Labor Market Consequences of Pay-equity Laws*

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Abstract

In 2018, Portugal enacted legislation to address gender wage gaps in the labor market. This legislation enforced pay equity laws on larger firms penalizing those with gaps exceeding five percent. Using rich administrative data which links employees to job-titles within firms, we analyze the law's impact both within and between genders using an event study design. First, we document that there exists a distribution of gender wage gaps across jobs within firms. We show that in treated firms, jobs with initial gaps exceeding five percent saw a 12% reduction, through slower male wage growth, while jobs with gaps less than zero percent saw reduction in female wage growth to eliminate the gap. However, jobs with initial gaps in between zero and five percent saw an unintended consequence of the law where gender wage gaps increased from 2.1% to 3.5% mainly from decreased female wage growth. The unintended (intended) consequences are more pronounced in industries with above median share of male (female) workers. We find no evidence to suggest that firms changed their size to avoid the law, and we find precise null effects of the impact of the law on the gender composition of workers within jobs, and hours worked by workers. Our findings underscore how the establishment of a well-intentioned but uniform target gender wage gap clarified the repercussions for gender imbalances thereby leading to unintended consequences.

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1 Introduction

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

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

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

Since the potential cost implications of the law would vary across treated firms based on their pre-existing gender wage gap, we estimate the law's effect separately for firm equal work subgroups with above and below the target five percent gender wage gap in the year preceding the policy implementation. Further, our empirical analysis exploits additional unique institu-

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

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

tional features of the Portuguese labor market—common in many other European countries—that influence the wage setting processes. In Portugal, the majority of workers are covered by a collective bargaining agreement (henceforth CBA) and wage setting in Portugal is described as a mixture of centralized and decentralized bargaining (Bhuller, Moene, Mogstad & Vestad 2022). CBAs in Portugal establish industry-wide job-title specific wage floors, but firms have substantial flexibility to supplement these floors with idiosyncratic "wage cushions" (see e.g., Card & Cardoso (2022), Card et al. (2016)). Given the substantial differences in the wage-setting process Card & Cardoso (2022) and the reasonable postulation that CBAs, like unions, would provide an additional layer of oversight that could mitigate gender disparities Bruns (2019), we separately estimate the law's effect for worker subgroups defined by CBA coverage.

To estimate causal parameters, our empirical analysis relies on the absence of systematic evidence that firms circumvent the law. We find no jump in the density of firm size at the policy threshold of 250 workers (henceforth firms with more than 250 works will be called large firms), which would be a sign that firms could precisely manipulate its employment to circumvent the law. In addition, we present evidence that employment changes in large firms in the post-policy period did not differ from the pre-policy periods, suggesting that these changes could be attributed as employment shocks. More importantly, we find no evidence that firms alter the gender composition within job-titles in order to circumvent the law.

Using an event study design, we identify and estimate the average treatment effect of the law on the gender wage gap in treated firms among workers producing equal work. Our empirical investigation uncovers substantial heterogeneity in the impact of the pay equity law on workers' wages on subgroups defined by whether the pre-existing gender wage gap within the firms' equal work subgroups exceeded the targeted 5%.[3]

Specifically, we find that among workers employed in treated firms and equal work subgroups with wage gaps above the targeted threshold, the law effectively reduced gender disparities. In these firms and equal work subgroups, the conditional gender wage gap decreased by 11.85%on average, declining from 12.1% in 2017 to 10.7% in 2019. This reduction stemmed from slower wage growth among male workers. Among 15% of the treated workforce where the gender wage gap was negative (males earned lower than their female co-workers), the gender gap closed completely (from -2.6% to 0.3%) through reduced female wage growth. These reductions in inequities were as intended by the law.However, among workers working in treated firms and equal work subgroups with pre-law average gaps in between zero and five percent experienced an exacerbation of gender disparities. We estimate that in these equal work subgroups within treated firms, the gender wage gap increased by 66.6% on average, rising from 2.1% to 3.5% . This widening of gender disparities was predominantly driven by larger reductions in wage growth among female workers. Probing further, we find that the unintended consequences of the pay equity law are more pronounced in industries with above median share of male workers, whereas we find no substantial impact of the law in industries with above median share of female workers. Furthermore, industries with above median

