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THE EFFECT OF MANDATED CHILD CARE ON FEMALE WAGES IN CHILE

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ABSTRACT

This paper studies the effect of mandated employer-provided child care on the wages of women hired in large firms in Chile. We use a unique employer-employee database from the country's unemployment insurance (UI) system containing monthly information for all individuals that started a new contract between January 2005 and March 2013. We estimate the impact of the program using regression discontinuity design (RDD) exploiting the fact that child care provision is mandatory for all firms with 20 or more female workers. The results indicate that monthly starting wages of the infra-marginal woman hired in a firm with 20 or more female workers are between 9 and 20 percent below those of female workers hired by the same firm when no requirement of providing child care was imposed.

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1 Introduction

One of the main characteristics of Chile's labor market is the low labor force participation of women. According to International Labor Organization (ILO) statistics for the period 2011-2013, Chile and Mexico have the lowest participation rate of women in Latin America.¹ The rate in Chile is also below the United States and the average rate for European countries by more than ten percentage points. The situation, however, has shown improvements over the past two decades, with the rate increasing from 31.8 in 1990 to 47.8 in 2012.²

Since the return to democratic governance at the end of the 1980s, Chile has formulated several public policies to reduce gender disparities in the labor market by stimulating female hiring, reducing discrimination, and promoting work and family balance (Henriquez and Riquelme, 2011). In the early 1990s, the government created the National Women Service -Servicio Nacional de la Mujer- an institution devoted to the promotion of equal opportunities for men and women in Chile. Since then, a number of laws and programs have been enacted to support women's rights and increase female access to the labor market.

One example is Article 203 of the Labor Code, which mandates that all firms with 20 or more female workers regardless of their age, marital status, or type of contract should provide a place near but independent from the workplace where mothers can leave their children under two years old during the workday and feed them as necessary.³ The employer must offer the service in a separate facility near the workplace, pay an external provider directly, or provide additional compensation to the female employee to cover the expense. The government must approve all child care centers through its National Board of Daycare Centers (the Junta Nacional de Jardines Infantiles, or JUNJI). This law supports the mothers transition back to work, while promoting the close motherchild relationship and the healthy development of the child. It is well know that problems with securing and affording child care can create conflict in the workfamily balance, and to the extent that this can lead to lost work days, the choice to give up paid work temporarily, or

¹LABORSTA database downloaded on December 1st 2014. We compared female labor force participation rates in Chile with those in the following countries: Argentina, Bolivia, Brazil, Colombia, Jamaica, Mexico, Panama, Paraguay, Peru, Uruguay, and Venezuela.

²According to the statistics presented in KILM/ILO 8th Edition, which are based on LABORSTA until 1995 and on OECD Labour force Statistics Database from then onward.

³The first law regulating the provision of child care to employees dates back to 1917 (Law 3186). Since then the law has been subject to several modifications (1993, 1995, 1998, 2002, 2007 and 2009), which are reflected in the current version of Article 203.

job loss, child care is implicated in reduced wages for mothers.

Aside from the potential positive impact of the policy on labor supply decisions, the immediate effect of the law is an increase in the cost associated with hiring and employing women. In fact, it creates a wedge between the labor costs of females and males, implying a new source of gender disparities. Standard economic theory suggests that the labor market effect of mandated legislation will be concentrated in employment with no change in wages. However, some effect on wages may be expected given the importance of child care in shaping employment decision of females.

The objective of this paper is to quantify the effect of the mandated employer-provided child care on the starting wages of women hired in large firms in Chile. The focus is on large firms because for small and medium firms compliance with the law is not high enough to obtain an accurate estimation of the wage adjustments made in response to the law. In particular, we concentrate on the wages of new female employees because the wage adjustment is more flexible in this case than for active workers.

We use a unique employer-employee database from the unemployment insurance (UI) system containing monthly information for all individuals that started a new contract between October 2002 and March 2013. We exploit the availability of longitudinal data to analyze the impact of the law on firm-specific new hires, those leading firms to cross the threshold of 20 female workers. Our results indicate that the policy has sizable effects on starting wages of women working in large firms created after 2005. Specifically, women hired in a firm with 20 or more female workers make CLP\$24,000 to CLP\$53,000 (approximately US\$39-US\$87) less than women hired when no requirement of providing child care was imposed (i.e., when firm has less than 20 female workers).⁴

The paper contributes to three strands of the literature. First, it informs on the effects of labor market regulations on wages, more precisely, on the effects of group-specific mandated benefits on group's wages. Identifying this pass-through is critical for understanding the efficacy of the policy. Second, the paper contributes to the debate on the unintended consequences of public policies in general and policies aimed to promote gender equality in particular. Third, the paper adds to the literature that analyzes the impact of non-linearities in legislation and their impact on labor market flexibility, especially those that have dynamic implications.

⁴Values are expressed in 2009 Chilean pesos. Values in dollars computed using the exchange rate in effect on April 2015 of 609.89 Chilean pesos per US dollar.

This paper offers a unique contribution to the recent analysis of the effect of mandated child care benefits in Chile. By using longitudinal data, it provides a precise estimate of the effect of the law on wages by following firms as they grow over time and compare individual wages within a firm before and after crossing the threshold. We discuss how the trivial comparison of firms above and below the threshold at a given point in time ignores fundamental differences across firms, which may bias the estimates in unpredictable ways.

The document is organized into seven parts. The second section summarizes the literature of the effects of mandatory legislation on the labor market. The third section describes the data used. The fourth section presents the conceptual framework. The fifth section explains in detail the empirical strategy of the paper. The sixth section presents the main results. Section seven concludes.

2 Effect of Mandatory Legislation on the Labor Market

Under the standard neoclassical labor demand-labor supply framework, mandatory legislation that increases labor costs reduces employment with no change in wages. This result is explained by the assumption of a perfectly elastic labor supply. Then, the cost is entirely paid by the employer. In the context of a not perfectly elastic supply of labor, however, part of the increase in labor costs will reduce wages, and thus, the effect on the labor market will be limited. In addition, the employee valuation of the benefit is important to determine its effect on wages and employment, since it determines whether workers accept lower wages when receiving mandated benefits.

Summers (1989) points out that “in terms of their allocational effects on employment, mandated benefits represent a tax at a rate equal to the difference between the employers cost of providing the benefit and the employee’s valuation”. Thus, in the limit and in the absence of asymmetric information between workers and firms, wage rigidity, or credit constraints, mandated benefits are borne by the workers in the form of lower wages, and have no effect on employment (e.g., Summers, 1989; Gruber and Krueger, 1991; Gruber, 1994). The extent to which the cost of mandatory legislation is translated into lower wages is an empirical question.

Several empirical papers provide evidence of a large effect of payroll taxes and mandated benefits on wages. For the United States Gruber and Krueger (1991) study the incidence of increases in the

cost of workers' compensation (referring specifically to insurance for workplace injuries) and find that 85 percent of the cost of the mandated employer-provided insurance shift to wages, with limited disemployment effect.

Similarly, Gruber (1994) analyzes the effects of mandated maternity benefits in the United States and, using a difference-in-difference estimation, finds that a large share of the cost is shifted to wages with only minor disemployment effects. In fact, Gruber finds that legislation causes young women's wages to fall by as much as 5 percent with no effect on their labor supply, suggesting a 100 percent pass-through rate.⁵

In the context of Latin America, most of the evidence uses the dramatic shifts in policy regimes that took place during the decade of reforms that started in the late 1980s and early 1990s. Heckman and Pagés (2004) present a comprehensive summary of the evidence on wage shifts for the region. In the case of Ecuador, they highlight the work of MacIsaac and Rama (1997), documenting that part of the increase in labor cost associated with mandated contributions to social security programs is shifted to workers in the form of lower base wages (i.e., the foundation on which benefits are paid). In fact, for an average cost of social security contributions and other mandated benefits that amounts at least 57 percent of the base wage, the authors find a 39 percent reduction in the base earnings of workers in firms that comply with these regulations compared with workers at non-compliant firms. Their empirical strategy consists in including a dummy variable that identifies compliant firms into the estimation of a wage equation using the Living Standards Measurement Study (LSMS) of 1994. The regression is performed on take-home wages and base wages controlling for individual variables (education, experience, gender, marital status, dummies for indigenous, urban and other geographical variables) and characteristics of the firm (dummies for modern, public, agriculture and unionization).

Mondino and Montoya (2004) analyze the effect of labor market regulations in Argentina during the period 1975-1996. Similar to MacIsaac and Rama (1997), they compare wages of workers who have access to social security programs with wages of uncovered workers; in the estimation of the wage equation, however, they control for the difference between the decision to participate in a job

⁵Not all empirical papers find evidence of large effects on wages. Baum (2003) finds little effect of maternity leave legislation on either employment rates or wages. He uses interstate variation in legislation and the Family and Medical Leave Act (FMLA) signed in 1993 by President Clinton. His results may be due to fact that the mandated leave is short and unpaid, and many employers provided maternity leave benefits prior to the statutes.

search and that of accepting a job offer. They find that the gross wages of non-covered workers were 8 percent higher than those of covered workers.

