Labor Market Consequences of Pay-equity Laws[*]

Md Moshi Ul Alam [†]
Clark University

Steven F. Lehrer
Queen's University
NBER

Nuno Sousa Pereira
University of Porto

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Abstract

Limited policy variation, data constraints in defining '*equal work*', and compliance issues have prevented rigorous causal analysis of pay equity laws. We address these challenges using Portugal's 2018 legislation, which penalized firms with over 250 employees for gender wage gaps exceeding 5%. Using administrative data that links employees to industry-defined job titles within firms, we analyze its impact both within and between genders using an event study design. We find the law achieved its intended effects in some areas while generating unintended consequences in others. Jobs with initial gaps exceeding 5% saw a 9% reduction, mainly through slower male wage growth. A small fraction of jobs with negative gaps saw reduced female wage growth, closing the gap by half. However, gaps in jobs initially between 0-5% unexpectedly widened by 21% due to slower female wage growth. These unintended consequences are more pronounced in male-dominated industries and cannot be explained by productivity differences. 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 reveal both intended and unintended consequences of pay equity laws implemented with uniform regulatory targets: while the 5% target effectively reduced large disparities, it inadvertently widened gaps below the threshold as firms, with enforcement rules now clarified, strategically adjusted wages but not employment, offering crucial insights for policy design in achieving "*equal pay for equal work*".

JEL Codes: J31, J38, J16, C55

Keywords: gender wage gap, pay equity, unintended consequences, matched employee employer data, event study

1 Introduction

Our understanding of gender inequalities in labor markets has markedly progressed over the past two decades, offering insights into both long-term trends and their underlying explanations (Goldin 2014, Goldin et al. 2017, Goldin & Mitchell 2017, Goldin et al. 2022, Olivetti et al. 2024). Yet, substantial gender disparities persist—most notably, the earnings gap between men and women working in the same occupations (Blau & Kahn 2017). These gaps remain even in countries that have enacted pay equity laws aimed at ensuring "equal pay for equal work". Despite the widespread adoption of such laws,[1] there is scant evidence on their effects on wage levels and the wage distribution. This gap in the evidence base is in part due to the lack of sufficient within-time variation in addition to existing between-time variation, since pay equity is embedded either as a constitutional right or as a federally mandated statute in most countries. Additionally, researchers face data constraints in defining 'equal work' and issues with firms circumventing existing regulations.

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. The law specifically mandates "equal pay between men and women for equal work" within firms employing more than 250 workers. Law 60/2018 requires each employer to submit their employee level pay data to The Ministry of Labor, Solidarity, and Social Security annually, 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.

Using an event study design and rich administrative matched employer-employee data covering the universe of private-sector workers in Portugal between 2014 and 2019, we estimate the causal effect of the law on the gender wage gap. Portuguese wage setting process along with this granular dataset allows us to define "equal work" precisely: workers employed at the same firm, in the same occupation, covered by the same collective bargaining agreement (henceforth CBA), and holding the same industry-defined job title. Conditional on this definition, we exploit variation in firm size over time to identify an average treatment effect for the treated (ATT) parameter. Our identifying assumption is that, in the absence of the law, the gender-specific wage trajectories of workers engaged in equal work within treated firms would have followed parallel trends to those in untreated firms, conditional on observable characteristics and job-group definitions that determine wages.

Identification of the ATT 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 employ-

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[1]Pay equity laws and their amendments have been implemented in most countries since the 1940s, with a significant increase in adoption from the 1970s onward. Notable examples include: France (1946, Labour Code), Germany (1949), Poland (1952, 1997), Iceland (1961, 1976, 2008, 2017), USA (1963), UK (1970, 1975, 1983, 1986, 2010), Belgium (1975, 1999), Greece (1975, 1984), Portugal (Constitution, 1976), Denmark (1976), Austria (1979), Sweden (1980), Luxembourg (1974, 1981), Italy (Constitution, 1977, 1991), Ireland (1974, 1977, 1998), Norway (1978), Spain (Constitution, Workers' Statute), Finland (1995), Israel (1996, 1998), Canada (1996, public sector), Romania (Constitution, 1991), Netherlands (Constitution, 1994), Slovakia (Constitution, 1992), Hungary (Labour Code), Latvia (Labour Code), Lithuania (Labour Code), Malta (Constitution, Equality Act), Liechtenstein (Civil Code), Czech Republic (2006, 2014), Bulgaria (Labour Code).

ment levels to circumvent the law. Furthermore, employment changes in large firms during the post-policy period were consistent with pre-policy trends, indicating that these changes were employment shocks. Additionally, we find no evidence that firms adjusted the gender composition within job titles. This result is not only important for identification of the ATT, but more importantly for highlighting broader labor market institutions (discussed in section 2) that prevent such manipulations.

Our empirical analysis is guided by institutional features of the Portuguese labor market and unique features of the Law. First, wage setting in Portugal is a mixture of centralized and decentralized bargaining, common in most European countries (Bhuller et al. 2022). Most workers in Portugal are covered by a CBA that establishes industry-wide, job-title specific wage floors. Wage variation exists within jobs because firms have substantial flexibility to supplement these floors with idiosyncratic "wage cushions" (see e.g., Card & Cardoso (2022), Card et al. (2016)). Second, the potential cost implications of Law 60/2018 would vary across treated sub-groups within firms based on the gender wage gap. Specifically, under Law 60/2018, if the government were to detect pay discrepancies exceeding 5%, the employer is required to 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. Given this nonlinear cost structure, we estimate the causal impact of the law separately for firm equal work subgroups whose average pre-policy (2014-2017) gender wage gaps fell above versus below the target 5% gender wage gap—a subsampling strategy that both captures heterogeneous treatment effects and ensures we satisfy the parallel trends assumption.[2]

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] We first 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, among 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.

In contrast, for workers in treated firms and equal work subgroups with pre-law unconditional average gaps in between zero and five percent, the gender gap widened. Specifically,

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[2]While it may seem more natural to classify subgroups based on the immediate pre-policy year (2017), this approach leads to violations of the parallel trends assumption. Using the average across 2014-2017 ensures that our subgroups satisfy this crucial identifying assumption. Note that for each sub-sample, all estimates we present are still interpreted relative to the estimated conditional gender wage gap in the pre-policy year 2017.

[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.

the conditional gender wage gap increased by 21.4% on average, rising from 7% to 8.5% . This widening of gender disparity was also primarily driven by larger reductions in wage growth among female workers.

Taken together, our findings of treatment effect heterogeneity across subgroups defined based on pre-existing gaps uncovers the potential 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. Further analysis reveals that the unintended consequences of the pay equity law were more pronounced in male-dominated industries—those with an above-median share of male workers. In contrast, female-dominated industries saw no significant adverse impact in equal work subgroups with small pre-existing gaps. Moreover, in female-majority industries, larger reductions in gender wage gaps were observed among jobs with initial gaps exceeding 5%.

We next examine if these differential effects of the policy were a result of correcting potential misalignment of compensation with worker productivity. Since weaker productivity-wage linkages are expected in lower-skill occupations (Mueller et al. 2017) we estimate whether the policy effects differ between high- and low-skill occupations.[4] In jobs within low-skill occupations, we present evidence of the intended effects of the policy, suggesting this finding is partly driven by productivity-based wage corrections. In contrast, we find that the unintended consequences of the policy impact both occupation groups uniformly, suggesting that they are not driven by productivity differences. Jobs with negative wage gaps also show effects concentrated primarily among low-skill occupations.[5] These findings imply that while pay equity laws can help correct existing wage-productivity misalignments in jobs with initially large disparities, uniform regulatory targets (like the 5% gap in our case), while well-intentioned can create unintended incentives for firms to strategically manipulate wage gaps in jobs with initially modest disparities.

These two findings that unintended consequences are more pronounced in male-dominated industries, combined with evidence that productivity differences do not drive these effects, suggests the law may have enabled certain firms to exploit the regulatory threshold design to widen gaps in jobs below 5%.[6]

We present a series of robustness checks that reinforce the credibility of our main findings. First, we estimate the intent-to-treat effects of Law 60/2018 using firms' size prior to the policy announcement and find consistent effect sizes. Second, to address potential misclassification of treatment status—arising from employment shocks or churn, since firm size is measured annually—we re-estimate our models excluding firms with 240 to 260 employees. The results

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[4]We thank an anonymous referee for this suggestion.

[5]Male wages accelerated in negative gap jobs in high-skill occupations. However, the share of these jobs is very small. Thus, the aggregate policy impact on jobs with negative gaps operates through slower female wage growth in low-skill occupations. This differential mechanisms by skill level in negative gap jobs likely reflect both productivity considerations and gender differences in bargaining power, particularly the documented "ask gap" where men negotiate more aggressively than women (see Roussille (2021)).

[6]In Appendix F, we discuss the plausibility of other mechanisms—including discriminatory preferences and firm risk aversion. These mechanisms are not necessarily exhaustive, as other firm-specific private information, such as workers' outside options, could also be used to justify or eliminate gaps.

remain robust to this exclusion. Third, to account for endogenous worker mobility, we restrict the sample to workers who remained with the same employer from 2014 to 2019 and again find consistent effects.

We also test for alternative explanations. Specifically, we find precise null effects of the policy on hours worked by either male or female employees, ruling out changes in working time as a driver of the observed wage effects. Moreover, we find no evidence that firms altered the gender composition within job roles, which suggests that the results are not driven by reallocation of workers across positions. Finally, we emphasize the importance of conducting the analysis at the level of "equal work" subgroups rather than aggregating to the firm level. Aggregation can obscure meaningful variation, as firms often contain multiple subgroups with differing initial gender pay gaps. This heterogeneity leads to offsetting effects in aggregate estimates, masking the true nature of firms' responses. By disaggregating to the level of comparable workers, our analysis more precisely captures the wage-setting adjustments firms make in response to pay equity legislation. These findings collectively strengthen the interpretation that the observed wage adjustments reflect deliberate responses by firms to the legal and compliance incentives introduced by Law 60/2018.

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) respectively present evidence on the causal impact of pay-equity legislations implemented at the federal level in the US, and at the province level in Ontario, Canada.[7] Our study differs by exploiting policy-induced variation in treatment across firms based on firm size within and between time. Furthermore, we leverage rich administrative matched employer-employee data that allow us to define "equal work" precisely—as workers in the same firm, occupation, industry-level CBA, and job title. In contrast, Bailey, Helgerman & Stuart (2024) and Baker & Fortin (2004) rely on broader classifications by industry, occupation, and state to define equal work. Additionally, our empirical setting differs markedly from these studies since Portugal's Law 60/2018 includes explicit enforcement mechanisms. See Appendix D for more details.

Next, our setting avoids the compliance challenges documented in other contexts: Baker & Fortin (2004) report widespread non-compliance with Ontario's law, while Passaro et al. (2023) find Chilean firms manipulating their size below the 10-employee threshold to circumvent treatment. Our results highlight that industry-defined CBA oriented labor market institutions—common in Europe—prevent firm size manipulation to circumvent the law, unlike labor markets in USA, Canada and Chile where wage setting is more decentralized primarily determined at the firm level.[8]

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[7]Bailey et al. (2024) employ two complementary research designs to analyze the 1964 Equal Pay Act implemented nationwide in the US—an intensity-based design exploiting pre-existing state pay equity laws, and a second leveraging pre-policy cross-state wage gap differences assuming they are plausibly random. Baker & Fortin (2004) use Quebec as a control group for Ontario where the pay equity legislation was adopted.

[8]Passaro et al. (2023) develop a model predicting that firms may engage in gender segregation to avoid falling under the scope of equal pay legislations. They find evidence in support of this prediction and widening of gender wage gap within local labor markets in Chile, where a pay-equity law was enforced for firms with 10 or more employees. The difference in firm compliance in their setting and ours also reflect the importance of the location of the policy threshold and how firm size could impact firm's compliance, beyond differences in labor market

Consistent with Bailey, Helgerman & Stuart (2024), our results highlight that pay equity laws can have nuanced and sometimes unintended impacts. While their work shows that the 1964 US Equal Pay Act narrowed the gender wage gap through accelerated wage growth for women, it also suggests that it may have constrained women's occupational advancement. In contrast, our findings within equal work subgroups show that explicit numerical targets in pay equity policies can shape firm behavior in strategic ways, sometimes producing unintended consequences in addition to the intended effects.

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 act on the disclosed information and negotiate for higher 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).[9] Consequently, it is not surprising that there is no empirical evidence indicating that pay transparency policies significantly boost female wage growth (Baker, Halberstam, Kroft, Mas & Messacar 2023, Bennedsen, Simintzi, Tsoutsoura & Wolfenzon 2022, Perez-Truglia 2020).[10]

In contrast, pay-equity laws shift the responsibility for reducing gender wage disparities directly onto employers. In the case of Portugal, noncompliance carries legal and financial consequences, distinguishing these laws from pay transparency measures, which typically hold firms accountable for disclosing wage information rather than enforcement of wage parity.

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 both the intended and unintended consequences of the policy, along with additional excercises and robustness checks. Section 7 presents evidence that suggest that the unintended consequences are concentrated in male-dominated industries and cannot be explained by productivity differences which explain the intended consequences. A final section draws the main conclusions.

2 Institutional details

To interpret the impact of the the pay equity law requires understanding of the wage setting process in Portugal.

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institutions. More details are discussed in Appendix D.

