The effect of pretrial detention of non-guilty people on labor market outcomes

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Abstract

The number of people incarcerated in Chile before being declared not-guilty in a final verdict increased 64.4% between 2007 and 2017, reaching approximately 3,000 individuals in 2017. This paper investigates the effects of this pretrial detention on labor outcomes. Using a data set that combines Chilean individual administrative data for pretrial incarceration and labor market outcomes between 2006 and 2016, we show a short term (i.e., six months) negative impact of pretrial detention on the probability of having a formal job of $-5.7$ to $-7.6\%$. In the case of wages, the short-term effect is about $-11.8\%$. These effects are between half and two thirds smaller, and still statistically significant, when we consider a mid-term horizon (i.e., 24 months). We also show that the effects are much larger for those who stayed longer in prison. Our estimations are robust to several placebo exercises.

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1 Introduction

Presumption of innocence is a keystone in all contemporary judicial systems and its violation contravenes Article N° 11 of the UN’s “Universal Declaration of Human Rights”. Regardless of whether it is “properly used”, one of the most damaging threats to this principle is pretrial detention. In fact, according to the *International Centre For Prison Studies*, in 2016 pretrial detentions represented 27% of total worldwide prisoners (see Walmisley (2016)), which means that approximately 2.57 million individuals were affected by this measure during 2016, an increment of 21% with respect to 2000. In Chile, the setting of this paper, the statistics of the Public Defender’s Office of Chile (Defensoría Penal Pública, DPP),¹ show that the ratio of pretrial detainees to total prisoners has risen from 21.9% in 2007 to 36% in 2017, an increase of 64.4% in that period.

As the Open Society Justice (UN) Initiative stated (see Open Society Fundations (2011)): “Pretrial detention is one of the worst things that can happen to a person: the detainee immediately loses his freedom, and can also lose his family, health, home, job, and community ties.”² This moral, social and economic cost is even more distressing when this legal measure is faced by individuals who are finally declared not-guilty by the same legal system that imprisoned them the first place. As in other countries, in Chile this phenomenon is not as rare as it should be and it is becoming more prevalent. According to DPP records, 1,500 innocent people were imprisoned during 2006, and that figure reached more than 2,800 in 2016, an increase of 90%. Furthermore, the time that these individuals spent incarcerated is not insignificant; for the 2,800 non-guilty incarcerated during 2016, 27% of them spent less than ten days in prison, around 53% between ten days and six months and, alarmingly, around 20% more than six months.

To contribute in assessing the negative impact of this precautionary measure, this paper evaluates one dimension of its costs, by studying the effect of pretrial detention on labor market outcomes for these non-guilty individuals, using Chilean data. To do so, we use a novel dataset that merges individual administrative data of pretrial incarceration and labor market outcomes between 2006 and 2016. The pretrial incarceration data comes from the DPP records and the labor market outcomes come from the ad-

¹This is the Chilean public institution that provides free legal representation for almost all individuals who have been imputed in a penal process.

²See also Open Society Fundations (2014) for further discussion on this important issue.
ministrative records of the Chilean unemployment insurance scheme. Therefore our data on employment and wages covers the individual labor performance of all people in the formal sector.

To isolate the effect of pretrial detention on accused but innocent people’s labor market outcomes, we restrict our estimation sample to individuals who were declared not-guilty in the final verdict of their penal process. Thus, the treatment group are non-guilty individuals who were imprisoned during (at least part of the) prosecution and the control group are non-guilty individuals who were always free during prosecution. Given that we have a panel which has many data points (i.e., months) before the beginning of the prosecution and after the final verdict, we can estimate the pretrial detention effect using different models that belong to the family of difference in difference estimators, namely, cross section diff-in-diff, panel diff-in-diff (controlling for individual fixed effects), and diff-in-diff matching. The last two methods take advantage of the longitudinal structure of the database.