In the case of Mexico, Marrufo (2001) examines the effects of the 1997 pension reform. After decomposing the effect of the reforms into the effect of a tax reduction and the effect of tying benefits to contributions, Marrufo finds that wages absorb 43 percent of the increase in social security taxes and 57 percent of the value of benefits. She controls for self-selection and also accounts for general equilibrium effects to overcome the problems with the difference-in-difference estimates that understate the true extent of wage adjustment.

Finally, in the case of Chile, Gruber (1997) uses the sharp exogenous reduction in employer-paid labor taxes produced by the privatization of the social security system to estimate its effect on the labor market. Gruber's estimates point to a 100 percent pass-through rate. As Heckman and Pagés (2004) point out, there are several caveats for his estimates. First, with the decline in payroll taxes, workers' contributions also increased and if measured wage payments by firms include employee contributions, then the higher measured wages will capture not only the effect of lower employer-paid taxes but also the higher employee-paid contributions. Second, measurement errors in the wage bill and tax payments may bias the estimates toward full shifting.

Using data from the 1994's national survey of socioeconomic conditions (Encuesta de Caracterización Socioeconómica Nacional-CASEN), Edwards and Cox-Edwards (2000) analyze the effect of a social security reform on labor market outcomes in Chile. The authors estimate a wage equation controlling for the decision to contribute to the social security system. They find that take-home wages for workers covered by mandatory pension, health, and life insurance were 9 percent lower than wages for non-covered workers. Using the fact that in 1994 social security contributions amounted to 20 percent of wages and were nominally paid by workers, their estimates suggest that the workers absorbed about 45 percent of the cost of the contributions in the form of lower wages, while the other 55 percent fell on employers.

As previously stated, these estimates of large pass-through rates rely on the absence of asymmetric information between workers and firms, wage rigidity, or credit constraints. While these assumptions may hold at the top of the wage distribution, they are unlikely to be true at the bottom, which implies lower shifts of the costs to wages. One important factor in the context of Latin America is the downward wage rigidity at the bottom of the distribution; thus, it is cru-

cial for the analysis to evaluate whether minimum wage binds. For example, Maloney and Nunez (2004) document that the minimum wage binds in Colombia, which is consistent with the weak pass-through effects found by Cardenas and Bernal (2004) for the country. In the case of Chile, it will be interesting to determine whether the results of Gruber (1997) were driven by the fact that the minimum wage does not bind or by the specificity of the study.

However, some of the theoretical predictions for general mandates may not hold in the case of group-specific mandates, such as the one we are considering for Chile. Gruber (1994, 1997) points out that the scope for wage adjustment might be more limited due to barriers to adjust relative wages. In addition, even with barriers for wage shifting, these mandates may discourage hiring of new employees belonging to the specific group, as employers seek to hire workers with lower benefit costs, altering the dynamics of the labor market. Thus, mandated benefit programs might work against the interests of those who most require the benefit being offered.

Two other papers analyze the effect of child care mandated benefits in Chile. Escobar (2014) uses data from the annual survey of manufacturing firms -Encuesta Nacional Industrial Anual (ENIA)- covering the period 1995-2007 to estimate the effect of Article 203 on firm's hiring decisions. In particular, he assesses the extent to which firms substitute female labor with male labor or capital to avoid the cost associated with the law. He documents the presence of concentration of firms below the threshold of 20 female workers, and no effects of the law on average wages per worker. Unfortunately, unlike the data used in this paper, ENIA does not contain worker-level data and only gathers information on a specific sector. This prevents a disaggregated dynamic analysis of wages by gender, limiting the scope of the findings regarding the wage adjustment. Our results confirm these limitations.

Villena et al. (2015) use a cross sectional sample (October 2010) from the same data analyzed in this paper to estimate the effect of the same law on wages. They focus on firms with more than 5 and less than 35 female workers and restrict the analysis to three industries. Based on a regression discontinuity approach implemented in a static framework, they find that firms shift the cost of mandated child care benefits to all workers -both females and males. The estimated local static average effect is a 4 percent reduction in wages of active workers. The small magnitude of this effect contrasts to the large effects presented in our paper. Several reasons explain these differences. First, Villena et al. (2015) does not take into account the evidence of limited compliance with

the law among small and medium size firms. Second, their static empirical analysis ignores firm dynamics. As we show, high frequency hires and dismissals changing the position of the firm with respect to the threshold are extremely common in Chile. This affects the interpretation of the static findings. In this paper, we deal with this challenge by analyzing individual hires over time at the firm level.⁶ Third, average effects obtained from cross-sectional variation across firms overlook the importance of firm heterogeneity. In contrast to the analysis of Villena et al. (2015), our dynamic set up controls for firm-specific fixed effects. To the best of our knowledge, this is the first paper addressing the impact of child care mandated benefits labor legislation taking into account both dynamic firm behavior and firm-level heterogeneity.

3 Data Description

The database from the country’s unemployment insurance (UI) system -Seguro de Cesantía- includes information on the demographic characteristics of individuals (e.g., age, gender, education, marital status) and their geographic location. It also contains information from the firms where the individuals work, such as salary history, type of contract, sector of activity, and hiring date. The size of the firm (number of workers) is computed by counting the number of registered workers in each firm. The employer collects the information and must submit it to the institution that administers the pension system in Chile (Administradora de Fondos de Cesantía, or AFC). This procedure is mandatory for all contracts that started after October 2, 2002. Workers that were already working before this date can enter voluntarily into the system. Once an individual is registered in the system, the information is updated on a monthly basis, even if the individual changes his or her employer.

Motivated by the nature of our main question and by some challenges of the data, we use a subsample of the firms that guarantees the accuracy of our estimates. We concentrate on *large and new firms*. Firms are classified as *large* if they employ 200 or more workers in a calendar year.⁷ The definition of *new* is based on the date of creation, so firms are classified as new if they registered a new contract for the first time in 2005. As a result, the period of analysis is 2005-2013. Each

⁶For example, using data from the Chilean unemployment insurance (UI) system, we calculated that 40 percent of the firms with less than 20 female workers in a given month, eventually pass the threshold within a year. Furthermore, around 14 percent of these firms pass the threshold in the next month.

⁷We compute the number of workers per firm on an annual basis.

decision is related to some features of the data, so we present the arguments for the selection of the sample as a response to overcome three main challenges.

First, the data do not contain information on the actual provision of child care; this is an important challenge because there is no perfect compliance with the law. In a situation of perfect compliance, we could rely exclusively on the number of female workers to classify firms facing the obligation to pay for child care services from those not facing such an obligation.

To overcome this challenge, we restrict the analysis to *large* firms because these firms are more likely to observe the law than small and medium firms. According to the 2011 National Labor Survey (Encla 2011), 28 percent of firms with more than 20 female workers do not comply with the law to provide child care services, a figure concentrated mainly in small and medium firms. In contrast, 90 percent of large firms provide child care services to their employees Dirección del Trabajo (2012).⁸ Also, large firms are subject to more stringent vigilance and control.

Columns 1 and 2 in Table 1 compare the sample of large firms with all the firms available in the data. Large firms represent less than 1 percent of all the firms in the data, but they are highly representative of the firms that are mandated to provide child care services to their employees.⁹ Based on the 2008 National Labor Study (Encla, 2008), large firms comprise 63 percent of all firms complying to the law (Dirección del Trabajo, 2008).¹⁰ In addition, as presented in Table 1, while only a very small fraction of the firms in the data crosses the threshold of having 20 female workers, 75 percent of large firms at some point cross the threshold between 2002 and 2013. For these firms, we observe the starting wages of all new female workers as the firm approaches the threshold. We exploit this feature of the data in our empirical approach.

[Table 1 about here.]

The second challenge of the UI data is the uncertainty about the total number of workers for some firms. Our data contain very rich information on all new contracts that started after October 2, 2002, when the law mandated that they be registered into the system. Workers with contracts before

⁸This percentage corresponds to firms that have 20 or more female workers and at least one with children at the eligible age range. The survey has detailed information on 3,153 firms 79,786 using sampling weights. The sample is representative by region and size of firm. The sample, which comes from the National Revenue Service, includes all the firms in the national territory that paid taxes in 2009. For more information, see Dirección del Trabajo (2012).

⁹70.31 percent of all the firms in the data have less than 5 workers; 13.3 percent have 5 to 9 workers; 12.8 percent have between 10 and 49 workers; and 2.7 percent have 50 and 199 workers.

¹⁰In 2008, the ENCLA had a specific module to analyze child care provision and compliance with the law.

October 2002 have not been automatically entered. They can, however, be entered voluntarily into the system. Consequently, there is no guarantee that the number of workers registered in the system coincides with the actual number of workers in each firm. To overcome the challenge created by the uncertainty in the total number of workers in general, and the total number of female workers in particular, we use only *new* firms.¹¹ We restrict the analysis to new firms because we can follow their expansion and have certainty that all workers are registered in the system and appear in the data. Finally, we compute the number of workers per firm on an annual basis. Each decision is related to some features of the data, so we present the arguments for the selection of the sample as a response to overcome three main challenges.

