Labor Market Consequences of Pay-equity Laws*

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Abstract

Portugal enacted legislation in 2018 targeting gender wage gaps, penalizing firms with over 250 employees that had gaps over 5%. Using administrative data that links employees to job-titles within firms, we analyze its impact both within and between genders using an event study design. First, we show that in treated firms, jobs with initial gaps exceeding 5% saw a 9% reduction, mainly through slower male wage growth. Jobs with negative gaps saw reduced female wage growth to close the gap by half. However, jobs with gaps between 0-5% unexpectedly rose by 21% due to slower female wage growth. These unintended consequences are more pronounced in male-dominated industries. Further, we find that firms did not change their size to evade the law, nor did it impact job gender composition, or hours worked. Our findings highlight how the establishment of a well-intentioned but uniform target gender wage gap that clarified the repercussions for gender imbalances thereby leading to unintended consequences.

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1 Introduction

Our understanding of gender inequalities in labor markets has markedly progressed over the past two decades, shedding light on both the shifts that have taken place and the underlying explanations (Goldin 2014, Goldin et al. 2017, Goldin & Mitchell 2017, Goldin et al. 2022). Nevertheless, numerous gender disparities persist, particularly in the earnings gap between men and women in the same job. Surprisingly, these gaps also persist in countries with pay equity laws that strive to ensure "equal pay for equal work". Despite their prevalence,[1] there is scant evidence regarding the impact of these laws on wage levels and the wage distribution. This gap in the evidence base is in part due to pay equity being either enshrined as a Constitutional Right or as a federally mandated law in most countries, providing researchers with limited variation to identify causal parameter(s) that estimate the impact of the policy on labor market outcomes.

This paper seeks to fill this gap in the labor economics literature by investigating the wage dynamics following the enactment of Law 60/2018 in Portugal, which strengthened pay equity measures. Specifically, this law aimed to ensure "equal pay between men and women for equal work" within firms with more than 250 employees.[2] Employers submit their employee-level pay data to the Ministry of Labor, Solidarity, and Social Security on an annual basis to assess if there are gender pay disparities. Failure to submit the annual report within the designated timeframe is considered a serious offense and can result in fines of up to EUR 13,000. If discrepancies exceeding five percent are detected, employers must promptly provide justifications based on objective criteria such as seniority or outline plans to rectify them. If the gender-based difference in pay remains unexplained, the firm would face repercussions that include financial penalties and potential sanctions, including license suspensions.

Using administrative matched employer-employee data on the universe of private sector workers in Portugal between 2014 and 2019 and an event study design, we estimate the causal impact of this pay equity law on wages. The richness of the data first allows us to define workers producing "equal work" as those who work in the same firm, in the same occupation, covered by the same collective bargaining agreement and have the same job title. Conditional on this definition of "equal work", we identify the causal effect of the law by exploiting variation in firm-size over time. We identify an average treatment effect for the treated parameter under the assumption that in absence of the pay equity law, the gender-specific wage trajectories of workers engaged in equal work within treated firms would resemble those in untreated firms, conditional on worker characteristics and definition of equal work subgroup that determine wages.

Since the potential cost implications of the law would vary across treated firms based on their pre-existing gender wage gap, we estimate the law's effect separately for firm equal work subgroups that are above and below the target five percent gender wage gap in the

[1] See Appendix C.1 for a detailed overview of pay equity laws and their enactment across various countries.

[2] Workers in Portugal were granted a Constitutional Right to equal pay for equal work since 1976, with subsequent amendments reinforcing this principle. Law 60/2018 comprises three key amendments to existing legislation: Law 10/2001 on annual reporting, Law 105/2009 governing the Labor Code, and Decree-Law 76/2012, which establishes the Commission for Equality in Labor and Employment (CITE). Additionally, much of the content of Portugal's Law 60/2018 aligns with a 2022 EU recommendation to member countries on achieving pay equity.

year preceding the policy implementation. Further, our empirical analysis exploits additional unique institutional features of the Portuguese labor market—common in many other European countries—that influence the wage setting processes. In Portugal, the majority of workers are covered by a collective bargaining agreement (henceforth CBA) and wage setting in Portugal is described as a mixture of centralized and decentralized bargaining (Bhuller, Moene, Mogstad & Vestad 2022). CBAs in Portugal establish industry-wide job-title specific wage floors, but firms have substantial flexibility to supplement these floors with idiosyncratic "wage cushions" (see e.g., Card & Cardoso (2022), Card et al. (2016)).

Our empirical analysis relies on the absence of systematic evidence that firms circumvent the law. We find no evidence of bunching in the density of firm size at the 250-worker threshold (hereafter referred to as "large firms"), indicating that firms did not manipulate their employment levels to circumvent the law. Furthermore, employment changes in large firms during the post-policy period were consistent with pre-policy trends, suggesting that these changes were employment shocks. Additionally, we find no evidence that firms adjusted the gender composition within job titles to evade compliance with the law.

Using an event study design, we identify and estimate the average treatment effect of the law on the gender wage gap in treated firms among workers producing equal work. Our empirical investigation uncovers substantial heterogeneity in the impact of the pay equity law on workers' wages on subgroups defined by the pre-existing gender wage gap within the firms' equal work subgroups exceeded the targeted five percent.[3]

First, we find that among workers employed in treated firms and equal work subgroups with unconditional wage gaps above the targeted threshold, the law effectively reduced gender disparities, as intended. In these firms and equal work subgroups, the conditional gender wage gap decreased by 9.03% on average, declining from 15.5% in 2017 to 14.1% in 2019. This reduction stemmed from slower wage growth among male workers.

Second, the 15% of the treated workforce where the unconditional gender wage gap was negative (males earned less than their female co-workers), the conditional gap closed by half (from -5.9% to 3.1%), through reduced female wage growth. However, among workers in treated firms and equal work subgroups with pre-law unconditional average gaps in between zero and five percent experienced a worsening of gender disparities. In these equal work subgroups within treated firms, we estimate the conditional gender wage gap increased by 21.4% on average, rising from 7% to 8.5% . This widening of gender disparity was primarily driven by larger reductions in wage growth among female workers. Further analysis reveals that the unintended consequences of the pay equity law were more pronounced in industries with an above-median share of male workers, while industries with an above-median share of female workers showed no substantial impact. Additionally, industries with an above-median share of female workers saw greater reductions in gender wage gaps for jobs with pre-existing gaps exceeding five percent, compared to industries with an above-median share of male workers.

These findings of treatment effect heterogeneity across subgroups uncovers the potential

[3]Note that we do not consider a regression discontinuity design to estimate the effects of this pay equity law. In settings such as ours, there is no plausible unobserved variable that would be monotonic with firm size and independently influence gender wage gaps far from the threshold. As a result, narrowing the analysis to a small bandwidth around the threshold is difficult to justify.

unintended consequences of such well-intentioned laws. While the policy successfully reduced gender disparities in jobs where men initially earned more than women by over 5%, and eliminated negative gaps where women initially earned more than men on average, it inadvertently widened wage gaps in firms where pre-policy positive gender wage gaps were below the five percent target.

Several mechanisms could explain these patterns, but we argue that an important role is played by the law's non-linear enforcement structure, where firms face penalties only when gender wage gaps exceed 5%. By clarifying and enforcing strict penalties for maintaining gender wage gaps, the law resolved previous uncertainties. As a result, risk-averse firms that previously maintained lower gaps due to regulatory uncertainty could now adjust these gaps towards the target rate. Additionally, we discuss the plausibility of alternative mechanisms in Section 7, including compensating differentials and moderately discriminatory firms exploiting labor market frictions that limit workers' ability to transition to more equitable employers.

We present robust evidence supporting our main findings. First, we estimate the intent-to-treat effects of the policy using firm size prior to the announcement of Law 60/2018 and find consistent effect sizes. Second, to address potential misclassification of treatment status due to employment shocks or churns—since firm size is observable only annually—we re-estimate our models excluding firms with 240–260 employees, and the results remain robust. Similarly, to address endogenous worker mobility we confirm our results when restricting the analysis to workers who remained with the same employer from 2014 to 2019. Third, we show that the policy had no impact on hours worked by either male or female employees, ruling out changes in working hours as a driver of the observed effects on hourly wages. We also do not find any evidence that firms adjusted the gender composition within jobs, eliminating the possibility that the effects stemmed from worker reallocation across roles. Lastly, we highlight the importance of analyzing equal work subgroups rather than aggregated firm-level data. Aggregating to the firm level overlooks the heterogeneity in responses across subgroups with varying initial pay gaps, resulting in offsetting effects and masking the richness in the true policy impacts. These findings strengthen our confidence in the interpretation of firms' wage-setting adjustments in response to the pay equity legislation.

Our paper contributes to the vast literature that both documents and provides an understanding of gender inequality in the labor market (see e.g., Blau & Kahn (2017), Goldin, Kerr, Olivetti & Barth (2017), Goldin (2014); among others). To the best of our knowledge, only Bailey, Helgerman & Stuart (2024) and Baker & Fortin (2004) present evidence on the causal impact of a pay-equity legislation by respectively studying the impact of nationwide Equal Pay Act of 1964 in the US and a policy implemented in a single Canadian province in the early 1990s. As discussed in Appendix D, our empirical setting differs sharply based on criteria to ensure enforcement and firm compliance with the law,[4] and by being able to exploit policy

[4] A concurrent paper by Passaro, Kojima & Pakzad-Hurson (2023) show evidence of non-compliance by firms in Chile who reduce their firm size below 10 (policy threshold) and gender segregate their workforce to avoid being subject to an equal pay law. They develop a theoretical model which shows that in absence of *protected status* (for example, gender, race etc.) firms do not have incentives to segregate and are less likely to avoid compliance. Our setting differs wherein Portugal in presence of oversight by CBAs, it is extremely difficult for firms to fire workers to fall below a policy threshold. Another important difference worth highlighting is that a policy defined firm size threshold of 250 (our case) is less likely to be manipulable and gender segregated both at the same time than a far

variation across firms of different sizes over time. Further, our data is richly detailed thereby providing a nuanced approach to defining "equal work," allowing us to compare individuals not just across firms but within the same job title as defined by industry-wide CBAs. Bailey, Helgerman & Stuart (2024) and Baker & Fortin (2004), working with historical Census and survey data, necessarily relied on broader classifications by industry, occupation, and state to define equal work. Our matched employer-employee data enables more granular comparisons within firms in line with identification of the policy parameter of interest. Consistent with Bailey, Helgerman & Stuart (2024), who studied the 1964 Equal Pay Act in the U.S., we find that pay equity laws can have nuanced impacts, though the nature of these effects differs substantially across settings. Their work shows how the Equal Pay Act reduced wage gaps through accelerated female wage growth while potentially constraining women's occupational advancement. In contrast, our findings highlight how specific features of modern pay equity policies, such as explicit numerical targets, can shape firms' strategic responses in ways that create unintended consequences.

Our study also complements the expanding literature on *pay transparency policies* which require employers to disclose information about compensation disparities between demographic groups. Compared to pay equity laws, pay transparency policies function differently in addressing gender pay gaps. They place the responsibility on underpaid workers to reduce wage inequities by using the disclosed information to negotiate improved wages with their employer (Cullen & Pakzad-Hurson 2023). However, these policies may introduce additional hurdles, particularly for women, given the well-documented gender differences in bargaining (Roussille 2021, Card, Cardoso & Kline 2016, Biasi & Sarsons 2022, Hall & Krueger 2012).[5] Consequently, it should not be surprising that there is no evidence indicating that pay transparency policies significantly boosted female wage growth (Baker, Halberstam, Kroft, Mas & Messacar 2023, Bennedsen, Simintzi, Tsoutsoura & Wolfenzon 2022, Perez-Truglia 2020).[6]

In contrast, pay-equity laws shift the responsibility for reducing gender wage disparities directly onto employers. Failure to comply with these policies in Portugal results in legal consequences for the employer, distinguishing them from pay-transparency policies that primarily hold firms accountable for disclosing information rather than addressing within-firm wage inequality.

The rest of the paper is organized as follows. In the next section, we provide a brief discussion of the Portugal's labor market institutions that pertain to both pay equity law and wage setting. Section 3 describes the data. Following this, in Section 4 we provide evidence that firms do not systematically circumvent the law, enabling us to outline the empirical strategy for estimating the causal impact of the law. We next discuss the identification assumptions and the event study framework in Section 5. Section 6 presents evidence of substantial unintended consequences of the policy and a discussion of the potential underlying mechanisms is

smaller policy defined firm size threshold of say 10.

[5] Additionally, research has documented negative impacts of pay transparency policy on the morale and productivity of lower paid employees (see e.g., Breza et al. (2018), Card et al. (2012), Cullen & Perez-Truglia (2022)).

[6] Generally, pay transparency policies have been shown to reduce gender wage gaps by curbing male wage growth. An exception is Gulyas, Seitz & Sinha (2023) who find no impact of a pay transparency policy on gender wage gap in Austria. Also see Agan et al. (2021) who document gender gap in recruiters' perception on employee quality based on whether salary history are disclosed.

provided in Section 7. A final section draws the main conclusions.

2 Institutional details

2.1 Pay-equity law in Portugal

Since 1976, all Portuguese workers had a Constitutional Right to equal pay. However, guidance, enforcement and repercussions for having gender disparities were ambiguous, until the announcement of Law 60/2018 on August 21, 2018. Law 60/2018 which mandated equal pay for equal work in all firms with over 250 workers aiming to promote gender pay equality. This Law was legally enforced effective February 22, 2019, whereby the Central government would now independently analyze pay gaps within these firms using the matched employer-employee data described in Section 3.[7] Pay gaps are independently analyzed by the government using the universe matched employer-employee data which we also use in this paper and is described in details in Section 3. If a pay gap is discovered or reported, government authorities notify the individual firm which would then have 120 days to justify or correct the wage gaps, failing which they face fines, exclusion from auctions and public tenders for up to two years. The law though applicable on firms with more than 250 workers was only enforced if the gender gap exceeded 5%. This level of '*accepted*' gender wage gap of five percent is ex-post in line with Article 9 of the May 10, 2023 report of the report on joint pay assessment by the European Union which recently recommended imposing fines on firms with hourly pay gaps exceeding five percent in member countries with pay equity legislation.

In addition to the government investigation, both workers and union representatives could file complaints with regulatory authorities if they suspect pay-inequity. Upon receiving the request, in the following 10 days Portuguese Commission for Employment Equality (CITE) notifies the company to, within 30 days, provide its response to it and disclose information on both the company's salary policy and criteria used to calculate the remuneration of the employee mentioned in the request as well as the employees of different sex in relation to whom the applicant considers to exist discrimination.

If the company fails to disclose this information within the 30 days period, it is considered that salary discrimination exists. Otherwise CITE will investigate and issue a final report in 180 days. In the event that pay discrimination is detected, the subsequent process requiring a company response as well as fines facing firms are identical. In total by 2023, 1540 firms received a notification to provide an official response. Also, whenever the Audit detects wage differences between men and women in a given enterprise, the department responsible for labor inspection (ACT) has 60 days in which to notify the employer that they have 120 days in which to present a plan for assessing the gender pay gap. This plan is implemented over the course of a year (12 months) and involves examining the components of the respective tasks based on objective criteria to prevent any chance of gender bias. After this period, the employer reports the outcome of the plan's implementation, demonstrating justified pay differences and the correction of any unjustified ones. Further details on the law are provided in

[7] The draft of this pay-equity law had been in discussion since 2017. The announcement of the Law in 2018 also stated that starting in February 2022, the law would apply to companies with more than 50 employees.

Appendix E.

While the law did not explicitly define "equal work", in practice, subsequent discussions in the popular press indicate that it is enforced at the job-title level within the treated firms. A pay gap would be considered justified if it is explained through differences in individual characteristics, such as education and tenure at the firm.[8] As such the law effectively aimed to promote gender equality within job title subgroups in the firm, while allowing for characteristics that could impact productivity and hence total wages received by workers, excluding overtime remuneration.

2.2 Wage setting in Portugal

Portugal's wage-setting system follows a two-tier structure common in Continental Europe. Industry-wide collective bargaining establishes job-title specific wage floors, while worker-firm negotiations provide flexibility, balancing centralized and decentralized wage determination. As Bhuller, Moene, Mogstad & Vestad (2022) point out, unions and collective bargaining are not synonymous concepts outside North America.[9]

In Portugal, vertical centralization of wage-setting primarily occurs through industry-wide collective bargaining agreements. Approximately 85% of private sector workers are governed by collective bargaining agreements at the industry level.[10] Additionally, workers with the same job-title within a firm but across different plants maybe subject to different CBAs due to regional differences.[11] The remaining roughly 15% of workers whose wage floors are not fixed by their job-title specific CBAs must bargain for their wages individually. Consequently, the data does not assign a job-tilte to these workers since (harmonized) job-titles that we use in our analysis are defined by CBAs.

Portuguese employers have considerable flexibility to pay idiosyncratic wage premiums to individual employees, on and above the collectively bargained wage floors. These "wage cushions" are common, vary by firm and worker characteristics, and change with changes in wage floors (Card & Cardoso 2022).[12] Beyond the wage floor and the wage cushion, workers typically receive regular earnings supplement which are payments such as meal allowances.[13] Additionally, for employees that have signed a sectoral or a firm-specific bargaining contract,

[8] There are no objective rules on how different these observable characteristics would need to be in order to justify any given level of gender wage gap.

[9] Typically, a much larger share of workers in Continental Europe are covered by collective bargaining (around 80%) than union density(around 10%). This is because legal frameworks which allow for automatic extension of benefits regardless of union status. See Bhuller, Moene, Mogstad & Vestad (2022) for a detailed discussion of cross-country differences in different wage setting practices.

[10] While union membership in the private sector is relatively low, widespread extension orders ensure that collective bargaining coverage reaches these very high rates.

[11] Industry-wide agreements serve to define an industry-specific minimum monthly wage, creating a wage floor for each job-title. There is very little horizontal co-ordination between industries or types of workers in the determination of these agreements (Bhuller, Moene, Mogstad & Vestad 2022). In contrast, union membership in Portugal is relatively low steadily declining from the 1990s, with less than 10% of workers in the private sector being unionized (Addison, Portugal & de Almeida Vilares 2023). This is the case with most of Continental Europe, where union membership has been declining over the past few decades while collective bargaining coverage has remained relatively stable and high (Bhuller, Moene, Mogstad & Vestad 2022).

[12] Card & Cardoso (2022) document that wage cushions on top of the industry-wide wage floors are typically larger for males than female workers conditional on worker characteristics available in our dataset.

[13] Within a given sectoral agreement, more productive firms have limited flexibility to assign their workers to higher floor categories.

typically adhere to agreed-upon normal working hours stipulated within the collective agreement. Thus, any firm attempting to justify a detected wage difference would be required to explain why differences in wage cushions are offered to workers of different genders that are employed in the same firm and job title.

3 Data sources

We use the *Quadros de Pessoal* data (henceforth QP, that translates to Personnel Records) which is an annual census of private firms matched to employees in Portugal from 2014 to 2019. This data is collected by the Ministry of Employment at the end of October from all firms with at least one paid employee. The data contains firm level and worker level information. At the firm level, QP includes information on region of operation, establishments, number of workers, industry of operation and volume of annual sales. At the worker level, QP includes information on the gender of the worker, various measures of monthly earnings (base, overtime, and regular payments), hours worked and various other demographic information. Crucially, QP includes the job title of each worker which we use to define equal work across genders within a firm.

3.1 Sample selection

In order to facilitate comparison with existing literature using the QP we mostly follow Card et al. (2016) in constructing our sample. We exclude unpaid family labor and restrict our sample to workers aged between 19 and 65. We keep only full-time workers who work in between 120 and 190 monthly hours who comprise 92% of our original sample. Close to 90% of female workers in Portugal who work in the private sector work full time (Card et al. 2016).

Hourly wages are computed by dividing the sum of base salary (wage floor + wage cushion) and regular earning supplements by normal hours of work.[14] We normalize all monetary measures in our data to 2019 euros. This leaves us with 35,809 firm-years and 6,613,573 worker-years.[15] Slightly over 15% of the workforce is not covered by any CBA and we lack job-title information for these workers. These workers are excluded from the analyses as are firms with any worker not covered by a CBA.

3.2 Summary statistics

Table 1 presents summary statistics for the pre-policy period (2014-2017), with Panel A showing statistics for all workers and by gender, Panel B comparing workers by firm size, and Panel C examining subgroups based on pre-policy gender wage gaps.

The workforce composition consists of 46.7% of workers being female, and 39.2% of all workers employed in firms with more than 250 employees (Panel A). Female workers have slightly lower representation in large firms at 38.6% compared to 39.7% for male workers. The

[14] The pay equity law specified the regular earnings supplement to be considered as part of total remuneration.

