special laws	Against public faith	Nonviolent thefts	Sex crimes	Drug law	Homicides & Thefts
Judge severity IV	0.072*	0.420***	0.203**	0.422***	0.160***	0.717***
	(0.042)	(0.072)	(0.086)	(0.124)	(0.058)	(0.118)
Observations	9,721	11,364	11,535	5,822	17,549	12,936

Notes: In panel A, this table reports first-stage results for the linear IV model that estimates the effect of pretrial detention on labor outcomes by crime severity. Panel B shows the estimations of the first-stage models, where the leave-out instrument is recalculated for different subgroups using all cases outside of these subgroups. The regressions in both panels are estimated using the sample as described in the notes of Table 2. The regressions include year interacted with court fixed effects. Robust standard errors clustered at the judge level in parentheses. Statistical significance at 1%, 5%, and 10% is indicated by ***, **, and *, respectively.

C Estimations considering individuals specific' linear trends

Table 14: PANEL DID ESTIMATIONS CONSIDERING INDIVIDUALS SPECIFIC' LINEAR TRENDS

	Wages		Employment	
	Six months	Two years	Six months	Two years
Petrial Detention	-21,827***	-24,489***	-0.078***	-0.089***
	(1,255)	(851)	(0.003)	(0.002)
R^2	0.73	0.61	0.52	0.36
Observations	1,712,148	6,664,425	1,712,148	6,664,425
Mean of dependent variable	158,321	166,121	0.437	0.440

Notes: This table present the estimations (by panel DiD) for the effect of pretrial detention on employment and monthly wages, considering individuals specific' linear trends. The regressions are estimated using the sample as described in the notes of Table 2. The regressions include year interacted with court fixed effects. Robust standard errors clustered at the judge level in parentheses. Statistical significance at 1%, 5%, and 10% is indicated by ***, **, and *, respectively.

D Robustness analysis: alternative specifications

D.1 Other Differences-in-differences models

In this subsection we present two other DiD models, the cross-sectional DiD model and the DiD matching model. The cross sectional DiD model, with set of covariates denoted by X_{it}^{cs}, considers the same set of control variables as the OLS, except that the year and month dummies are set at the beginning of the prosecution when $t = 0$ and at the time of the sentence when $t = 1$. The estimated model is given by:

$$Y_{i,t} = \alpha_0 + \alpha_1 PreTrial_i + \alpha_2 \mathbb{1}[t = 1] + \beta^{cs} PreTrial_i * \mathbb{1}[t = 1] + \gamma' X_{it}^{cs} + \epsilon_{it},\ t \in \{0, 1\}. \quad (8)$$

The DiD matching is an approach proposed by Heckman et al. (1997), which combines the matching estimation technique with the advantages of the panel DiD estimator. Let $Y_{i,t}(PreTrial_i)$ denote the potential outcome of individual i at time t, and $\Delta Y_{i,t}(PreTrial_i) = Y_{i,1}(PreTrial_i) - Y_{i,0}$ denote the potential increment in the outcome

Y between time $t = 0$ and $t = 1$. The average treatment effect (ATE) is therefore defined as $\beta^m = E[\Delta Y_{i,t}(1) - \Delta Y_{i,t}(0)]$.

For individuals who were treated we only observe $\Delta Y_{i,t}(1) = Y_{i,1} - Y_{i,0}$, and we impute $\Delta Y_{i,t}(0)$ using the matching procedure. More specifically, for each i treated ($PreTrial_i = 1$), we assign the set of matches $J_G(i)$ corresponding to the G nearest neighbors in the untreated group ($PreTrial_i = 0$) using the Mahalanobis metric, given the following covariates (X^m_{it}): gender, Chilean, and indigenous dummies, pretreatment average wage and employment rate (using 27 to 36 months before prosecution), location of the court (by region), type of prosecuted crime, and year and month of the sentence. We report estimates for $G = 3$. The imputed value for $\Delta Y_{i,t}(0)$, denoted by $\widehat{\Delta Y}_{i,t}(0)$, is the average difference outcome of those individuals in the set of matches, that is $\widehat{\Delta Y}_{i,t}(0) = \frac{1}{G}\sum_{j\in J_G(i)} \Delta Y_{j,t}$. Similarly, we can also assign to each untreated individual i ($PreTrial_i = 0$) the set of G nearest neighbors in the treated group $J_G(i)$. In this case, we observe $\Delta Y_{i,t}(0) = Y_{i,1} - Y_{i,0}$ and impute $\widehat{\Delta Y}_{i,t}(1) = \frac{1}{G}\sum_{j\in J_G(i)} \Delta Y_{j,t}$. In this setting, an estimate of the ATE is:

