Wage Cyclicality of New and Continuing Jobs

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PRELIMINARY DRAFT

Abstract

This paper provides evidence of the effect of aggregate macro conditions on individual wages, using administrative data for the universe of wage earners and firms in Chile between 2005 and 2016. Our data allows us to distinguish direct job-to-job transitions from hires out of non-employment and, among the latter, first-time entrants and job movers that went through a non-employment spell. Moreover, we identify the precise duration of the non-employment spell. In line with most of the literature, we find that wages of newly created jobs are significantly more sensitive to aggregate unemployment than those of continuing jobs. Crucially, we find that the highest wage procyclicality holds for new hires from short-duration non-employment, which suggests that wage cyclicality not only reflects the cyclicality of match quality for job-to-job transitions, but rather actual wage procyclicality across different types of new hires. Our results are robust to the use of alternative business cycle indicators, and the inclusion/exclusion of different sets of fixed effects, including match-specific fixed effects. We find that wage procyclicality of the different job types is practically symmetric over the business cycle, and is mainly concentrated in higher-income and older workers in large firms.

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1 Introduction

A growing empirical literature has analyzed the cyclical behavior of wages using panel data for the US and several European countries [1], analyzing, in particular, how individual wages respond to aggregate unemployment, usually distinguishing between existing jobs and new hires. Most of the literature finds that individual wages are sensitive to aggregate conditions, in particular for new hires. However, Gertler & Trigari (2009) and Hagedorn & Manovskii (2013) argue that existing empirical studies may be picking up the procyclical nature of match quality of job-to-job transitions. In this line of argument, Gertler *et al.* (2016) use higher frequency data to separate job-to-job transitions from new hires non-employment. They find that the excess sensitivity of new jobs is present in the former transitions, but not in the latter. Under that interpretation, wages themselves are not procyclical, but only the quality of job transitions. Wages in new employment relationships move up when unemployment is low not because wages for a given job increase, but because there are more transitions towards higher quality jobs.

This paper contributes to this literature by using tax records that provide a census of workers and firms in Chile between 2005 and 2016. Our data allows us to identify the employment relationships at a monthly frequency. This gives us a detailed characterization of new hires, which expands the previous literature in a relevant dimension. While most of the literature has lumped all new hires as a unique category that compares to job keepers, we, as in Gertler *et al.* (2016), can separate direct job-to-job transitions from hires out of non-employment. Moreover, we can further separate hires from non-employment into first-time entrants and job movers that went through a non-employment spell. For this latter category, we can identify the precise duration of the non-employment transition. Additionally, detailed information on firms allows us to build proxies that control for job transitions across occupations, and to explore heterogeneity in the cyclicality of wages across workers and firms of different types.

Our results show that this finer characterization of new hires is important for the cyclical behavior of wages. We highlight three main results that are of interest to the recent debate on the relevant margins of wage adjustment throughout the business cycle. First, and in line with previous literature, we find that wages of newly created jobs are significantly more sensitive to aggregate unemployment than that of continuing jobs. However, and contrary to the evidence provided in Gertler *et al.* (2016) for the US, the larger sensitivity of new jobs is a feature of both job-to-job transitions as well as

[1] See, for example, Bils (1985); Barlevy (2001); Carneiro *et al.* (2012); Martins *et al.* (2012); Font *et al.* (2015); Gertler *et al.* (2016); Stüber (2017) and Hahn *et al.* (2018).

(and, in particular) new hires from short-duration non-employment, who display the highest wage procyclicality. These results are robust to the use of alternative measures of cyclical activity, and the inclusion/exclusion of different sets of fixed effects, including, as in Hahn *et al.* (2018), match-specific fixed effects. In this regard, our results support the view that individual wages are procyclical, in particular for new hires out of relatively short periods of non-employment.

Second, wage procyclicality of the different worker types is practically symmetric over different phases of the business cycle (it is, indeed, slightly higher for new hires when unemployment increases), but is increasing for the different types of new hires over the firms' life cycle, being lowest in firms that are born or firms that expand their employment level, and highest if maintain or reduce their level of employment.

Third, when we explore heterogeneity in the cyclicality of wages across workers of different types, we find that, regardless of the type of employment relationship, wage procyclicality increases in the worker's age and income, and that men's wages are significantly more procyclical than women's. Finally, though the wage cyclicality differential between new hires and continuing jobs is present for all types of workers and firm sizes, it is particularly high in larger firms (over 200 workers, where wages of job stayers are least procyclical) and for older and better paid workers.

The outline of the paper is as follows. Section 2 describes the data. Section 3 presents the empirical strategy and the baseline results for wage cyclicality for existing jobs and different types of new hires. Section 4 explores heterogeneity in the cyclical response of wages across firms and workers of different types.

2 Data

We use data from Chiles tax collection agency (*Servicio de Impuestos Internos* -SII), which mandates firms to withhold and pay throughout the year the estimated income tax accruing to each individual employee in its payroll. Firms must fill a yearly statement (Form 1887) which includes each employees annual taxable wage (including base salary, incentive pay, bonuses, employer-provided benefits, and overtime pay) and the detail about the months of the year in which the worker was employed. The data, therefore, covers the entire universe of firms formally registered in the SII, as well as the universe of dependent wage-earners working under a formal contract in those firms. Given that firms and workers are properly identified in the database, we can track each firm's payroll details over time, as well as each worker's labor history across firms and time (with monthly frequency).

Covering the period 2005 to 2016, the original data includes information of about 600,000 firms, 9 million workers, and 36 million employment relationships.

A particular feature of the database is the monthly frequency with which the labor history of workers is recorded. We use this information to characterize in detail each employment relationship in each year. First, we identify continuing workers from new hires. Then, among new hires, we identify those that are entering the labor force from those that were previously employed. Finally, we define different groups of *job changers* based on the length of their nonemployment spell (if any). Our main goal in this paper is to analyze the wage cyclicality of these different groups of workers.

As explained above, our database includes, for each year, information about each formal employment relationship (ER) held by each worker in the economy. While most observations (ER-year) in the original dataset correspond to full-time and permanent jobs, there are also many observations that correspond to part-time or temporary jobs (or both). Our analysis focuses on full-time and (relatively) permanent ERs, which we define in the following way:

1. We only observe in the data whether ERs are part- or full-time for years 2015 and 2016. However, since wages of full-time employees must be equal or above the minimum (legal) wage, we focus our analysis on ERs that satisfy this condition. Specifically, we only consider ERs whose average monthly wage in a given year is at least 90% the minimum wage in that year.

2. An ER in a given year is considered *permanent* if the individual is employed in at least six months out of a 24-month period centered at the beginning of the ER in the year. For example, if the first registered month of work of an active ER in 2011 is March, we require the individual to register at least 6 months of work in the same firm in the period between March 2010 and February 2012, for the *ER in 2011* to be considered a *permanent* one. When that condition is not satisfied, we define de ER-year as *transitory*.

Additionally, and in order to avoid the results being affected by outliers, we drop extremely high wages of each year (i.e. observations above the percentile 99.95 of the wage distribution in each year).

2.1 Categorization of Employment Relationships

Once the *permanent* and *above-the-minimum-wage* ER-year's are identified, we proceed to categorize them based on the individuals' labor status in the 12 months preceding the beginning of the ER in the year.

The first categorization simply distinguishes the ER-year's that, in a given year, continue in an ER from the previous year (we call them *keepers*), from those that begin a new ER (we call them *new hires*). Formally, **an individual employed in a given firm and year is defined as a**:

- ***new hire***, if he was not employed in the same firm in any of the 12 months preceding the beginning of the ER in the year; or is defined as a
- ***keeper***, otherwise.

For example, if the first registered month of work of an active ER in 2011 is March, we require the individual to register at least one month of work in the same firm in the period between March 2010 and February 2011, for the *ER in 2011* to be defined as a *keeper*. Otherwise (i.e. if the individual did not work in the firm in mentioned period), the *ER in 2011* is defined as a *new hire*.

By construction, this categorization applies only to ERs of 2006 (for which the information of 2005 is used) and subsequent years. Table 1 shows that almost 10.3 million observations are defined as *new hires* (22.8% of the baseline sample), while 34.7 million observations are defined as *keepers* (77.2% of the baseline sample). As we can see from the Table, there are substantial differences between these two categories of ERs. *Keepers*, for example, remain, on average, 10.5 months per calendar year in the same ER (compared to 7.2 for *new hires*), and earn, on average, CL$705.4 thousand per month (compared to CL$512.6 for *new hires*).

A second and more detailed categorization of ERs distinguishes, among *new hires*, those that enter the labor force (i.e. workers that register their first employment relationship in the database - we called them *entrants*) from those that were previously employed in a different ER (called *changers*). Formally, **a *new hire* in a given year is** an:

- ***entrant*** if the individual registers no (*permanent* and *above-the-minimum-wage*) ER; or a
- ***changer*** if he registers a previous ER, but in a different firm.

As shown in Table 1, almost 30% of *new hires* are *entrants* (about 3 million observations). Workers in both categories are quite similar in terms of the number of months per year they remain in the same ER (7.1 and 7.2 months, on average, respectively). However, and as expected, they differ substantially in their wages, with *changers* earning, on average, 61% more than *entrants* (CL$576 vs CL$357 thousand, respectively).

A third categorization of ERs distinguishes, among changers, those that make direct job-to-job transitions (we call them *direct changers*) from those that experience a period of nonemployment between jobs. Formally, **a *changer* in a given year is**:

- ***direct changer (0m)*** if the he makes a direct job-to-job transition (with 0 months of nonemployment between the end of the previous ER and the beginning of the current ER); or an
- ***indirect changer (1+m)*** if he goes through a nonemployment spell of at least one month before the beginning of the ER in the year.

