



The effects of equal pay laws on firm pay premiums: Evidence from Chile[☆]

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ABSTRACT

This paper analyzes Chile's 2009 Equal Pay for Equal Work Law (EPL) and its effects on firm pay premiums. The law affected firms with 10 or more workers and specified penalties by firm size, including a disclosure requirement for firms with 200 or more workers. We use matched employer-employee data to estimate worker-firm fixed-effects models, decomposing the firm's contribution to the gender pay gap into bargaining power and sorting channels. The EPL reduces the firm premium gender gap by 6.1%, driven by the bargaining power channel. The effects are larger in firms exposed to higher penalties and disclosure requirements.

1. Introduction

Many countries have witnessed stagnation in the narrowing of the gender pay gap in recent decades, contrasting with the rapid advances in the educational attainment and labor market experience of women (Albrecht et al., 2003; Bailey, 2006; Blau and Kahn, 2017; Bruns, 2019; Goldin, 2014). The recent literature has attempted to reconcile this conflicting evidence by studying the role of firm-specific premiums in the gender wage gap (Card et al., 2016) and its evolution over time (Brunns, 2019; Casarico and Lattanzio, 2019). Gender differentials in firm-specific premiums explain a sizable proportion of the gender pay gap and play a central role in explaining recent trends in wage inequality (Abowd et al., 1999; Barth et al., 2021; Card et al., 2018). Indeed, Bruns (2019) provided strong evidence for the role of firm pay premiums in the marked slowdown in gender convergence in Germany since the 1990s. These effects could be attributed to “changes in employers’ pay policies that have been detrimental to women.”

While the literature on firm-specific pay premiums provides novel insights into the evolution (or stagnation) of the gender pay gap, little

is known about how public policy can affect these premiums. In this paper, we analyze the effect of Chile's 2009 Equal Pay for Equal Work Law (EPL) on firm-specific wage premiums. We first assess firms’ contribution to the gender pay gap using matched employer-employee data from Chile for the 2005–2013 period. We estimate a wage model with two high-dimensional fixed effects following Abowd et al. (1999) (AKM, hereafter). This approach permits us to compute the firm-specific pay premium for males and females in connected firms (by workers’ mobility) and decompose them into sorting effects, i.e., the effect due to female workers sorting into low-paying firms, and bargaining power effects, i.e., the effect due to workers’ ability to extract firm rents when negotiating, following Card et al. (2016) (CCK, hereafter).¹

We then use CCK's framework to assess the effect of Chile's 2009 EPL on the contribution of firms to the gender wage gap (firm-specific gender pay gap). The law established equal remuneration for work of equal value in firms with 10 or more workers and monetary penalties that increase with firm size, as measured by the number of permanent workers. It also mandates firms with 200 or more workers to disclose information about their wage-setting and promotional procedures by

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¹ Rigdon (2012) and Bowles et al. (2005) who conducted laboratory experiments to observe women's bargaining power in wage determination. They found that women ask for a lower starting salary, meaning that they earn less than men. Additionally, women are less likely to initiate negotiations and are less effective at negotiating than men. The literature suggests that this phenomenon might occur because women might feel less deserving of a monetary prize or because they expect a backlash if they negotiate for their rights. See Azmat and Petrongolo (2014) for a review of the experimental evidence from gender and labor markets.

providing a registry of all positions or functions in the company according to their essential technical skills (disclosure requirement). Thus, we estimate AKM models for male and female workers before and after the law, allowing us to implement a difference-in-differences (DID) analysis to assess whether the law affected the firm-specific gender pay gap (captured by gender differences in firm fixed-effects). By implementing a simple reweighting scheme, we show that our DID results correspond to the effect of the EPL on the bargaining power effect. We also perform comparisons of sorting and bargaining power before and after the law using firms with fewer than 10 workers as a benchmark. Finally, we assess whether the law affected sorting patterns following an approach similar to that of [Casarico and Lattanzio \(2019\)](#). In particular, we estimate the effect of the law on the probability of moving to a firm in a higher/lower quartile of the pay premium distribution. We do so using a DID approach, using females as the treated group and males as the untreated group, before and after the law.

We find that firms explain approximately 39% of the gender pay gap, which averaged 25 log points in the analyzed period (2005–2013). Sorting effects account for approximately 32 to 36 pp (82–92%), and bargaining power effects account for 3 to 7 pp (8–18%). The EPL helps to reduce the gender pay gap explained by firms by 1.5 pp (6.1%). The reductions are larger in firms with 200 and more workers exposed to higher fines and the disclosure requirement, ranging between 1.9 and 2.5 pp (7.8–10.2% of the pay gap). We find that the effect of the law manifests through bargaining power only, with no impact on the sorting channel. We argue that the limited effect of the law is due to the reduced share of the bargaining power effect relative to the sorting effect of the firm's total contribution to the pay gap.

Our paper contributes to the literature in three ways. First, it is the first paper, to the best of our knowledge, to assess the effects of an EPL on the firm-specific pay premium gap. The Chilean case provides a unique opportunity to evaluate the impact of an EPL on the contribution of firms to the gender gap. While many countries passed EPLs in the 1970s or earlier, when no matched employer-employee data existed, Chile passed its EPL in 2009, when this type of data was already available.² Indeed, we have access to rich administrative matched employer-employee data from the Unemployment Insurance registry (UI), allowing us to construct a large monthly panel dataset with 60 million observations. Second, we can decompose the law's impact on bargaining power and sorting channels using the framework of CCK. Third, our paper provides estimates of bargaining power and sorting effects in the context of a medium-income country with labor market institutions different from those of the developed world.

Our paper relates to three broad streams of the literature. First, our study contributes to the literature studying the role of firms in determining the pay gap. Using matched employer-employee data from Portugal, [Card et al. \(2016\)](#) showed that gender differentials in the firm-specific pay premium explain approximately 20% of the gender pay gap. As described above, they decomposed firms' contribution to the gender gap into sorting and bargaining power effects, finding that sorting effects explain most of the gap (80%), with bargaining power explaining approximately one fifth.³ Our paper also relates to [Barth et al. \(2021\)](#), who analyzed workers' movements within and across firms to measure shifts in the salaries of men and women over the lifecycle. They found that differential mobility between firms by gender could explain approximately 27% of the widening of the overall gender pay gap for college-educated workers.

Our paper also contributes to the literature on EPLs and their effects on the gender pay gap. Evidence on the effectiveness of EPLs remains

scarce. [Gunderson \(1975\)](#) assessed the effect of equal pay legislation on male-female pay differentials using Canadian data. Using ordinary least squares (OLS), he found no significant effect of the law on Canada's gender pay gap. [Neumark and Stock \(2006\)](#) studied the effects of sex and race discrimination laws on employment and earnings in the US. Using a difference-in-difference-in-differences approach (DDD), they assessed how changes in employment and earnings differ among states that did and did not pass sex and race discrimination laws. They found a negative effect on employment and a negative short-term effect on earnings, "but with a growing positive effect over time that offsets this."⁴

Finally, since the EPL includes a disclosure requirement for large firms, our study relates to the literature on the effect of transparency laws on wage inequality. For example, [Mas \(2017\)](#) studied the effect of a transparency mandate in California that required all municipal salaries to be posted online. He found a 7% decline in average compensation of top managers and a 75% increase in their quit rate; these results are consistent with the hypothesis of aversion to excessive remuneration. [Baker et al. \(2019\)](#) examined a similar policy that disclosed the salaries of university faculties in Canada when they exceeded specified thresholds, finding a decrease in average salaries and a contraction of the gender wage gap.

The rest of this paper is organized as follows. [Section 2](#) describes the institutional background, and [Section 3](#) describes the data. [Section 4](#) presents the empirical models, [Section 5](#) reports the results, and [Section 6](#) concludes the study.

2. Institutional background

Despite the rapid advance of the female labor participation rate in the 2000s, Chile has shown one of the lowest rates relative to OECD and Latin American countries, reaching 48% in 2012 ([Verick, 2014](#)).⁵ Although public policies targeted at increasing the female labor force participation rate have had positive results, some considerations must be taken into account to understand this still low figure. For example, the incorporation of females into the labor market has not been homogeneous; there is gender segregation by industry ([Henríquez and Riquelme, 2010](#)). Often, this situation has meant that women are in lower-quality jobs than men are.

Recent measures of the gender pay gap have indicated that Chilean female workers earn between 18% and 23% less than male workers, placing the country in the 60th percentile of the gender wage gap distribution among OECD countries ([OECD, 2015](#); [Petricarà and Bueno, 2009](#)).⁶ Although this percentage is lower than that in the 1960s, when women earned half the average wage earned by men, it is still sizable and significant ([PNUD, 2010](#)). Additionally, [Fuentes and Vergara \(2018\)](#) reported a consistent drop in the gap in the period of 1990–2006, from 38.6% to 18.5%, and then continuing, with ups and downs within an approximate 20% range since then. This stagnation in the

⁴ In a meta-analysis of the effectiveness of ratifying international anti-discrimination conventions (not strictly EPLs), [Weichselbaumer and Winter-Ebmer \(2007\)](#) found that countries that ratify them exhibit a reduction of 3.6 pp in the residual gender pay gap. Other articles, indirectly related to ours, have studied the effects of quotas on corporate decisions and the gender gap. See [Matsa and Miller \(2013\)](#) and [Bertrand et al. \(2019\)](#) for more details.

⁵ [Henríquez and Riquelme \(2010\)](#) showed that female labor force participation increased from 30.9% in the 1990s to 41.3% in 2009, while the percentage of women contributing to household income rose from 28.7% to 38.6%. Furthermore, one third of Chilean households are headed by a woman, and this proportion grows to 43.2% in low-income families and 47.9% in homeless households, in which the woman's income is often the only household income.

⁶ [Blau and Kahn \(2003\)](#) used data for 22 countries to study international differences in the gender pay gap. They found that the greater that the extent of collective bargaining coverage is, and the more compressed that the wage distribution is, the smaller that the gender pay gap is. Relative to OECD countries, Chile has an unequal wage distribution, with a Gini coefficient of 0.46 in 2013, and low coverage of collective bargaining, at 17.3% in 2013, ([OECD, 2020](#)).

² For instance, the Equal Pay Act of 1963 in the US ([Wilkins, 2012](#)), the 1969 equal pay legislation in Ontario, Canada ([Gunderson, 1975](#)), and the UK Equal Pay Act of 1970 ([Manning \(1996\)](#)).

³ Other papers using the CCK framework are [Sorkin \(2017\)](#) for the US, [Coudin et al. \(2018\)](#) for France, [Bruno \(2019\)](#) for Germany, and [Casarico and Lattanzio \(2019\)](#) for Italy.

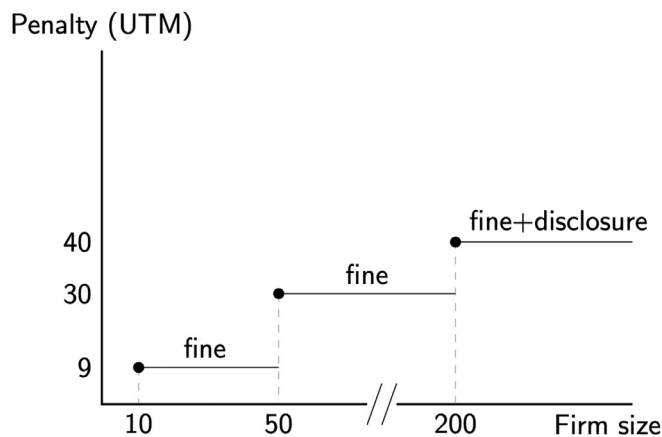


Fig. 1. Penalties for EPL Infringement by Firm Size *Notes:* This figure shows the penalty structure established by the Equal Pay Law by firm size. Penalties increase with firm size, as measured by the number of permanent workers. Monetary units are *Unidad Tributaria Mensual* (UTM), which are equivalent to US\$ 72.5 at an exchange rate of CLP\$ 670 per USD. Firms with fewer than 10 workers are not affected by the EPL.

narrowing of the wage gap is consistent with the findings in developed countries (Blau and Kahn, 2017; Bruns, 2019; Goldin, 2014).