[15] Note that the final estimation sample will differ since resulting from our definition of equal work, those observations who form singleton fixed effect sets, will get dropped during the estimation because they lack within variation.

raw gender wage gap is substantial - male workers earn an average monthly wage of 1,376 euros compared to 1,050 euros for female workers, representing a difference of 326 euros per month. This translates to a log hourly wage gap of 0.208 (1.900 - 1.692). Female workers also work slightly fewer monthly hours on average (168.2 vs 169.7). Worker characteristics are relatively balanced across genders, with similar average age (about 40 years) and tenure at firm (approximately 9.4 years).

Comparing workers by firm size (Panel B), large firms employing over 250 workers pay higher wages, with average monthly wages of 1,357 euros compared to 1,138 euros in smaller firms. The log hourly wage differential between large and small firms is 0.16 (1.900 - 1.740). Workers in large firms have longer average tenure (10.4 vs 8.7 years) but work slightly fewer monthly hours (167.9 vs 169.7). The gender composition is similar across firm sizes, with females making up 47.1% of workers in small firms and 46.0% in large firms.

Panel C reveals substantial heterogeneity when grouping workers by their pre-policy gender wage gaps. Approximately 41% of workers are in jobs where the gender wage gap exceeds 5%, while 30% are in jobs with gaps between 0-5%, and 29% in jobs with negative gaps (where women earn more than men). Jobs with gender wage gaps above 5% have the highest average wages (1,472 euros monthly) and the lowest female representation (45.2%). In contrast, jobs with negative gender wage gaps or gaps between 0-5% have higher female representation (54.2% and 52.9% respectively) but lower average wages (1,194 and 1,063 euros respectively). Workers in jobs with larger gender gaps also tend to have longer tenure (10.5 years compared to about 7.5 years in other categories) and are more likely to be employed in large firms (52.7% versus 44.3% for negative gap jobs).

Examining the unconditional gender wage gaps within equal-work subgroups across the three gap categories in large and small firms (reported in Table 2) reveals substantial variation in wage disparities. In 2017, the pooled sample exhibits an overall gender wage gap of 5.14% within these subgroups, with considerable variation across categories. In subgroups where the gap exceeded 5%, the unconditional wage differential was 15.67%, while in those with negative gaps, women earned 6.75% more than men on average. Notably among jobs where the gap was more than 5%, large firms exhibited smaller wage gaps (13.56% compared to 18.14% in small firms) within both the above 5% and below 0% categories, whereas the gaps remained comparable for subgroups with differentials between 0-5%.

4 Evidence on firm responses to pay equity legislation

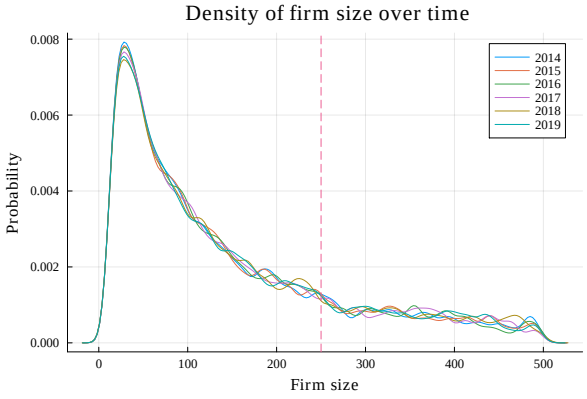
We examine two potential threats to the identification of causal parameters beyond the ITT, like the ATT. First, large firms may have endogenously reduced their size, and second, firms may have altered the gender composition of their workforce within job titles to circumvent the law. In this section, we provide evidence that neither of these threats is empirically supported.

4.1 Distribution of firm size over time

First, we show that the distribution of firm size over time exhibits no systemic bunching to the left of the 250 worker threshold following the years after the policy. Figure 1 and 2 display the

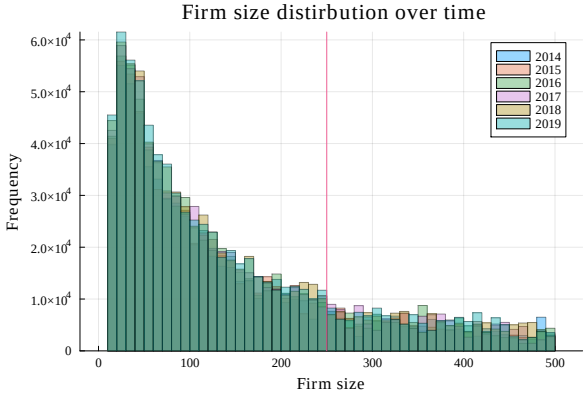
densities and histograms of annual firm size between 2014 and 2019, respectively.[16] Two key observations emerge in these figures. Notice that, we observe that the distributions of firm size remain consistent across years. Further, we do not observe any bunching of firms to the left of the threshold of 250 workers in the post-law years of 2018 and 2019. This suggests that firms did not endogenously respond to the law by reducing their firm size to avoid it. Additionally, intent-to-treat estimates of the impact of the policy on firm size which were around the threshold of 250 workers reported in Table 10 and in Appendix Figure A.?? show neither statistical nor substantial effects to suggest that firms did not systematically alter their size to circumvent the law.[17]

Figure 1: Densities of firm size over time



Notes: This figure plots the kernel density of firm size for each year in between 2014 and 2019, and overlay them on top of one another. The vertical red line represents the firm size of 250. The pay equity law was announced in 2018.

Figure 2: Histograms of firm size over time



Notes: This figure plots the histogram of firm size for each year in between 2014 and 2019, and overlay them on top of one another. The vertical red line represents the firm size of 250. The pay equity law was announced in 2018.

4.2 Employment changes in firms of various sizes over time

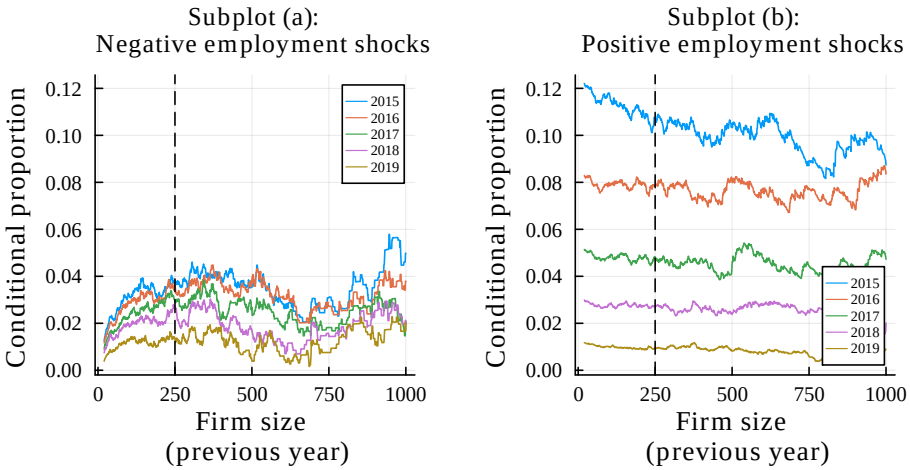
The previous subsection does not imply that firm size remained unchanged. We now examine how employment shocks, which can alter firm size, vary over time and across firms of different initial sizes. Figure 3 illustrates how employment shocks vary, conditional on the number of workers employed in the preceding year. In Figure 3(a), for any given year, each point represents a firm size from the previous year (x-axis) and the proportion of firms of that size that experienced a negative employment shock, resulting in a size reduction (y-axis). Figure 3(b) shows the same but for firms that experienced a positive employment shock, leading to an increase in firm size.

There is no evidence of systemic changes in employment among firms with more than 250 workers (or up to 1,000 workers), relative to how employment changes occurred in similarly sized firms over time. The data indicate that the proportion of firms near the 250-worker threshold experiencing employment shocks has remained relatively consistent across years. In

[16] In Appendix Figure 13 we plot the corresponding empirical CDFs.

[17] Specifically, for firms who employed in between 200 and 300 workers before the policy, we estimate $log(size_{jt}) = \sum_{s \neq 2017} \alpha_s D_j * \mathbb{I}[t = s] + \theta_j + \theta_{industry,t} + \epsilon_{jt}$ where D_j is the indicator for whether firm j's size in 2017 was above 250, $\theta_j, \theta_{industry,t}$ are firm and industry by year fixed effects. α_s represents the intent to treat effect of the policy on firm size.

Figure 3: Employment shocks conditional on firm size



Notes: This figure plots the proportion of firms receiving employment shocks (on the y-axis) conditional on their firm size in the last year (on the x-axis) over the years 2014-2019. For a given firm size in a given year on the x-axis, the left (right) sub-plot plots the proportion of firms receiving a negative (positive) employment shock in the next year, and the right sub-plot plots the proportion of firms receiving a negative employment shock. The lines of different colors represent different years in each sub-plot. The vertical dashed line represents the firm size of 250.

fact, employment shocks have been decreasing over time, but this reduction follows a pattern similar to pre-policy years, showing no systemic deviation attributable to the policy.

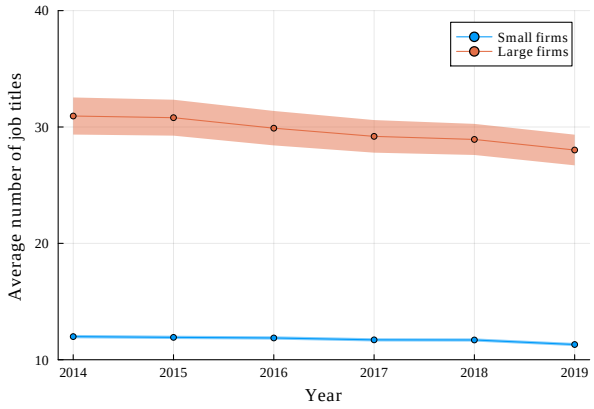
Moreover, these employment shocks are not significantly different around the 250-worker threshold compared to other thresholds up to 700 workers. If firms were responding to the policy, we would expect a spike in the proportion and number of firms experiencing negative employment shocks in 2018 and 2019, accompanied by a decline in positive shocks. Such a pattern would suggest that post-policy employment changes differ markedly from those in pre-policy years, making them less likely to be interpreted as mere shocks.

However, Figure 3 provides no discernible systemic pattern indicating that firms are adjusting their employment in response to the policy. Consistent with the lack of evidence of systematic changes in employment in large firms over time, Appendix Figure 14 provides graphical evidence showing that the aggregate proportion of the workforce employed in large and small firms did not vary between 2014 to 2019. This implies that observed employment changes can be considered exogenous shocks unrelated to the policy. Consequently, firms do not appear to be strategically altering their size to avoid the policy, supporting the validity of using firm size as a measure to define treatment.

4.3 Job titles over time

In Figure 4 we plot the average number of job titles in firms employing above 250 workers and firms employing below 250 workers over time. The shaded regions represent the 95% confidence bands. We observe that the average number of job titles in these two types of firms did not change much over time. In particular, we do not see any evidence on the number of job-titles being adjusted by large firms in order to work around the law. This is unsurprising because job-titles and job-title specific wage floors are set by collective bargaining agreements which are primarily industry-wide.

Figure 4: Job titles over time



Notes: This figure plots the average number of job titles in firms employing above 250 workers and firms employing below 250 workers over time. The shaded regions represent the 95% confidence bands. The pay equity law was announced in 2018.

5 Identification and estimation

5.1 Treatment definition

Let $j(i,t)$ represent the firm in which worker i is employed at time t. We define treatment for a given time period t as an indicator defined below.

$$D_{j(i,t)t} = \begin{cases} 1, & \text{if} \quad \#worker_{j(i,t)t} \geq 250 \\ 0, & \text{if} \quad \#worker_{j(i,t)t} < 250 \end{cases}$$

This definition of treatment does not put any restriction on the values of $D_{j(i,t)t}$ for other time periods $t' \neq t$. Hence, we allow the treatment to vary with firm size for all periods. Under standard assumptions which we delineate below, variation in firm size across time will identify the average treatment effect of the law on the treated.

The definition of treatment above differs from that commonly used in the pay transparency literature, where the policy rule to disclose pay structures within a firm is based on whether the firm's size exceeds a certain threshold. In these studies, treatment is defined by whether the firm's size was above this threshold in the year prior to policy implementation, and the estimated treatment effect is interpreted as an "intent to treat" effect. However, we do not use this definition for our primary results, though we consider it as a robustness check, for both economic and econometric reasons.

Economically, this definition assumes that firms can freely choose their size, which is inconsistent with the labor market monopsony literature (Card, Cardoso, Heining & Kline 2018, Card 2022, Lamadon, Mogstad & Setzler 2022). In this literature, firm size is determined in equilibrium, considering both the labor supply curve faced by the firm and the wage schedule, which reflects the firm's underlying production function.

From an econometric perspective, using a time-invariant treatment definition prevents the inclusion of firm-specific time-invariant characteristics. This limitation hinders the analyst's ability to capture systemic differences based on firm size. Consequently, the estimates heavily

depend on the assumption that firms are similar in both observed and unobserved characteristics relative to the threshold. This comparison excludes the possibility that firms experiencing churn may be more susceptible to changes in treatment status, potentially introducing bias due to misclassification, an issue we examine in Section 7.

5.2 Defining "equal work"

As in most matched employer-employee administrative datasets, in the QP as well we do not observe direct productivity measures of workers. However, the institutional details of the wage-setting process in Portugal offers plausible avenues which aide us in objectively defining "equal work" in line with how the law is enforced.

The key advantage of the QP data is that it has information on the job-title of the worker and the Collective Bargaining Agreement (CBA) that each worker is covered by. This information is typically absent in most matched employer-employee data, but crucial in defining "equal work". The CBA are typically industry-wide contracts between the firm and the union which specifies the wage floor for each job title. Within CBAs firms have flexibility to add cushions on top of CBA specified wage floor. These wage cushions vary with changes in wage floors negotiated in the industry-wide CBAs. Hence, our preferred definition of "equal work" is workers of the same job title in the same occupation and covered by the same CBA within a firm.

5.3 Pre-policy gender variation of workers and gender wage gaps within job titles

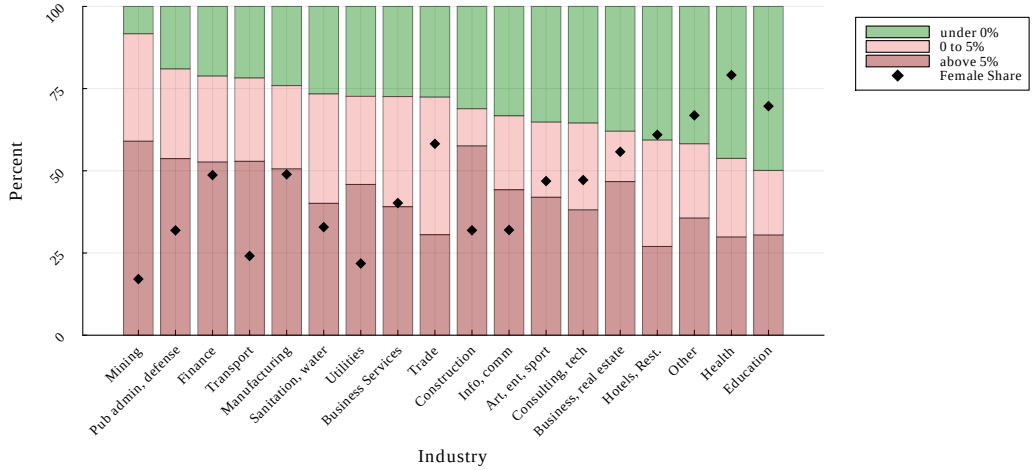
To identify our causal parameter of interest, we require variation in the gender of workers within job titles. In our data 43% of workers are employed in job titles where all workers within the firm year are of the same gender. This highlights the extent of gender segregation within job titles. Consequently, we are only able to construct average pre-policy gender wage gaps for the remaining 57% of workers, and can only identify the causal effect of the law on this subset of workers. Figure 5 shows that within firms job-titles with gender variation in workers differ in their gender wage gaps. Figure 5 also shows that industries with higher shares of female (male) workers are also associated with higher (lower) shares of negative gender wage gaps.

5.4 Identification

Our research design identifies the causal effect of the policy by comparing the within equal work cell differences in wages between male and female workers in treated firms to the corresponding differences in wages in their counterparts in untreated firms over time. Our design includes firm-occupation-CBA specific job title fixed effects, allowing us to account for any unobserved differences that explain how different firms systematically adds wage cushions on top of industry-wide CBA specified wage floors.

Identification of the average treatment effect on the treated parameter requires a conditional parallel trends assumption, which in our context requires the average differences in wages between male and female workers producing equal work in treated firms would have

Figure 5: Share of within job gender wage gap categories and share of female workers by industry



Notes: This figure on plots (a) the share of workers working in jobs with average pre-policy gender wage gaps below 0% (shaded green), between 0% and 5% (shaded pink), and above 5% (shaded dark red) for each industry and (b) the share of female workers in each industry represented by the black dots. The industries are sorted in the descending order of the share of workers in jobs with positive gender wage gaps.

evolved in parallel to the average differences in wages between male and female workers producing work of equal value in the untreated firms in the absence of the policy. Hence, given sufficient power to test for pre-trends, we can test for conditional parallel trends assumption in the gender wage gap within a triple difference event study framework. We formally specify the full set of assumptions for our research design in Appendix B.1 that additionally include having a sharp design, and no anticipation.

It is important to highlight that the identifying variation provided by the policy is on the wage differences between gender of large firms relative to small firms within job-titles. The variation provided by the policy is silent on wage differences within gender across firms of different sizes. Additionally, within the design-based approach by construction we can only identify the effects of the policy in partial equilibrium, invoking SUTVA. SUTVA implicitly requires that the potential outcomes of a firm is not affected by the treatment status of other firms.

5.5 Estimation: event-study framework

To estimate this causal parameter of interest, we build on the framework of Bailey, Helgerman & Stuart (2024), extending it to compare wages between male and female workers within job titles.[18] Our main event study specification can be expressed as

[18]Note that the average causal effect of Law 60/2018 on employment is not identified within the design-based framework. This is because the outcome—in this case employment—itself affects probability of treatment. In other words, the potential outcomes of employment are not independent of the treatment status and thus will induce simultaneity bias. Consequently, it is not straightforward to make claims on suggestive evidence on worker productivity by looking at the impact of the policy on employment and the volume of sales or profits. In our data 97.3% of firms always employed either less than, or always employed more than 250 workers. We cannot accurately forecast that in the absence of the policy, whether these firms would have still stayed on their respective side of the policy threshold. Hence, making causal claims on the effects of the policy on the employment level in treated firms

$$y_{ijt} = \theta_{equalwork(j(i))} + \sum_{s \in \mathcal{S}} \alpha_s * D_{j(i,s)s} \times \theta_s \times Male_i + \sum_{s \in \mathcal{S}} \gamma_s * D_{j(i,s)s} \times \theta_s + \quad (1)$$
$$\theta_{g(i),b(i)} + \tau * D_{j(i,t)t} \times Male_i + \psi D_{j(i,t)t} + \delta Male_i +$$
$$X'_{ij(i,t)t}\beta + \delta_{CBA \times t} + \delta_{ind} + e_{ijt}$$

where $\theta_{equalwork(j(i))}$ is the equal work fixed effect within a firm j where worker i works which forms the basis of our identification. In particular, $\theta_{equalwork(j(i))} \equiv \theta_{firm_j \times CBA_i \times occupation_i \times job-title_i}$ compares workers who work in the same firm, under the same CBA, in the same occupation and with the same job-title.[19] We follow Goldin (2006) and Goldin (2002), Bailey et al. (2024) by including a gender by year of birth fixed effect $\theta_{g(i),b(i)}$ to flexibly account for potnetial differential aspirations of birth cohorts by gender. Additionally, since Card & Cardoso (2022) document that CBAs in Portugal are typically renegotiated every two years on average, but the timing of the renegotiation of each CBA is unobserved in the data, we allow for a flexible CBA by year fixed effect.[20] $\delta_{ind}, \delta_{CBA \times t}$ are industry, and time-specific CBA fixed effects respectively. Finally, we include a time-specific CBA fixed effect to account for time-variant unobserved changes in CBA which changed the wage floor across firms over time. Allowing for further flexibility by incorporating industry by time and occupation by time fixed effects does not change our results.

We estimate equation (1) on data in the set of years $\mathcal{S} \equiv \{2014 : 2019\} \setminus \{2017\}$ which excludes 2017, the year prior to policy announcement. The primary parameters of interest are $\{\alpha_s\}_s$ that represent the change in gender-wage gap between large and small firms in year s relative to year 2017. In the pre-policy periods of $s < 2017$—with α_{2017} normalized to zero. Ideally, α_s should not exhibit any statistical differences from zero, serving as a test for parallel trends in the evolution of gender wage gap between large and small firms before the policy was implemented. The average conditional gender wage gaps in the base year, within jobs in large firms is given by by $\tau + \delta$.