As Table 1 shows, about two-third of changers are *indirect changers*, who work on average 7.3 months in the year they begin their new job, and earn CL$501.8 per month. In contrast, *direct changers* work on average 7 months in the year they change jobs, and earn CL$716.1 per month, being thus the best-paid category (almost 43% more than *indirect changers*).

Finally, the fourth and most detailed categorization of ERs identifies four sub-categories among *indirect changers*, depending on the length of the unemployment spell between ERs. Formally, **a *changer* in a given year is**:

- ***changer 1-2m*** if he spends 1 or 2 months nonemployed before beginning the ER in the year (i.e. if there is a period of 1 or 2 months between the first registered month of ER in the year and the latest registered month in any other -*permanent* and *above-the-minimum-wage*- ER);
- ***changer 3-6m*** if he spends between 3 and 6 months nonemployed before beginning the ER in the year;
- ***changer 7-11m*** if he spends between 7 and 11 months nonemployed before beginning the ER in the year; or
- ***changer 12+m*** if he spends 12 or more months nonemployed before beginning the ER in the year.

From Table 1 we can see that *indirect changers* can be divided into two groups of similar size and average wage: those that spend less than one year nonemployed and those that spend one year or more before starting a new ER. In the first group, both the number of months worked in the year and the wage decrease the with length of the nonemployment spell. The fact that *changer 12+m* work more months in the year and earn more than *changer 7-11m* suggests that there may be other reasons (besides unemployment) behind longer-term nonemployment (for example, individuals may leave the labor force voluntarily and re-enter only if the wage is sufficiently high, or might be temporarily out of the labor force while studying).

The final sample used for the baseline regressions includes almost 45 million observations, almost 6 million workers and 337 thousand firms for the period 2006-2016 (3.76 million workers and 167 thousand firms per year, on average).

While the average real monthly wage in the sample is CL$661,4 thousand, the median is CL$407,3 thousand, implying a median-mean ratio of 61.6% for the entire period[2]. As shown in the table, there is wide dispersion of wages across categories, with the mean ranging from CL$357,2 thousand (*entrants*) to CL$716,1 thousand (*direct changers*).

[Table 1 here]

3 Baseline Econometric Specification and Results

In this section we analyze how real wages at the individual level are affected by the business cycle. Though with a different set of controls, our basic econometric specification follows the literature inaugurated by Bils (1985). We estimate the following equation:

$$\ln w_{ift} = \alpha_i + \eta_{it}^{g,a} + \gamma_f + \theta_{ft}^{fsize} + \sum_{s\in S} \vartheta_s \cdot \mathbb{I}_s + \sum_{s\in S} \delta_s \cdot \mathbb{I}_s \cdot t + \sum_{s\in S} \beta_s \cdot \mathbb{I}_s \cdot unemp_t + \zeta_{ift}^{months} + \varepsilon_{ift} \quad (1)$$

Where:

- $\ln w_{ift}$ is the log monthly real wage of individual i, working in firm f in year t (nominal annual wages are divided by the number of months the worker was employed in the year, and deflated with the CPI).

- α_i and $\eta_{it}^{g,a}$ are individual and gender-age fixed effects[3] that account for unobserved heterogeneity correlated with observables at the individual level.

[2]According to US Social Security Administration, the *net compensation* median-average ratio in the US for the same period is about 65.8%. See https://www.ssa.gov/oact/cola/central.html

[3]Information on individuals' gender and age is obtained from the Chilean Electoral Service (Servel). The Chilean Internal Revenue Service (SII) links the databases and modify the original IDs, so that actual individuals or firms cannot be identified.

- γ_f and θ_{ft}^{fsize} are firm and firm-size fixed effects[4] that account for heterogeneity correlated with observables at the firm level.

- s is the type of employment relationship in year t, and S is the specific ER-type categorization used in the regression. Following the categorizations previously made, S can be $S(1) = \{k, n\}$; $S(2) = \{k, e, c\}$; $S(3) = \{k, e, c_{0m}, c_{1+m}\}$; or $S(4) = \{k, e, c_{0m}, c_{1-2m}, c_{3-6m}, c_{7-11m}, c_{12+m}\}$ (where k, n, e and c stand, respectively, for *keeper*, *new hire*, *entrant*, and *changer*).

- $\mathbb{I}_s$ is an indicator variable that takes value 1 for type-s ($s \in S$) ERs in year t, and 0 otherwise.

- ϑ_s is a ER-type ($s \in S$) fixed effect, that accounts for systematic differences in wages across ER types.

- δ_s is type-s' wages annual trend. We allow such trends to be different for the different types of ERs.

- $unemp_t$ is the unemployment rate, which is used to capture business cycle conditions at the aggregate level.

- β_s captures the effect of an increase in 1 percentage point in the unemployment rate on (the log of) a type-s ER's real wage. As such, the coefficient can be interpreted as the semi-elasticity of type-s ER's real wage with respect to the aggregate unemployment rate.

- ε_{ift} is a random error term with standard properties.

Additionally, a fixed effect ζ_{ift}^{months} is included in the baseline specification. This fixed effect is defined by the specific combination of calendar months in which the ER (worker-firm) is active in a given year. Given that ERs are reported as either active or inactive in each of the twelve months of the year, there are, in principle, $2^{12} - 1 = 4,095$ different combinations that can be controlled for. The inclusion of this set of dummies is intended to (at least partially) correct for the effects of:

- Potential measurement error in monthly wages given by the fact that total number of months worked by an individual in a given firm and year is usually rounded up, so that our measure of monthly wage (computed as the ratio between the annual wage received by the individual in a given firm and year, and the number of months worked by the individual in the firm in the year) is a lower bound of the true monthly wage. For example, an individual that works

[4] Based on the firm' number of employees in each year, we define 5 size categories.

for a firm for 11.5 months in a calendar year is reported as having worked for 12 months, and an individual that works for 0.5 months is reported as having worked 1 month. Clearly, this type of measurement error is inversely related to the total number of months in which the ER is reported as active.[5]

- Measurement error given by the fact that individuals' nominal annual wages reported by firms are the sum of nominal monthly wages paid over the year. As inflation is usually positive, deflating all the values of a given year by the same number tends to underestimate (overestimate) real payments made in the first (last) months of the year. For example, if two individuals have a similar reported nominal annual wage in a given year, but one of them only works in January and the other only works in December. Deflating both wages by the same number would ignore the fact that, with positive inflation over the year, the real wage of the first worker is actually higher.

- Differences in monthly wages that simply reflect the effect of seasonal payments, such as extra payments that many workers in Chile receive in the months of September or December.

3.1 Results

Table 2 presents the results of the first set of regressions[6]. Only the estimated coefficients and standard errors of the semi-elasticities of real wages with respect to the unemployment rate (i.e. the $\beta_s' s$) are reported. The regression in column (1) categorizes the workers as *keepers* and *new hires*. The results imply that *new hires*' wages are more procyclical than *keepers*'. The estimation implies that, everything else constant, a 1 percentage point increase in the unemployment rate reduces (relative to the trend) the real wage of *keepers* by 0.99%, and that of *new hires* by 1.72%. The coefficients as well as the difference between them are highly significant.

[Table 2 here]

In regression (2) of Table 2, we split the *new hires* into two groups: *entrants* (those that do not register a previous *permanent* ER) and *changers* (those that register a previous *permanent* ER), and estimate separately their wage-unemployment semi-elasticity[7]. As expected, the coefficient for *keepers* does

[5]Only for years 2015 and 2016 we observe whether individuals work part- or full-time in a given month.

[6]We use the Stata command *reghdfe*, which allows for the inclusion of multiple, high dimensional fixed effects. We opt for excluding singleton observations for which fixed effects cannot be correctly estimated.

[7]As mentioned above, the regression also estimate a set of ER-type dummies and time-trends, which are not reported.

not change. The coefficient for *entrants* is -1.11, and that for *changers* is -2.06. Regression (3) distinguishes between *direct* and *indirect changers*. The estimation shows that, while wages of both types of *changers* are more procyclical than those of *keepers* and *entrants*, *indirect changers* register a wage procyclicality substantially higher than that of *direct changers* (-2.14 and -1.53, respectively). The regression in column (4), which estimates separately the wage cyclicality of *indirect changers* depending on the length of the nonemployment spell, shows that, regardless of the length of such period, wages of all types of *indirect changers* are more procyclical than any other ER category. Wage procyclicality initially increases in the length of the nonemployment period, reaches its maximum for *changers* that go through a period of nonemployment of 3 to 6 months (with a semi-elasticity of -2.51), and then decreases as the period of nonemployment continues.

In line with the findings of the literature, we find that *new hires*' wages are more procyclical than *keepers*'. However, and in contrast with the findings of Gertler *et al.* (2016), we find that this is true for all types of *new hires*, and not only for *new hires from employment* (i.e. *direct changers*), as suggested by these authors, who find that the excess wage cyclicality of *new hires* is given primarily by the cyclicality of job-to-job transitions (i.e. *direct changers*), while the cyclicality of *new hires* from nonemployment (which includes *entrants* and *changers (1+m)*) is not different from that of *keepers*.[8] Our results, instead, not only show that *indirect changers*' wages are significantly more procyclical than *keepers*'s, but they are also more procyclical than those of *direct changers*, a fact that is consistent with Pissarides (2009)'s hypothesis that wages are flexible at the relevant margin.