By estimating these diff-in-diff models, we show a short term (i.e., six months) negative impact of pretrial detention on the probability of having a formal job of between $-2.5$ and $-3.3$ percentage points (pp), a $5.7 - 7.6\%$ reduction in that probability. In the case of wages, the short-term effect is around $-14,500$ Chilean pesos (CLP) per month, which represents a drop of $-11.8\%$. These effects are one half to two thirds smaller, and still statistically significant, when we consider a mid-term horizon (i.e., 24 months). We also show that the effects are much higher for those who stayed longer in prison.\footnote{This result is contrary to the finding in Kling (2006), who states that there is no substantial evidence of a negative effect of incarceration length on employment or earnings.} To do so, we divide the treatment group in terciles, according to the time they spent imprisoned, and we present the treatment effects for each tercile. In the case of the short-term effect on employment probability, the effects for terciles one, two, and three, are $-1.6$, $-2.7$, and $-5.4$ pp, respectively. In the case of the mid-term effect on wage, the effects for terciles one, two, and three, are $-9,003$, $-20,981$, and $-24,127$ CLP, respectively.

Our estimations are robust to placebo exercises, in which we estimate the effect on employment probability and average wage defining a fake treatment date, by only using pre treatment data. These exercises show that in all diff-in-diff models the point estimates are very small (positive and negative), all of which are not statistically significant.
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 one that estimates this effect for non-guilty individuals.

The closest paper to this research is ?, who show that pretrial detention decreases formal sector employment and the receipt of employment and tax-related government benefits. They use data linking over 420,000 criminal defendants from two large, urban counties in the US 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 in their database. Compared to our paper, their estimates should be viewed as an average of effects over different types of individuals who, suffering pretrial detention, could be non-guilty individuals (our group), individuals whose final verdict was guilty but with no time in jail, and those whose verdict does involve incarceration. Because of this, our estimates cannot be compared directly with theirs.

Our paper is also related to the literature that studies the negative impact of imprisonment on labor market outcomes, emphasizing effects through stigmatization, personal attitudes and capabilities, and discrimination (see Barbarino and Mastrobuoni (2014), Mueller-Smith (2015), Nagin et al. (2009), Western et al. (2001); see also Chalfin and McCrary (2017), and references therein, for a comprehensive literature review). Other evidence has shown that the negative effects could be moderate in magnitude and rather short-lived (see Grogger (1995)). A different result is provided by Bhuller et al. (2016), who use data from Norway to show that imprisonment increases participation in programs directed at improving employability, raising employment and earnings while discouraging further criminal behavior. Their findings demonstrate that time spent in prison could be pro human capital accumulation when individuals were not working prior to incarceration and when incarceration has a focus on rehabilitation.

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 concludes.
2 Pretrial Detention in Chile

Chile reformed its Criminal Justice System in a gradual processes which started in 2000 (in some geographic regions) and finalized its implementation in 2005. The reform put in place 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 (see Blanco et al. (2004) for a detailed description of this reform). 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), Guarantee Court (Juzgados de Garantía-special courts to safeguard the rights of the defendant and the victim during the investigation process) and Oral Criminal Trial Courts (Tribunales Orales de Juicio Penal). The DPP, which is one of the sources of information of this paper, records all defendants that use their services, including detailed information on the penal cause.

Unlike the legal system in the US, the defendants in Chile do not have the option of posting bail in order to avoid pretrial detention. The decision on this precautionary measure is delivered by a judge after it is requested by a prosecutor, and it is set in an instance where the defense attorney participates. The legal arguments that may be invoked by the prosecutor to request such a measure are the clear danger of escape by the prosecuted, the situation in which the defendant represents a danger to society and that the imprisonment of the prosecuted favors the investigation of the criminal case (see Riego and Duce (2011)). Despite the legitimacy that these arguments might have, it is a common complaint among DPP attorneys (and the director of the DPP) that this measure has been applied without rigurosity, particularly in the last few years.