[3]Note that a regression discontinuity design is not appropriate to estimate the effects of such a pay equity law. Fundamentally, there is no plausible unobserved variable(s) that would be monotonic in firm size to directly affect gender wage gaps far from the threshold independently of the policy, to force the researcher to only focus on a narrow bandwidth around the threshold.

share of female workers experienced larger drops in gender wage gaps in jobs with above five percent wage gaps as compared to industries with above median share of male workers.

Our findings underscore the importance of considering subgroup differences when estimating policy effects, as the varying average treatment effects across subgroups serve as a warning about the potential unintended consequences of such laws. Thus, while the law aimed to promote pay equality, it simultaneously led to a reduction in gender disparities in firms initially above their target wage gap, while inadvertently exacerbating wage gaps in firms previously below the target wage gap.

While multiple mechanisms could explain these patterns, we argue that the law's non-linear enforcement structure contingent on baseline gender wage gaps within treated firms plays an important role. By both clarifying and enforcing strict penalties for maintaining gender wage gaps, the law resolved previous uncertainties. Thereby risk-averse firms that previously maintained lower gaps due to regulatory uncertainty could now widen these gaps towards the target rate. We further discuss the plausibility of alternative mechanisms, including compensating differentials and moderately discriminatory firms exploiting labor market frictions that could hinder workers from sorting away to more equitable firms in Section 8.

We provide further evidence that validate our empirical results. First, we estimate intent to treat effects of the policy using firm size prior to Law 60/2018 announcement and find similar effect sizes. Second, potential misclassification of treatment status can arise due to employment shocks or churns and firm size is only observable at a single point in time annually in our data. We demonstrate our results are robust to this concern by re-estimating our empirical models excluding each firm that employs between 200 and 300 workers. Similarly, we demonstrate our results are robust to endogenous mobility of workers, by restricting our model to only use data on workers that never switched firms between 2014 and 2019. Next, we show that the law had no impact on the hours worked by both male and female workers and hence the estimated effects on hourly wages cannot be explained by any impact on hours worked. Finally, we show that the law had no impact on gender composition of workers within jobs and hence the estimated effects cannot be derived through firms reallocating workers across jobs. Thus, we are confident on how firms adjusted their wage setting across genders in response to the pay equity legislation. Finally, if we were to ignore that the law applied to equal work subgroups and estimated the policy effects on firms the mean impacts miss out on the richness of the policy impacts as the majority of our firms are characterized by subgroups with widely different initial pay gaps whose responses are offset when aggregated to the firm level.

This paper contributes to the vast literature that both documents and provides an understanding of gender inequality in the labor market (see e.g., Blau & Kahn (2017), Goldin, Kerr, Olivetti & Barth (2017), Goldin (2014); among others). To the best of our knowledge, Bailey et al. (2024) and Baker & Fortin (2004) present evidence on the causal impact of a pay-equity legislation by respectively studying the impact of nationwide Equal Pay Act of 1964 in the US and a policy implemented in a single Canadian province in the early 1990s. Our empirical

setting differs sharply based on criteria to ensure firm compliance with the law [4] [5] and by being able to exploit policy variation across firms of different sizes over time. Further, our data is rich enough to provide 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.[6]

Our paper also complements the expanding literature on *pay transparency policies* which require employers to disclose information about compensation disparities between demographic groups. Compared to pay equity laws, pay transparency policies function differently in addressing gender pay gaps. They place the responsibility on underpaid workers to reduce wage inequities by using the disclosed information to negotiate improved wages with their employer (Cullen & Pakzad-Hurson 2023). However, these policies may introduce additional hurdles, particularly for women, given the well-documented gender differences in bargaining (Roussille 2021, Card, Cardoso & Kline 2016, Biasi & Sarsons 2022, Hall & Krueger 2012).[7] Consequently, it should not be surprising that there is no evidence indicating that pay transparency policies significantly boosted female wage growth (Baker, Halberstam, Kroft, Mas & Messacar 2023, Bennedsen, Simintzi, Tsoutsoura & Wolfenzon 2022, Perez-Truglia 2020).[8]