3.2 Robustness of the Results

The main results from the baseline regressions can be summarized as follows. First, wages at the individual level are procyclical: semi-elasticities of real wages with respect to aggregate unemployment range approximately between -1 and -2.5, depending on the type of ER. Second, there are significant differences in the wage procyclicality of different types of ER: *keepers*, on the one extreme, are the least sensitive to aggregate conditions, with a semi-elasticity of -1. On the other extreme, *indirect changers* are, on average, the most procyclical, with a semi-elasticity of -2.14. Third, there is some heterogeneity in the procyclicality of wages among *indirect changers*, depending on the length of the nonemployment spell: it first increases in the length of the nonemployment spell (up to a spell of 3-6

[8] Differences in the dataset, the particular sample, the econometric specification and the definitions of worker types used Gertler *et al.* (2016) and us could explain, in part, the different results. For example, they use quarterly data (which makes more difficult the precise characterization of job transitions) for men, age 20-60, and do not control for firm heterogeneity in their specification. However, as we show below in Table 3, even when we use a similar specification and sample as in Gertler *et al.* (2016), our results suggest that *changers (3-6m)* have the highest wage procyclicality.

months) to then decrease as the nonemployment spell continues.

Before going deeper in our analysis, it is worth to analyze the robustness of these results. In particular, we question to what extend these results are affected by (1) the sample used or the inclusion/exclusion of a particular set of fixed effects, (2) the use of the unemployment rate as a cyclical indicator and, (3) the existence of informal employment relationships not necessarily captured in our dataset.

3.2.1 Different Samples and Sets of Fixed Effects

In Table 3 we examine how sensitive our estimations are to the sample used or the inclusion of different sets of fixed effects. For comparison, regression (1) replicates the baseline regression from column (3) of Table 2), that includes worker, gender-age, firm, and firm size fixed effects.

Our baseline regression includes the entire universe of workers and firms under a formal contract. We replicate the baseline regression estimating separately the coefficients for the sub-sample of men ages 20 to 60, which is the sample used by Gertler *et al.* (2016).[9]. The estimates are reported in column (2) of Table 3. As the table shows, all the estimates are higher (in absolute value) than in the baseline regression (particularly for *entrants*), but the results are qualitatively unaffected.

Several recent papers that analyze the sensitivity individual wages to aggregate conditions include, among their controls, job title fixed effects[10]. The idea behind the inclusion of such fixed effects is that, not controlling for occupation (or firm-occupation) leads to a procyclical bias (Gertler *et al.* (2016), Stüber (2017)), given that changes in wages could be simply reflecting cyclical job up or downgrading. Given that we do not observe job titles in our data, we infer a series of occupation categories for workers using the distribution of wages within firms. Specifically, we compute our within firm wage-category index as follows:

1. For each firm and year, we compute the average real wage as ratio between the wage bill and the number of months effectively worked by its employees.

2. We demean log wages at the firm-year level. This is, for each ER-year, we compute the difference between the log of the real wage and the log of the average monthly real wage in the firm-year. This difference provides information about how close the wage of each worker in a given year is, in relative terms, to the average wage in the firm.

[9]We use the entire sample (so that the estimation of fixed effects is not affected), but identify the set of men ages 20-60 with a dummy variable, for which we estimate the wage-unemployment semi-elasticities separately.

[10]See, for example, Carneiro *et al.* (2012), Martins *et al.* (2012), Gertler *et al.* (2016) and Stüber (2017).

3. Based on the entire distribution (for all firms and years) of (log) ”firm-year demeaned real wages”, we identify 6 firm-year specific wage categories, namely: (1) below the 10-percentile, (2) between the 10- and 25-percentile, (3) between the 25- and 50-percentile, (4) between the 50- and 75-percentile, (5) between the 75- and 90-percentile, and (6) above the 90-percentile. The idea is that these categories reflect different job titles within a firm-year. Notice that, since the categorization is made based on the entire distribution, the categories that apply to each firm-year are not necessarily equal. For example, firms with little dispersion of wages across workers will probably have only categories 3 and 4, while firms with only very low and very high wages will only have categories 1 and 6. Firms with higher wage heterogeneity will likely have more wage categories.

[Table 3 here]

In regression (3) of Table 3 we re-estimate our baseline regression including our job-position FE and excluding firm and firm-size FE (i.e. we ignore heterogeneity correlated with observables at the firm level), which replicates the set of controls used by Gertler *et al.* (2016) in their baseline regression. The estimated wage-unemployment semi-elasticities are slightly lower (in absolute terms) than in our baseline regression, but qualitatively similar. In regression (4) we include additionally firm and firm-size FE (i.e. we replicate our baseline regression including additionally job-position FE). As we can see, the controlling for job-position reduces significantly (in absolute terms) all the coefficients, which suggests that part (about 40%) of our baseline estimates is explained by cyclical job up or downgrading (as proposed by Gertler *et al.* (2016)). But the results show that there is still large wage procyclicality for all ER types, and that their relative order and magnitudes are unaffected.

Finally, in column (5) of Table 3 we follow Hahn *et al.* (2018) and include match-specific (firm-worker) fixed effects to fully control for match quality up and downgrading over the business cycle. As we can see, estimates are smaller in magnitude than in the baseline regression, and wages of *direct changers* are now as cyclical as those of *keepers*. However, wages of *indirect changers* remain significantly more cyclical than the rest (more than 50%, on average), confirming our results.

3.2.2 Different Cycle Indicators

Our results from Table 2 suggest that real wages are sensitive to changes in the aggregate unemployment rate. Does that imply that wages respond to the business cycle, or we are simply capturing a co-movement that is only present in the labor market? We explore this in Table 4, in which we compare our baseline specification (column (1), in which the unemployment rate is used as a business cycle indicator), with other four similar specifications in which different aggregate cycle indicators are used[11]. As we show below, using different business cycle indicators (both, at the economy-wide and sector level) we find that individual wages are procyclical in general, those of *keepers* are the least procyclical and those of *indirect changers* the most procyclical.

In regression (2) we use GDP growth as business cycle indicator. In line with our results from the baseline regression, the estimates the regression imply that higher GDP growth imply higher growth of individual wages and, in particular, for *changers*. Specifically, an increase by 1 percentage point in the rate of growth of GDP increases (relative to the trend) the real wage of *keepers* and *entrants* by 0.16% and 0.06%, respectively, and that of *direct* and *indirect changers* by 0.50% and 0.49%, respectively (they are not statistically different from each other). Thus, wages of *changers* are three times as procyclical as those of *keepers*, a relative difference that is higher than the one implied by the baseline regression.

The cycle indicator used in regression (3) is the rate of growth of formal employment, computed as the annual rate of growth of the total number of worker-months formally employed in a given year (including part- and full-time workers, as well as temporary and permanent employment relationships). It is important to highlight that, while the unemployment rate (computed as the fraction of the labor force that is unemployed -including formal and informal workers) is measured by the Chilean National Statistical Office (INE) based on survey data, the rate of growth of formal employment used in regression (3) is computed by us with administrative data from the Chilean Internal Revenue Service (SII). The results from regression (3) confirm, once again, that real wages are procyclical, with *indirect changers* leading the ranking. In particular, the estimates imply that an increase by 1 percentage point in the rate of growth of formal employment leads to a 0.06% increase in the real wage of *keepers* and *entrants*, a 0.22% increase in the wage of *direct changers*, and a 0.34% increase in the wage of *indirect changers*. The results from regression (3) are, therefore, *qualitatively* similar to those of regression (1), but *quantitatively* stronger, in the sense that *changers* are between 4 and 6 times as procyclical as *keepers* (in comparison to 2 times in the baseline regression).

The cyclical indicators used in regressions (1) through (3) capture, in different ways, macro aggregate conditions. In regressions (4) and (5) of Table 4, on the other hand, we analyze how individual wages are affected by sector-specific conditions, as opposed to aggregate conditions. The idea is that,

[11]In order to facilitate the comparison, all five regressions in Table 4 have the same specification and are run over the same sample as the baseline regression (column (3) of Table 2), except, of course, for the business cycle indicator used in each case.

contrary to the *aggregate* cyclical indicators used before, *sectoral* cyclical indicators are not affected by aggregate shocks that hit all sectors proportionally, but are affected by sector-specific shocks that may not have aggregate effects. In column (4) we use sectoral formal employment growth (defined in the same way as total formal employment growth, but at the CIIU Rev. 4 section level) as a cycle indicator. The estimates show a pattern similar to that found in the previous regressions: wages respond positively to employment growth at the sectoral level, and in particular those of *changers*, being *Indirect changers* the most procyclical. Interestingly, while wages of *keepers* and *entrants* respond similarly to both total and sectoral employment growth, *changers* respond significantly more to the economy-wide conditions than to the sectoral conditions. This result might be explained by the fact that *changers* can change sectors as they look for better jobs, and therefore are (relatively) more affected by other sector's cyclical conditions.

Given that we do not have a measure of unemployment at the sector level, we construct an indicator that measures the extent to which the level of employment in a given sector is below, in line with, or above what could be considered a "reasonable" level. Specifically, for each sector and year we proceed to: (1) compute the employment share (as a fraction of total employment); (2) estimate a linear trend and the deviations of the employment share from such trend (i.e. the residuals of the regression), in order to (at least partially) account for changes in sectoral employment shares due to structural transformation, and; (3) express the deviations as a percentage of the estimated trend. Regression (5) uses this measure as cyclical indicator. The estimations are, again, qualitatively similar to the baseline regression's: wages are procyclical in general, *keepers* are the least procyclical and *indirect changers* the most procyclical (around 2.5 times as procyclical as *keepers*).