The parameters γ_s represent the wage gap between female workers working in large and small firms in year s relative to year 2017. The parameters $\alpha_s + \gamma_s$ represent the wage gap between male workers working in large and small firms in year s relative to year 2017. To make causal claims on the mechanisms of changes in the gender wage gap both γ_s and $\alpha_s + \gamma_s$ in the pre-policy periods of $s < 2017$, additionally provide a test for parallel trends in the evolution of the within-gender wage gap between large and small firms before the policy was implemented. It is important to note that while it still might be possible that the between gender parallel trends hold i.e. α_s is statistically indistinguishable from zero for all $s < 2017$, the within gender parallel trends might fail in equal magnitude for both males and females. In such a situation although one can make causal claims on the effect of the law on the gender

is beyond the scope of the reduced form framework in this paper.

[19] We do not include worker fixed effects in the estimating equation, since the policy exploits variation to compare between workers of different gender. Further, including a worker fixed effect will subsume the time-invariant dummy of the gender of the worker. Consequently, we cannot identify the base gender wage gap in small firms in the year prior to policy implementation.

[20] Additionally, the change in the wage floor upon renegotiation does not completely pass through to total wages as firms may adjust wage cushions in response.

wage gap, the evidence will remain silent to make causal claims on the mechanisms of how did the wages of males and females evolve because of the policy.

6 Results

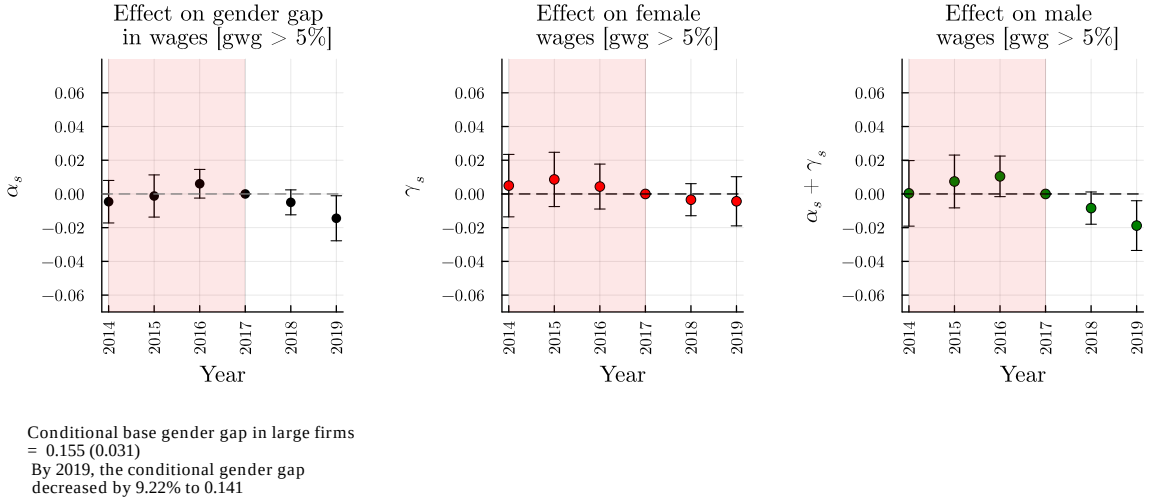
In this section, we present the results jointly estimating the impact of the law on gender wage gaps, female wages, and thereby on male wages and separately for subgroups defined based on whether the job-titles within the firm had a pre-existing wage gap exceeding 5%. Firms with baseline gender wage gaps above five percent had to reduce wage disparities among its workers within the job title, else potentially face repurcussions.

6.1 Jobs with pre-policy gender wage gap above five percent

Figure 6 plots the policy impact for workers in treated firms with jobs where the baseline gender wage gaps were above 5%, with the full set of coefficient estimates reported in column 3 of Table 3. In the first panel of Figure 6 estimates of α's are presented over time that capture the average effect of Law 60/2018 on the gender gap in large firms relative to small firms as compared to their difference in the base year 2017.

We find that on average the pay equity law reduced the conditional gender wage gap in the treated firms from 15.5% in 2017 by 1.4pp(p-value = 0.003) to 14.1% in 2019. This approximately represents a 9.03% reduction in the gender wage gap within two years of the law. The first panel of Figure 6 also shows that there is no statistical difference in the evolution of the gender wage gap within job-titles between large and small firms prior to the announcement of the law between 2014 and 2016 relative to how they differed in 2017. This finding is also reassuring as it supports the validity of the conditional parallel trends assumption.

Figure 6: Jobs where average pre-policy gender wage gap was above five percent



Notes: The figure presents ATT estimates showing the effect of the pay equity law on gender gaps in wages (*left subplot*), on female wages (*middle subplot*), and on male wages (*right subplot*) in jobs where average pre-policy gender wage gaps were above five percent. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

The second and third panels of Figure 6 present estimates of α's and $(\alpha + \gamma)$'s, that respectively capture the effect of Law 60/2018 on the female wage gap between large and small firms relative to the gap in 2017 and the corresponding gap for male wages. We observe that the overall reduction in the gender wage gap within job-titles in the first panel of Figure 6 was driven primarily by a larger reduction in male wage growth without impacting female wage growth. Specifically, male wage growth fell by around 1.9pp in two years after announcement of the law.

6.2 Jobs with pre-policy gender wage gap below zero percent

In our data approximately 15% of workers were employed in jobs in the pre-policy period that had an average gender wage gap under zero percent, i.e., where men on average earned less than women. These jobs are prevalent in female dominated industries such as health and social work, education, and public administration.

The first panel of Figure 7 illustrates the estimates from column 1 of Table 3, showing that the pay equity law increased the gender wage gap in these jobs by 2.0 to 2.9 percentage points in 2018 and 2019 respectively. As a result the conditional (negative) gender wage gap in the treated firms with these jobs reduced from -5.9% in 2017 to -3.1% in 2019 thereby closing nearly half of the negative gender wage gap that favored women. This was almost entirely driven by a reduction in female wage growth in jobs where women initially out-earned their male coworkers. The second panel shows that women in these roles experienced a decline in wage growth by 1.9 to 2.1 percentage points on average, while the third panel documents that male wage growth remained both statistically and economically unchanged in both years after the announcement of the law. Additionally, we did not find evidence of a statistically significant difference in the evolution of the gender wage gap between large and small firms before the law's announcement (2014–2016) compared to the period leading up to its implementation in 2017.

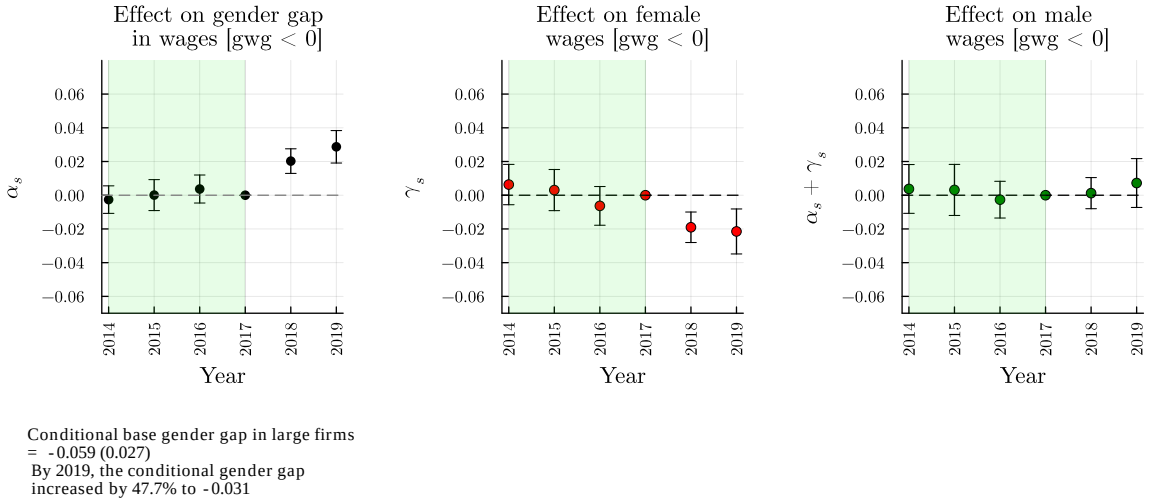
6.3 Jobs with pre-policy gender wage gap between zero and five percent

Next, we report the results for workers in treated firms with jobs where the baseline gender wage gap ranged between 0% and 5%, as shown in column 2 of Table 3. Under the enforcement rules, these firms were not required to make any adjustments to comply with Law 60/2018. However, starting in 2018, the legislation effectively eliminated uncertainty regarding potential consequences, including financial penalties, for maintaining a gender wage disparity.

The first panel of Figure 8 shows that the pay equity law significantly increased the conditional gender wage gap in these firms from 7% by 1.1pp in 2018 (p-value = 0.0004) and 1.4pp (p-value = 0.008) in 2019, thereby increasing the conditional gender wage gap to approximately 8.5% on average. This represents a 21.4% increase in the conditional gender wage gap within two years of the law's announcement. Importantly, between 2014 and 2016, there was no statistical difference in the evolution of the gender wage gap between large and small firms, relative to how they differed in 2017—supporting our conditional parallel trends assumption.

The overall increase in the gender wage gap shown in Figure 8 is due to a larger decline in

Figure 7: Jobs where average pre-policy gender wage gap was below zero

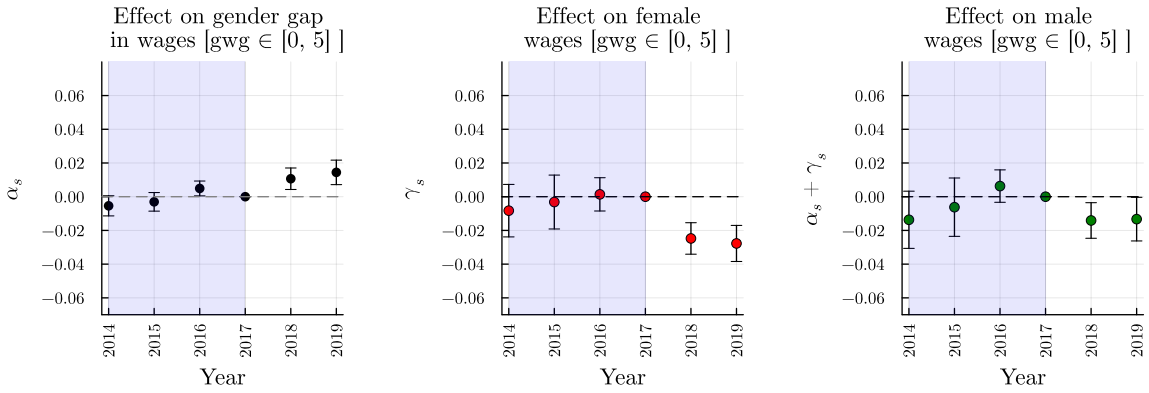


Conditional base gender gap in large firms
= -0.059 (0.027)
By 2019, the conditional gender gap
increased by 47.7% to -0.031

Notes: The figure presents ATT estimates showing the effect of the pay equity law on gender gaps in wages (*left subplot*), on female wages (*middle subplot*), and on male wages (*right subplot*) in jobs where average pre-policy gender wage gaps were below zero percent. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

female wage growth relative to male wage growth, as shown in the second and third panels of Figure 9. Specifically, female wage growth in treated firms fell by at least 2.5pp -2.8pp (p-value < 0.0001) whereas male wage growth declined by only 1.3pp to 1.4pp percentage points.

Figure 8: Jobs where average pre-policy gender wage gap was between zero and five percent



Conditional base gender gap in large firms
= 0.07 (0.019)
By 2019, the conditional gender gap
increased by 21.02% to 0.085

Notes: The figure presents ATT estimates showing the effect of the pay equity law on gender gaps in wages (*left subplot*), on female wages (*middle subplot*), and on male wages (*right subplot*) in jobs where average pre-policy gender wage gaps were in between zero and five percent. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

6.4 Impact of the pay-equity law on male and female dominated industries

To shed additional insights in how male and female wage growth adjusted following Law 60/2018, we classify an industry as gender-dominated if the average share of workers of one gender within jobs in that industry exceeds 50%.[21] We next investigate whether the law's impact on workers' wages varies between female-dominated and male-dominated industries.

In jobs where the average pre-policy gender wage gaps were under 0% as shown in Figure 9 there are no discernible *differences* in the law's impact on gender wage gaps or the growth of female and male wages between male and female dominated industries (Panels (a) and (b) respectively). However, if we pay close attention to the conditional base gender wage gap in treated firms in these industries, we find that male dominated industries had smaller negative gaps than female dominated industries. This implies that the law had a larger impact on male dominated industries than in female dominated industries. Specifically, within two years the law reduced the conditional negative gender wage gap by 75% (from -4.8% to -1.2%) in male dominated industries, while the reduction in female dominated industries was 25% (from -9.4% to -7.0%). All these estimates are statistically significant at the 95% confidence level.

In contrast, in jobs with pre-policy gender wage gaps above 5%, as depicted in Figure 10, the law had a more pronounced effect in industries with an above-median share of female workers. Comparatively, industries with an above-median share of male workers exhibited a trend toward slower male wage growth, though these changes were statistically insignificant at the 95% confidence level. Therefore, the intended consequences of the law are more evident in industries with an above median share of female workers.

In jobs where the average pre-policy gender wage gaps ranged between 0% and 5%, as shown in Figure 11, the unintended consequence of the law—widening gender wage gaps through a greater reduction in female wage growth—is both statistically and economically significant in industries with an above-median share of male workers. In contrast, industries with an above-median share of female workers experience nearly equal reductions in male and female wage growth, resulting in no statistically significant change in the gender wage gap in treated firms within these industries. In summary, the key observation from the estimates presented in Figures 9 and 10 is that the law's unintended consequences are more pronounced in industries with an above-median share of male workers.

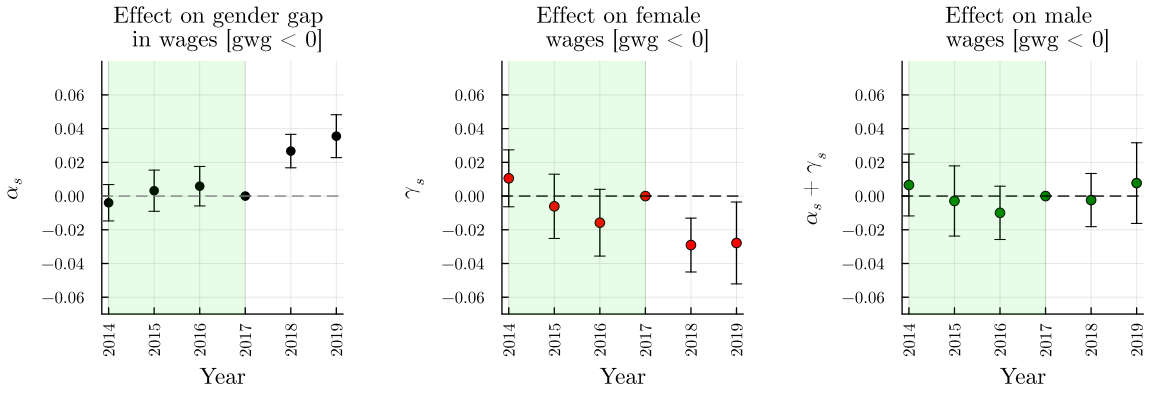
6.5 Pooling all workers together masks the differential impacts on wage dynamics

If we estimated equation 1 by pooling the above subsamples, a different policy message would likely emerge. Pooling the subsamples and including all job titles within the firm would constrain the wage dynamics for male and female workers in treated firms to be identical across job titles and occupations, effectively ruling out differential policy responses based on baseline gender wage gaps within firms. For completeness, Figure 12 presents the time-varying estimates of the policy effect, with the full set of coefficients shown in column 4 of Table 3. Upon

[21] Male dominated industries include construction, manufacturing, transport, finance, mining, sanitation and water, art, entertainment and sport, information and communication, consulting and technology, public administration and defense, business services and utilities. Female dominated industries include health, education, hotels and restaurants, business and real estate, trade, and other services.

Figure 9: Jobs where average pre-policy gender wage gaps were under 0%

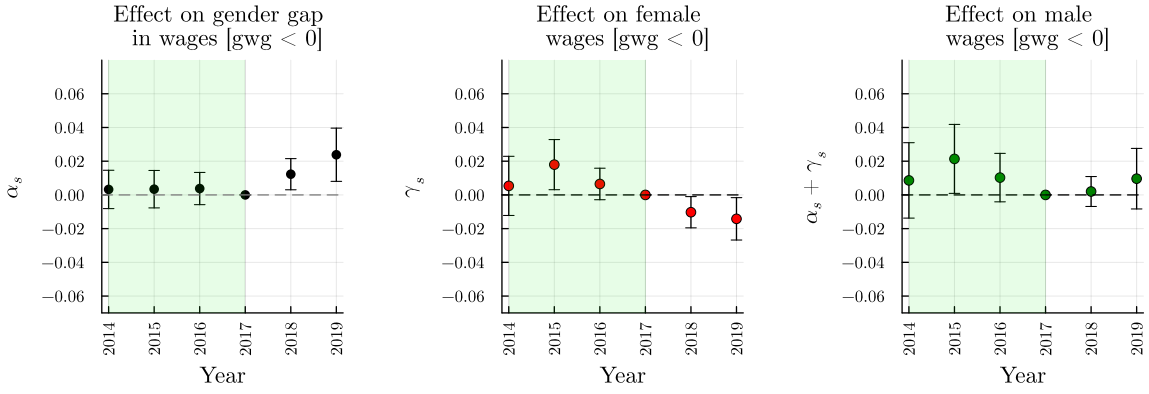
(a) Industries with above median share of male workers



Conditional base gender gap in large firms
= -0.048 (0.037)
By 2019, the conditional gender gap
increased by 74.9% to -0.012

Notes: The figure presents ATT estimates showing the effect of the pay equity law on gender gaps in wages (*left subplot*), on female wages (*middle subplot*), and on male wages (*right subplot*) in jobs where average pre-policy gender wage gaps were below zero percent, in industries with above median share of male workers. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

(b) Industries with above median share of female workers

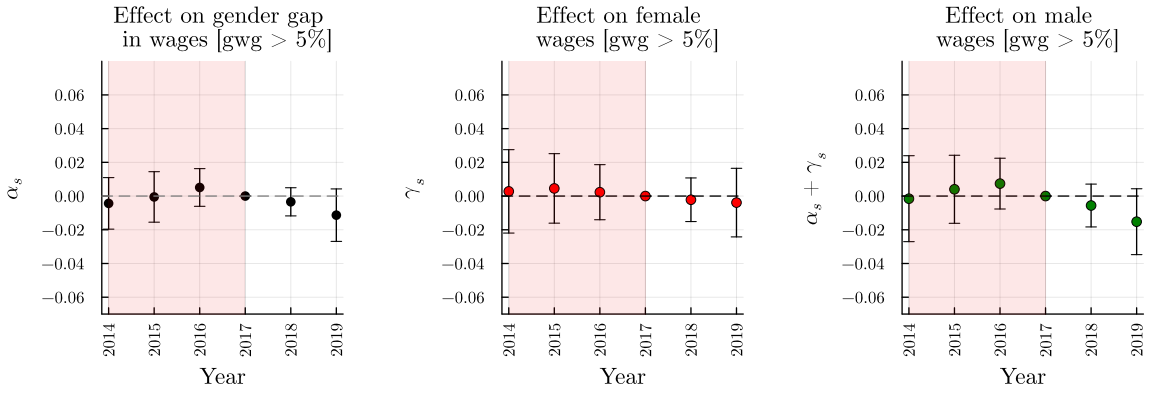


Conditional base gender gap in large firms
= -0.094 (0.037)
By 2019, the conditional gender gap
increased by 25.25% to -0.07

Notes: The figure presents ATT estimates showing the effect of the pay equity law on gender gaps in wages (*left subplot*), on female wages (*middle subplot*), and on male wages (*right subplot*) in jobs where average pre-policy gender wage gaps were below zero percent, in industries with above median share of female workers. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

Figure 10: Jobs where average pre-policy gender wage gaps were above 5%

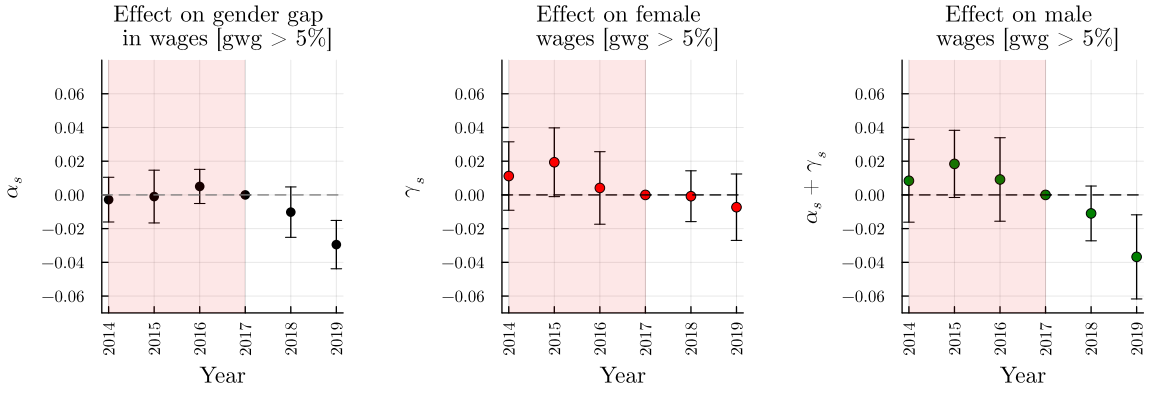
(a) Industries with above median share of male workers



