

# Scarring effects of sudden involuntary unemployment: Evidence for Chile

Elías Albagli      Mario Marcel      Antonio Martner  
Matias Tapia

Central Bank of Chile

This version: June 3, 2019

## Abstract

Using an administrative census for all formal firms and employees in Chile between 2005 and 2017, we estimate the wage and employment costs of job displacements. Our main contribution to the literature is to distinguish between unexpected displacements, associated to sudden firm closures, and expected displacements, in which the firm closure process occurs over a long period of time. We document that wage losses, even in the long run, are much larger for workers that experience unexpected displacement. This is consistent with the idea that workers cannot prepare for abrupt terminations - by looking for a job earlier, for example - and thus suffer more from being displaced. Moreover, abrupt closures are not subject to changes in the composition of labor observed in slower closures. We also find that losses are heterogeneous across workers, and are larger for older individuals and workers with larger tenure and previous wage growth within the firm. This is consistent with theories of job specific human capital/match quality. (JEL J31, J63, J64)

## 1 Introduction

Involuntary unemployment spells can have significant costs for workers in terms of future wages, the probability of being unemployed, and the overall

evolution of their labor market perspectives. Conceptually, these costs can arise from various sources<sup>1</sup>. Involuntary separations can destroy value, either because they break a high quality worker-firm specific match or because job (sector) specific skills are lost. In that case, wages can fall, either because the worker has to rebuild her job ladder searching for a new high quality match, a process that can take time and involve several job transitions, or because she has to acquire new skills (Jacobson, LaLonde, and Sullivan 1993 (JLS from now on). Additionally, the unemployment spell itself can depreciate human capital (Becker 1962), interrupting learning by doing processes, particularly in early stages of working life (Oreopoulos, Von Wachter, and Heisz 2012). Alternatively workers with high tenure may have wages that exceed their actual productivity by leveraging their seniority (Salop and Salop 1976), so their wages can be significantly lower if forced to reallocated. Finally, involuntary separations can be costly in terms of signaling , reducing future wages (Gibbons and Katz 1991), and with long lasting durable effects of wage losses (Couch and Placzek 2010).

However, estimating these costs can be hard, due to selection bias in the set of displaced workers. Typically, involuntary unemployment is not a random and exogenous process, but reflects poor performance or mismatch between firms and workers. Thus, displaced workers have less skills or worse performance than those that remain employed, and their overall labor trajectories after displacement might simply reflect their personal characteristics, and not the impact of being displaced. As a consequence, estimations of the effects of displacement using all involuntary job separations in the economy can be biased.

The seminal paper by JLS address these concerns by using massive layoffs/firm closures as a source of exogenous variation in individual displacements. Under this interpretation, displacement is exogenous from the perspective of the individual worker, and is not (at least not solely) related to her performance. The general idea behind this identification strategy has been adopted in a large and growing literature for the US; Flaaen, Shapiro, and Sorkin 2017 , Lachowska, Mas, and Woodbury 2018, among the most recent) and several advanced economies as the UK (Gregory and Jukes 2001),

---

<sup>1</sup>see Carrington and Fallick 2017 for a survey of the various theories developed in the literature.

Norway (Eliason and Storrie 2006), Portugal (Raposo, Portugal, and Carneiro 2015) and Austria (Schwerdt 2011). While the main empirical regularities of this literature suggest effects of displacements in lifetime earnings are large (due both to the effect in future wages and the frequency of employment) and persistent, its range is wide and depend on workers characteristics and macroeconomic conditions, quantitative results are sensitive to the specific definition of treatment and control groups.

Using a census of formal employment for Chile between 2005 and 2017, our main contribution to the literature is to distinguish between expected and unexpected displacements. In particular, we distinguish between "sudden" firm closures, in which the firms showed no clear early signs of its demise, and expected closures, in which the firm's closing process occurred over a longer time. In all the previous literature, this distinction is not made, and the set of displacement episodes is a mixture of anticipated and unanticipated events. We argue that these types of displacements are conceptually different in at least two dimensions and, in consequence, it is likely that the economic consequences associated to them also differ.

First, workers that receive early signals of displacement can prepare for the event in various ways, such as increasing the intensity of on-the-job-search while their job still holds or accumulating assets. Anticipation should allow them to create a buffer that can smooth out the effect of displacement, at least in the short term, both because they potentially can have access to better options and because they might be better prepared to face unemployment. Workers that are surprised by displacement, on the other hand, might have not been searching for a job as actively, so on impact their set of new job options might be worse. Moreover, as they were not able to prepare beforehand, they might be forced to be accept worse jobs, as they have not created a buffer. The distinction is also relevant because the composition of workers changes as firms experience a process of decline over time. As documented in Schwerdt 2011, the composition of early leavers versus stayers in firms that shrink over time before closing is not random. This implies that significant selection biases might still be present if the set of mass layoffs/firm closures includes firms who were dying/struggling over a significant period. On those cases, the exogenous displacement shock was probably anticipated by the firm and its workers, and separation decisions prior to the displacement event already reflected that.

Qualitatively, our results are consistent with the international literature, in the sense that job displacements are associated to significant and persistent income losses for the average worker. However, as expected, the distinction between sudden and protracted displacements is statistically and economically relevant. Wage losses associated to sudden firm closures are much larger than those found with the standard closure event used in the previous literature. We believe this is a significant insight that was not present in the existing literature.

Our results suggest that workers that were relatively unaware of the probability of being displaced, and thus were surprised to a significant extent, experience larger costs. Interestingly, this is not only true on impact, but also over a longer horizon, suggesting strong persistence of the consequences of initial employment decisions made by surprised displaced workers under distress. Additionally, abrupt closures are not subject to the changes in the composition of labor observed in slower closures. These results are robust to workers and firms characteristics (see Section 4), as well as the definitions of the treatment and control groups and the displacement event (see Appendix D).

The paper also explores the differences in impact across different types of workers. Differences in wage losses across worker types appear to be consistent with ideas on human capital specificity/idiosyncratic match quality and the costs of the destruction of valuable job ladder. In particular, losses are larger for older workers, workers with larger tenure in the firm, and workers that had steeper wage growth in the closing firm. Workers with long tenure and steeper wage growth were probably in particularly successful matches, and have a hard time finding a match of similar quality after displacement, while older workers might have less time and incentives to invest in rebuilding their skills given the proximity of retirement.

The rest of the paper is organized as follows. Section 2 describes the data, while Section 3 discusses identification issues and presents its results, Section 4 discusses the heterogeneity in results by workers and firms characteristic and the timing of reemployment. Section 5 concludes.

## 2 Data

Our data comes from Chile’s Internal Revenue Service (Servicio de Impuestos Internos, SII). The SII dataset has unique identifiers for both workers and companies, allowing us to track individuals and firms over time. Identifiers are anonymized to guarantee confidentiality. All formal firms in the country must report to the SII, so the data is a census of all firms and the complete labor force with a wage contract in Chile. Each firm must present an annual statement reporting the sum of wages, overtime wages, labor earnings and any other similar income (excluding disability, pensions and retirement payments) for each individual worker with a labor contract. While the statement (and the income information) is annual, firms must also report the specific months in which a worker was employed in the firm. Thus, for any given month, we can identify the employment status of an individual worker, and a measure of her average monthly labor income in that year. From the firm side, we can identify the composition of its workforce and wage structure at any particular month.

Given that we only observe workers when they are employed in a firm, “unemployment” is strictly non-employment in the formal sector. Thus, it can include actual unemployment, inactivity, self employment, informal employment etc. The data also contains information on the firm’s economic sector. Firms also report information on variables such as sales, intermediate costs, and capital, which can be used to calculate measures of productivity.