[Table 4 here]

3.2.3 The Effect of Employment Informality

We have verified that our results are robust to the use of different samples, sets of controls, and business cycle indicators. An important question, however, is whether our results are driven -or seriously affected- by informal employment arrangements not covered by our dataset. If, for example, an individual that maintains an informal employment relationship while (formally) nonemployed finds a formal job, we would consider him as a *new hire out of nonemployment* (i.e. either an *entrant* or an *indirect changer*, depending on whether the individual registers a previous formal employment), though we should probably consider him a *direct changer*, given that he had a job at the time his formal employment relationship begins. A high prevalence of such cases would explain, at least partially, the high (in absolute value) wage procyclicality *indirect changers*, which in our data are even more procyclical than *direct changers*.

How are our results affected by informal employment? Given that employment informality is relatively low in Chile (and in particular, for the sectors included in our analysis), we expect its impact on our estimations to be limited. According to the National Employment Survey (ENE, conducted by the Chilean National Statistical Office -INE), 13.3% of total employment covered in our database (which excludes domestic work) in 2016 was under an informal contract.[12] In order to test the effect of employment informality, we use the National Employment Survey to compute a measure of ”exposure to employment informality”, based on the fraction of informal employment arrangements registered for each year, individual type (characterized by gender and age) and firm type (characterized by size and sector), which we use to match our data[13]. We then estimate Equation 1 with a different set of coefficients for each quartile of the ”exposure to informality”-year distribution[14]. Table 5 presents the estimations of the wage elasticities to aggregate unemployment for each group. The table shows, first, that the wage sensitivity to the unemployment rate is, on average, increasing in the exposure to employment informality[15] and, second, that regardless of the exposure to informality, the results are qualitatively similar to those of the baseline regression for each quartile, with *indirect changers* displaying significantly higher wage procyclicality than *direct changers*, *entrants* and *keepers*. This result makes us confident that our main conclusions from the baseline specification are not driven by informal employment arrangements not included in our dataset.

[Table 5 here]

4 Heterogeneous Effects

In this section we explore the extent to which the results from the baseline regressions vary for different groups of individuals and firms. In particular, we analyze how the sensitivity of individual wages to aggregate conditions varies over the business cycle and with differences in gender, income and age (at the individual level), and in size, sector, and growth stage (at the firm level).

[12]According to the survey, informality is highest in the *accommodation and food service activities* (29.3%), followed by *Agriculture, forestry and fishing* (26.8%), *Transportation and storage* (17.7%) and *construction* (19.2%).

[13]Based on observables (year, gender, age, firm size and sector), we impute the ”exposure to employment informality” computed from the National Employment Survey.

[14]In order to keep the sample and fixed effects estimations unaltered, we run only one regression but we estimate separately four sets of wage-unemployment semi-elasticities, depending on the exposure to informality.

[15]Given the way we impute the exposure to informality, this correlation does not imply causality.

The simplest (and most time-saving) way in which heterogeneous effects can be explored is to estimate Equation 1 separately for each subset of observations under consideration (e.g. different categories of individuals or firms). Such methodology, however, suffers from the fact that the estimation of the different sets of fixed effects included in the regressions is seriously affected[16]. In order to avoid this shortcoming, we opt for jointly estimating the coefficients of the different subsets being considered in each case. Specifically, we estimate Equation 2 for each set H for which heterogeneous effects are analyzed (for example, the set H could be $H(\textit{gender}) = \{\textit{Men, Women}\}$, $H(\textit{firm size}) = \{\textit{Micro, Small, Medium, Large}\}$, etc.). The equation now includes the interaction between the ER-type indicator $\mathbb{I}_{\mathrm{s}}$ and the individual (or firm) category indicator $\mathbb{I}_{\mathrm{h}}$ (that takes value 1 if the individual (or firm) belongs to category h, $h \in H$). We, therefore, estimate simultaneously the coefficients for each type of ER (s) and each group of individuals (or firms) being analyzed (h). The equation takes the following form:

$$\begin{aligned} \ln w_{ift} \; = \; & \alpha_i + \eta_{it}^{g,a} + \gamma_f + \theta_{ft}^{fsize} + \sum_{h \in H} \sum_{s \in S} \vartheta_{hs} \cdot \mathbb{I}_{\mathrm{s}} \cdot \mathbb{I}_{\mathrm{h}} + \sum_{h \in H} \sum_{s \in S} \delta_{hs} \cdot \mathbb{I}_{\mathrm{s}} \cdot \mathbb{I}_{\mathrm{h}} \cdot t \\ & + \sum_{h \in H} \sum_{s \in S} \beta_{hs} \cdot \mathbb{I}_{\mathrm{s}} \cdot \mathbb{I}_{\mathrm{h}} \cdot unemp_t + \zeta_{ift}^{months} + \varepsilon_{ift} \end{aligned} \tag{2}$$

4.1 Exploring Asymmetries

We begin by exploring the presence of asymmetries over the business cycles. Specifically, we question if the documented wage cyclicality differs depending on whether the unemployment rate is increasing or decreasing. During the period between 2006 and 2016, the unemployment rate in Chile increased in 2008, 2009, 2014 and 2016, and decreased in 2006-2007, 2010-2013 and 2015. In Table 6 we estimate equation 2 defining set H as $H(\textit{BC Phase}) = \{\textit{Unempl. Increases, Unempl. Decreases}\}$ and S is $S(3) = \{k, e, c_{0m}, c_{1+m}\}$. Surprisingly, the estimated semi-elasticities for the different ER types are very similar in both business cycle phases. Though all estimates are higher (in absolute value) for each ER type when the unemployment rate increases, only the coefficients of *direct changers* in both columns are statistically different from each other at 5% level of significance. The coefficients for *keepers*, *entrants* and *indirect changers* in column (1) are statistically different to those of column (2) only

[16] Given, for example, that the same individuals work for firms of different size or sector, splitting the sample into particular groups of firms would reduce the number of observations per individual and, therefore, make the estimation of the individual fixed effect less precise. Similarly, a firm may belong to different firm-size categories in different years (or go through different growth stages, or employ individuals that differ in gender or age), making the estimation of the firm fixed effect less precise if the sample is split into those categories.

at 10%). Thus, the table suggests that, though slightly higher when the unemployment increases, the documented wage procyclicality for the different ER types is strongly stable over the business cycle.

[Table 6 here]

4.2 Heterogeneous Effects by Wage Level

We now explore differences in wage cyclicality by wage level. That is, we question whether the documented wage procyclicality for the different types of ERs (Table 2) is common to ERs of different wage levels, or is concentrated in a particular type of ER (e.g. "high-wage" jobs). We do so by categorizing wages based on how high or low they are with respect to the wages in the same sector and year. Specifically, we categorize each ER-year observation into three equally-sized groups depending on whether the monthly wage belongs to the first, second, or third tercile of the sector-year wage distribution, and we estimate equation 2, where the set H is defined as $H(\textit{Wage Tercile}) = \{t1, t2, t3\}$ and S is $S(4) = \{k, e, c_{0m}, c_{1-2m}, c_{3-6m}, c_{7-11m}, c_{12+m}\}$.

[Table 7 here]

Table 7 presents the estimates by wage tercile, where $t1$ ($t3$) identifies the ERs with the lowest (highest) wages in each sector and year. The first result that arise from the table is that the pattern of wage procyclicality found in the baseline specification is verified in each group, regardless of the wage level: wages of *keepers* and *entrants* are, on average, the least procyclical[17], and those of *indirect changers* the most procyclical.

Another important result from Table 7 is that, besides the common pattern of wage procyclicality across ER categories by wage level, higher wages display, for each ER category, significantly higher wage procyclicality. Wage-unemployment semi-elasticities in the third tercile are between 2 and 5 times higher (in absolute value) than those in the first tercile.

4.3 Heterogeneous Effects by Gender

In this section we explore differences in wage cyclicality by gender. Table A.1 in Appendix A presents the employment distribution and wages by gender and ER-type. The sample includes 2.23 million women (37.5% of workers), who account for 34.5% of observations (ER-year), and 3.73 million men (62.5% of workers) who account for 65.5% of the observations. Besides the lower participation of

[17]The only exception here are *changers (0m)* of the second tercile, who have a particularly low wage procyclicality (in absolute value).

women in the labor force in Chile, this significant imbalance between men and women in our data (there are almost two men per women) is explained by the fact that our database excludes domestic work which, in Chile, is mainly done by women. Table A.1 shows there are relatively more *keepers* and *entrants* among women (and, consequently, more *changers* among men), which suggests that women are less prone to change jobs than men. Finally, it is worth to mention that men's wages are higher, in particular for *keepers* (22%) and *direct changers* (17.5%). For *entrants* the gap is 6%, and for *indirect changers* 5.3% on average.

Table 8 presents the results of estimating equation 2, where the set H is defined as $H(\textit{gender}) = \{\textit{Men, Women}\}$ and S is $S(4) = \{k, e, c_{0m}, c_{1-2m}, c_{3-6m}, c_{7-11m}, c_{12+m}\}$. The estimations show that the same pattern found for the entire sample is present for both, women and men separately: *keepers'* wages are the least sensitive to macro conditions, followed by *entrants'*, *direct changers'* and *indirect changers'*, respectively. The most elastic are, in both cases, *changers* from a 3/6-months period of nonemployment. But the table also shows that, despite following similar patterns, the estimates for men are significantly higher (in absolute value) for all ER-types. The wage-unemployment semi-elasticity for men is, on average, 0.9 percentage points (in absolute values) higher. The results suggest, therefore, that women are less prone to change jobs and that, when they change, their wage sensitivity is significantly lower than that of men. Finally, it is interesting to point that the coefficient for women *changers (12+m)* is particularly low (in absolute value), implying that the procyclicality of wages of women hired after a relatively long period of nonemployment is significantly lower than that of men in a similar situation, which suggests that the motives behind such long periods os nonemployment might be different for women and men.