$$\hat{\beta}^m = \frac{1}{N_1 + N_0}\left(\sum_{i:PreTrial_i=1} \Delta Y_{i,t}(1) - \widehat{\Delta Y}_{i,t}(0) + \sum_{i:PreTrial_i=0} \widehat{\Delta Y}_{i,t}(1) - \Delta Y_{i,t}(0)\right), \tag{9}$$

where $N_1 = \sum_{i:PreTrial_i=1} 1$ is the total number of treated individuals, and $N_0 = \sum_{i:PreTrial_i=0} 1$ is the total number of non-treated individuals.

Because the DiD matching procedure finds controls by minimizing the distance among individuals' observables, to the extent that these observables explain why some individuals are treated and other are not, this approach has the advantage that it can be more robust to the existence of time trends for different outcomes, which are function on these observables – namely, if these observables have specific effect on the time trend of the outcome, this is going to be properly controlled by the approach. In other words, a DiD matching approach can be thought as a non-parametric approach to control for time trends that are functions of observables.

The results from these two approaches (the cross-sectional DiD and DiD matching models) are presented in Tables 15 (for employment rate) and 16 (for average monthly wages). To facilitate a comparison with the main results, we also include the point estimates from the panel DiD (also reported in Tables 7 and 8). As these tables show, the point estimates are very stable across different DiD models.

Table 15: Effect of pretrial detention on employment rate (different DiD models)

	Cross-Section (1)	Panel (2)	Matching (3)
Six months of employment			
Petrial Detention	-0.099***	-0.098***	-0.082***
	(0.004)	(0.003)	(0.003)
R^2	0.14	0.76	.
Observations	284,838	285,358	142,418
Mean of dependent variable	0.465	0.465	0.437
Two years of employment			
Petrial Detention	-0.089***	-0.085***	-0.079***
	(0.003)	(0.003)	(0.003)
R^2	0.26	0.75	.
Observations	253,094	253,550	126,546
Mean of dependent variable	0.464	0.463	0.441

Notes: This table presents the results for the impact of pretrial detention on employment rate, considering different DiD models (using the sample described at Table 2). The results are presented for two time horizons: the average for the six months after the final trial verdict and the average for the two years after the final trial verdict. Column (1) presents the cross sectional DiD model, Column (2) presents the panel DiD model, and Column (3) presents the results from the DiD matching model. The mean of the dependent variable is calculated by only considering the control group. Statistical significance at 1%, 5%, and 10% is indicated by ***, **, and *, respectively.

Table 16: Effect of pretrial detention on monthly wages (different DiD models)

	Cross-Section (1)	Panel (2)	Matching (3)
Six months of wages			
Petrial Detention	-31,428***	-32,227***	-27,529***
	(1,679)	(1,337)	(1,431)
R^2	0.36	0.87	.
Observations	284,838	285,358	142,418
Mean of dependent variable	161,072	161,095	158,304
Two years of wages			
Petrial Detention	-32,275***	-32,176***	-30,638***
	(1,493)	(1,385)	(1,502)
R^2	0.46	0.85	.
Observations	253,094	253,550	126,546
Mean of dependent variable	160,199	160,170	164,986

Notes: This table presents the results for the impact of pretrial detention on monthly wages, considering different DiD models (using the sample described at Table 2). The results are presented for two time horizons: the average for the six months after the final trial verdict and the average for the two years after the final trial verdict. Column (1) presents the cross sectional DiD model, Column (2) presents the panel DiD model, and Column (3) presents the results from the DiD matching model. The mean of the dependent variable is calculated by only considering the control group. Statistical significance at 1%, 5%, and 10% is indicated by ***, **, and *, respectively.