Aiming at reducing the documented wage gap, in November 2009, an EPL (Law No. 20,348) was enacted, becoming the most important regulation passed on this subject in decades. The law establishes that employers must comply with the principle of equal pay between men and women who perform the same job. Specifically, it requires firms with 10 or more permanent workers to establish a grievance procedure for any employee who feels discriminated against because of his or her gender. If a worker who has followed the internal procedure defined by his or her company receives no response or if the response is unsatisfactory, he or she can report these practices to the Labor Inspection Office, which oversees the complaint. The participation of the Labor Inspection Office is not essential since the worker may go directly to the labor courts.

The law also mandates firms with 200 or more workers to describe all positions or functions in the company according to their essential technical skills (disclosure requirement). Both the grievance procedure and the registration of jobs and functions must be part of the firms' internal regulations.

There are monetary penalties for firms that violate the law, varying by firm size as measured by the number of permanent workers. Firms with 10 to 49 workers are exposed to a fine of 9 *Unidad Tributaria Mensual* (UTM), nearly US\$ 653.⁷ Those with 50 to 199 workers are exposed to fines of UTM 30, equivalent to US\$ 2,176. Finally, firms with 200 or more workers face fines of UTM 40, or approximately US\$ 2,900. See Fig. 1 for a graphical illustration.

The penalty structure gives us the opportunity to evaluate the effects of the law at the intensive margin and to assess different treatments in terms of the amount of the fine and the disclosure requirement.

Finally, it is important to note that, while firms with fewer than 10 workers correspond to 37% of the firms in our sample, they only account for 5% of total employment. In contrast, firms with 200 or more workers represent 4% of the total number of firms but account for nearly 47% of total employment. Hence, the law could potentially affect a significant proportion of the labor force.

3. Data

The main source of information is a 30% sample of matched employer-employee data from the Chilean UI registry. By law, the unemployment fund administrator is required to collect, on a monthly basis, all contributions to the individual unemployment accounts (and solidarity fund) for each labor relationship. Hence, our data consider only formal workers, who account for nearly 70% of the labor force.⁸ Specifically, we have access to information on individual characteristics, such as age, education, gender, marital status, region, time of affiliation with the insurance, and monthly taxable wages. The registry does not include hours worked, so we use monthly wages. However, the incidence of part-time work in the formal sector is only 4% in Chile, so the effects of part-time work on our analysis are very limited.⁹ The registry includes information about applications for withdrawals from an individual's UI account, such as the number of withdrawals an individual is allowed to collect, the date of and reason for an application, the date of and reason for contract termination, and the individual's former employer.

The UI system started in October 2002 with new job contracts, so the UI data have become more representative over time. According to Sehnbruch and Carranza (2015), in 2005, they represented approximately 50% of all wage earners, reaching 80% of Chilean formal wage earners in 2012. Hence, our sample spans from November 2005 to October 2013 (T=96) and includes monthly information on 80% of wage earners.

With these data, we construct a monthly panel, which is described in Table 1. Columns (1) and (2) show information on male and female workers in the overall sample, respectively, and the rest of the columns show the subsamples used to estimate the double fixed-effect models that are discussed later. We focus on males between 19 and 65 years of age and females between 19 and 60 years of age (65 and 60 are the male and female retirement ages, respectively) with at least one year of effective experience in the formal labor market (more than 12 contributions). For the 2005–2013 period, we have data for 1.2 million workers, who are observed between 13 and 96 times (55 on average) with an employer identifier in each month. Since we have a monthly panel with a relatively large span (T=96), we have many individual-month observations with more than 44 million observations for men and 20 million for women. Other features of the data include that females are younger, more educated, and earn 21 log points less than males in the overall analysis sample. The average firm size in terms of employees ranges from 15 to 22 workers total in the overall sample.

Columns (3) and (4) of Table 1 report information for male and female workers in a connected set, i.e., a set that includes person-month observations for a group of firms linked by worker mobility. Columns (5) and (6) report information for male and female workers in a dual-connected set, i.e., a set that includes observations of individuals working at firms in a connected set for both genders. The statistics among the different samples are quite similar, with some variations. For example, the raw pay gap is slightly greater in the dual-connected set than in the overall sample (0.24 instead of 0.21), and the number of observations in the dual-connected set is approximately 92% of that of the entire sample. The large proportion of firms in the dual-connected set is due to the high mobility in the Chilean labor market, preventing the limited mobility bias problem reported by Bonhomme et al. (2020). Indeed, job reallocation in Chile, as measured by the average annual entry and exit rates of workers at the firm level, was approximately 37% in 2005–2014, which is significantly higher than that for a sample of 25 OECD countries (Albagli et al., 2017; Bassanini and Garnero, 2013).

⁸ According to Bassi et al. (2011), informality in Chile varies by cohort, being 30% for youth and nearly 20% for adults. Informality also showed a declining pattern over the 1980–2010 period.

⁹ Rau (2010) showed that only 9% of wage earners work part-time, and 48% of them have work contracts. These figures imply that approximately 4% of workers in the formal sector are part time.

⁷ A *Unidad Tributaria Mensual* (UTM) is a Tributary Unit equal to CLP\$ 48,595 as of May 2019. At the exchange rate of CLP\$ 670 per USD, one UTM equals US\$ 72.5.

Table 1
Descriptive Statistics for Three Different Samples.

	Connected Sets of Workers/Firms					
	Overall analysis		All-Connected		Dual-Connected	
	Males (1)	Females (2)	Males (3)	Females (4)	Males (5)	Females (6)
Age:						
Mean Age	36.5	34.6	36.3	34.5	36.2	34.5
Fraction ≤ 30 years old	0.35	0.40	0.36	0.41	0.36	0.41
Fraction ≥ 50 years old	0.15	0.09	0.14	0.09	0.14	0.09
Education:						
Mean Years Schooling	9.3	9.9	9.4	9.9	9.4	9.9
Fraction with High School	0.51	0.57	0.51	0.57	0.51	0.57
Fraction with Degree	0.08	0.12	0.08	0.12	0.08	0.12
Mean Log Real Hourly Wage (standard dev.)	8.08 (0.82)	7.87 (0.82)	8.13 (0.76)	7.90 (0.74)	8.14 (0.76)	7.90 (0.74)
Fraction in Metropolitan Region	0.55	0.56	0.54	0.55	0.55	0.57
Mean Firm Size (No. emp's)	15	22	15	18	19	20
Fraction Females at Firm	0.24	0.60	0.28	0.64	0.28	0.64
Number person-month obs.	44,110,540	20,380,928	41,667,999	20,348,942	39,558,329	19,765,874
Number of persons	763,535	415,287	724,698	400,740	717,691	396,769
Number of firms	218,348	138,084	136,891	117,923	108,235	108,235

Notes: Overall analysis sample (columns 1 and 2) includes formal male workers between 19–65 years and formal female workers between 19–60 years (retirement age for females is 60 and for males is 65). All statistics are calculated across individual-month observations. Wages are measured in real CLP per month as of December 2008. A connected set includes a group of firms linked by worker mobility. A dual-connected set includes person-month observations of individuals working at firms that are in the connected sets of both genders.

4. Modeling framework

4.1. Empirical model

We use the wage bargaining model developed by CCK, which is explained in detail in [Appendix A](#).

Let worker i 's wage in period t at firm $j = J(i, t)$ be denoted by $w_{iJ(i,t)}$. Additionally, let worker i 's gender be $G(i) \in \{F, M\}$. The estimating equation is simply

$$w_{iJ(i,t)} = \alpha_i + \psi_{J(i,t)}^{G(i)} + X'_{it}\beta^{G(i)} + r_{it}, \quad (1)$$

where α_i is an individual fixed effect, and $\psi_{J(i,t)}^{G(i)}$ is a firm fixed effect representing the gender-specific firm pay premium. Time variant worker i 's characteristics are X_{it} , $\beta^{G(i)}$ is a vector of parameters for gender $G(i)$, and the term r_{it} is a composite error. This specification allows us to analyze the difference between firm effects on females and firm effects on males. Additionally, as discussed later, it allows us to estimate the firm effects before and after the EPL to implement a DID analysis.

As noted by [Abowd et al. \(2002\)](#) and CCK, the firm effects in [Eq. \(1\)](#) are identified within a “connected set” of firms that are linked by worker mobility. Estimation of the AKM model is performed in the largest connected set.

For unbiased estimates, the model must comply with the following condition:

$$E\left[(r_{it} - \bar{r}_i)(D_{it}^j - \bar{D}_i^j) | G(i)\right] = 0 \quad \forall j \in \{1, \dots, J\}, \quad (2)$$

where $D_{it}^j \equiv \mathbf{1}[J(i, t) = j]$ indicates whether individual i is employed at firm j at time t , and the bars over D and r represent time averages. CCK discussed three channels through which this condition would be violated and proposed some graphical tests to assess the plausibility of the orthogonality condition. First, there should be neither an “Ashenfelter dip” in job leavers’ wages prior to their exit nor wage growth for recent joiners. These specifications exclude connections between firm-wide shocks and mobility rates. The second channel refers to the relationship between mobility and idiosyncratic match effects between worker i and firm j . The additive separable model predicts symmetry in the gains and losses of workers who move up (say from firm A to B) and those who move down (from firm B to A). This prediction is inconsistent with mobility driven by the job match effect, which implies positive

wage gains for movers in both directions ([Eeckhout and Kircher, 2011](#)). The last channel relates to transitory wage shocks correlated with the direction of firm-to-firm mobility, for example, whether workers who are performing well are more likely to be promoted and move to firms with higher wages or whether those who are not promoted are more likely to move to firms with lower salaries. In [Appendix B](#), we provide graphical evidence supporting the exogenous mobility assumption implied by [Eq. \(2\)](#).

A second threat relates to the limited mobility bias problem in the estimation of the firm fixed effects reported by [Bonhomme et al. \(2020\)](#). This issue occurs when firms are weakly connected by workers’ mobility, resulting in a downwardly bias correlation between estimated worker and firm effects, among other issues. As discussed in the previous section, Chile shows the highest average annual entry and exit rates of workers at the firm level in a sample of OECD countries ([Albagli et al., 2017](#); [Bassanini and Garnero, 2013](#)). However, we assess the relevance of this limited mobility bias in two ways. First, we implement [Kline et al. \(2020\)](#) bias correction and compare the uncorrected and corrected worker and firm effects correlations. Second, we follow an approach proposed by [Bonhomme et al. \(2020\)](#), which computes firm fixed effects using a clustering approach (k-means). In [Appendix B](#), we show the results of these approaches finding negligible mobility bias.