Tax records do not provide information on worker characteristics. However, we merge the data on workers income and employment status with data from Chilean Civil Registry and Identification Service (Registro Civil e Identificación) to get information on gender and date of birth. Also we merge the SII data with Labor Direction (Dirección del Trabajo) on end of labor relationship reason. To preserve the confidentiality of individual tax statements, we never have access to the real firm and worker identifiers (RUTs) in the tax records. SII directly provides the data to the Central Bank with fake identifiers. Merges with other datasets are done directly by SII with the same fake identifiers.

Over the complete dataset, we do some additional work. For each worker, we define a unique main relationship at any point in time (the one with

highest wage). Firms with less than 10 workers are dropped. And finally we try to distinguish actual firm closures from events that look like firm property transformation, re-branding, branches or M&As, in which a significant share of the closing firm’s workforce is immediately reallocated in a new firm.

Monthly data is grouped into quarterly data, to generate a full industry coverage linked employer-employee panel for 2005-2017 period on quarterly basis. More than 5.5 million workers employed in more than 250k firms, are observed in the data. The database structure allows us to observe firm closures, which are defined as firms that report workers on quarter  $t$ , but not on quarters  $t + 1$ ,  $t + 2$  and all following quarters.

### 3 Identification and results

#### 3.1 Estimation strategy

The JLS 1993 seminal paper for displacement literature set the first identification strategy based on a difference in difference feature described below.

$$E(w_{it}|D_{i,s} = 1, I_{i,s-p}) - E(w_{it}|D_{i,v} = 0)$$

Where  $w_{it}$  denotes the earnings of worker  $i$  at date  $t$  and  $D_{i,s} = 1$  if worker  $i$  was displaced at date  $s$  (and  $D_{i,s} = 0$  otherwise). The information available at date  $s - p$  is given by  $I_{i,s-p}$  and  $p$  is sufficiently large that the events that eventually lead to displacement would not have begun by date  $s - p$ . Using this feature, the estimation strategy developed by JLS 1993, is presented in equation 1, and will be used for all estimations in this work.

$$w_{it} = \alpha_i + \gamma_t + \beta X_{it} + \sum_{k=-m}^t D_{it}^k \delta_k + \epsilon_{it} \tag{1}$$

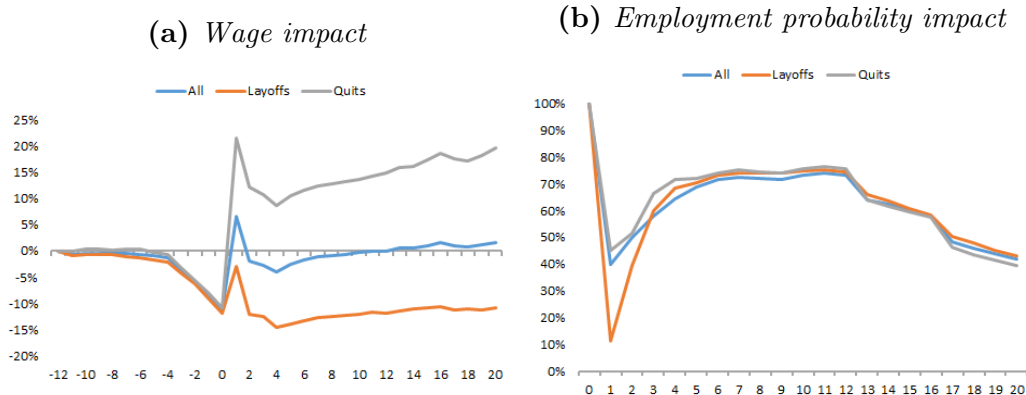
Where  $w$  is natural logarithm of the wage received by worker  $i$  at period  $t$ .  $\alpha$  stands for a worker fixed effect and  $\gamma$  represents the time effect, the year in this case. The vector  $X$  considers workers observable characteristics, gender and age in this setup.  $D_k$  is a dummy representing the distance of the current observation with respect to the displacement event. This means that  $D_0$  will take the value 1 at the time of the displacement event,  $D_1$  will be 1 the quarter following the displacement and zero otherwise,  $D_2$  will take the

value 1 two periods after displacement and so on ( $D_{-1}$  equals to 1 one quarter before displacement event). Considering  $m$  periods previous to displacement and  $t$  periods after displacement. Then, parameter  $\delta_k$  will represent the effect of percentage wage loss or gain  $k$  periods before or after displacement, with respect to the control group, and become the most relevant parameter in this equation to observe wage changes.

### 3.2 Normal turnover

To start our analysis, we use the JLS 1993 strategy explained before, but focus on the effects of normal turnover. This is, for workers that were employed in a firm in quarter  $t - 1$  but not in  $t$ , and got reemployed at some point, we estimate wage losses and employment probability, shown in Figure 1. Of course, this does not address identification concerns, as a large share of these transitions are voluntary and endogenous to worker characteristics, but this illustrates the mechanisms of job transitions in the data, and the potential sources of bias.

**Figure 1:** *Normal turnover.*



The wage process around a job separation episode can be decomposed in four stages. Initially, up to one year before separation there is almost no difference in wage with respect to all other workers not changing jobs at the same time. During the second stage, s one year before separation, there is a relative decline in wage with respect to workers that do not lose their jobs. The third stage reflects adjustment after job separation, which typically lasts

up to a year. The final stage is associated to the long term impact on wage of the job separation. As for the probability of being employed, shown in panel (b), only 10 % of workers can get a job the immediate quarter after changing a job, but five quarters after the probability is stabilized at 70 %, and starts going down after three years. These four stages can be identified in most of the estimations in this paper.

### 3.3 Literature applied to Chilean data

To provide a direct comparison of the previous literature to the Chilean data, and before moving forward to our baseline estimation, we will replicate two identification strategies. First, the identification exercise used by JLS 1993 and most of the subsequent literature, based on massive layoffs events. Massive layoffs are defined as events in which (a) employment in a firm falls at least 30% in a given quarter (2008(Q4) in this case) (b) employment in that quarter is less than 130% of the employment level in the firm four quarters before; and (c) the firm has at least 50 employees in all quarters. Under this definition, displaced workers must (a) have at least 24 quarters of tenure at the time of the massive layoff; (b) lose their job up to 4 quarters before the massive layoff and (c) get a new job by the end of sample (2017 (Q4) in our data). The control group includes workers that were continuously employed in other firms until the massive layoff and did not lose their jobs after it. In this exercise both  $m$  and  $t$  are 12 because of the treatment group construction and the time series available in our data. As a second exercise, we replicate the identification strategy in Schwerdt 2011, that uses firm closure as the event that defines displacement, and where workers need to have (a) at least one year of tenure by the moment of the work loss and (b) work until one or two quarters before the firm closure, in order to enter the treatment group. The control group is formed by workers with at least one year of tenure that do not lose their jobs at the same time as workers in the treatment group.

