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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

Around a third of prisoners worldwide (2.8 million) are incarcerated before trial. This paper combines Chilean individual administrative data for criminal cases and labor market outcomes to estimate, by differences-in-differences and instrumental variable approach, the effect of pretrial detention on labor outcomes. Because those pretrial detentions are the most difficult to justify, we focus our analysis on individuals who were free after their final verdict, either because they were found non-guilty or because their convictions didn't involve incarceration. The results show a negative impact of pretrial detention of 10% on the probability of having formal employment and an 11% decrease in wages, during the six months following the verdict. The magnitudes of these effects are reduced over time, but they remain relevant even 24 months after the final verdict. The evidence suggests that the fact that pretrial detention forces individuals out of the labor market is more relevant that any extra costs due to incarceration such as social stigma.

Keywords. Wages and unemployment, Criminal Law, Pretrial detention, Incarceration. **JEL Classification:** J31, J65, J71, K14, K41.

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1 Introduction

Around a third of prisoners worldwide (2.8 million) receive what is known as "pretrial detention" (see Walmsley (2016)), i.e., a judicial measure where an accused is incarcerated before his trial starts, as a precautionary or investigative mechanism. Advocates of pretrial detention usually justify this measure based on the concern that the accused will not appear in court, that he might be a danger to others, or that he might interfere with the ability to investigate the crime. However, not only is pretrial detention a threat to the presumption of innocence of the accused, which is a keystone in all contemporary judicial systems,[1] but it could also have moral, social and economic costs for the accused, including impacting their post trial labor market outcomes.[2] These costs are even more problematic when we consider those who are innocent or whose punishment would not include incarceration, the population on which we specifically focus in this paper.

In Chile, where this study takes place, the ratio of pretrial detainees to total prisoners has risen from 21,9% in 2007 to 36% in 2017.[3] Worryingly, if we focus on those individuals who were not incarcerated after the conclusion of their legal proceedings, either because they were found non-guilty or received noncustodial sanctions, pretrial detention increased from 9,543 in 2008 to 17,055 in 2018. The time that these individuals spend in pretrial incarceration is not insignificant either: while 25% of them spent less than ten days in prison, 59% spent between ten days and six months and, alarmingly, 16% were incarcerated for more than six months for cases where they would not receive a single day of prison time after final verdict.

To assess the potential negative impacts of this precautionary measure, this paper evaluates the effect of pretrial detention on labor market outcomes using Chilean data. To do so, we use a novel dataset that merges individual administrative data on pretrial incarceration and labor market outcomes between 2008 and 2016. The pretrial incarceration data comes from the Public Defender's Office records and the labor market outcome data come from the administrative records of the Chilean unemployment insurance scheme. This data on employment and wages includes the monthly individual

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

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

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

labor performance for all people who work in the formal private sector.

We study the effect of pretrial detention on post final verdict labor market outcomes for those accused individuals who did not have any legal impediment to working after the end of trial, i.e. those who were free after the final verdict. Specifically, our treatment group are accused individuals who were incarcerated pre trial for some period of time but were free after their final verdicts, and our control group are those accused individuals who were not incarcerated either pre or post trial. Given that we have a panel database spanning several months before the beginning of the prosecution and after the final verdict, we can estimate the pretrial detention effect using different models from the family of differences-in-differences (DiD) estimators, namely, cross section DiD, panel DiD (controlling for individual fixed effects), and DiD matching. The last two methods take advantage of the longitudinal structure of the database. We complement these models by estimating an IV linear model, which takes advantage of the quasi-random assignment of the detention judges, who determine pretrial detention but not final verdicts.

There are two empirical challenges to tackle in order to obtain estimates that can be interpreted as causal evidence in this context. Our independent variable of interest is endogenous, because the pretrial detention decision is made by judges who are probably observing individual characteristics that we – as econometricians – can only observe partially. These charateristics are probably correlated with labor outcomes. We present evidence suggesting that both DiD and IV models properly address this concern. Meanwhile we select our sample based on trial outcomes, which could be affected by the treatment, leading to selection bias. In fact, as Dobbie et al. (2018) and Leslie and Pope (2017) show in the US context, we do find that pretrial detention increases the probability of being incarcerated after the final verdict. On this regard, we prove that this selection bias is properly addressed by DiD models that control for individual fixed effects, and that IV models fail in handling this econometric challenge. For these reasons, we argue that the most reliable specifications are the panel DiD and the matching DiD. That said, both approaches – DiD and IV models– deliver point estimates that confirm the negative and relevant impact of pretrial detention on labor outcomes.

By estimating the 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 of between 4.4 and 5.4 percentage points (pp), a $9.2 - 11.3\%$ reduction. In respect of wages, the negative short-term effect is around $19,500$ Chilean pesos (CLP) per month (about 28 US dollars), which represents a drop of 10.6% on average. Regarding the persistence of these effects, for the case of employment we show that the magnitude of the pretrial detention effect decreases as time passes, but the effect is always larger than 2 pp, even 24 months after treatment. In the case of wages, we do not observe a relevant reduction in the effect after 6 months, even though we study the effects up to 24 months post trial. The IV model delivers point estimates whose magnitudes are larger than the DiD estimates, but with less statistical precision. That said, these IV results give more more support to the hypothesis that pretrial detention has a relevant impact on employment and wages.

We also show that the effects are much larger for those who stayed in prison longer.[4] To do so, we divide the treatment group into terciles, based on the time they spent in pretrial imprisonment. In the case of the short-term effect on employment probability, the effects for the first, second, and third terciles are -3, -5, and -9 pp., respectively. In the case of the middle-term effect on wages the effects for the first, second, and third terciles are $-18,618$ CLP (26 US dollars), $-24,318$ CLP (34 US dollars), and $-37,675$ CLP (53 US dollars), respectively.

Regarding mechanisms, we discuss the relevance of two broad explanations for our results. In the first place, we discuss the importance of the fact that being detained pretrial forces the accused out of the labor market. We refer to this explanation as the *labor market mechanism hypothesis*. In the second, it could be the case that pretrial detention carries with an additional and specific impact on labor outcomes beyond simply being unable to work during the period of detention. We call this explanation as the *incarceration mechanism hypothesis*. Our results suggest that the labor market mechanism is more important than the incarceration mechanism. For example, we show that the effect of losing a job due to a firm bankruptcy on future wages and employment, a negative shock that is beyond the worker's control, is similar in magnitude to the effect on labor outcomes of pretrial detention. Nonetheless, we do find evidence suggesting that the *incarceration mechanism* also plays a (probably minor) role.[5]

[4] This result is contrary to the findings in Kling (2006) and Landersø (2015).

[5] This discussion is related to the studies that consider whether incarceration and a criminal history generate stigma in the labor market, see Bushway (2004); Finlay (2009); and Pager (2003).

This paper has two main contributions to the literature. First, to the best of our knowledge, this is one of the first papers that estimates a causal effect of pretrial detention on labor outcomes, and the first paper to do so for a developing country, where jail conditions tend to be worse.[6] Furthermore, this is the first paper that estimates this effect for individuals whose final verdict does not include incarceration (including non guilty).[7] Secondly, by taking advantage of our labor market data, we shed some light on what mechanisms could explain the effect of pretrial detention on labor outcomes.

Our paper is related to the scarce literature that studies the impact of imprisonment on labor market outcomes. We should note that the scarcity of this literature is probably due to data constraints. This scant literature also has very mixed results. On one hand, 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)). Others, however, have shown that the negative effects could be moderate in magnitude and rather short-lived (see Grogger (1995)). On the other hand, some positive results from incarceration have been found by Nagin and Waldfogel (1995) and Bhuller et al. (2016). The latter uses data from Norway to show that imprisonment increases participation in programs designed to improve employability and to increase employment rates and earnings while discouraging further criminal behavior. Their findings demonstrate that time spent in prison could potentially be *pro human capital accumulation* for individuals outside the labor market when incarceration has a focus on rehabilitation.

The closest paper to this research is Dobbie et al. (2018), which shows that pretrial detention decreases formal sector employment and receiving employment and tax-related government benefits. They use data linking over 420,000 criminal defendants from two large, American, urban, counties to administrative court and tax records. Their empirical strategy exploits exogenous variation in pretrial release given the quasi-random assignment of cases to bail judges.[8] In our paper we also consider this methodology. Compared to our research, their estimations include all accused who receive pretrial in-

[6]To see other papers using data from a developing country to estimate the effect of pretrial detention on recidivism, see Cortés et al. (2019) and Ferraz and Ribeiro (2019).

[7]Thus, our findings contrast with the results in Harding et al. (2018). They find an effect of imprisonment on employment but mainly through incapacitation in prison.

[8]This source of exogenous variation have also 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).

carceration, including those who then receive additional prison time as a consequence of their final verdict. Thus our estimates should not be directly compared to theirs. The goal of our paper is to identify the effect of pretrial incarceration specifically on those who would not face incarceration otherwise for being accused of a crime (innocent or guilty but with lesser punishment). Indeed, the negative effects that we find provide an starting point to develop compensation schemes for those who are unfairly affected.

The rest of the paper proceeds as follows. Section 2 briefly describes the Chilean legal system and the condition to invoke pretrial detention. Section 3 describes the data, and presents some stylized facts of labor market outcomes for treatment and control groups that motivate our empirical strategy in Section 4. Section 5 presents our findings on the impact of pretrial detention on employment probability and average wage. Section 6 discuss possible mechanism to explain our results. Finally, Section 7 concludes.

2 Pretrial Detention in Chile

Chile's reformation of its criminal justice system was a gradual processes which started in 2000 (in some geographic regions) and finalized in 2005. The reform included a new penal code that replaced the former written, secret, and inquisitional system – 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), courts where the detention hearing is 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 in the exercise of this labor records all defendants that use their services, including detailed information on the penal cause.

In this new system the penal process has the following stages. It starts with the arrest, in most of the cases because the accused is caught by the police in *flagrante delicto* (i.e., in the commission of the crime), in other cases the Public Prosecutor conducted an investigation to find the suspect. This stage ends in the detention hearing at the Guarantee Court, where the detention judge chooses among three possible outcomes: to

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

begin a penal proceeding, an alternative end (including compensation agreements and the conditional suspension of proceedings), or to simply dismiss the proceedings. It should be noticed that most of the cases end in Guarantee Court either in alternative ends or dismissal. As a general matter, a penal proceeding is only for severe crimes. In this paper we focus on these type of crimes.

When the detention judge decides to begin a penal proceeding, she must decide the length of the trial (considering the time required for an investigation), and if any precautionary measures are needed. Pretrial detention is the most severe precautionary measure. This precautionary measure is requested by the prosecutor and the defense attorney can argue against it. The legal arguments that may be invoked by the prosecutor to request such a measure are the chance of flight from prosceuciton, if the defendant represents a danger to society, or that imprisonment will help with the investigation (see Riego and Duce (2011)). At least in theory, the prosecutor must make a very strong argument when they are discussing pretrial detention for a minor. Unlike the legal system in the US, defendants in Chile do not have the option of posting bail in order to avoid pretrial detention.