D.2 Estimations with a more comprehensive definition for a severe crime

Table 17: EFFECT OF PRETRIAL DETENTION ON EMPLOYMENT RATE (WITH THE THRESHOLD FOR SEVERE CRIME AT 5% DETAINED PRETRIAL)

	OLS (1)	DiD Panel (2)	IV (3)
Six months of employment			
Petrial Detention	-0.127***	-0.108***	-0.341***
	(0.003)	(0.003)	(0.125)
R^2	0.31	0.77	0.31
Observations	171,268	343,168	82,574
Mean of dependent variable	0.473	0.498	0.489
Two years of employment			
Petrial Detention	-0.118***	-0.093***	-0.318***
	(0.003)	(0.003)	(0.116)
R^2	0.29	0.75	0.27
Observations	151,469	303,486	73,305
Mean of dependent variable	0.474	0.492	0.490

Notes: This table presents the results for the impact of pretrial detention on employment rate, considering the panel DiD and the IV models (using a larger sample where a severe crime is defined as one where 5% of cases receive pretrial detention). The results are presented for two time horizons: the average for the six months after final trial verdict and the average for the two years after final trial verdict. In order to group the results of a number of specifications in one table, we only present the point estimates and standard errors (in parentheses) for the parameter of interest. Column (1) presents the OLS results (Eq. 1), which is a useful reference point. Column (2) presents the panel DiD model (Eq. 2), and Column (3) presents the results from the IV model (Eq. 6). The mean of the dependent variable is calculated by only considering the control group. Statistical significance at 1%, 5%, and 10% is indicated by ***, **, and *, respectively.

Table 18: Effect of pretrial detention on monthly wages (with the threshold for severe crime at 5% detained pretrial)

	OLS (1)	DiD Panel (2)	IV (3)
Six months of wages			
Petrial Detention	-46,230***	-36,841***	-160,078***
	(1,323)	(1486)	(57,575)
R^2	0.58	0.88	0.59
Observations	171,268	343,168	82,574
Mean of dependent variable	177,861	179,963	185,440
Two years of wages			
Petrial Detention	-45,788***	-37,478***	-192,334***
	(1,354)	(1540)	(69,131)
R^2	0.53	0.86	0.53
Observations	151,469	303,486	73,305
Mean of dependent variable	183,519	176,957	191,941

Notes: This table presents the results for the impact of pretrial detention on average monthly wages, considering the panel DiD and the IV models (using a larger sample where a severe crime is defined as one where 5% of cases receive pretrial detention). The results are presented for two time horizons: the average for the six months after final trial verdict and the average for the two years after verdict. In order to group the results of a number of specifications in one table, we only present the point estimates and standard errors (in parentheses) for the parameter of interest. Column (1) presents the OLS results (Eq. 1), which is a useful reference point. Column (2) presents the panel DiD model (Eq. 2), and Column (3) presents the results from the IV model (Eq. 6). The mean of the dependent variable is calculated by only considering the control group. Statistical significance at 1%, 5%, and 10% is indicated by ***, **, and *, respectively.

D.3 Estimations with the IV sample

Table 19: Effect of pretrial detention on employment rate (IV estimation sample)

	OLS (1)	DiD Panel (2)	IV (3)
Six months of employment			
Petrial Detention	-0.120***	-0.093***	-0.178**
	(0.004)	(0.004)	(0.084)
R^2	0.32	0.77	0.32
Observations	68,814	137,628	68,814
Mean of dependent variable	0.450	0.478	0.450
Two years of employment			
Petrial Detention	-0.113***	-0.082***	-0.210***
	(0.004)	(0.004)	(0.075)
R^2	0.29	0.76	0.29
Observations	61,479	122,958	61,479
Mean of dependent variable	0.455	0.475	0.455

Notes: This table presents the results for the impact of pretrial detention on employment rate, considering the panel DiD model (using the IV estimation sample). The results are presented for two time horizons: the average for the six months after final trial verdict and the average for the two years after verdict. In order to group the results of a number of specifications in one table, we only presented the point estimates and standard errors (in parentheses) for the parameter of interest. Column (1) presents the OLS results (Eq. 1), which is a useful reference point. Column (2) presents the panel DiD model (Eq. 2), and Column (3) presents the results from the IV model (Eq. 6). These column estimates are, by construction, the same as in Table 7 (Column (3)). The mean of the dependent variable is calculated only considering the control group. Statistical significance at 1%, 5%, and 10% is indicated by ***, **, and *, respectively.