Figure 2 presents results of both identifications. While there is an impact of being displaced using the two methodologies, there are many differences between both identification strategies results, from the starting point of wage differences between treatment and control groups, to the four different steps explained in the normal turnover subsection. The point here is that identification of treatment and control group together with the definition of

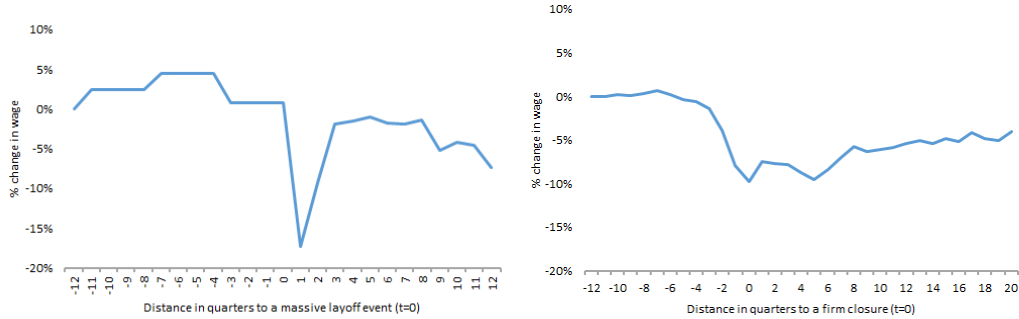


displaced workers matters in wage impacts found.

**Figure 2:** *Two Literature identification strategies*

(a) *Jacobson, LaLonde & Sullivan, 1993*

(b) *Schwerdt, 2011*



More than the actual definition of massive layoff or firm closure, both seems to be use quite often in the literature ( see Flaaen, Shapiro, and Sorkin 2017 ) identification on treatment and control group are the key definitions in order to avoid as much as possible bias that every labor relationship end rely. The standard approach that has dominated the displacement literature have at least three main issues that could harm the identification in terms of introducing bias. The first one is the definition that yields on control groups including workers that have never been displaced, a subset of the labor force with high wage growth and strong observable and unobservable characteristics. As discussed in Krolikowski 2016, different control groups, that allow for unemployment in the control group, can yield significantly different results. The second one is the absence of considerations of workers taking employment decisions when actually observing (or at least suspecting) their employer decadency. And the third one is consider displaced workers leaving the firm at their period of firm’s disappearance to be similar with workers leaving the closing firm up to one year before its actual end of activities, as suggested by Krolikowski 2016 and Schwerdt 2011. We explore more deeply these problems and propose solutions for the three issues in the next subsections.

### 3.4 Identification strategy

Even-though massive layoffs might be relatively uncorrelated with the performance of individual workers, firm closures could be a cleaner exercise, as

they affect all workers in the firm at the same time. Moreover, it can include closures even in firms with less than 50 workers.

The treatment and control group construction has been largely discussed in the literature due its importance on displacement effects under different definitions of both groups. Flaaen, Shapiro, and Sorokin 2017 did a treatment and control group sample selection documentation of most papers published until November 2017, documenting also different measure of displacement measures. Workers and firms characteristics are both relevant dimensions where definitions has to be made. Even tough many different approaches of both dimensions definitions have been used in the literature, we follow the philosophy (not the exact same definition for treatment and control groups) propose by Krolkowski 2016, adapting its proposal for treatment group to Chilean data available and define the control group without conditioning on post displacement status. We now explain both groups construction.

### **Treatment group**

In our baseline estimation, a worker has to meet two sets of conditions to be included in the treatment group. From the firm perspective, i) the worker has to be employed in a firm that experiences a closure (the firm identifier disappears in a given quarter and do not show again in the data), and ii) the firm must have, at the moment of closure, ten or more employees. From the worker perspective, to be eligible to enter the treatment group a worker must, i) stop working in a firm up to eight quarters before closure, ii) have at least 12 quarters of tenure before leaving the job, and iii) get re-employed by 2017 Q4. The treatment group includes only workers with at least three years of previous labor history with respect to firm separation and that have at least three years ahead to get reemployed. This latter rule is impose in order to have workers with at least 12 quarters of tenure and at least 12 quarters of future information. Because the tax data cover the 2005-2017 period, displaced workers must lost their jobs between 2008 and 2014 in order to enter the treatment group.

### **Control group**

The control group used is composed by workers continuously employed at least 12 quarters within the period 2008-2014 (in any firm), that do not

work at a closing firm and do not lose their job at the same time as the set of treated workers. Quarters with no reported wages will be accounted as missing values.

For both groups, there is a discussion in the literature about imputing zeros to the wage of non-employed workers. This imputation exacerbates measured wage losses, in particular when workers can work independently or in the informal sector, and will not be used in this work. Our main interest is to focus on observed wage losses of employed workers who suffer a displacement event, with respect to workers that did not lose their job.

Following the discussion from the massive layoffs setup, we relax the requirements to participate in the control group as Krolikowski 2016 suggested, which is now composed by workers that are not displaced at the same time as the treatment group but could be displaced in other quarters.

### **Firm closure definition**

We allow for two kinds of firm closure, the normal one, where a closure is defined when a firm is observed in period  $t$  for the last time, meaning it does not appear on periods from  $t + 1$  on. Changes in composition prior to firm closure imply that the process that leads to the displacement event has consequences in the decisions regarding employment of both workers and firms. In consequence, and looking forward for a refined identification we define a second type of firm closure; sudden firm closures, in which workers are displaced abruptly, as cleaner source of exogenous variation, closer to the idea of an unanticipated shock. Sudden closures are defined as firms that had no decline on total employment from four quarters before closure until its closing quarter, meaning that the average firm employment did not decline until the quarter the firms closes<sup>2</sup>. This does not mean that the firm has the same group of workers- that there was no rotation- , but that net employment flows were non-negative.

### **Firm leaving timing**

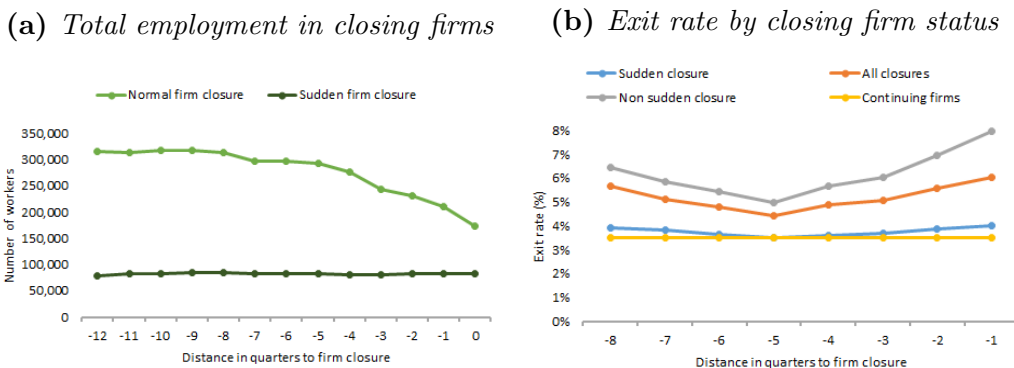
---

<sup>2</sup>We perform the same strategy using revenue instead of employment for robustness reasons, results are presented in the Appendix D.

As documented by Schwerdt 2011, identification on treatment can be troublesome if the composition of the firm’s workforce changes prior to displacement. In many cases, closures are not unexpected, even though the final stage of sustained and observable decadence is uncertain. In that case, workers anticipate that future displacement is likely, and will make decisions accordingly if they can. As Figure 3a confirms, many workers leave the non sudden closing firm from 8 quarters before closing. Two effects may arise. On the one hand, high-skilled workers, with better job market prospects, might start actively looking for a new job, jumping out the ship as soon as a good chance emerges to escape to a new job with a similar wage. On the other hand, the troubled firm might try to improve its prospect by firing its less productive workers, or those for which firing costs are smaller. These two effects can significantly bias the set of workers at the time of displacement in a firm that has experienced a sustained deterioration, relative to a firm that closes abruptly.