In fact, the numbers support the complaint of DPP members and authority. In 2007 there were 63.1 individuals per 100,000 inhabitants affected by pretrial detention. This number increased to 88.8 individuals per 100,000 inhabitants in 2017, an increase of 40.7%. Moreover, the percentage of pretrial detainees in relation to total prisoners rose from 21.9% in 2007 to 36% in 2017, an increment of 64.4%.

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 blurred (see Riego and Duce (2011)). In addition to media influence and the pressure of public opinion, an aspect that may explain the strengthening of pretrial detention
in Chile 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. Among others measures, this law introduced the practices of “identity control” and “detention in case of flagrancy” as new instances that, in practice, expand the conditions to use pretrial detention.

In sum, although the new penal system, implemented between 2000 and 2005, was inspired by the presumption of innocence principle, Chile presents a high fraction of pretrial detainees in relation to total prisoners. Moreover, this fraction has been increasing the last decade.

3 Data and Stylized Facts

3.1 Data

We assembled an individual-level administrative dataset from the national employment insurance scheme (Ministry of Labor) and the Public Defender’s Office (Defensoría Penal Pública, DPP). The DPP is the institution in Chile which provides free legal representation for almost 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 observe the alleged crime but we do not observe the final verdict. Thus given our treatment and control group definition we do not include those cases in our sample. That said, less than 10% of the individuals prosecuted are only represented by a private attorney: with no participation of an attorney of the DPP. In this paper, we use the DPP’s records of prosecutions from 2006 to 2016.

The information provided by the DPP is very detailed about the imputed crime (the specific typification that we group in broader categories), the court, the time in jail during the prosecution (if any), and the final verdict. From the latter we observe whether the prosecuted person was finally declared guilty or not-guilty.

The source for the labor market data are the administrative records of the unemployment insurance scheme. As is detailed in Acevedo et al. (2010), the 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. 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. For this reason it took time to have all the workers contributing to the system, and full implementation was only achieved around 2010.

We focus on individuals whose prosecutions started after 2007, because in those cases we have enough labor market information before the treatment. Furthermore, we only keep data for first-time offenders (according to the data) because some prosecuted individuals may have low pretreatment labor numbers, only because they were imprisoned at that time.

To isolate the effect of pretrial detention on non-guilty people labor outcomes, we restrict our estimation sample to individuals who were declared non-guilty in their final verdict. Thus, the treatment group are non-guilty individuals who were imprisoned during (at least part of the) prosecution and the control group are non-guilty individuals who were always free during prosecution. Because there are many imputed crime categories that have very few individuals facing a pretrial detention, we restrict the control group (and also the treatment group) to individuals whose imputed crime category has at least a 10% of cases with pretrial detention. Finally, we restrict our sample to those individuals who have at least one month worked in the formal sector (i.e., contributing to the unemployment insurance scheme) during the two years before the beginning of prosecution. We argue that it does not make sense to estimate the impact of pretrial detention on formal labor market outcomes for individuals who never participated in the formal sector.

Table 1 shows the descriptive statistics for the estimation sample of the covariates and dependent variables. This table reports the information for the control and treatment groups separately. There are some aspects of these statistics that are useful to highlight. First, the individuals in the treatment group are accused of more severe crimes, a difference that is attenuated given the restriction we imposed to the control group to be part of the sample. We also observe that males are slightly more present in the treatment group, although in both groups males are overrepresented with respect to the general population (as is usually the case in crime accusations). Second, the treatment group is composed of individuals with low average wages (below minimum wage). There are three reasons for this, namely, the probability of being employed is low (between 44 and
51%) and the average wage calculation considers unemployment as a zero, they probably have low productivity (and low bargaining power), and they probably have other sources of income from the informal labor market.