4.2. The role of firms on gender wage disparities

Using a sample of workers at firms that are doubly connected for males and females, we can first determine the role of firms in the gender pay gap. Then, we can decompose the difference between the average pay premium received by each gender into bargaining power and sorting effects in an Oaxaca-type wage decomposition. Note that the average pay premium received by males can be denoted as $E[\psi_{j(i,t)}^M | male]$, and the average premium received by females, similarly, as $E[\psi_{j(i,t)}^F | female]$. By calculating the difference and performing a simple algebraic manipulation, we can obtain the following decompositions:

$$\begin{aligned} E[\psi_{j(i,t)}^M | male] - E[\psi_{j(i,t)}^F | female] \\ = E[\psi_{j(i,t)}^M - \psi_{j(i,t)}^F | female] + E[\psi_{j(i,t)}^M | male] - E[\psi_{j(i,t)}^M | female]. \end{aligned} \quad (3)$$

The first term of the right-hand side of Eq. (3) corresponds to what CCK called the bargaining power effect, which can be obtained by calculating the difference between ψ_j^M and ψ_j^F across the distribution of jobs held by female workers. The last term of the right-hand side of Eq. (3) is the average sorting effect that compares the average value of ψ_j^M across the jobs held by male versus female workers. Certainly, Eq. (3) can be derived in an alternative manner: the bargaining power effect can be estimated using the distribution of jobs held by male workers, and the sorting effect can be calculated by comparing the average value of female pay premiums across jobs held by male versus female workers. We present both in the results section.¹⁰

4.3. The effect of the equal pay law on firm pay premiums

As discussed in Section 2, in November 2009, an EPL entered into force in Chile, which affected firms with 10 or more workers. To assess the effects of the EPL on the contribution of firms to the gender pay gap, we split the sample into two subintervals: 2005–2009 (before) and 2010–2013 (after). This split sample allows us to implement the following DID analysis:

$$(\hat{\psi}_{jt}^M - \hat{\psi}_{jt}^F) = \mu_j + \delta post_t + \gamma post_t \times d_{10j} + u_{jt}, \quad (4)$$

where μ_j is a firm fixed effect, and $(\hat{\psi}_{jt}^M - \hat{\psi}_{jt}^F)$ corresponds to the contribution of firm j to the gender gap in time t , obtained by fitting Eq. (1) to the two samples described above (before and after the reform). The variable $post_t$ is a post law dummy with $t \in \{before, after\}$, and d_{10j} is a dummy equal to one if firm j has 10 or more workers at baseline, thereby defining treated and control firms. Then, the parameter accompanying the interaction term, $post_t \times d_{10j}$, gives us the average effect of the law on the difference in pay premiums received by men and women in treated firms.

Since $E[\psi_{j(i,t)}^M - \psi_{j(i,t)}^F | female]$ corresponds to the bargaining power channel in Eq. (3), using the distribution of jobs held by females as a weighting scheme in the estimation of Eq. (4) allows us to interpret γ as the effect of the EPL on the bargaining power channel in the treated firm.¹¹

As explained before, the penalty structure gives us the opportunity to assess the heterogeneous effects of the law by firm size (see Fig. 1). We then estimate a variant of Eq. (4) that includes three different treatment dummy variables: $d_{10/49}$, for firms with between 10 and 49 workers (exposed to a fine of UTM 9), $d_{50/199}$ for firms with between 50 and 199 workers (exposed to a fine of UTM 30), and d_{200} for firms with 200 or more permanent workers (exposed to a fine of UTM 40 plus the disclosure requirement).

$$(\hat{\psi}_{jt}^M - \hat{\psi}_{jt}^F) = \mu_j + \delta post_t + \sum_{i \in \{10/49, 50/199, 200\}} \gamma_i post_t \times d_{ij} + u_{jt}. \quad (5)$$

Hence, when using the distribution of jobs held by females as a weighting scheme, the parameters γ_i in Eq. (5) allow us to assess the heterogeneous effects of the EPL in the bargaining power channel by firm size. An alternative approach is to compare $E[\psi_{j(i,t)}^M - \psi_{j(i,t)}^F | female]$ before and after the law for firms of different sizes, using firms with fewer than 10 workers as a benchmark.

In addition to the parallel pretrend assumption in our research design, we require that firms do not respond to the law by adjusting their size, as defined by the number of workers. Since firm size is a choice variable, firms could react to the policy by decreasing their numbers of workers to avoid the regulation or decrease the monetary penalty.

These reactions might also be motivated by changes in economic conditions. If a firm is affected by a transitory shock, it might be more willing to decrease its number of workers. To avoid any interaction of treatment status with time-varying unobserved shocks, we define the treatment variable at baseline, i.e., using pretreatment firm-size measures. The treatment variable is fixed, reducing this threat. However, firms can still reduce their numbers of workers in reaction to the policy. For example, a firm with 10 workers before the law could reduce the number to nine after the policy. In this case, we could have some firms not affected by the EPL in the treatment group, and our estimates could be downwardly biased and should be interpreted as a lower bound. We provide an analysis of parallel pre-trends and firm size distribution in Appendix D.

Assessing the effect of the EPL on the sorting channel in a linear model, as we did for the bargaining power channel, is more complicated since there are different weighting schemes in the terms $E[\psi_{j(i,t)}^M | male] - E[\psi_{j(i,t)}^M | female]$. Instead, we follow two approaches. First, we compare the sorting term before and after the law for firms of different sizes, using firms with fewer than 10 workers as a benchmark. This approach is similar to that of Bruns (2019). Second, we assess whether there is any change in the probability of moving to firms in a higher/lower wage premium quartile for females after the reform relative to males. This approach is similar to that of Casarico and Lattanzio (2019), who remarked that gender differentials in firm mobility are an important driver of the gender gap in wage growth (Del Bono and Vuri, 2011; Loprest, 1992; Manning and Swaffield, 2008). Thus, we fit linear probability models to assess the effects of the law on firm mobility in a DID approach as follows:

$$Pr[Q_{i,t,k} \times Q_{i,t-1,j} = 1 | \cdot] = \alpha_i + X_{it}'\beta + \delta post_t + \gamma post_t \times female_i, \quad (6)$$

where $Q_{i,t,k}$ is a dummy variable equal to one if person i is in a firm in quartile k in time t . Thus, $Q_{i,t,k} \times Q_{i,t-1,j}$ is a dummy variable equal to one if person i moves from a firm in quartile j in $t-1$ to a firm in quartile k in t , with $k \neq j$; $k, j \in \{1, 2, 3, 4\}$ and $t \in \{2005m11, \dots, 2013m10\}$. Quartiles are calculated using the estimated firm fixed effects distribution for males and females separately in the sample of firms in the dual-connected set. We fit two separate versions of Eq. (6): one for cases with $k > j$ when estimating the effects of the law on the probability of moving up and another for $k < j$ when estimating the effects on the probability of moving down. The vector of covariates X_{it} is the same as that used in Eq. (1), α_i is a worker fixed effect, $post_t$ is the previously defined post law dummy, and $female_i$ is a dummy equal to one if person i is female.

5. Results

5.1. Estimation sample

We estimate Eq. (1) separately for men and women, including individual effects, firm effects, and individual characteristics, such as age and education (see Section 5.2 for a full description of the covariates).

The estimation sample corresponds to the largest connected set described in columns (3) and (4) of Table 1. As shown and due to our long panel (96 months), the largest connected set is approximately 96% of the overall sample. Thus, the generous length of the panel and the high level of labor mobility in the Chilean labor market allow most of the firms to be connected in the period analyzed (Albagli et al., 2017). Indeed, 94% of all person-month observations for male workers and 99% of all person-month observations for female workers are included in the largest connected set. More importantly, the mean characteristics of workers included in the largest connected set and those of workers from the overall sample are very similar.

Once the AKM models are estimated for male and female workers, as in Card et al. (2016), we focus on a narrow sample of workers at firms that are doubly connected, that is, connected at the same time for men and women. In this dual-connected set, we estimate fixed effects for each firm for both genders ($\psi_{j(i,t)}^M, \psi_{j(i,t)}^F$) to perform the Oaxaca-type wage

¹⁰ In such cases, the decomposition is $E[\psi_{j(i,t)}^M | male] - E[\psi_{j(i,t)}^F | female] = E[\psi_{j(i,t)}^M - \psi_{j(i,t)}^F | male] + E[\psi_{j(i,t)}^F | male] - E[\psi_{j(i,t)}^F | female]$.

¹¹ This reweighting approach can be implemented directly by estimating Eq. (4) weighted by the fraction of females at each firm before the law came into effect.

decompositions. The dual-connected sample is described in columns (5) and (6) of Table 1 and includes approximately 92% of the observations of the overall analysis sample. As shown, the sample averages are quite close.

5.2. Estimation results from the worker-firm models

In Table 2, we present the estimation results from the two-way fixed-effect model for men and women in the largest connected set of workers of each gender (described in columns (3) and (4) of Table 1). The covariates included are workers and firm fixed effects, year dummies shown in levels and interacted with four education categories (no education, primary, secondary and tertiary), and quadratic and cubic age terms interacted with the education dummies.¹² We present a summary of the parameter estimates, such as the standard deviation of the person and firm effects and the standard deviation of the covariates (across person-month observations). As shown, person fixed effects have larger variances than firm effects, as found in the literature. We also compute the correlation between person and firm effects. We find that these correlations are positive for both males (0.292) and females (0.306), indicating that firms that pay more tend to have more highly skilled workers.

Evidence on the type of assortative matching is mixed. While positive assortative matching has been reported in Brazil (Morchio and Moser, 2020), Portugal (Card et al., 2016), Germany (Card et al., 2013) and New Zealand (Maré and Hyslop, 2007), negative assortative matching has been found in Washington state and France (Abowd et al., 2002) and in Italy (Casarico and Lattanzio, 2019). However, there is evidence that these correlations can be substantially downwardly biased if the mobility of workers across firms is limited (Andrews et al., 2008). In a recent paper, Bonhomme et al. (2020) implements bias correction techniques to matched employer-employee data from the US and some European countries, finding that the discussed correlations are positive, ranging from 0.24 (Sweden) to 0.34 (Austria and Italy). Hence, our estimated correlations are in line with those found by Bonhomme et al. (2020). As discussed in Section 3, labor mobility in Chile is quite high, attenuating the limited mobility bias problem.

Along this line, we implement two strategies to assess whether we face a limited mobility bias problem with our data. First, we use the Kline et al. (2020) approach to address this issue by a leave-one-out estimation of the variance components in the AKM model and by comparing uncorrected and bias corrected estimates of the worker and firm fixed-effects correlation. Second, we implement an alternative earnings decomposition based on a firm clustering approach developed by Bonhomme et al. (2020).¹³ In Appendix B, we present the results for these two approaches finding that limited mobility bias is negligible in our sample. Our results are consistent with Huneus et al. (2021), who found that limited mobility bias in Chilean data is negligible.

Finally, the last part of Table 2 shows fit statistics for an alternative model that relaxes the assumption of linear separability of the fixed effects of Eq. (1). Instead, this “job-match effects model” includes dummies for each worker-firm match. As observed, the match-effects model provides a modest improvement in fit of only 5–6%. In Appendix B, we provide additional robustness checks that provide evidence favoring our additive specification of Eq. (1).

¹² As in Card et al. (2016), we exclude a year dummy, which, in our case, is 2005. The linear term of age is also excluded to avoid collinearity between year and age when person effects are included.

¹³ Bonhomme et al. (2020) suggested that mobility bias appears when there are many firms and few movers. Their approach consisted of reducing the dimensionality of the problem by grouping firms in few clusters based on the similarity of their pay distributions.

Table 2

Summary of Estimated Two-way Fixed Effects Models for Males and Females .

	All Males (1)	All Females (2)
Standard deviation of log wages	0.759	0.737
Number of person-month observations	41,667,999	20,348,942
<u>Summary of Parameter Estimates:</u>		
Number of person effects	724,698	400,740
Number of firm effects	136,891	117,923
Std. dev. of person effects (across person-month obs.)	0.457	0.501
Std. dev. of firm effects (across person-month obs.)	0.323	0.260
Std. dev. of Xb (across person-month obs.)	0.181	0.164
Correlation of person/firm effects	0.292	0.306
RMSE of model	0.380	0.365
Adjusted R-squared of model	0.754	0.761
Correlation of estimated male/female firm effects	0.544	
<u>Comparison job-match effect models:</u>		
Number of job-match effects	3,265,544	1,469,560
RMSE of match-effects model	0.321	0.312
Adjusted R-squared of match-effects model	0.816	0.817
Std. dev. of match effect*	0.203	0.189

Notes: See text. All specifications include dummy variables for workers and firms, year dummies interacted with education dummies, and quadratic and cubic terms in age interacted with education dummies (total of 40 parameters). Job-match effects models include dummy variables for each worker-firm job match plus the other covariates in the basic models. Estimating samples include only observations in the largest connected set. *Std. deviation of match effect computed as square root of difference in mean squared errors between model and job-match effect model.