[Table 8 here]

4.4 Heterogeneous Effects by Age

In this section we explore differences in wage cyclicality by age. The employment distribution and average wages by for workers of different age and ER-type are presented in Table A.2, in the appendix, from which several interesting facts can be highlighted. First, and as expected, the proportion of *entrants* and *changers* is highest among the youngest workers (up to 30 years) and decreases monotonically with age. The proportion of *keepers*, on the other hand, increases monotonically with age. Second, wages increase with age *on average*, but not for each ER category. The average wage increases by more than 60% between the first and second age groups, and by 9% between the second and third groups. Such average trend is dominated by *keepers*, whose wages increase with age, on average, by the same proportions. *Entrants'* wages, on the other hand, do not vary with age, which suggests that "entry" jobs (understood as the first job an average person finds) do not vary with age. *Changers'* wages, finally, follow an inverted-U shape with age, initially increasing significantly, and then decreasing marginally (except for *changers 12m+*, whose wages increase monotonically with age). Third, in terms of wage levels, while *direct changers* are the best paid among young and middle age workers, *keepers* are the best paid among the oldest workers. This finding is consistent with (and somehow explains) the fact that the proportion of changers declines over time. In each group, wages of *indirect changers* is significantly lower than that of *direct changers*, and decreases with the length of the nonemployment spell (except for *changers (12+m)* among older workers). *Entrants* are, at all ages, the worst paid. Fourth, the dispersion of wages across ER types increases with age, in particular during the first years in the labor force. The ratio between the best and the worst paid category is 1.59 for the youngest, 2.09 for the middle-age, and 2.20 for the oldest workers.

Table 9 presents the results of estimating equation 2, where the set H is defined as $H(\textit{age}) = \{\textit{up to 30y, 31 to 44y, 45y or more}\}$ and S is $S(4) = \{k, e, c_{0m}, c_{1-2m}, c_{3-6m}, c_{7-11m}, c_{12+m}\}$. Once again, the results confirm same pattern of wage sensitivities across ER types for each of the three age groups: *keepers'* wages are the least sensitive to macro conditions (not statistically different to *entrants*), followed by *direct changers'*. The most elastic are, in all cases, *indirect changers*. Interestingly, the dispersion in the wage procyclicality of the different ER-types in each group increases with age: the standard deviation among the coefficients of each group is 0.43, 0.63, and 0.82 for the youngest, the middle-aged, and the oldest workers. This is explained by the fact that the wage sensitivity of *keepers* and *indirect changers* diverges with age: while the wage-unemployment semi-elasticity of *keepers* decreases (in absolute value) from 1.22 (among the youngest) to 0.72 (among the oldest), and that of *indirect changers* increases from 2.18 (*changers 1-6m* among the youngest) to 2.73 (*changers 6-11m* among the oldest).

[Table 9 here]

4.5 Heterogeneous Effects by Firm Size

A novel feature of our database is the fact that it covers the universe of firms formally operating in Chile. The range of firm sizes (measured here as the number of effective worker-year in a given year) in our data goes from 1 all the way to more than 50,000 workers. Despite the fact that the regressions control by firm size through the inclusion of firm-size fixed effects (5 categories), our interest now is to explore whether the documented wage procyclicality varies with the size of the firm. We do so by grouping the firms operating in a given year into four groups, depending on their number of employees[18]: (1) 1-9 employees; (2) 10-49 employees; (3) 50-199 employees; and (4) 200 or more employees. Table A.3 shows the distribution of employment (ER-year) and firms as well as the average wage by firm size, for the years 2006-2015.

Table 10 presents the estimation of equation 2, where the set H is defined as $H(\textit{firm size}) = \{$*Micro (1-9 Emp.), Small (10-49 Emp.), Medium (50-199 Emp.), Large (200+ Emp.)*$\}$ and S is $S(3) = \{k, e, c_{0m}, c_{1+m}\}$.

Some interesting results arise from the table. First, we verify that, independently of the firm size, *indirect changers* are, again, the most procyclical ER-type, followed by *direct changers*, and by the groups of *entrants* and *keepers*.

Second, the Table 10 allows us to see that for the first three categories of firms (i.e. firms with less than 200 employees), *keepers* are not the the least procyclical, but *entrants*. Moreover, for firms with 10-199 employees, *keepers*' wages are as procyclical as *direct changers*'. That is, it is only in the largest firms (200+ employees, that account for more than 57% of employment) where *keepers* are significantly less procyclical than all types of *new hires*. Thus, our results confirm the findings of other studies that, using data from (relatively) large firms (usually 30+ or 50+ workers), find that *new hires* wages are more procyclical. Additionally, our results show that such difference in the wage procyclicality of *new hires* and *keepers* (as documented in column 1 of Table 2) is not clear if we only consider the smaller firms (in particular, firms with less than 200 workers).

Finally, and in line with the previous result, Table 10 shows that the dispersion of the coefficient estimates is particularly high in the largest firms. This is due, as mentioned above, by the fact that the wage sensitivity of *keepers* to macro conditions is particularly low in such firms. We conjecture that this finding could be related to the prevalence of collective union agreements, which in Chile operate almost exclusively at the individual firm-level, and whose coverage increases monotonically with firm size.

[Table 10 here]

[18]We follow the classification of firms made by the Chilean legislation, but any other categorization could have been used. In order to avoid overestimating the number of employees in firms with a large fraction of temporary workers, we define firm size as the total number of months worked by the firms's employees in a given year, divided by 12.

4.6 Heterogeneous Effects by Firm Dynamics

We now explore differences in wage procyclicality over the firm's life cycle. In particular, we analyze how the wage sensitivity to macro conditions vary depending on the evolution of the firm employment level. We do so by categorizing firms depending on whether they are expanding, maintaining, or reducing their employment level. Specifically, we say that a firm is (1) expanding / (2) maintaining / (3) reducing its employment level if the annual rate of growth (i.e. the log-difference) of its employment level[19] is (1) higher than 3.5%/ (2) between -3.5% and 3.5% / (3) lower than -3.5%, respectively. Additionally, we group the firms that are born (for which we have no previous record of employment) in a fourth category. We re-estimate equation 2 interacting the ER-type indicator variables (S is $S(3) = \{k, e, c_{0m}, c_{1+m}\}$) with indicator variables of the firm life cycle stage (H is defined as $H(\textit{firm dynamics}) = \{\textit{Is born, Expands, Maintains, Reduces}\}$).

Table A.4, in the appendix, presents the employment distribution and average wage by ER type for each of these categories, from which we can highlight some interesting facts. First, employment is almost equally distributed between firms that *expand* or *are born*, on the one hand, which jointly account for (51.1% of employment, on average), and firms that *maintain* or *reduce* their employment level, on the other, which concentrate 48.9% of employment. However, these groups differ significantly in terms of job creation: 68.8% of newly created jobs is concentrated in firms that *expand* or *are born*, compared to 31.2% in firms that *maintain* or *reduce* their employment level. Second, in terms of pay, the highest wages (for each ER-type category) are paid in firms that *maintain* their employment level, while firms that *reduce* their personnel pay the lowest ones. Firms that *are born* pay to *new hires*, on average, 3% more than firms that *expand* their employment level. And, third, wage dispersion within ER-type across types of firms (measured with both, the standard deviation and the coefficient of variation) is highest for *keepers* and lowest for *entrants*.

[Table 11 here]

Table 11 presents the estimation of equation 2, where the set H is defined as $H(\textit{Firm Dynamics}) = \{\textit{Is Born, Expands, Maintains, Reduces}\}$ and S is $S(3) = \{k, e, c_{0m}, c_{1+m}\}$, and allows us to extract some interesting conclusions. First, the table shows that the same pattern of wage cyclicalities found in our baseline regressions (Table 2) is verified for the different ER-types over the firms' life cycle: wages of *keepers* and *entrants* are, for each stage, the least procyclical, and wages of *indirect changers* are always

[19] In order to avoid overestimating the number of employees in firms with a large fraction of temporary workers, we define employment level as the total number of months worked by the firms's employees in a given year, divided by 12.

the most procyclical.

Second, we can see from Table 11 that for each of the ER-types among *new hires* (*entrants*, *direct changers* and *indirect changers*), wage procyclicality increases over the firms' life cycle: it is lowest (for each ER-type) for firms that are born, slightly higher for firms that grow, even higher for firms that maintain their level of employment, and highest in firms that shrink[20]. This finding, and the fact that, as Table A.4 shows, employment is relatively more concentrated in higher firm-dynamic stages when the unemployment rate increases, explains why the wage procyclicality of the different *new hires* ER-types is higher when the unemployment increases (as seen in Table 6).

Table 11 shows that *keepers* in firms that *maintain* their employment level are the least procyclical among all types of employment relationships, firms and workers analyzed in the paper. This particular group of workers, whose wages are practically acyclical (with a wage-unemployment semi-elasticity of -0.33), accounts for 23.0% of the ER-year in our sample.

4.7 Heterogeneous Effects by Sector

In this section we analyze how the wage cyclicality for different types of ERs, vary by economic sector. Table A.5, in the appendix, presents the employment (ER-year) distribution and average wages across sectors. From column (13) of the table we can see that there is wide dispersion in terms of size, with most of employment concentrated in Personal Services (27.7%), Commerce, Hotels and Restaurants (17.7%), Industry (12.8%), Construction (11.3%) and Public Administration (10.2%). Interestingly, the Mining sector, that directly contributed with 13% of GDP during the period, only accounted for 1.9% of total employment.