Table 20: Effect of pretrial detention on monthly wages (IV estimation sample)

	OLS (1)	DiD Panel (2)	IV (3)
Six months of wages			
Petrial Detention	-41,536***	-30,492***	-92,635**
	(1,724)	(1958)	(38,019)
R^2	0.58	0.88	0.58
Observations	68,814	137,628	68,814
Mean of dependent variable	163,293	166,178	163,293
Two years of wages			
Petrial Detention	-40,537***	-31,087***	-90,647**
	(1,849)	(2055)	(46,093)
R^2	0.52	0.85	0.52
Observations	61,479	122,958	61,479
Mean of dependent variable	170,560	165,170	170,560

Notes: This table presents the results for the impact of pretrial detention on average monthly wages, considering the panel DiD model (using the IV estimation sample). The results are presented for two time horizons: the average for the six months after the final trial verdict and the average for the two years after the final trial verdict. In order to group the results of a number of specifications in one table, we only present the point estimates and standard errors (in parentheses) for the parameter of interest. Column (1) presents the OLS results (Eq. 1), which is useful as a reference point. Column (2) presents the panel DiD model (Eq. 2), and Column (3) presents the results from the IV model (Eq. 6). This column estimates are, by construction, the same as in Table 8 (Column (3)). The mean of the dependent variable is calculated by only considering the control group. Statistical significance at 1%, 5%, and 10% is indicated by ***, **, and *, respectively.

E The impact of pretrial detention for accused individuals who were released after verdict

E.1 Potential Selection Bias

If we focus our attention on cases where the verdict was not guilty or a non-custodial sentence was passed, and given the fact that our treatment may affect this status, we need to investigate to what extent selection bias could be an issue in this empirical setting.

To discuss potential selection bias, we use equation (2) but add two elements to the analysis. First, we allow for the fact that the effect of pretrial detention is heterogeneous across individuals (β_i), such that $\beta_i = \bar{\beta} + e_i$. Second, we introduce a selection equation $I_i = \mathbb{1}[\theta_0 + \theta_1 PreTrial_i - I_i^* > 0]$, where I_i is a binary variable that takes the value of one if the verdict is not guilty or a non-custodial sentence is passed (i.e., i belongs to our estimation sample) or zero otherwise. And I_i^* is a continuous variable, which is unobservable by the econometrician, that measures all the information that judges have to support their decision regarding whether or not to detain the individual pretrial. Note that this selection equation allows for the fact that pretrial detention decreases the probability of being part of our estimation sample ($\theta_1 < 0$), conditional on I_i^*. Combining these new elements with equation 2, we have:

$$E[Y_{it}|\omega_i, X_{it}, PreTrial_i, t, I_i = 1] = \alpha\mathbb{1}[t=1] + \bar{\beta}PreTrial_i * \mathbb{1}[t=1] + \gamma' X_{it} + \omega_i \tag{10}$$
$$+ E[\epsilon_{it}|\omega_i, X_{it}, PreTrial_i, t, I_i = 1] + PreTrial_i * \mathbb{1}[t=1]E[e_i|\omega_i, X_{it}, PreTrial_i, t, I_i = 1],$$