[9]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)).

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

2.1 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. This is unlike wage setting in North America which is primarily determined through individual worker-firm bargaining. Furthermore, Bhuller, Moene, Mogstad & Vestad (2022) point out, unions and collective bargaining are not synonymous concepts outside North America. Typically, a large share of workers in Continental Europe are covered by collective bargaining (around 80%) and union density is low (around 10%). This is because legal frameworks which allow for the automatic extension of benefits regardless of union status.

In Portugal, wage-setting is vertically centralized primarily through industry-wide collective bargaining agreements. Although union membership in the private sector is relatively low, the widespread use of extension mechanisms ensures that these agreements cover approximately 85% of the private sector workforce. 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 are defined by CBAs.

Industry-wide collective bargaining agreements in Portugal establish job-title-specific minimum monthly wages, effectively setting wage floors across sectors. However, Portuguese employers retain considerable discretion to offer idiosyncratic wage premiums—commonly referred to as wage cushions—above these negotiated minimums. These wage cushions are widespread, vary systematically by firm and worker characteristics, and tend to adjust with changes in the collectively bargained wage floors (Card & Cardoso 2022). Importantly, Card & Cardoso (2022) also document that male workers receive larger wage cushions than their female counterparts. In addition to base wages and wage cushions, most employees receive regular earnings supplements, such as meal allowances. For workers covered by sectoral or firm-specific collective agreements, standard working hours are also stipulated in the CBA. As a result, any firm attempting to justify a detected gender-based wage difference would need to explain why wage cushions differ for male and female employees holding the same job title within the same firm.

2.2 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 mandated equal pay for equal work in all firms with over 250 workers. This Law was legally enforced effective Febru-

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[11]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).

ary 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. The draft of this pay-equity law had been in discussion since 2017.[12] 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 (that has the acronym *CITE*)[13] notifies the firm provide its response within 30 days. The company in their response must disclose information on both their 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 firm fails to disclose this information within the 30 day 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 firm 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 now have 120 days in which to present a plan for assessing the gender pay gap. This plan would be 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.

As discussed in Appendix E that provides further details on Law 60/2018, when introduced, the legislation also required the Ministry of Labour Solidarity, and Social Security publish detailed information on gender wage gaps within firms—a feature that aligns with many pay transparency laws (see, e.g., Bennedsen et al. (2022)). However, in practice, the Ministry has only released highly aggregated statistics, such as the number of violations and the number of cases at various stages of the remediation process. Given this limited public disclosure, we interpret our findings as primarily reflecting the mandatory remediation component of the law, rather than broader transparency effects.

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.

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[12]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.

[13]The acronym CITE is based on the Portuguese name *Comissao para a Igualdade no Trabalho e no Emprego*.

A pay gap would be considered justified if it is explained through differences in individual characteristics, such as education and tenure at the firm.[14] 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.

3 Data sources

We use the *Quadros de Pessoal* data (henceforth QP, that translates to Personnel Records), an annual census of private firms matched to their employees in Portugal from 2014 to 2019. The data are collected by the Ministry of Employment each year in late October from all firms with at least one paid employee. QP includes 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 records each worker's job title, which allow us to define equal work across genders within a firm.

3.1 Sample selection

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.[15] 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.[16] 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 B.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.

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[14]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.

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

[16]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 be dropped during the estimation because they lack within variation.

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 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 balanced across genders, with similar average age (about 40 years) and tenure at firm (approximately 9.4 years).

Panel B compares workers by firm size. Large firms (more than 250 employees) 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 gender wage gaps in labor market outcomes based on the size of pre-policy (2014–2017) gender wage gaps within equal-work subgroups. 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).

Table B.2 further disaggregates the average pre-policy (2014-2017) unconditional gender wage gaps within equal-work subgroups by firm size. The pooled sample exhibits an overall gender wage gap of 5.07% within these subgroups, with considerable variation across categories. In subgroups where the gap exceeded 5%, the unconditional wage differential was 15.87%, while in those with negative gaps, women earned 7.35% more than men on average. Notably among jobs where the gap was more than 5%, large firms exhibited smaller wage disparities. This pattern holds for both the high-gap and negative-gap categories, while the 0–5% category shows similar differentials across firm sizes.

4 Evidence on firm responses to pay equity legislation

We examine two potential threats to the identification of causal parameters beyond the ITT. 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 potential concerns is supported by the data.

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.[17] 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. This result is robust to additional fixed effects as presented in the evidence in Table G.1 and in Figure A.G.3 which show statistically insignificant effects of the policy on firm size which were around the threshold of 250 workers.[18]

Figure 1: Densities of firm size over time
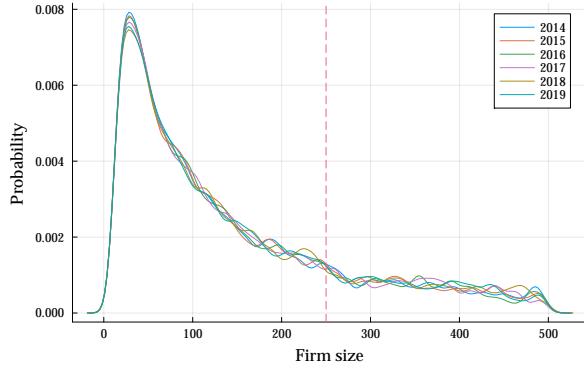
Notes: This figure plots the kernel density of firm size for each year in between 2014 and 2019, and overlay them on top of one another. The vertical red line represents the firm size of 250. The pay equity law was announced in 2018.

Figure 2: Histograms of firm size over time
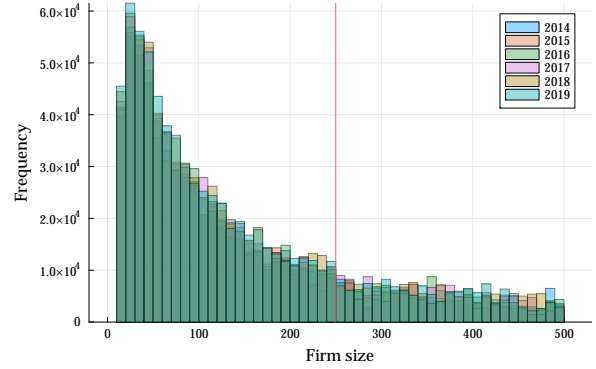
Notes: This figure plots the histogram of firm size for each year in between 2014 and 2019, and overlay them on top of one another. The vertical red line represents the firm size of 250. The pay equity law was announced in 2018.

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

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[17]In Figure A.1 we plot the corresponding empirical CDFs.

[18]Specifically, for firms that 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 average effect of the policy on firm size.

Figure 3: Employment shocks conditional on firm size

Subplot (a): Negative employment shocks — Subplot (b): Positive employment shocks
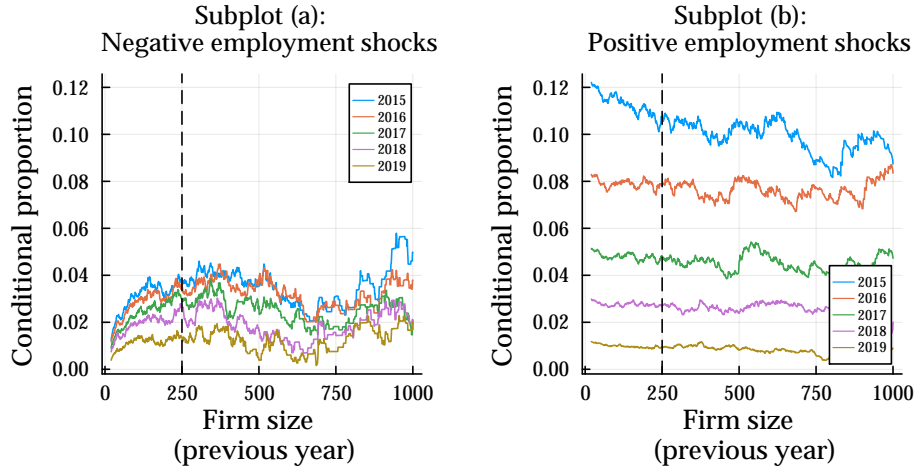
Notes: This figure plots the proportion of firms receiving employment shocks (on the y-axis) conditional on their firm size in the last year (on the x-axis) over the years 2014-2019. For a given firm size in a given year on the x-axis, the left (right) sub-plot plots the proportion of firms receiving 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.

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 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, Figure A.2 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

Figure 4: Job titles over time
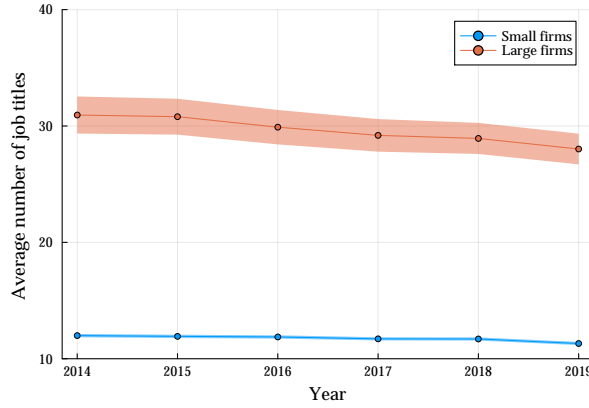
Notes: This figure plots the average number of job titles in firms employing above 250 workers and firms employing below 250 workers over time. The shaded regions represent the 95% confidence bands. The pay equity law was announced in 2018.

because job-titles and job-title specific wage floors are set by collective bargaining agreements which are primarily industry-wide.

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. 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

13

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"

Institutional features of Portugal's wage-setting system provide a credible basis for defining "equal work" in a manner consistent with how the law is enforced. A key advantage of the QP is its inclusion of both job titles and the specific CBA that covers each worker. This highlights the importance of institutional wage setting processes in analysing any policy that aims to acheive equal pay for equal work. These variables are often unavailable in comparable datasets, but are critical for constructing a meaningful definition of equal work and how the law was implemented during our sample period. As discussed in Section 2, CBAs set minimum wage floors by job title and firms retain flexibility to pay "wage cushions" above the negotiated floors, generating wage variation within equal work groups.

Accordingly, our preferred definition of "equal work" refers to workers within the same firm who share the same job title, belong to the same occupation, and are covered by the same CBA. Last, we note that as with all matched employer-employee administrative datasets, the QP does not include direct measures of worker productivity.

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

To identify our causal parameter of interest, we rely on within-job-title variation in worker gender. In our data, 43% of workers are employed in job titles where all workers within the firm-year are of the same gender, underscoring the extent of occupational gender segregation. As a result, we can only construct average pre-policy gender wage gaps—and identify the causal impact of the law—for the remaining 57% of workers in mixed-gender job titles. Figure 5 shows that job-titles with gender variation within firm exhibit substantial heterogeneity in gender wage gaps. Figure 5 also shows that industries with higher shares of female (male) workers are also associated with higher (lower) incidence of negative gender wage gaps, where women earn more than men.

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 add 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

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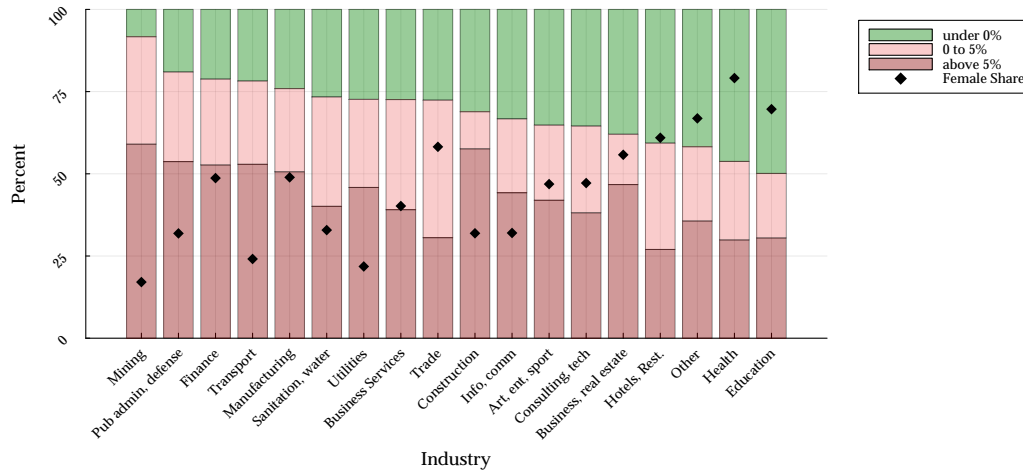
Notes: This figure on plots (a) the share of workers working in jobs with average pre-policy gender wage gaps below 0% (shaded green), between 0% and 5% (shaded pink), and above 5% (shaded dark red) for each industry and (b) the share of female workers in each industry represented by the black dots.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

wages between male and female workers producing equal work in treated firms would have 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 C 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.