Table 1: Summary statistics of the dependent variables and covariates

<table>
<thead>
<tr>
<th></th>
<th>Treated</th>
<th></th>
<th>Control</th>
<th></th>
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<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>S.D</td>
<td>Mean</td>
<td>S.D</td>
</tr>
<tr>
<td><strong>Outcome: average real wage</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Six months before trial</td>
<td>122,036</td>
<td>179,838</td>
<td>170,013</td>
<td>236,343</td>
</tr>
<tr>
<td>Six months after verdict</td>
<td>114,705</td>
<td>188,115</td>
<td>177,742</td>
<td>256,234</td>
</tr>
<tr>
<td>Two years before trial</td>
<td>116,340</td>
<td>149,497</td>
<td>162,471</td>
<td>207,866</td>
</tr>
<tr>
<td>Two years after verdict</td>
<td>117,594</td>
<td>174,992</td>
<td>175,548</td>
<td>234,797</td>
</tr>
<tr>
<td><strong>Outcome: employment probability</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Six months before trial</td>
<td>0.44</td>
<td>0.38</td>
<td>0.51</td>
<td>0.40</td>
</tr>
<tr>
<td>Six months after verdict</td>
<td>0.37</td>
<td>0.40</td>
<td>0.48</td>
<td>0.42</td>
</tr>
<tr>
<td>Two years before trial</td>
<td>0.42</td>
<td>0.29</td>
<td>0.50</td>
<td>0.32</td>
</tr>
<tr>
<td>Two years after verdict</td>
<td>0.36</td>
<td>0.34</td>
<td>0.46</td>
<td>0.36</td>
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<tr>
<td><strong>Covariates</strong></td>
<td></td>
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<td></td>
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<tr>
<td>Male</td>
<td>0.94</td>
<td>0.24</td>
<td>0.91</td>
<td>0.28</td>
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<tr>
<td>Foreign</td>
<td>0.01</td>
<td>0.08</td>
<td>0.01</td>
<td>0.08</td>
</tr>
<tr>
<td>Indigenous</td>
<td>0.02</td>
<td>0.14</td>
<td>0.01</td>
<td>0.11</td>
</tr>
<tr>
<td>Severe crimes</td>
<td>0.66</td>
<td>0.47</td>
<td>0.58</td>
<td>0.49</td>
</tr>
<tr>
<td>Judicial process in Metropolitan Region</td>
<td>0.28</td>
<td>0.45</td>
<td>0.34</td>
<td>0.47</td>
</tr>
</tbody>
</table>

3.2 Stylized Facts

Before presenting our empirical strategy and discussing our estimation results, we provide some graphical 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 wages, and the probability of being employed. In Figure 1, we show the employment rate (panel a) and average wage (panel b) for the periods before the beginning of the trial 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 individuals to calculate these averages by the number of months before the beginning of their trial, in which case the value in the horizontal axis is negative, or by the number of months after their final verdict, in which case 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 $225.8 \ (215.3)$ days, with standard deviation equal to $160.1 \ (182.1)$.

The second type of graphs is simply the difference in the employment rate (average wage) between the treatment and the control group, presented in Figure 2. Thus, while the first type of graphs is useful to highlight the level for the employment rate and wages for control and treatment groups and to see the pretreatment parallel trends between these two dynamics, the second type of graphs can also help to study the parallel trend (which in this case requires a constant path for the difference), and it is clearer in presenting the evidence for the main result of this paper by showing the change in the differences between control and treatment groups pre and post treatment.

Figure 1: **Employment and wages dynamics for the treatment and control groups**

(a) Employment

(b) Wages

There are different aspects of the data that can be learned from these plots. First, both before and after treatment, control individuals have a better performance in the labor market with respect to treatment individuals. During the months before prosecu-
tion, control individuals have on average between six and eight percentage points more than the treatment group and the differences in wages are between 40,000 and 50,000 CLP (which represent 16% and 38% of the control group average, respectively). Second, the parallel trends condition appears to be satisfied, which can be directly observed in Figure 1, and also in how constant the pre-treatment dynamics are in Figure 2. Third, there is a clear discrete change after pretrial detention in the differences between control and treatment groups in employment and average wage, which supports the main result of this paper. As Figure 2 (panel a) shows, in the case of employment this change is about 2.5 percentage points on average, and, as (panel b) shows, in the case of wages the change is around 12,000 CLP (which represent around 6% and 10% of the treatment group average, respectively). Finally, Figure 2 shows that the increase in the differences between treatment and control group on labor outcomes are more severe right after the final verdict, but the increase in these differences do not fade out even after two years after the end of the trial.