In this context, the unobserved component of the selection equation can be related to the principal equation in two ways.[22] In a first structure for the unobserved components, we allow that $I_i^* \not\perp\!\!\!\perp \epsilon_{it}$ (and $I_i^* \perp\!\!\!\perp e_{it}$). In this case $E[\epsilon_{it}|\omega_i, X_{it}, PreTrial_i, t, I_i = 1] \neq 0$ and can be written as $H(Pretrial_i)$ where H is an unknown function. Note, however, that in this context, equation (10) can be rewritten as:

$$E[Y_{it}|\omega_i, X_{it}, PreTrial_i, t, I_i = 1] = \alpha\mathbb{1}[t=1] + \bar{\beta}PreTrial_i * \mathbb{1}[t=1] + \gamma' X_{it} + \tilde{\omega_i},$$

where $\tilde{\omega_i} = \omega_i + H(Pretrial_i)$. Therefore, this structure for the unobserved components does not generate an identification issue in the context of the panel DiD model. It does, however, generate an endogeneity issue if we estimate $\bar{\beta}$ using an IV approach, as in equation (6), a specification that only uses the cross-sectional nature of the database.[23]

In a second structure for the unobserved components, we allow that $I_i^* \not\perp\!\!\!\perp e_{it}$ (but

[22] We are assuming that – conditional on fixed effects – the only source of endogeneity is the selection bias – namely $E[\epsilon_{it}|\omega_i, X_{it}, PreTrial_i, t] = 0$. Moreover, without loss of generality, we make the following normalization: $E[e_i|\omega_i, X_{it}, PreTrial_i, t] = 0$, which means that $\bar{\beta}$ is the ATE of pretrial detention for the full sample (i.e., $i|I_i = 0$ *or* $I_i = 1$).

[23] If a selection bias did exist, it would bias our results toward to zero, given that those who are not part of the treatment group because they were sentenced to prison are probably already less likely to be employed or to have a high salary. Hence, this selection process would artificially increase the average monthly wage and the employment rate of the treatment group.

$I_i^* \perp\!\!\!\perp \epsilon_{it}$), in this case $E[e_{it}|\omega_i, X_{it}, PreTrial_i, t, I_i = 1] \neq 0$. Given this structure, equation (10) can be written as:

$$E[Y_{it}|\omega_i, X_{it}, PreTrial_i, t, I_i = 1] = \alpha \mathbb{1}[t = 1] + (\bar{\beta} + E[e_{it}|I_i = 1])PreTrial_i * \mathbb{1}[t = 1] \\ + \gamma' X_{it} + \omega_i,$$

Note that here we also do not have an endogeneity issue, but we must be careful in the interpretation of our estimate. In particular, we must interpret our point estimates of $\bar{\beta}$ as the average treatment effect for those individuals who are part of our estimation sample $(i|I_i = 1)$. Accordingly, this second structure for unobserved components only has consequences for the external validity of our results.

In sum, and considering all the elements we have discussed, we think that our best specifications to estimate the effect of pretrial detention on labor outcomes are the longitudinal difference-in-differences specifications.

E.2 Estimations

Table 21: Effect of pretrial detention on employment rate (sample members released after final trial verdict)

	Six months of employment	Two years of employment
Petrial Detention	-0.054***	-0.044***
	(0.005)	(0.004)
R^2	0.77	0.76
Observations	180,178	158,942
Mean of dependent variable	0.500	0.497

Notes: This table presents the results for the impact of pretrial detention on employment rate, considering the panel DiD approach, using the sample described in Table 2 but further restricting the sample to accused individuals who were released after the final trial verdict. The results are presented for two time horizons: the average for the six months after the final trial verdict and the average for the two years after the final trial verdict. The mean of the dependent variable is calculated only considering the control group. Statistical significance at 1%, 5%, and 10% is indicated by ***, **, and *, respectively.

Table 22: Effect of pretrial detention on average wage (sample members released after final trial verdict)

	Six months of wages	Two years of wages
Petrial Detention	-20,772***	-20,392***
	(2,271)	(2,406)
R^2	0.88	0.86
Observations	180,178	158,942
Mean of dependent variable	184,103	182,402

Notes: This table presents the results for the impact of pretrial detention on average monthly wages, considering the panel DiD approach, using the sample described at Table 2 but further restricting the sample to accused individuals who were released after the final trial verdict. The results are presented for two time horizons: the average for the six months after the final trail verdict and the average for the two years after final trial verdict. Statistical significance at 1%, 5%, and 10% is indicated by ***, **, and *, respectively.