Columns 3 and 4 in Table 1 present basic descriptive statistics on new firms and firms that are both new and large. Our final sample uses information on new hires in this subsample of 1,912 firms. This selection of the sample reduces the external validity of our results in the sense that we are not including the complete universe of firms in Chile, but we gain confidence in the precision of our estimates.

The third challenge is the presence of large variations in the total number of workers in a firm within a year. Having monthly data is definitively an advantage for our study, but the registration process and actualization of the information do not necessarily coincide with the monthly frequency. In some cases the frequency mismatch responds to the typical timing of the registration process of registration for new employees (minimum 3 to 4 months). In other cases, it responds to the possibility of avoiding monthly actualization of the information in the system. In consequence, the number of employees recorded in the system in a given month may not coincide with the real number of workers in the firm.¹² To overcome this issue, we use the maximum number of workers observed in a year in each firm, as the variable that determines whether or not the firm is mandated to provide child care services to its employees.

Finally, since we are interested in comparing the wage of females in the margin as the firm

¹¹The sample contains 525,181 new firms, which accounts for nearly 63 percent of the 837,899 firms in the sample.

¹²For example, when a new person is hired, employers must submit his or her information to the institution that administers the pension system in Chile (Administradora de Fondos de Censatias-AFC) within 10 days. Between the actual hiring date and the date the individual appears in the system, however, there is a window of three months (date of hiring, report the new employee, first payment, creation of account, and next wage). Regarding the actualization of information, it is possible that some individuals do not appear every month in the data due to a delay in paying the monthly contributions. In theory, once an individual is registered in the system, the information is updated on a monthly basis because the contributions must be paid every month. However there is a window of 90 days before firms have to pay the penalty fee associated with the delay.

approaches the threshold of 20 female workers, we restrict the analysis to the starting wage of each woman in each firm.¹³ As a result, the outcome of interest is the wage offered to (and accepted by) a woman in a firm that is very close to having a total of 20 female workers. Ideally, we would like to analyze only the starting wage of the 20th female, since that is exactly the point of discontinuity. In reality, we are close to that ideal because the data include all new contracts that started after October 2002 and, by concentrating on new firms, we may be able to observe the hiring of the 20th female worker.

Table 2 presents the description of the data on females hired for the first time in large new firms. “N_workers” is the average of the number of workers employed in the firm hiring new female workers over the period 2005-2013; “N_female workers” is the average number of female workers employed in the firm where the individual was hired; the analogous is true for “N_male workers”; “Starting wage” for female workers is the average of the real starting wage in Chilean pesos of 2009 for the females in the sample; “Average wage females/males firm” corresponds to the average wage of female or male workers working at the firm where the female was hired; “Above” is a dummy variable for the firms with twenty or more female workers: the number of observations corresponds to individual hires and “N_firms” is the number of firms considered.

[Table 2 about here.]

4 Conceptual Framework: Firm’s Behavior at the Margin

One important consideration is the discontinuous nature of the policy. Unlike standard mandates, the increase in the labor costs associated with hiring females only activates when the firm reaches the threshold of 20 female workers. This section briefly discusses the behavior of the firm at the margin.

Assume the firm only has two inputs in production: female workers f and male workers m . The production function is described as follows:

$$Q = F(f, m; \theta)$$

¹³In this context, if one woman is hired by different firms, we include her as multiple observations of starting wages.

where θ is a parameter that summarizes all other characteristics of the production process.

Given the prices of the inputs, w_f and w_m , the price taker firm maximizes profits. The first order conditions are:

$$w_f = F_f(f, m; \theta)$$

$$w_m = F_m(f, m; \theta)$$

As a result, the optimal allocation of workers given the relative prices is given by:

$$\frac{w_m}{w_f} = \frac{F_m(f, m; \theta)}{F_f(f, m; \theta)} \quad (1)$$

If the production function exhibits constant returns to scale, equation (1) can be expressed as follows:

$$\begin{aligned} \frac{w_m}{w_f} &= \frac{F_m(m/f, 1; \theta)}{F_f(1, m/f; \theta)} \\ &= \kappa(m/f; \theta) \\ &= \kappa(R; \theta) \end{aligned}$$

In this context, the policy affects the cost of female labor, acting as a tax. The wage of females in firms with 20 or more female workers will be $\tilde{w}_f = w_f + \tau$. In consequence, the optimal selection of inputs will be given by:

$$\frac{\tilde{w}_m}{w_f} = \kappa(\tilde{R}; \theta) \quad (2)$$

A firm with 19 female workers will experience a change in relative wages. In response, the profit maximizing behavior implies adjusting the optimal fraction of male to female workers, changing from R to \tilde{R} . Without the policy, the firm would continue increasing the scale of production without changing the optimal ratio of male to female workers R .

In the process of expansion, a firm in the margin faces two options to increase the scale of production. The first alternative is to substitute female workers with male workers, to avoid the change in relative prices implied by the policy (not hiring the 20th female worker). This option implies altering the optimal combination of inputs and, as a result, it has a direct impact on employment levels and is associated with the concentration of firms just before the before the

threshold is reached. This concentration is referred in the literature as “bunching”. The viability of this option, however, depends on the elasticity of substitution between female and male labor.

The second alternative is to hire the 20th female worker and pay the additional cost associated while maintaining the optimal ratio of male to female workers. In this paper, we are interested in quantifying the effect of the policy on female wages.

5 Empirical Framework: Sharp RDD with Firm-specific Fixed Effects

The empirical strategy employed in this paper is a “sharp” regression discontinuity design (RDD). This strategy is used to compute the causal effect of the legislation on the outcome of interest, i.e., the starting wage of women working in large firms in Chile between 2005 and 2013.

Using the basic setting for the Rubin Causal Model (RCM) the outcome observed can be written as:

$$w_i = \begin{cases} w_i(0) & \text{if } D_i = 0 \\ w_i(1) & \text{if } D_i = 1 \end{cases} = w_0(1 - D_i) + w_1 D_i$$

$$D_i = \mathbf{1}\{F_i \geq 20\}$$

where $w_i(0)$ represents the wage for female workers without exposure to the treatment, $D_i = 0$ (i.e., women working in firms with less than 20 female workers); $F_i < 20$ and thus, not receiving child care services by their employers; $w_i(1)$ is then the wage given exposure to the treatment, i.e., for women working in firms with 20 or more female workers and in consequence, receiving employer-paid child care services for their children under two years of age.

We are interested in the average causal effect of the treatment at the discontinuity point:

$$\tau = E[w_i(1) - w_i(0) | F_i = 20] \tag{3}$$

To determine the validity of our approach we need to ensure that at least four basic assumptions for RD are satisfied (see Nichols, 2007). The first assumption is that the treatment is not randomly assigned. This assumption is satisfied because only firms with 20 or more female workers are

mandated to provide child care services to their female employees, an observable variable. In addition, we restrict the analysis to large firms where the compliance to the law is high to ensure that provision of child care is a deterministic function of the number of women working at the firm.

The second assumption is the presence of a discontinuity in the wage of females when the number of female workers is 20, so the selection on observables at the threshold is also satisfied. Figure 1 presents the average of the starting wage for females by the number of women working at the firm where they were hired.¹⁴

[Figure 1 about here.]

The third assumption is that there is no manipulation of the running variable. Finally, the fourth assumption requires that the other variables -demographic characteristics and characteristics of the firm where the individual is employed- are smooth functions of the assignment variable conditional on treatment -that is, the only reason the outcome variable should jump at the threshold is due to the discontinuity in the level of treatment.

To discuss in detail the last two assumptions, we introduce some additional notation. In this case the running variable -number of female workers- is discrete so the conditions for non-parametric or semiparametric methods are not satisfied. Instead we regress w_{ij} on a low-order polynomial in F_{ij} . According to Lee (2008a) if the polynomial function correct the conventional ordinary least squares (OLS) inference is appropriate.¹⁵

$$w_{ij} = \alpha + G(\tilde{F}_{ij}) + D_i * G_p(\tilde{F}_{ij}) + \tau D_i + \gamma_j + \epsilon \quad (4)$$

$$\text{where } \tilde{F}_{ij} = F_{ij} - 20,$$

and $G_p(\tilde{F}_{ij})$ is a p order polynomial with $G_p(\tilde{F}_{ij}) = \beta_0 \tilde{F}_{ij} + \beta_1 \tilde{F}_{ij}^2 + \dots + \beta_p \tilde{F}_{ij}^p$, $D_i * G_p(\tilde{F}_{ij})$ that allows different polynomials on the two sides of the discontinuity, γ_j captures firm-level fixed effects and τ is the variable of interest.

¹⁴As a robustness check, we present the analogous figure in Figure 5, but for the starting wages of male workers. As expected, no discontinuity is present. In the same spirit, Figures 6 and 7 test for discontinuities around a different cutoff point, 15 and 25 female workers. As expected, there is no discontinuity in starting female wages around different cutoffs.

¹⁵Some authors have followed a similar approach (e.e., Lee, 2008b). For a discussion on the methodology and one alternative procedure for inference refer to Lee (2008a).