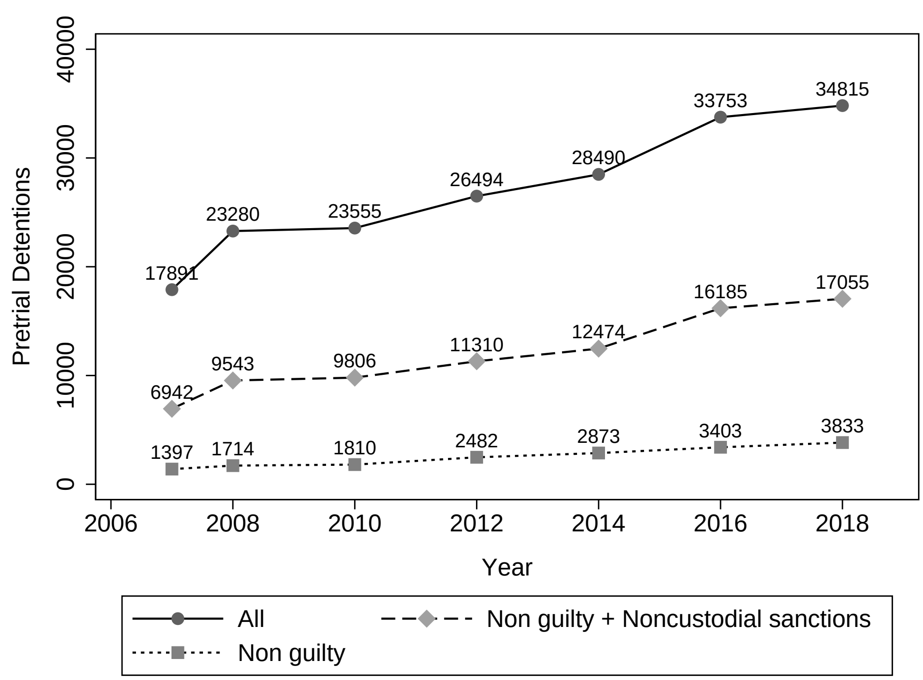
There are several outcomes from a trial ranging from non-guilty to conviction with prison. One factor that is very relevant to our paper is that the decision of pretrial detention is kept separately from other trial outcomes through the use of the two different courts. Note also that while there is one judge in the detention court, trials in the oral proceedings court consist of three judges.

Respect for defendants' human rights was one of the principal motivations for criminal justice system reform. However, as time has passed, this original motivation has been somewhat forgotten (see Riego and Duce (2011)). Thus pretrial detention has become much more common than it was originally planned to be. In addition to media influence and the pressure of public opinion, an aspect that may explain this was the application of the so called "agenda corta anti delincuencia" (short agenda against delinquency), which started in 2008 with the enforcement of Law N° $20,253$. This law, in practice, expanded the possibilities to use pretrial detention.

Figure 1 presents this tendency, showing that pretrial detention has become more frequent between 2007 (17,891 cases) and 2018 (34,815 cases), which means that the fraction of cases with 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 total prisoners rose from 21.9% in 2007 to 36% in 2017, an increase of 64.4%. As a consequence of this tendency, as Figure 1 shows, it also increased the number of individuals who were detained pretrial but who were either found non-guilt or whose punishment did not include incarceration at all. This is the group we focus o in this paper, because we want to isolate the effect of pretrial detention on labor outcomes that is due to after incarceration consequences from the effect that is due to incapacitation.

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

Figure 1: Evolution of pretrial detention



Note: This figure shows the evolution of pretrial detention from 2007 to 2018, using the DPP administrative records. The dynamic is shown for three groups: a) **All:** considers the full sample; b) **Non guilty + Noncustodial sanctions:** includes all the individuals whose final verdict implies no time in prison, this is the group considered to build the estimation sample; 3) **Non guilty:** includes those individuals whose final verdict declared them non guilty.

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

3 Data and Stylized Facts

3.1 Data

We assemble an administrative individual-level dataset from the national employment insurance scheme (Ministry of Labor) and the Public Defender's Office (DPP). As we 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's attorney, because they have hired a private attorney, we have data on their alleged crime but we do not have data on the final verdict. Thus given our treatment and control group definition we do not include those cases in our sample. That said, less than 5% of the prosecuted are only represented by a private attorney without DPP participation. In this paper, we use the DPP's prosecution records from 2006 to 2016.

The information provided by the DPP is very detailed for the crime (the specific typification that we group into broader categories), the court, the time in jail during the prosecution (if any), and the final verdict. From the latter we determine whether the prosecuted person was declared guilty (with his sanction) or not-guilty. In addition to this information, the Research Department of the Supreme Court gave us data on all the guarantee judges who decided pretrial detentions between 2008 and 2017. As it will be clear, this information is useful for our empirical strategy because it gives us exogenous variation in the probability of receiving a pretrial detention.

The source for the labor market data are the administrative records from the unemployment insurance scheme. As is detailed in Acevedo et al. (2010), this unemployment insurance covers all enrolled workers over 18 years old who are employed in private sector salaried jobs. Temporary workers are also included in the system, but individuals who have been always (i.e. since 2002) unemployed or always working in the public or informal sector are excluded (in Chile the informal sector is about 30%). That said, participation in this unemployment insurance is compulsory for all employees who started a new job after October 2002, and it is voluntary for those workers who were already employed at that time. Therefore, full implementation of this insurance was only achieved after 2005. This database provides monthly data on wages, type of worker contract (full or part time), and data on the company that employs them including size (measured by the number of workers), economic sector, and a specific company ID. The latter is useful since it allows us to see if a given worker is keeping the same job over time or is constantly switching among jobs. As it is clear in the corresponding section, the possibility of keeping one's pretrial job is an important mechanism in explaining the effect of pretrial detention on labor market outcomes.

We focus on individuals whose prosecutions started after 2007 due to data availability. Furthermore, we only consider first time offenders (according to the DPP data) because some prosecuted individuals, who are not first time offenders, may have low pre treatment labor numbers, only because they were imprisoned at that time.

To study the effect of pretrial detention on labor outcomes of individuals who did not have any legal impediments for working after the end of their trials, we restrict our estimation sample to individuals who were either declared non-guilty or did not receive jail time as part of their punishment. Thus, the treatment group are those who received pre- but not post-trial detention, while the controls are those who never received any detention. We further restrict the study to those being accused for severe crimes, *i.e.* crimes for which more than 10% of cases receive pretrial detention. Finally, we restrict our sample to those individuals who have worked at least one month in the formal sector (i.e. contributed to the unemployment insurance scheme) during the two years before the beginning of prosecution as it does not make any sense to include those who have not been formally working.

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 these groups using the labor market information because most of the differences between these two groups are due to the lack of this labor market information for those who are excluded from the estimation sample. From this table, we can observe that the estimation sample has more men, they are more likely to have been charged with more severe crimes and thus consequently they are more likely to have pretrial detentions, additionally their trials tend to be longer. Of course, the tendency towards more severe crime and higher chance of pretrial detention is purposeful.

Table 1: Estimation sample versus population

Variable	All	Est. Sample	Norm. Dif.	p-value
Male	0.78	0.91	-0.372	0.000
Indigenous	0.02	0.01	0.029	0.000
Foreign	0.01	0.01	0.045	0.000
Severe crimes	0.05	0.24	-0.545	0.000
Judicial process in Metropolitan Region	0.37	0.40	-0.050	0.000
Pretrial detention	0.01	0.09	-0.351	0.000
Days in judicial process	94	175	-0.382	0.000
Days in pretrial detention	115	111	0.031	0.025
Observations	1,082,883	89,918		

Note: In this table the first column (**All**) considers all the individuals in the DPP data base (between years 2007 and 2016) who are 18 years old or older and whose final verdict was either non-guilty or guilty but with noncustodial sanctions. 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 worked for at least one month in the formal sector during the two years before the beginning of prosecution; and b) individuals accused of crimes where pretrial detention is an important possibility, i.e. in 10% or more of cases. Norm. Dif. denotes the normalized differences in the means.

To have a clear picture of our estimation sample, Table 2 shows the descriptive statistics for the covariates, reporting the information for the control and treatment groups separately. We would like to highlight a few factors. First, the treatment group is more likely to be male and of indigenous decent. Second, the individuals in the treatment group are accused of more severe crimes and their trials last longer. Third, those in the control group tend to have longer labor contracts although there are not differences in terms of firm size or industry. It should be noticed that, even though our control and treatment groups are very different, none of our empirical strategies require them to be equal, something that we will discuss in the next section.

Table 2: Summary statistics for the covariates by treatment status

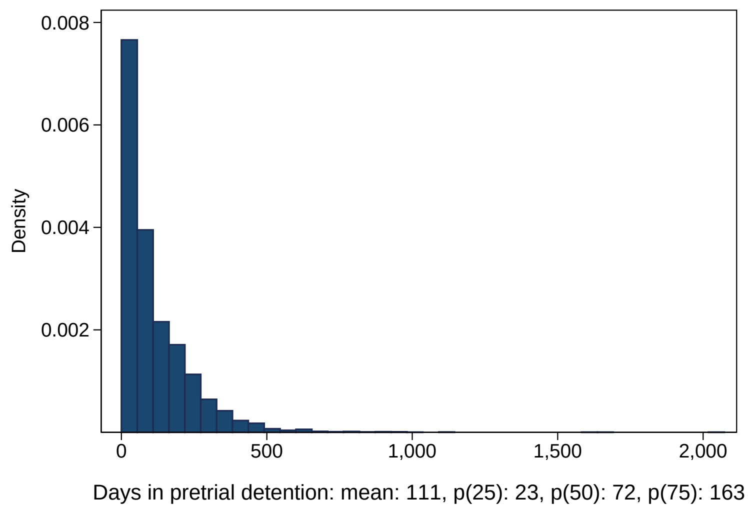
	Treated		Control	
	Mean	S.d	Mean	S.d
Demographic variables				
Male	0.9448	0.2284	0.9109	0.2848
Foreign	0.0072	0.0847	0.0053	0.0727
Indigenous	0.0215	0.1452	0.0124	0.1107
Judicial variables				
Judicial process in Metropolitan Region	0.2998	0.4582	0.4048	0.4909
Severe crimes	0.5780	0.4939	0.2057	0.4042
Days in judicial process	254	243	167	231
Type of contract				
Indefinite term	0.3171	0.0017	0.4465	0.0008
Fixed term	0.6829	0.0013	0.5535	0.0010
Firm size				
Micro	0.1188	0.0022	0.1426	0.0009
Small	0.2360	0.0021	0.2219	0.0009
Midium	0.2169	0.0018	0.2125	0.0006
Big	0.4283	0.0013	0.4230	0.0012
Firm sector				
Agriculture-Silviculture-Fishing	0.0990	0.0025	0.0875	0.0010
Mining	0.0070	0.0056	0.0122	0.0017
Manufacture	0.1005	0.0018	0.1054	0.0009
Electricity-Gas-Water	0.0019	0.0125	0.0026	0.0035
Construction	0.3055	0.0011	0.2365	0.0010
Commerce	0.1186	0.0022	0.1387	0.0006
Services	0.2963	0.0017	0.3246	0.0011
Transportation-Communication	0.0673	0.0022	0.0880	0.0010
No information	0.0040	0.0068	0.0045	0.0022
Observations	7,894		82,195	

Note: This table reports the summary statistics for the covariates considered in our estimated models, comparing control and treatment groups. This table considers the estimation sample, namely, individuals in DPP data base (between years 2007 and 2016) who are 18 years old or older and whose final verdict was either non-guilty or guilty but with noncustodial sanctions; who have have worked for at least one month in the formal sector during the two years before the beginning of prosecution and accused of crimes where pretrial detention is an important possibility, i.e. in 10% or more of cases The standard errors are in parenthesis.