5.3. The effect of firms on wages and their contribution to the gender pay gap

Before presenting the results for firm-specific pay premiums and the gender pay gap, we must normalize the firm effects because they are non-negative in the wage-setting model. Then, we must identify the threshold level of the surplus paid by firms, and those with a surplus less than that threshold are considered “no-surplus firms.” We use firms in the food industry as the “no-surplus” firms since we observe that they have one of the smallest wage premiums on average in our data.¹⁴

As discussed by CCK, the normalization of firm effects does not affect the estimation of the sorting effects (which are invariant to normalization) but it does affect the bargaining power effect. In appendix Table C1, we present an alternative normalization using firms in the hotel industry as a robustness check and find similar results.

Next, we quantify the effect of firm-specific pay premiums using normalized firm effects for male and female workers. In Table 3, we present the results for various subsamples. The first row shows the results for the entire dual-connected set. Column (1) shows that the pay gap in this sample is 24.5 log points. Columns (2) and (3) show the male and female premiums, respectively, and column (4) shows the difference between the two, corresponding to firms’ total contribution to the pay gap. The percentage of the overall male-female pay gap is in parentheses. Thus, approximately 39% of the pay gap (9.6 log points) is explained by firms. Next, in columns (5) to (8), we compute the terms contained in Eq. (3). When the Oaxaca-type decomposition of Eq. (3) is implemented, we find that nearly 8 log points (out of 9.6) are explained by sorting effects (column (5)). This figure corresponds to almost 32% of the gender pay gap (82% of the firm’s total contribution to the pay gap). The rest, 1.7 log points, is explained by bargaining power effects (column (8)), corresponding to 7% of the gender pay gap (18% of the total contribution of firms to the pay gap). In contrast, when the decomposition is imple-

¹⁴ In one of the specifications implemented by Card et al. (2016), the authors used hotels and restaurants as the no-surplus firms for the normalization of firm effects, finding similar results for their normalization based on value added, which was their preferred specification.

Table 3
Contribution of Firm-Specific Pay Premiums to the Gender Wage Gap at Dual Connected Firms.

	Gender Wage Gap (1)	Means of Firm Premium:		Total Contribution of Firm Components (4)	Decomposition of Contribution of Firm Component:			
		Male Premium Among Men (2)	Female Premium Among Women (3)		Sorting		Bargaining	
					Using Male Effects (5)	Using Female Effects (6)	Using Male Distribution (7)	Using Female Distribution (8)
All	0.245	0.251	0.155	0.096 (39.0)	0.078 (32.0)	0.088 (36.0)	0.007 (3.0)	0.017 (7.0)
<u>By Age Group:</u>								
Up to Age 30	0.129	0.227	0.151	0.076 (58.5)	0.059 (45.2)	0.069 (53.4)	0.007 (5.1)	0.017 (13.3)
Age 31–40	0.248	0.279	0.175	0.104 (41.8)	0.084 (34.0)	0.091 (36.8)	0.012 (5.0)	0.019 (7.8)
Over Age 40	0.352	0.250	0.139	0.111 (31.5)	0.096 (27.3)	0.107 (30.4)	0.004 (1.1)	0.015 (4.2)
<u>By Education Group:</u>								
Less than High School	0.303	0.226	0.121	0.105 (34.6)	0.089 (29.5)	0.106 (34.8)	-0.001 (-0.2)	0.016 (5.1)
High School	0.283	0.263	0.155	0.108 (38.1)	0.090 (31.8)	0.096 (33.9)	0.012 (4.2)	0.018 (6.3)
University	0.325	0.351	0.251	0.100 (30.9)	0.078 (23.9)	0.069 (21.2)	0.032 (9.8)	0.023 (7.0)
<u>No. of workers at the firm:</u>								
Less than 10	0.082	0.147	0.155	-0.008 (-10.2)	0.004 (4.3)	0.022 (26.3)	-0.030 (-36.5)	-0.012 (-14.5)
10+ and less than 50	0.091	0.188	0.177	0.011 (12.2)	0.014 (14.9)	0.027 (29.1)	-0.015 (-16.8)	-0.002 (-2.6)
50+ and less than 200	0.247	0.257	0.166	0.092 (37.4)	0.078 (31.5)	0.082 (33.1)	0.011 (4.3)	0.014 (5.9)
More than 200	0.362	0.308	0.142	0.166 (45.9)	0.132 (36.4)	0.138 (38.1)	0.028 (7.8)	0.034 (9.5)

Notes: Estimating samples include male and female workers in a dual connected set. Column 1 shows the gender wage gap, as measured by the difference between the mean log wages of males and females, estimated over all workers in the subset indicated by the row title. Estimated firm effects are from models described in columns 1 and 2 of Table 2. Columns 2 and 3 report the firm wage premiums for males and females, respectively. Column 4 computes the total contribution of firm wage premiums to the gender wage gap expressed in column 1. Columns 5–8 show the contributions of the sorting effect and the bargaining effect to the gender wage gap, computed using the CCK decomposition described in the text. The percentages of the overall male-female wage gap (column 1) are in parentheses.

mented the other way (as explained in footnote 10), the sorting effect increases to 8.8 log points (36%), and the bargaining power effect decreases to 0.7 log points (3%). In this case, sorting explains 92% and bargaining power 8% of the firm's total contribution to the pay gap.

Compared to the estimates in the literature, our findings for sorting effects are close to, albeit higher than, those found by Sorkin (2017) for the United States (27%). Regarding European countries, our results are larger than those found by Card et al. (2016) for Portugal (15%) and by Casarico and Lattanzio (2019) for Italy (21 to 23%). Sorkin (2017) suggested that part of the sorting effect might be due to differences in hours (there exist low- and high-hour firms). Given that our data do not contain hourly wages, like the Portuguese data do, the estimates might not consider this effect.¹⁵

Furthermore, we must bear in mind that the levels of development reached by Chile are still far from the OECD average in certain areas. Specifically, Chile falls behind in the share of females who are STEM graduates and medical doctors (OECD, 2017a; 2017b), which are two of the best-paying fields of work in the country. According to MIFUTURO (2019), nine of the 10 best-paying degrees in Chile correspond to STEM majors, while only 21% of Chilean STEM graduates are female (OECD, 2017a). This proportion is low compared to the OECD average, which is 39%, and Portugal's 57% (OECD, 2017a). Similarly, although medicine is the fourth highest-paid degree in the country, the percentage of female doctors in Chile is 39%, whereas in Portugal, it

is 53% (OECD, 2017b). The former could explain the difference in the magnitude of our estimates for the sorting effect.

We also implement the Oaxaca-style decomposition from Eq. (3) in different subpopulations. In particular, we explore the heterogeneity of the effects by age (up to age 30, between 31 and 40, and older than age 40), education group (less than high school, high school, and some college and more), and size of the firm (as measured by the number of workers). As can be seen, the gender pay gap increases with age, is relatively stable by education group and is large for those with a fixed-term contract relative to workers with permanent contracts.¹⁶ Interestingly, the total contribution of firms to the gender pay gap decreases monotonically with age and decreases (non-monotonically) with education. Sorting effects are the most important component of firms' total contribution. However, for more-educated workers (university), the bargaining power effect explains a considerably greater proportion of the firms' contribution to the pay gap than it does for workers with a high school education or less (see columns (7) and (8) for the three education groups).

When examining firms of different sizes, we find that the pay gap tends to increase with size. Firms with fewer than 10 workers exhibit a gap of only 8.2 log points, and the firm contribution is only -0.8 log points (only -10.2% of the pay gap). Similarly, firms with 10 or more but fewer than 50 workers show a pay gap of 9.1 log points with a firm

¹⁵ Other articles providing evidence on the relevance of sorting and segregation on the gender pay gap are Bayard et al. (2003) for the US and Ludsteck (2014) for Germany.

¹⁶ A widening gender pay gap over the lifecycle has been reported in many countries, and some factors underlying it relate to the role of raising children, the demand by female workers for flexibility, and less internal advancement for mothers or glass ceiling effects. See Goldin et al. (2017) and Card et al. (2016) for evidence in the US and Portugal, respectively.

Table 4
DID Effects of the Equal Pay Law on the Firm Wage Premium Gap.

Δ_G Firm Effects	(1)	(2)	(3)	(4)
post	0.019*** (0.004)	0.019*** (0.004)	0.019*** (0.004)	0.019*** (0.004)
post $\times$ d10	-0.015** (0.005)	-0.018*** (0.004)		
post $\times$ d10/49			-0.006 (0.005)	-0.008* (0.004)
post $\times$ d50/199			-0.014** (0.005)	-0.018*** (0.005)
post $\times$ d200			-0.019*** (0.005)	-0.025*** (0.005)
N	63,866	63,866	63,866	63,866
R ²	0.001	0.001	0.001	0.002
Firm fixed effects	Yes	Yes	Yes	Yes
Firm size weights	Yes	No	Yes	No
Female workers weights	No	Yes	No	Yes

Notes: This table shows results from the difference-in-differences analysis of the effect of Chile's 2009 EPL on gender differentials on firm-specific wage premiums described in subsection 4.3. The dependent variable is Δ_G Firm Effects = $\hat{\psi}_{jt}^M - \hat{\psi}_{jt}^F$. Columns 1 and 2 show the results from the specification in Eq. 4 with different weighting schemes. Columns 3 and 4 show the results from the specification in Eq. 5. As discussed in Section 4.3, Columns 2 and 4 can be interpreted as the effect of the EPL on the bargaining power channel. Clustered standard errors at the firm level are in parentheses. *** p<0.001, ** p<0.01, * p<0.05.

contribution of 0.1 log points (12.2%). In both cases, bargaining power helps to reduce the pay gap. The gap increases to almost 25 log points for firms with 50 to 200 workers, with a total contribution of firms of 9.2 log points (37.4%). Finally, for firms with more than 200 workers, the pay gap is 36 log points, with firms contributing 16.6 log points (46% of the pay gap), and bargaining power increases its relevance in explaining the pay gap, reaching 3.4 log points (9.5% of the overall wage gap).

Finally, appendix Table C2 presents decomposition results by economic sector. The mining sector exhibits the largest gender pay gap (0.48), while the finance sector achieves the smallest (0.09). Moreover, the contribution of firms to the pay gap in both industries is the largest (58% and 60%, respectively).¹⁷

5.4. The effect of EPL on the contribution of firms to the pay gap

To implement a DID analysis, we split the sample into two periods: before (November 2005 to November 2009) and after the enactment of the law (December 2009 to October 2013). Thus, we estimate Eq. (1), obtain two sets of firm fixed effects and normalize them using the food industry in each period as “no-surplus firms.” Then, we construct a short panel for connected firms with two periods, $t \in \{before, after\}$. Ultimately, we have a dataset with 31,933 connected firms in the two periods, providing us with a sample of 63,866 observations.¹⁸

In Table 4, we present the results of the DID analysis on the effect of EPL on the firm-specific wage premium gap, described in subsection 4.3. All of the specifications include firm fixed effects and clustered standard errors at the firm level. In column (1), we present the estimated coefficients of Eq. (4) using firm size (at the baseline) as the frequency weight. The key parameter in this specification is that of the interaction term, $post_t \times d10_j$, implying a reduction of approximately 1.5 pp (or 6.1%) in the contribution of firms to the gap ($\hat{\psi}_{jt}^M - \hat{\psi}_{jt}^F$) for treated

firms (more than 10 permanent workers). This effect is statistically significant at the 5% level. In column (2), we fit Eq. (4) using the number of female workers (at the baseline) as the frequency weight. Since $E[\hat{\psi}_{jt}^M - \hat{\psi}_{jt}^F | female]$ can be interpreted as the bargaining power effect, as in Card et al. (2016), and the interpretation of the parameter of interest in this reweighted regression is the effect of the EPL on the bargaining power channel. The effect of the law is -1.8 pp (7.3%), and it is statistically significant at the 1% level.¹⁹

In columns (3) and (4) of Table 4, we present the results for Eq. (5), in which we allow for heterogeneous effects by firm size (which determines different treatment intensities, as discussed previously). While the effects of the law in firms with more than 10 and fewer than 50 permanent workers are small and statistically nonsignificant, in firms with between 50 and 199 workers, they are significant (1% level), with a magnitude of -1.4 to -1.8 pp (5.7% to 7.3% of the total pay gap). Interestingly, the fines for violating the EPL for firms in this bin size are more than three times the fine for firms with fewer than 50 workers. Finally, the effect of the law on firms with 200 or more workers ranges from -1.9 to -2.5 pp (7.8% to 10.2%), which is significant at the 0.1% level. It is worth noting that the effects for firms with 200 and more workers are significantly larger than those for firms with 10–49 and 50–199 workers at the 1% level (joint test). As discussed previously, firms with 200 or more workers are exposed to a fine of UTM 40 (approximately US\$ 2,900), and the disclosure requirement explains why their response, adjusting for male and female wage premiums, is larger.