In principle, the expected discrete change in the provision of child care services mandated by the law encourages the use of RDD to estimate the causal effect of the program on wages. The intuition behind this strategy is that firms that lie just below the threshold and firms that lie just above the threshold are statistically comparable except for the fact that one group of firms is mandated to offer child care service to its female employees and the other group of firms is not. As a result, any discontinuity in the conditional distribution of wages at the threshold could be interpreted as the effect of the law.

The running variable in this case, however, is the maximum number of females working in the firm within a year. In theory, as a result of the optimization problem, firms decide the number of employees and the wage. So, a given firm may reduce its labor cost by not hiring (or firing) the 20th woman, because it is not longer mandated to provide child care services to its female employees. The endogeneity of the running variable creates a threat for identification and, in consequence, a threat for the validity of RDD estimates to reflect the causal effect of the law on wages.

We exploit the availability of longitudinal data to analyze the impact of the law on firm-specific new hires, those leading firms to cross the threshold. One key observation is that large firms tend to cross the threshold at some point. The substitution strategy is not sustainable in the long run for these firms. An evidence of this behavior in our data is that only 24 percent of large and new firms are below the threshold of 20 female workers for the whole period (2005-2013).¹⁶ As a result, the distinction between firms that are below the threshold and those that are above it vanishes in the presence of longitudinal data, reducing the threat to the identification strategy. To justify the identification strategy, we need to formally test for manipulation on the running variable and demonstrate that women hired when the firm is below the threshold are statistically comparable to those hired when the firm is just above the threshold, except for the fact that one group receives employer-paid child care services while the other group does not.

Testing for Manipulation in the Running Variable

A simple graphical test shows the lack of evidence on firms bunching just before reaching the threshold of 20 female workers and the lack of discontinuities in the distribution of firms by the

¹⁶An important fraction of these firms are new firms created in the last two years, which means that the percentage of firms that never cross the threshold would be lower if we control for truncation.

number of female workers. Figure 2 presents the density of firms (new and large) by the number of female workers using both monthly and annual frequency data. In both cases, we observe more mass below than above the cutoff and a tendency but no discontinuity around the cutoff point.

[Figure 2 about here.]

We use a discrete-adjusted version of the test proposed by McCrary (2008) to formally test for potential manipulation of the running variable. This test consists in computing an estimator for the size of discontinuity around the cutoff in the density function of the running variable.

Table 3 presents the estimation of the coefficient associated with a dummy variable indicating whether the firm has 20 or more female workers D . We estimate a regression of the (log) of the frequency of firms on a low-order polynomial in the number of female workers \tilde{F}_{ij} and D . We present estimates for two specifications, one using a polynomial of degree 2 and another using a polynomial of degree 5.¹⁷ We do not compute bandwidths in our parametric approach, but we still need to restrict the regression to the observations that are close to the threshold. For that reason, we present estimates using two different subsamples. The first subsample corresponds to firms with 10 to 30 female workers (Window N_females=10), and the second to firms with 5 to 35 female workers (Window N_females=15). In all cases, we present the results with and without additional controls for the average number of workers and the average wage for female and male workers.

We find no evidence of a statistically significant discontinuity in the distribution of firms around the cutoff point. This is true for all the specifications presented. The lack of evidence of bunching is important to justify the validity of our RDD results.¹⁸

[Table 3 about here.]

Testing for Discontinuities in Observable Characteristics Around the Cutoff

Our identification strategy compares starting wages of females hired when firms are just below the threshold with the starting wages of females hired right after the firms crosses it. In this context, we need to ensure that women hired when the firm is just below and just above the threshold are

¹⁷We test the fit of the polynomial functional form using a simple goodness of fit test after a graphical analysis of the data. In general, the best fit is attained with polynomials degree 2 and 5, we present the two extreme values but, we prefer the specification with the polynomial with the lowest degree following Gelman and Imbens (2014).

¹⁸Escobar (2014) finds that manufacturing firms tend to concentrate below the threshold of twenty workers but we finds no evidence of bunching at 19 female workers.

statistically comparable except for the fact that one group receives employer-paid child care services and the other group does not. If this is true, any discontinuity in the conditional distribution of wages at the threshold could be interpreted as the effect of the law. For this purpose, we test for discontinuities in observable characteristics, demographic characteristics, and characteristics of the firm around the threshold.

Figures 3 and 4 present the results of running polynomial regressions on different characteristics of both the individuals and the firms near the cutoff point. The fact that we observe multiple first-time hires in different firms at different times -including the same woman on different dates- help us to ensure the plausibility of the assumptions. The figures evidence that the available covariates (age, marital status, highest completed degree, type of contract and size of the firm) are smooth functions of the assignment variable, conditional on treatment. In consequence, the only reason for the outcome variable to jump at the cutoff is the discontinuity in the level of treatment.

We find that the average age is a decreasing function of the number of female workers in the firm. This should not represent a problem since we are using a local estimation. If we consider that wages increase with age (experience, tenure, etc.), having a relatively older composition of the treatment group would imply that the difference with the control group is even higher than estimated (considering that starting wages increase with age).¹⁹

[Figure 3 about here.]

[Figure 4 about here.]

Our tests cannot rule out the presence of differences in unobservables such as ability, which in general is associated with higher wages. In that case our estimates would be a lower bound of the total effect.

6 Results

Table 4 presents the results of the estimation associated with the regression presented in Equation 4. We test the fit of the polynomial functional form using a simple goodness of fit test after a graphical analysis of the data. In general, the best fit is attained with polynomials of degree

¹⁹Note that RDD does not require these characteristics to be uncorrelated with wages, RDD only requires this characteristics do not have a discontinuous effect on wages (Nichols, 2007).

2, 3 and 4. The results are fairly constant across specifications but as a general rule we prefer specification associated to the polynomial with the lowest degree following Gelman and Imbens (2014).

We present estimates corresponding to two different ranges of data. In the first case, we use females hired for the first time in firms with 10 to 30 female workers. In the second case we use a broader window of 15 female workers around the cutoff point. In all cases, we present the results with and without additional controls for age, age squared, type of contract, schooling, year hired, and region of residence.²⁰ Although the results are not very different, it is useful to evaluate the impact of controlling for other variables that affect starting wages.

In principle, if the identification strategy is valid, covariates should be redundant. According to Imbens and Lemieux (2008), however, including covariates may be useful to eliminate small sample biases present in the basic specification, and improve the precision. In addition, they can be useful for evaluating the plausibility of the identification strategy.

On the other hand, Nichols (2007) considers that including covariates is generally a very bad idea. Although the covariates may improve efficiency by reducing residual variance, they could also reduce efficiency due to estimation error in their coefficients. In addition, any violations of the assumptions that such covariates are exogenous and have a linear impact on mean treatment and outcomes could greatly increase bias. The estimate of the effect slightly decreases in magnitude and is less precise after controlling for covariates.

[Table 4 about here.]

Our results indicate that women hired in large firms of 20 or more female workers with mandatory employer-paid child care services are penalized with lower starting wages compared with the wages of women hired in firms with no such requirements. The size of the difference ranges between CLP\$24,000 to CLP\$53,000 (US\$39-US\$87) depending on the specification.²¹

The appendix presents the results of our estimation using different sizes of firms to confirm that only large firms adjust starting wages in reaction to the policy. More precisely, in firms with 20-50 workers (small to medium according to the traditional categories used in the country) we do not

²⁰There is no consensus on the convenience of controlling for other variables.

²¹Values expressed in Chilean pesos of 2009. Values in dollars computed using the exchange rate in effect on April 2015 of 609.89 Chilean pesos per US dollar.

find any statistically significant difference in starting wages of females. (see Tables 5 and 6). The estimated effects increase when we restrict the sample to firms with 100 or more workers, but the magnitude is smaller compared to our results for large firms -following the traditional definition of 200 of more workers- (see Table 7).

As a robustness check, we also present results for firms with 250 or more workers (see Table 8). These results are in line with the observation that only large firms comply with the law and in consequence adjust starting wages accordingly. It is also consistent with the observation that larger firms are subject to stringent vigilance and control procedures.

The most conservative way of interpreting the results is as a lower bound of the total effect of the legislation on the wages of females working at large firms. The imperfect compliance with the law is likely to be associated with an underestimation of the difference in wages between women working at firms with mandatory employer-paid child care services and women working at firms without this requirement. We assume that all large firms that employ 20 or more workers are paying for child care services when, in fact, 10 percent of firms do not comply with the law. Women hired in noncompliant firms should not experience any effect on their wage. If we were able to remove those observations, the average wage of female workers above the threshold will be even lower, which implies a larger difference with the wage of females below the threshold.

Considering the reported average per capita monthly cost of providing child care services (CLP\$75.000 -CLP\$100.000) the results at first sight may suggest a low degree of pass-through.²² Nevertheless, to analyze the results in perspective, it is important to consider that the cost of child care provision is only paid to the women with young children, while the wages include all registered female workers. In addition, the present estimates only consider starting wages. Given the importance of initial conditions on future wages, the effect of the policy must be analyzed not only in terms of the magnitude in a given period, but also in terms of the long-run consequences implied. In particular, a low starting wage is highly correlated with lower future wages.

²²The cost of child care service provision from (Dirección del Trabajo, 2008). Values are expressed in Chilean pesos of 2008.