The table also shows wide dispersion in the relative importance of the different ER-types in each sector's employment. Column 2 shows that the fraction of *keepers* in sectoral employment is highest in the Public Administration (89.2%) and lowest in Construction (with 63.6% or, equivalently, 36.4% of *new hires*). The share of *entrants* in sectoral employment (column 4) is highest in Commerce, Hotels and Restaurants (8.0%) and Construction (7.9%), and lowest in Mining (3.1%) and the Public Administration (3.8%). The share of *direct changers* in sectoral employment (column 7) is highest in Construction (8.3%) and Financial and Business Services (8.2%) and lowest in the public sector (1.8%); and the share of *indirect changers* (column 10) is highest in Construction (20.2%) and lowest in Mining (6.8%) and Public Administration (5.2%).

[20] Actually, firms that *reduce* their level of employment have the highest (in absolute terms) wage cyclicality for *keepers* as well. Wage-unemployment semi-elasticities in such firms range from -1.53 (*entrants*) to -3.1 (*indirect changers*).

Columns 5, 8 and 11 of Table A.5 show, additionally, the relative importance of different ER-types among *new hires*. We can see from column 5 that the share of *new hires* that are *entrants* is highest in the Public Administration (35.3%), Commerce, Hotels and Restaurants (33.6%), Agriculture, Forestry, Fishing and Hunting (32.9%), and Personal Services (31.9%), and is lowest in Mining (18.5%) and Financial and Business Services (20.7%). *Direct changers* are relatively more important among *new hires* (column 8) in Mining (40.0%), Financial and Business Services (38.8%) and Utilities (33.4%) that are, precisely, the three sectors with significantly higher wages (column 12). On the other hand, the Public Administration, Agriculture and Construction are the sectors with the lowest rate of *direct changers* among their *new hires*.

Finally, Table A.5 shows that there is wide dispersion in wages across sectors and ER-types, with wages in the Mining sector being, on average, more than 5 times as high as in the Agriculture, Forestry, Fishing and Hunting sector, and about 3 times the average wage in the economy.

Table 12 presents the estimation of equation 2, in which H is the set of sectors. The table shows that, except for Utilities and Public Administration, *indirect changers*' wages are the most procyclical (in some cases, as Financial and Business Services, the coefficient of *indirect changers* is not statistically different to that of other ER-type, but they are always among the highest estimates for each sector). The table also shows wide dispersion in wage cyclicality across sectors and ER-types. The sectors with the most procyclical wages are Construction (with coefficients ranging between -2.27 and 3.36) and Mining (with coefficients ranging between -1.32 and 4.06), while those with the least procyclical wages (other than Public Administration) are Utilities and Personal Services. Interestingly, wages in the Public Administration are *countercyclical*, with coefficients ranging between 0.33 (*direct changers*) and 0.94 (*keepers*).

[Table 12 here]

5 Concluding Remarks

This paper provides evidence of the effect of aggregate macro conditions on individual wages, using administrative data for the universe of wage earners and firms in Chile between 2005 and 2016. Our data allows us to distinguish direct job-to-job transitions from hires out of non-employment and, among the latter, first-time entrants and job movers that went through a non-employment spell. Moreover, we identify the precise duration of the non-employment spell. Our results show that this finer characterization of new hires is important for the cyclical behavior of wages. We highlight three main results that are of interest to the recent debate on the relevant margins of wage adjustment throughout the business cycle.

First, we find that wages of newly created jobs are significantly more sensitive to aggregate unemployment than that of continuing jobs. Interestingly, the larger sensitivity of new jobs is a feature of both job-to-job transitions as well as (and, in particular) new hires from short-duration non-employment. These results are robust to the use of alternative measures of cyclical activity, and the inclusion/exclusion of different sets of fixed effects, including match-specific fixed effects. In this regard, our results support the view that individual wages are procyclical, in particular for new hires out of relatively short periods of non-employment.

Second, we find that the wage procyclicality of the different worker types is practically symmetric over different phases of the business cycle (it is, indeed, slightly higher for new hires when unemployment increases), but is increasing for the different types of new hires over the firms' life cycle, being lowest in firms that are born or firms that expand their employment level, and highest if maintain or reduce their level of employment.

Finally, when we explore heterogeneity in the cyclicality of wages across workers of different types, we find that, regardless of the type of employment relationship, wage procyclicality increases in the worker's age and income, and that men's wages are significantly more procyclical than women's. Finally, though the wage cyclicality differential between new hires and continuing jobs is present for all types of workers and firm sizes, it is particularly high in larger firms (over 200 workers, where wages of job stayers are least procyclical) and for older and better paid workers.

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Table 1: Distribution of Employment Relationship-Year Observations

	Number of Obs. (000's)	Monthly Wage (000' CL$2013) Mean	Median	Months in ER per calendar year Mean	Median
Keepers	34,704.4	705.4	434.0	10.55	12
New Hires	10,264.5	512.6	332.6	7.17	7
Entrants	2,987.7	357.2	259.0	7.07	7
Changers	7,276.7	576.4	374.9	7.21	7
Changers (0m) [Direct Changers]	2,535.1	716.1	442.6	6.96	7
Changers (1+m) [Indirect Changers]	4,741.7	501.8	346.7	7.34	7
Changers (1-2m)	1,055.8	517.6	360.9	6.76	7
Changers (3-6m)	674.7	498.3	348.3	6.59	7
Changers (7-11m)	668.8	477.0	337.4	6.43	6
Changers (12+m)	2,342.3	502.7	342.7	8.08	8
Total (ER-year)	44,968.9	661.4	407.3	9.78	12

	Per year (Avg.)	Sample 2006/16
Number of Workers (000's)	3,761.8	5,967.8
Number of Firms (000's)	167.0	337.0

Table 2: Semi-Elasticity of Real Wages with respect to the Unemployment Rate

Dep. Var.: log of real monthly wage	(1)	(2)	(3)	(4)
Keepers	-0.99	-1.00	-0.99	-1.00
	(0.01)	(0.01)	(0.01)	(0.01)
New Hires	-1.72			
	(0.01)			
Entrants		-1.11	-1.11	-1.11
		(0.02)	(0.02)	(0.02)
Changers		-2.06		
		(0.02)		
Changers (0m) [Direct Changers]			-1.53	-1.53
			(0.03)	(0.03)
Changers (1+m) [Indirect Changers]			-2.14	
			(0.02)	
Changers (1-2m)				-2.31
				(0.04)
Changers (3-6m)				-2.51
				(0.05)
Changers (7-11m)				-2.34
				(0.05)
Changers (12+m)				-1.89
				(0.03)

Notes: Robust standard errors in parenthesis. Fixed effect regressions. All regressions include 44,968,911 observations (5,967,805 active workers and 337,012 active firms in the period 2006-2016), and include worker, gender-age, firm and firm size fixed effects. Each regression includes, additionally, dummies and time trends for each ER type (e.g. keeper, entrants, etc.). In order to correct for measurement error and seasonal effects, the regressions include dummies that control for the exact months worked in the year. The sample includes all formal firms and workers in the period 2006-2016 (information from year 2005 is used to identify the status of ERs in 2006), and ER-year observations in which the worker is active in at least 6 out of 24-month window centered at the first month of the year in which the ER is active. Only ER-year observations with monthly wage above 90% of the minimum wage of the year. In order to prevent estimates being affected by extreme values, we eliminate ER-year observations with monthly wages above the 99.95 percentile of the wage distribution of the year.

Table 3: Semi-Elasticity of Real Wages with respect to the Unemployment Rate

Dep. Var: log of real monthly wage	(1)	(2)	(3)	(4)	(5)
Keepers	-0.99	-1.31	-0.75	-0.59	-0.77
	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)
Entrants	-1.11	-1.69	-0.92	-0.72	-0.69
	(0.02)	(0.03)	(0.02)	(0.01)	(0.02)
Direct Changers (0m)	-1.53	-1.85	-1.48	-0.98	-0.78
	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)
Indirect Changers (1+m)	-2.14	-2.49	-1.94	-1.30	-1.18
	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)
Obs. (000')	44,968.9	26,965.6 (out of 44,968.9)	44,968.9	44,968.9	42,693.7
Men age 20-60 (as in Gertler *et al.*, 2016)		X			
Worker FE	X	X	X	X	
Gender-Age FE	X	X	X	X	X
Firm FE	X	X		X	
Firm Size FE	X	X		X	X
Job Position FE			X	X	
Match (worker-firm) FE					X

Notes: Standard errors in parenthesis. Reg. (1) is the baseline specification (as presented in column (3) of Table 2). Reg. (1) and (2) include the standard sets of FE. Reg. (2) uses the same sample but estimates separately the coefficients for the sub-sample of men age 20-60 (the sample used by Gertler *et al.*, 2016) and the rest. Reg. (3) excludes firm and firm-size FE. Reg. (4) includes the standard FE and, additionally, job position FE. Reg. (5) includes match-specific (firm-worker) FE. The number of observations in Reg. (5) is lower due to the exclusion of additional singleton observations for which FEs cannot be correctly estimated. Each regression includes, additionally, dummies and time trends for each ER type (e.g. keeper, entrants, etc.), and fixed effects that control for the exact months worked in the year.