3.2 Stylized Facts

Figure 2 shows the histogram for days in pretrial detention for those who were not imprisoned after their verdict was rendered, our focus in this paper. As can be observed, although a considerable fraction of the prosecuted spent only few days in prison, many of them spent months in prison. Indeed, the average pretrial detention time for the individuals of focus is 111 days, whereas 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



Note: This figure shows an histogram for the days in pretrial detention, considering the estimation sample of this paper, namely, individuals in DPP data base (between years 2007 and 2016) who are 18 years old or older and whose final verdict was either non-guilty or guilty but with noncustodial sanctions; who have at least one month worked in the formal sector during the two years before the beginning of prosecution; and whose imputed crime category has at least a 10% of cases with pretrial detention.

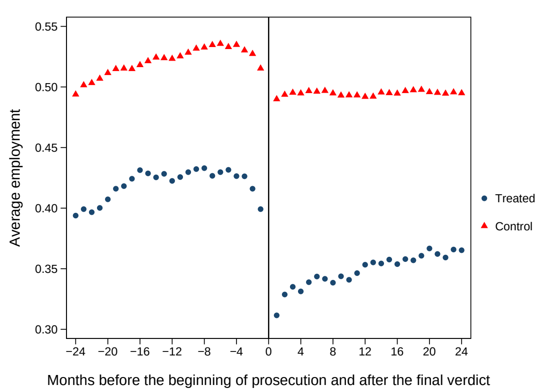
Before presenting our empirical strategy and discussing our estimation results, we provide some visual evidence of the impact of pretrial detention on labor outcomes. To do so we present two types of graphs for the dynamics of our dependent variables: monthly average wages and employment rate. In Figure 3, we show the employment rate (panel a) and average wage (panel b) for the periods before the beginning of prosecution and after the final verdict. To be able to compare the labor dynamics of individuals whose prosecutions begin in different months and years, we group them to calculate these averages by the number of months before the commencement of their prosecution

– in that case the value in the horizontal axis is negative –or by the number of months after their final verdict, for which the value in the horizontal axis is positive. Notice that this implies that the time between -1 and 1 is equal to the duration of the trial, a variable that is heterogeneous across individuals and which average length for the treatment (control) group is equal to 254 (167) days with the standard deviation equal to 243 (231).

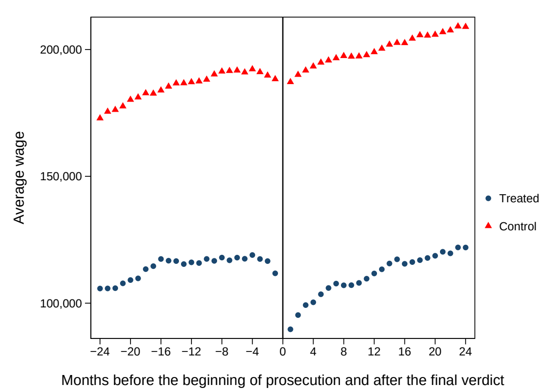
The second type of graphs are simply the differences in the employment rate or average wages, between the treatment and the control group, presented in Figure 4.

Figure 3: Employment and wages dynamics for the treatment and control groups

(a) Employment rate



(b) Average wages



Note: These figures show the dynamic for the employment rate (panel a) and average wages (panel b), pre and post prosecution, for treatment (triangles) and control groups (circles). Each dot represents the average value of employment or wages, for a particular month, X months before the beginning of the prosecution (when X has a negative value at the horizontal axes) or X months after the final verdict (when X has a positive value at the horizontal axes). Notice that X equals to zero refers for a period that lasts differently across individuals, which goes from the beginning of prosecution to the end of trial (with its final verdict).

These plots illuminate a few aspects of the data. First, both before and after treatment, control individuals perform better in the labor market compared to treatment individuals. During the months before prosecution, control individuals have on average between eight and twelve percentage points higher probability of being employed and monthly wages that are about 70,000 CLP (108 US dollars) higher, which represents 37% of the control group average.[10] Second, the parallel trends condition appears to

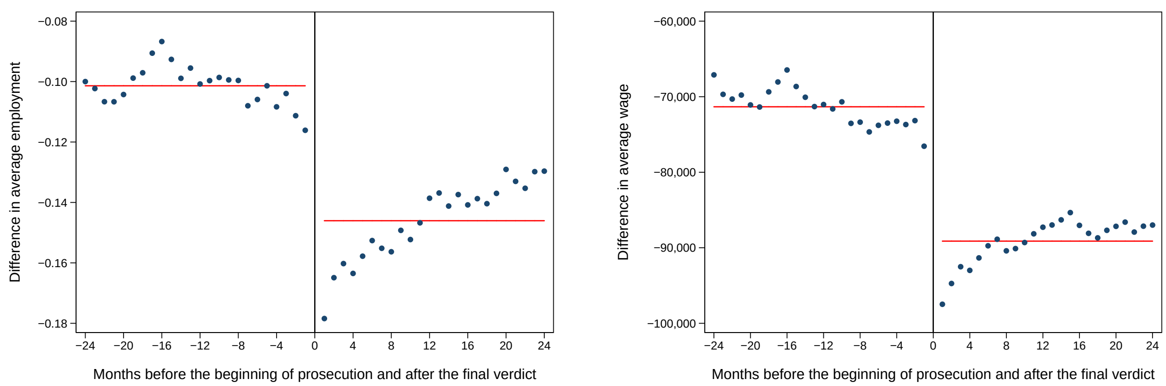
[10]Note that the treatment group is comprised of individuals who make less then minimum wage.

be satisfied, which can be directly observed in Figure 3. It is also evidenced in the consistency of the pre-treatment dynamics in Figure 4. This is then formally tested in Sections **??** and 5.1.1. Third, there is a clear discrete change after pretrial detention in the differences between treatment and control groups in both employment rates and average monthly wages, which supports this paper's main result. As Figure 4 (panel a) shows, in the case of employment this change is about 4 percentage points on average. Meanwhile (panel b) shows that in the case of wages the change is around $18,000$ CLP (these figures are around 8% and 10% of the control group average respectively). Finally, Figure 4 shows that the increase in the differences between the treatment and control groups in labor outcomes are more severe right after the final verdict, but the increase in these differences do not fade out over a period of two years after the end of the trial.

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

(a) Employment rate

(b) Average wages



Note: These figures show the dynamic for the employment rate (panel a) and average 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 at the horizontal axes) and X months after the final verdict (when X has a positive value at the horizontal axes). Notice that X equals to zero refers for a period that lasts differently across individuals, which goes from the beginning of prosecution to the end of trial (with its final verdict).

There are three reasons for this. In the first place, the probability of being employed is low (between 44 and 51%) and the average wage calculation considers unemployment as zero wages. In the second place, they probably have low productivity (and low bargaining power). Finally, many could be employed in the informal labor market, thus their wages are not observable.

4 Empirical Strategy

We use different empirical strategies to estimate the effect of pretrial detention on labor market outcomes. On one hand, we consider a set of models which fall within the differences-in-differences (DiD) approach (Section 4.1). On the other hand, we consider the IV approach, taking advantage of the quasi random assignment of detention judges (Section 4.2). We also present the OLS results, but only as a reference point. For reasons that are developed below, our preferred approach is the DiD estimations that take advantage of the longitudinal aspect of the data. That said, as noted in Section 5, all the estimation procedures deliver similar results in qualitative terms.

4.1 Differences-in-Differences approaches

The DiD estimation uses the discrete change between pre and post trial 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 now introduce some notations that are used across this section.

Let Y_{it} be the outcome of interest for individual i at time $t \in \{0, 1\}$, which can be the average 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 will be equal to 6 or 24 months. As stated, all the individuals considered in our sample were either found non-guilty or their verdict did not include jail time as a sanction; the treated ($PreTrial_i = 1$) were incarcerated during their prosecutions (at least for a fraction of it) and the control ($PreTrial_i = 0$) were always free.

All of these models include a different set of covariates X. The OLS set of covariates (X_{i1}^{ols}) includes dummies for gender, being Chilean, and ethnicity, pre treatment average wage and employment rate (6 or 24 months before prosecution), location of the court (region), type of crime, trial duration, and the year and month of the sentence. 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 panel DiD set of covariates (X_{it}^{p}) are the year and month of the beginning of the prosecution (when $t = 0$) and year and month of the sentence (when $t = 1$), and ω_i is individual i fixed effect. Finally, the covariates used in the DiD-matching (X_{it}^m) are gender, Chilean, and indigenous dummies, pre treatment average wage and employment rate (using 27-36 months before prosecution), location of the court (region), type of prosecuted crime, and year and month of the sentence.

In all these models the parameters of interest are denoted by β. For example, $\widehat{\beta^{cs}}$ is the estimated effect of pretrial detention using the cross sectional DiD approach (i.e. without controlling for individual fixed effects).

The specifications of the estimated models are the following:[11]

OLS:

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

Difference-in-differences, cross sectional data:

$$Y_{i,t} = \alpha_0 + \alpha_1 PreTrial_i + \alpha_2 \mathbb{1}[t=1] + \beta^{cs} PreTrial_i * \mathbb{1}[t=1] + \gamma' X_{it}^{cs} + \epsilon_{it},\ t \in \{0,1\}. \tag{2}$$

Difference-in-differences, panel data:

$$Y_{i,t} = \alpha \mathbb{1}[t=1] + \beta^p PreTrial_i * \mathbb{1}[t=1] + \gamma' X_{it}^p + \omega_i + \epsilon_{it},\ t \in \{0,1\}. \tag{3}$$

Difference-in-differences matching:

This is an approach proposed by Heckman et al. (1997), which combines the matching estimation technique with the advantages of the panel difference in differences estimator. Let $Y_{i,t}(PreTrial_i)$ denote the potential outcome of individual i at time t, and $\Delta Y_{i,t}(PreTrial_i) = Y_{i,1}(PreTrial_i) - Y_{i,0}$ the potential increment in the outcome Y between time $t = 0$ and $t = 1$. Thus the average treatment effect (ATE) is 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. In particular, for each i treated ($PreTrial_i = 1$),

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

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. In the results section 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 average treatment effect (ATE) is:

$$\hat{\beta}^m = \frac{1}{N_1 + N_0}\left(\sum_{i:PreTrial_i=1} \Delta Y_{i,t}(1) - \widehat{\Delta Y}_{i,t}(0) + \sum_{i:PreTrial_i=0} \widehat{\Delta Y}_{i,t}(1) - \Delta Y_{i,t}(0)\right), \tag{4}$$

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.