It is important to note that our findings, while small, are significant and show some convexity with penalty size (as proxied by firm size). Hence, firms exposed to larger fines react more than those exposed to lower fines. Additionally, since $E[\hat{\psi}_{jt}^M - \hat{\psi}_{jt}^F | female]$ represents the bargaining power channel, our DID parameters are expected to be small, given that most of the firm's contribution to the gender pay gap occurs through sorting, rather than bargaining power effect.

In appendix Table D.1, we show the results of this DID analysis using an alternative normalization of the fixed effects (hotel industry), and the point estimates decrease by between 20% and 30% but remain statistically significant at the 5% level in most of the cases. Appendix Table D2 presents the results of the DID analysis using male weights instead of female weights for the bargaining power interpretation. Again, the results are significant, although the point estimates decrease by between 30% and 40% and remain statistically significant at the 5% level.

Table 5 shows a before and after comparison of the sorting and bargaining power channels, à la Bruns (2019). Bargaining power is calculated using female employment, and sorting is calculated using male firm fixed effects. Columns (1) and (2) show the before and after results, respectively, for the bargaining power channel, and column (3) shows the difference between columns (2) and (1). Columns (4) and (5) do the same but for the sorting channel, and column (6) expresses the difference between them. Panel A uses all workers, and small increases in bargaining and sorting effects can be seen after the law comes into effect (0.9 and 1.7 pp, respectively). Panel B shows comparisons for different firm sizes, and panel C shows simple DID estimates using firms with fewer than 10 workers as a control group. In particular, column (3) and panel C show a simple DID estimator in which we compare the bargaining power channel of treated firms of different sizes with that of firms with fewer than 10 workers before and after the law. Column (6) and panel C show the same results for the sorting channel. While we found negligible effects of the law on sorting effects (approximately 0.1 pp), the effects on bargaining power are -1.1 pp for firms with 50–199 workers and -1.7 pp for firms with 200 or more workers. These results are consistent with those reported in Table 4.

¹⁷ The results showing that the mining and finance sectors have the largest and smaller gender pay gaps, respectively, are consistent with the findings of Chuang et al. (2018) for Taiwan, and they differ from those in Card et al. (2016) for Portugal, where nonmetallic minerals and transportation are the sectors with the largest and smaller gender pay gaps, respectively.

¹⁸ In Appendix D we provide the results normalizing the firm fixed effects using the hotel industry as the no-surplus firms.

¹⁹ In Appendix D, we present the results of the DID analysis using male weights, finding results consistent with those in Table 4.

Table 5
Before and After Comparisons, Sorting and Bargaining.

	Bargaining			Sorting		
	Before	After	Difference	Before	After	Difference
	(2005–2009)	(2010–2013)		(2005–2009)	(2010–2013)	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: All workers</i>	0.010	0.019	0.009	0.068	0.085	0.017
<i>Panel B: Firm size</i>						
<10 (untreated)	-0.024	-0.003	0.021	-0.018	0.001	0.020
10–49 (treated)	-0.015	0.007	0.022	0.006	0.019	0.013
50–199 (treated)	0.004	0.014	0.010	0.052	0.075	0.024
200+ (treated)	0.026	0.030	0.004	0.107	0.129	0.022
<i>Panel C: Dif-in-dif</i>						
10–49	0.009	0.010	0.001	0.024	0.018	-0.006
50–199	0.028	0.017	-0.011	0.070	0.074	0.004
200+	0.049	0.033	-0.017	0.125	0.128	0.003

Notes: This table shows a simple comparison of the bargaining and sorting channels before and after the reform. Bargaining is calculated using female employment and sorting is calculated using male firm fixed effects. Columns (1) and (2) show the before and after results for the bargaining power channel, and column (3) shows the difference between columns (2) minus (1). Columns (4) and (5) do the same but for the sorting channel and column (6) express the difference between them. Panel A uses all workers, Panel B compares different firm sizes, and Panel C shows simple difference-in-differences estimates using firms with fewer than 10 workers as a control group. Column (3) and Panel C show a simple DID estimator in which we compare the bargaining power channel of treated firms of different sizes with that of firms with fewer than 10 workers before and after the law goes into effect. Column (6) and Panel C show the same results for the sorting channel.

Table 6
DID Effects of the Equal Pay Law on the Probability of Moving to a Firm in a Higher/Lower Fixed-Effect Quartile.

	Probability of moving up		Probability of moving down	
	(1)	(2)	(3)	(4)
<i>post</i> × <i>female</i>	0.003*** (0.001)	0.001 (0.001)	-0.001 (0.002)	0.000 (0.001)
Observations	59,663,482	59,662,627	59,663,482	59,662,627
R-squared	0.005	0.044	0.010	0.057
Sector FE	Yes	Yes	Yes	Yes
Worker FE	No	Yes	No	Yes
Baseline Probability	0.034	0.034	0.032	0.032

Notes: This table shows average marginal effects from linear probability models where the dependent variable is a dummy variable equal to one if a worker moves to a firm in a higher (columns 1 and 2) or lower (columns 3 and 4) fixed-effect quartile. Clustered standard errors at the firm level are in parenthesis. *** p<0.001, ** p<0.01, * p<0.05.

Interestingly, the results in bargaining power come from an increase in this channel for small firms of 2 pp and a slower growth for larger firms, 0.4 pp for those with 200 or more workers (panel B). This result is consistent with findings in the literature on transparency and the gender gap. For example, [Bennedsen et al. \(2019\)](#) analyzed the effect of a legislation change in Denmark in 2006 that required firms to disclose gender-disaggregated wage statistics. They found that the law reduced the gender pay gap by 2 pp, and it manifested through slower wage growth for males.²⁰

[Table 6](#) shows the results of a linear probability model for the probability of moving to a firm in a higher/lower quartile of the firm premium distribution, similar to the approach of [Casarico and Lattanzio \(2019\)](#). Firm premiums are the firm fixed effects estimated in the dual-connected set ([Table 2](#)). The covariates vector includes the post dummy, $post_t$; a female dummy and its interaction with $post_t$ (which is the parameter of interest); all of the covariates included in [Table 2](#); and economic sector fixed effects. The dependent variables are two dummies equal to one if a worker moves to a firm in a higher/lower quartile of firm fixed ef-

fects. The estimating sample is the dual-connected sample described in [Table 1](#). Columns (1) and (2) show the effects of the law on the probability of moving to a firm in a higher premium quartile, and columns (3) and (4) show the effects on the probability of moving to a firm in a lower premium quartile. As shown, there is no evidence of changes in the sorting pattern after the law for females relative to males once we control for worker fixed effects.

Hence, our results suggest a small, but significant, effect of the EPL on bargaining power effects, especially for firms with 200 and more workers; these results are consistent using our DID strategy in [Table 4](#) and the before-after comparisons in [Table 5](#). We believe that the mechanism through which the law manifests is by limiting employers' bargaining power through a deterrence channel ([Becker, 1968; Stigler, 1970](#)). This dissuasion effect might be more important for firms with 200 and more workers since they face significant monetary penalties in case of violation, as discussed in [Section 2](#). However, as shown in [Section 5.3](#), bargaining power explains a small proportion of firms' contribution to the pay gap (up to 3.4 pp in firms with 200 and more workers), which could explain the limited effect of the EPL on the firm wage premium gap.

²⁰ See also [Baker et al. \(2019\)](#), who provided evidence for the effect of public sector salary disclosure laws on university faculty wages in Canada.

Table 7
DID Effects of Placebo Reform.

Δ_G Firm Effects	(1)	(2)	(3)	(4)
post	0.021** (0.008)	0.018** (0.007)	0.020** (0.008)	0.017** (0.007)
post $\times$ d10	0.003 (0.008)	0.003 (0.008)		
post $\times$ d10/49			-0.000 (0.009)	0.002 (0.008)
post $\times$ d50/199			0.003 (0.009)	0.004 (0.008)
post $\times$ d200			0.005 (0.009)	0.005 (0.008)
N	42,632	42,632	42,632	42,632
R ²	0.009	0.007	0.0091	0.007
Firm fixed effects	Yes	Yes	Yes	Yes
Firm size weights	Yes	No	Yes	No
Female worker weights	No	Yes	No	Yes

Notes: This table shows results from the difference-in-differences analysis of the effect of a 2007 placebo reform on gender differentials in firm-specific wage premiums described in [subsection 4.3](#). The dependent variable is Δ_G Firm Effects = $\hat{\psi}_{jt}^M - \hat{\psi}_{jt}^F$. Columns 1 and 2 show the results from the specification in [Eq. 4](#) with different weighting schemes. Columns 3 and 4 show the results from the specification in [Eq. 5](#). As discussed in [Section 4.3](#), Columns 2 and 4 can be interpreted as the effect of the EPL on the bargaining power channel. Clustered standard errors at the firm level are in parentheses. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

5.5. Placebo reform and other checks

The DID results presented above should be treated with caution. In particular, any DID approach suffers from threats to the identification assumptions (for example, that the only change affecting “treated” firms was the EPL and parallel trends in $(\hat{\psi}_{J(i,t)}^M - \hat{\psi}_{J(i,t)}^F)$).

Hence, we first implement a placebo reform analysis using November 2007 as the law enactment date (two years before the actual enactment date) and restrict the sample to the 2005–2009 period; thus, we split the pretreatment period into 2 subperiods: 2005–2007 (before) and 2007–2009 (“placebo” after). Then, we estimate [Eq. \(1\)](#) before and after the placebo reform date and construct a panel dataset of treated and untreated firms, in the same way that we did in [Section 5.4](#). The number of doubly connected firms in this analysis decreases to 21,316. [Table 7](#) shows the results of [Eqs. \(4\) and \(5\)](#). As shown, there are no significant effects of this placebo reform on the contribution of firms to the gender pay gap.

In appendix D, we present an analysis of parallel pretrends. [Appendix Fig. D1](#) depicts the evolution of the gender pay gap by year for firms with different numbers of workers. As shown, the evolution of the average gender pay gap for each firm group is relatively stable before the EPL. [Appendix Fig. D2](#) plots the firm contribution to the gap, i.e. $\hat{\psi}_{J(i,t)}^M - \hat{\psi}_{J(i,t)}^F$, by year for each firm group defined by size. To do so, we estimate [Eq. \(1\)](#) for each year and subtract male and female firm fixed effects. As shown, the estimated firm contribution to the gap, $\hat{\psi}_{J(i,t)}^M - \hat{\psi}_{J(i,t)}^F$, moves parallel before the law.