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.[19] Our main event study specification can be expressed as

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[19]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 the probability of treatment. In other words, the potential outcomes of employment are not independent of the treatment status and 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 is beyond the scope of the reduced form framework in this paper.

$$y_{ijt} = \theta_{equalwork(j(i))} + \sum_{s \in \mathcal{S}} \alpha_s * D_{j(i,s)s} \times \theta_s \times Male_i + \sum_{s \in \mathcal{S}} \gamma_s * D_{j(i,s)s} \times \theta_s + \quad (1)$$

$$\theta_{g(i),b(i)} + \tau * D_{j(i,t)t} \times Male_i + \psi D_{j(i,t)t} + \delta Male_i +$$

$$X'_{ij(i,t)t}\beta + \delta_{CBA \times t} + \delta_{ind} + e_{ijt}$$

where $\mathcal{S} \equiv \{2014 : 2019\} \setminus \{2017\}$ is the set of years that excludes 2017, the year prior to policy announcement, $\theta_{equalwork(j(i))}$ 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. 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, we allow for a flexible CBA by year fixed effect $\delta_{CBA \times t}$.[20] Finally, δ_{ind} are industry fixed effects. Allowing for further flexibility by incorporating industry by time and occupation by time fixed effects does not change the results.

We do not include worker fixed effects in the estimating equation, since the policy exploits variation between workers of different gender within equal-work cells. Additionally, including a worker fixed effect will subsume the time-invariant dummy of the worker's gender and, we would not be able to identify the conditional base gender wage gap in small firms in the year prior to policy implementation.[21]

We estimate equation (1) on data in the set of years 2014-2019. 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 with θ_s being the time dummies for $s \in \mathcal{S}$. 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 the between-gender parallel trends assumption may hold—i.e., the estimated coefficients α_s is statistically indistinguishable from zero for all $s < 2017$,—within-gender parallel trends may fail for both men and women in equal

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[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 (Card & Cardoso 2022). The fixed effect $\delta_{CBA \times t}$ will also account for any such changes as well as all other time-variant unobserved changes in CBA.

[21]Furthermore, as discussed in the Appendix of Bonhomme et al. (2019), inclusion of a worker-firm fixed effect requires additional assumptions on exogenous mobility.

magnitude. In such a case, we can still make causal claims about the effect of the law on the gender wage gap, but we cannot make causal statements about the underlying mechanisms—specifically, how the wages of men and women evolved individually as a result 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 jobs associated with gaps above five percent had to reduce wage disparities among its workers within the job title, else potentially face repucussions.

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 B.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.

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 the two years following the 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 B.3, showing that the pay equity law increased the gender wage gap in these jobs by 2.0 to 2.9 percentage points

Figure 6: Jobs where average pre-policy gender wage gap was above five percent
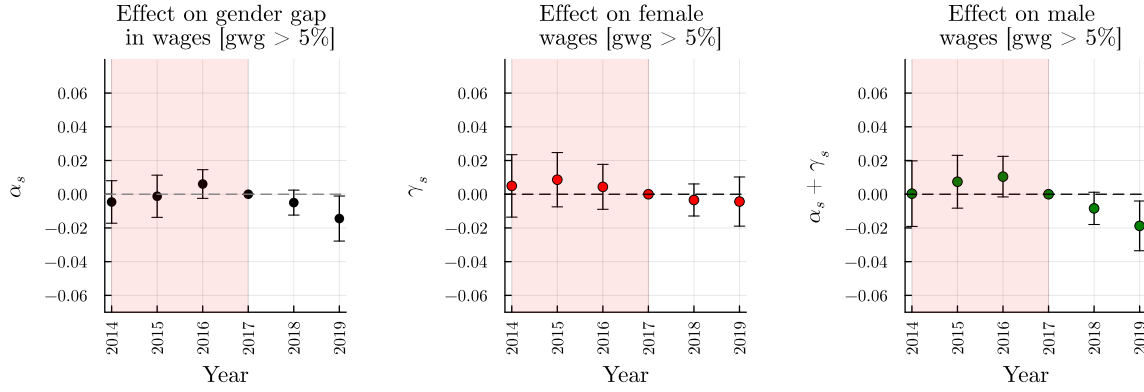
Conditional base gender gap in large firms
= 0.155 (0.031)
By 2019, the conditional gender gap
decreased by 9.22% to 0.141

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.

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 B.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 statis-

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.
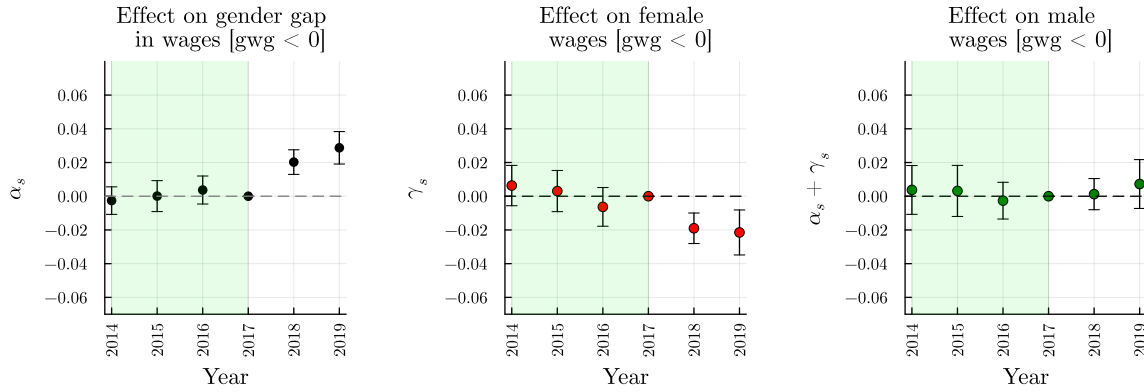
tical difference in the evolution of the gender wage gap between large and small firms, relative to how they differed in 2017—supporting our conditional parallel trends assumption.

The overall increase in the gender wage gap shown in Figure 8 is due to a larger decline in 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
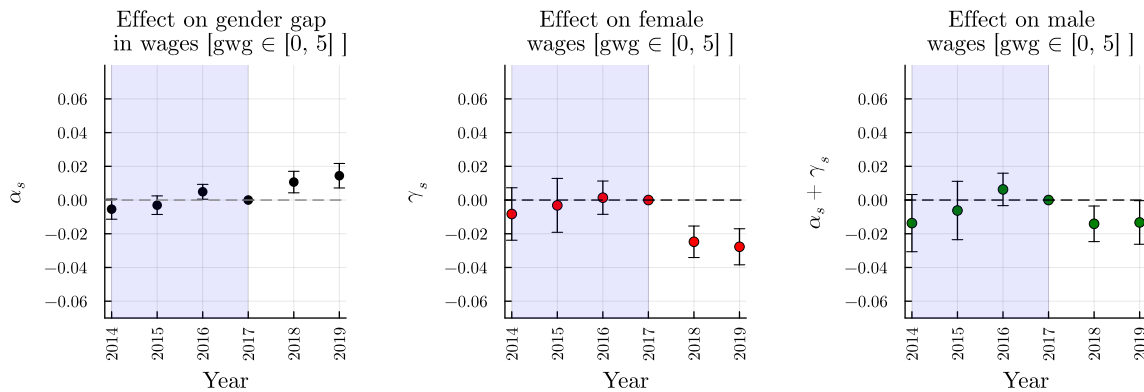
Conditional base gender gap in large firms
= 0.07 (0.019)
By 2019, the conditional gender gap
increased by 21.02% to 0.085

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 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 pre-existing gender wage gaps within firms. For completeness, Figure A.4 presents the time-varying estimates of the policy effect, with the full set of coefficients shown in column 4 of Table B.3. Upon examining Figure A.4, 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.

Furthermore, the second and third panels of Figure A.4 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 B.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.

6.5 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%.[22] 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 A.5 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 A.6, the law had a more pronounced effect in industries with an above-median share of female

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[22]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.

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 A.7, 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 A.5 and A.6 is that the law's unintended consequences are more pronounced in industries with an above-median share of male workers.

6.6 Can productivity gaps explain the results?

Worker productivity exhibits stronger ties to wages in high-skill occupations relative to low-skill occupations (Mueller et al. 2017). Since administrative matched employer-employee datasets lack individual-level productivity measures, we exploit this differential relationship to test whether the estimated policy impacts are driven by firms aligning wages more closely to productivity. Specifically, if gaps reflected mis-alignment of worker productivity and their wages, we would expect: (1) larger impacts in low-skill occupations where the productivity-wage link is weaker, and (2) minimal impacts in high-skill occupations where wages closely track productivity. Conversely, uniform effects across skill levels would indicate strategic firm responses rather than productivity-based adjustments.

Our results presented in Figure A.8 and reported in Tables B.6 and B.7 reveal a clear pattern: productivity considerations help explain the intended consequences of the law but not the unintended ones. In jobs with pre-policy gaps exceeding 5%, gap reduction occurs exclusively among low-skill workers—a 15.19% decrease by 2019 driven by slower male wage growth. High-skill occupations show no significant effects. Similary, among jobs with negative wage gaps, effects are concentrated among low-skill occupations where the conditional gap reduces by 64.26% through slower female wage growth. A very small subset of jobs with negative gaps within the high skill occupations exhibit accelerated male wage growth.[23] In both cases, the policy impacts being concentrated in low-skill occupations suggests the law corrected gaps that potentially existed because of productivity-wage misalignment.

In contrast, jobs with modest positive gaps (0-5%) show the most telling pattern. This unintended effect occurs uniformly across both skill levels—gap widening of 16.82% in high-skill and 24.28% in low-skill occupations—and is driven by reduced female wage growth. The presence of this pattern in high-skill occupations, where wages closely track productivity, indicates that productivity differences cannot explain these unintended consequences.

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[23]Despite constituting a small share, this differential impact likely reflect factors beyond productivity such as gender differences in bargaining power, or the documented "ask gap" particularly in high-skill occupations where individual negotiation could be more common conditional on men earning lower than women on average.

These findings suggest the law's intended effects may indeed address productivity-wage misalignments, particularly in low-skill occupations where such gaps could persist. The dominant mechanism of slower male wage growth in low-skill jobs suggests firms may be addressing situations where males were overcompensated relative to productivity, while simultaneously ensuring compliance with the regulatory threshold. Similarly, impacts in negative gap jobs also being concentrated among low-skill occupations suggest an analogous mechanism at play. However, the uniform widening of gaps in the 0-5% range across skill levels points to strategic firm responses to the 5% target rather than productivity-based adjustments. This distinction is crucial for understanding both the law's successes and its unintended consequences.

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 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.[24]

$$y_{ijt} = \theta_{equalwork(j(i))} + \sum_{s \in \mathcal{S}} \alpha_s^{itt} * D_{j(i,2017)} \times \mathbb{1}[t = s] \times Male_i + \sum_{s \in \mathcal{S}} \gamma_s^{itt} * D_{j(i,2017)} \times \theta_s^{itt} \quad (2)$$

$$+ \; \theta_{g(i),b(i)}^{itt} \tau^{itt} * D_{j(i,2017)} \times Male_i + \delta^{itt} Male_i \quad (3)$$

$$+ \; X'_{ij(i,t)t} \beta^{itt} + \delta_{CBA \times t}^{itt} + \delta_{ind}^{itt} + + e_{ijt}^{itt}$$

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. Estimating equation 2 differs from estimating 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.

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[24]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.

We report the event study estimates of equation 2 in Figure A.9 and Table B.8. 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.

6.7.2 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 A.10 and A.11 and in tables B.9 and B.10. 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.

6.7.3 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. Clearly such labor market dynamics are not specific to firms of size around 250, rather these are common over the firm size distribution. 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 B.12 and Figure A.12. 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 B.13 Figure A.13.

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 A.14 and Table B.11, 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.

6.7.5 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 G.2 and Figure G.1. We find precise null effects on the gender composition within jobs. This alleviates concerns that any changes in the wage growth that we observe are driven by changes in the reallocation of male and female workers across jobs.

7 Conclusion

To address gender-based wage inequalities, many countries have implemented policies to promote "*equal pay for equal work*". However, evidence of their causal impact remains limited due to their uniform mandates that lack clean identifying variation, data constraints in defining "equal work", and ability for firms to circumvent regulations. In 2018, Portugal introduced comprehensive pay equity legislation requiring firms with over 250 employees to justify or remediate gender pay gaps exceeding 5% within job titles or face penalties—providing unique variation that addresses these longstanding empirical challenges.

Our paper highlights how labor market institutions and wage-setting are crucial for credibly studying pay equity laws. Portugal's industry-level CBAs that define job titles and their wage floors provide the key component to define "equal work". Furthermore, CBA oversight at the industry level plausibly makes it extremely challenging for firms to manipulate employment and circumvent the law, in comparison to countries where wage setting is primarily at the firm level. We empirically verify no systematic manipulation of firm size around the 250-worker threshold. Given this, three features enable identification of the ATT of the pay equity law on worker wages: the 250-worker threshold provides policy variation, industry-wide CBAs allow us to precisely define 'equal work', and firms' latitude in adding wage cushions on top of CBA-set wage floors creates within-job-title wage variation.

Using administrative matched employer-employee data and an event study design with "equal work" fixed effects, we document substantial heterogeneity in the law's impact that aggregate analyses would obscure. In jobs with average pre-existing gaps exceeding 5%, the policy reduced conditional wage gaps by 9% through slower male wage growth. In the small fraction of jobs where women initially outearned men, negative gaps halved through reduced female wage growth. However, in jobs with gaps between 0-5%—below the enforcement threshold—disparities widened by 21% due to disproportionately slower female wage growth.

Reassuringly, we find evidence suggesting that the law's intended effects of reducing large wage gaps can be explained by correcting wages that were initially misaligned with produc-

tivity. However, the law's unintended effects cannot be explained by productivity differences. Furthermore, the unintended effects were more pronounced in male-dominated industries. In Appendix F, we discuss the plausibility of various mechanisms—including taste-based discrimination—that future research could empirically test.