It should be noticed, that in terms of the assumptions that these models need to ensure identification of β, the last two are those with less demanding conditions. In particular, they both need there to be no variable that varies between $t = 0$ and $t = 1$ that influences both the probability of being incarcerated during the prosecution and the labor market outcomes. That said, in Section 5 we see that unlike the OLS, the other three models have similar results, which is consistent with the fact that the pre trend dynamics between treated and control groups seems to be parallel. Nevertheless, remember that the OLS approach is only presented as a reference point, given that the assumption that we have included all relevant covariates, which is required by OLS for delivering causal parameters, is not realistic in this context.

4.1.1 General Differences-in-differences approach

Following Duflo (2001), we generalize the panel DiD identification strategy previously introduced to an interaction terms analysis by allowing for period-by-period contrasts. 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 grouping months into 8 periods of six months each, where the omitted category is the period between 18 and 24 months before the beginning of prosecution, we run the following model:

$$Y_{it} = \sum_{n=2}^{8} \beta_n PreTrial_i * \mathbb{1}[t = n] + \sum_{n=2}^{8} \delta_n * \mathbb{1}[t = n] + \omega_i + \epsilon_{it}, \tag{5}$$

where i is the individual subindex, t is the period subindex, and – as above – ω_i is individual i fixed effect. 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 retention on period n in respect to the first period. Notice that because the first four periods (including period one, the reference category) are pretreatment, the identification strategy is suitable for causal interpretation to the extent that the point estimates of β_2, β_3, and β_4, are close to zero. Conditional on that, which is a simple way to test parallel trends, the point estimates for β_5, β_6, β_7, and β_8 can be interpreted as the effects of pretrial detention after those different amounts of time have passed.

4.2 Instrumental variable

The instrumental variable considered in this paper is a leave-out means that capture how the detention judge (quasi-random) assignment impacts the probability of having a pretrial detention.

For constructing this instrumental variable we use a dataset that includes all criminal records, with and without data on labor outcomes. Specifically this is the sample described in the first column of Table 1. From this starting point, we only use cases who had detention judges with more than 10 cases yearly. For an 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:

$$\text{PreTrial}_{jc} = \alpha_0 + \alpha_1' \text{court}_c \times \text{year}_{jc} + \xi_{jc} \tag{6}$$

We then proceed by calculating the judge severity score variable, denoted by $Z^{\text{judge}}_{j(i)}$:[12]

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

To clarify the judge severity score measures the propensity that a given detention judge has of giving pretrial detention to any given individual. As previous research has noted, this procedure is numerically equivalent to the judge fixed effect in a jackknife regression of pretrial detention (or incarceration) estimated over all years. As a result, our two-stage least squares estimators 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 as to avoid having an artificial strong identification given by the direct linkage between the individual's own endogenous outcome and the instrument

Given this instrument, we can estimate the effect of pretrial detention on labor outcomes in a two stage least squared (2SLS) fashion by considering the following two equations:

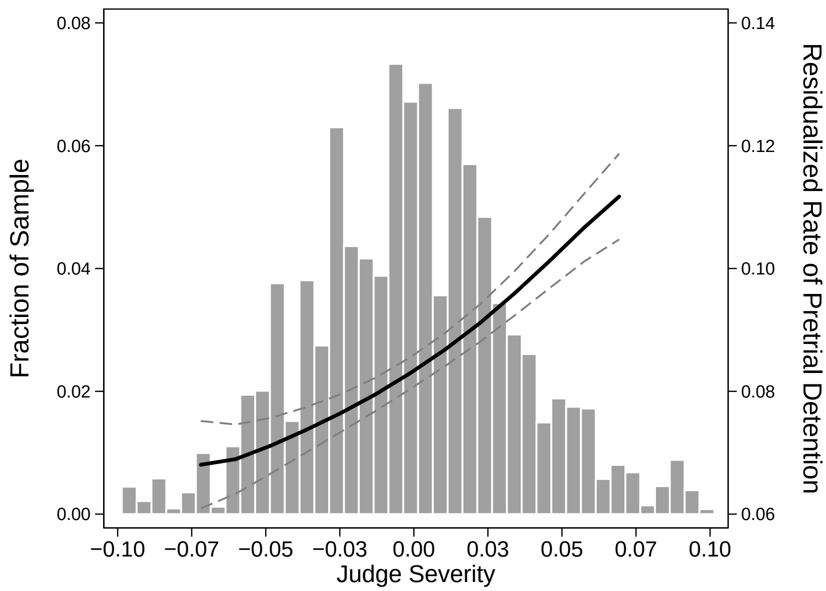
$$\begin{aligned} Y_{i1} &= \alpha^1 + \beta^{iv} PreTrial_i + \gamma^1 X_{i1} + \theta^1 \text{court}_c \times \text{year}_{jc} + \epsilon^1_{i1} \qquad (7) \\ PreTrial_i &= \alpha^2 + \beta^2 Z^{\text{judge}}_{j(i)} + \gamma^2 X_{i1} + \theta^2 \text{court}_c \times \text{year}_{jc} + \epsilon^2_{i1}. \end{aligned}$$

IV Variation

In Figure 5 we present the distribution of the instrumental variable used in this paper. The sample used to construct the instruments consists of 907 judges. The average judge handle 936 cases. In the estimation sample, the mean of the severity score variable is -0.0002 with a standard deviation of 0.0389. The severity measure ranges from -0.057 (5th percentile) to 0.060 (95th percentile), which in turn implies that moving from a less severe to a more severe judge is associated with a 10 pp. increase in receiving pretrial detention.

[12] $j(i)$ is the detention judge that decided the pretrial detention of individual i.

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 nonparametric 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 LATE, four conditions need to be met: (i) a non-trivial first stage, (ii) instrument independence, (iii) instrument exclusion, and (iv) monotonicity. We now turn to discuss each of these conditions in our research.

Non-trivial First Stage

Figure 5 shows the effect of the judge severity score on a individual's pretrial detention status, estimated via a local linear regression of the former against the latter after adjusting for court by year fixed effects, i.e. the residualized rate. The pretrial detention status varies monotonically along judge severity score in a fairly linear fashion, although it seems that the slope is steeper at the beginning. This suggest that moving away from the least severe judge towards a more neutral one increases the chances of ending up in pretrial detention more aggressively than moving from a neutral one to a fairly severe judge.

The first stage estimation is presented in Table 3, showing that our instrumental variable is highly predictive of whether the individual ends with a pretrial detention. We present two specifications: controlling for employment outcomes during the 6 months before the prosecution and controlling for employment outcomes during the 24 months before the prosecution. Both specifications deliver similar results. The magnitudes suggests that if a given individual is facing a judge who is 10 pp. more likely to give pretrial detention (a more severe judge), then he is between 8 and 9 pp. more likely to get pretrial detention.

Independence

A key condition to be met is that our instrument is as good as a random assignment. In order to verify that this assumption is true in our context, we present in Table 4 the same kind of analysis that would be performed in an actual experiment to assess the compliance and the randomization of the experiment. The first column displays the coefficients of a regression of the pretrial detention variable (our endogenous variable) against the covariates described in the rows, while the second column shows the same regression but now having judge severity (our instrumental variable) as the dependent variable. In all these models we control for year interacted with court fixed effects. Therefore, the null hypothesis of all parameters equal to zero, a hypothesis that is tested using the F Test, does not consider these fixed effects. In Table 4 we note that gender, pre treatment labor outcomes, crime severity, and firm sector dummies are highly predictive of receiving pretrial detention, whereas almost none of these variables seem to predict the severity of the assigned judge. This is further corroborated by the p-value of the joint significance test, which in the case of the second column is not able to reject the null hypothesis that all the coefficients are equal to zero (p-value equal to 0.19).

Note that it is the combination of these two regressions is what makes this test a convincing approach. Because while the first column shows that these covariates are very relevant in predicting the endogenous variable, the second column shows that they are not correlated with the instrumental variable, similar to what you would need to test the validity of a RCT.

Exclusion & Monotonicity

The exclusion restriction for the judge severity IV requires that detention judge assignment only impacts individuals' outcomes through the probability of receiving pretrial detention. This is likely to be the case, because, conditional on deciding to begin a penal proceeding, the only role of these judges is to prescribe precautionary measures. Recall that the verdict is determined by three different judges in a completely different court.[13]

The monotonicity assumption requires that an accused who were sent to pretrial detention with a lenient judge could have also faced pretrial detention with a more severe one and vice versa. One common approach in the literature to (indirectly) test this assumption is to estimate the first stage regression for different groups and to obtain point estimates for the instrument that are all of the same sign and have similar magnitudes. This is what we get from Table 5, where we present the first stage for different groups of imputed crimes, by individuals' pretreatment average wages (bellow and above the median), the size of the pretreatment firm (big firms versus the rest), and crime severity (bellow and above the median). This table shows that in all cases but one (crime severity), the point estimates of the first stage are very similar across different groups. Even in the case where the point estimate is different (low crime severity), we do not have flip in the sign. These results suggest that monotonicity assumption holds in our setting.

[13] A detention judge may also give a sentence if the case is *simple* enough; this occurs in an abbreviated trial process only for non-severe crimes. In general it is defined during the detention hearing at the Guarantee Court. However we have dropped this handful of cases from our data.

Table 3: First Stage: Judge severity score

	(1)	(2)
Judge severity IV	0.277***	0.305***
	(0.035)	(0.037)
Six months of wages	-0.000***	
	(0.000)	
Six months of employment	-0.031***	
	(0.004)	
Two years of wages		-0.000***
		(0.000)
Two years of employment		-0.034***
		(0.005)
Male	0.028***	0.028***
	(0.004)	(0.004)
Foreign	0.007	0.006
	(0.020)	(0.022)
Indigenous	-0.009	-0.007
	(0.016)	(0.018)
Days in judicial process	0.000***	0.000***
	(0.000)	(0.000)
Crime severity	0.007***	0.006***
	(0.000)	(0.000)
Fixed term contract	0.009***	0.004
	(0.003)	(0.003)
Sector = Mining	-0.015	-0.019
	(0.012)	(0.012)
Sector = Manufacture	-0.014**	-0.015**
	(0.006)	(0.007)
Sector = Electricity-Gas-Water	-0.020	-0.009
	(0.025)	(0.031)
Sector = Construction	0.005	0.007
	(0.006)	(0.006)
Sector = Commerce	-0.017***	-0.017**
	(0.006)	(0.007)
Sector = Services	-0.010*	-0.011*
	(0.006)	(0.006)
Sector = Transportation-Communication	-0.015**	-0.016**
	(0.007)	(0.007)
Firm size = Small	0.007	0.006
	(0.004)	(0.005)
Firm size = Midium	-0.000	0.001
	(0.004)	(0.005)
Firm size = Big	-0.003	-0.002
	(0.004)	(0.004)
Constant	-0.088***	-0.077***
	(0.009)	(0.010)
F test	62.28	66.87
Observations	46,081	40,842
Court by Time Fix 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. The model is estimated on the sample described in the notes of Table 2. Regression includes year interacted with court fixed effects. Robust standard errors clustered at the judge level in parentheses. ***, ** and * indicate statistical significance at the 1%, 5% and 10%, respectively.