7 Conclusions

The present document quantifies the impact of mandated provision of child care services in Chile finding sizable effects on the starting wages of female employees. This is not surprising, given that mandated benefits can be interpreted as a tax. As Summers (1989) points out, the tax rate is equal to the difference between the employer's cost of providing the benefit and the employee's valuation. Since it is difficult to compute precisely the employee's valuation, we estimate the extent to which the cost of mandatory legislation translates into lower wages -specifically, lower starting wages.

Our results indicate that women hired in firms with mandatory employer-paid child care services are penalized with lower starting wages compared with the wages of their counterparts hired with no such requirements. As presented in Table 3, the size of the difference in large firms created after 2005 ranges between CLP\$24,000 to CLP\$53,000 (US\$39-US\$87) depending on the specification.²³ These numbers represent between 9 and 20 percent of the average starting wage of women hired in firms below the threshold of 20 female workers.

As a caution note, it is important to clarify that this paper concentrates on one margin of the adjustment: starting wages of female workers. We estimate the size of the adjustment for large firms when they decide to cross the threshold of 20 female workers. In consequence, our results may not be interpreted as evidence for the lack of adjustment through substitution of workers by gender. The law may have an important impact on the hiring patterns of other firms that never cross the threshold (e.g., medium-sized firms). This analysis is more difficult to perform rigorously, however, considering the low compliance with the law by those firms.

This paper highlights the adverse unintended effects of a law for the group that is intended to benefit from it. The objective of the law is to guarantee the right of working mothers to have child care services and to promote the child-mother close relationship and healthy development of the children, as well as reduce gender disparities in the labor market. The law creates a distortion, however, affecting differentially the cost of hiring women. This creates a wage disadvantage for women, lower incentives to participate, and higher gender disparities. Finally, considering the importance of starting wages on future wages, computing the long-run effects of the lower starting wages for women may be an interesting extension of the present paper.

²³Values expressed in Chilean pesos of 2009. Values in dollars computed using the exchange rate in effect on April 2015 of 609.89 Chilean pesos per US dollar.

8 Appendix

Robustness Checks

Graphical Discontinuity Test: Different Cutoffs and Different Populations

As a robustness check, we present check the presence of any discontinuities in the starting wages of male workers. As expected, no discontinuity is present. In the same spirit, we test for discontinuities around a different cutoff point, 15 and 25 female workers. As expected, there is no discontinuity in starting female wages around different cutoffs.

[Figure 5 about here.]

[Figure 6 about here.]

[Figure 7 about here.]

Results Variations on Size of the Firm

[Table 5 about here.]

[Table 6 about here.]

[Table 7 about here.]

[Table 8 about here.]

Results from Using Nonparametrical Approach

Estimates for large and new firms are presented in Table 9. Controlling for the uncertainty on the total number of workers produce a slightly higher, but less precise, results. In particular, the estimated effect oscillates between CLP\$26,780 and CLP\$40,643 in starting wages.²⁴

[Table 9 about here.]

²⁴Values expressed in Chilean pesos of 2009.

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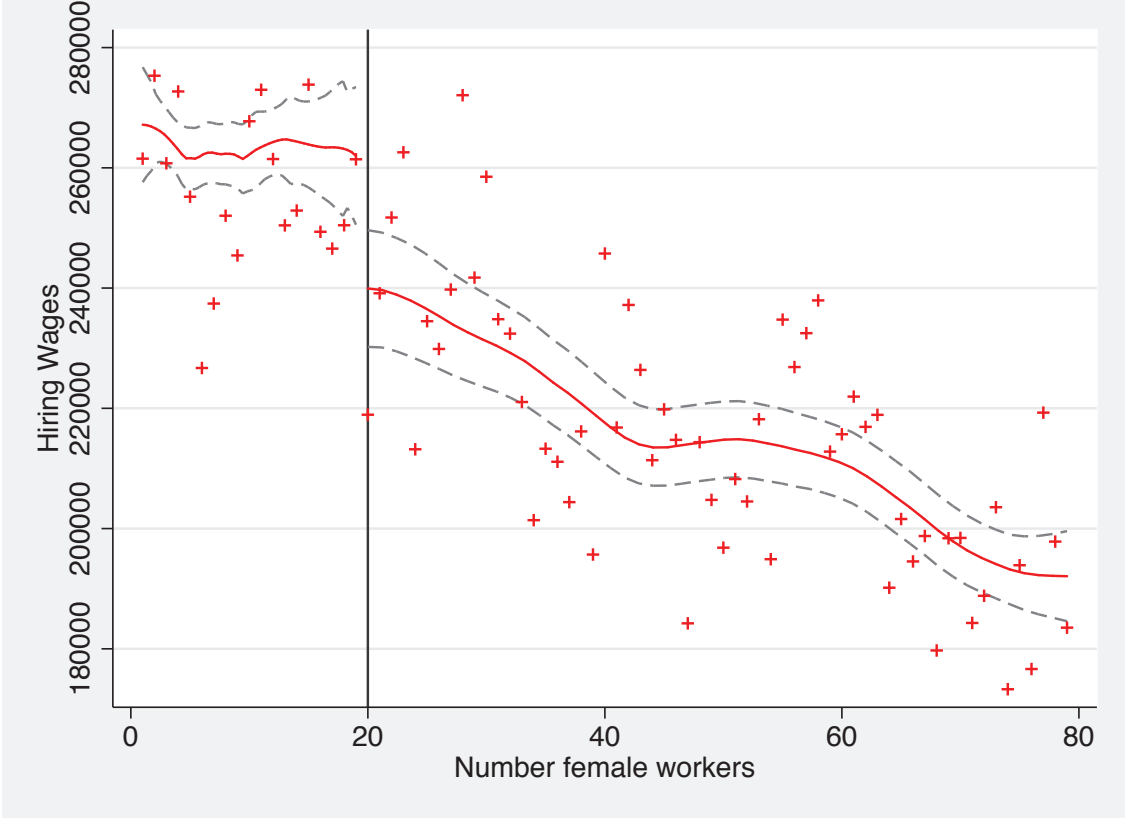
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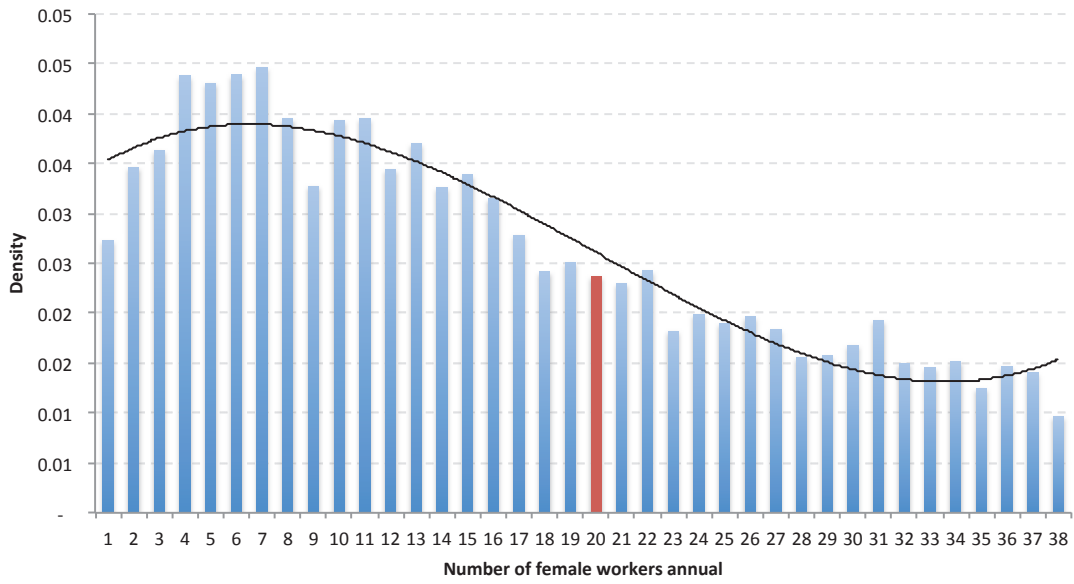
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Figure 1: Average wage females

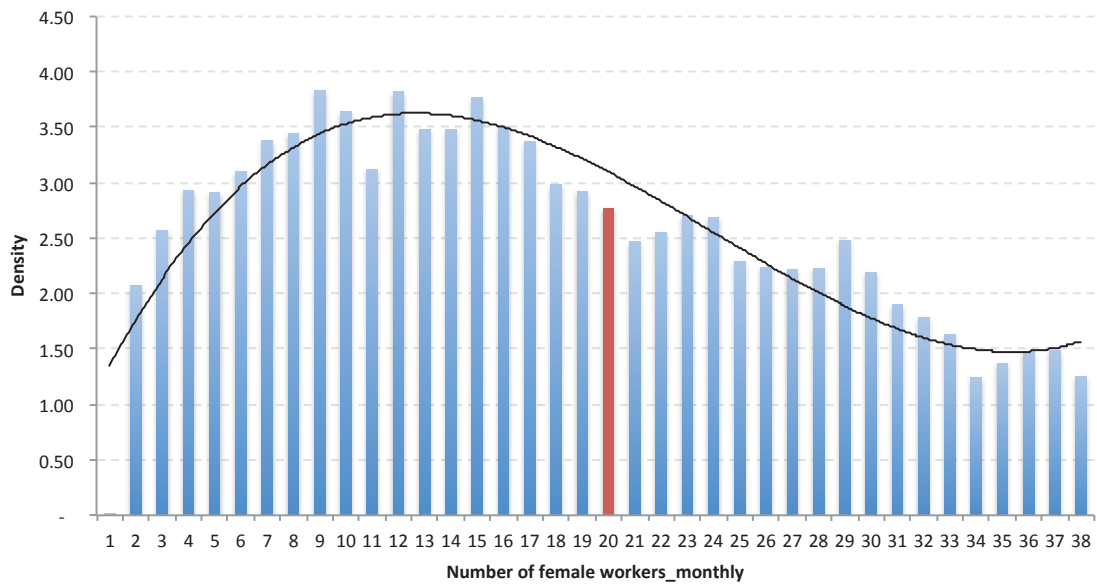


Note: The figure presents the average of the starting wage for females by the number of women working at the firm where they were hired.