When workers can anticipate their firm downfall, they might start looking more actively for a job. If the search is successful, the chance to get more employment offers raises and consecutively is more likely to get a new job. Figure 3b suggest that the latter is true, by showing that as the anticipated firm (non sudden closures) closure approaches the exit rate of the firm (ratio workers leaving the firm over total employment) raises, while the exit rate for sudden closures remain stable and close to the continuing firms one.

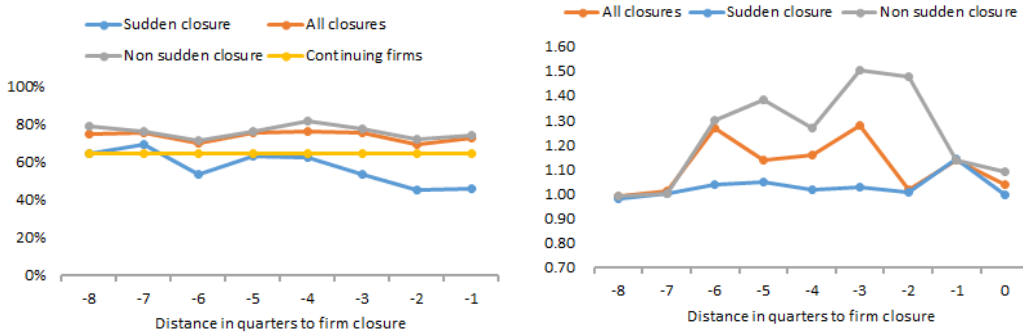
**Figure 3:** *Employment dynamics before closing by closing type*



To reinforce the idea that workers from anticipated closures increase their job search intensity, we check for job to job transitions <sup>3</sup> (JTJ) with respect to total workers who leave the firm at a given quarter. While Figure 4a shows that consistently workers from sudden closures have a small JTJ exit percentage than non sudden closures, 4b reveals that JTJ transitions come back wages increase from 7 to 2 quarters before firm closure. This could mean that workers who in fact observe firm downfall, increment their job search intensity, get job offers and accept those jobs, which pay much more than jobs offered to workers involve in no JTJ transitions.

**Figure 4:** *Job to Job transitions*

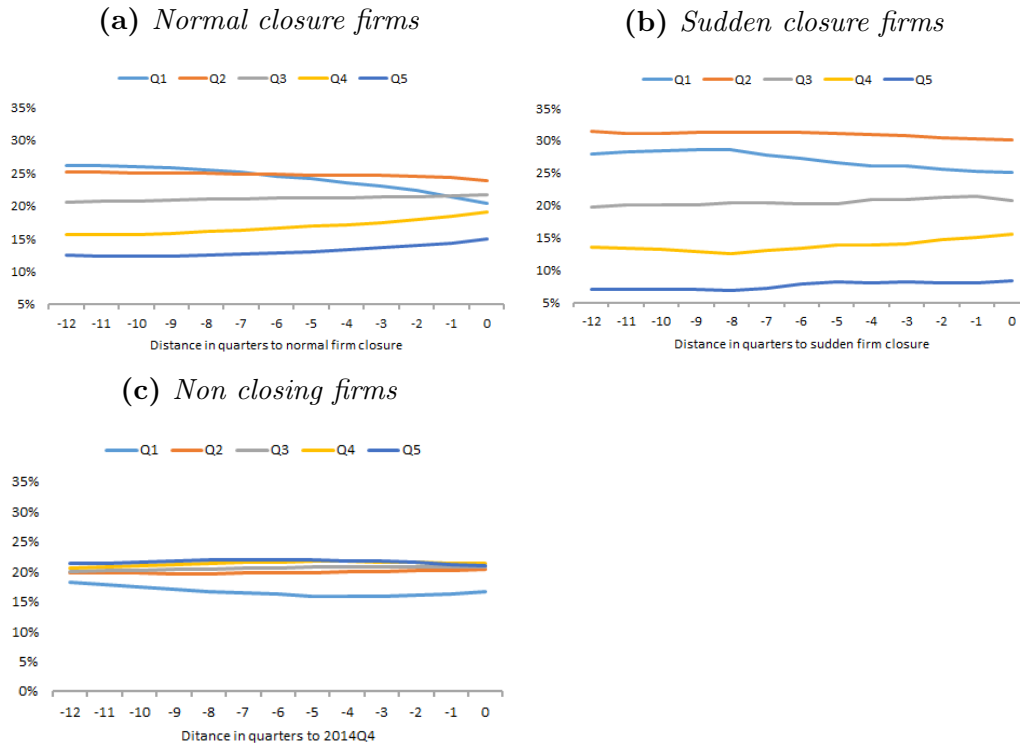
(a) *JTJ exits as percentage of total exits* (b) *Ratio JTJ vs NTJT come back wage*



To check for composition bias existence within the treatment group, we rank workers by wage quintiles (where the first is the lower wage one) and graph them by distance to firm closure. We differentiate for normal and sudden closures and compare them with no closing firms (Figure 5). Even though wage quintiles from sudden closures firms have different wage quintile levels than non closing firms, they remain relatively stable as firm closure approaches, similar to non closing firms. In contrast, normal firm closures suffer changes in their wage composition quintiles when approaching to the closure, highlighting the existence of changes in workers composition of firms that do not close abruptly from 5 quarters before its closure.

<sup>3</sup>workers who leave the firm in the quarter  $t$  and get reemployed in a different firm the immediate next quarter ( $t + 1$ ).

**Figure 5:** *Worker composition by firm type (wage quintiles)*



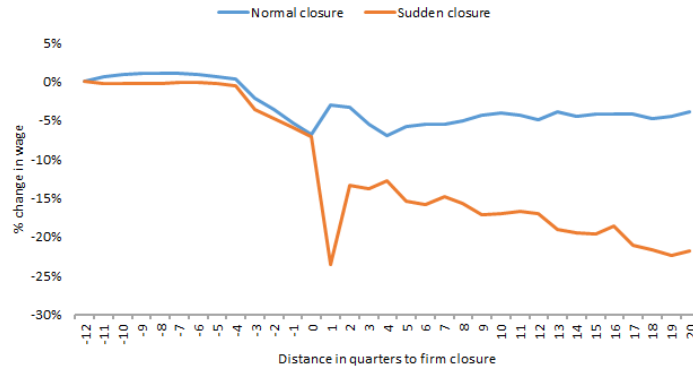
Appendix A show basics statistics of the data base used, characterizing normal closure and sudden closure treatment groups.

### 3.5 Results

We have information up to 36 quarters before the event of displacement for all displaced workers, never the less, we will only show twelve periods before firm closure because of the tenure rule of treatment group (at least 12 quarters of tenure). Alternatively we will focus our analysis on post displacement effects only up to five years after displacement effects (we have up nine years); where more than 70% of displacement workers have data. Figure 6 show equation 1 estimations for the complete treatment group, workers who leave the firm 8 to 0 quarters before closure, for normal and sudden firm closures. We normalize to zero the difference between treatment and control group 12 quarters before

closure because of the tenure rule.