Conditional base gender gap in large firms = 0.129 (0.036)
By 2019, the conditional gender gap decreased by 8.98% to 0.117

Notes: The figure presents ATT estimates showing the effect of the pay equity law on gender gaps in wages (*left subplot*), on female wages (*middle subplot*), and on male wages (*right subplot*) in jobs where average pre-policy gender wage gaps were above five percent, in industries with above median share of male workers. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

(b) Industries with above median share of female workers

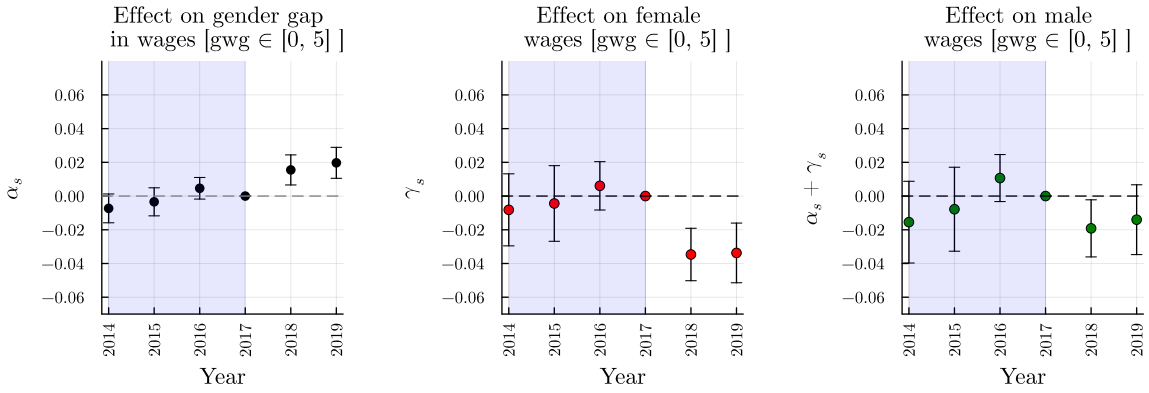


Conditional base gender gap in large firms = 0.25 (0.052)
By 2019, the conditional gender gap decreased by 11.76% to 0.221

Notes: The figure presents ATT estimates showing the effect of the pay equity law on gender gaps in wages (*left subplot*), on female wages (*middle subplot*), and on male wages (*right subplot*) in jobs where average pre-policy gender wage gaps were above five percent, in industries with above median share of female workers. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

Figure 11: Jobs where average pre-policy gender wage gaps were in between 0 and 5%

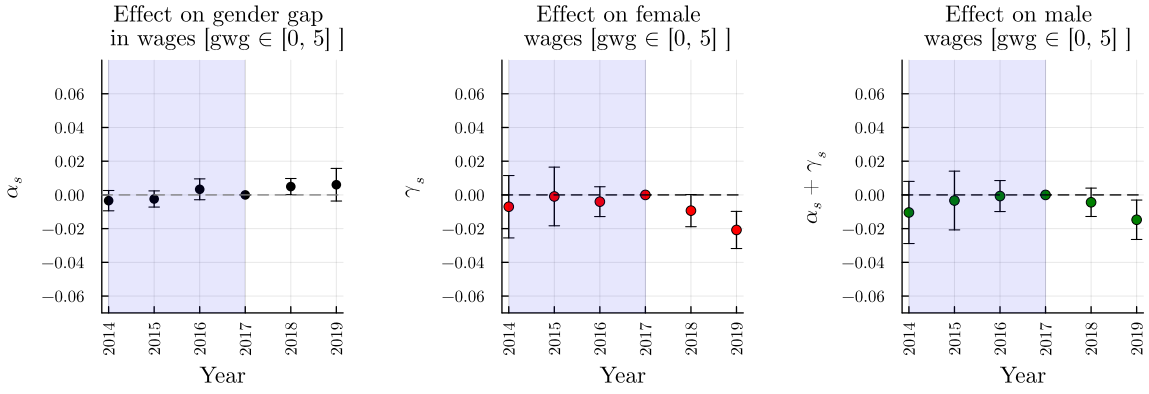
(a) Industries with above median share of male workers



Conditional base gender gap in large firms
= 0.061 (0.021)
By 2019, the conditional gender gap
increased by 31.91% to 0.08

Notes: The figure presents ATT estimates showing the effect of the pay equity law on gender gaps in wages (*left subplot*), on female wages (*middle subplot*), and on male wages (*right subplot*) in jobs where average pre-policy gender wage gaps were in between zero and five percent, in industries with above median share of male workers. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

(b) Industries with above median share of female workers

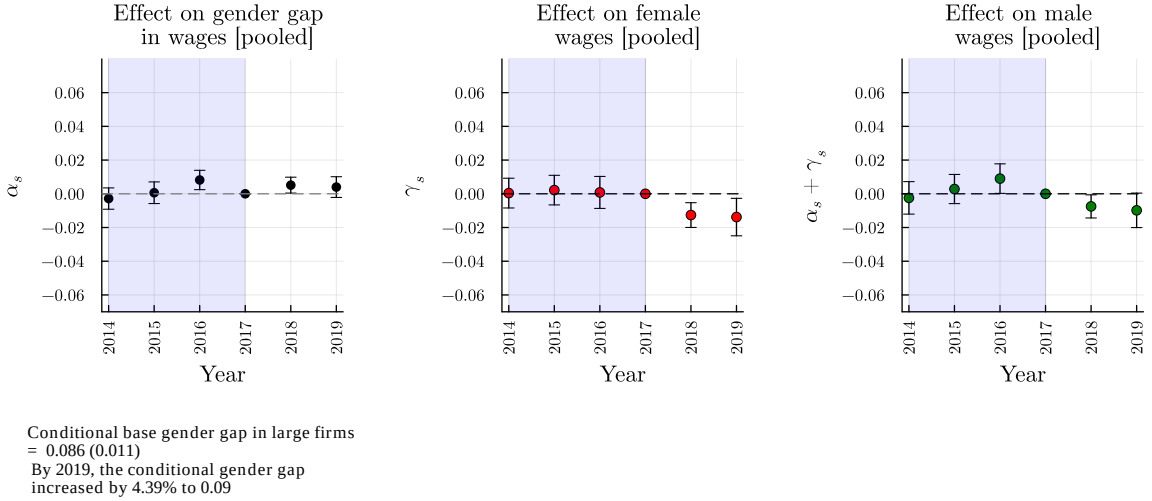


Conditional base gender gap in large firms
= 0.101 (0.033)
By 2019, the conditional gender gap
increased by 5.52% to 0.107

Notes: The figure presents ATT estimates showing the effect of the pay equity law on gender gaps in wages (*left subplot*), on female wages (*middle subplot*), and on male wages (*right subplot*) in jobs where average pre-policy gender wage gaps were in between zero and five percent, in industries with above median share of female workers. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

examining Figure 12, we observe that, both before and after the policy, there is no significant impact of the pay equity law on the evolution of the gender wage gap between treated and untreated firms. The conditional baseline gender wage gap in large firms was approximately 8.6% in 2017 and their unconditional gap was 5.24%. This suggests that the law's varying impacts on firms with gender wage gaps in job titles that are both above and below the 5% target often are offsetting thereby appearing obscured when all workers are analyzed together.

Figure 12: Wage growth of all workers pooled together



Notes: The figure presents ATT estimates showing the effect of the pay equity law on gender gaps in wages (*left subplot*), on female wages (*middle subplot*), and on male wages (*right subplot*) in jobs where average pre-policy gender wage gaps were pooled sample. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

Furthermore, the second and third panels of Figure 12 show that wage growth for male and female workers in treated firms declines almost equally after the introduction of Law 60/2018. This masks the differential impact the law had on gender-specific wage growth, as presented in the corresponding panels of Figures 6, 7 and 8 from the estimates reported in columns 1, 2 and 3 of Table 3. This econometric evidence where the overall heterogeneity is largely obscured, underscores the importance of using subsamples to estimate the underlying policy effects based on initial wage gaps.

6.6 Intent-to-treat effects

To identify the intent to treat effects of the pay equity law, we redefine the treatment variable to ensure that it is pre-determined—as an indicator variable of whether a firm had more than 250 workers prior to the announcement of Law 60/2018. For worker i in 2017, the firm is redefined as treated if it had more than 250 employees in that year. We define the treatment dummy as follows.

$$D_{j(i,2017)} = \mathbb{1}[\#worker_{j(i,2017)} \geq 250]$$

Note that incorporating this time-invariant firm specific measure of treatment precludes

the inclusion of both a large firm effect and a firm fixed effect due to perfect collinearity, and results in the following estimating equation.[22]

$$
\begin{aligned}
y_{ijt} = \; & \theta_{equalwork(j(i))} \; + \; \sum_{s \in \mathcal{S}} \alpha_s^{itt} * D_{j(i,2017)} \times \mathbb{1}[t = s] \times Male_i \; + \; \sum_{s \in \mathcal{S}} \gamma_s^{itt} * D_{j(i,2017)} \times \theta_s^{itt} \\
& + \; \theta_{g(i),b(i)}^{itt} \tau^{itt} * D_{j(i,2017)} \times Male_i \; + \; \delta^{itt} Male_i \\
& + \; X'_{ij(i,t)t} \beta^{itt} \; + \; \delta_{CBA \times t}^{itt} \; + \; \delta_{ind}^{itt} \; + \; + \; e_{ijt}^{itt}
\end{aligned}
$$

We can interpret estimates of α_s^{itt} as intent-to-treat policy effects for each time period s and the remaining variables are defined as in our main estimating equation for the average treatment effect. This estimating equation differs from equation 1 and we cannot obtain an estimate of the conditional wage gap between large and small firms in 2017, since a large firm effect is not separately identified.

We report the event study estimates in Figure 16 and Table 4. We find that the ITT estimates are not observationally different from the ATT estimates. This is primarily driven by the fact that only 1.73% of firms experience sufficient churn thereby changing their treatment status—plausibly exogenous to the law as discussed in Section 4—to fall on either side of the 250 worker firm size threshold over the entire period.

6.7 Additional exercises and robustness checks

In this subsection, we address additional concerns and present evidence demonstrating that our main results remain robust to these issues.

6.7.1 Impact on hours worked by male and female workers

Changes in hourly wages could potentially be driven by variations in hours worked. To address this concern, we estimate the impact of the law on hours worked by workers. We report the ATT and ITT estimates respectively in Figures 17 and 18 and in tables 5 and 6. We find neither economic nor statistically significant effects on hours worked by both male and female workers and consequently no impact on the gender gap in hours worked. This is reassuring and suggests that the policy did not induce firms to change hours worked by workers to offset changes in hourly wages.

6.7.2 Impact on gender composition within jobs

Firms could may have also responded to the law by endogenously changing the gender composition within jobs. To address this concern, we estimate the impact of the law on the proportion of females (or males) within jobs. We report the ATT estimates in Table 11 and Figure 22. We find precise null effects on the gender composition within jobs. This alleviates concerns

[22] Some parameters in this setup will not be identified by construction. Specifically, compared to the estimation of the average treatment on the treated, we cannot identify the effect of being in a large firm. As a result, we do not have estimates for the conditional gender wage gap between large and small firms for either male or female workers in 2017.

that any changes in the wage growth that we observe are driven by changes in the reallocation of male and female workers across jobs.

6.7.3 Potential measurement error in treatment

In the data, firm size is observed at a single point in time each year. However, firms with workforce sizes close to 250 workers may fluctuate above or below it within a given year due to regular churn and unanticipated employment shocks, independent of the law.[23] Such movement could result in these firms being exposed to different treatment statuses within the same year. This irregularity in treatment status may induce misclassification of treatment. Consequently, our estimates of the law's impact would be attenuated towards zero, with the degree of attenuation depending on the extent of unobserved variance in firms' differential treatment status throughout the year.

To address this potential concern, we remove firms employing between 240 and 260 workers and re-estimate equation 1 on the remaining sample. We argue that for firms well above or well below the 250-worker threshold are unlikely to experience exogenous employment shocks that would change their treatment status. Our results are reported in Table 8 and Figure 20. Notice that our main findings are robust to the exclusion of observations that may be subject to misclassification. Additionally, we conducted further robustness checks by excluding firms with between 220 and 280 employees, as shown in Table 9 Figure 21.

6.7.4 Potential concern on endogenous mobility

Our results may be affected if the policy induced gender differential mobility relative to the pre-policy periods. Employees often move because of an associated wage increment, an expectation thereof, or due to some non-pecuniary benefit. It is possible that the policy induced more females to sort into larger firms in expectation of higher wage growth or reduced the mobility of existing female workers in large firms.

To examine whether our results are driven by differential mobility of workers, we re-estimate our empirical model using a restricted sample of workers who did not change firms during the entire sample period (2014-2019). As shown in Figure 19 and Table 7, our main results—previously estimated with the full sample—remain consistent in sign, magnitude, and statistical significance. This suggests that endogenous mobility, if present, does not drive our core findings.

7 Plausible mechanisms driving the unintended consequences of the pay-equity law

We discuss three mechanisms that may underlie our main results, each stemming from the non-linear cost structure imposed by fines if a firm's gender wage gap both exceeded 5% and

[23] Note that such labor market dynamics are not specific to firms of size around 250, rather these are common around any workforce size.

remained unaddressed. The enforcement of Law 60/2018 was strict and immediate, eliminating uncertainties about potential penalties for having gender pay disparities. Consequently, this abrupt non-linear shift in anticipated costs influenced firms' wage-setting policies. Although data limitations prevent us from separately identifying the relative importance of each mechanism, we discuss them individually due to their distinct welfare implications.

7.1 Risk aversion of firms

Prior to the implementation of Law 60/2018, risk-averse firms were plausibly more likely to maintain smaller gender wage gaps due to uncertainties about potential repercussions in an undefined regulatory environment. Risk-averse firms would have tended to err on the side of caution, minimizing wage disparities to avoid possible scrutiny. However, by establishing a salient 5% threshold, the policy eliminated the ambiguities that previously constrained the actions of risk-averse firms. Instead of cautiously avoiding an undefined wage gap, such risk-averse firms can now easily adjust their wage policies with greater precision, ensuring their gender wage gaps remain below the explicitly specified 5% threshold.

7.2 Compensating differentials

Given the extensive literature on gender differences in the valuation of non-wage amenities (see e.g., Wiswall & Zafar (2018)), it is plausible that female workers might still be willing to supply labor at lower wage growth in exchange for better or more non-wage amenities. Therefore, firms could potentially increase their wage gaps by offering additional amenities or benefits to female employees, while maintaining that overall compensation remains equitable after incorporating non-wage amenities. As long as the cost of providing these amenities is lower than the wage savings, firms could adjust wages while staying within the target gender wage gap. However, identifying the impact of time-varying, unobserved amenities on wage inequality remains challenging without additional exogenous product market variations (Lamadon, Mogstad & Setzler 2022), observable data on non-wage amenities (Dey & Flinn 2005), or exogenous variations thereof (Mas & Pallais (2017), Wiswall & Zafar (2018), Alam et al. (2023)).

7.3 Taste based discrimination

In perfectly competitive labor markets, discriminatory firms would exit the market because labor can move freely to their preferred employer in absence of any friction. However, growing evidence strongly suggests that labor markets are monopsonistic (see, e.g., Card (2022)), it is plausible that the labor market consists of a non-negligible mass of discriminatory firms.

If so, then in such firms the equilibrium gender wage gaps would result from balancing the firm's preferences to discriminate (see, e.g., Becker (1957)) with their expected cost of discrimination. Firms with gender wage gaps below 5% prior to 2018 would face a lower expected cost to discriminate after Law 60/2018. These firms could then increase the gender wage gap by reducing female wage growth. Additionally, labor market frictions may limit workers from switching to their most preferred employer in the presence of discrimination. As long as there

exists a non-zero mass of discriminatory firms with low baseline gender wage gaps, such preferences to discriminate could explain why we find that gender wage gaps increased in firms that were initially below the threshold gap. Finally, we want to stress that our evidence of an average reduction in male wage growth—albeit much smaller than that of female coworkers—strongly suggests that not all firms with low baseline gaps engaged in discriminatory practices.

Understanding the dominant underlying mechanism that underlies our main results is important to make welfare statements on the consequence of these laws. For example, if the mechanism of compensating differentials were dominant then the welfare implications would significantly differ than if the mechanism of taste based discrimination were dominant. Future research should work towards separating these the underlying mechanisms.

8 Conclusion

To address longstanding gender-based employment discrimination, many countries have implemented policies to promote wage equality for "equal work." However, evidence of the causal impact of such laws on the gender wage gap remains limited, partly due to their uniform mandates. In 2018, Portugal introduced one of the most comprehensive pay equity laws, requiring employers to submit employee-level pay data to the government. If analysts detected an unexplained gender pay gap exceeding 5% within job titles and occupations, employers were mandated to develop pay remediation plans or face penalties.

Using matched employee-employer data from Portugal, we define "equal work" as employment within the same firm, occupation, job title, and collective bargaining agreement (CBA). The policy initially applied to firms with more than 250 employees—a variation we leverage in an event study design with an “equal work” fixed effect to estimate its impact on workers. Recognizing that the policy effects depend on pre-policy wage gaps, we analyze outcomes by groups with different initial gender wage gaps.

Our findings reveal substantial variation in policy impacts that aggregate firm-level analyses would obscure. In jobs with pre-existing gender wage gaps exceeding 5%, the policy reduced the gap by 9% primarily through reduced male wage growth. In the small fraction of jobs where women initially outearned men, the policy reduced the conditional negative gender wage gap by half. However, in jobs with baseline gaps between 0% and 5%, gender wage disparities widened due to slower wage growth for women. This unintended consequence was more pronounced in male-dominated industries, whereas female-dominated industries witnessed the intended effects.

This heterogeneity highlights complexities that diverge from pathways reported in Bailey et al. (2024). Possible mechanisms for these unintended outcomes include firm risk aversion, variations in worker bargaining power, the "ask gap," preferences for nonpecuniary benefits among women, and residual discriminatory practices. Further research is needed to disentangle these factors and evaluate the broader welfare implications.

A limitation of our study is the absence of worker productivity data, preventing analysis of wage disparities conditional on productivity. Nevertheless, we find it unlikely that firms with

larger initial gender gaps also had proportionate productivity gaps. Thus, our results likely reflect factors beyond productivity differences.

The unintended consequences we identify are significant, especially as elements of Portugal's law, including the 5% gap target, will be adopted EU-wide by June 2026 for employers with 100+ employees. As governments work toward equal pay for equal work, our findings underscore how employers adapt when required to demonstrate pay equity, offering valuable insights for future policy design.