Finally, since firm size, which determines the treatment status, is a choice variable, one threat to our identification strategy might occur if firms respond to the policy by changing their size. Hence, we compute histograms by firm group, defined by their numbers of workers, to verify that the firm size distribution does not change because of the policy. [Appendix Fig. D3](#) shows the distribution of firms by the average number of workers from 2009 to 2012. As shown, there are no changes, either before or after, and there is no bunching around the relevant firm size of 10. We do not show the other cutoffs (50 and 200) since most firms have fewer than 50 workers, and there is no action either.

6. Conclusions

We study the firm-specific determinants of the gender pay gap using a rich dataset of matched employer-employee data from Chile. We estimate two-way fixed-effects models and implement recent methodologies ([Bruns, 2019](#); [Card et al., 2016](#); [Casarico and Lattanzio, 2019](#)) that allows us to decompose firms’ contribution to the gender pay gap into two channels: bargaining power and the sorting effect. We use the CCK framework to assess the effects of the EPL (which combines fines for violators that increase with firm size and a disclosure requirement for firms with 200 or more workers) on firm wage premiums.

The main results indicate that firms’ total contribution explains 39% of the gender pay gap. Sorting effects account for 82% to 92% of the firm effects (32 to 36 pp), leaving little room for the EPL to operate since it would typically aim to affect the bargaining power channel to alter the gender pay gap. However, in firms with 200 or more workers, the bargaining power channel accounts for up to 20% (3.4 pp) of the firm’s total contribution.

We estimate the models before and after the enactment of the EPL to assess whether this type of initiative can affect firms’ contributions to the gender pay gap. Given that the law excluded firms with fewer than 10 workers, we implement a DID framework using small firms as a control group. We find a reduction of 1.5 pp (6.1%) in firms’ contribution to the gender pay gap in treated firms, with some heterogeneous results by treatment intensity. Firms with 200 or more workers react substantially more than those facing smaller fines with reductions of 1.9 to 2.5 pp (7.8 to 10.2%). The effect is driven by the bargaining power channel only. No effects on sorting patterns were found.

Appendix A. Wage Setting Model

This section closely follows [Card et al. \(2013\)](#) and [Card et al. \(2016\)](#). This model presents the wages for workers in multiple periods. The model allows us to observe worker i in period t in firm $j = J(i, t)$. Additionally, we observe the gender of worker i , denoted by $G(i) \in \{F, M\}$.

Similar to a wage model, the worker earns a salary $w_{iJ(i,t)t}$ in each period that is equal to the outside option (a_{it}) available to worker i in period t , plus a gender-specific share ($\gamma^{G(i)} \in [0, 1]$) of the surplus ($S_{iJ(i,t)}$) generated by the job match between worker i and firm j in period t .

$$w_{iJ(i,t)t} = a_{it} + \gamma^{G(i)} S_{iJ(i,t)t}. \quad (\text{A.1})$$

The literature argues that $\gamma^F < \gamma^M$, meaning that women obtain a lower proportion of a firm’s surplus than their male colleagues. This phenomenon is a key issue to test with data. The surplus $S_{iJ(i,t)}$ has three components:

$$S_{iJ(i,t)t} = \bar{S}_{J(i,t)t} + \phi_{J(i,t)t} + m_{iJ(i,t)t}. \quad (\text{A.2})$$

The first component $\bar{S}_{J(i,t)t}$ represents time-invariant factors. The second component, $\phi_{J(i,t)t}$, captures time-varying factors that change the surplus for the employees. The final term, $m_{iJ(i,t)t}$, is an individual worker’s portion of the surplus according to his or her particular abilities or characteristics.

The outside option for the worker, a_{it} , is composed of three elements: the permanent characteristics of each employee α_i (factors invariant in time); a set of observable characteristics X_{it} , and a transitory component ε_{it} .

$$a_{it} = \alpha_i + X'_{it}\beta^{G(i)} + \varepsilon_{it}. \quad (\text{A.3})$$

These equations make up the following wage model:

$$w_{iJ(i,t)t} = \alpha_i + \psi_{J(i,t)t}^{G(i)} + X'_{it}\beta^{G(i)} + r_{it}, \quad (\text{A.4})$$

where $\psi_{J(i,t)t}^{G(i)} \equiv \gamma^{G(i)} \bar{S}_{J(i,t)t}$, and $r_{it} \equiv \gamma^{G(i)} (\phi_{J(i,t)t} + m_{iJ(i,t)t}) + \varepsilon_{it}$ is a composite error. This model employs a two-way fixed effect estimation including workers and firms, which allows us to analyze the difference between firm effects on females and firm effects on males.

Table B1
AKM and Limited Mobility Bias Corrections.

	Males (1)	Females (2)
<i>Panel A: Kline et al.(2020) Leave-out approach:</i>		
Number of movers	644,159	323,330
Number of firms	109,073	67,008
Number of person-month observations	40,009,873	17,836,020
Correlation of Firm, Person Effects:		
Baseline correlation (biased)	0.30	0.35
The leave-out correlation (bias-corrected)	0.31	0.37
<i>Panel B: Bonhomme et al.(2019) Firm Clustering Approach:</i>		
Number of movers	724,698	400,740
Number of clusters	60	60
Number of person-month observations	41,671,748	20,355,157
Correlation of Firm, Person Effects:		
Correlation of Firm, Person Effects (biased)	0.29	0.30
Correlation of Firm, Person Effects (Bonhomme et al.)	0.26	0.29

Notes: Panel A shows summary results for Kline et al. (2020) leave-out approach and compare biased and bias-corrected estimates of firm and person correlation. Panel B shows the Bonhomme et al. (2019) firm clustering approach using the k-means clustering algorithm with k=60.

Appendix B. Mobility bias and exogenous mobility

B1. Mobility bias

Estimation of AKM models are subject to the *mobility bias critique* (Andrews et al., 2008)

However, there is evidence that these correlations can be substantially downwardly biased if the mobility of workers across firms is limited (Andrews et al., 2008). This issue occurs when firms are weakly connected by workers mobility, which translates in a downwardly bias correlation between estimated worker and firm effects.

In this section, we implement two strategies to assess whether we face a the limited mobility bias problem with our data. First, we use the approach of Kline et al. (2020) that address this issue by a leave-one out estimation of the variance components in the AKM model and compare uncorrected and bias corrected estimates of the worker and firm effects correlation. Second, we implement an alternative earnings decomposition based on a firm clustering approach developed by Bonhomme et al. (2020).²¹

In panel A of Table B1 we present a summary of the results of the AKM models on the leave one-out connected set using KSS routine. As can be seen, the difference between the plug-in (biased) and the bias-corrected estimates is negligible. For the male sample, the variance of the firm and person effects are the same in both approaches and, more specially, the correlation between person and firm fixed effects is 0.30 and 0.31. For females, the results are very similar as well with the correlation between person and firm fixed effects equal to 0.35 with the plug-in method and 0.37 with the bias corrected estimates.

In panel B we can see the results implementing the Bonhomme et al. (2020) approach using the k-means clustering algorithm with k=60. We observe that the correlation is 0.27 and 0.30 for males and females, respectively. In comparison with our main model, these correlations are 0.30 for males and females so they very close. Our results are consistent with those of Huneeus et al. (2021), who find that limited mobility bias in this administrative data is negligible. Furthermore, this is aligned with Lachowska et al. (2020) that show that mobility bias decreases with the number of periods of the sample. As we mention in Section 3, our sample is a monthly panel with

a relatively large span (96 periods) from November 2005 to October 2013. This allows us to hypothesize that our estimation should not change with a bias correction.

Note that the estimating samples are not the same. When implementing B1 the estimating sample is the leave-out connected set. As the authors explain, “this is the largest connected set of firms that remains connected after any worker from the associated graph is removed.” Now, when implementing Bonhomme et al. (2020), the estimating sample is the largest connected set by workers mobility among clusters, not firms. Hence, the sample size with Bonhomme et al. (2020)’s approach is larger than the leave-out one in B1.

B2. Exogenous mobility

This section presents descriptive evidence to analyze and test facts that are consistent with Eq. (1) and the exogenous mobility condition (2).

Figs. B1 and B2 plot the wage trajectory for men and women before and after moving from firms in the lowest wage quartile (1st) and for those who leave firms in the highest wage quartile (4th). There are many important features in these figures. First, workers who move between firms experience changes in their salaries, which suggests that there is a significant firm-specific pay premium component in the salaries of both genders. Second, there are no wage shocks within the origin firm for movers from a higher or a lower wage, for example, from 4th to 1st or from 4th to 4th (Figs. B1 and B2 show parallel lines). Third, wage changes for job changers who remain in the same quartile are all relatively small (an average regression-adjusted wage change for movers using the coefficients from a model of wage changes fit to the sample of those who remain in the same job over a given four-year interval does not show a significant difference). Fourth, movements from the lowest to the upper quartile are symmetric to those from the highest to the lowest quartile. Additionally, there is no “Ashenfelter’s dip” before the job switch.

Finally, Figs. B3 and B4 show residual plots for models for male and female workers, respectively. The figures plot the mean residuals of the estimated models in Table 2 for 100 cells, classified by the decile of worker fixed effects and by the decile of firm fixed effects. The residuals are negligible for both genders in all cells. The average residuals in absolute value are less than 0.03, which suggests that the log-linear specification of Eq. (1) provides a good approximation of the wage-determination process.

²¹ Bonhomme et al. (2020) suggest that mobility bias appears when there are many firms and few movers, they approach consists on reducing the dimensionality of the problem by grouping firms in few clusters based on the similarity of their pay distributions.

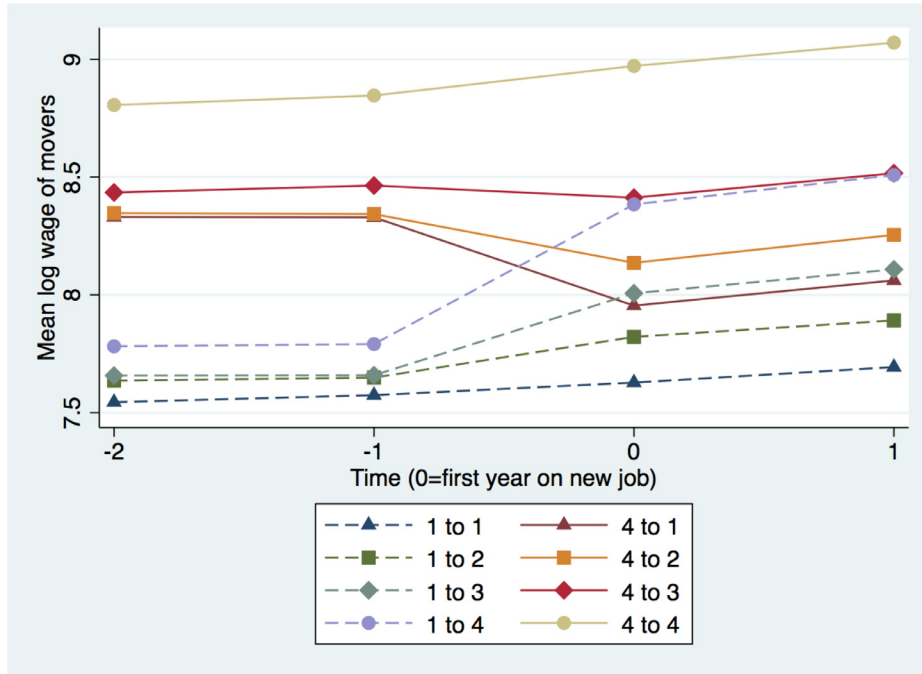


Fig. B1. Mean Wages of Male Job Changers by Quartile at Origin and Destination Firm Notes: This figure shows the mean log wage of male job changers by quartile at the origin and destination firms in 2005–2013. Quartiles are computed using coworkers wages at the origin firm (leave-one-out strategy) and at the destination firm.