Figure 2: DYNAMICS FOR THE DIFFERENCES IN EMPLOYMENT AND WAGES BETWEEN TREATMENT AND CONTROL GROUPS

(a) Employment

(b) Wages
4 Empirical Strategy

We consider different empirical models to estimate the effect of pretrial detention on labor market outcomes. All but one (the OLS approach) have the same source of identification, which is the discrete change between pre and post trial outcomes, that is observed in both panels of Figure 2. This source of identification naturally leads to a difference in difference approach.

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, \( M \) months before the beginning of the trial (when \( t = 0 \)) and \( M \) months after the final verdict (when \( t = 1 \)), where \( M \) can be equal to 6 or 24 months. As stated, all the individuals considered in our sample had a not-guilty final verdict, but the treated (\( PREDET_i = 1 \)) were incarcerated during the prosecution (at least a fraction of it) and the control (\( PREDET_i = 0 \)) were always free. The function \( I(A) \) takes the value of one when \( A \) is true and zero otherwise. These models include a difference set of controls \( X \). The OLS model includes the following set of control variables, \( X_{1\text{ols}} \): gender, Chilean, indigenous, pre treatment average wage and employment rate (6 or 24 months before prosecution), location of the court (region), type of prosecuted crime, and year and month of the sentence. The difference in difference model using cross sectional data, includes the following set of control variables, \( X_{it}^{cs} \): gender, Chilean, indigenous, pre treatment average wage and employment rate (using months 36 to 27 before prosecution), location of the court (region), type of prosecuted crime, and year and month of the beginning of the prosecution (when \( t = 0 \)) and year and month of the sentence (when \( t = 1 \)). The difference in difference model using panel data, includes the following set of control variables, \( X_{it}^{p} \): year and month of the beginning of the prosecution (when \( t = 0 \)) and year and month of the sentence (when \( t = 1 \)). Finally, the difference in difference-matching model, includes the following set of covariates to match treated with controls, \( X_{it}^{m} \): gender, Chilean, indigenous, pre treatment average wage and employment rate (using months 36 to 27 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 \( \beta \). For example, \( \hat{\beta}_{cs} \) is the estimated effect of pretrial detention using a difference in difference approach which only exploits the cross-sectional dimension of the data (i.e. without controlling for individual fixed effects). The specifications of the estimated models are the following:
OLS:

\[ Y_{i1} = \alpha + \beta^{ols}PREDET_i + X^{ols}_{i1}\gamma + \epsilon_{i1} \]  

(1)

Difference in difference, cross section:

\[ Y_{it} = \alpha_0 + \alpha_1PREDET_i + \alpha_2I(t = 1) + \beta^{cs}PREDET_i \times I(t = 1) + X^{cs}_{it}\gamma + \epsilon_{it}, \quad t \in \{0, 1\}. \]  

(2)

Difference in difference, panel:

\[ Y_{it} = \alpha I(t = 1) + \beta^pPREDET_i \times I(t = 1) + X^p_{it}\gamma + \omega_i + \epsilon_{it}, \quad t \in \{0, 1\}. \]  

(3)

**Difference in difference, matching:** this is an approach proposed by Heckman et al. (1997), which combines matching technique with the advantages of difference in difference with individual fixed effects. Let \( Y_{i,t}(PREDET_i) \) denote the potential outcome of individual \( i \) at time \( t \), and \( \Delta Y_{i,t}(PREDET_i) = Y_{i,1}(PREDET_i) - Y_{i,0} \) the potential increase in the outcome \( Y \) between time \( t = 0 \) and \( t = 1 \). Then, the average treatment effect (ATE) is defined as \( \beta^m = E[\Delta Y_{i,t}(1) - \Delta Y_{i,t}(0)] \).