In contrast, pay-equity laws shift the responsibility for reducing gender wage disparities directly onto employers. Failure to comply with these policies in Portugal results in legal consequences for the employer, distinguishing them from pay-transparency policies that primarily hold firms accountable for disclosing information rather than addressing within-firm wage inequality. These mixed findings mirror the message from Bailey et al. (2024) who conclude that while 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, it also stifled women's progression in high paying jobs in the long run. Taken together our results have key implications that inform effective pay equity laws.

The rest of the paper is organized as follows. In the next section, we provide a brief discus-

[4] As Bailey et al. (2024) discuss the 1964 US Equal Pay Act, lacked regulatory teeth and did not provide an explicit benchmark for what is an acceptable gender wage differential. Similarly, Baker & Fortin (2004) conclude their null results arise since few firms in the treated province complied with the legislation. Further details on both papers and how our study differs are provided in Appendix D.

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

[6] As discussed in Appendix D, neither Bailey et al. (2024) nor Baker & Fortin (2004) utilized matched employer employee data. Since their datasets lack information on specific employers, broad classifications by industry, occupation, and state are required to determine equal work.

[7] Additionally, research has documented negative impacts of pay transparency policy on the morale and productivity of lower paid employees (Breza, Kaur & Shamdasani 2018, Card, Mas, Moretti & Saez 2012, Cullen & Perez-Truglia 2022).

[8] Examples of pay transparency policies include Canada's 1996 law for public sector jobs, Denmark's 2006 Act 562 requiring firms with over 50 employees to report gender-specific statistics by occupation, the UK's 2018 policy for firms with over 250 employees to publish gender pay gaps, and similar policy introduced in Austria in 2011. In general, pay transparency policies are found to reduce gender wage gaps by depressing male wage growth. An exception is Gulyas, Seitz & Sinha (2023) who find no impact of a pay transparency policy on gender wage gap in Austria.

sion of the Portugal's labor market institutions that pertain to both pay equity law and wage setting. Section 3 describes the data. Following this, in Section 4 we provide evidence that firms do not systematically circumvent the law, enabling us to outline the empirical strategy for estimating the causal impact of the law. We next discuss the identification assumptions and the event study framework in Section 5. Section 6 presents evidence of substantial unintended consequences of the policy and a discussion of the potential underlying mechanisms is provided in Section 7. A final section draws the main conclusions.

2 Institutional details

2.1 Pay-equity law in Portugal

Since 1976, all Portuguese workers had a Constitutional Right to equal pay. However, guidance, enforcement and repercussions for having gender disparities were ambiguous, until the announcement of Law 60/2018. On August 21, 2018 the Portuguese government announced Law 60/2018 which mandated equal pay for equal work in firms with over 250 workers aiming to promote gender pay equality.[9] Pay gaps are independently analyzed by the government using the universe matched employer-employee data which we also use in this paper and is described in details in Section 3. If a pay gap is discovered or reported, government authorities notify the firms 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 enforced if the gender gap exceeded 5%.[10] Additionally, workers, or union representatives may file complaints with regulatory authorities if they suspect pay-inequity. By 2023, 1540 companies with a gender wage gap larger than 5% and were notified by the authorities to justify the pay gaps, else were subject to fines up to 13,000 euros. If pay discrimination is proven, employers are required to address the pay difference within 120 days. Further details on the law are provided in Appendix E.