Figure 2: Density of Firms by Number of Female Workers



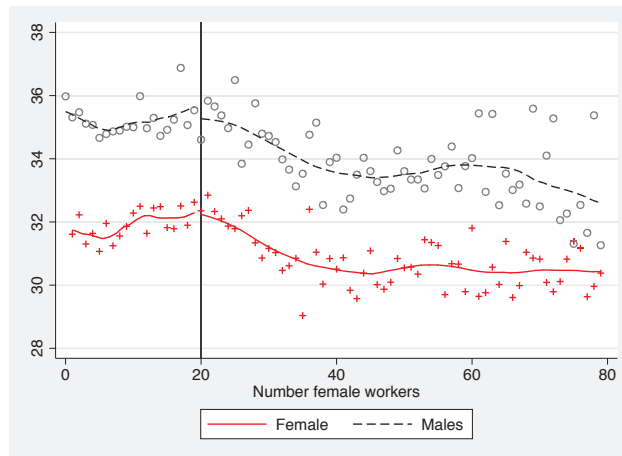
(a) Annually



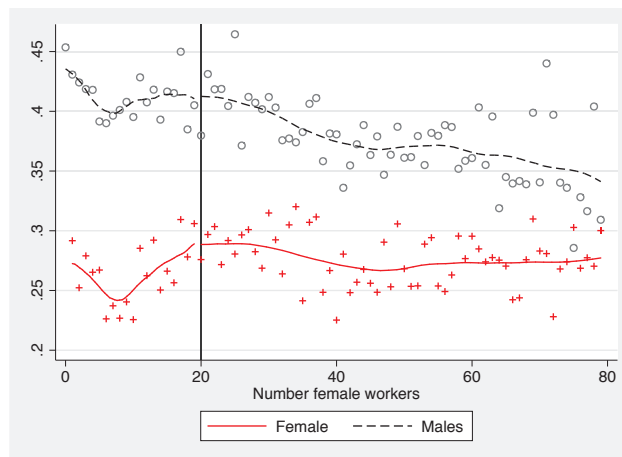
(b) Monthly

Note: The figure presents the density of firms (new and large) by the number of female workers using both monthly and annual frequency data.

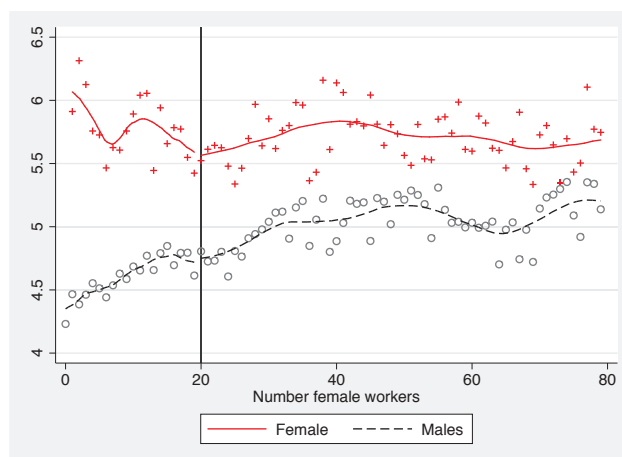
Figure 3: Graphical Discontinuity Test: Observable Individual Characteristics by Gender



(a) Age



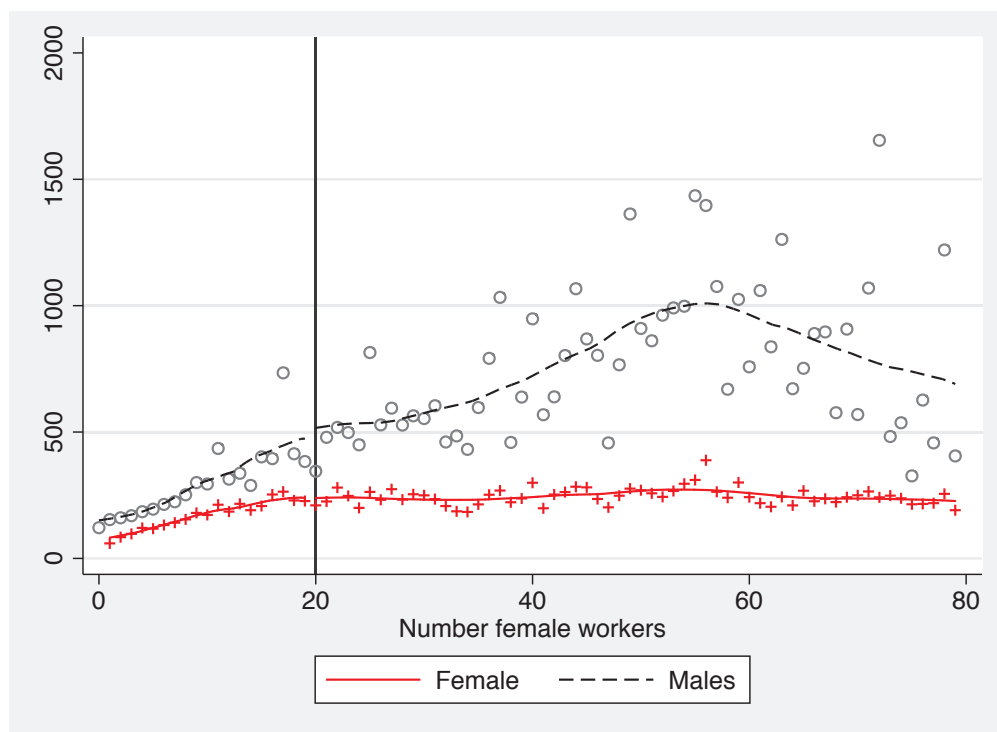
(b) Civil Status



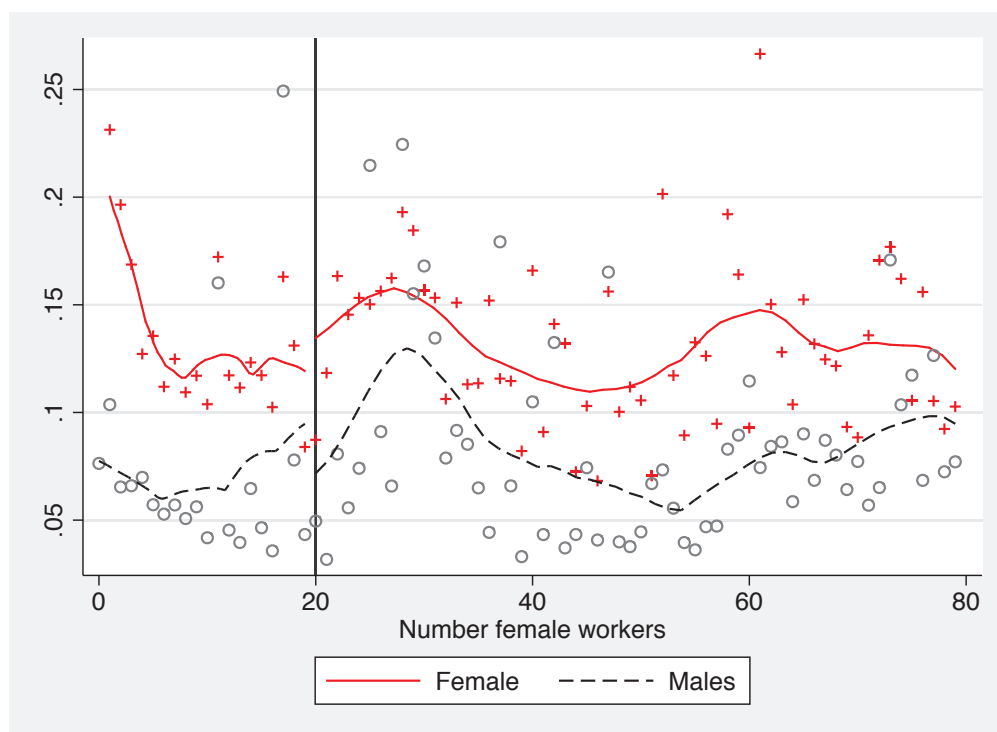
(c) Highest Grade Attended

Note: The figure presents the results of running polynomial regressions on different characteristics of both the individuals near the cutoff point.