Table 4: Randomization test for judge severity score

	Pretrial Detention	Judge severity IV
Male	0.027***	-0.000
	(0.004)	(0.001)
Foreign	0.004	-0.006**
	(0.022)	(0.002)
Indigenous	-0.006	0.003
	(0.018)	(0.002)
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	-0.035***	-0.000
	(0.005)	(0.001)
Crime severity	0.006***	0.000
	(0.000)	(0.000)
Fixed term contract	0.004	0.000
	(0.003)	(0.000)
Sector = Mining	-0.019	-0.002
	(0.012)	(0.002)
Sector = Manufacture	-0.015**	-0.001
	(0.007)	(0.001)
Sector = Electricity-Gas-Water	-0.010	-0.002
	(0.031)	(0.004)
Sector = Construction	0.007	-0.001
	(0.006)	(0.001)
Sector = Commerce	-0.017**	-0.000
	(0.007)	(0.001)
Sector = Services	-0.011*	-0.001
	(0.006)	(0.001)
Sector = Transportation-Communication	-0.017**	-0.001
	(0.007)	(0.001)
Firm size = Small	0.006	-0.000
	(0.005)	(0.001)
Firm size = Midium	0.001	-0.001*
	(0.005)	(0.001)
Firm size = Big	-0.002	-0.001
	(0.004)	(0.001)
Constant	-0.077***	0.001
	(0.010)	(0.002)
Joint Test	0.0000	0.1851
Observations	40,842	40,842
Court by Time Fix Effects	Yes	Yes

Notes: This table reports the reduced form results that are testing the random assignment of cases to detention judges. 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 a F-test of the joint significance of the variables listed in the rows with the standard errors clustered at the judge level. Therefore, it 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. ***, ** and * indicate statistical significance at the 1%, 5% and 10%, respectively.

Table 5: First stage estimation for different groups

	Above median of wages	Below median of wages	Big and medium firms	Small and micro firms	Above median severity	Below median severity
Judge severity IV	0.227***	0.340***	0.277***	0.316***	0.333***	0.045
	(0.046)	(0.054)	(0.040)	(0.067)	(0.095)	(0.100)
Constant	0.061***	0.108***	0.084***	0.085***	0.086***	0.074***
	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)	(0.004)
Observations	23,358	22,808	30,252	15,914	23,069	23,097

Notes: This table reports first-stage results for the linear IV model that estimates the effect of pretrial detention on labor outcomes, by crime severity. Thus, the regression is estimated using the sample as described in the notes of Table 2. Regression includes year interacted with court fixed effects. Robust standard errors clustered at the judge level in parentheses. ***, ** and * indicate statistical significance at the 1%, 5% and 10%, respectively.

4.3 Selection bias and the interpretations of our estimates

Given that we focus our attention on cases where the sentence was either non-guilty or a non-custodial sanction, and the fact that this is a post treatment definition, we need to investigate to what extent selection bias could be an issue in our empirical setting. Indeed, as shown in Dobbie et al. (2018) and Leslie and Pope (2017), pretrial detention may increase the probability of a guilty sentence. To address this concern, our first step is to replicate these models, to study whether in the Chilean context pretrial detention increases the probability of being declared guilty or having a custodial sanction. Because in this case the DiD is not implementable (the dependent variable does not have a pre-treatment value) and given the validity of our IV, we address this question by estimating a IV linear model, where the dependent variable is non-guilty or non-custodial sanction (which includes non-guilty), the endogenous variable is pretrial detention, and the instrument is the judge severity score.

The results of this exercise are presented in Table 6. As this table shows, our estimates are in line with previous literature, in the sense that pretrial detention increases the probability of having a worse sentence. Although this finding could be discussed on its own merits, we will now focus on to what extent these results could lead to selection bias in our empirical settings.

To discuss potential selection bias, we use equation (3), but add two elements to the analysis. Firstly, we allow that the effect of pretrial detention is heterogeneous across individuals (β_i), such that $\beta_i = \bar{\beta} + e_i$. Secondly, 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 sentence is either non-guilty or a non-custodial sanction (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 about not choosing to send the accused to prison. Notice that this selection equation allows 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 3, we have:

Table 6: The impact of pretrial detention on sentencing outcomes

	Custodial sanction
Pretrial detention	0.169**
	(0.080)
Male	-0.131***
	(0.004)
Foreign	0.092***
	(0.017)
Indigenous	0.010
	(0.013)
Crimes special laws	0.062***
	(0.007)
Drug law crimes	0.169***
	(0.007)
Homicides	0.157***
	(0.036)
Nonviolent thefts	0.156***
	(0.008)
Sex crimes	0.093***
	(0.009)
Thefts	0.139***
	(0.022)
Constant	0.824***
	(0.289)
Observations	126,508
Court by Time Fix 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 implies imprisonment. In other words, the dependent variable takes the value of zero if the individual belongs to the estimation sample for our main empirical models (those that estimate the effect of pretrial retention on labor outcomes), and it takes the value of one if final verdict sentences a custodial sanction. 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. ***, ** and * indicate statistical significance at the 1%, 5% and 10%, respectively.

$$E[Y_{it}|\omega_i, X_{it}, PreTrial_i, t, I_i = 1] = \alpha\mathbb{1}[t = 1] + \bar{\beta}PreTrial_i * \mathbb{1}[t = 1] + \gamma' X_{it} + \omega_i \quad (8)$$
$$+ E[\epsilon_{it}|\omega_i, X_{it}, PreTrial_i, t, I_i = 1] + PreTrial_i * \mathbb{1}[t = 1]E[e_i|\omega_i, X_{it}, PreTrial_i, t, I_i = 1],$$

In this context, the unobserved component of the selection equation can be related to the principal equation in two ways.[14] 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 that case $E[\epsilon_{it}|\omega_i, X_{it}, PreTrial_i, t, I_i = 1] \neq 0$

[14]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 lost 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 $I_i = 1$).

and can be written as $H(Pretrial_i)$ where H is an unknown function. However, notice that in this context, equation (8) 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. Notice, however, that it does generate an endogeneity issue if we estimate $\bar{\beta}$ by instrumental variable approach, as in equation (7), a specification that only uses the cross sectional nature of the database.[15]

In a second structure for the unobserved components, we allow that $I_i^* \not\perp\!\!\!\perp e_{it}$ (but $I_i^* \perp\!\!\!\perp \epsilon_{it}$), in that case $E[e_{it}|\omega_i, X_{it}, PreTrial_i, t, I_i = 1] \neq 0$. Given this structure, equation (8) can be written as:

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

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

In sum, and considering all the elements that were discussed, we think that our best specifications to estimate the effect of pretrial detention on labor outcomes are the longitudinal differences-in-differences specifications, both the linear one and the one that combines DiD with matching. We support this conclusion with the fact that the pre-treatment parallel trends assumptions totally holds in our context (proved in Section

[15] If this selection bias exists, this would bias our results toward to zero, given that those who are not part of the treatment group because they ended up with prison time probably already are less likely to be employed or to have a high salary. Hence, this selection process would artificially increase the average wage and the employment rate of the treatment group.

5.1.1) and that the potential selection bias is not a – internal validity – problem when we include an individual fixed effects.

5 Results

In this section we present the results of the DiD and instrumental variable approaches.

5.1 Differences-in-differences results

The results of the effect of pretrial detention on employment and wages using the DiD models described in Section 4.1, are shown in Tables 7 and 8 respectively.

We first present the *short-term* impact of pretrial detention on employment. In this case the dependent variable in $t = 0$ is the employment rate during the six months before treatment and in $t = 1$ it is the employment rate during the six months after treatment. About the timing of these effects, notice that *before treatment* refers to before the beginning of prosecution and *after treatment* refers to after the verdict. Table 7 shows that the *short-term* impact of pretrial detention on the employment rate is between 4.4 and 5.5 percentage points, considering the three DiD models previously described: cross sectional DiD (column 2), panel DiD (column 3), and matching DiD (column 4). These effects represent a decrease in the likelihood of being employed between 9.2% (4.4/48) and 11.3% (5.4/48). 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 between 4.4 and 4.8 percentage points, which represent a decrease of between 9.1% (4.4/48.3) and 9.9% (4.8/48.3). All these point estimates are statistically significant at the 1% level.

In the case of the average monthly wage, Table 8 shows that the three DiD models report a *short-term* negative effect of pretrial detention of between $18,200$ and $20,700$ Chilean pesos (CLP) on the average monthly wage (about 30 US dollars), which represents a decrease of between 9.9% $(18,200/184,000)$ and 11.3% $(20,700/184,000)$. When we focus our attention on the *mid-term* effects, the impact of pretrial on the average monthly wage is about $20,000$ CLP, which represent a decrease of 10.5% $(20,000/190,000)$. All these point estimates are statistically significant at the 1% level.

Table 7: Effect of pretrial detention on employment rate (Differences-in-differences models)

	OLS	Diff in Diff		
		Cross-Section	Panel	Matching
	(1)	(2)	(3)	(4)
Six months of employment				
Pretrial Detention	-0.085***	-0.054***	-0.054***	-0.044***
	(0.004)	(0.006)	(0.005)	(0.006)
R^2	0.32	0.14	0.77	.
Observations	89,918	179,836	180,178	89,918
Mean of dependent variable	0.480	0.500	0.500	0.480
Two years of employment				
Pretrial Detention	-0.080***	-0.046***	-0.044***	-0.048***
	(0.004)	(0.005)	(0.004)	(0.005)
R^2	0.29	0.26	0.76	.
Observations	79,317	158,634	158,942	79,317
Mean of dependent variable	0.483	0.497	0.497	0.483

Note: This table presents the results for the impact of pretrial detention on employment rate, considering three DiD models (using the sample described at Table 2). The results are presented for two time horizons: considering the average for the six months after verdict and the average for the two years after verdict. To group the results of many specifications in one table, it is only presented the point estimate and standard errors (in parenthesis) for the parameter of interest. Panel (2) presents the results from the cross-section DiD model (Eq. 2); Panel (3) presents the results from the panel DiD model (Eq. 3); and Panel (4) presents the results from the DiD matching model (Eq. 4). Panel (1) presents the OLS results (Eq. 1), which is useful as a reference point. The mean of the dependent variable is calculated by only considering the control group. ***, ** and * indicate statistical significance at the 1%, 5% and 10%, respectively.

There are two more aspects of these tables to highlight. In the first place, the magnitudes are very similar if we compare *short-term* to *mid-term* effects. The latter must be taken with a note of caution given that the mid-term effect estimation requires more months of data post treatment, thus we have a smaller sample. Indeed, as we show in Section 5.1.1, when we use the same sample – and specification – to estimate the effect of pretrial detention for different periods after treatment, we do find a clear reduction in the effect on employment over time in the point estimates. In the second place, in the case of employment and wages, OLS models present much higher point estimates. 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 in time and affect labor market outcomes.