While the law did not explicitly define "equal work", in practice, it is enforced at the job-title level within the treated firms. A pay gap would be considered justified if it is explained through differences in individual characteristics, such as education and tenure at the firm.[11] Typically, since most workers are covered by a CBA, any firm justifying wage differences using such characteristics would be prepared to explain differences in wage cushions offered to different workers on top of the identical wage floor determined by the CBA under which these workers work in the same job-title. As such the law effectively aimed to promote gender wage equality while allowing for characteristics that could impact productivity and hence total wages received by workers, excluding overtime remuneration.

[9] The draft of this pay-equity law had been in discussion since 2017, though the announcement of the final version of the law in 2018 specified legal enforcement to begin from February 2019. The announcement also stated that starting in February 2022, the law would apply to companies with more than 50 employees.

[10] This level of '*accepted*' gender wage gap of five percent was 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.

[11] 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.

2.2 Wage setting in Portugal

Portugal's wage-setting system follows a two-tier structure common in Continental Europe. Industry-wide collective bargaining establishes job-title specific wage floors, while worker-firm negotiations provide flexibility, balancing centralized and decentralized wage determination. As Bhuller, Moene, Mogstad & Vestad (2022) point out, unions and collective bargaining are not synonymous concepts outside North America.[12]

In Portugal, vertical centralization of wage-setting primarily occurs through industry-wide collective bargaining agreements. Approximately 85% of private sector workers are governed by collective bargaining agreements at the industry level.[13] Additionally, workers with the same job-title within a firm but across different plants maybe subject to different CBAs due to regional differences.[14] The remaining roughly 15% of workers whose wage floors are not fixed by their job-title specific CBAs must bargain for their wages individually. Consequently, the data does not assign a job-tilte to these workers since (harmonized) job-titles that we use in our analysis are defined by CBAs.

Portuguese employers have considerable flexibility to pay idiosyncratic wage premiums to individual employees, on and above the collectively bargained wage floors. These "wage cushions" are common, vary by firm and worker characteristics, and change with changes in wage floors (Card & Cardoso 2022).[15] Beyond the wage floor and the wage cushion, workers typically receive regular earnings supplement which are payments such as meal allowances.[16] Additionally, for employees that have signed a sectoral or a firm-specific bargaining contract, typically adhere to agreed-upon normal working hours stipulated within the collective agreement.

3 Data sources

We use the *Quadros de Pessoal* data (henceforth QP, that translates to Personnel Records) which is an annual census of private firms matched to employees in Portugal from 2014 to 2019 for the primary results of our paper. This data is collected by the Ministry of Employment at the end of October from all firms with at least one paid employee. The data contains firm level, establishment level and worker level information. At the firm level, QP includes information

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

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

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

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

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

on region of operation, establishments, number of workers, industry of operation and volume of annual sales. At the worker level, QP includes information on the gender of the worker, various measures of monthly earnings (base, overtime, and regular payments), hours worked and various other demographic information. Crucially, QP includes the job title of each worker which we use to define equal work across genders within a firm. The second source of the data comes from the Integrated Business Accounts System - IBAS (*Sistema de Contas Integradas das Empresas SCIE*). This data provides us information on annual firm level value added data along with other business records which we can link to the QP data.[17]

3.1 Sample selection

In order to facilitate comparison with existing literature using the QP we mostly follow Card et al. (2016) in constructing our sample. We exclude unpaid family labor and restrict our sample to workers aged between 19 and 65. We keep only full-time workers who work in between 120 and 190 monthly hours who comprise 92% of our original sample.[18] Hourly wages are computed by dividing the sum of base salary (wage floor + wage cushion) and regular earning supplements by normal hours of work.[19] 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 with 48% female worker-years.[20] Around 15% of the workforce who are not covered by any CBA are dropped since the data does not contain job-title information on these workers. In our main analysis we also drop firms who have any worker who is not covered by a CBA.