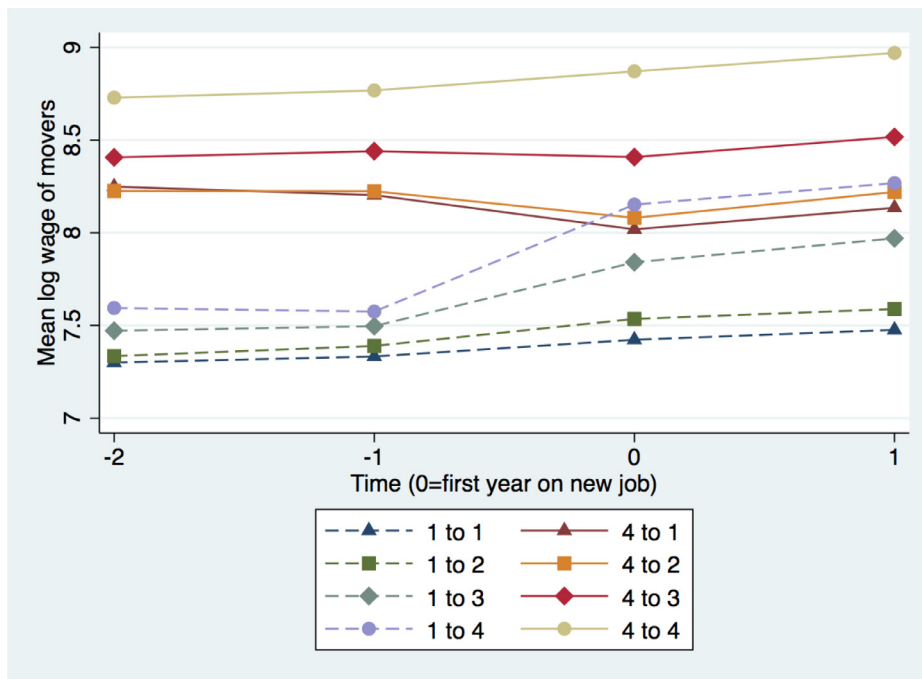


Fig. B2. Mean Wages of Female Job Changers by Quartile at Origin and Destination Firm Notes: This figure shows the mean log wage of female job changers by quartile at the origin and destination firms in 2005–2013. Quartiles are computed using coworkers wages at the origin firm (leave-one-out strategy) and at the destination firm.

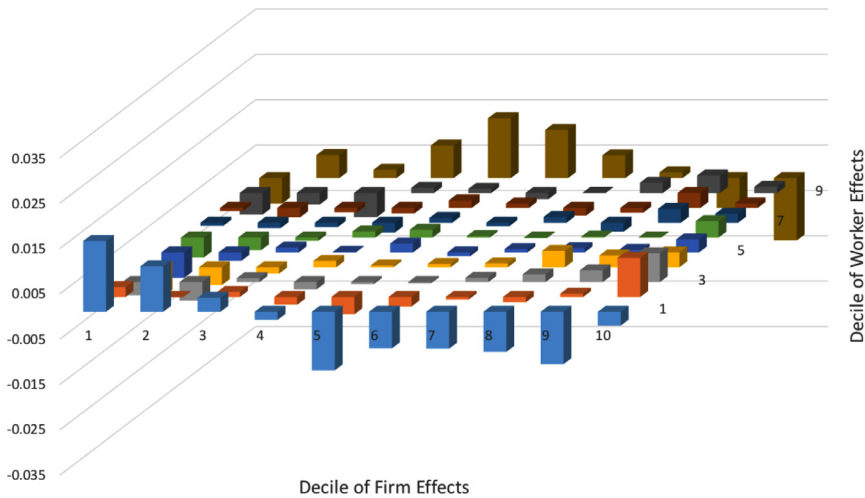


Fig. B3. Mean Residuals for Males by Decile of Worker and Firm Effects *Notes:* Notes: This figure plots mean residuals from the log wage model for male workers (column 1 of Table 2), grouped by decile of estimated individual fixed effects and decile of estimated firm fixed effects.

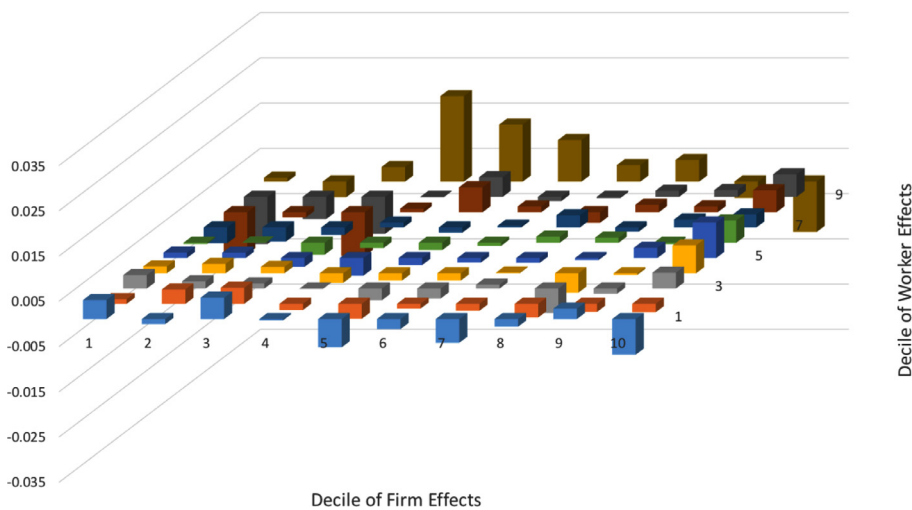


Fig. B4. Mean Residuals for Males by Decile of Worker and Firm Effects *Notes:* Notes: This figure plots mean residuals from the log wage model for female workers (column 2 of Table 2), grouped by decile of estimated individual fixed effects and decile of estimated firm fixed effects.

Appendix C. Alternative Normalization and other results

As noted by Abowd et al. (2002) and Card et al. (2016), the two-way fixed effects (Eq. (1)) are identified within a “connected set” of firms that are linked by worker mobility. Since there is more than one connected set, we estimate the models in the largest connected set separately for men and women. As in any fixed effect model, the fixed effects are relative to a base category (which is typically introduced as a linear restriction). Hence, the wage premium for any firm will be relative to a base category of the reference firm.

Another feature of the estimated fixed effects in our unrestricted linear models is that there will be positive and negative firm fixed effects.

However, in the model presented, firm effects are non-negative. For this reason, normalization was performed for a set of “low-surplus” firms to 0.

In the normalization of the fixed effects, a “control industry” was used in which the surplus captured by workers is 0. In this paper, we use firms in the food industry as a control. Card et al. (2016) used the hotel and restaurant sector to this end (in one of the specifications). An alternative industry used in the literature is hotels and accommodations. In Table C1, we present the results of the Oaxaca-style decomposition with this alternative normalization. The results are in line with those presented in Table 3.

Table C1
Contribution of Firm-Specific Pay Premiums to the Gender Wage Gap: Alternative Normalization

	Gender Wage Gap (1)	Means of Firm Premium:		Total Contribution of Firm Components (4)	Decomposition of Contribution of Firm Component:			
		Male Premium Among Men (2)	Female Premium Among Women (3)		Sorting		Bargaining	
					Using Male Effects (5)	Using Female Effects (6)	Using Male Distribution (7)	Using Female Distribution (8)
All	0.245	0.187	0.099	0.088 (36.0)	0.071 (29.1)	0.077 (31.5)	0.011 (4.4)	0.017 (6.8)
By Age Group:								
Up to Age 30	0.129	0.164	0.094	0.070 (54.1)	0.053 (41.2)	0.062 (47.5)	0.009 (6.6)	0.017 (13.0)
Age 31–40	0.248	0.212	0.116	0.097 (39.1)	0.077 (31.2)	0.081 (32.7)	0.016 (6.3)	0.020 (7.9)
Over Age 40	0.352	0.187	0.088	0.099 (28.2)	0.085 (24.3)	0.091 (25.8)	0.008 (2.4)	0.014 (3.9)
By Education Group:								
Less than High School	0.303	0.165	0.073	0.092 (30.5)	0.079 (26.1)	0.089 (29.4)	0.003 (1.1)	0.014 (4.5)
High School	0.283	0.197	0.098	0.099 (34.9)	0.082 (29.0)	0.084 (29.9)	0.014 (5.1)	0.017 (5.9)
University	0.325	0.276	0.176	0.100 (31.0)	0.074 (22.6)	0.066 (20.3)	0.035 (10.7)	0.027 (8.3)
No. of workers at the firm:								
Fewer than 10	0.082	0.106	0.105	0.001 (1.1)	0.003 (4.2)	0.024 (28.6)	-0.023 (-27.6)	-0.003 (-3.1)
10+ and fewer than 50	0.091	0.134	0.117	0.018 (19.3)	0.012 (13.1)	0.027 (30.0)	-0.010 (-10.8)	0.006 (6.22)
50+ and fewer than 200	0.247	0.190	0.105	0.085 (34.6)	0.069 (27.8)	0.071 (28.9)	0.014 (5.7)	0.017 (6.8)
More than 200	0.362	0.235	0.088	0.147 (40.6)	0.121 (33.3)	0.118 (32.6)	0.029 (8.0)	0.027 (7.3)

Notes: Estimating samples include male and female workers in a dual connected set. Column 1 shows the gender wage gap, as measured by the difference in mean log wages between males and females, estimated over all workers in the subset indicated by the row title. Estimated firm effects are from the models described in columns 1 and 2 of Table 2. Columns 2 and 3 report the firm wage premiums for males and females, respectively. Column 4 computes the total contribution of firm wage premiums to the gender wage gap expressed in column 1. Columns 5 through 8 show the contributions of the sorting effect and the bargaining effect to the gender wage gap, computed using the CCK decomposition as described in the text. The percentages of the overall male-female wage gap (column 1) are in parentheses.

Table C2
Contribution of Firm-Specific Pay Premiums to the Gender Wage Gap by Economic Sector .

	Gender Wage Gap (1)	Means of Firm Premium:		Total Contribution of Firm Components (4)	Decomposition of Contribution of Firm Component:			
		Male Premium Among Men (2)	Female Premium Among Women (3)		Sorting		Bargaining	
					Using Male Effects (5)	Using Female Effects (6)	Using Male Distribution (7)	Using Female Distribution (8)
All	0.245	0.251	0.155	0.096 (39.0)	0.078 (32.0)	0.088 (36.0)	0.007 (3.0)	0.017 (7.0)
By Economic Sector:								
Agriculture	0.254	0.096	0.066	0.030 (11.8)	0.038 (15.0)	0.040 (15.9)	-0.010 (-4.1)	-0.008 (-3.2)
Fishing	0.320	0.402	0.344	0.058 (18.1)	0.038 (11.7)	0.044 (13.7)	0.014 (4.4)	0.020 (6.4)
Mining	0.481	0.716	0.434	0.280 (58.4)	0.216 (44.9)	0.193 (40.1)	0.088 (18.3)	0.065 (13.5)
Manufacturing	0.327	0.300	0.192	0.108 (33.1)	0.098 (29.9)	0.102 (31.3)	0.006 (1.8)	0.011 (3.2)
Construction	0.164	0.293	0.226	0.067 (41.2)	0.082 (50.4)	0.082 (50.4)	-0.015 (-9.2)	-0.015 (-9.2)
Commerce	0.299	0.224	0.136	0.088 (29.4)	0.074 (24.7)	0.079 (26.4)	0.009 (3.0)	0.014 (4.6)
Hotels & Restaurants	0.210	0.125	0.090	0.035 (16.6)	0.005 (2.4)	0.030 (14.5)	0.004 (2.1)	0.030 (14.2)
Finance	0.092	0.368	0.312	0.056 (60.3)	0.007 (7.3)	0.007 (7.7)	0.048 (52.7)	0.049 (53.1)
Education	0.091	0.207	0.221	-0.014 (-15.6)	-0.005 (-5.1)	0.001 (0.6)	-0.014 (-16.2)	-0.010 (-10.6)
Services	0.148	0.207	0.177	0.030 (19.8)	0.010 (6.8)	0.013 (8.8)	0.016 (11.0)	0.019 (13.0)

Notes: Estimating samples include male and female workers in a dual connected set. Column 1 shows the gender wage gap, as measured by the difference in mean log wages between males and females estimated over all workers in the subset indicated by the row title. Estimated firm effects are from models described in columns 1 and 2 of Table 2. Columns 2 and 3 report the firm wage premiums for males and females, respectively. Column 4 computes the total contribution of firm wage premiums to the gender wage gap expressed in column 1. Columns 5–8 show the contributions of the sorting effect and the bargaining effect to the gender wage gap, computed using the CCK decomposition described in the text. The percentages of the overall male-female wage gap (column 1) are in parentheses.