Table 8: Effect of pretrial detention on average wage (Differences-in-differences models)

	OLS	Diff in Diff		
		Cross-Section	Panel	Matching
	(1)	(2)	(3)	(4)
Six months of wages				
Petrial Detention	-32,890***	-19,073***	-20,772***	-18,240***
	(1,873)	(2,781)	(2,271)	(2,219)
R^2	0.59	0.38	0.88	.
Observations	89,918	179,836	180,178	89,918
Mean of dependent variable	183,973	184,069	184,103	183,973
Two years of wages				
Petrial Detention	-31,849***	-19,540***	-20,392***	-21,061***
	(1,961)	(2,425)	(2,406)	(2,344)
R^2	0.54	0.48	0.86	.
Observations	79,317	158,634	158,942	79,317
Mean of dependent variable	190,803	182,408	182,402	190,803

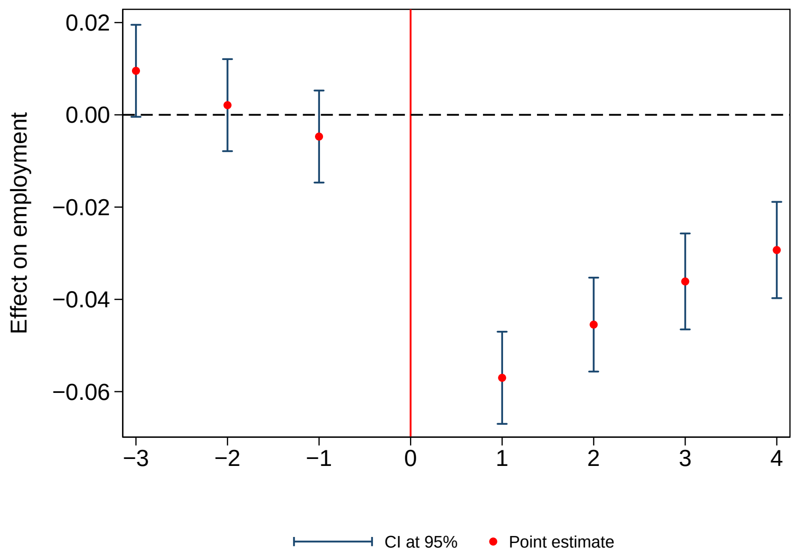
Note: This table presents the results for the impact of pretrial detention on average wage, considering three DiD models (using the sample described at Table 2). The results are presented for two time horizons: considering the average for the six months after verdict and the average for the two years after verdict. To group the results of many specifications in one table, it is only presented the point estimate and standard errors (in parenthesis) for the parameter of interest. Panel (2) presents the results from the cross-section DiD model (Eq. 2); Panel (3) presents the results from the panel DiD model (Eq. 3); and Panel (4) presents the results from the DiD matching model (Eq. 4). Panel (1) presents the OLS results (Eq. 1), which is useful as a reference point. The mean of the dependent variable is calculated by only considering the control group. ***, ** and * indicate statistical significance at the 1%, 5% and 10%, respectively.

5.1.1 General Approach

Figures 6 and 7 present the estimations of equation (5); i.e. 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 respectively. The dynamics of these two plots are similar and can be summarized into three findings. In the first place, the empirical strategy does not find a statistically significant effect before treatment, which is consistent with the parallel trend condition. Because pretrial detention occurs at different time for different individuals, and hence it is unlikely that there is another treatment (*i.e.,* policy) that happens at the same time as pretrial detention for all individuals, the relevant test to ensure the identification of the causal parameter in the DiD context is the verification of the pre-treatment parallel trends assumption. Thus, Figures 6 and 7 support the idea that the DiD estimates can be interpreted as a causal effect.

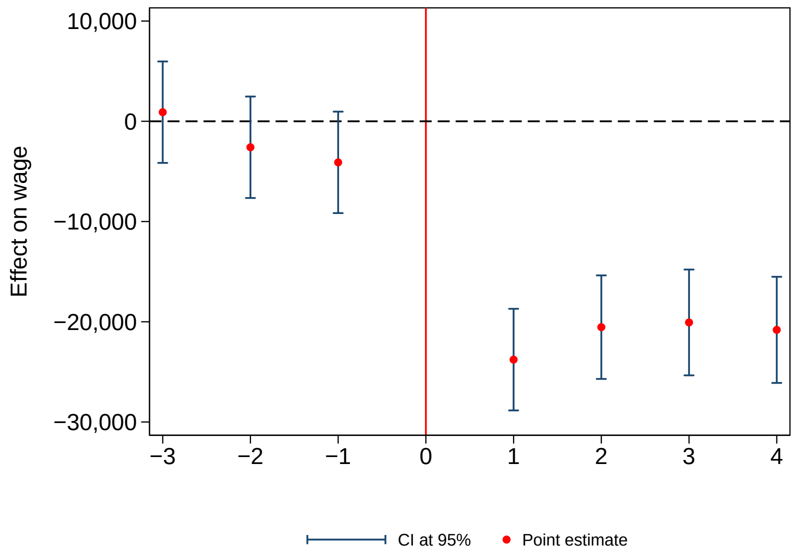
In the second place, there is statistically significant effect post treatment, whose magnitudes are in line with the point estimates presented at the beginning of this section (in Tables 7 and 8). Finally, there is a clear reduction in the point estimates in the case of employment over time, but the 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)



Note: This figure shows the point estimates and their 95% confidence intervals for the effect of pretrial retention on employment rate, by estimating Equation 5, considering the estimation sample described in Table 2.

Figure 7: Impact of pretrial detention on average wage (general approach)



Note: This figure shows the point estimates and their 95% confidence intervals for the effect of pretrial retention on average wage, by estimating Equation 5, considering the estimation sample described in Table 2.

5.1.2 Heterogeneity

Following a similar empirical strategy we used to estimate the average effect, we can study whether the effect of pretrial detention on labor outcomes increases with the the length of the pretrial detention. To do so, we follow the DiD matching approach, which provides a flexible and easy way to compare magnitudes across different groups.

To study this heterogeneity, we define terciles ($T_i \in \{1, 2, 3\}$) for the distribution of days in pretrial prison. Note that the amount of time varies incredibly over terciles, while the first tercile are imprisoned for 18 days on average, it jumps to 79 for the second and 251 for the third.

As we described in the empirical strategy section, for each treated individual we observe $\Delta Y_{i,t}(1)$ and for each individual in the control group, we can estimate $\widehat{\Delta Y}_{i,t}(0)$. Thus for each treated individual, we have an estimation of how his outcome changed as a consequence of the pretrial detention (i.e., $\Delta Y_{i,t}(1) - \widehat{\Delta Y}_{i,t}(0)$). Given this, we can calculate the average effect for tercile τ as:[16]

[16]Notice that by construction we estimate the average treatment effect for the treated for each tercile.

$$\frac{3}{N_1} \sum_{i:\ PreTrial_i=1} (\Delta Y_{i,t}(1) - \widehat{\Delta Y}_{i,t}(0)) \mathbb{1}[T_i = \tau],\ \forall\, \tau\ \in \{1, 2, 3\}. \tag{9}$$

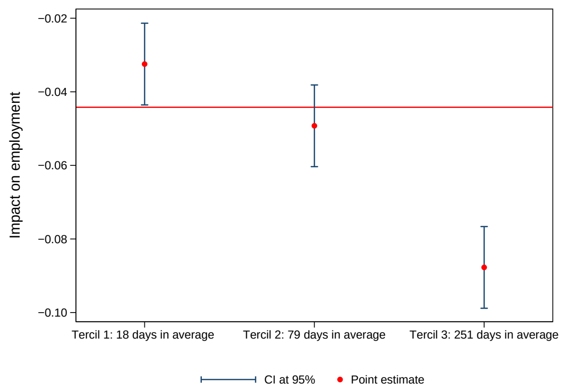
These estimated parameters are presented in Figures 8 and 9. These figures show the average treatment effect for the treated by tercile, and as a reference point, we also present (in the red line) the average treatment effect for the entire estimated sample, namely, the point estimates presented in Tables 7 and 8. In all cases, for both employment and wage, and for both short and mid-term, there is a clear pattern of a step gradient for the magnitudes of the treatment effect as individuals spend more time imprisoned. That said, for these three terciles the effect is statistically significant.

Specifically, the short-term effects on employment rate for the first, second, and third terciles are -1.6, -2.7, and -5.4 percentage points respectively. In case of the medium term impact, these numbers correspond to -0.01, -2.7, and -4.9. Thus, the mid-term effect for the third tercile represents a decrease of 11.2% (4.9/43.6). In the case of wages, the corresponding short-term effects are $-9,003$, $-20,981$, and $-24,127$ CLP. These numbers are equal to $-10,499$, $-19,706$, and $-20,601$ in the mid-term. Specifically the effect on the third tercile represents a decrease of 17.7% $(20,601/116,300)$.[17]

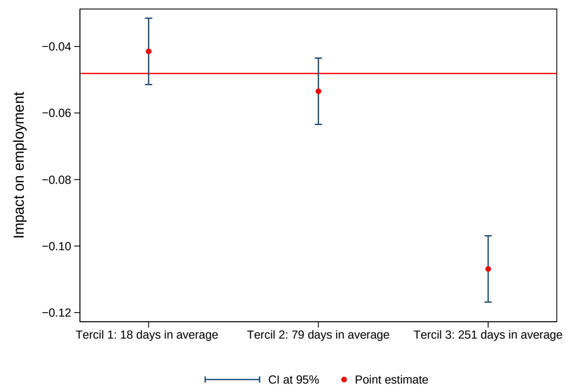
[17] All these results are consistent with the estimations reported in Appendix A, where we allow for heterogeneity depending on the time spent imprisoned, both in the cross section and in the panel DiD models.

Figure 8: Effect of pretrial detention on employment rate, by the duration of pretrial detentions

(a) sort-term effect: 6 months



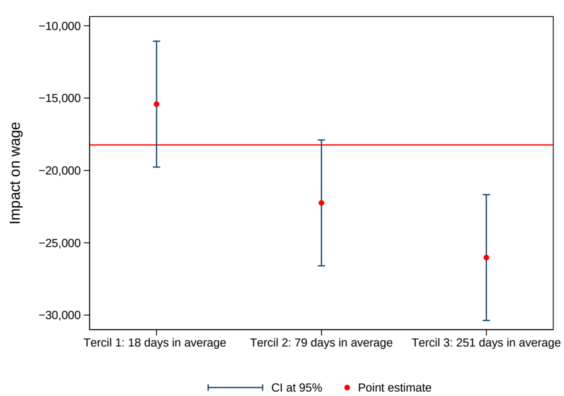
(b) mid-term effect: 24 months



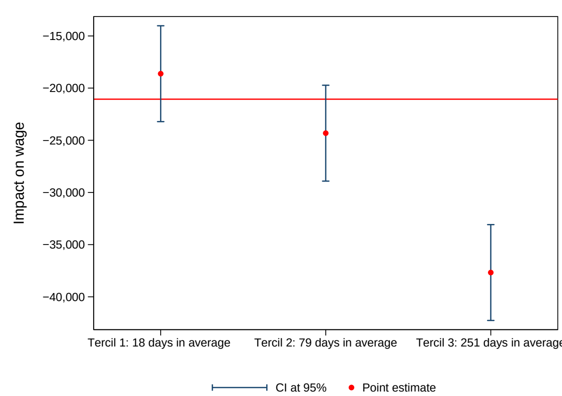
Note: These figures show the point estimates and their 95% confidence intervals for the effect of pretrial retention on employment rate, by estimating model for the three terciles of pretrial duration (Equation 9), considering the estimation sample described in Table 2.