Our findings reveal both the successes and unintended consequences of well-enforced pay-equity laws with uniform regulatory targets. While the 5% regulatory target successfully reduced large disparities, it may have inadvertently encouraged firms to widen smaller gaps up to the allowable limit, by clarifying the repercussions only if wage gaps exceeded the uniform target. Such heterogeneity highlights complexities that diverge from pathways documented in Bailey et al. (2024). The unintended consequences we identify are significant—especially as the EU prepares to adopt similar policies based on gender gap targets by June 2026 for employers with 100+ employees—offering important insights for future policy design to achieve "*equal pay for equal work*".

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A Tables and Figures

Figure A.1: Empirical Cumulative Distribution of firm size over time

Subplot (a): ECDF of firm size   Subplot (b): Zoomed in ECDF

Subplot (c): Conditional ECDF
Firm size in between 200 and 300
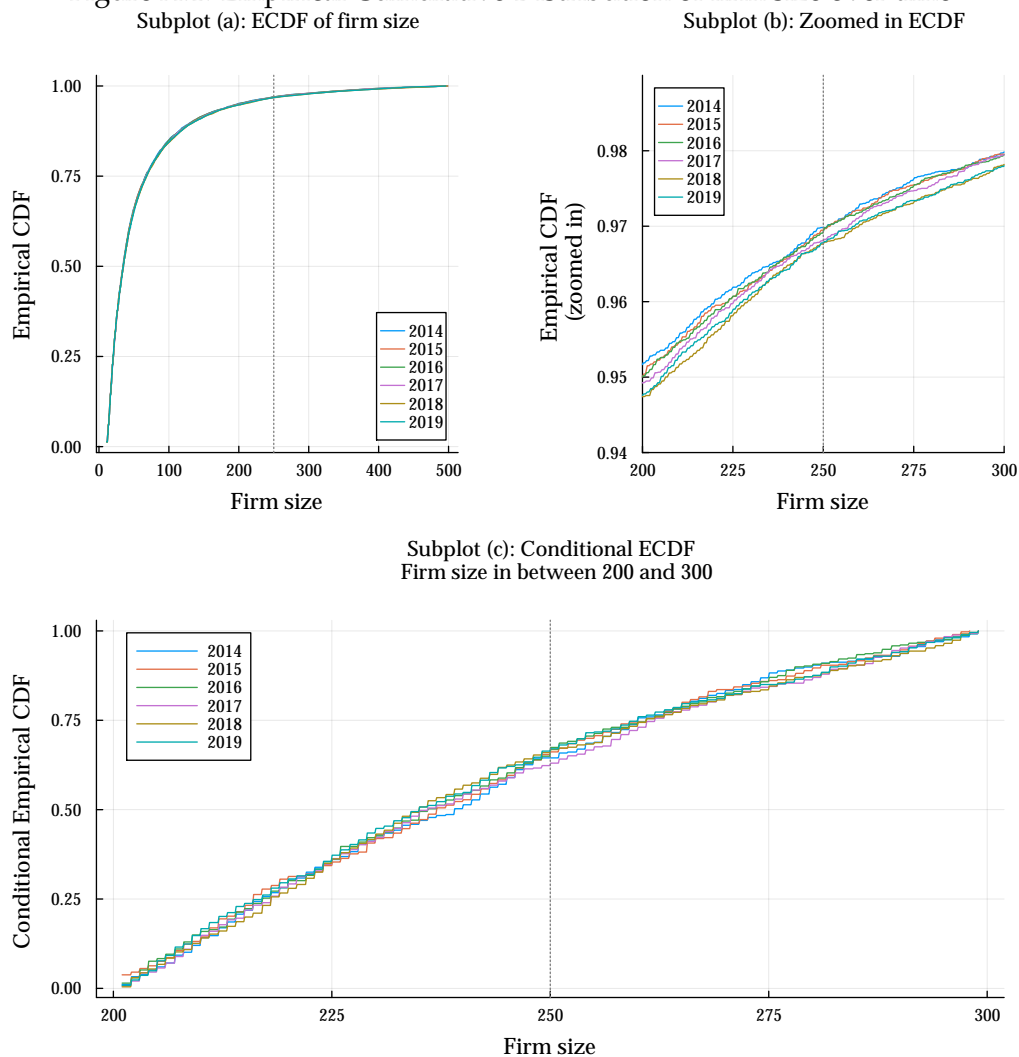
Notes: In Figure A.1-(a) we plot the empirical cumulative distribution function (ECDF) of firm size for each year from 2014 to 2019. We zoom in on the part of the ECDF around the threshold of 250 workers in Figure A.1-(b). In Figure A.1-(c) we plot the conditional ECDF by conditioning on firm size being in between 200 and 300 workers.

Figure A.2: Proportion of workers working in firms employing above 250 workers over time
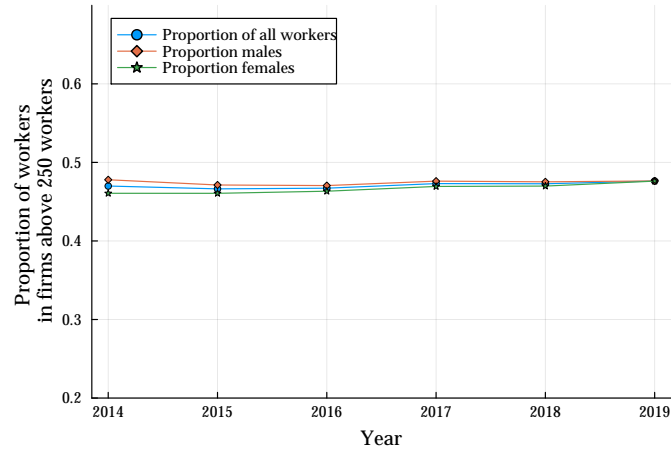
Notes: This figure plots the proportion of workers working in firms employing above 250 workers over time, for all workers and also by gender of workers. The pay equity law was announced in 2018.

Figure A.3: Share of all workers and female across industries
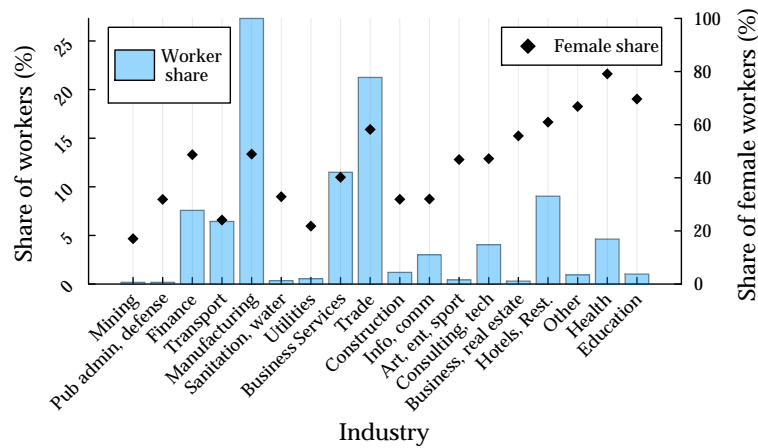
Notes: This figure plots the share of all workers by industry on the left y-axis represented by the blue bars, and the share of female workers on the right y-axis represented by the black dots. The industries are sorted in the descending order of the share of workers in jobs with positive gender wage gaps as in Figure 5.

Figure A.4: Wage growth of all workers pooled together
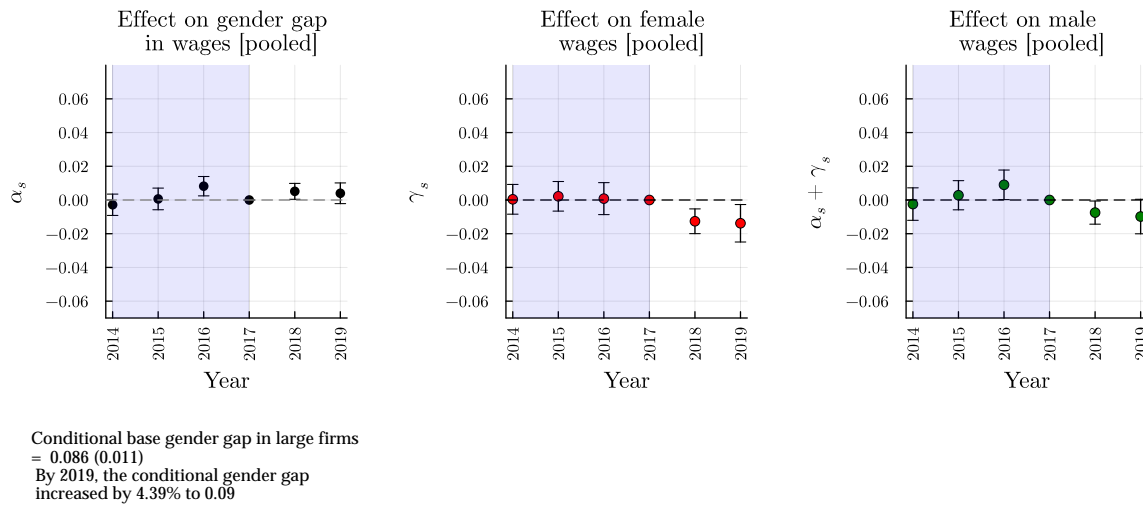
Conditional base gender gap in large firms
= 0.086 (0.011)
By 2019, the conditional gender gap
increased by 4.39% to 0.09

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.

31

Figure A.5: Jobs where average pre-policy gender wage gaps were under 0%
(a) Industries with above median share of male workers
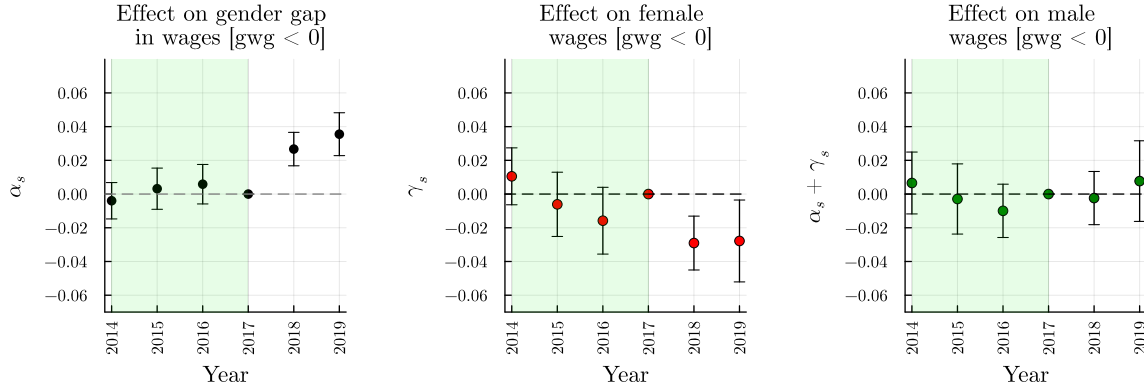
Conditional base gender gap in large firms
= - 0.048 (0.037)
By 2019, the conditional gender gap
increased by 74.9% to - 0.012

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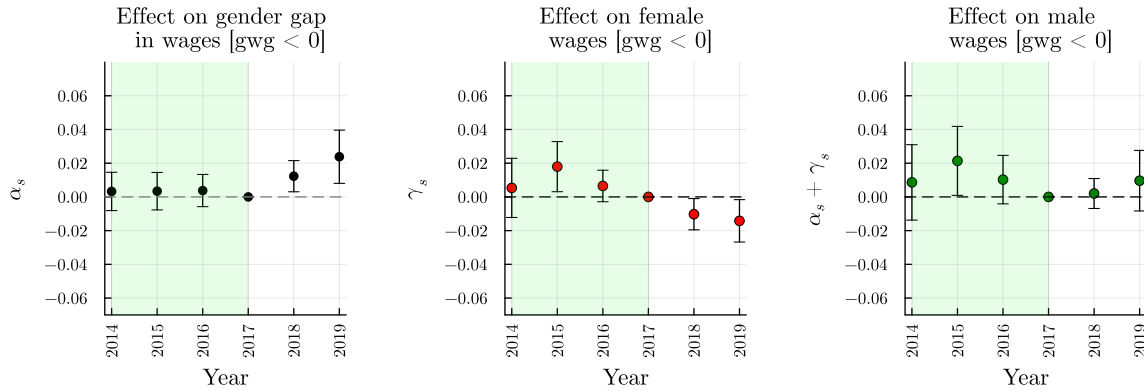
Conditional base gender gap in large firms
= - 0.094 (0.037)
By 2019, the conditional gender gap
increased by 25.25% to - 0.07