**Figure 6:** *All displacements. Displaced workers from 0 to 8 quarters before closure*

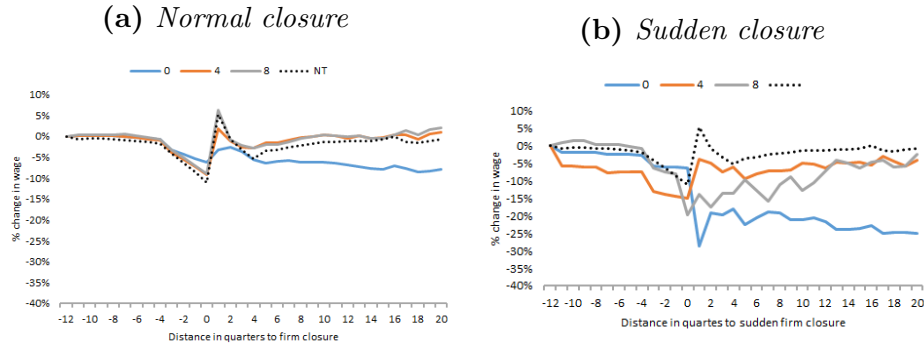


As discussed before, one source of bias might come from the distance to firm closure that workers leaves a firm. Using treatment and control groups definitions, 9 different treatment subsets are built conditional on workers leaving the firm from 8 to 0 quarters before closure. Treatment 0 include workers that leave the firm the closing quarter, treatment 1, workers that leave the firm one period before it closes, and so on until treatment 8. For each treatment group, we generate a control group following the rules explained subsection 3.4. Looking at the effect in net terms<sup>4</sup> (Figure 7 ) and comparing the effects with normal turnover effects (NT) helps to observe more clearly the effect. The latter remain true and even deepen the wage losses when looking only to males between 25 and 55 years old (prime workers).

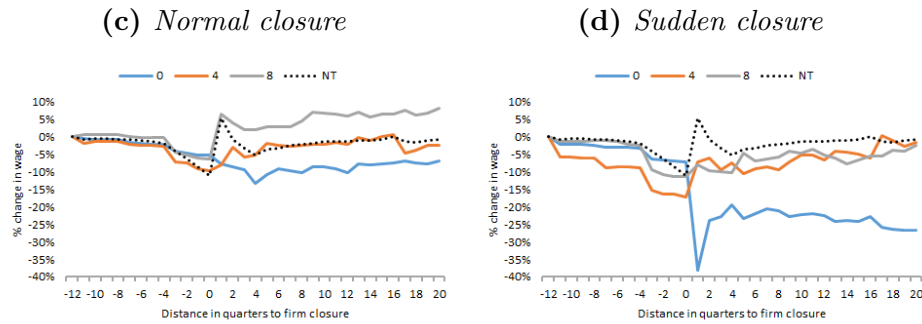
<sup>4</sup>Net terms account for the wage losses normalizing to zero wage differences between treatment and control groups 12 quarters before closure.

**Figure 7: NET effects by distance to closure separation**

All workers



Prime workers



From the latter we can acknowledge at least 3 facts; fact 1) Initial differences between treatment and control groups in wage (see from 12 periods before displacement) vary depending on the distance to closure separation, for both normal and sudden closures. As confirmed when looking observable characteristics on workers<sup>5</sup>, displaced workers are intrinsically different to workers in the control group. Fact 2) Long term losses (5 years after displacement) are way bigger for workers who leave the firm at closure, compare to early leavers ( 4 and 8 quarters before closing), for both types of closures. Remarkably, net effects on wage of treatment groups 4 and 8 are very similar and close to zero in the long run. Moreover, early leavers have almost no losses in wages 20 quarters after displacement, and look like normal turnover shown before. Fact 3) The net cost in wage of being surprised by a sudden

<sup>5</sup>See Appendix B for age, gender, wage and tenure treatments and control group compositions.

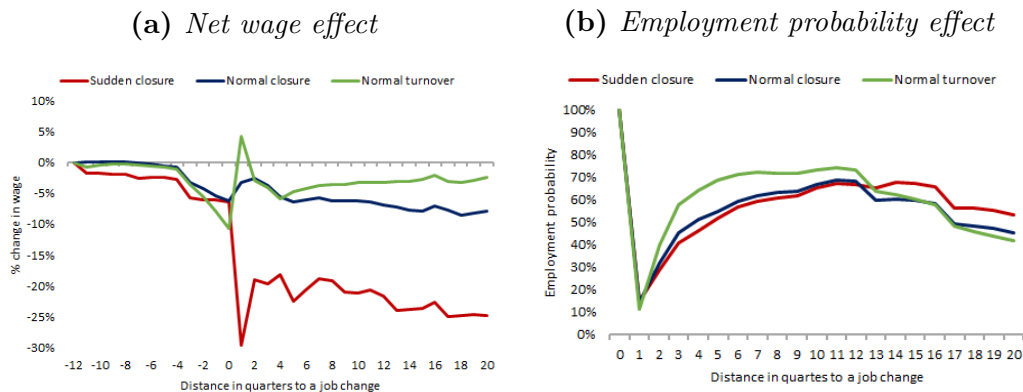


closure and remain until the end of the firm is 17% higher than being involved in a normal firm closure process until the end (-25% vis a vis -8%).

Thus, both the initial characteristics of workers, and their outcomes after displacement, vary significantly along the firm's closing process. Then, this identification allowed us to clean two issues introducing wage cost biases, the timing of separation (by looking at workers who remain at the firm until it closes), and the fact that workers observe firm's downfall taking employment decisions (using sudden closures).

Figure 8 briefs the main results found for workers remaining at the firm until its last quarter of life. While using a normal closure identification strategy, wage losses after 5 years of a worker's displacement will represent an 8% wage loss, significant, but only 6% bigger loss than normal turnover loss in the same period (2%). When using the sudden firm closure identification strategy the wage loss 5 years after displacement jumps up to 25%, suggesting a huge cost in wage for those workers. In terms of employment probability, displaced workers do get lower employment rates than normal turnover, but being involved in a sudden closure does not affect in terms of employment much more than suffer a job loss in a normal closure episode (Figure 8, panel b)

**Figure 8:** *Wage and employment probability effects comparison*



## 4 Effects heterogeneity

The impact found in the previous section on unemployment and wage due to sudden firm closures episodes were calculated for the Chilean economy as a whole. While aggregate effects are useful to document the phenomenon, in order to understand its causes we decompose the impact found by workers characteristics, timing of reemployment and year of displacement. We will use the sudden firm closure approach for workers who remain at the firm until its closure in this section, and show only wage cost; employment probabilities analysis is well represented in the latter Figure.

### 4.1 Worker's characteristics

We separate all displaced workers in tenure quartiles at the displacement quarter<sup>6</sup> and estimate their wage losses. Figure 9 shows that while having from 12 to 14 quarters of tenure (quartile 1) at time of displacement generates a 9% wage loss 5 years after displacement, having 21 to 32 quarters of tenure (quartile 4) generates a 31% wage loss for the same period of time. So as higher the level of tenure when the displacement take place, higher is the wage loss. Consistent with ideas of models with on-the-job human capital accumulation, job ladders (Krolikowski 2017) , and match-specific productivity.