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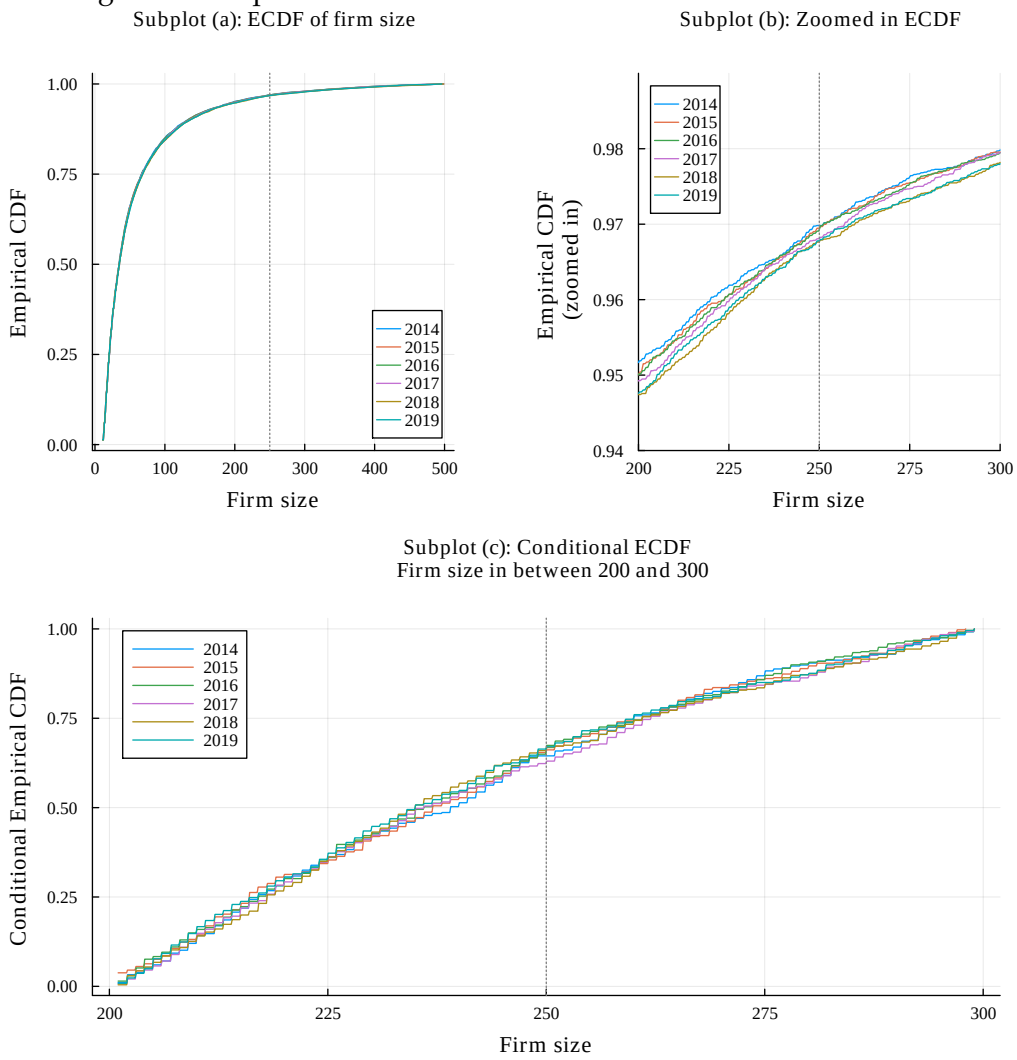
A Appendix-A

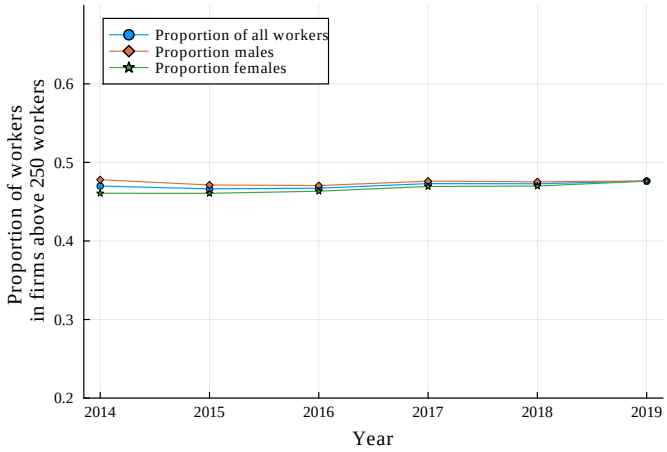
A.1 Tables and Figures

Figure 13: Empirical Cumulative Distribution of firm size over time



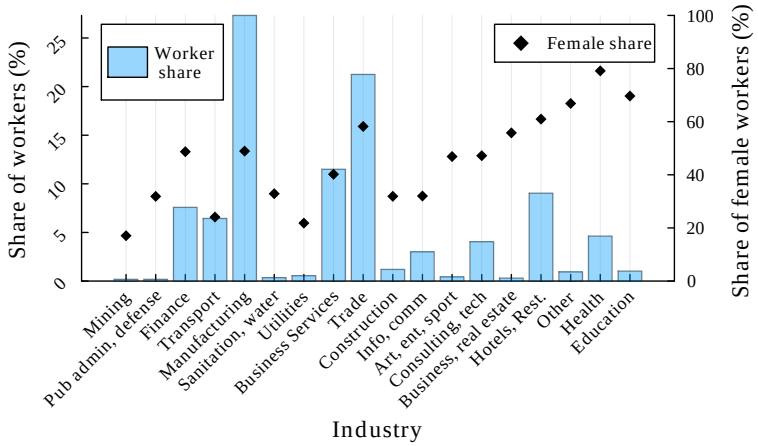
Notes: In Figure 13-(a) we plot the empirical cumulative distribution function (ECDF) of firm size for each year from 2014 to 2019. We zoom in on the part of the ECDF around the threshold of 250 workers in Figure 13-(b). In Figure 13-(c) we plot the conditional ECDF by conditioning on firm size being in between 200 and 300 workers.

Figure 14: Proportion of workers working in firms employing above 250 workers over time



Notes: This figure plots the proportion of workers working in firms employing above 250 workers over time, for all workers and also by gender of workers. The pay equity law was announced in 2018.

Figure 15: Share of all workers and female across industries



Notes: This figure plots the share of all workers by industry on the left y-axis represented by the blue bars, and the share of female workers on the right y-axis represented by the black dots. The industries are sorted in the descending order of the share of workers in jobs with positive gender wage gaps as in Figure 5.

Table 1: Summary Statistics in the Pre-Policy Period (2014-2017)

Panel A: All workers and by gender

	All			Female			Male		
	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N
Female	0.467	0.499	3,438,667	1.000	0.000	1,605,057	0.000	0.000	1,833,610
Firm size > 250	0.392	0.488	3,438,667	0.386	0.487	1,605,057	0.397	0.489	1,833,610
Monthly hours	169.016	8.488	3,438,667	168.192	9.110	1,605,057	169.738	7.832	1,833,610
Monthly wage	1223.916	1474.772	3,438,667	1049.919	762.874	1,605,057	1376.224	1876.079	1,833,610
Log hourly wage	1.803	0.536	3,438,667	1.692	0.486	1,605,057	1.900	0.558	1,833,610
Age	40.392	10.757	3,438,667	40.252	10.518	1,605,057	40.514	10.961	1,833,610
Tenure at firm	9.388	9.582	3,438,430	9.375	9.361	1,604,987	9.400	9.772	1,833,443

Panel B: By firm size

	Firm size below 250			Firm size above 250		
	Mean	Std. Dev.	N	Mean	Std. Dev.	N
Female	0.471	0.499	2,091,863	0.460	0.498	1,346,804
Firm size > 250	0.000	0.000	2,091,863	1.000	0.000	1,346,804
Monthly hours	169.740	7.814	2,091,863	167.892	9.329	1,346,804
Monthly wage	1138.189	1332.586	2,091,863	1357.067	1663.070	1,346,804
Log hourly wage	1.740	0.501	2,091,863	1.900	0.571	1,346,804
Age	40.498	10.865	2,091,863	40.226	10.585	1,346,804
Tenure at firm	8.732	9.319	2,091,667	10.408	9.891	1,346,763

Panel C: By pre-policy gender wage gap

	Above 5% GWG			Above 0% below 5% GWG			Below 0% GWG		
	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N
Female	0.452	0.498	848,777	0.529	0.499	620,754	0.542	0.498	591,613
Firm size > 250	0.527	0.499	848,777	0.621	0.485	620,754	0.443	0.497	591,613
Monthly hours	168.024	8.935	848,777	168.658	9.211	620,754	168.464	9.133	591,613
Monthly wage	1472.371	2049.881	848,777	1063.439	810.464	620,754	1194.032	983.112	591,613
Log hourly wage	1.969	0.590	848,777	1.695	0.492	620,754	1.780	0.547	591,613
Age	40.695	10.329	848,777	37.840	10.642	620,754	38.410	10.692	591,613
Tenure at firm	10.531	9.905	848,731	7.497	8.388	620,739	7.408	8.613	591,597

Notes: This table reports worker-year summary statistics in the pre-policy years of 2014-2017, categorized by overall worker data and gender (Panel A), firm size (Panel B), and gender wage gap (Panel C). The sample consists of all full time workers in Portugal, aged between 19 and 65 who are covered by a Collective Bargaining Agreement in the pre-policy period (2014-2017). In Panel C, by construction, this sample only has workers who work in jobs in which workers of both genders were employed, such that a gender wage gap is defined. Tenure at firm will have lower number of total observations because it is not defined for new hires until they complete a year at the firm.

Table 2: Unconditional Gender Wage Gaps (%) by Firm Size and Gap Category, 2017

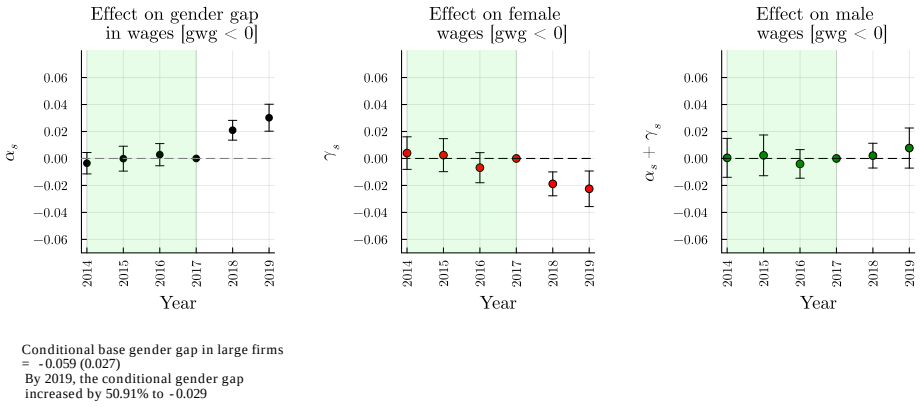
Category	Firm size < 250	Firm size > 250	Pooled Sample
All Firms	5.24	5.06	5.14
Jobs with Gap > 5%	18.14	13.56	15.67
Jobs with Gap < 0%	-8.48	-4.79	-6.75
Jobs with $0\% \leq$ Gap $\leq 5\%$	2.14	2.19	2.17

Note: The table reports unconditional gender wage gaps within jobs categorized based on their average gender wage gap in 2017.

A.2 Intent-to-treat effects

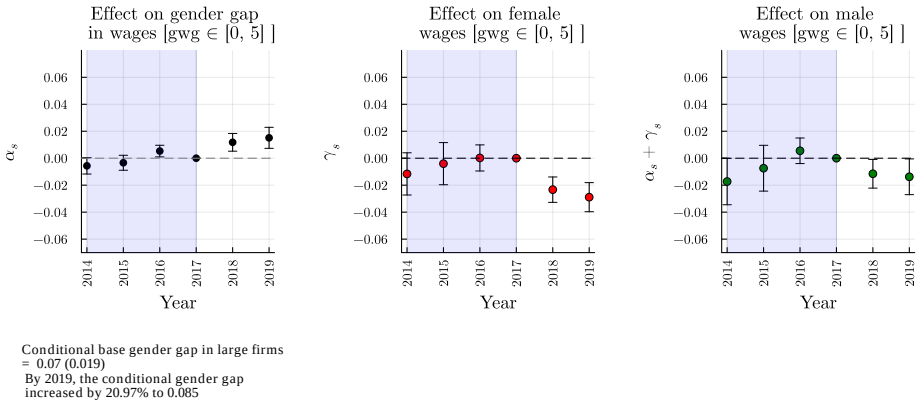
Figure 16: Event-study ITT estimates on wages

(a) Jobs where average pre-policy gender wage gap was below zero percent



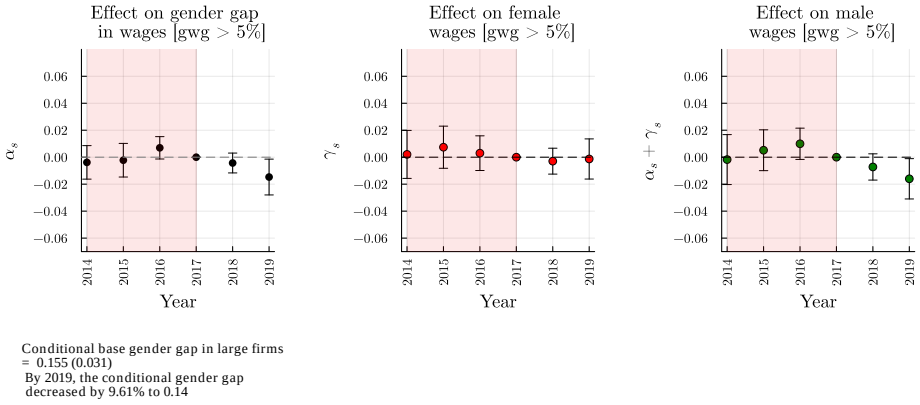
Notes: The figure presents ITT estimates showing the effect of the pay equity law on gender gaps in wages (*left subplot*), on female wages (*middle subplot*), and on male wages (*right subplot*) in jobs where average pre-policy gender wage gaps were below zero percent. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

(b) Jobs where average pre-policy gender wage gap was in between zero and five percent



Notes: The figure presents ITT estimates showing the effect of the pay equity law on gender gaps in wages (*left subplot*), on female wages (*middle subplot*), and on male wages (*right subplot*) in jobs where average pre-policy gender wage gaps were in between zero and five percent. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

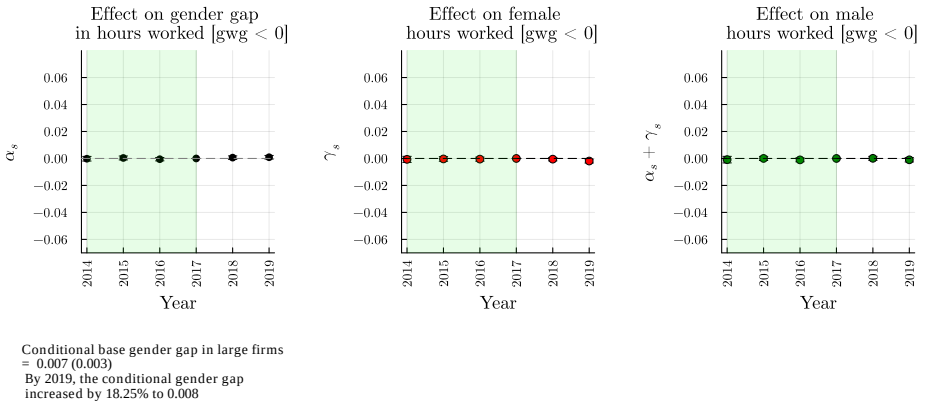
(c) Jobs where average pre-policy gender wage gap was above five percent



Notes: The figure presents ITT estimates showing the effect of the pay equity law on gender gaps in wages (*left subplot*), on female wages (*middle subplot*), and on male wages (*right subplot*) in jobs where average pre-policy gender wage gaps were above five percent. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

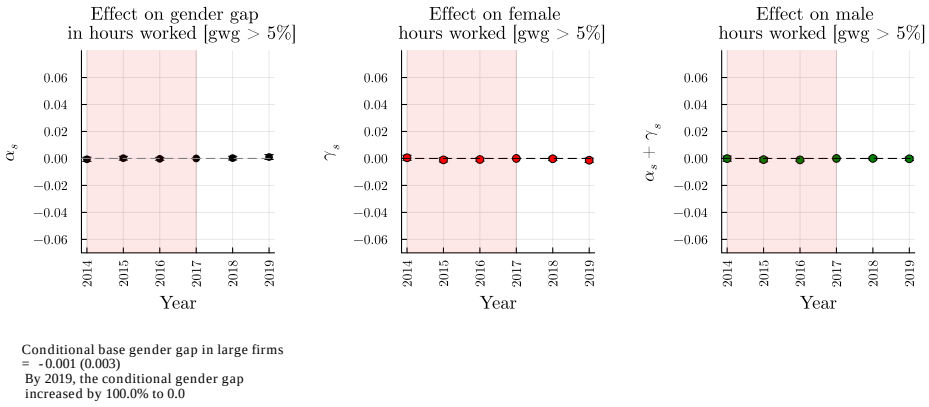
Figure 17: Event-study ATT estimates on hours worked

(a) Jobs where average pre-policy gender wage gap was below zero percent



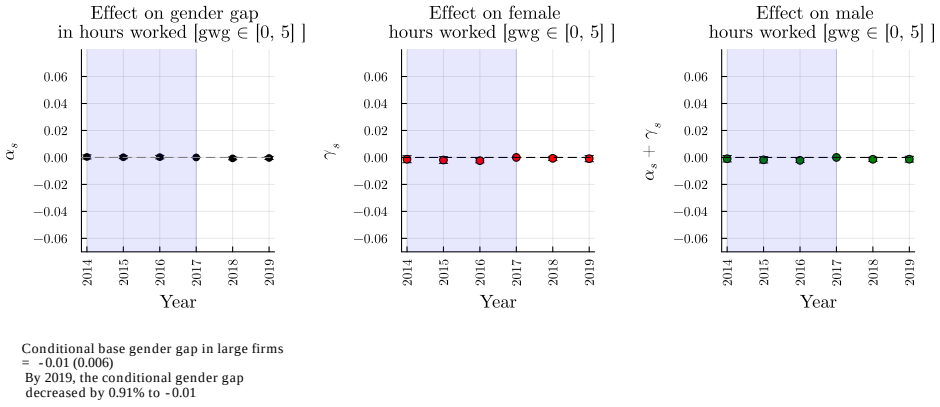
Notes: The figure presents ATT estimates showing the effect of the pay equity law on gender gaps in hours worked (*left subplot*), on female hours worked (*middle subplot*), and on male hours worked (*right subplot*) in jobs where average pre-policy gender wage gaps were below zero percent. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

(b) Jobs where average pre-policy gender wage gap was above five percent



Notes: The figure presents ATT estimates showing the effect of the pay equity law on gender gaps in hours worked (*left subplot*), on female hours worked (*middle subplot*), and on male hours worked (*right subplot*) in jobs where average pre-policy gender wage gaps were above five percent. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

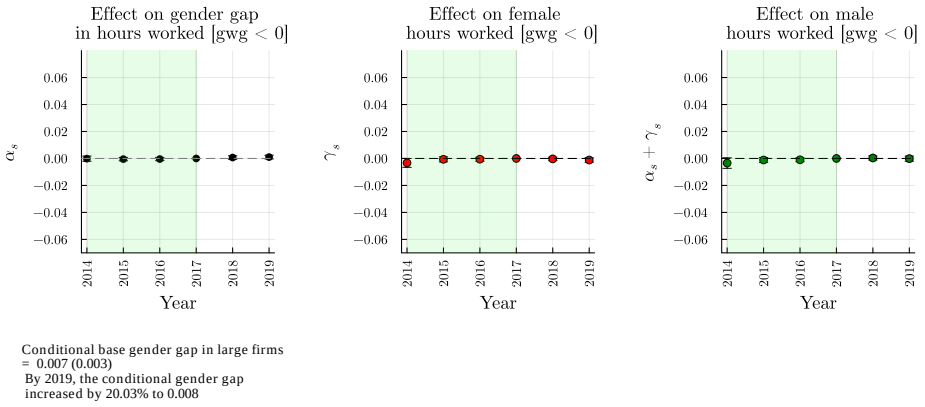
(c) Jobs where average pre-policy gender wage gap was in between zero and five percent



Notes: The figure presents ATT estimates showing the effect of the pay equity law on gender gaps in hours worked (*left subplot*), on female hours worked (*middle subplot*), and on male hours worked (*right subplot*) in jobs where average pre-policy gender wage gaps were in between zero and five percent. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

Figure 18: Event-study ITT estimates on hours worked

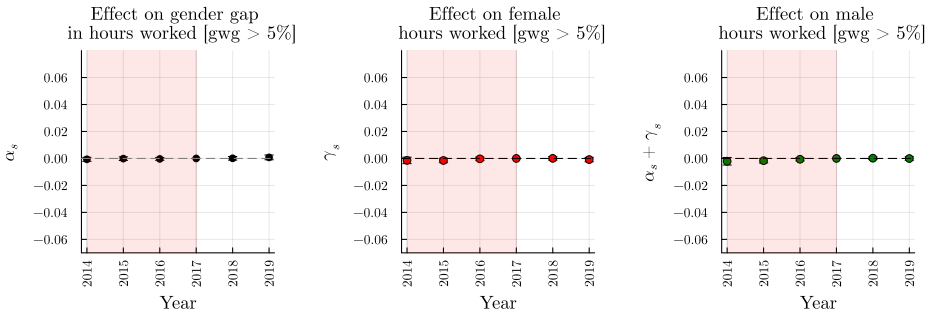
(a) Jobs where average pre-policy gender wage gap was below zero percent



Conditional base gender gap in large firms
= 0.007 (0.003)
By 2019, the conditional gender gap
increased by 20.03% to 0.008

Notes: The figure presents ITT estimates showing the effect of the pay equity law on gender gaps in hours worked (*left subplot*), on female hours worked (*middle subplot*), and on male hours worked (*right subplot*) in jobs where average pre-policy gender wage gaps were below zero percent. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

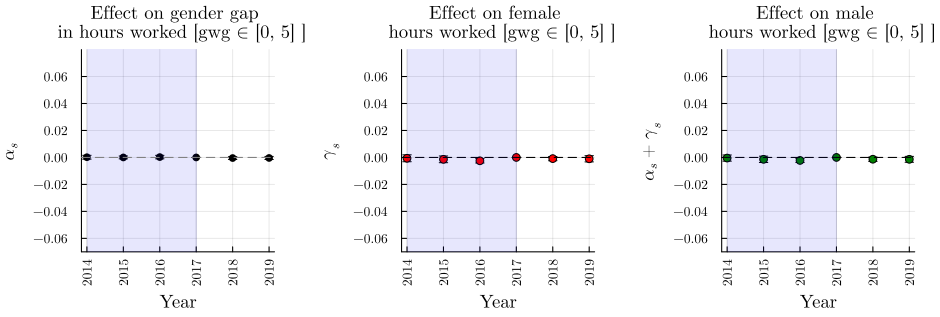
(b) Jobs where average pre-policy gender wage gap was above five percent