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 A.6: Jobs where average pre-policy gender wage gaps were above 5%
(a) Industries with above median share of male workers
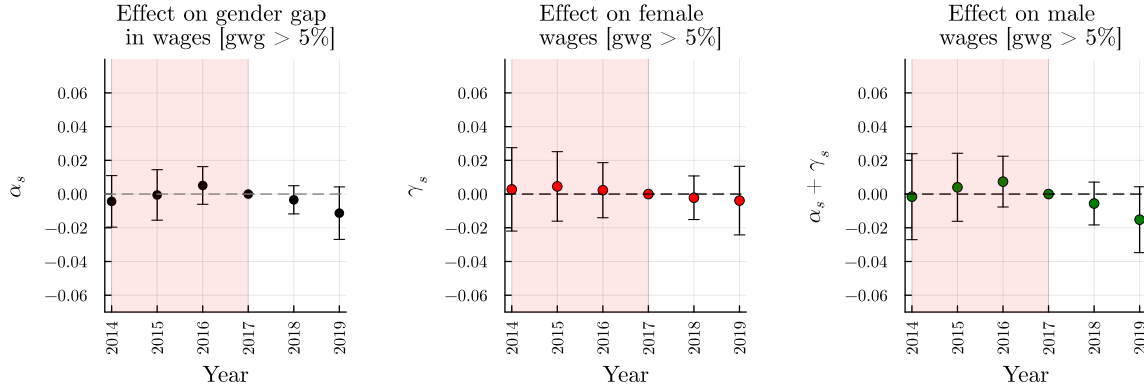
Conditional base gender gap in large firms
= 0.129 (0.036)
By 2019, the conditional gender gap
decreased by 8.98% to 0.117

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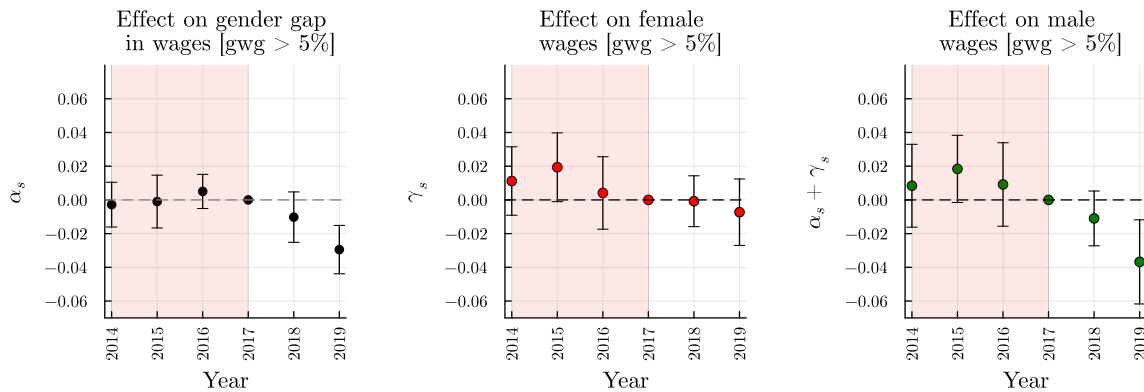
Conditional base gender gap in large firms
= 0.25 (0.052)
By 2019, the conditional gender gap
decreased by 11.76% to 0.221

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.

33

Figure A.7: Jobs where average pre-policy gender wage gaps were in between 0 and 5%

(a) Industries with above median share of male workers

Effect on gender gap in wages [gwg ∈ [0, 5]]

Effect on female wages [gwg ∈ [0, 5]]

Effect on male wages [gwg ∈ [0, 5]]
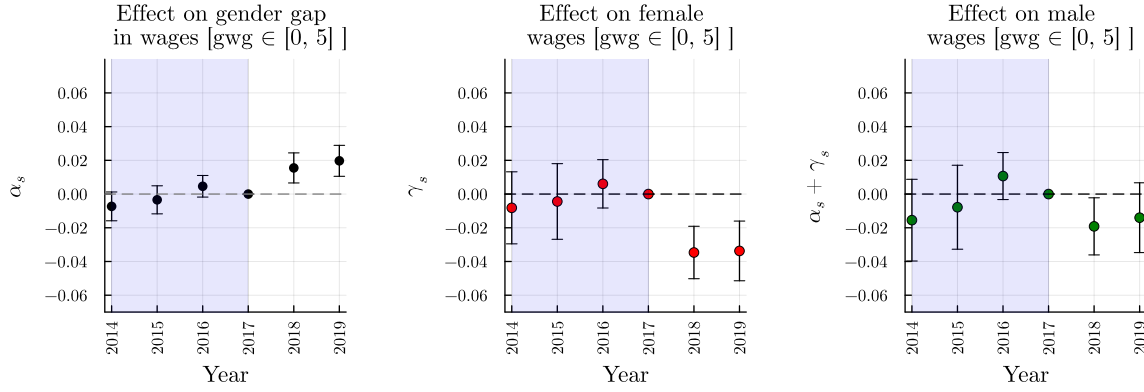
Conditional base gender gap in large firms
= 0.061 (0.021)
By 2019, the conditional gender gap
increased by 31.91% to 0.08

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

Effect on gender gap in wages [gwg ∈ [0, 5]]

Effect on female wages [gwg ∈ [0, 5]]

Effect on male wages [gwg ∈ [0, 5]]

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.

Figure A.8: Event-study ATT estimates on wages in high and low skill occupations
(a) Jobs where average pre-policy gender wage gap was below zero percent

High-skill: Conditional base gap = -0.095 (0.052)
By 2019: increased by 37.51% to -0.059

Low-skill: Conditional base gap = -0.034 (0.018)
By 2019: increased by 64.08% 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. This sample only includes workers in low skill occupations.. 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

High-skill: Conditional base gap = 0.153 (0.053)
By 2019: decreased by 2.96% to 0.148

Low-skill: Conditional base gap = 0.152 (0.021)
By 2019: decreased by 15.25% to 0.129

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 only includes workers in low skill occupations.. 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

High-skill: Conditional base gap = 0.125 (0.052)
By 2019: increased by 16.78% to 0.146

Low-skill: Conditional base gap = 0.043 (0.018)
By 2019: increased by 23.2% to 0.053

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 only includes workers in low skill occupations.. 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 A.9: Event-study ITT estimates on wages
(a) Jobs where average pre-policy gender wage gap was below zero percent

Conditional base gender gap in large firms
= - 0.059 (0.027)
By 2019, the conditional gender gap
increased by 50.91% to - 0.029

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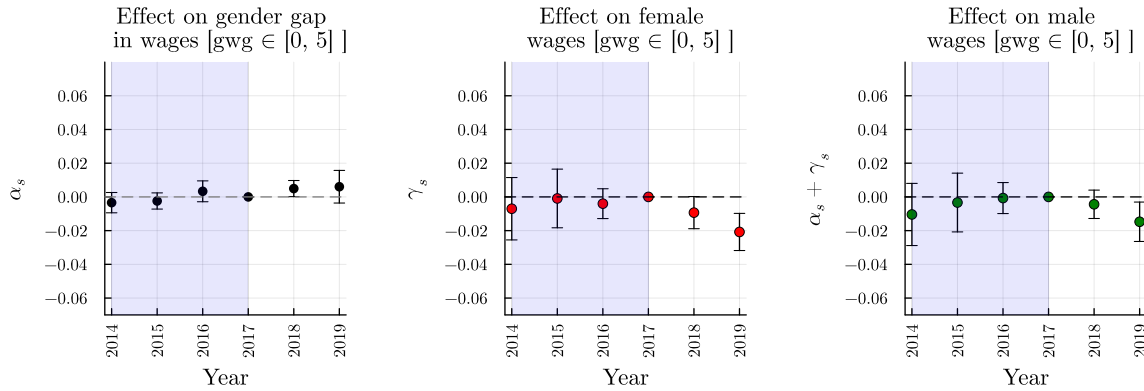
(b) Jobs where average pre-policy gender wage gap was in between zero and five percent

Conditional base gender gap in large firms
= 0.07 (0.019)
By 2019, the conditional gender gap
increased by 20.97% to 0.085

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.

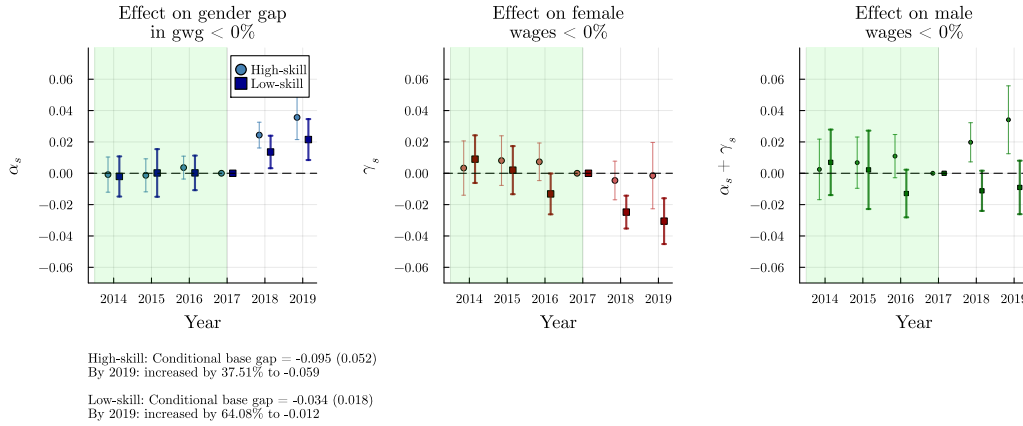### (c) Jobs where average pre-policy gender wage gap was above five percent

Conditional base gender gap in large firms
= 0.155 (0.031)
By 2019, the conditional gender gap
decreased by 9.61% to 0.14

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 A.10: Event-study ATT 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 18.25% to 0.008

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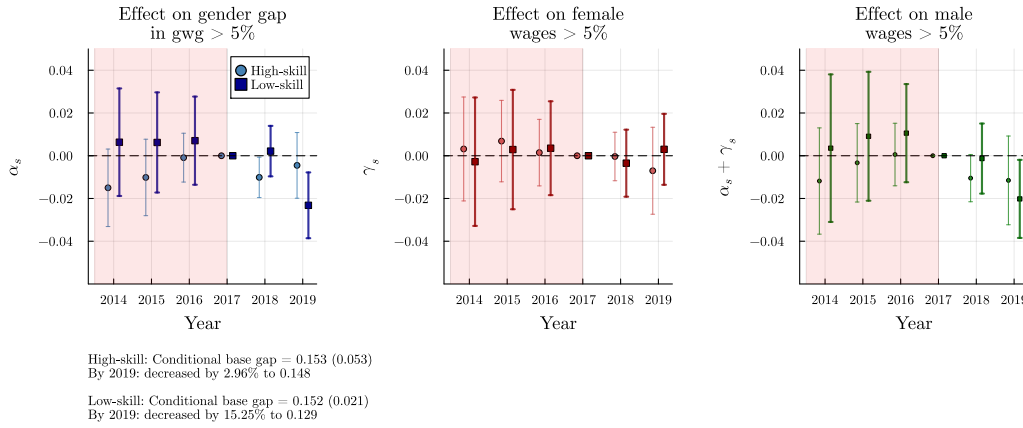
(b) Jobs where average pre-policy gender wage gap was above five percent

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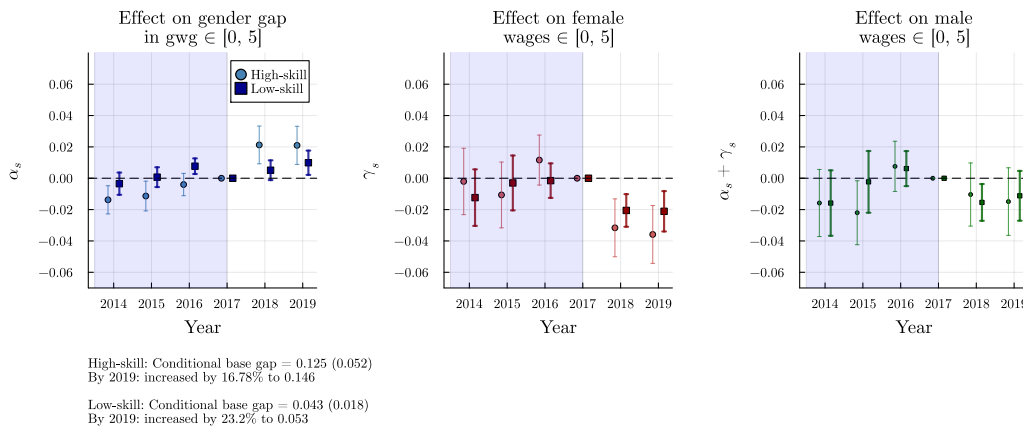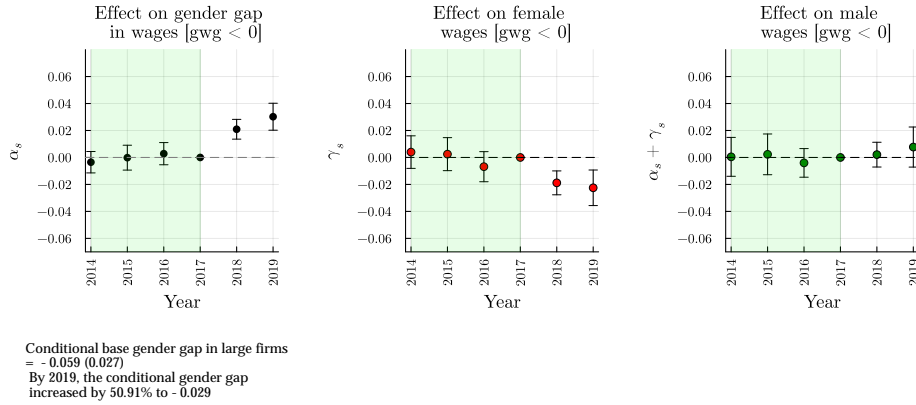By 2019, the conditional gender gap
increased by 100.0% to 0.0