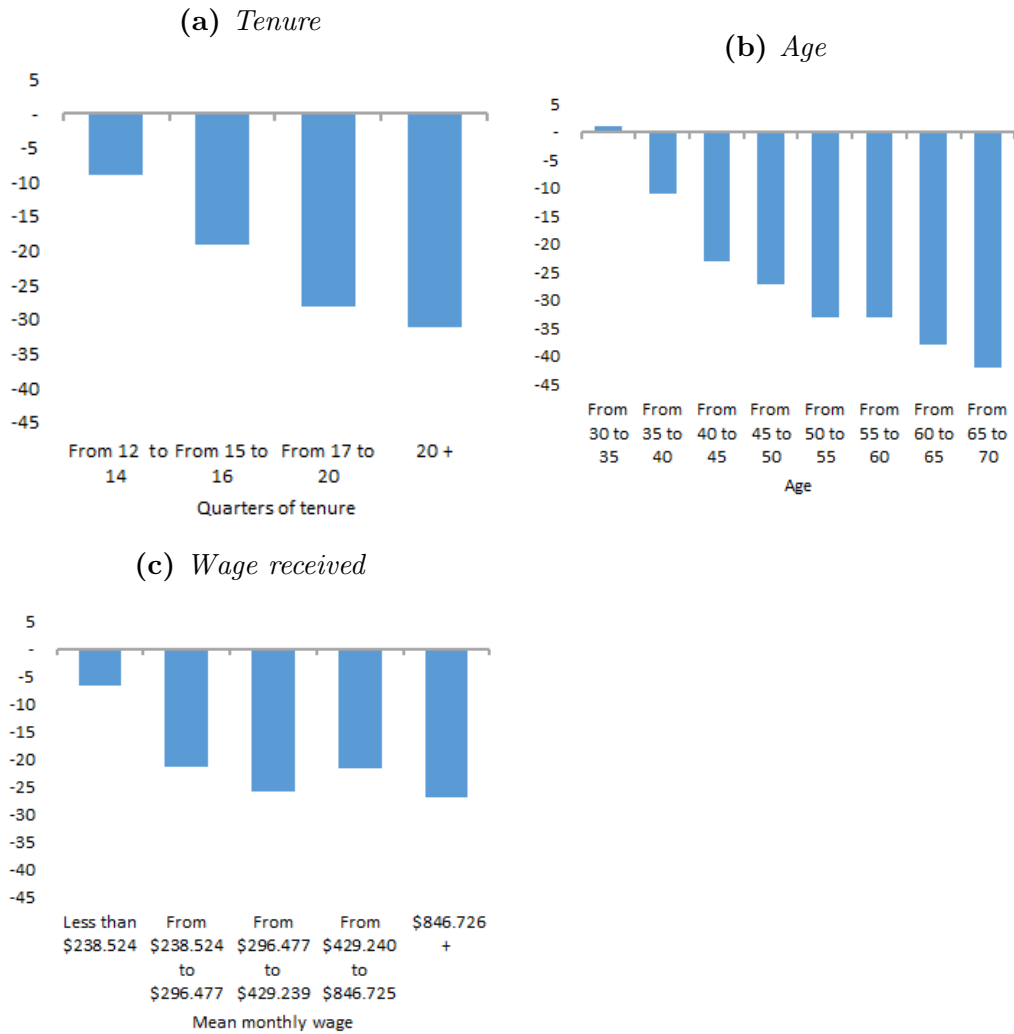
Reinforcing the idea of the job ladder existence, younger displaced workers (from 30 to 35 years old) have gains in wage 5 years after involuntary changing jobs, while as older a worker is displaced, its wage cost will be higher. From a gender perspective, almost no difference on wage loss is observed for female and male displaced workers until 5 years after displacement (23% woman and 25% man).

Finally high pay workers are strongly harm in wage 5 years after being displaced with respect to displaced workers with lower salaries ( quintile 1, 7% wage loss, and quintile 5, 26% wage loss). While low pay workers have almost no wage harm when displaced, high pay workers experiment important losses in wage.

---

<sup>6</sup>Defined as quarters continuously worked at the same firm.

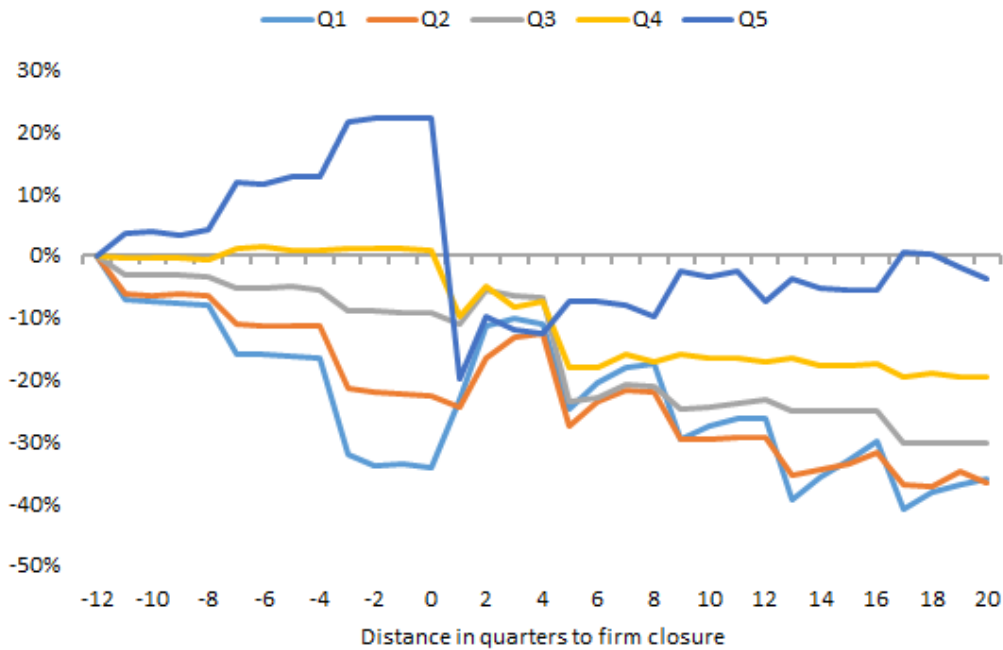
**Figure 9:** Net wage loses (%) 5 years after displacement event by workers characteristics



Other source of heterogeneity is the observed worker job ladder within firms. Presumably, workers who have a faster wage growing path in a given firm are in a good match employer-employee. By sudden closing firm we order workers by its wage growth, rank them in quintiles and run equation 1. Figure 10 reveals that workers with a steeper job ladder (Q5) have better wages than the control group before firm closure, and have a huge wage loss when getting displaced (42%). As probably in addition to be in a good match

employer-employee, they are relatively high productive workers, they manage to recover 20 % of the wage loss five years after displacement event. On the other side of the job ladder (Q1), workers who have little wage growth in a sudden closing firm, benefit in wage when displacement occurs, but 5 years after the event they are even worse as they were before the closure.

**Figure 10:** *Effects by job ladder steepness*



## 4.2 Timing of reemployment

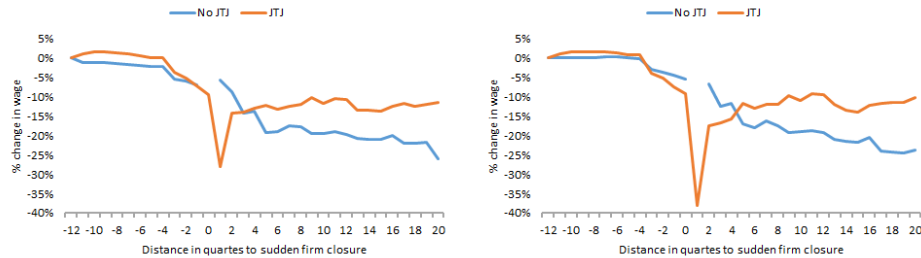
The timing of reemployment will also generate heterogeneity for short and long term effects in wage of displaced workers.

We re estimate the effects for sudden displaced workers that remain at the firm up to its closing quarter, differentiating two types of transitions; job to job transitions (JTJ), where a displaced worker gets a new job the immediate following quarter after displacement and all other transitions (no JTJ). Figure 11 show that JTJ transitions have initially a bigger wage cost than no JTJ

transitions, but are able to recover a bigger share of the initial loss one year after the displacement, and get fewer long run losses with respect to no JTJ transitions. This results remain true for prime workers.