For individuals that were treated we observe only \( \Delta Y_{i,t}(1) = Y_{i,1} - Y_{i,0} \) and we impute \( \Delta Y_{i,t}(0) \) using the matching procedure. For each \( i \) treated \( (PREDET_i = 1) \), we assign the set of matches \( J_G(i) \) corresponding to the \( G \) nearest-neighbors in the untreated group \( (PREDET_i = 0) \) 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 \( i \) \( (PREDET_i = 0) \) 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 \( \widehat{\Delta Y}_{i,t}(1) = \frac{1}{G} \sum_{j \in J_G(i)} \Delta Y_{j,t} \). An estimate of the average treatment effect, as:

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

(4)
where \( N_1 = \sum_{i: \text{PREDET}_i = 1} 1 \), is the total number of treated individuals and \( N_0 = \sum_{i: \text{PREDET}_i = 0} 1 \), 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 \( \beta \), the last two are the ones with less demanding conditions. In particular, they need that there is 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 the next section we see that unlike OLS, the other three models report similar results, which is consistent with the fact that the pre trend dynamics between treated and controls seems to be parallel, something that is confirmed by the placebo exercises we perform at the end of the next section. Nevertheless, the OLS approach is only presented as a reference point, given that the assumption that we have included all relevant covariates, which is the assumption that OLS requires to deliver causal parameters, is not realistic in this context.

5 Results

The results of estimating the empirical models described in the previous section are shown in tables 2 and 3, for the effect of pretrial detention on employment and wages, respectively.

In the case of the employment rate, considering 6 months before the beginning of prosecution and 6 months after the final verdict, Table 2 shows that the short-term effect of pretrial detention is between 2.5 and 3.3 percentage points, taking into account the three difference in difference models. These effects represent a decrease of between 5.7% (2.5/43.6) and 7.6% (3.3/43.6). Considering 24 months before and after the treatment to calculate the employment rate, the mid-term effects of pretrial detention on the employment rate is between 0.8 and 2.1 percentage points, which represents a decrease of between 1.9% (0.8/42.3) and 5% (2.1/42.3). All these point estimates are statistically significant, except for the diff-in-diff matching estimator considering 24 months to calculate the employment rate.

In the case of the average wage six months before the beginning of prosecution and six months after the final verdict, as Table 3 shows, the three difference in difference models report a short-term negative effect of pretrial detention of around 14,500 CLP on the average wage, which represents a decrease of 11.8% (14,500/122,000). When we
Table 2: Effect of pretrial detention on employment

<table>
<thead>
<tr>
<th></th>
<th>OLS</th>
<th>Diff in Diff</th>
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<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>Cross-Section (2)</td>
</tr>
<tr>
<td></td>
<td>(3)</td>
<td>Panel Matching (4)</td>
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<table>
<thead>
<tr>
<th>Six months of employment</th>
<th></th>
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<tbody>
<tr>
<td>Pretrial Detention</td>
<td>-0.052***</td>
<td>-0.032***</td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
<td>(0.011)</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.29</td>
<td>0.11</td>
</tr>
<tr>
<td>Observations</td>
<td>16,717</td>
<td>33,434</td>
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<table>
<thead>
<tr>
<th>Two years of employment</th>
<th></th>
<th></th>
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</thead>
<tbody>
<tr>
<td>Pretrial Detention</td>
<td>-0.036***</td>
<td>-0.021***</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.31</td>
<td>0.24</td>
</tr>
<tr>
<td>Observations</td>
<td>16,717</td>
<td>33,434</td>
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</tbody>
</table>

focus our attention on a longer period to calculate average wages pre and post treatment (24 months), the *mid-term* effects of pretrial on average wage is between 9,952 and 6,392 CLP, which represent a decrease of between 8.5% (9,952/116,300) and 5.4% (6,392/116,300). All these point estimates are statistically significant, most of the cases at 1%.