Notes: The figure presents 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

Conditional base gender gap in large firms
= - 0.01 (0.006)
By 2019, the conditional gender gap
decreased by 0.91% to - 0.01

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 A.11: 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.
38

Figure A.12: 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

Conditional base gender gap in large firms
= - 0.056 (0.029)
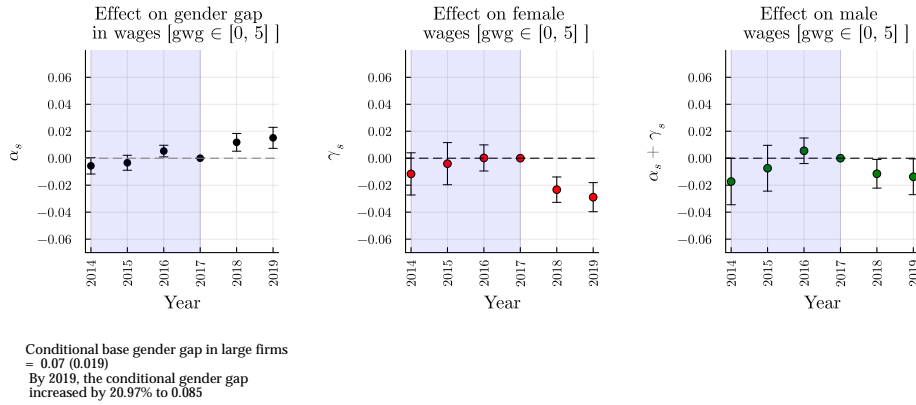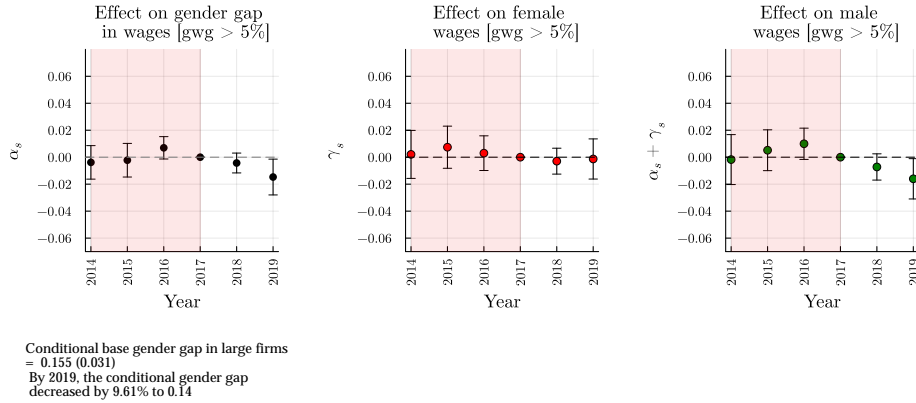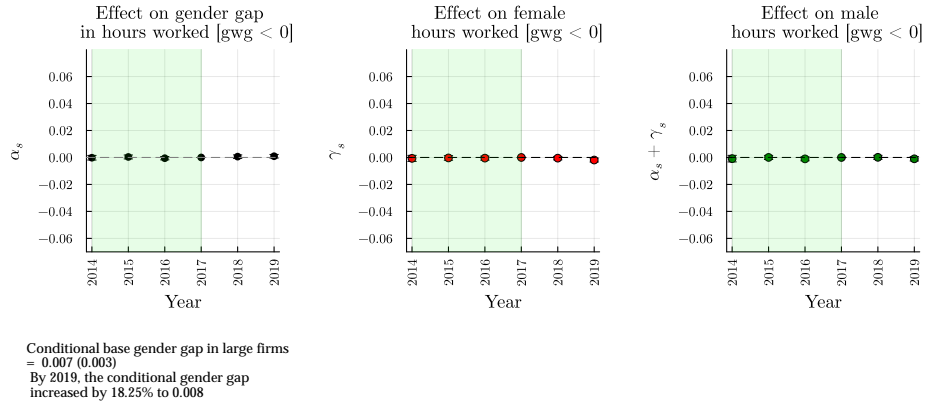By 2019, the conditional gender gap
increased by 51.85% to - 0.027

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
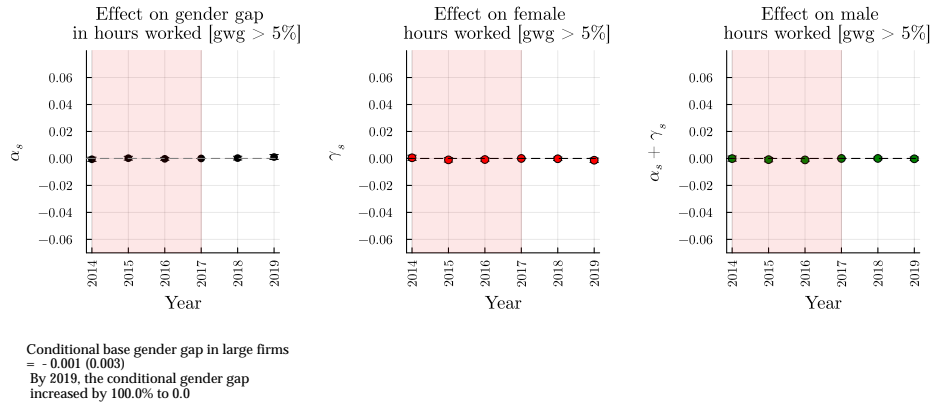

Conditional base gender gap in large firms
= 0.148 (0.031)
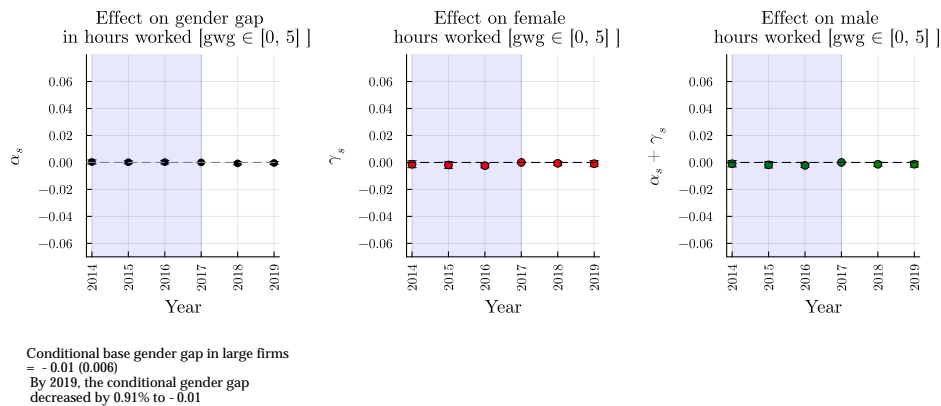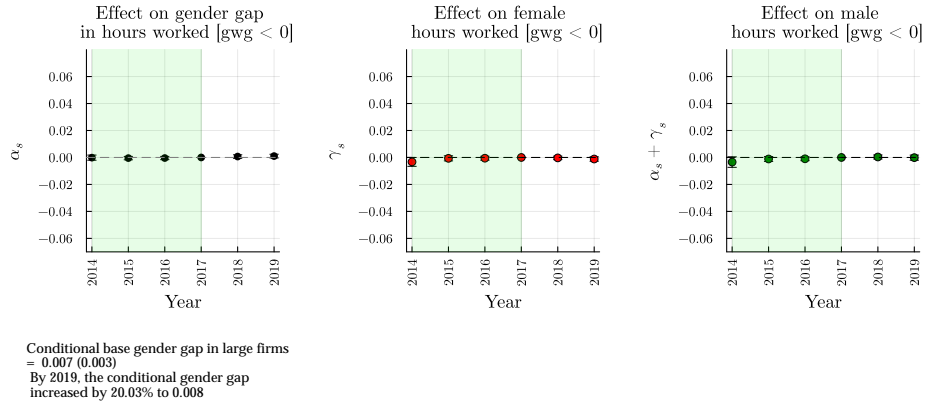By 2019, the conditional gender gap
decreased by 9.16% to 0.134

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

Conditional base gender gap in large firms
= 0.075 (0.02)
By 2019, the conditional gender gap
increased by 19.4% to 0.09

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 A.13: 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

Conditional base gender gap in large firms
= - 0.056 (0.029)
By 2019, the conditional gender gap
increased by 51.85% to - 0.027

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

Conditional base gender gap in large firms
= 0.148 (0.031)
By 2019, the conditional gender gap
decreased by 9.16% to 0.134

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
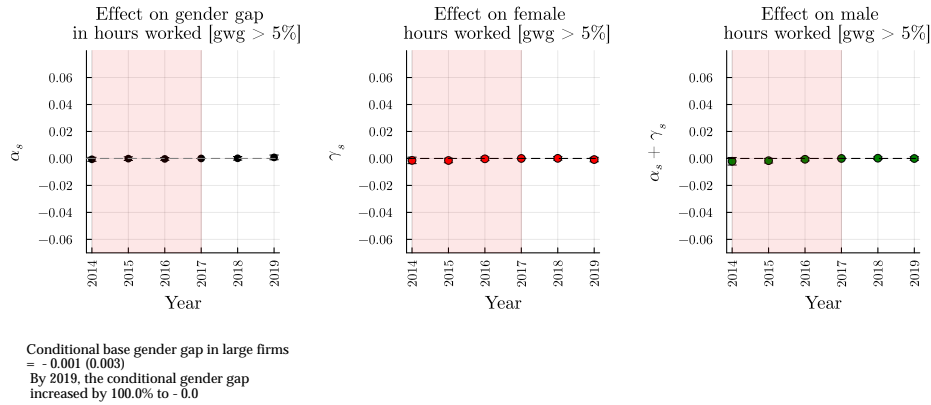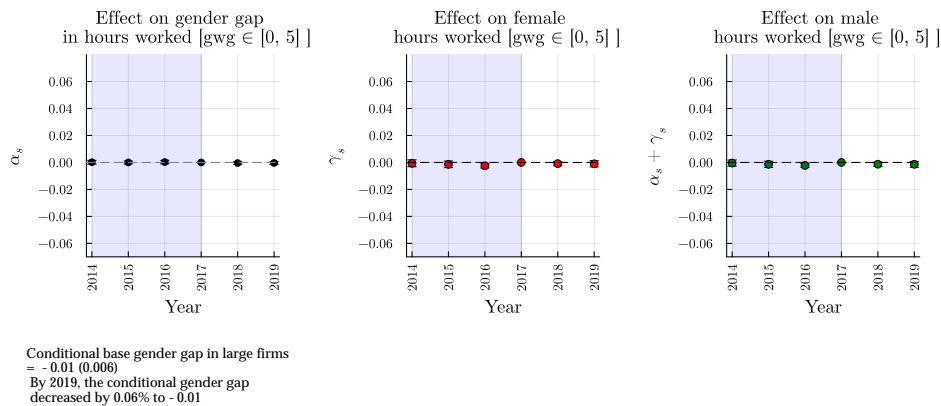

Conditional base gender gap in large firms
= 0.075 (0.02)
By 2019, the conditional gender gap
increased by 19.4% to 0.09

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.
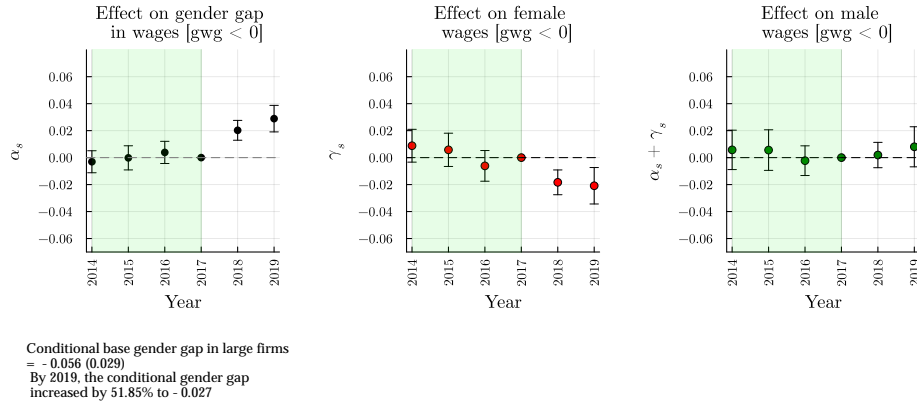
Figure A.14: Event-study ATT estimates on wages of stayers
(a) Jobs where average pre-policy gender wage gap was below zero percent

Conditional base gender gap in large firms
= - 0.063 (0.027)
By 2019, the conditional gender gap
increased by 42.69% to - 0.036

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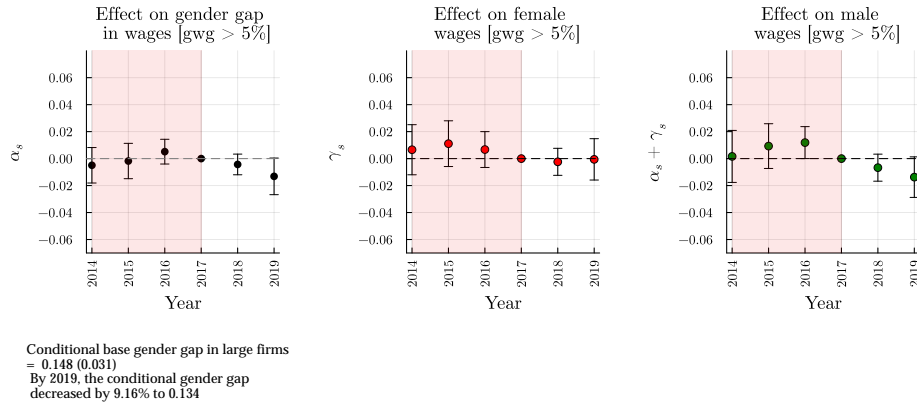
(b) Jobs where average pre-policy gender wage gap was above five percent