**Figure 11:** *Wage losses by job to job transitions status*

(a) *Net wage impact ALL workers* (b) *Net wage impact PRIME workers*

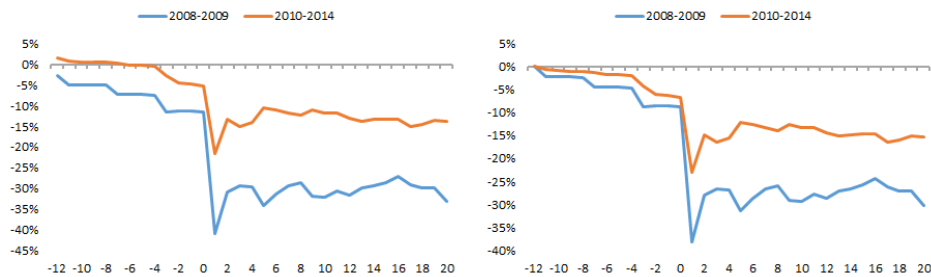


### 4.3 Effects on the cycle

Even-though we only have 12 years in our data set, we do have a two year period of economic recession in Chile (2008-2009). When estimating the effects for sudden displaced workers that went down with the firm closure in crisis years (2008-2009) and those from no crisis years (2010-2014) we observe differences in wage losses. Figure 12 show that being displaced in a crisis year have a deeper (-42% vs -22%) short run effect together with a long lasting wage effect (-30% vs -15%) five years after displacement, than being displaced in a non crisis period.

**Figure 12:** *Wage losses by period of displacement*

(a) *Gross wage impact* (b) *Net wage impact*



## 5 Concluding remarks

Job displacements can be costly for the future prospects of workers. Consistent with the previous literature, we find that displaced workers in firm closure episodes in Chile experience large and persistent wage losses. Losses are significantly larger when looking at sudden firm closures, which we think are a better measure of an unexpected shock because workers are (relatively) unprepared for the displacement event. This means that, in average, this new identification deal with two biases of displacement effects. First, clean for early displaced workers who have almost no wage effects with respect to sinking ship displaced workers. And second, internalize that workers of non sudden closures lost their jobs in a non exogenous feature, so their wage an employment costs are lower than workers involved in a more realistic exogenous event, such as sudden firm closures, that is a closure identification to a natural experiment. The year of separation, firm and worker characteristics, together with the timing of re entering the job market will determine how deep is the effect on wage losses of displaced worker from sudden closures.

Future work should focus on building an explicit analytic framework to rationalize results found, in particular in terms of the difference between expected/unexpected displacement.

## References

- Gary S Becker. Investment in human capital: A theoretical analysis. *Journal of political economy*, 70(5, Part 2):9–49, 1962.
- William J Carrington and Bruce Fallick. Why do earnings fall with job displacement? *Industrial Relations: A Journal of Economy and Society*, 56(4):688–722, 2017.
- Kenneth A Couch and Dana W Placzek. Earnings losses of displaced workers revisited. *American Economic Review*, 100(1):572–89, 2010.
- Marcus Eliason and Donald Storrie. Lasting or latent scars? swedish evidence on the long-term effects of job displacement. *Journal of Labor Economics*, 24(4):831–856, 2006.
- Aaron B Flaaen, Matthew D Shapiro, and Isaac Sorkin. Reconsidering the consequences of worker displacements: Firm versus worker perspective. Technical report, National Bureau of Economic Research, 2017.
- Robert Gibbons and Lawrence F Katz. Layoffs and lemons. *Journal of labor Economics*, 9(4):351–380, 1991.
- Mary Gregory and Robert Jukes. Unemployment and subsequent earnings: Estimating scarring among british men 1984–94. *The Economic Journal*, 111(475):607–625, 2001.
- Louis S Jacobson, Robert J LaLonde, and Daniel G Sullivan. Earnings losses of displaced workers. *The American economic review*, pages 685–709, 1993.
- Pawel Krolikowski. Choosing a control group for displaced workers. *ILR Review*, pages –, 2016.
- Pawel Krolikowski. Job ladders and earnings of displaced workers. *American Economic Journal: Macroeconomics*, 9(2):1–31, 2017.
- Marta Lachowska, Alexandre Mas, and Stephen A Woodbury. Sources of displaced workers long-term earnings losses. Technical report, National Bureau of Economic Research, 2018.

Philip Oreopoulos, Till Von Wachter, and Andrew Heisz. The short-and long-term career effects of graduating in a recession. *American Economic Journal: Applied Economics*, 4(1):1–29, 2012.

Pedro S Raposo, Pedro Portugal, and Anabela Jesus Moreira Carneiro. Decomposing the wage losses of displaced workers: The role of the reallocation of workers into firms and job titles. 2015.

Joanne Salop and Steven Salop. Self-selection and turnover in the labor market. *The Quarterly Journal of Economics*, pages 619–627, 1976.

Guido Schwerdt. Labor turnover before plant closure:leaving the sinking ship vs.captain throwing ballast overboard. *Labour Economics*, 18(1):93–101, 2011.



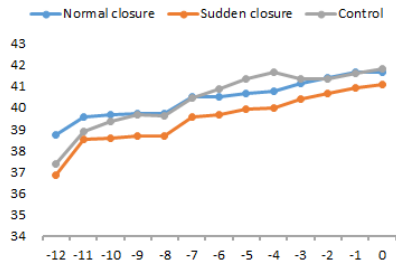
## Appendix A Basics Stats

**Table 1:** *Data base description*  
*Sample characteristics at the closing quarter for treatment groups and*  
*sample mean 2008-2014 for control group*

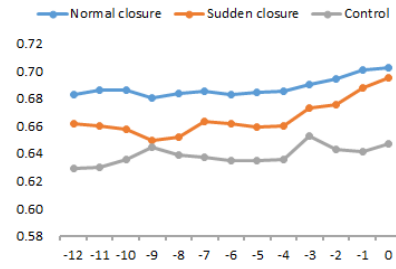
	Treatment (all closures)	Treatment (sudden closures)	Control
<b>Firms</b>			
Observations	4,383	1,511	64,272
Employment (N workers, mean)	50	63	82
Wage paid (real 2017 CLP, mean)	465,522	409,227	571,054
Agriculture (share)	9%	11%	10%
Manufacture (share)	14%	14%	11%
Construcion (share)	14%	18%	10%
Wholesale (share)	20%	18%	20%
Professional activities (share)	8%	7%	9%
<b>Workers</b>			
Observations	108,382	42,291	4,811,766
Males (%)	69%	73%	68%
Age (mean)	40.8	40.1	40.8
Wage received (real 2017 CLP, mean)	638,926	671,227	751,821
Tenure (quarters, mean)	19	18	12