Conditional base gender gap in large firms
= -0.001 (0.003)
By 2019, the conditional gender gap
increased by 100.0% to -0.0

Notes: The figure presents ITT estimates showing the effect of the pay equity law on gender gaps in hours worked (*left subplot*), on female hours worked (*middle subplot*), and on male hours worked (*right subplot*) in jobs where average pre-policy gender wage gaps were above five percent. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

(c) Jobs where average pre-policy gender wage gap was in between zero and five percent



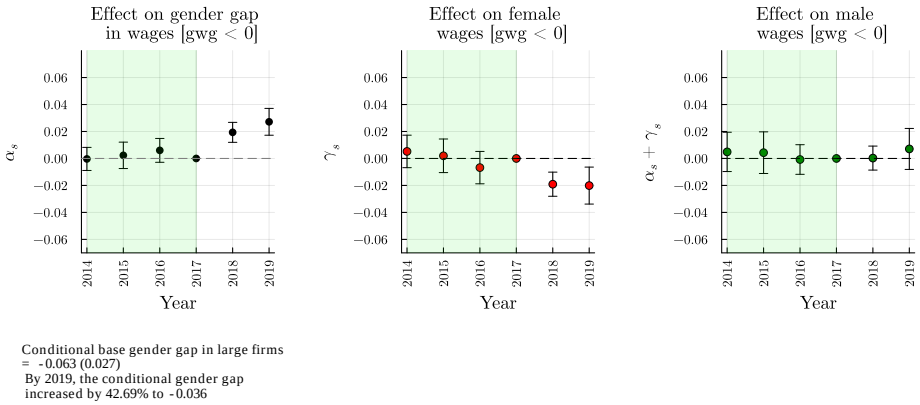
Conditional base gender gap in large firms
= -0.01 (0.006)
By 2019, the conditional gender gap
decreased by 0.06% to -0.01

Notes: The figure presents ITT estimates showing the effect of the pay equity law on gender gaps in hours worked (*left subplot*), on female hours worked (*middle subplot*), and on male hours worked (*right subplot*) in jobs where average pre-policy gender wage gaps were in between zero and five percent. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

A.3 Stayers

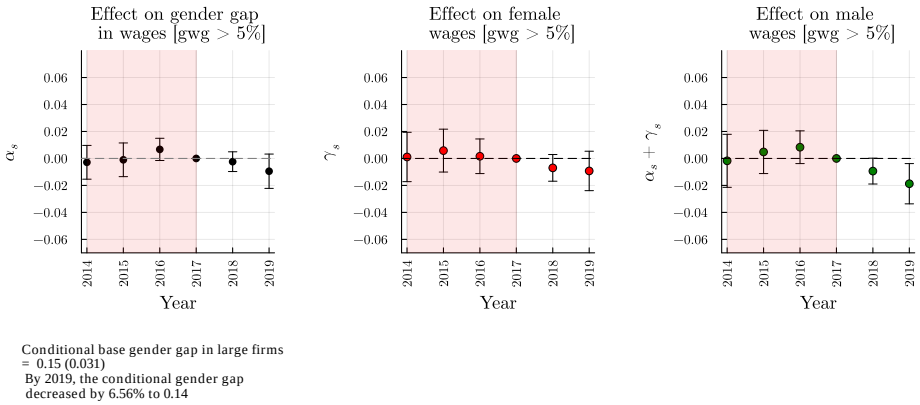
Figure 19: Event-study ATT estimates on wages of stayers

(a) Jobs where average pre-policy gender wage gap was below zero percent



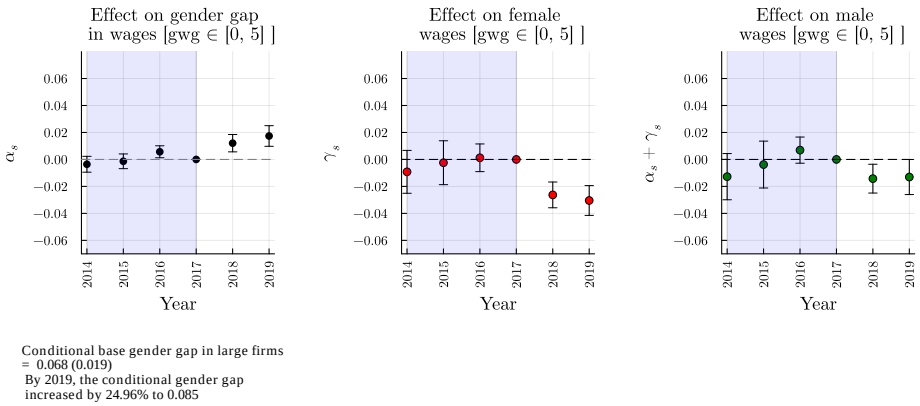
Notes: The figure presents ATT estimates showing the effect of the pay equity law on gender gaps in wages (*left subplot*), on female wages (*middle subplot*), and on male wages (*right subplot*) in jobs where average pre-policy gender wage gaps were below zero percent. This sample of workers did not change jobs during the sample period.. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

(b) Jobs where average pre-policy gender wage gap was above five percent



Notes: The figure presents ATT estimates showing the effect of the pay equity law on gender gaps in wages (*left subplot*), on female wages (*middle subplot*), and on male wages (*right subplot*) in jobs where average pre-policy gender wage gaps were above five percent. This sample of workers did not change jobs during the sample period.. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

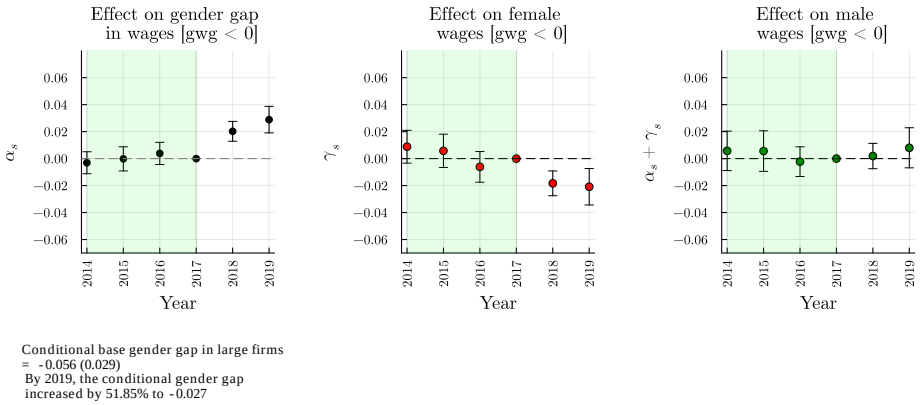
(c) Jobs where average pre-policy gender wage gap was in between zero and five percent



Notes: The figure presents ATT estimates showing the effect of the pay equity law on gender gaps in wages (*left subplot*), on female wages (*middle subplot*), and on male wages (*right subplot*) in jobs where average pre-policy gender wage gaps were in between zero and five percent. This sample of workers did not change jobs during the sample period.. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

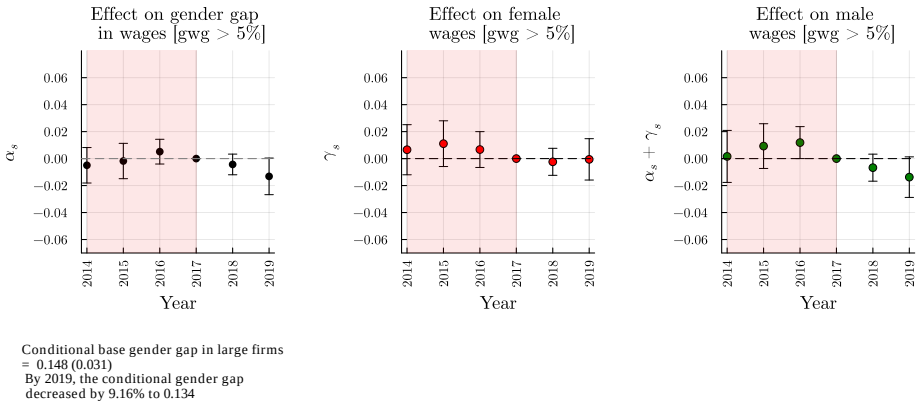
Figure 20: Event-study ATT estimates on hours after removing firms employing in between 240 and 260 workers

(a) Jobs where average pre-policy gender wage gap was below zero percent



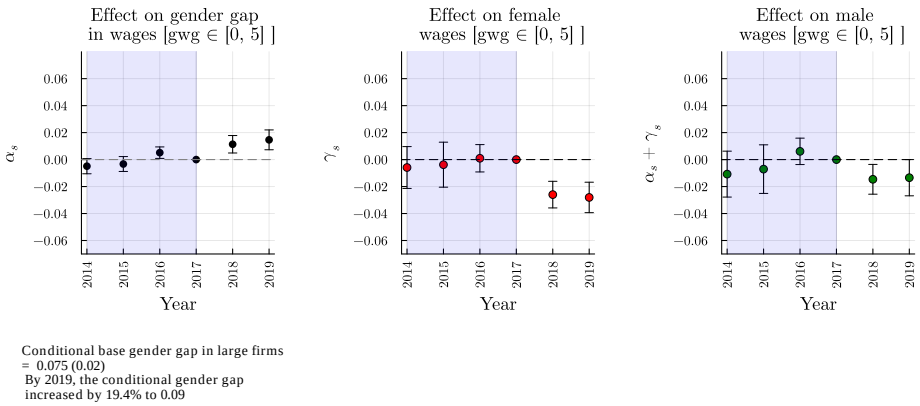
Notes: The figure presents ATT estimates showing the effect of the pay equity law on gender gaps in wages (*left subplot*), on female wages (*middle subplot*), and on male wages (*right subplot*) in jobs where average pre-policy gender wage gaps were below zero percent. This sample removes firms employing in between 240 and 260 workers.. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

(b) Jobs where average pre-policy gender wage gap was above five percent



Notes: The figure presents ATT estimates showing the effect of the pay equity law on gender gaps in wages (*left subplot*), on female wages (*middle subplot*), and on male wages (*right subplot*) in jobs where average pre-policy gender wage gaps were above five percent. This sample removes firms employing in between 240 and 260 workers.. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

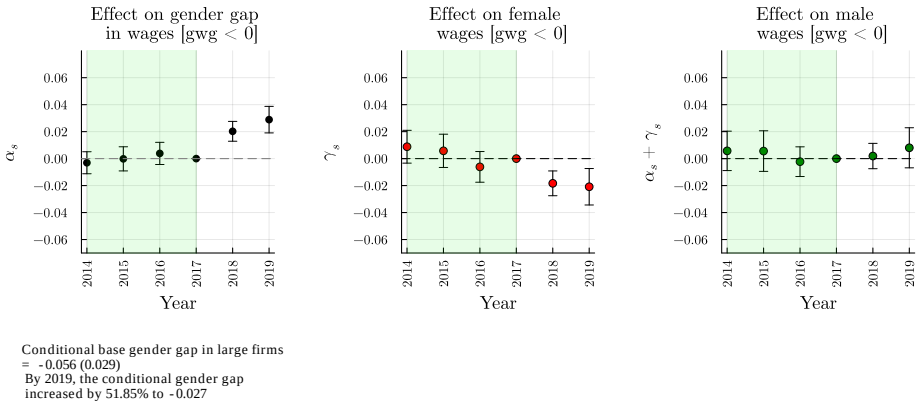
(c) Jobs where average pre-policy gender wage gap was in between zero and five percent



Notes: The figure presents ATT estimates showing the effect of the pay equity law on gender gaps in wages (*left subplot*), on female wages (*middle subplot*), and on male wages (*right subplot*) in jobs where average pre-policy gender wage gaps were in between zero and five percent. This sample removes firms employing in between 240 and 260 workers.. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

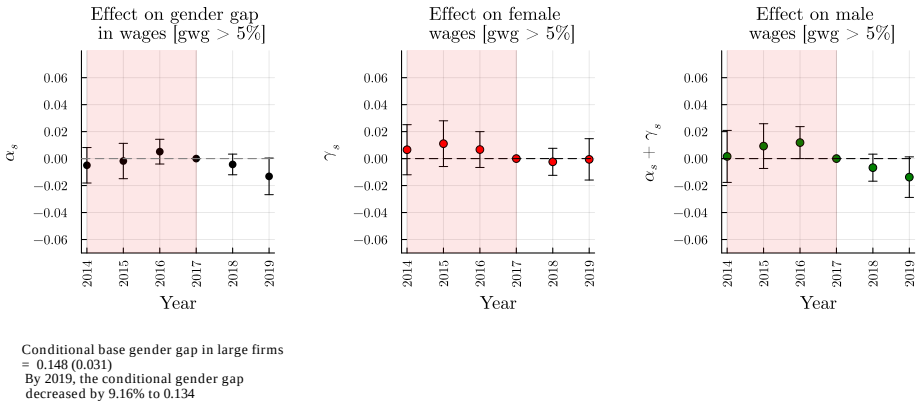
Figure 21: Event-study ATT estimates on wages after removing firms employing in between 220 and 280 workers

(a) Jobs where average pre-policy gender wage gap was below zero percent



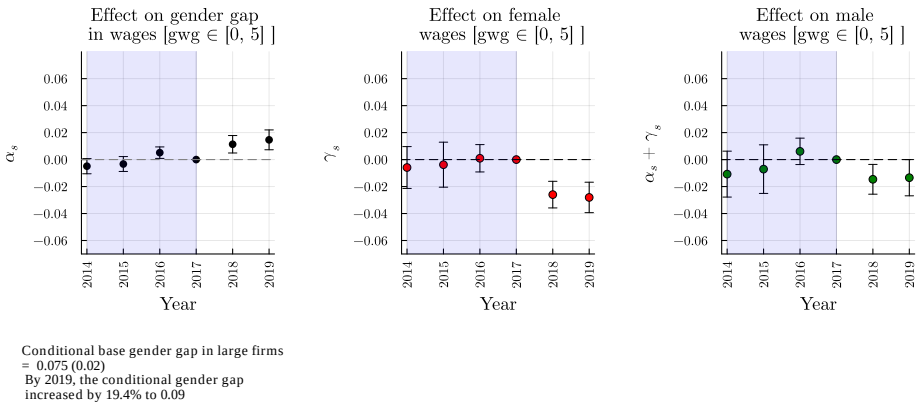
Notes: The figure presents ATT estimates showing the effect of the pay equity law on gender gaps in wages (*left subplot*), on female wages (*middle subplot*), and on male wages (*right subplot*) in jobs where average pre-policy gender wage gaps were below zero percent. This sample removes firms employing in between 220 and 280 workers.. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

(b) Jobs where average pre-policy gender wage gap was above five percent



Notes: The figure presents ATT estimates showing the effect of the pay equity law on gender gaps in wages (*left subplot*), on female wages (*middle subplot*), and on male wages (*right subplot*) in jobs where average pre-policy gender wage gaps were above five percent. This sample removes firms employing in between 220 and 280 workers.. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

(c) Jobs where average pre-policy gender wage gap was in between zero and five percent



Notes: The figure presents ATT estimates showing the effect of the pay equity law on gender gaps in wages (*left subplot*), on female wages (*middle subplot*), and on male wages (*right subplot*) in jobs where average pre-policy gender wage gaps were in between zero and five percent. This sample removes firms employing in between 220 and 280 workers.. The x-axis shows years from 2014-2019, with 2017 as the reference year and the shaded area indicates pre-policy years. The y-axis shows the coefficient estimates with 95% confidence intervals.

A.4 Estimation tables of robustness and additional exercises

Table 3: ATT Estimates of the effect of the pay-equity law on log hourly wages

	$GWG_{prepolicy} < 0\%$ (1)	$GWG_{prepolicy} \in [0\%, 5\%]$ (2)	$GWG_{prepolicy} > 5\%$ (3)	Pooled (4)
D_{jt}	0.00	0.024***	0.027**	0.018***
	(0.008)	(0.007)	(0.012)	(0.005)
Male	-0.081***	0.078***	0.211***	0.096***
	(0.027)	(0.019)	(0.025)	(0.011)
Female wages in 2019	-0.021***	-0.028***	-0.004	-0.014**
	(0.007)	(0.005)	(0.007)	(0.006)
Female wages in 2018	-0.019***	-0.025***	-0.003	-0.013***
	(0.005)	(0.005)	(0.005)	(0.004)
Female wages in 2016	-0.006	0.001	0.004	0.00
	(0.006)	(0.005)	(0.007)	(0.005)
Female wages in 2015	0.003	-0.003	0.009	0.002
	(0.006)	(0.008)	(0.008)	(0.004)
Female wages in 2014	0.006	-0.008	0.005	0.00
	(0.006)	(0.008)	(0.009)	(0.005)
$D_{jt} \times$ Male	0.022***	-0.008***	-0.056***	-0.010***
	(0.004)	(0.002)	(0.011)	(0.004)
Gender wage gap in 2019	0.029***	0.014***	-0.014**	0.004
	(0.005)	(0.004)	(0.007)	(0.003)
Gender wage gap in 2018	0.020***	0.011***	-0.005	0.005**
	(0.004)	(0.003)	(0.004)	(0.002)
Gender wage gap in 2016	0.004	0.005**	0.006	0.008***
	(0.004)	(0.002)	(0.004)	(0.003)
Gender wage gap in 2015	0.00	-0.003	-0.001	0.00
	(0.005)	(0.003)	(0.006)	(0.003)
Gender wage gap in 2014	-0.003	-0.005*	-0.005	-0.003
	(0.004)	(0.003)	(0.006)	(0.003)
Male wages in 2019	0.007	-0.013**	-0.019*	-0.01
	(0.008)	(0.007)	(0.01)	(0.006)
Male wages in 2018	0.001	-0.014**	-0.008	-0.007*
	(0.006)	(0.006)	(0.006)	(0.004)
Male wages in 2016	-0.003	0.006	0.01	0.009
	(0.007)	(0.006)	(0.008)	(0.006)
Male wages in 2015	0.003	-0.006	0.007	0.003
	(0.008)	(0.009)	(0.01)	(0.006)
Male wages in 2014	0.004	-0.014	0.0	-0.002
	(0.007)	(0.009)	(0.011)	(0.006)
Equal work FE	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓
CBA-year FE	✓	✓	✓	✓
Dependent mean (all)	1.789	1.697	1.975	1.811
Dependent mean (untreated 2017)	1.743	1.607	1.87	1.753
N	840,093	1,321,856	1,214,541	5,216,271
R^2	0.907	0.917	0.882	0.902

Notes: The table above shows the ATT estimates from the event study design equation. D_{jt} is an indicator of firm size greater than 250 and *Male* is an indicator for whether the worker is male. The coefficient of the interaction of D_{jt} with the indicator of each year t and the indicator of *Male* gives the estimate of the gender pay equity law on gender wage gaps in that year. The coefficient of the interaction of D_{jt} with the indicator of each year t gives the effect of the pay equity law on female wages. All equations control for observable characteristics X_{ijt} that matter for wage setting as per the institutional details and are described in the text. Standard errors are clustered at the firm level.