Table 4: Semi-Elasticity of Real Wages with respect to Different Cycle Indicators

Cycle Indicator:	Unempl. Rate, % (1)	GDP Growth, % (2)	Total Empl. Growth, % (3)	Sect. Empl. Growth, % (4)	Sect. Empl. Dev. Trend, % (5)
Keepers	-0.99	0.16	0.06	0.06	0.06
	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Entrants	-1.11	0.06	0.06	0.06	0.06
	(0.02)	(0.01)	(0.01)	(0.00)	(0.00)
Changers (0m)	-1.53	0.50	0.22	0.17	0.10
	(0.03)	(0.01)	(0.01)	(0.00)	(0.01)
Changers (1+m)	-2.14	0.49	0.34	0.18	0.15
	(0.02)	(0.01)	(0.00)	(0.00)	(0.00)

Notes: Standard errors in parenthesis. Fixed effect regressions. Regressions (2)-(5) include the same sample (44,968,911 observations) and specification as regression (1) (the baseline specification as presented in column (3) of Table 2), except for the business cycle indicator, which different in each regression.

Table 5: Semi-Elasticity of Real Wages with respect to Aggregate Unemployment

	Exposure to Employment Informality (quartiles)			
Dep. Var: log of real monthly wage	q1	q2	q3	q4
Keeper	-0.90	-0.64	-1.19	-1.28
	(0.01)	(0.01)	(0.01)	(0.01)
Entrant	-1.56	-0.80	-1.01	-1.15
	(0.07)	(0.06)	(0.04)	(0.03)
Direct Changer (0m)	-1.32	-1.43	-1.62	-1.73
	(0.06)	(0.06)	(0.05)	(0.05)
Indirect Changer (1+m)	-1.97	-1.91	-2.41	-2.15
	(0.04)	(0.04)	(0.04)	(0.03)

Notes: Robust standard errors in parenthesis. Fixed effect regression. The regression includes 44,968,911 observations (5,967,805 active workers and 337,012 active firms in the period 2006-2016) with a similar sample and specification as the baseline regression (column (3) of Table 2). Exposure to employment informality is estimated from National Employment Survey (ENE, conducted by the National Statistical Office, INE), and imputed at the ER-year level based on the year, the individual's gender and age, and the firm's size and economic sector (4-digit, CIIU Rev. 4). The interactions ER types-unemployment are interacted with quartile-specific dummy variables.

Table 6: Semi-Elasticity of Real Wages with respect to Aggregate Unemployment

Dep. Var: *log of real monthly wage*	Unemployment Increases (1)	Unemployment Decreases (2)
Keepers	-0.95	-0.93
	(0.01)	(0.01)
Entrants	-0.89	-0.79
	(0.02)	(0.03)
Direct Changers (0m)	-1.10	-0.88
	(0.03)	(0.03)
Indirect Changers (1+m)	-1.98	-1.90
	(0.02)	(0.02)

Notes: Robust standard errors in parenthesis. Fixed effect regression. The regression includes 44,968,911 observations (5,967,805 active workers and 337,012 active firms in the period 2006-2016) with similar sample and specification as the baseline regression (column (3) of Table 2). The interactions ER types-unemployment are interacted with business cycle phase-specific dummy variables. The unemployment rate increased in 2008, 2009, 2014 and 2016, and decreased in 2006-2007, 2010-2013 and 2015.

Table 7: Heterogeneous Effects: Wage Procyclicality & Worker's Income

	Wage Tercile (within sector-year)		
Dep. Var: *log of real monthly wage*	t1	t2	t3
Keepers	-0.40	-0.81	-0.93
	(0.01)	(0.01)	(0.01)
Entrants	-0.37	-0.79	-1.06
	(0.02)	(0.02)	(0.04)
Changers (0m)	-0.50	-0.43	-1.14
	(0.03)	(0.03)	(0.04)
Changers (1-2m)	-0.50	-0.94	-1.64
	(0.04)	(0.04)	(0.06)
Changers (3-6m)	-0.81	-1.81	-2.46
	(0.05)	(0.05)	(0.08)
Changers (7-11m)	-0.78	-2.05	-2.45
	(0.05)	(0.05)	(0.08)
Changers (12+m)	-0.45	-1.60	-2.34
	(0.03)	(0.03)	(0.04)

Notes: Robust standard errors in parenthesis. Fixed effect regression. Only the estimated semi-elasticities of real wages with respect to aggregate unemployment are reported. The regression includes 44,968,911 observations (5,967,805 active workers and 337,012 active firms in the period 2006-2016) with similar sample and specification as the baseline regression (column (4) of Table 2). The interactions ER types-unemployment are interacted with wage tercile-specific dummy variables.

Table 8: Heterogeneous Effects: Wage Procyclicality & Gender

Dep. Var: *log of real monthly wage*	Women	Men
Keepers	-0.48	-1.27
	(0.01)	(0.01)
Entrants	-0.50	-1.55
	(0.03)	(0.03)
Changers (0m)	-0.79	-1.85
	(0.05)	(0.03)
Changers (1-2m)	-1.55	-2.55
	(0.08)	(0.04)
Changers (3-6m)	-1.88	-2.69
	(0.11)	(0.05)
Changers (7-11m)	-1.50	-2.59
	(0.11)	(0.05)
Changers (12+m)	-0.79	-2.32
	(0.05)	(0.03)

Notes: Robust standard errors in parenthesis. Fixed effect regression. Only the estimated semi-elasticities of real wages with respect to aggregate unemployment are reported. The regression includes 44,968,911 observations (5,967,805 active workers and 337,012 active firms in the period 2006-2016) with similar sample and specification as the baseline regression (column (4) of Table 2). The interactions ER types-unemployment are interacted with gender-specific dummy variables.

Table 9: Heterogeneous Effects: Wage Procyclicality & Worker Age

Dep. Var: *log of real monthly wage*	Worker's Age (years)		
	Up to 30	31 to 44	45 or more
Keepers	-1.22	-1.12	-0.72
	(0.01)	(0.01)	(0.01)
Entrants	-1.22	-1.10	-0.83
	(0.03)	(0.04)	(0.05)
Changers (0m)	-1.40	-1.60	-1.60
	(0.05)	(0.04)	(0.06)
Changers (1-2m)	-2.16	-2.37	-2.30
	(0.06)	(0.06)	(0.08)
Changers (3-6m)	-2.18	-2.66	-2.67
	(0.08)	(0.08)	(0.10)
Changers (7-11m)	-2.05	-2.36	-2.73
	(0.08)	(0.08)	(0.10)
Changers (12+m)	-1.93	-2.08	-1.70
	(0.05)	(0.04)	(0.05)
SD within age group	0.44	0.63	0.82

Notes: Robust standard errors in parenthesis. Fixed effect regression. Only the estimated semi-elasticities of real wages with respect to aggregate unemployment are reported. The regression includes 44,968,911 observations (5,967,805 active workers and 337,012 active firms in the period 2006-2016) with similar sample and specification as the baseline regression (column (4) of Table 2). The interactions ER types-unemployment are interacted with age group-specific dummy variables. The standard deviation is computed among the different ER-type coefficients within each age group.

Table 10: Heterogeneous Effects: Wage Procyclicality & Firm Size

Dep. Var: *log of real monthly wage*	1-9 Emp.	10-49 Emp.	50-199 Emp.	200+ Emp.
Keepers	-1.18	-1.61	-1.43	-0.65
	(0.02)	(0.01)	(0.01)	(0.01)
Entrants	-1.04	-1.29	-1.28	-0.99
	(0.06)	(0.05)	(0.05)	(0.03)
Direct Changers (0m)	-1.75	-1.64	-1.49	-1.49
	(0.09)	(0.06)	(0.06)	(0.04)
Indirect Changers (1+m)	-1.83	-2.24	-2.25	-2.10
	(0.06)	(0.04)	(0.04)	(0.03)
SD within firm-size group	0.40	0.40	0.43	0.63

Notes: Robust standard errors in parenthesis. Fixed effect regression. Only the estimated semi-elasticities of real wages with respect to aggregate unemployment are reported. The regression includes 44,968,911 observations (5,967,805 active workers and 337,012 active firms in the period 2006-2016) with similar sample and specification as the baseline regression (column (3) of Table 2). The interactions ER types-unemployment are interacted with firm size-specific dummy variables.

Table 11: Heterogeneous Effects: Wage Procyclicality & Firm Dynamics

	Firm Dynamics (Employment level)			
	Is born	Expands	Maintains	Reduces
Dep. Var: *log of real monthly wage*	(1)	(2)	(3)	(4)
Keepers	-	-1.06	-0.33	-1.64
		(0.01)	(0.01)	(0.01)
Entrants	-0.85	-0.85	-1.32	-1.53
	(0.08)	(0.03)	(0.06)	(0.05)
Direct Changers (0m)	-1.19	-1.49	-1.75	-1.82
	(0.08)	(0.04)	(0.09)	(0.06)
Indirect Changers (1+m)	-1.71	-1.77	-2.26	-3.10
	(0.07)	(0.02)	(0.05)	(0.04)

Notes: Robust standard errors in parenthesis. Fixed effect regression. Only the estimated semi-elasticities of real wages with respect to aggregate unemployment are reported. The regression includes 44,968,911 observations (5,967,805 active workers and 337,012 active firms in the period 2006-2016) with similar sample and specification as the baseline regression (column (3) of Table 2). The interactions ER types-unemployment are interacted with firm dynamics-specific dummy variables.