Conditional base gender gap in large firms
= 0.15 (0.031)
By 2019, the conditional gender gap
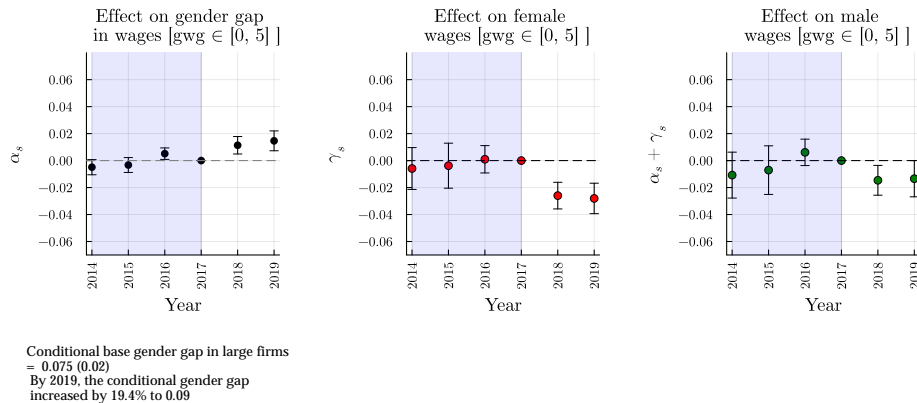decreased by 6.56% to 0.14

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

Conditional base gender gap in large firms
= 0.068 (0.019)
By 2019, the conditional gender gap
increased by 24.96% 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. 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.
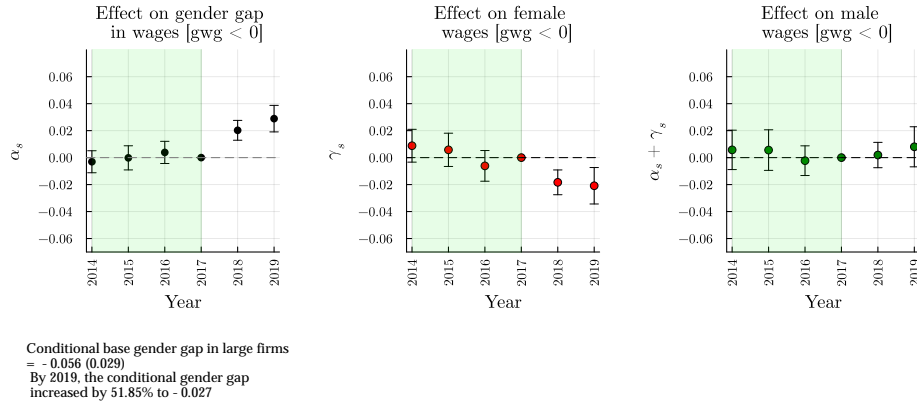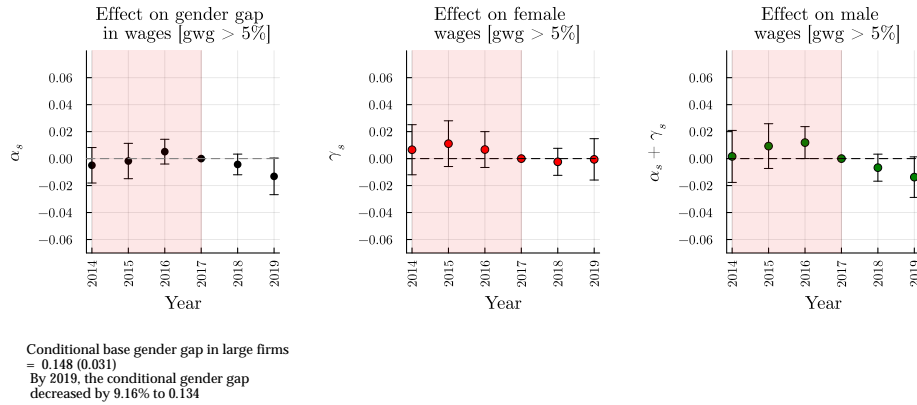
B Tables

Table B.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 B.2: Unconditional Gender Wage Gaps (%) by Firm Size and Gap Category, 2014-2017

Category	Firm size < 250	Firm size ≥ 250	Pooled Sample
All Firms	5.21	4.94	5.07
Jobs with Gap > 5%	18.74	13.30	15.87
Jobs with 0% ≤ Gap ≤ 5%	2.07	2.17	2.13
Jobs with Gap < 0%	-9.04	-5.22	-7.35

Note: The table reports unconditional gender wage gaps within jobs categorized based on their average gender wage gap in 2014-2017.

Table B.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 B.4: Estimates of the effect of the pay-equity law on log hourly wages of workers in industries with above median share of female workers

	$GWG_{prepolicy} < 0\%$	$GWG_{prepolicy} \in [0\%, 5\%]$	$GWG_{prepolicy} > 5\%$	Pooled
	(1)	(1)	(1)	(1)
D_{jt}	-0.019*	0.021*	-0.005	-0.001
	(0.011)	(0.013)	(0.025)	(0.009)
Male	-0.116***	0.100***	0.278***	0.102***
	(0.036)	(0.033)	(0.052)	(0.020)
Female wages in 2019	-0.014**	-0.021***	-0.007	-0.012***
	(0.006)	(0.006)	(0.010)	(0.004)
Female wages in 2018	-0.010**	-0.009*	-0.00	-0.007**
	(0.005)	(0.005)	(0.008)	(0.003)
Female wages in 2016	0.006	-0.004	0.004	0.001
	(0.005)	(0.005)	(0.011)	(0.004)
Female wages in 2015	0.018**	-0.00	0.019*	0.009
	(0.008)	(0.009)	(0.010)	(0.005)
Female wages in 2014	0.005	-0.007	0.011	0.00
	(0.009)	(0.009)	(0.010)	(0.006)
$D_{jt} \times$ Male	0.022***	0.002	-0.028***	-0.002
	(0.005)	(0.002)	(0.008)	(0.003)
Gender wage gap in 2019	0.024***	0.006	-0.029***	0.00
	(0.008)	(0.005)	(0.007)	(0.004)
Gender wage gap in 2018	0.012***	0.005**	-0.010	0.003
	(0.005)	(0.002)	(0.008)	(0.003)
Gender wage gap in 2016	0.004	0.003	0.005	0.004
	(0.005)	(0.003)	(0.005)	(0.003)
Gender wage gap in 2015	0.003	-0.002	-0.00	0.00
	(0.006)	(0.002)	(0.008)	(0.003)
Gender wage gap in 2014	0.003	-0.003	-0.003	-0.00
	(0.006)	(0.003)	(0.007)	(0.003)
Male wages in 2019	0.01	-0.015**	-0.037***	-0.011*
	(0.01)	(0.007)	(0.012)	(0.006)
Male wages in 2018	0.002	-0.004	-0.011	-0.004
	(0.007)	(0.005)	(0.011)	(0.004)
Male wages in 2016	0.01	-0.001	0.009	0.005
	(0.007)	(0.006)	(0.012)	(0.005)
Male wages in 2015	0.021**	-0.003	0.018	0.009
	(0.009)	(0.009)	(0.013)	(0.006)
Male wages in 2014	0.009	-0.01	0.008	0.0
	(0.011)	(0.01)	(0.012)	(0.007)
Equal work FE	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓
CBA-year FE	✓	✓	✓	✓
Dependent mean (all)	1.663	1.606	1.906	1.725
Dependent mean (untreated 2017)	1.664	1.571	1.851	1.719
N	380,600	557,273	329,501	1,951,652
R^2	0.891	0.898	0.869	0.896

Notes: The table above shows the ATT estimates from the event study design equation on the sample of workers in industries with above median share of female workers. 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 B.5: Estimates of the effect of the pay-equity law on log hourly wages of workers in industries with above median share of male workers

	$GWG_{prepolicy} < 0\%$	$GWG_{prepolicy} \in [0\%, 5\%]$	$GWG_{prepolicy} > 5\%$	Pooled
	(1)	(1)	(1)	(1)
D_{jt}	0.009	0.035***	0.041***	0.030***
	(0.012)	(0.009)	(0.012)	(0.008)
Male	-0.067*	0.078***	0.196***	0.103***
	(0.037)	(0.020)	(0.028)	(0.013)
Female wages in 2019	-0.028**	-0.034***	-0.004	-0.016
	(0.012)	(0.009)	(0.010)	(0.010)
Female wages in 2018	-0.029***	-0.035***	-0.002	-0.015**
	(0.008)	(0.008)	(0.007)	(0.006)
Female wages in 2016	-0.016	0.006	0.002	-0.00
	(0.010)	(0.007)	(0.008)	(0.007)
Female wages in 2015	-0.006	-0.004	0.005	0.00
	(0.010)	(0.011)	(0.011)	(0.006)
Female wages in 2014	0.011	-0.008	0.003	0.003
	(0.009)	(0.011)	(0.013)	(0.006)
$D_{jt} \times$ Male	0.020***	-0.017***	-0.067***	-0.020***
	(0.005)	(0.003)	(0.013)	(0.006)
Gender wage gap in 2019	0.036***	0.020***	-0.011	0.006
	(0.006)	(0.005)	(0.008)	(0.005)
Gender wage gap in 2018	0.027***	0.016***	-0.003	0.008**
	(0.005)	(0.005)	(0.004)	(0.003)
Gender wage gap in 2016	0.006	0.005	0.005	0.009**
	(0.006)	(0.003)	(0.006)	(0.005)
Gender wage gap in 2015	0.003	-0.003	-0.00	0.002
	(0.006)	(0.004)	(0.008)	(0.005)
Gender wage gap in 2014	-0.004	-0.007*	-0.004	-0.004
	(0.006)	(0.004)	(0.008)	(0.004)
Male wages in 2019	0.008	-0.014	-0.015	-0.009
	(0.014)	(0.01)	(0.013)	(0.011)
Male wages in 2018	-0.002	-0.019**	-0.006	-0.008
	(0.01)	(0.009)	(0.008)	(0.007)
Male wages in 2016	-0.01	0.011	0.007	0.009
	(0.012)	(0.008)	(0.01)	(0.009)
Male wages in 2015	-0.003	-0.008	0.004	0.003
	(0.012)	(0.012)	(0.013)	(0.008)
Male wages in 2014	0.007	-0.015	-0.002	-0.001
	(0.01)	(0.012)	(0.015)	(0.007)
Equal work FE	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓
CBA-year FE	✓	✓	✓	✓
Dependent mean (all)	1.894	1.769	2.001	1.864
Dependent mean (untreated 2017)	1.824	1.639	1.881	1.777
N	459,440	764,342	884,976	3,263,040
R^2	0.909	0.922	0.886	0.902

Notes: The table above shows the ATT estimates from the event study design equation on the sample of workers in industries with above median share of male workers. 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 B.6: ATT Estimates of the effect of the pay-equity law on wages in high-skill occupations

	High-skill, $GWG_{prepolicy} < 0\%$ (1)	High-skill, $GWG_{prepolicy} \in [0\%, 5\%]$ (1)	High-skill, $GWG_{prepolicy} > 5\%$ (1)
D_{jt}	-0.010	0.032***	0.043***
	(0.008)	(0.012)	(0.013)
Male	-0.133***	0.137***	0.228***
	(0.051)	(0.052)	(0.043)
Female wages in 2019	-0.001	-0.036***	-0.007
	(0.011)	(0.009)	(0.010)
Female wages in 2018	-0.005	-0.032***	-0.00
	(0.006)	(0.009)	(0.006)
Female wages in 2016	0.007	0.012	0.001
	(0.006)	(0.008)	(0.008)
Female wages in 2015	0.008	-0.011	0.007
	(0.008)	(0.011)	(0.010)
Female wages in 2014	0.003	-0.002	0.003
	(0.009)	(0.011)	(0.012)
$D_{jt} \times$ Male	0.038***	-0.012***	-0.075***
	(0.005)	(0.004)	(0.017)
Gender wage gap in 2019	0.036***	0.021***	-0.005
	(0.007)	(0.006)	(0.008)
Gender wage gap in 2018	0.024***	0.021***	-0.010**
	(0.004)	(0.006)	(0.005)
Gender wage gap in 2016	0.004	-0.004	-0.00
	(0.004)	(0.004)	(0.006)
Gender wage gap in 2015	-0.001	-0.011**	-0.010
	(0.005)	(0.005)	(0.009)
Gender wage gap in 2014	-0.00	-0.014***	-0.015
	(0.006)	(0.005)	(0.009)
Male wages in 2019	0.034***	-0.015	-0.012
	(0.013)	(0.011)	(0.013)
Male wages in 2018	0.02***	-0.01	-0.01
	(0.008)	(0.011)	(0.008)
Male wages in 2016	0.011	0.008	0.001
	(0.007)	(0.009)	(0.01)
Male wages in 2015	0.007	-0.022*	-0.003
	(0.01)	(0.012)	(0.013)
Male wages in 2014	0.002	-0.016	-0.012
	(0.011)	(0.012)	(0.015)
Equal work FE	✓	✓	✓
Industry FE	✓	✓	✓
CBA-year FE	✓	✓	✓
Dependent mean (all)	2.161	2.143	2.306
Dependent mean (untreated 2017)	2.078	1.999	2.182
N	357,303	426,243	601,046
R^2	0.884	0.907	0.848