Table 4: ITT Estimates of the effect of the pay-equity law on log hourly wages

	$GWG_{prepolicy} < 0\%$	$GWG_{prepolicy} \in [0\%, 5\%]$	$GWG_{prepolicy} > 5\%$	Pooled
	(1)	(2)	(3)	(4)
Male	-0.081***	0.078***	0.211***	0.096***
	(0.027)	(0.019)	(0.025)	(0.011)
$D_j \times 1[t = 2019]$	-0.022***	-0.029***	-0.001	-0.015***
	(0.007)	(0.006)	(0.008)	(0.006)
$D_j \times 1[t = 2018]$	-0.019***	-0.023***	-0.003	-0.013***
	(0.005)	(0.005)	(0.005)	(0.004)
$D_j \times 1[t = 2016]$	-0.007	0.00	0.003	0.00
	(0.006)	(0.005)	(0.007)	(0.005)
$D_j \times 1[t = 2015]$	0.002	-0.004	0.007	0.002
	(0.006)	(0.008)	(0.008)	(0.004)
$D_j \times 1[t = 2014]$	0.004	-0.012	0.002	-0.002
	(0.006)	(0.008)	(0.009)	(0.004)
$D_j \times$ Male	0.022***	-0.008***	-0.056***	-0.010**
	(0.004)	(0.002)	(0.011)	(0.004)
$D_j \times 1[t = 2019] \times$ Male	0.030***	0.015***	-0.015**	0.004
	(0.005)	(0.004)	(0.007)	(0.003)
$D_j \times 1[t = 2018] \times$ Male	0.021***	0.012***	-0.004	0.006**
	(0.004)	(0.003)	(0.004)	(0.002)
$D_j \times 1[t = 2016] \times$ Male	0.003	0.005**	0.007*	0.008***
	(0.004)	(0.002)	(0.004)	(0.003)
$D_j \times 1[t = 2015] \times$ Male	-0.00	-0.003	-0.002	0.001
	(0.005)	(0.003)	(0.006)	(0.003)
$D_j \times 1[t = 2014] \times$ Male	-0.004	-0.006*	-0.004	-0.003
	(0.004)	(0.003)	(0.006)	(0.003)
Male wages in 2019	0.008	-0.014**	-0.016	-0.011*
	(0.008)	(0.007)	(0.01)	(0.006)
Male wages in 2018	0.002	-0.012**	-0.007	-0.007
	(0.006)	(0.006)	(0.006)	(0.004)
Male wages in 2016	-0.004	0.006	0.01	0.008
	(0.007)	(0.005)	(0.008)	(0.006)
Male wages in 2015	0.002	-0.007	0.005	0.003
	(0.008)	(0.008)	(0.01)	(0.006)
Male wages in 2014	0.0	-0.017**	-0.002	-0.005
	(0.007)	(0.009)	(0.011)	(0.005)
Equal work FE	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓
CBA-year FE	✓	✓	✓	✓
Dependent mean (all)	1.789	1.697	1.975	1.811
Dependent mean (untreated 2017)	1.743	1.607	1.87	1.753
N	840,093	1,321,856	1,214,541	5,216,271
R^2	0.907	0.917	0.882	0.902

Notes: The table above shows the ITT estimates from the event study design equation. D_j is an indicator of firm size greater than 250 in 2017 and *Male* is an indicator for whether the worker is male. The coefficient of the interaction of D_j with the indicator of each year t and the indicator of *Male* gives the estimate of the gender pay equity law on gender wage gaps in that year. The coefficient of the interaction of D_j with the indicator of each year t gives the effect of the pay equity law on female wages. All equations control for observable characteristics X_{ijt} that matter for wage setting as per the institutional details and are described in the text. Standard errors are clustered at the firm level.

Table 5: ATT Estimates of the effect of the pay-equity law on log hours worked

	$GWG_{prepolicy} < 0\%$	$GWG_{prepolicy} \in [0\%, 5\%]$	$GWG_{prepolicy} > 5\%$	Pooled
	(1)	(2)	(3)	(4)
D_{jt}	0.001	-0.00	0.00	-0.00
	(0.001)	(0.001)	(0.00)	(0.00)
Male	0.005**	-0.011*	-0.00	-0.001
	(0.003)	(0.006)	(0.003)	(0.001)
Female wages in 2019	-0.002***	-0.00	-0.001	-0.001**
	(0.00)	(0.001)	(0.00)	(0.00)
Female wages in 2018	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Female wages in 2016	-0.00	-0.002***	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Female wages in 2015	-0.00	-0.002	-0.001	-0.00
	(0.00)	(0.001)	(0.00)	(0.00)
Female wages in 2014	-0.00	-0.001	0.00	-0.00
	(0.001)	(0.001)	(0.00)	(0.00)
$D_{jt} \times$ Male	0.002*	0.001**	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Gender wage gap in 2019	0.00	-0.00	0.001	0.00*
	(0.00)	(0.00)	(0.00)	(0.00)
Gender wage gap in 2018	0.00	-0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Gender wage gap in 2016	-0.00	0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Gender wage gap in 2015	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Gender wage gap in 2014	-0.00	0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Male wages in 2019	-0.001	-0.001	-0.0	-0.0
	(0.001)	(0.001)	(0.001)	(0.001)
Male wages in 2018	0.0	-0.001	0.0	-0.0
	(0.001)	(0.001)	(0.001)	(0.001)
Male wages in 2016	-0.001	-0.002**	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.0)
Male wages in 2015	0.0	-0.002	-0.001	-0.0
	(0.001)	(0.001)	(0.001)	(0.001)
Male wages in 2014	-0.001	-0.001	-0.0	-0.0
	(0.001)	(0.002)	(0.001)	(0.001)
Equal work FE	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓
CBA-year FE	✓	✓	✓	✓
Dependent mean (all)	5.126	5.127	5.124	5.129
Dependent mean (untreated 2017)	5.129	5.132	5.131	5.133
N	840,093	1,321,856	1,214,541	5,216,271
R^2	0.472	0.413	0.548	0.475

Notes: The table above shows the ITT estimates from the event study design equation. D_j is an indicator of firm size greater than 250 in 2017 and *Male* is an indicator for whether the worker is male. The coefficient of the interaction of D_j with the indicator of each year t and the indicator of *Male* gives the estimate of the gender pay equity law on gender wage gaps in that year. The coefficient of the interaction of D_j with the indicator of each year t gives the effect of the pay equity law on female wages. All equations control for observable characteristics X_{ijt} that matter for wage setting as per the institutional details and are described in the text. Standard errors are clustered at the firm level.

Table 6: ITT Estimates of the effect of the pay-equity law on log hours worked

	$GWG_{prepolicy} < 0\%$	$GWG_{prepolicy} \in [0\%, 5\%]$	$GWG_{prepolicy} > 5\%$	Pooled
	(1)	(2)	(3)	(4)
Male	0.005**	-0.011*	-0.00	-0.001
	(0.003)	(0.006)	(0.003)	(0.001)
$D_j \times 1[t = 2019]$	-0.001	-0.001	-0.00	-0.00
	(0.00)	(0.001)	(0.00)	(0.00)
$D_j \times 1[t = 2018]$	-0.00	-0.00	0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
$D_j \times 1[t = 2016]$	-0.00	-0.002***	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
$D_j \times 1[t = 2015]$	-0.00	-0.001	-0.002*	-0.00
	(0.00)	(0.001)	(0.00)	(0.00)
$D_j \times 1[t = 2014]$	-0.003**	-0.00	-0.001	-0.001
	(0.002)	(0.001)	(0.001)	(0.00)
$D_j \times$ Male	0.001*	0.001**	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
$D_j \times 1[t = 2019] \times$ Male	0.001	-0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
$D_j \times 1[t = 2018] \times$ Male	0.00	-0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
$D_j \times 1[t = 2016] \times$ Male	-0.00	0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
$D_j \times 1[t = 2015] \times$ Male	-0.00	0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
$D_j \times 1[t = 2014] \times$ Male	-0.00	0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Male wages in 2019	-0.0	-0.001	-0.0	-0.0
	(0.001)	(0.001)	(0.001)	(0.001)
Male wages in 2018	0.0	-0.001	0.0	0.0
	(0.001)	(0.001)	(0.001)	(0.001)
Male wages in 2016	-0.001	-0.002**	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.0)
Male wages in 2015	-0.001	-0.001	-0.002	-0.0
	(0.001)	(0.001)	(0.001)	(0.001)
Male wages in 2014	-0.003*	-0.0	-0.002	-0.001
	(0.002)	(0.001)	(0.001)	(0.001)
Equal work FE	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓
CBA-year FE	✓	✓	✓	✓
Dependent mean (all)	5.126	5.127	5.124	5.129
Dependent mean (untreated 2017)	5.129	5.132	5.131	5.133
N	840,093	1,321,856	1,214,541	5,216,271
R^2	0.472	0.413	0.548	0.475

Notes: The table above shows the ITT estimates from the event study design equation. D_j is an indicator of firm size greater than 250 in 2017 and *Male* is an indicator for whether the worker is male. The coefficient of the interaction of D_j with the indicator of each year t and the indicator of *Male* gives the estimate of the gender pay equity law on gender wage gaps in that year. The coefficient of the interaction of D_j with the indicator of each year t gives the effect of the pay equity law on female wages. All equations control for observable characteristics X_{ijt} that matter for wage setting as per the institutional details and are described in the text. Standard errors are clustered at the firm level.

Table 7: Estimates of the effect of the pay-equity law on log hourly wages of stayers

	$GWG_{prepolicy} < 0\%$ (1)	$GWG_{prepolicy} \in [0\%, 5\%]$ (2)	$GWG_{prepolicy} > 5\%$ (3)	Pooled (4)
D_{jt}	0.001	0.029***	0.034***	0.021***
	(0.008)	(0.007)	(0.012)	(0.005)
Male	-0.084***	0.077***	0.209***	0.095***
	(0.027)	(0.019)	(0.025)	(0.011)
Female wages in 2019	-0.020***	-0.030***	-0.009	-0.016***
	(0.007)	(0.006)	(0.007)	(0.006)
Female wages in 2018	-0.019***	-0.026***	-0.007	-0.014***
	(0.005)	(0.005)	(0.005)	(0.004)
Female wages in 2016	-0.007	0.001	0.002	-0.00
	(0.006)	(0.005)	(0.007)	(0.005)
Female wages in 2015	0.002	-0.002	0.006	0.00
	(0.006)	(0.008)	(0.008)	(0.004)
Female wages in 2014	0.005	-0.009	0.001	-0.002
	(0.006)	(0.008)	(0.009)	(0.004)
$D_{jt} \times$ Male	0.021***	-0.009***	-0.059***	-0.011***
	(0.004)	(0.002)	(0.012)	(0.004)
Gender wage gap in 2019	0.027***	0.017***	-0.009	0.006*
	(0.005)	(0.004)	(0.006)	(0.003)
Gender wage gap in 2018	0.019***	0.012***	-0.002	0.006**
	(0.004)	(0.003)	(0.004)	(0.002)
Gender wage gap in 2016	0.006	0.006**	0.007	0.008***
	(0.005)	(0.002)	(0.004)	(0.003)
Gender wage gap in 2015	0.002	-0.001	-0.001	0.00
	(0.005)	(0.003)	(0.006)	(0.003)
Gender wage gap in 2014	-0.00	-0.004	-0.003	-0.003
	(0.004)	(0.003)	(0.006)	(0.003)
Male wages in 2019	0.007	-0.013*	-0.019*	-0.01
	(0.009)	(0.007)	(0.01)	(0.007)
Male wages in 2018	0.0	-0.014**	-0.009	-0.009*
	(0.006)	(0.006)	(0.006)	(0.005)
Male wages in 2016	-0.001	0.007	0.008	0.008
	(0.008)	(0.006)	(0.008)	(0.006)
Male wages in 2015	0.004	-0.004	0.005	0.001
	(0.008)	(0.009)	(0.01)	(0.005)
Male wages in 2014	0.005	-0.013	-0.002	-0.004
	(0.008)	(0.009)	(0.011)	(0.006)
Equal work FE	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓
CBA-year FE	✓	✓	✓	✓
Dependent mean (all)	1.816	1.717	1.998	1.828
Dependent mean (untreated 2017)	1.756	1.615	1.879	1.76
N	761,604	1,151,623	1,122,810	4,771,451
R^2	0.910	0.922	0.885	0.905

Notes: The table above shows the ATT estimates from the event study design equation on the sample of workers who did not switch firms in the sample period.. D_{jt} is an indicator of firm size greater than 250 and *Male* is an indicator for whether the worker is male. The coefficient of the interaction of D_{jt} with the indicator of each year t and the indicator of *Male* gives the estimate of the gender pay equity law on gender wage gaps in that year. The coefficient of the interaction of D_{jt} with the indicator of each year t gives the effect of the pay equity law on female wages. All equations control for observable characteristics X_{ijt} that matter for wage setting as per the institutional details and are described in the text. Standard errors are clustered at the firm level.

Table 8: Estimates of the effect of the pay-equity law on log hourly wages of workers in firms not employing in between 240 and 260 workers

	$GWG_{prepolicy} < 0\%$	$GWG_{prepolicy} \in [0\%, 5\%]$	$GWG_{prepolicy} > 5\%$	Pooled
	(1)	(2)	(3)	(4)
D_{jt}	0.004	0.026***	0.031*	0.024***
	(0.014)	(0.009)	(0.016)	(0.009)
Male	-0.081***	0.084***	0.207***	0.099***
	(0.028)	(0.019)	(0.025)	(0.011)
Female wages in 2019	-0.021***	-0.028***	-0.00	-0.014**
	(0.007)	(0.006)	(0.008)	(0.006)
Female wages in 2018	-0.018***	-0.026***	-0.002	-0.013***
	(0.005)	(0.005)	(0.005)	(0.004)
Female wages in 2016	-0.006	0.00	0.007	0.00
	(0.006)	(0.005)	(0.007)	(0.005)
Female wages in 2015	0.006	-0.004	0.011	0.003
	(0.006)	(0.009)	(0.009)	(0.005)
Female wages in 2014	0.009	-0.006	0.007	0.002
	(0.006)	(0.008)	(0.009)	(0.005)
$D_{jt} \times$ Male	0.025***	-0.008***	-0.060***	-0.011***
	(0.004)	(0.002)	(0.011)	(0.004)
Gender wage gap in 2019	0.029***	0.015***	-0.013*	0.005
	(0.005)	(0.004)	(0.007)	(0.003)
Gender wage gap in 2018	0.020***	0.011***	-0.004	0.006**
	(0.004)	(0.003)	(0.004)	(0.003)
Gender wage gap in 2016	0.004	0.005**	0.005	0.008**
	(0.004)	(0.002)	(0.005)	(0.003)
Gender wage gap in 2015	-0.00	-0.003	-0.002	0.00
	(0.005)	(0.003)	(0.007)	(0.003)
Gender wage gap in 2014	-0.003	-0.005*	-0.005	-0.003
	(0.004)	(0.003)	(0.007)	(0.003)
Male wages in 2019	0.008	-0.013*	-0.014	-0.009
	(0.009)	(0.007)	(0.01)	(0.007)
Male wages in 2018	0.002	-0.015**	-0.007	-0.007
	(0.006)	(0.006)	(0.006)	(0.005)
Male wages in 2016	-0.002	0.006	0.012	0.008
	(0.007)	(0.006)	(0.008)	(0.006)
Male wages in 2015	0.006	-0.007	0.009	0.003
	(0.008)	(0.009)	(0.011)	(0.006)
Male wages in 2014	0.006	-0.011	0.002	-0.001
	(0.007)	(0.008)	(0.012)	(0.006)
Equal work FE	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓
CBA-year FE	✓	✓	✓	✓
Dependent mean (all)	1.788	1.699	1.975	1.811
Dependent mean (untreated 2017)	1.741	1.604	1.861	1.75
N	796,192	1,274,004	1,156,034	4,960,076
R^2	0.908	0.918	0.884	0.903

Notes: The table above shows the ATT estimates from the event study design equation after removing workers who work in firms with size in between 240 and 260. D_{jt} is an indicator of firm size greater than 250 and *Male* is an indicator for whether the worker is male. The coefficient of the interaction of D_{jt} with the indicator of each year t and the indicator of *Male* gives the estimate of the gender pay equity law on gender wage gaps in that year. The coefficient of the interaction of D_{jt} with the indicator of each year t gives the effect of the pay equity law on female wages. All equations control for observable characteristics X_{ijt} that matter for wage setting as per the institutional details and are described in the text. Standard errors are clustered at the firm level.

Table 9: Estimates of the effect of the pay-equity law on log hourly wages of workers in firms not employing in between 220 and 280 workers

	$GWG_{prepolicy} < 0\%$	$GWG_{prepolicy} \in [0\%, 5\%]$	$GWG_{prepolicy} > 5\%$	Pooled
	(1)	(2)	(3)	(4)
D_{jt}	0.004	0.026***	0.031*	0.024***
	(0.014)	(0.009)	(0.016)	(0.009)
Male	-0.081***	0.084***	0.207***	0.099***
	(0.028)	(0.019)	(0.025)	(0.011)
Female wages in 2019	-0.021***	-0.028***	-0.00	-0.014**
	(0.007)	(0.006)	(0.008)	(0.006)
Female wages in 2018	-0.018***	-0.026***	-0.002	-0.013***
	(0.005)	(0.005)	(0.005)	(0.004)
Female wages in 2016	-0.006	0.00	0.007	0.00
	(0.006)	(0.005)	(0.007)	(0.005)
Female wages in 2015	0.006	-0.004	0.011	0.003
	(0.006)	(0.009)	(0.009)	(0.005)
Female wages in 2014	0.009	-0.006	0.007	0.002
	(0.006)	(0.008)	(0.009)	(0.005)
$D_{jt} \times$ Male	0.025***	-0.008***	-0.060***	-0.011***
	(0.004)	(0.002)	(0.011)	(0.004)
Gender wage gap in 2019	0.029***	0.015***	-0.013*	0.005
	(0.005)	(0.004)	(0.007)	(0.003)
Gender wage gap in 2018	0.020***	0.011***	-0.004	0.006**
	(0.004)	(0.003)	(0.004)	(0.003)
Gender wage gap in 2016	0.004	0.005**	0.005	0.008**
	(0.004)	(0.002)	(0.005)	(0.003)
Gender wage gap in 2015	-0.00	-0.003	-0.002	0.00
	(0.005)	(0.003)	(0.007)	(0.003)
Gender wage gap in 2014	-0.003	-0.005*	-0.005	-0.003
	(0.004)	(0.003)	(0.007)	(0.003)
Male wages in 2019	0.008	-0.013*	-0.014	-0.009
	(0.009)	(0.007)	(0.01)	(0.007)
Male wages in 2018	0.002	-0.015**	-0.007	-0.007
	(0.006)	(0.006)	(0.006)	(0.005)
Male wages in 2016	-0.002	0.006	0.012	0.008
	(0.007)	(0.006)	(0.008)	(0.006)
Male wages in 2015	0.006	-0.007	0.009	0.003
	(0.008)	(0.009)	(0.011)	(0.006)
Male wages in 2014	0.006	-0.011	0.002	-0.001
	(0.007)	(0.008)	(0.012)	(0.006)
Equal work FE	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓
CBA-year FE	✓	✓	✓	✓
Dependent mean (all)	1.788	1.699	1.975	1.811
Dependent mean (untreated 2017)	1.741	1.604	1.861	1.75
N	796,192	1,274,004	1,156,034	4,960,076
R^2	0.908	0.918	0.884	0.903

Notes: The table above shows the ATT estimates from the event study design equation after removing workers who work in firms with size in between 200 and 300. D_{jt} is an indicator of firm size greater than 250 and *Male* is an indicator for whether the worker is male. The coefficient of the interaction of D_{jt} with the indicator of each year t and the indicator of *Male* gives the estimate of the gender pay equity law on gender wage gaps in that year. The coefficient of the interaction of D_{jt} with the indicator of each year t gives the effect of the pay equity law on female wages. All equations control for observable characteristics X_{ijt} that matter for wage setting as per the institutional details and are described in the text. Standard errors are clustered at the firm level.

B Appendix-B

B.1 Identifying Assumptions

We make the following identifying assumptions in our triple difference event-study framework.

(A-1) Sharp design: For all $(i, j, t) \in \{1, \ldots, N_{j,t}\} \times \{1, \ldots, J\} \times \{1, \ldots, T\}, D_{i,j,t} = D_{j,t}$

The sharp-design assumption specifies that the treatment status of a firm is the treatment status of each worker working in the firm for all workers, firms and time periods. In our case, this implies that prior to 2018, firms were unaware of this policy, and thus we can plausibly use 2017 as our base-year.

(A-2) No Anticipation: For all j, for all $\mathbf{d} \in \{0, 1\}^T, Y_{j,t}(\mathbf{d}) = Y_{j,t}(d_1, \ldots, d_t)$

The no-anticipation assumption specifies that no firm at any time predicted at which period its treatment stauts would change.

(A-3) Conditional PT: For all $t \neq t'$, denoting ∞ as the potential state of the world where a unit is never treated, we have

$$\Big(\mathbb{E}\left[Y_{ijt}(\infty) \mid D_{jt} = 1, F_i = 0, \theta_{equal}, X_{ijt}\right] - \mathbb{E}\left[Y_{ijt'}(\infty) \mid D_{jt'} = 1, F_i = 1, \theta_{equal}, X_{ijt'}\right]\Big) -$$
$$\Big(\mathbb{E}\left[Y_{ijt}(\infty) \mid D_{jt} = 1, F_i = 0, \theta_{equal}, X_{ijt}\right] - \mathbb{E}\left[Y_{ijt'}(\infty) \mid D_{jt'} = 1, F_i = 1, \theta_{equal}, X_{ijt'}\right]\Big)$$
$$=$$
$$\Big(\mathbb{E}\left[Y_{ijt}(\infty) \mid D_{jt} = \infty, F_i = 0, \theta_{equal}, X_{ijt}\right] - \mathbb{E}\left[Y_{ijt'}(\infty) \mid D_{jt'} = \infty, F_i = 1, \theta_{equal}, X_{ijt'}\right]\Big) -$$
$$\Big(\mathbb{E}\left[Y_{ijt}(\infty) \mid D_{jt} = \infty, F_i = 0, \theta_{equal}, X_{ijt}\right] - \mathbb{E}\left[Y_{ijt'}(\infty) \mid D_{jt'} = \infty, F_i = 1, \theta_{equal}, X_{ijt'}\right]\Big)$$

The conditional parallel trends assumption specifies that the evolution of the difference in average wages between male and female workers in the treated firms would have happend in parallel to that in the control firms, in absence of the policy, conditional on the workers being compared are those who produce work of equal value in a given firm at a given time period, and their observables which are unaffected by treatment status.