## Appendix B Treatment and control groups characteristics prior to closure

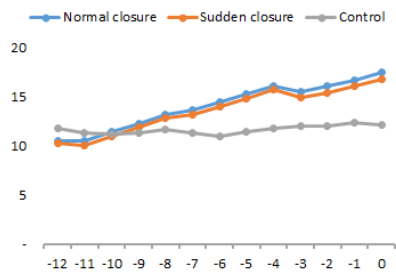
(a) *Age: Years*



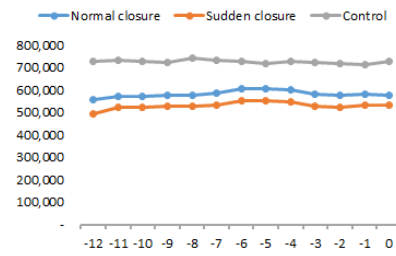
(b) *Gender: Male percentage*



(c) *Tenure: Quarters*

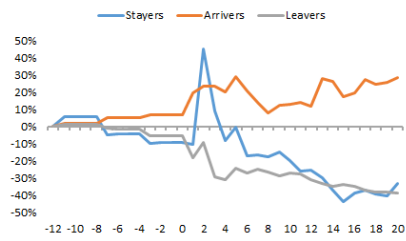


(d) *Wage: real 2017 CLP*

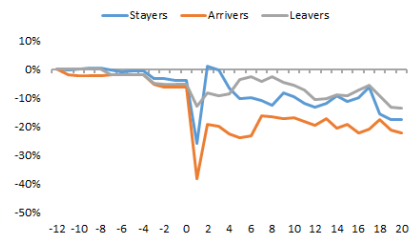


## Appendix C Wage losses by selected industries

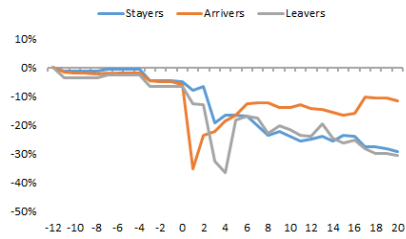
(a) *Mining*



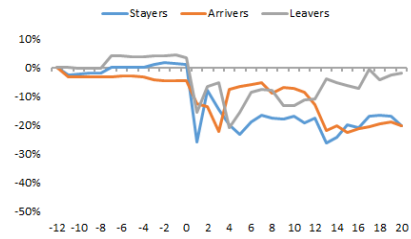
(b) *Agriculture*



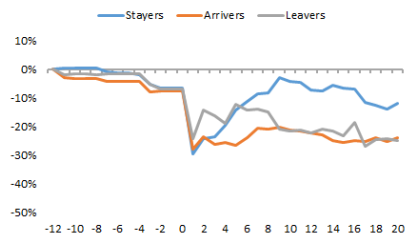
(c) *Manufacture*



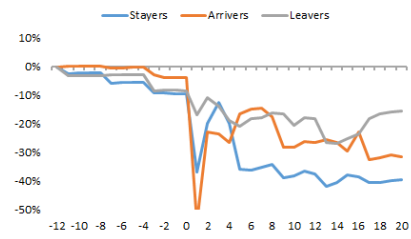
(d) *Construction*



(e) *Wholesale and retail trade*



(f) *Professional activities*



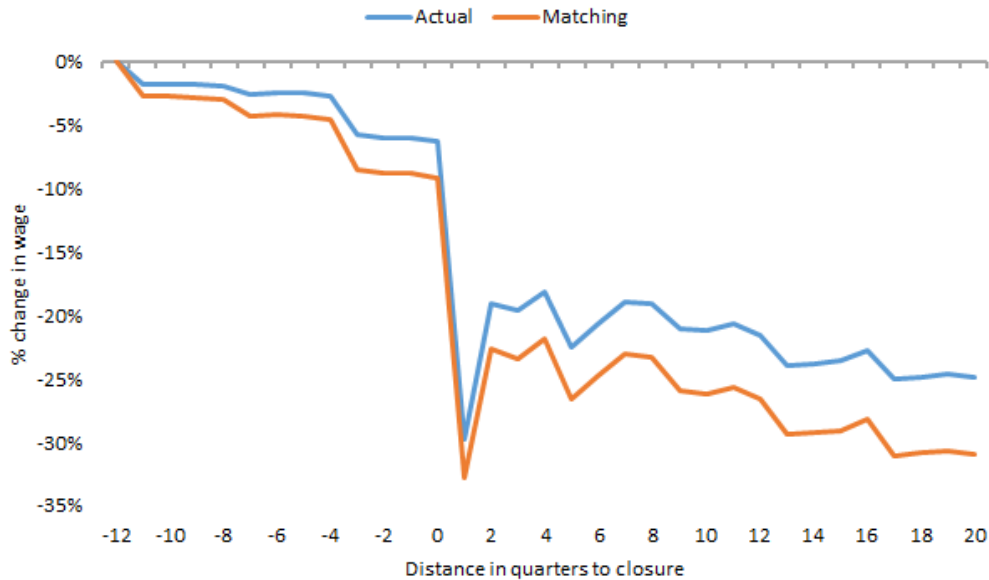
## Appendix D Robustness

Looking forward to study if the main results remain true using different approaches for the control group selection and the definition of suddenness on firm closures, we perform two robustness exercises.

### D.1 Control group selection by one to one exact matching

For each displaced workers from sudden firm closures data base, we perform an exact matching on age, gender, ISCI rev 4, firm size (labor quartiles) and wage paid group ( wagebill quartiles), at the quarter of displacement on control group individuals. We are able to match 99.75% of workers. Results remain fairly robust in wage losses time trend and has little difference on the long term wage loss level, as confirmed by Figure 15.

Figure 15: *Matching one to one*



### D.2 Sudden firm closure definition sales based

While sudden firm closures labor measure can capture the non surprise closure event in terms of workers, one could perform the same exercise using actual

sales. We define sudden firm closures sales based as firms that had no decline on total sales from four quarters before closure until its closing quarter, meaning that the average firm sales did not decline until the quarter the firms closes. This does not mean that the firm has the same level of sales- that there was no sales variation- , but that net sales changes were not negative.

**Figure 16:** *Sudden closure sales based*

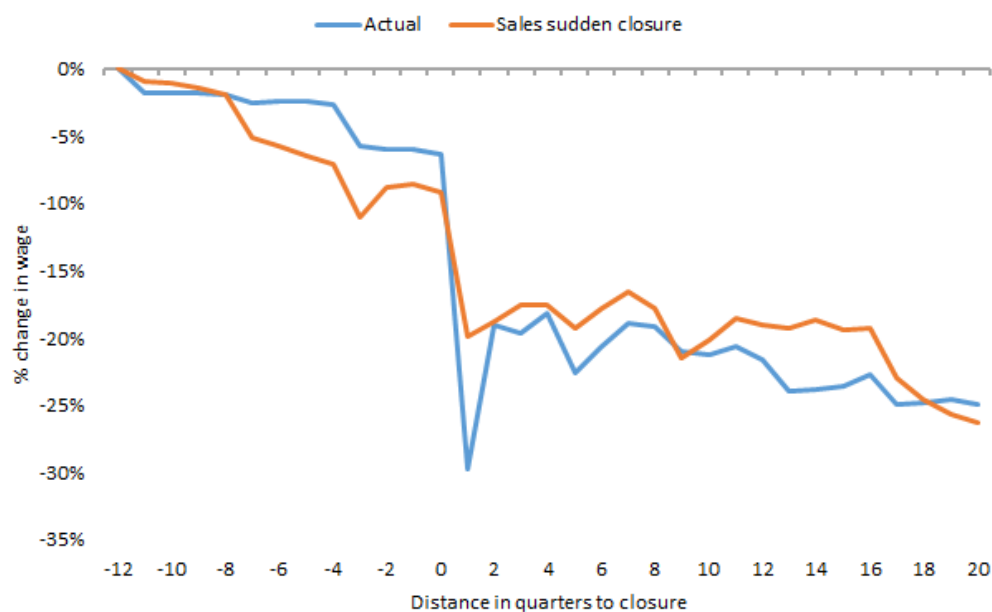


Figure 16 confirm the tendency and suddenness of the closure event remain similar, while quarter to quarter variation is slightly different and the fall in the following quarter of closure is less deep in the sales sudden definition. Even-though the dynamics are no the same the long term effect and the suddenness of the event still true when using sales in stead of employment as sudden variable.