Figure 4: Graphical Discontinuity Test: Observable Characteristics of the Job by Gender



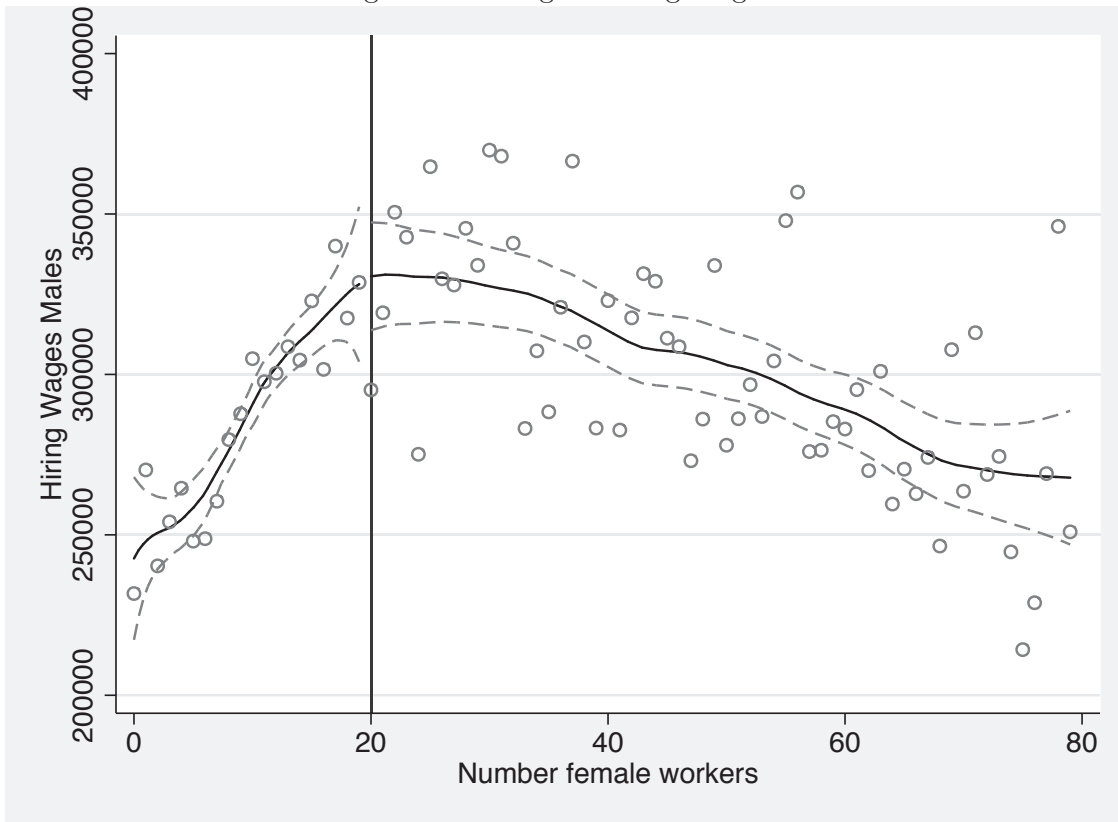
(a) Total Number of Workers



(b) Fixed Term Contract

Note: The figure presents the results of running polynomial regressions on different characteristics of associated with the job near the cutoff point.

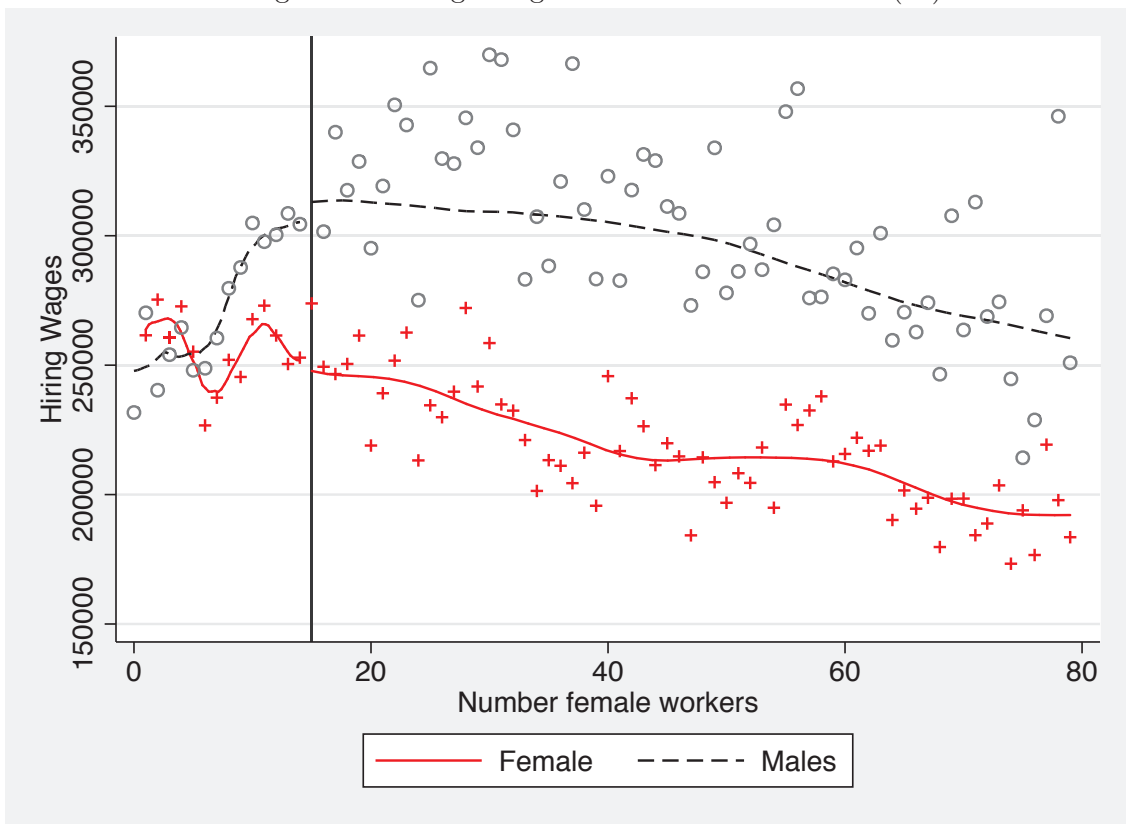
Figure 5: Average Starting Wage: Males



Note:

The figure presents the average of the starting wage for males by the number of women working at the firm where they were hired.

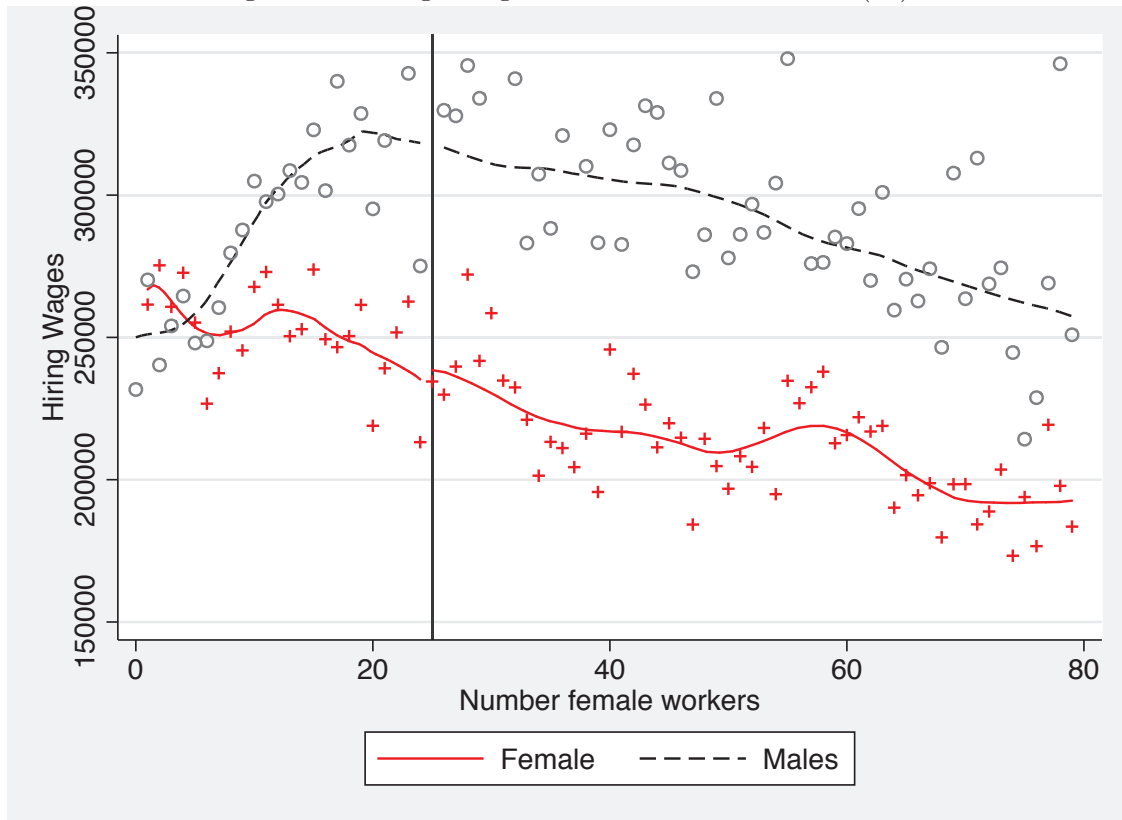
Figure 6: Average Wage Females: Different cutoff (15)



Note:

The figure presents the average of the starting wage for females by the number of women working at the firm where they were hired.