Table 12: Heterogeneous Effects: Wage Procyclicality by Economic Sector

Dep. Var: *log of real monthly wage*	Keepers	Entrants	Changers (0m)	Changers (1+m)	Empl. Share
Agriculture, forestry, fishing and hunting	-1.11	-0.79	-1.82	-1.66	4.4%
	(0.02)	(0.09)	(0.12)	(0.08)	
Mining	-2.38	-4.82	-1.32	-4.06	1.9%
	(0.05)	(0.37)	(0.26)	(0.22)	
Industry	-1.53	-1.40	-1.21	-2.22	12.8%
	(0.01)	(0.06)	(0.08)	(0.05)	
Utilities	-0.56	-2.52	-0.09	-0.44	0.8%
	(0.05)	(0.31)	(0.29)	(0.22)	
Construction	-2.34	-2.27	-3.05	-3.36	11.3%
	(0.02)	(0.06)	(0.06)	(0.04)	
Commerce, Hotels & Restaurants	-1.48	-0.97	-1.44	-1.69	17.7%
	(0.01)	(0.05)	(0.06)	(0.04)	
Transportation and communication	-1.35	-1.92	-1.83	-2.72	8.0%
	(0.02)	(0.08)	(0.08)	(0.06)	
Financial and business services	-1.54	-2.36	-1.02	-2.27	4.3%
	(0.03)	(0.17)	(0.14)	(0.13)	
Real State	-1.44	-0.90	-1.83	-1.77	0.7%
	(0.07)	(0.27)	(0.31)	(0.24)	
Public Administration	0.94	0.77	0.42	0.33	10.2%
	(0.01)	(0.08)	(0.17)	(0.08)	
Personal Services	-0.58	-0.69	-1.04	-1.70	27.7%
	(0.01)	(0.04)	(0.05)	(0.04)	
Other/Unidentified	1.23	0.78	-3.48	-5.63	0.3%
	(0.11)	(0.43)	(0.87)	(0.41)	

Notes: Robust standard errors in parenthesis. Fixed effect regression. Only the estimated semi-elasticities of real wages with respect to aggregate unemployment are reported. The regression includes 44,968,911 observations (5,967,805 active workers and 337,012 active firms in the period 2006-2016) with similar sample and specification as the baseline regression (column (3) of Table 2). The interactions ER types-unemployment are interacted with industry-specific dummy variables.

A Appendix

Table A.1: Employment Distribution & Wages, by Gender & ER Type

	Women			Men		
	Employment		Avg. Wage	Employment		Avg. Wage
	N ('000)	Share (%)	('000 CL$2013)	N ('000)	Share (%)	('000 CL$2013)
Keepers	12,257.0	79.1	618.7	22,447.4	76.2	752.7
Entrants	1,245.8	8.0	344.7	1,742.0	5.9	366.1
Changers (0m)	746.1	4.8	637.6	1,789.0	6.1	748.9
Changers (1-2m)	245.6	1.6	488.4	810.2	2.7	526.4
Changers (3-6m)	157.8	1.0	466.1	516.9	1.8	508.2
Changers (7-11m)	156.9	1.0	454.5	512.0	1.7	483.9
Changers (12+m)	687.2	4.4	492.5	1,655.1	5.6	506.9
Total ER-year	15,496.3	100.0	586.7	29,472.7	100.0	700.7
Total Workers	2,235.5			3,732.3		

Table A.2: Employment Distribution & Wages, by Age & ER Type

	Up to 30 years			31 to 44 years			45 years or more		
	N	(%)	Wage	N	(%)	Wage	N	(%)	Wage
Keepers	8,352.3	67.1	466.9	13,965.9	78.8	748.0	12,386.3	83.7	818.1
Entrants	1,795.6	14.4	341.7	730.6	4.1	385.5	461.5	3.1	372.5
Changers (0m)	816.8	6.6	543.3	1,092.0	6.2	807.6	626.2	4.2	781.9
Changers (1-2m)	369.8	3.0	414.5	431.3	2.4	574.0	254.8	1.7	571.6
Changers (3-6m)	238.4	1.9	400.6	276.1	1.6	553.1	160.3	1.1	549.4
Changers (7-11m)	235.8	1.9	388.2	273.4	1.5	525.2	159.6	1.1	525.5
Changers (12+m)	636.8	5.1	387.5	954.6	5.4	532.3	750.8	5.1	562.8
Total ER-year	12,445.5	100.0	445.5	17,723.8	100.0	714.4	14,799.6	100.0	779.4
Total Workers	2,058.2			2,115.5			1,794.1		

Notes: N is expressed in thousands. *Wage* is the average monthly wage ('000 CL$2013).

Table A.3: Firms & Employment Distribution by Firm Size

Firm Size	Total Sample, 2006-2016				Average, 2006-2016				
	Empl. (ER-year)		Firms		Workers		Wage	Firms	
	N ('000)	Share	N	Share	N ('000)	Share	'000 $2013	N	Share
1-9 Emp.	3,794.1	8.4	253,444	75.2	317.3	8.4	462.8	118,472	70.9
10-49 Emp.	7,265.4	16.2	66,209	19.6	604.8	16.1	523.1	37,059	22.2
50-199 Emp.	8,159.1	18.1	12,996	3.9	673.6	17.9	614.7	8,375	5.0
200+ Emp.	25,750.4	57.3	4,363	1.3	2166.1	57.6	744.5	3,137	1.9
Total	44,968.9	100.0	337,012	100.0	3,761.8	100.0	661.4	167,042	100.0

Notes: In order to compute the total number of firms in the sample (column 3), firms are categorized according to their size in the last year in which they are included in the database. *Shares* are expressed in percentage points.

Table A.4: Employment Distribution & Avg. Wage, by Firm Dynamics

	Firm Dynamics (Evolution of Employment Level)							
	Is born		Expands		Maintains		Reduces	
	N	(%)	N	(%)	N	(%)	N	(%)
Firms per year (avg.)	15.9	9.5	65.7	39.4	35.4	21.2	50.0	30.0
Workers per year (avg.)	65.7	1.7	1,841.7	49.0	1,032.3	27.4	822.1	21.9
Total ER-year	895.9	2.0	22,077.8	49.1	11,998.9	26.7	9,996.3	22.2
ER-year Observations								
	(%)	Wage	(%)	Wage	(%)	Wage	(%)	Wage
Keepers	-	-	72.1	693.2	86.2	794.9	84.5	618.8
Entrants	31.2	383.9	8.2	350.6	4.1	388.4	4.1	330.8
Changers (0m)	31.8	682.9	7.0	711.6	3.0	800.7	3.5	675.9
Changers (1+m)	37.0	495.1	12.7	489.9	6.7	573.3	7.9	473.8
Total	100.0	520.1	100.0	640.4	100.0	763.7	100.0	597.6
Share of ER-year observations, by firm dynamics and business cycle phase (%)								
	Is born		Expands		Maintains		Reduces	
Unemp. Decreases	2.2		51.2		26.5		20.1	
Unemp. Increases	1.6		45.5		27.0		25.9	

Notes: Wage is the average wage ('000 CL$2013); *N* is expressed in thousands. The sample includes 44,968,911 ER-year observations (5,967,805 active workers and 337,012 active firms in the period 2006-2016). We say that a firm (1) expands/ (2) maintains/ (3) reduces if the annual rate of growth (i.e. the log differential) of its employment level is (1) higher than 3.5%/ (2) between -3.5% and 3.5%/ (3) lower than -3.5%, respectively.

Table A.5: Employment Distribution & Avg. Wages, by Economic Sector

	Sector	Keepers		Entrants			Direct Changers			Ind. Changers			Total	
		Wage	% Sect.	Wage	% Sect.	% NH	Wage	% Sect.	% NH	Wage	% Sect.	% NH	Wage	% Empl.
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
1	Agriculture, forestry, fishing and hunting	378	80.1	265	6.5	32.9	428	4.2	21.2	343	9.1	45.9	369	4.4
2	Mining	2,111	83.5	605	3.1	18.5	1,640	6.6	40.0	951	6.8	41.5	1,955	1.9
3	Industry	630	79.7	315	6.0	29.6	662	4.8	23.5	448	9.5	46.9	595	12.8
4	Utilities	1,093	80.1	519	4.7	23.8	1,228	6.7	33.4	687	8.5	42.8	1,040	0.8
5	Construction	565	63.6	333	7.9	21.7	599	8.3	22.8	472	20.2	55.4	531	11.3
6	Commerce, Hotels & Restaurants	582	76.1	300	8.0	33.6	631	5.6	23.4	433	10.3	43.0	547	17.7
7	Transportation and communication	724	76.6	381	6.1	26.2	723	6.3	26.7	516	11.0	47.0	680	8.0
8	Financial and business services	1,257	78.8	522	4.4	20.7	1,171	8.2	38.8	803	8.6	40.5	1,179	4.3
9	Real State	701	74.5	380	7.4	29.1	820	7.0	27.5	532	11.1	43.5	667	0.7
10	Public Administration	822	89.2	505	3.8	35.3	871	1.8	16.8	722	5.2	47.9	806	10.2
11	Personal Services	666	76.9	377	7.4	31.9	680	5.8	25.3	505	9.9	42.7	629	27.7
12	Other/Unidentified	724	65.4	428	5.2	15.1	788	12.4	35.8	584	17.0	49.0	692	0.3
	Total	705	77.2	357	6.6	29.1	716	5.6	24.7	502	10.5	46.2	661	100.0

Notes: Wage is the average wage ('000 CL$2013); % *Sect.* is the share of sectoral employment of the ER-type (i.e. *keepers*, *entrants*, etc.), expressed in percentage points; % *NH* is the share of new hires of the sector that are of a particular ER-type (i.e. *entrants*, *direct changers*, *indirect changers*), expressed in percentage points; % *Empl.* is the share of total employment in the sample (ER-year) in sector. The sample includes 44,968,911 ER-year observations (5,967,805 active workers and 337,012 active firms in the period 2006-2016).