Notes: The table above shows the ATT estimates from the event study design equation for workers in high skill occupations. 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 B.7: ATT Estimates of the effect of the pay-equity law on wages in low-skill occupations

	Low-skill, $GWG_{prepolicy} < 0\%$ (1)	Low-skill, $GWG_{prepolicy} \in [0\%, 5\%]$ (1)	Low-skill, $GWG_{prepolicy} > 5\%$ (1)
D_{jt}	0.002	0.022***	0.011
	(0.015)	(0.009)	(0.018)
Male	-0.044**	0.048***	0.183***
	(0.018)	(0.017)	(0.017)
Female wages in 2019	-0.031***	-0.021***	0.003
	(0.007)	(0.007)	(0.008)
Female wages in 2018	-0.025***	-0.021***	-0.003
	(0.005)	(0.005)	(0.008)
Female wages in 2016	-0.013**	-0.002	0.004
	(0.007)	(0.006)	(0.011)
Female wages in 2015	0.002	-0.003	0.003
	(0.008)	(0.009)	(0.014)
Female wages in 2014	0.009	-0.012	-0.003
	(0.008)	(0.009)	(0.015)
$D_{jt} \times$ Male	0.010**	-0.005**	-0.031***
	(0.004)	(0.002)	(0.011)
Gender wage gap in 2019	0.022***	0.010**	-0.023***
	(0.007)	(0.004)	(0.008)
Gender wage gap in 2018	0.014**	0.005	0.002
	(0.005)	(0.003)	(0.006)
Gender wage gap in 2016	0.00	0.008***	0.007
	(0.006)	(0.003)	(0.011)
Gender wage gap in 2015	0.00	0.00	0.006
	(0.008)	(0.003)	(0.012)
Gender wage gap in 2014	-0.002	-0.003	0.006
	(0.007)	(0.004)	(0.013)
Male wages in 2019	-0.009	-0.011	-0.02*
	(0.01)	(0.008)	(0.012)
Male wages in 2018	-0.011	-0.015**	-0.001
	(0.008)	(0.006)	(0.01)
Male wages in 2016	-0.013	0.006	0.011
	(0.009)	(0.006)	(0.015)
Male wages in 2015	0.002	-0.002	0.009
	(0.011)	(0.009)	(0.019)
Male wages in 2014	0.007	-0.016	0.004
	(0.01)	(0.01)	(0.02)
Equal work FE	✓	✓	✓
Industry FE	✓	✓	✓
CBA-year FE	✓	✓	✓
Dependent mean (all)	1.526	1.525	1.651
Dependent mean (untreated 2017)	1.507	1.459	1.59
N	482,723	895,503	613,432
R^2	0.804	0.830	0.779

Notes: The table above shows the ATT estimates from the event study design equation for workers in low skill occupations. 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 B.8: ITT 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)
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 B.9: ATT Estimates of the effect of the pay-equity law on log hours worked

	$GWG_{prepolicy} < 0\%$ (1)	$GWG_{prepolicy} \in [0\%, 5\%]$ (2)	$GWG_{prepolicy} > 5\%$ (3)	Pooled (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 B.10: ITT Estimates of the effect of the pay-equity law on log hours worked

	$GWG_{prepolicy} < 0\%$ (1)	$GWG_{prepolicy} \in [0\%, 5\%]$ (2)	$GWG_{prepolicy} > 5\%$ (3)	Pooled (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 B.11: 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 B.12: 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

	$GWG_{prepolicy} < 0\%$ (1)	$GWG_{prepolicy} \in [0\%, 5\%]$ (2)	$GWG_{prepolicy} > 5\%$ (3)	Pooled (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 B.13: 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.

C Identifying Assumptions

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

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

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 $\mathbf{d} \in \{0, 1\}^T, Y_{j,t}(\mathbf{d}) = Y_{j,t}(d_1, \dots, d_t)$

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

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

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

$$
=
$$

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

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

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

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

$$
=
$$

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

Observe that a gender-specifc conditional parallel trends would imply our conditional par-

allel 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).

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).

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 objective benchmarks for "acceptable" gender wage gaps. 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]

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.[27] Our empirical setting differs sharply not only by the near

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[25]This data restriction of the US Census and the ACS consequently requires necessary assumptions of any endogenous mobility to only occur within state by industry by occupation groups highlighted by Bailey et al. (2024). 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, 1982a,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.

[27]Specifically, Baker & 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 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 attenuated inference of the estimated effect towards

perfect degree of firm compliance with the law, but also from having more accurate information on employee wages and exploiting policy variation in firm size over time.

In line with non-compliance with pay equity laws, recent work by Passaro, Kojima & Pakzad-Hurson (2023) document non-compliance of firms with pay-equity law in Chile. Their analysis focuses on the intent to treat and involves data from firms that had at least 6 and no more than 13 total workers at the time of policy announcement. 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 difficult for firms to fire workers to fall below a policy threshold. Another important difference is that a policy defined firm size threshold of 250 (our case) is less likely to be manipulable and gender segregated than a far smaller policy defined firm size threshold like Chile's policy which had a firm size threshold of 10 workers. Finally, their result on widening of gender wage gaps within local labor markets, is driven by firms non-compliance with the law resulting in gender seggregation and hence compositional changes in local labor markets. In contrast, since firms in our setting comply with treatment assignment of the law, consequently we do not observe any evidence of compositional changes.

It is important to highlight the importance of the role of industry-defined collective bargaining agreements (CBAs) in our setting. Our results in comparison with the literature suggest that the presence of wage setting and bargaining at the industry level is crucial to prevent non-compliance by firms. Labor markets such as the ones in US, Canada and Chile, where the presence of industry-wide CBAs is not as prevalent, firms are more likely to non-compliance with pay-equity laws.

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 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.

To date, only highly aggregated statistics have been reported (e.g., the number of violations per month, or the number of cases at various stages of the remediation process). This limited public disclosure does not allow one to reasonably infer a gender wage gap within firms, thereby is different than many pay transparency laws (see, e.g., Bennedsen et al. (2022)).

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statistical insignificance.

As such, throughout the main text we interpret our findings as primarily reflecting the mandatory remediation component of the law, and not reflecting broader transparency channels.

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

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. These mechanisms are not exhaustive. Firms could additionally leverage private information on worker's outside offers, and bargaining power to justify continued gaps, or find it profitable to pay subsequent fines rather than fully eliminate wage disparities.

F.1 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.

F.2 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 target gender wage gap of 5%.

F.3 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 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. While Portugal et al. (2020) argues that amenities could vary across workers within a firm but not at job title level, so our equal work fixed effect and the absence of changes in gender composition across job titles within firms renders this mechanism as unlikely. That said, convincing evidence could only be obtained by identifying the impact of time-varying, unobserved amenities on wage inequality. This 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)).

G Results on firm level and firm-job level outcomes

Table G.1: 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 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. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

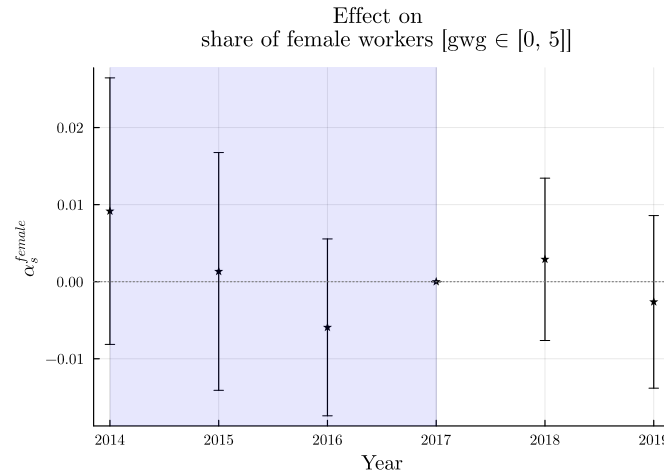Table G.2: Estimates of the effect of the pay-equity law on share of within job female workers

	$GWG_{prepolicy} < 0\%$ (1)	$GWG_{prepolicy} \in [0\%, 5\%]$ (2)	$GWG_{prepolicy} > 5\%$ (3)	Pooled (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. $^* p < 0.10$, $^{**} p < 0.05$, $^{***} p < 0.01$.
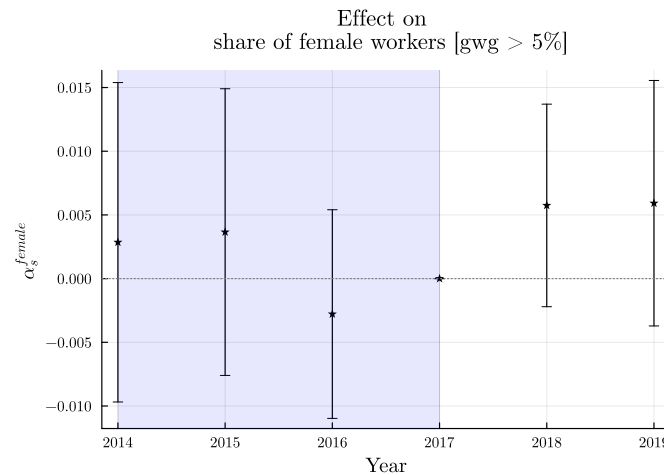
Figure G.1: Impact of the law on the within job share of female workers
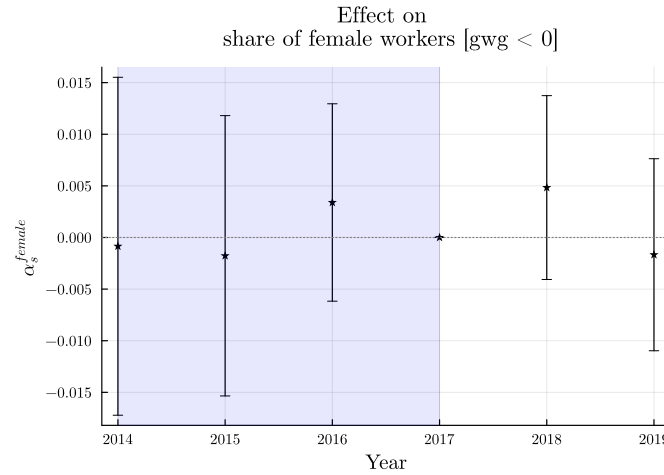
Effect on
share of female workers [gwg < 0]

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.
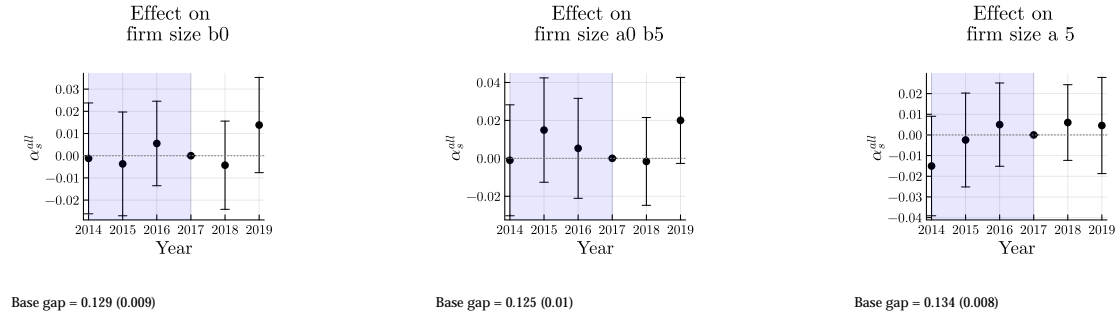
Effect on
share of female workers [gwg ∈ [0, 5]]

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.
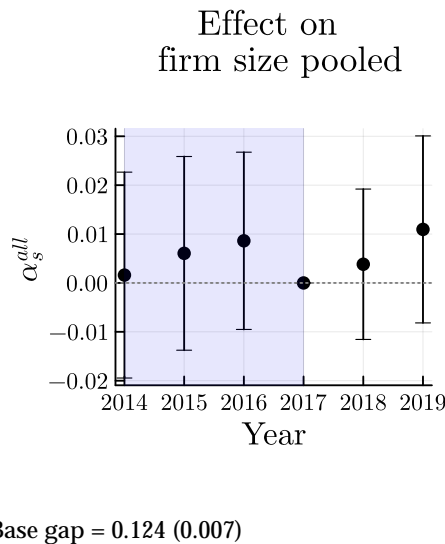
Effect on
share of female workers [gwg > 5%]

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 G.2: ITT estimates of the effect of the law on firm size employing between 200 and 300 workers pre-policy

Base gap = 0.129 (0.009) Base gap = 0.125 (0.01) Base gap = 0.134 (0.008)

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.

Figure G.3: ITT estimates on firm size employing between 200 and 300 workers pre-policy

Base gap = 0.124 (0.007)

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.

65