Figure 7: Average Wage Females: Different cutoff (25)



Note: The figure presents the average of the starting wage for females by the number of women working at the firm where they were hired.

Table 1: Descriptive Statistics: Firms by Size and Time of Creation (2005-2013)

	All firms	Large	New	New and large
N_firms	837,899	7,190	525,181	1,912
Crossing	22,033	5,397	7,419	1,224
	2.6%	75.1%	1.4%	64%
N_times crossing	2.82	3.00	2.20	2.41
Observations (month-year)	30,451,078	704,903	13,392,272	109,434

Note: Firms are classified as large if they employ 200 or more workers in a calendar year. Crossing refers to the number of firms that cross the threshold of 20 female workers at least once. N_times crossing refers to the average number of times firms cross the threshold.

Table 2: Descriptive Statistics: Characteristics of Firms where Female Workers were Hired for the First Time and Starting Wage

Variable	Mean	Std. Dev.	Min	Max
N_workers	2,059.023	3,248.03	200	20,620
N_female workers	927.6	1,733.77	1	12,101
N_male workers	653.96	1,245.55	0	8,572
Starting wage	174,975	152,566.9	21,063.31	1,253,090
Average wage females firm	212,256.8	163,370.9	0	2,015,287
Average wage males firm	247,426.1	187,017.5	0	2,030,768
Above	0.96	0.19	0	1
Obs (hires)	1'397,823			
N firms	1,912			

Note: “N_workers” is the average of the number of workers employed in the firm hiring new female workers over the period 2005-2013, “N_female workers” is the average number of female workers employed in the firm where the individual was hired; the analogous is true for “N_male workers”, “Starting wage” for female and male workers is the average of the real starting wage in Chilean pesos of 2009 for the females and males in the sample, “Average wage females/males firm” corresponds to the average wage of female or male workers working at the firm where the female was hired, “above” is a dummy variable for the firms with 20 or more female workers: the number of observations corresponds to individual hires and “ N firms” is the number of firms considered.

Table 3: Adapted McCrary Test for Discrete Running Variable

Window N_females	10	15
Poly. degree 2	-0.08	-0.03
Poly. degree 2 covar	0.04	-0.03
Poly. degree 5	-0.15	0.10
Poly. degree 5 covar	-0.27	0.11

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Notes: The table presents the results of the estimation for the size of discontinuity around the cutoff in the density function of the running variable. “Poly. degree 2” and “Poly. degree 5” refer to the results corresponding to the use of polynomials of degree 2 and 5, respectively. In both cases, we present the results with and without additional controls for the average number of workers and the average wage for female and male workers; “covar” refers to the estimations with additional covariates. “Window Nfemales” refers to the subsample of firms used in the estimation we use a window of 10 and 15 female workers around the threshold of 20, we used clustered-consistent standard errors (clustering on individual values of the number of female workers centered around zero, \tilde{F}_{ij}).

Table 4: Main Results: Female Starting Wages at New and Large Firms

Polynomial order		10-30 female workers		5-35 female workers	
2	Baseline	-28,781.9	***	-29,042	**
2	Covariates	-26,310.4	***	-24,433	*
3	Baseline	-26,121.9	*	-16,266	
3	Covariates	-23,694.1		-13,174	
4	Baseline	-33,657.3		-11,051	
4	Covariates	-52,772.0	*	-3,837	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Notes: The table presents the results of the estimation associated with the regression presented in Equation 4. We present the results associated polynomials of degree 2, 3 and 4, which are the regressions that best fitted the data. The first column presents results in the subsample of females hired for the first time in firms with 10-30 female workers, while the second column includes a wider range of 15 female workers around the discontinuity point. In all cases, we present the results for the baseline case (with no covariates) and for the case that controls for covariates (age, age squared, type of contract, schooling, year and region of residence). We used clustered-consistent standard errors (clustering at the firm level).

Table 5: Main Results: Female Starting Wages New Firms with More than 20 Workers

Polynomial order		10-30 female workers		5-35 female workers
2	Baseline	-940.67		1899.8
2	Covariates	-1232.15		-1711.7
3	Baseline	-5875.2	*	-1333.1
3	Covariates	-6831.06	*	-3685.6
4	Baseline	-7768.11		-3740.99
4	Covariates	-8858.21		-1220.88

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Notes: The table presents the results of the estimation associated with the regression presented in Equation 4. We present the results associated with polynomials of degree 2, 3, and 4, which are the regressions that best fitted the data. The first column presents results in the subsample of females hired for the first time in firms with 10-30 female workers, while the second column includes a wider range of 15 female workers around the discontinuity point. In all cases, we present the results for the baseline case (with no covariates) and for the case that controls for covariates (age, age squared, type of contract, schooling, year and region of residence). We used clustered-consistent standard errors (clustering at the firm level).

Table 6: Main Results: Female Starting Wages New Firms with More than 50 Workers

Polynomial order		10-30 female workers		5-35 female workers
2	Baseline	-5355.65		1064.98
2	Baseline	-691.96		-1535.7
3	Baseline	-13633.34	**	-6236.7
3	Baseline	-8557.67		-1079.3
4	Baseline	-9428.04		-6436.73
4	Baseline	-9261.08		-1845.45

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Notes: Table presents the results of the estimation associated with the regression presented in Equation 4. We present the results associated polynomials of degree 2, 3, and 4, which are the regressions that best fitted the data. The first column presents results in the subsample of females hired for the first time in firms with ten to thirty female workers, while the second column includes a wider range of 15 female workers around the discontinuity point. In all cases, we present the results for the baseline case (with no covariates) and for the case that controls for covariates (age, age squared, type of contract, schooling, year and region of residence). We used clustered-consistent standard errors (clustering at the firm level).

Table 7: Main Results: Female Starting Wages New Firms with More than 100 Workers

Polynomial order		10-30 female workers		5-35 female workers	
2	Baseline	-23441.56	***	-18387.96	***
2	Baseline	-16223.23	**	-15296.6	**
3	Baseline	-26863.64	***	-12240.2	
3	Baseline	-19012.6	*	-3207.9	
4	Baseline	-25323.78	*	-12240.16	
4	Baseline	-16361.28		-8699.83	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Notes: Table presents the results of the estimation associated with the regression presented in Equation 4. We present the results associated polynomials of degree 2, 3, and 4, which are the regressions that best fitted the data. The first column presents results in the subsample of females hired for the first time in firms with ten to thirty female workers, while the second column includes a wider range of 15 female workers around the discontinuity point. In all cases, we present the results for the baseline case (with no covariates) and for the case that controls for covariates (age, age squared, type of contract, schooling, year and region of residence). We used clustered-consistent standard errors (clustering at the firm level).

Table 8: Main Results: Female Starting Wages New Firms with More than 250 Workers

Polynomial order		10-30 female workers	5-35 female workers
2	Baseline	-49435.79	***
2	Baseline	-43026.15	***
3	Baseline	-45938.86	**
3	Baseline	-51608.77	**
4	Baseline	-5429.66	
4	Baseline	-15457.68	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Notes: Table presents the results of the estimation associated with the regression presented in Equation 4. We present the results associated polynomials of degree 2, 3 and 4, which are the regressions that best fitted the data. The first column presents results in the subsample of females hired for the first time in firms with ten to thirty female workers, while the second column includes a wider range of 15 female workers around the discontinuity point. In all cases, we present the results for the baseline case (with no covariates) and for the case that controls for covariates (age, age squared, type of contract, schooling, year and region of residence). We used clustered-consistent standard errors (clustering at the firm level).

Table 9: RDD Results on Female Wages-Optimal Bandwidth

	Wald estimate	Bandwidth	SE	Obs.
Baseline	-26,780	3.3	29,832.0	909,204
Baseline, N	-31,870.9	3.3	29,637.7	909,204
Baseline, N, Wm	-36,924.5 *	3.3	27,539	909,204
Baseline, N, Wm, Wf	-40,410.2*	3.3	27,292	909,204
Baseline, R, Wm, Wf	-40,643.7*	3.3	27,288	909,204

Notes: The variables used as covariates are: age, age squared, education, geographic location and year of starting. N is the total number of workers, Wm the average wage of males in the firm, Wf the average wage of females in the firm and R the ratio male to female workers. Optimal bandwidth is computed using Imbens and Kalyanaraman (2012). ***0.01, **0.05, *0.1.