In these two cases, employment and wages, the OLS models present much higher point estimates. These magnitudes are consistent to the fact that, unlike the other models, OLS does not control for unobserved variables that are stable in time and affect labor market outcomes.
<table>
<thead>
<tr>
<th></th>
<th>OLS (1)</th>
<th>Cross-Section (2)</th>
<th>Panel (3)</th>
<th>Matching (4)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Six months of wages</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Petrial Detention</td>
<td>-18,608***</td>
<td>-13,136**</td>
<td>-12,061**</td>
<td>-14,419***</td>
</tr>
<tr>
<td></td>
<td>(4,488)</td>
<td>(6,571)</td>
<td>(4,678)</td>
<td>(3,762)</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.52</td>
<td>0.30</td>
<td>0.86</td>
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<tr>
<td>Observations</td>
<td>16,717</td>
<td>33,434</td>
<td>33,434</td>
<td>16,089</td>
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<tr>
<td><strong>Two years of wages</strong></td>
<td></td>
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<td></td>
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<tr>
<td>Petrial Detention</td>
<td>-15,316***</td>
<td>-14,067***</td>
<td>-12,684***</td>
<td>-6,393*</td>
</tr>
<tr>
<td></td>
<td>(4,257)</td>
<td>(5,346)</td>
<td>(4,570)</td>
<td>(3,442)</td>
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<tr>
<td>$R^2$</td>
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<td>0.41</td>
<td>0.83</td>
<td>.</td>
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<tr>
<td>Observations</td>
<td>16,717</td>
<td>33,434</td>
<td>33,434</td>
<td>16,089</td>
</tr>
</tbody>
</table>

### 5.1 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 is higher the longer the time individuals were in prison before the final verdict. To do so, we follow the diff-in-diff matching approach, which provides a flexible and easy way to compare magnitudes.

To study this heterogeneity, we define the terciles ($T_i \in \{1, 2, 3\}$) for the distribution of days in pretrial prison. These terciles group very different people in terms of the time spent imprisoned. The individuals who belong to the first tercile have 12 days imprisoned on average, and this number is 69 and 220 days in the case of tercile 2 and 3, respectively.

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

$4$Notice that by construction in this case we estimate for each tercile the average treatment effect for the treated.
\[
\frac{3}{N_1} \sum_{i: \text{PREDET}_i=1} (\Delta Y_{i,t}(1) - \Delta \overline{Y}_{i,t}(0))I(T_i = c), \quad \forall \ c \in \{1, 2, 3\}. \tag{5}
\]

These estimated parameters are presented in Figures 3 and 4. These figures present the average treatment effect for the treated who belong to each tercile, and as a reference point, we also present (in red) the average treatment effect for the entire estimated sample, namely, the point estimates presented in Tables 2 and 3. In all cases, for employment and wage, and for the short and mid-term effect, there is a clear pattern of a step gradient for the magnitudes of the treatment effect as individuals spend more time imprisoned.

Specifically, in the case of employment, the short-term effects for terciles 1, 2 and 3 are $-1.6$, $-2.7$, and $-5.4$ percentage points, respectively. In case of the medium term, these numbers correspond to $-0.01$, $-2.7$, and $-4.9$. Thus, the mid-term effect for the tercile 3 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 case of the mid-term effect. Relatively, the mid-term effect for the tercile 3 represents a decrease of 17.7\% ($20,601/116,300$).

**Figure 3: Heterogenous effect of pretrial detention on employment**

(a) SHORT-TERM EFFECT: 6 MONTHS

(b) MID-TERM EFFECT: 24 MONTHS
5.2 Placebo exercises

Taking advantage of the long time for which we observe employment and wage data for treatment and control groups, we run placebo estimations for all the models considered in Tables 2 and 3, to rule out the possibility that our results are biased because the latent outcome –labor outcomes that would be observed in the absence of pretrial detention—and the propensity to face a pretrial detention are simultaneously determined. The intuition of these exercises is the same that is behind the formal tests for pre-treatment parallel trends, in the sense that to have causal estimations, treatment and control groups may have different latent outcomes but these differences should be constant over time.