It is important to highlight, that the equal pay policy does not provide variation to impose conditional parallel trends assumption within gender. A gender-specific conditional parallel trends would be a stronger assumption than the one specified above. The above conditional parallel trends assumption does not impose any restriction on how male and female wages by themselves would have evolved in absence of the policy in large and small firms. A gender specific conditional parallel trends would have implied, that for all $g \in \{0, 1\}$, we have

$$\Big(\mathbb{E}\left[Y_{ijt}(\infty) \mid D_{jt} = 1, F_i = g, \theta_{equal}, X_{ijt}\right] - \mathbb{E}\left[Y_{ijt'}(\infty) \mid D_{jt'} = 1, F_i = g, \theta_{equal}, X_{ijt'}\right]\Big)$$
$$=$$
$$\Big(\mathbb{E}\left[Y_{ijt}(\infty) \mid D_{jt} = \infty, F_i = g, \theta_{equal}, X_{ijt}\right] - \mathbb{E}\left[Y_{ijt'}(\infty) \mid D_{jt'} = 1, F_i = g, \theta_{equal}, X_{ijt'}\right]\Big)$$

Observe that a gender-specifc conditional parallel trends would imply our conditional parallel trends assumption in (A3) but the converse is not necessarily true. Moreover, a gender specific parallel trends assumption implies that the researcher is comparing workers of a given gender who produce work of equal value, whereas the policy requries a comparison of between gender comparison of workers who produce work of equal value.

This is to highlight that even if the gender-specific parallel trends test might fail, in the joint estimation of the effect of the law on the genderwage gap and on gender-specific wages, we could still have no differential pre-trends in the gender wage gap between large and small firms, as long as the gender-specific parallel trends fail equally. However, to identify the gender specific effects of the law, we require the assumption of conditional gender-specific parallel trends for either males or females. Given conditional gender specific conditional parallel trends of males (females) along with between gender conditional specific parallel trends, will imply gender specific conditional parallel trends of females (males).

C Appendix-C

C.1 Overview of pay equity laws in different countries

Pay equity laws have a long history, evolving over multiple decades to address gender wage disparities across various countries. France, with a long-standing commitment to gender equality, incorporated pay equity into its Labour Code in 1946, making it one of the earliest adopters. Germany's constitution of 1949 also laid the foundation for gender pay equity.

In the United States, the Equal Pay Act of 1963 was one of the earliest comprehensive laws aimed at abolishing wage discrimination based on gender. Canada implemented its first pay equity law in 1996 for jobs in the public sector, contributing to a broader global effort toward gender pay equity.

The United Kingdom followed with the Equal Pay Act of 1970, which later evolved through amendments in 1975, 1983, 1986, and 2010 to strengthen its provisions. The Nordic countries have been particularly progressive, with Sweden introducing its first Equal Pay Act in 1980 and Iceland continuously refining its laws from 1961 through to its most recent amendment in 2017, making it the first country to enforce equal pay through certification requirements for employers. In other parts of Europe, countries like Austria (1979), Belgium (1975, 1999), and Denmark (1976) passed their laws, progressively amending them to align with EU directives. Similarly, in Southern Europe, Portugal enshrined pay equity in its 1976 constitution, while Spain's Workers' Statute provides the legal framework.

Eastern European countries such as Poland and Romania integrated pay equity into their constitutions post-World War II, reflecting the broader socialist commitments to gender equality. The Czech Republic and Slovakia, emerging from the Czechoslovak state, adopted specific pay equity laws and constitutional protections after 1992.
These examples demonstrate the varied yet convergent global commitment to eliminating gender wage disparities through legislative action.

D Relation of our work to existing literature

This paper contributes to the vast literature that both documents and provides an understanding of gender inequality in the labor market (see e.g., Blau & Kahn (2017), Goldin, Kerr, Olivetti & Barth (2017), Goldin (2014); among others). To the best of our knowledge, only Baker & Fortin (2004) present evidence on the causal impact of a pay-equity legislation implemented all across Ontario, Canada in the early 1990s. While they do not find evidence of a significant impact of pay equity legislation on wages, that is suggested to be due to few firms complying with the legislation.[24] Our empirical setting differs sharply not only by the near perfect degree of firm compliance with the law, but also from having more accurate information on employee wages and exploiting policy variation across firms of different sizes over time.

[24]Specifically, Baker and Fortin (2004) estimate the impact of pay-equity legislation in the Canadian province of Ontario using the neighboring province of Quebec as a comparison group. They rely on wage data from survey responses as opposed to administrative data and the low levels of firm compliance with the law may introduce concerns related to selection into treatment based on unobservables. Thus, the study can be viewed as measuring an intent to treat and the potential of measurement error in the dependent variable could have biased the estimated effect towards statistical insignificance.

Our work is closely related to Bailey, Helgerman & Stuart (2024), who analyze the impact of nationwide Equal Pay Act of 1964 in the US. To deal with the lack of clean identifying variation, they employ two complementary research designs: one that exploits variation in pre-existing state-level pay equity laws, and another that leverages differences in gender wage gaps across states–requiring a strong assumption that these pre-policy differences across states are plausibly random. Their findings suggest that the Equal Pay Act of 1964 significantly narrowed wage disparities of around 40-50% by nearly a fifth through the accelerated wage growth for women but stifled women's progression in high paying jobs in the long run.

Our study diverges from and improves upon Bailey, Helgerman & Stuart (2024) in several ways. Firstly, we utilize a more credible variation to identify the effects of Portugal's 2018 pay equity act, which initially excluded firms with fewer than 250 employees from policy enforcement. Our data provides a nuanced approach to defining "equal work," allowing us to compare individuals not just across firms but within the same job title as defined by industry-wide CBAs. This level of specificity is a stark contrast to the historical Census and ACS datasets, which, lacking information on specific employers, are limited to broad classifications by industry, occupation, and state.[25] The richness of our data allows us also to mitigate many other potential concerns which we discuss later. Significantly, our analysis is set apart by the Portuguese government's enforcement strategy, which imposed penalties on companies with gender wage disparities over five percent. This contrasts with the 1964 US Equal Pay Act, which lacked such regulatory teeth, offering no such explicit benchmarks for acceptable gender wage differentials. Additionally, the degree to which occupational segregation influences gender wage gaps has diminished since the 1960s, reflecting broader shifts in the determinants of wage disparities in recent years.[26]

E Further details on the 2018 pay equity law in Portugal

Law 60/2018 can be viewed as containing four different mechanisms to enforce equal pay for equal work and of equal value. The first mechanism as discussed in the text involves the annual collection of statistical information to calculate pay differences. Employers must submit employee-level pay data for analysis by the Ministry of Labor, Solidarity, and Social Security. Women and men are grouped by the same work or work of equal value. The Ministry then publishes detailed information on the gender pay gap broken down by several criteria including company and profession, which is closely ted to job title and occupation. These calculations include all of employees in fulltime employees and all aspects of their remuneration including overtime. It is important to state explicitly that Law No.105/2009 required Portuguese

[25] This data restriction of the US Census and the ACS consequently requires necessary assumptions of any endogenous job mobility to only occur within state by industry by occupation groups highlighted by Bailey et al. Their data also restricts them to use weekly wages in the absence of data on hours worked to construct measures of hourly wages as done by us following Card et al. (2016).

[26] Bailey, Helgerman & Stuart (2024) argue that robust, causal evidence on the efficacy of pay equity laws across countries is sparse. They suggest that while there's extensive literature on pay equity laws, affirmative action policies and anti-discrimination legislations (Beller 1979, 1982*a*,*b*, Leonard 1984, Manning 1996, Carrington, McCue & Pierce 2000, Baker & Fortin 2004, Holzer & Neumark 2006, Kurtulus 2012, Helgerman 2023) most lack rigorous causal interpretation due to data scarcity, questions of internal validity due to lack in identifying variation, and variability in policy enforcement.

employers to submit pay data to the government annually. Law 60/2018 only enables the government to publish gender pay gap information to accompany that data.

Second, after the pay data has been submitted, the Ministry has 60 days to inform the employer if an evaluation plan is required to rectify gender-based pay inequities. If pay differences have been identified, companies then have 120 days to prepare and submit an evaluation plan to the Authority for Work Conditions (ACT), which must demonstrate either that the pay discrepancies are justified or explain what measures the company will implement to rectify gender-based pay inequities within the next year. Employers that fail to submit an evaluation plan when required are subject to sanctions and may be prohibited from bidding on public contracts.

Third, companies are obligated to ensure a transparent remuneration policy based on objective and non-discriminatory criteria such as seniority. This mechanism is under Article 31 of the labor code and strives to create equal working conditions so that Portuguese employees must receive equal pay for equal work or work of equal value. Yet, there are currently no legal requirements for employers to post salary details to applicants or employees in Portugal regarding specific positions.

The fourth mechanism is quite unique in that any worker or union representative can make a request in writing to the Commission for Equality in Labour and Employment (that has the acronym CITE, as the Portuguese name is Comissao para a Igualdade no Trabalho e no Emprego) to investigate potential pay discrimination. In accordance with Article 6 of the law. CITE is required to respond to this request to issue an opinion on that delivers a conclusive judgment on any allegation of gender discrimination in context of the law. The process involves CITE being required to inform the employer within 10 days of having received a written claim. The written claim must identify the opposite-gender worker(s) concerned. Once contacted by CITE, the employer then has a 30-day window to detail their pay policies and elucidate how the wages of the claimant and specified worker(s) were determined. A failure to provide this information is construed as an unjustified gender wage disparity. If the employer provides that information, CITE conducts an investigation and if they conclude that the evidence supports pay discrimination, employers are then required to address the pay difference within 180 days. CITE's final binding decision is shared with the claimant, employer, and ACT within 60 days of the prescribed deadlines, and unexplained gender wage differences are assumed to be prejudiced.

F Results on firm level and firm-job level outcomes

Table 10: ITT estimates of the effect of the pay-equity law on firm size for firms employing in between 200 and 300 workers

	$GWG_{prepolicy} < 0\%$	$GWG_{prepolicy} \in [0\%, 5\%]$	$GWG_{prepolicy} > 5\%$	Pooled
	(1)	(2)	(3)	(4)
$D_j \times 1[t = 2019]$	0.014	0.020*	0.005	0.011
	(0.011)	(0.012)	(0.012)	(0.010)
$D_j \times 1[t = 2018]$	-0.004	-0.002	0.006	0.004
	(0.010)	(0.012)	(0.009)	(0.008)
$D_j \times 1[t = 2016]$	0.006	0.005	0.005	0.009
	(0.010)	(0.013)	(0.010)	(0.009)
$D_j \times 1[t = 2015]$	-0.004	0.015	-0.002	0.006
	(0.012)	(0.014)	(0.012)	(0.010)
$D_j \times 1[t = 2014]$	-0.001	-0.001	-0.015	0.002
	(0.013)	(0.015)	(0.012)	(0.011)
Firm FE	✓	✓	✓	✓
Industry-year FE	✓	✓	✓	✓
Occupation-year FE	✓	✓	✓	✓
Equal work FE	✓	✓	✓	✓
Dependent mean (all)	5.488	5.483	5.486	5.481
Dependent mean (untreated 2017)	5.406	5.406	5.407	5.407
N	16,171	12,479	8,172	77,417
R^2	0.918	0.931	0.895	0.893

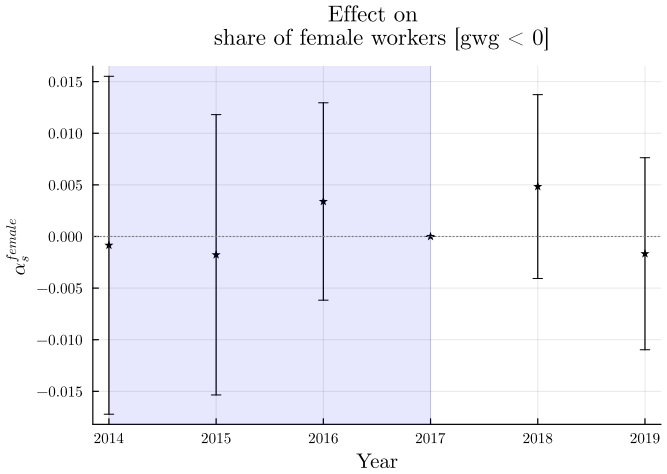
Notes: This table reports intent-to-treat (ITT) estimates of the effect of the pay equity law on firm size for firms employing between 200 and 300 workers. D_j is an indicator variable for firms with more than 250 workers in 2017. The coefficients of $D_j \times 1[t = year]$ represent the differential change in log firm size relative to 2017. Standard errors clustered at the firm level appear in parentheses. All specifications include firm fixed effects, industry-year fixed effects, occupation-year fixed effects, and equal work fixed effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 11: Estimates of the effect of the pay-equity law on share of within job female workers

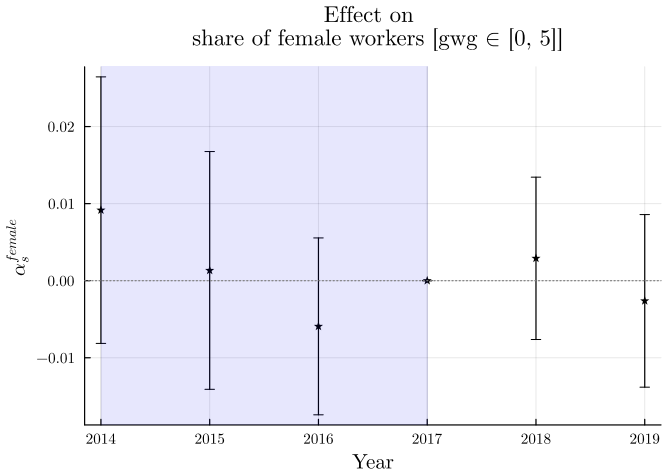
	$GWG_{prepolicy} < 0\%$	$GWG_{prepolicy} \in [0\%, 5\%]$	$GWG_{prepolicy} > 5\%$	Pooled
	(1)	(2)	(3)	(4)
D_{jt}	0.001	0.002	-0.005	-0.002
	(0.007)	(0.009)	(0.008)	(0.002)
$D_{jt} \times 1[t = 2019]$	-0.002	-0.003	0.006	0.001
	(0.005)	(0.006)	(0.005)	(0.001)
$D_{jt} \times 1[t = 2018]$	0.005	0.003	0.006	0.002*
	(0.005)	(0.005)	(0.004)	(0.001)
$D_{jt} \times 1[t = 2016]$	0.003	-0.006	-0.003	-0.001
	(0.005)	(0.006)	(0.004)	(0.001)
$D_{jt} \times 1[t = 2015]$	-0.002	0.001	0.004	0.00
	(0.007)	(0.008)	(0.006)	(0.002)
$D_{jt} \times 1[t = 2014]$	-0.00	0.009	0.003	0.00
	(0.008)	(0.009)	(0.006)	(0.002)
Industry-year FE	✓	✓	✓	✓
Occupation-year FE	✓	✓	✓	✓
Equal work FE	✓	✓	✓	✓
Dependent mean (all)	0.532	0.519	0.479	0.444
Dependent mean (untreated 2017)	0.532	0.518	0.482	0.446
N	167,889	126,715	98,708	1,003,919
R^2	0.878	0.915	0.729	0.944

Notes: This table reports ATT estimates of the effect of the pay equity law on the share of female workers within job titles. D_{jt} is the indicator for firms with more than 250 workers. The coefficients of $D_{jt} \times 1[t = year]$ represent the differential change in the share of female workers within jobs relative to 2017. Standard errors clustered at the firm level appear in parentheses. All specifications include industry-year fixed effects, occupation-year fixed effects, and equal work fixed effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

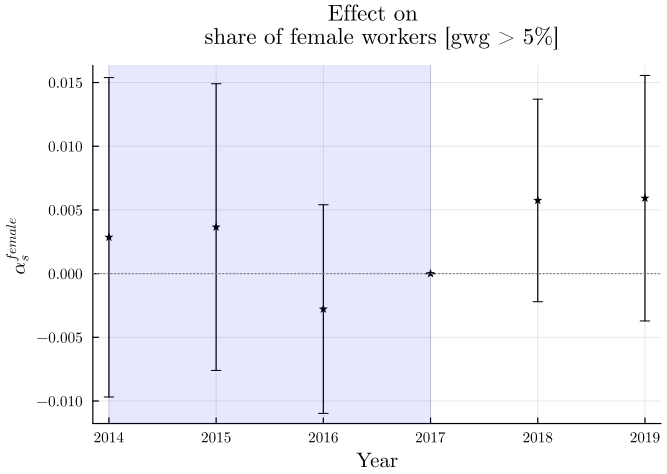
Figure 22: Impact of the law on the within job share of female workers



Notes: This figure shows the estimated effect of the pay equity law on the within-job share of female workers in jobs where the pre-policy gender wage gap was below zero (women earned more than men). The x-axis represents years from 2014-2019, with 2017 as the reference year. The y-axis shows the coefficient estimates (α_s^{female}) with 95% confidence intervals. The shaded area indicates pre-policy years. No statistically significant changes are observed in either pre- or post-policy periods.

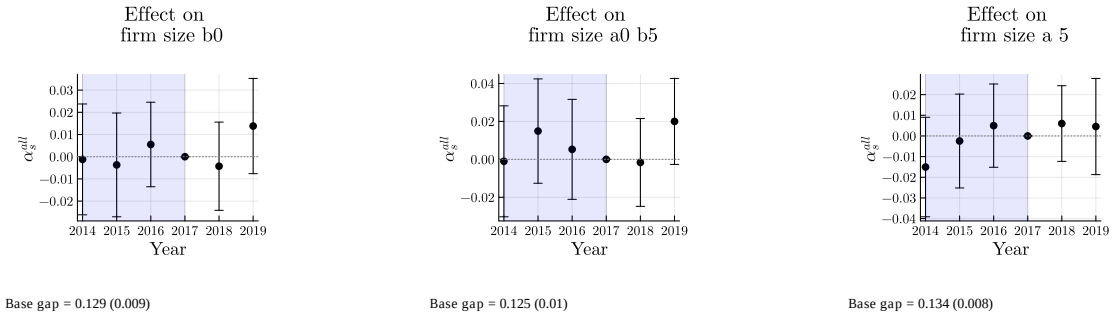


Notes: This figure shows the estimated effect of the pay equity law on the within-job share of female workers in jobs where the pre-policy gender wage gap was between zero and five percent. The x-axis represents years from 2014-2019, with 2017 as the reference year. The y-axis shows the coefficient estimates (α_s^{female}) with 95% confidence intervals. The shaded area indicates pre-policy years. No statistically significant changes are observed in the gender composition within these jobs following the policy implementation.

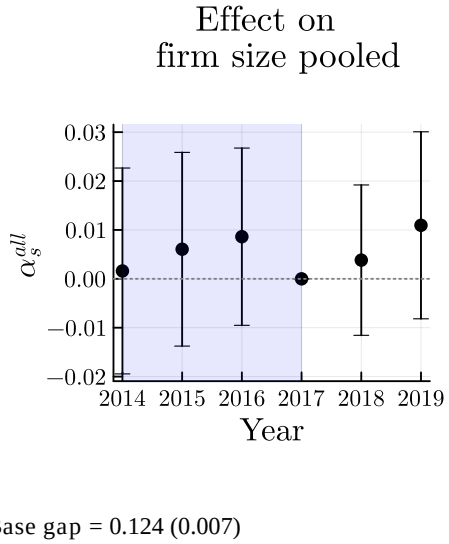


Notes: This figure shows the estimated effect of the pay equity law on the within-job share of female workers in jobs where the pre-policy gender wage gap was above five percent. The x-axis represents years from 2014-2019, with 2017 as the reference year. The y-axis shows the coefficient estimates (α_s^{female}) with 95% confidence intervals. The shaded area indicates pre-policy years. The results show no evidence that firms adjusted gender composition within jobs with high wage gaps to circumvent the law.

Figure 23: ITT estimates of the effect of the law on firm size employing between 200 and 300 workers pre-policy



Notes: This figure presents ITT estimates showing the effect of the pay equity law on log firm size for firms who employed in between 200 and 300 workers in 2017, across three subgroups based on average pre-policy gender wage gaps: below 0% (left), between 0-5% (middle), and above 5% (right). The x-axis shows years from 2014-2019, with 2017 as the reference year. The y-axis shows coefficient estimates (α_s^{all}) with 95% confidence intervals. The shaded area indicates pre-policy years. Conditional base gaps (with standard errors) are reported below each subplot.



Notes: This figure presents ITT estimates showing the effect of the pay equity law on log firm size for firms who employed in between 200 and 300 workers in 2017, pooling all workers The x-axis shows years from 2014-2019, with 2017 as the reference year. The y-axis shows coefficient estimates (α_s^{all}) with 95% confidence intervals. The shaded area indicates pre-policy years. Conditional base gaps (with standard errors) are reported below each subplot.