Figure 9: Effect of pretrial detention on average wage, by the duration of pretrial detentions

(a) sort-term effect: 6 months



(b) mid-term effect: 24 months



Note: These figures show the point estimates and their 95% confidence intervals for the effect of pretrial retention on average wage, by estimating model for the three terciles of pretrial duration (Equation 9), considering the estimation sample described in Table 2.

5.2 Instrumental variable results

We now present the results of estimating the IV linear model described in section 4.2 (Eq. (7)), whichs takes advantage of the quasi-random assigment of detention judges, following the approach of Dobbie et al. (2018). Before reviewing the estimations, it should be recalled that even though our instrument passes all the tests designed to check its validity, this approach does not address the potential selection bias (described in section 4.3).

Table 9 presents the results of the IV linear model for the employment rate and monthly wages and for short and mid-term effects. The points estimates are much larger than then those obtained estimating DiD models (between six and three times), though with less precision. That said, it should be noted that while these are LATE estimates, the DiD models deliver ATT or ATE estimates, depending on the case. Overall these results, although very different in terms of magnitudes in respect to DiD models, much larger and less precise estimated, they give more more support to the hypothesis that pretrial detention has a relevant impact on employment and wages both in the short term (the following 6 months) and the longer term (following 24 months).

Table 9: Effect of pretrial detention on labor outcomes using the quasi-random assignment of detention judges (2SLS)

	Wage		Employment	
	Six months	Two years	Six months	Two years
Tratado	-169,201**	-113,484	-0.284**	-0.200
	(67,999)	(80,768)	(0.144)	(0.127)
Six months of wages	0.841***		0.000***	
	(0.011)		(0.000)	
Six months of employment	-69,959***		0.488***	
	(4,402)		(0.007)	
Two years of wages		0.885***		0.000***
		(0.015)		(0.000)
Two years of employment		-53,763***		0.549***
		(6,271)		(0.008)
Male	22,038***	26,951***	0.051***	0.058***
	(3,406)	(4,115)	(0.007)	(0.007)
Foreign	-6,921	-17,862	-0.026	-0.046*
	(10,051)	(14,152)	(0.023)	(0.024)
Indigenous	-11,079	-10,407	-0.012	-0.002
	(11,421)	(12,529)	(0.018)	(0.020)
Days in judicial process	46.7***	28.9***	0.000*	-0.000
	(8.72)	(9.30)	(0.000)	(0.000)
Crime severity	626.5	615.6	0.001	0.001*
	(446.5)	(517.72)	(0.001)	(0.001)
Fixed term contract	-21,702***	2,591	-0.055***	-0.001
	(2,358)	(2,462)	(0.004)	(0.004)
Sector = Mining	51,006**	44,984*	-0.020	-0.008
	(22,172)	(26,850)	(0.020)	(0.019)
Sector = Manufacture	6,863*	8,401*	0.015*	0.020**
	(3,841)	(4,366)	(0.008)	(0.008)
Sector = Electricity-Gas-Water	39,273	12,130	0.037	0.013
	(24,703)	(20,915)	(0.039)	(0.034)
Sector = Construction	2,165	-3,268	0.003	-0.011*
	(3,251)	(3,327)	(0.007)	(0.006)
Sector = Commerce	7,865**	17,071***	0.008	0.028***
	(3,709)	(3,903)	(0.008)	(0.007)
Sector = Services	8,671***	13,945***	0.009	0.014**
	(3,158)	(3,421)	(0.007)	(0.007)
Sector = Transportation-Communication	4,333	12,186***	0.009	0.017**
	(4,184)	(4,513)	(0.009)	(0.008)
Firm size = Small	10,001***	5,527*	0.012**	0.010
	(3,035)	(3,190)	(0.006)	(0.006)
Firm size = Midium	9,661***	4,546	0.013**	0.012**
	(3,207)	(3,265)	(0.006)	(0.006)
Firm size = Big	12,240***	5,872**	0.023***	0.023***
	(2,804)	(2,913)	(0.006)	(0.005)
Constant	-52,838**	-27,405	-0.158	0.068**
	(23,933)	(17,028)	(0.097)	(0.027)
Observations	46,081	40,842	46,081	40,842
Court by Time Fix Effects	Yes	Yes	Yes	Yes
Cluster at judge level	Yes	Yes	Yes	Yes

Note: This table presents the two stage least squared estimations for the impact of pretrial detention on average wage and employment rate (Equation 7), using data for the six months and the two years after verdict. The model is estimated on the sample described in the notes of Table 2. The IV is the judge severity measure, which is estimated following the procedure described in Subsection 4.2. All regressions include year interacted with court fixed effects. Robust standard errors clustered at the judge level in bracket. ***, ** and * indicate statistical significance at the 1%, 5% and 10%, respectively.

6 Mechanisms

There are several different mechanisms that can explain our results. On one hand, it could be the case that the impact of pretrial detention on labor outcomes is the general and natural effect of spending some months outside of the labor market. We denote this explanation as the *labor market mechanism hypothesis*. In this case, the effect of pretrial detention on labor outcomes is explained by the time that the individual was out of the labor market, and not by the reason why he wasn't in the labor market. On the other hand, it could be the case that pretrial detention carries an extra and specific impact on labor outcomes due to incarceration. We denote this explanation as the *incarceration mechanism hypothesis*. In this case, the reason why he was out of the labor market is relevant, not simply the non-participation.

6.1 The relevance of the labor market mechanism hypothesis

We assess the quantitative relevance of the labor market mechanism hypothesis in different ways. First, we compare the effect of pretrial retention to the effect of losing a job due to a negative shock that is beyond the worker'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 the treatment and control group samples, but only consider labor outcome data before prosecution (i.e. before our treatment). Specifically, we divide the period before the beginning of prosecution into three sub periods: between 17 and 24 months before prosecution (treating this as being before the firm bankruptcy), between 9 and 16 months before prosecution (treating this as the period in which the firm goes bankrupt), and between 1 and 8 months before prosecution (after potential bankruptcy, e.g. after job loss). In this context, the treated are those individuals who where working at firm j, 17 months before prosecution while that firm j went bankrupt between 9 and 16 months before prosecution. Naturally, the control group is composed by individuals who where working at firm j, 17 months before prosecution but that firm j did not go bankrupt between 9 and 16 months before prosecution. As in our panel DiD specification, we use data on wages and employment pre and post treatment, namely, labor outcomes averages between 17 and 24 months before prosecution and between 1 and 8 months before prosecution. For 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 10 and 11 present the results of this exercise for the employment rate and average wages respectively. In the case of employment, Table 10 shows a strong similarity between the impacts of pretrial detention and firm bankruptcy. Meanwhile Table 11 shows that the magnitude of the pretrial retention effect is 50% higher than the magnitudes of the impact of firm bankruptcy (33.000 versus 22.000), although this result is not statistically significant. It should be noted that these point estimates are different from the estimates reported in Tables 7 and 8, because in the current exercise – to try to compare the effects of these two treatments – we restrict the sample to those individuals who were working the month before the beginning of prosecution.

Table 10: A comparison between the effect on the employment rate of a firm bankruptcy versus the effect of pretial retention

	OLS	Diff in Diff		
		Cross-Section	Panel	Matching
	(1)	(2)	(3)	(4)
Pretrial Detention	-0.116***	-0.116***	-0.105***	-0.110***
	(0.008)	(0.009)	(0.007)	(0.010)
R^2	0.16	0.16	0.68	.
Observations	45,427	90,854	91,008	45,427
Mean of dependent variable	0.687	0.752	0.752	0.687
Firm bankruptcy	-0.155***	-0.124***	-0.123***	-0.113**
	(0.034)	(0.040)	(0.035)	(0.044)
R^2	0.15	0.26	0.68	.
Observations	45,530	91,060	91,318	14,851
Mean of dependent variable	0.654	0.717	0.717	0.654

Note: This table shows the point estimates and standard errors (in parenthesis) for the effect of pretrial retention and firm bankruptcy on employment rate, by estimating OLS, cross section DiD, panel DiD, and DiD matching models. To estimate the effect of a firm bankruptcy we use treatment and control groups, but considering their information before prosecution. In particular, we divide the period before the beginning of prosecution into three sub periods: between 17 and 24 months before prosecution (treating this as being before the firm bankruptcy), between 9 and 16 months before prosecution (treating this as the period in which the firm goes bankrupt), and between 1 and 8 months before prosecution (after potential bankruptcy, e.g. after job loss). Thus the treated are those individuals who where working at firm j, 17 months before prosecution while that firm j went bankrupt between 9 and 16 months before prosecution. Naturally, the control group is composed by individuals who where working at firm j, 17 months before prosecution but that firm j did not go bankrupt between 9 and 16 months before 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. ***, ** and * indicate statistical significance at the 1%, 5% and 10%, respectively.

Table 11: A COMPARISON BETWEEN THE EFFECT ON MONTHLY WAGES OF A FIRM BANKRUPTCY VERSUS THE EFFECT OF PRETIAL RETENTION

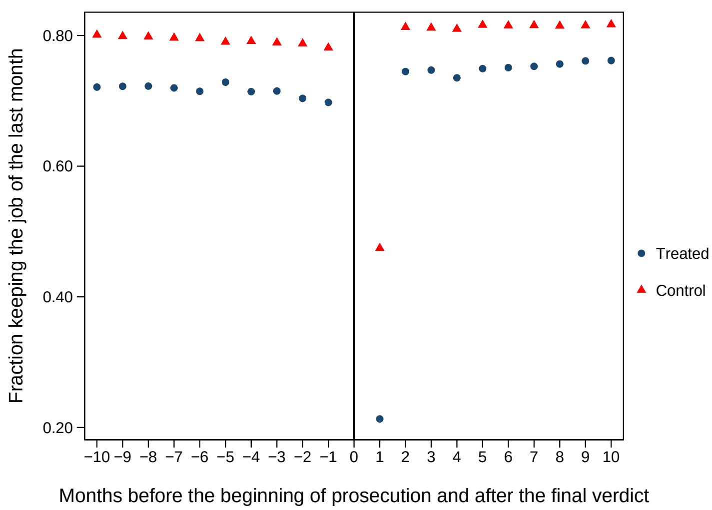
	OLS	Diff in Diff		
		Cross-Section	Panel	Matching
	(1)	(2)	(3)	(4)
Pretrial Detention	-46,350***	-34,998***	-32,987***	-35,878***
	(3,779)	(5,134)	(4,134)	(4,593)
R^2	0.61	0.49	0.89	.
Observations	45,427	90,854	91,008	45,427
Mean of dependent variable	290,424	302,296	302,296	290,293
Firm bankruptcy	-44,614**	-23,903	-22,048	-11,855
	(17,322)	(20,634)	(20,656)	(14,710)
R^2	0.56	0.63	0.87	.
Observations	45,530	91,060	91,318	14,851
Mean of dependent variable	264,757	278,167	278,167	268,037

Note: This table shows the point estimates and standard errors (in parenthesis) for the effect of pretrial retention and firm bankruptcy on average wage, by estimating OLS, cross section DiD, panel DiD, and DiD matching models. To estimate the effect of a firm bankruptcy we use treatment and control groups data before prosecution. In particular, we divide the period before the beginning of prosecution into three sub periods: between 17 and 24 months before prosecution (treating this as being before the firm bankruptcy), between 9 and 16 months before prosecution (treating this as the period in which the firm goes bankrupt), and between 1 and 8 months before prosecution (after potential bankruptcy, e.g. after job loss). Thus the treated are those individuals who where working at firm j, 17 months before prosecution while that firm j went bankrupt between 9 and 16 months before prosecution. Naturally, the control group is composed by individuals who where working at firm j, 17 months before prosecution but that firm j did not go bankrupt between 9 and 16 months before 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. ***, ** and * indicate statistical significance at the 1%, 5% and 10%, respectively.