These placebo exercises are built as follows: (1) we only consider wage and employment data before the beginning of prosecution; (2) we define treatment and control groups as we did previously, namely, treated are those who faced a pretrial detention and controls are those who did not; (3) we change the timing of the treatment, by creating a fake treatment in month $FT$, such that this fake treatment is between 18 and 7 months before the real beginning of prosecution ($FT \in \{-18, -17, ..., -7\}$). In practice, this changes the definition of $Y_{i,t}$, such that if, for example, $FT$ is equal to $-12$, the placebo value of $Y_{i,0}^P$ is the average value of $Y$ between 24 and 13 months before the beginning of prosecution and the placebo value of $Y_{i,1}^P$ is the average value of $Y$ between 11 and 1 months before the beginning of prosecution. (4) Given these
values of the dependent variable we run the same models specified by Equations 1, 2, 3, and 4. The covariates for these placebos are almost all the same as \( X_1^{ols}, X_{cs}^{it}, X_{it}^{p}, \) and \( X_{it}^{m} \) (respectively). The only type of covariates that differ are those that specify months and years (pre or post treatment), given that the timing in the placebo exercise is by construction different than the timing of the real treatment.

Figure 5 presents the placebo results for employment, which replicate the models presented in Table 2, and figure 6 shows the placebo results for wages, which replicate the models presented in Table 3. As can be observed, all the placebo exercises, but OLS estimations, show point estimates which are close to zero and all of them are not statistically significant, supporting the idea that the main results of these paper can be interpreted as a causal effect. The fact that OLS models show significant effect, and with relevant magnitudes, reinforces the concern that there are some variables that we do not observe as econometricians that affect pre and post treatment labor outcomes and the probability of facing a pretrial detention, which biases the OLS estimations.
Figure 5: PLACEBO EFFECT OF PRETRIAL DETENTION ON EMPLOYMENT

(a) OLS

(b) DIFF-IN-DIFF CROSS SECTION

(c) DIFF-IN-DIFF PANEL

(d) DIFF-IN-DIFF MATCHING
Figure 6: Placebo effect of pretrial detention on wage

(a) OLS
(b) Diff-in-diff cross section
(c) Diff-in-diff panel
(d) Diff-in-diff matching
6 Conclusion

In this paper we estimate the negative impact of pretrial detention of non guilty individuals on labor market outcomes. The effects found on employment rate and average wage are considerable and they last for at least two years, which is the maximum horizon that we can study given our empirical strategy and the period for which we have data.

Being imprisoned before the verdict, when this verdict is finally not-guilty, can imply a negative impact in many dimensions, namely, psychological, socioemotional, and economical. In an interesting report, Ahumada et al. (2010) states that the fiscal cost of pretrial detention in Chile was 92.48 million dollars in 2007. In this paper, we contribute to this agenda by estimating one relevant part of the individual economic cost.

There are different mechanisms that can explain the result we find in this paper. It could be the case, for instance, that the impact on the average wage and the employment rate is the direct and simple effect of spending some months outside the labor market due to incarceration, a cost due to either human capital depreciation or because individuals are foregoing years of experience. In any of these cases, the effect of pretrial detention could be entirely explained in terms of the time that these individuals lost. It could also be the case, however, that pretrial detention carries with it an extra cost due to social stigma.

Another mechanism that could be assessed is the relevance of the first job right after leaving prison. As can be observed in Figure 2, the largest impact of pretrial detention on labor market outcomes occurs right after the end of prosecution. Thus, it could be the case that if some public policy achieves the goal of ensuring a good job to these individuals right after the end of their pretrial detention, then most of the labor market effect could disappear.

We are currently studying these mechanisms, which will be included in a next draft.

Given that pretrial detention of non-guilty persons can be thought, at least ex post, as a pure loss of social welfare, it should be an outcome that everybody should agree on the necessity of decreasing its prevalence. In this context, our results could be useful to raise awareness about the prevalence and negative impact of this controversial measure.
References