Appendix D. Alternative specifications for the DID and other checks

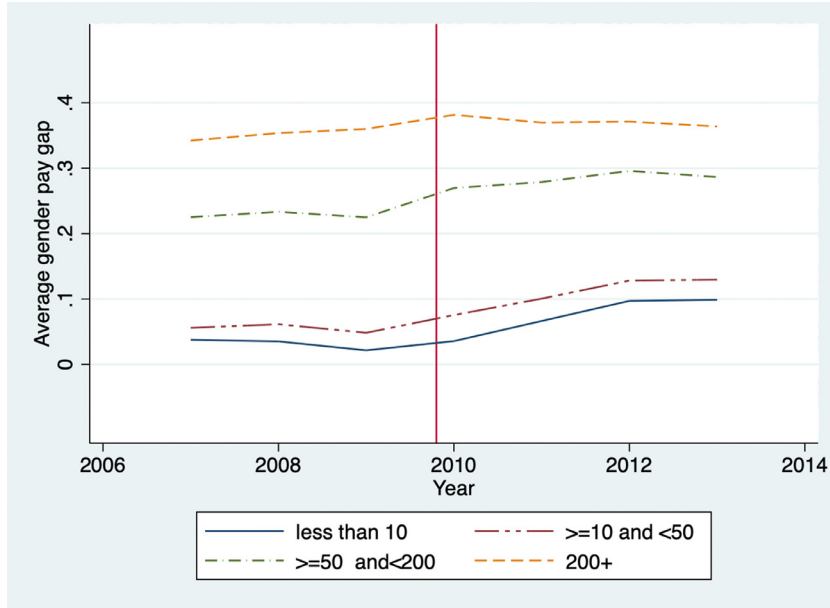


Fig. D1. Gender pay gap by firm number of workers *Notes:* This figure plots the raw wage gap evolution for treated firms (between 10 and 49, between 50 and 199, and 200 and more workers) and untreated firms (less than 10 workers) for 2005–2013.

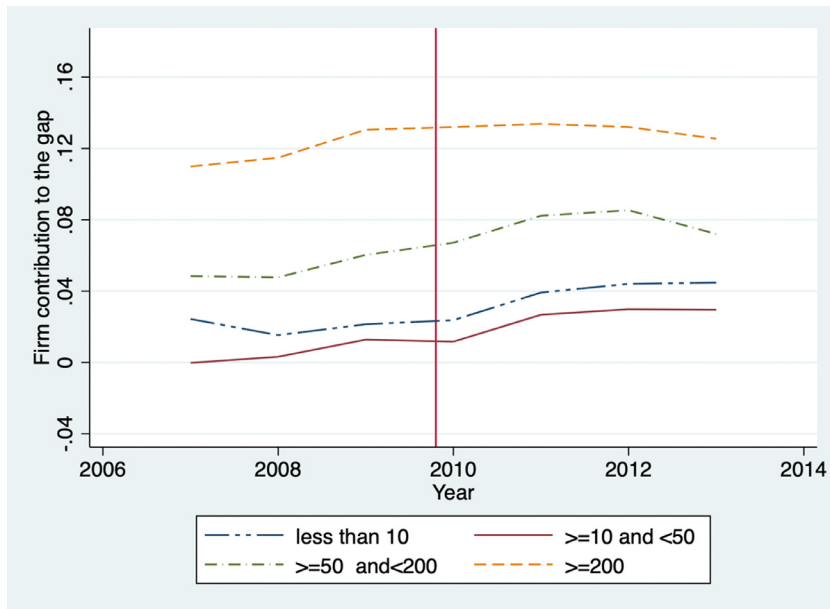
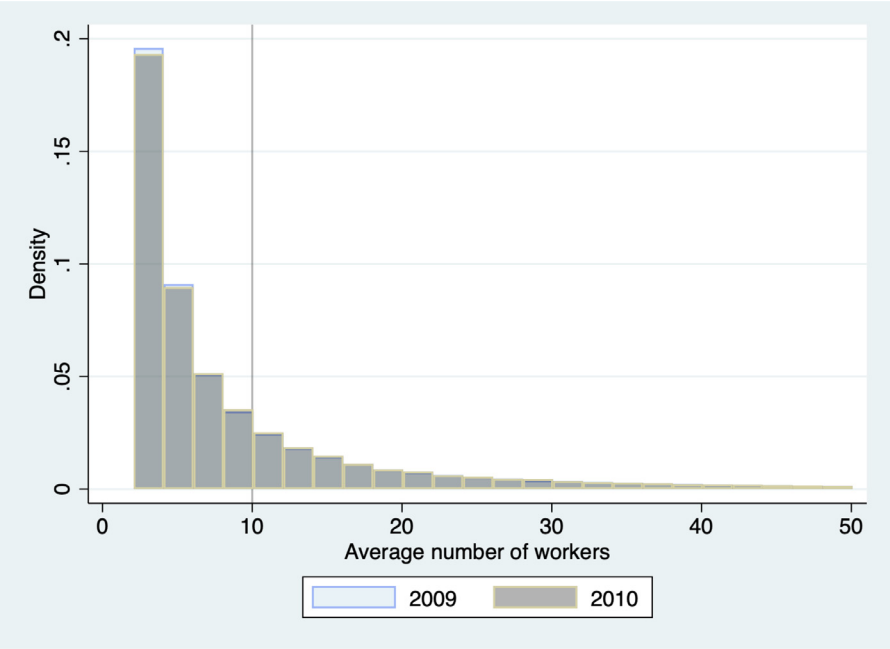
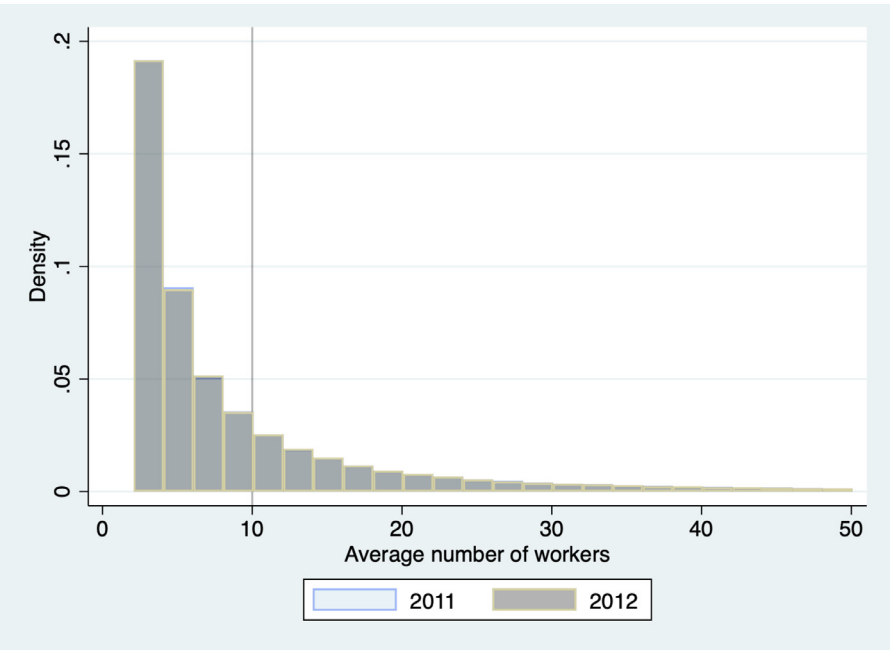


Fig. D2. Firm contribution to the gap by firm number of workers *Notes:* This figure plots the average firm contribution to the gap for treated firms (between 10 and 49, between 50 and 199, and 200 and more workers) and untreated firms (less than 10 workers) for 2005–2013.

Fig. D3. Firm size distribution between 2009 and 2012.



(a) Before and the year of EPL



(b) After the EPL

Table D.1

DID effects of the Equal Pay Law, Alternative Normalization.

Δ_G Firm Effects	(1)	(2)	(3)	(4)
<i>post</i>	0.020*** (0.004)	0.019*** (0.003)	0.020*** (0.004)	0.019*** (0.003)
<i>post</i> $\times$ <i>d10</i>	-0.009* (0.004)	-0.012** (0.004)		
<i>post</i> $\times$ <i>d10/49</i>			-0.003 (0.004)	-0.005 (0.004)
<i>post</i> $\times$ <i>d50/199</i>			-0.009 (0.005)	-0.014*** (0.004)
<i>post</i> $\times$ <i>d200</i>			-0.011* (0.005)	-0.016*** (0.004)
N	63,866	63,866	63,866	63,866
R ²	0.004	0.003	0.004	0.003
Firm fixed effects	Yes	Yes	Yes	Yes
Firm size weights	Yes	No	Yes	No
Female workers weights	No	Yes	No	Yes

Notes: This table shows results from the difference-in-differences analysis of the effect of Chile's 2009 EPL on gender differentials in firm-specific wage premiums as described in [subsection 4.3](#). The dependent variable is Δ_G Firm Effects = $\hat{\psi}_{jt}^M - \hat{\psi}_{jt}^F$. Columns 1 and 2 show the results from the specification in [Eq. 4](#) with different weighting schemes. Columns 3 and 4 show the results from the specification in [Eq. 5](#). As discussed in [Section 4.3](#), Columns 2 and 4 can be interpreted as the effect of the EPL on the bargaining power channel. Clustered standard errors at the firm level are in parentheses. *** p<0.001, ** p<0.01, * p<0.05.

Table D2

DID Effects of the Equal Pay Law, Male Weights.

Δ_G Firm Effects	(1)	(2)	(3)	(4)
<i>post</i>	0.019*** (0.004)	0.022*** (0.004)	0.019*** (0.004)	0.022*** (0.004)
<i>post</i> $\times$ <i>d10</i>	-0.015** (0.005)	-0.013** (0.005)		
<i>post</i> $\times$ <i>d10/49</i>			-0.006 (0.005)	-0.008 (0.005)
<i>post</i> $\times$ <i>d50/199</i>			-0.014** (0.005)	-0.016** (0.006)
<i>post</i> $\times$ <i>d200</i>			-0.019*** (0.005)	-0.015* (0.006)
N	63,866	63,866	63,866	63,866
R ²	0.0008	0.0020	0.0014	0.0022
Firm fixed effects	Yes	Yes	Yes	Yes
Firm size weights	Yes	No	Yes	No
Male workers weights	No	Yes	No	Yes

Notes: This table shows results from the difference-in-differences analysis of the effect of a 2007 placebo reform on gender differentials in firm-specific wage premiums described in [subsection 4.3](#). The dependent variable is Δ_G Firm Effects = $\hat{\psi}_{jt}^M - \hat{\psi}_{jt}^F$. Columns 1 and 2 show the results from the specification in [Eq. 4](#) with different weighting schemes. Columns 3 and 4 show the results from the specification in [Eq. 5](#). As discussed in [Section 4.3](#), Columns 2 and 4 can be interpreted as the effect of the EPL on the bargaining power channel. Clustered standard errors at the firm level are in parentheses. *** p<0.001, ** p<0.01, * p<0.05.

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