A second approach to assess the quantitative relevance of the labor market mechanism hypothesis is to take advantage of the fact that we have the firm ID. Therefore we can compare the monthly probability of remaining employed at the same company pre and post treatment between control and treatment groups. We present these probabilities in Figure 10, where blue points represent the probabilities for the treated group and the red points represent the probabilities for the control one. Notice that almost all of the points in the figure show the probability of keeping the job at the firm where the individual was working the previous month, but the first blue and red points after verdict cannot refer to the previous' month's firm. In fact they represent the probabilities of keeping the job one held immediately before prosecution. As this figure shows, the dynamics of these probabilities are very similar pre prosecution and post trial with the exception of the first value after trial. This means that, conditional on having a job the month after the verdict (i.e., after incarceration for many of the treated), the difference in job stability between the control and treatment groups is very similar after and before judicial process.

This is indirect evidence that the labor market mechanism hypothesis is playing a relevant role. It does not seem very plausible that negative consequences due to incarceration disappear only one month after leaving prison. Note, however, that this is conditional on being employed after leaving prison due to pretrial detention.

Figure 10: Probability of keeping ones job



Note: This figure shows the probability of keeping the job in the same firm as the previous month, for control and treatment groups. Thus, this probability is only calculated for those who were working the previous month. When the horizontal axes is negative, it represents months before the beginning of the prosecution and when the horizontal axes is positive, it represents months after the final verdict. Notice that this axes equals to zero refers to a period that lasts differently across individuals, going from the beginning of prosecution to the end of trial. The values of the dots in period "1" is equal to the probability of keeping the job in the same firm as one month before the beginning of prosecution.

Taken together, the results of these two exercises seem to suggest that the most relevant mechanism behind our results – regarding the impact of pretrial detention on labor outcomes – is the fact that these individuals are forced to be out of labor market for some months.

6.2 The relevance of the incarceration mechanism hypothesis

We do not have data that would allow us to directly test the relevance of the specific to incarceration mechanism hypothesis. However, using our database we can empirically test whether being prosecuted has some effect on labor market outcomes. Because both the control and treatment groups are prosecuted, by definition this exercise cannot explain the effect of pretrial detention on labor outcomes, but this is an indirect way to study whether the social stigma that may arise because of prosecution (or incarceration) may play a relevant role in the determination of labor outcomes.

The nature of our data implies that we can only test the effect of prosecution using our control group sample, because they are the only individuals who face a *pure effect* of prosecution on labor outcome, given that they are not incarcerated during prosecution. In consequence, by construction, the result of this exercise -that is not considering the treatment group- cannot explain the outcomes differences that we find due to pretrial detention, between control and treatment groups.

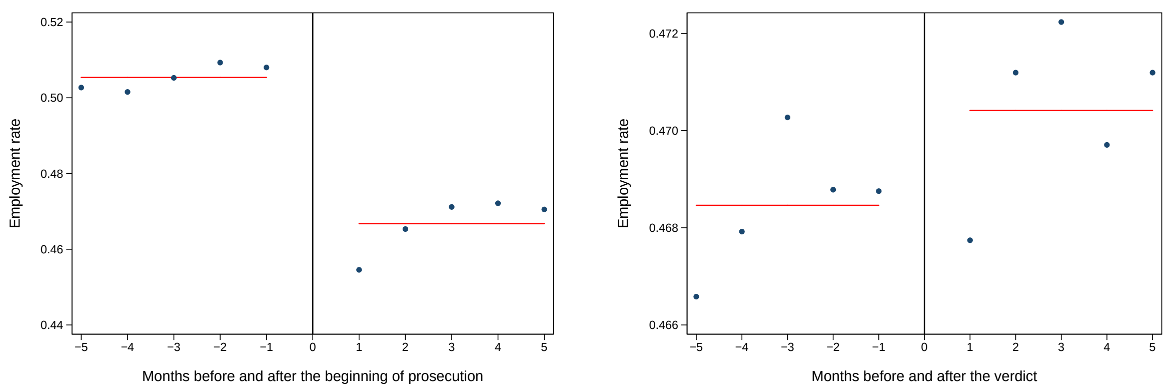
Thus we study the impact of the start of the prosecution and final verdict (without custodial sanction) on the employment rate for the control group. As stated, we focus on this group, because we seek to isolate the effect of positive or negative signals about criminal culpability from the effect of being incarcerated, which is not possible to do if we consider individuals who spent some time imprisoned. To do so, Figure 11 (panel a) presents the employment rate for the months before and after the prosecution's commencement. As can be observed, there is a reduction of between 4 and 5 pp in the employment rate right after this event. Likewise, Figure 11 (panel b) presents the employment rate for the months before and after the final verdict (innocent or a sanction that do not include imprisonment). This figure shows that there is a rise of 0.3 pp in the employment rate after a verdict of not guilty or noncustodial sanction. Given that in these two cases incarceration was not a factor, it is reasonable to attribute the observed effects to some factors that are specific to prosecution and not about being out of the labor market.

Overall, the discussion about possible explanations for our results point in the direction that the labor market mechanism is more important than the incarceration mechanism. However, the latter does not seem to be irrelevant. Indeed, these two conclusions are consistent with the results showed in Tables 8 and 9 (Section 5.1.2). On 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 were the most important mechanism, the actual time of incarceration would not be important. On the other hand, even in the case of individuals who only spend a few days incarcerated due to pretrial de-

Figure 11: Impact of judicial decisions on employment rate

(a) Pre and post prosecution beginning

(b) Pre and post final verdict



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

tention (remember the first tercile is on average 12 days), the effect of pretrial detention on labor outcomes has a relevant magnitude and is statistically significant. If stigma was not a factor, 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, socioemotional, and economic – for both the individual and for society. Indeed Ahumada et al. (2010) found that the fiscal cost of pretrial detention in Chile was 92.48 million US dollars in 2007. In this paper, we contribute to this discussion by estimating the negative impact of pretrial detention on labor market outcomes for those individuals who did not receive incarceration after their trials ended. 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 that we can study given our empirical strategy and data. Regarding mechanisms, our results suggest that the fact that pretrial detention forces individuals to be out of the labor market is more relevant that any extra cost due to incarceration (e.g. social stigma). However, the latter also plays a factor, just a lesser one.

To have a comprehensive evaluation of the impact of pretrial detention on the society is a complex task that is beyond the scope of this paper. Because the justification of pretrial detention is generally to promote the criminal investigation and trial, and because society values these outcomes, the evidence presented in this paper it is not enough to conclude that pretrial detention should not exist. There very well may be a benefit to society, of the advantages of pretrial incarceration on the criminal investigation. In other words, one relevant step in comprehensively evaluating pretrial detention would be to estimate the effect of eliminating pretrial detention on the investigative and procsecution processes.[18]

That said, from the point of view of the individuals who are affected by pretrial detention and then who are either found innocent or whose crimes are not deemed worthy of incarceration, pretrial detention is an unjustified individual cost. It is not unreasonable to discuss some kind of compensation or mitigation for such individuals, who are not an insignificant part of the legal system.

Regarding possible schemes to compensate for the effect of pretrial detention on labor outcomes, there are two impacts to consider. First, there is its direct effect on labor income. This effect should be simple to determine, given the average wage before

[18]An interesting take in this regard is the study of the impact of alternative precautory measures as in the case of Di Tella and Schargrodsky (2013).

pretrial detention and the time incarcerated due to this precautionary measure. Second, however, is the ex post effect of pretrial detention given by its negative impact on future average wages and employment probability, as estimated in this paper. Our findings suggest that both the control and treatment groups keep the same labor dynamics pre and post trial should they be returning to work right after the conclusion of their trials (see Figure 10).[19] Therefore a possible avenue to attenuate this ex post effect is to design public policies which promote a new employment directly following trial proceedings.

[19] This is in line with Engelhardt (2010), who points out the relevance of the duration of the job search after being released from prison.

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Appendix

A Heterogeneity estimating DiD linear models

Table 12: Effect of pretrial detention on employment rate, by the duration of pretrial detentions

	OLS	Diff in Diff	
		Cross-Section	Panel
	(1)	(2)	(3)
Six months of employment			
Pretrial Detention	-0.0650***	-0.0349***	-0.0355***
	(0.0058)	(0.0083)	(0.0061)
Pretrial Detention · Days Incarcerated	-0.0002***	-0.0002***	-0.0002***
	(0.0000)	(0.0000)	(0.0000)
R^2	0.32	0.14	0.77
Observations	89,918	179,836	180,178
Mean of dependent variable	0.4801	0.5000	0.5000
Two years of employment			
Pretrial Detention	-0.0587***	-0.0268***	-0.0265***
	(0.0056)	(0.0070)	(0.0056)
Pretrial Detention · Days Incarcerated	-0.0002***	-0.0002***	-0.0002***
	(0.0000)	(0.0000)	(0.0000)
R^2	0.29	0.26	0.76
Observations	79,317	158,634	158,942
Mean of dependent variable	0.4832	0.4968	0.4968

Note: This table presents the results for the impact of pretrial detention on employment rate ,allowing for an interaction with the days in prison, considering two linear DiD models (using the sample described at Table 2). The results are presented for two time horizons: considering the average for the six months after verdict and the average for the two years after verdict. The mean of the dependent variable is calculated by only considering the control group. ***, ** and * indicate statistical significance at the 1%, 5% and 10%, respectively.

Table 13: Effect of pretrial detention on average wage, by the duration of pretrial detentions

	OLS	Diff in Diff	
		Cross-Section	Panel
	(1)	(2)	(3)
Six months of wages			
Pretrial Detention	-28,482***	-16,519***	-16,947***
	(2,552)	(3,904)	(2,962)
Pretrial Detention · Days Incarcerated	-40**	-23	-35**
	(16)	(24)	(17)
R^2	0.59	0.38	0.88
Observations	89,918	179,836	180,178
Mean of dependent variable	183,992	184,103	184,103
Two years of wages			
Pretrial Detention	-25,453***	-15,218***	-15,303***
	(2,636)	(3,413)	(3,135)
Pretrial Detention · Days Incarcerated	-59***	-39*	-47**
	(17)	(22)	(19)
R^2	0.54	0.48	0.86
Observations	79,317	158,634	158,942
Mean of dependent variable	190,793	182,402	182,402

Note: This table presents the results for the impact of pretrial detention on average wage, allowing for an interaction with the days in prison, considering two linear DiD models (using the sample described at Table 2). The results are presented for two time horizons: considering the average for the six months after verdict and the average for the two years after verdict. The mean of the dependent variable is calculated by only considering the control group. ***, ** and * indicate statistical significance at the 1%, 5% and 10%, respectively.