3.2 Summary statistics

Table 1 reports the summary statistics of worker characteristics, and disaggregate it by gender for the year prior to the announcement of the law. Half of the labor force are employed in firms employing more than 250 workers. On average 95% male workers work full-time and that share falls to 89% for females. The raw hourly gender wage gap in 2017 was 0.19 log points which is 10.21% of the average log hourly wage rate with males earning on average 330 euros higher than female monthly and working on average 7.6 hours more per month or 20 minutes more daily on average than females. Approximately 17% of the workers are not covered by a collective bargaining agreement and this too is similar between males and females.

Summary statistics disaggregated by large and small firms are reported in Table **??**. Female workers comprise 46.8% of the labor force in small firms while in large firms the share is half of the workforce. The key differences are that monthly hours worked in large firms are lower than those worked in small firms by 9.5 hours per month, while average gross real wages earned are higher in larger firms by around 113 euros per month. Both of these contribute to

[17] Firm value added is often used as a measure of firm performance (Guiso, Pistaferri & Schivardi 2005, Lamadon, Mogstad & Setzler 2022) and is defined as firm's total revenue minus the cost of goods and services.

[18] Close to 90% of female workers in Portugal who work in the private sector work full time (Card et al. 2016).

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

[20] Note that the estimation sample will differ from these numbers slightly because observations who form singleton fixed effect sets, within our definition of equal work will get dropped during the estimation because they provide no variation.

higher hourly real wage rate. Note that there is a higher share of workers in large firms are not covered by a CBA (20.8%) than in small firms (13.1%).

Summary statistics disaggregated by firms above and under five percent gender wage gaps are reported in Table **??**. In 2017, around 61% of workers worked in firms with gender wage gaps above five percent[21] In these firms females comprised around 54% of the workforce, with just under half of the treated workforce employed by these firms. The hourly wage in these firms exceeded those in firms with under five percent of the wage gap by around 8% on average. In other aspects firms under five percent gender wage gap look on average the same as the firms above the target wage gap, except that females comprised around 37% of the workforce in these firms.

4 Evidence on firm responses

We investigate two possibilities that could present potential threats to our identification of causal parameter beyond the ITT. First large firms could have endogenously reduced their size, and second firms could have changed the gender composition of their workforce within jobs in order to circumvent the law. In this section, we provide evidence that neither of these threats have empirical support.

4.1 Distribution of firm size over time

First, we show that the distribution of firm size over time exhibits no systemic bunching on the left of the 250 threshold in the years after the policy. Figure 1 and 2 respectively plot and overlay the density and histogram of firm size in each year from 2014 to 2019.[22] Across these figures we highlight two key observations. First, we observe that the distributions of firm size are very similar across years. Second, we do not observe any bunching of firms to the left of the threshold of 250 workers in the post-law periods of 2018 and 2019. This evidence suggests that firms did not endogenously respond to the law by reducing their firm size in order to avoid the law. Additionally, intent-to-treat estimates of the impact of the policy on firm size which were around the threshold of 250 workers reported in Table 9 and in Appendix Figure A.24 show neither statistical nor substantial effects to suggest that firms systematically avoided the law by changing their size.[23]

4.2 Employment changes in firms of various sizes over time

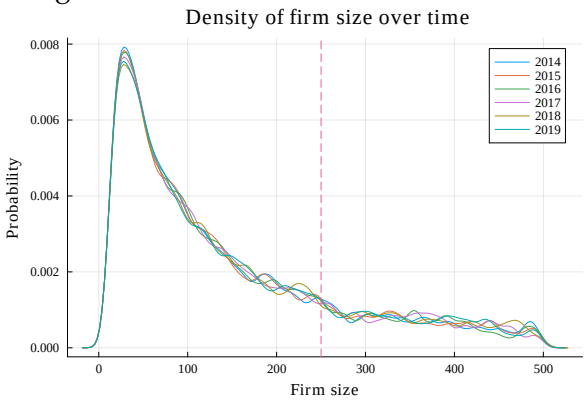
The previous subsection does not imply that firm size remained unchanged. We next examine how employment shocks that can alter firm size vary over time and across firms of different initial sizes. Figure 3 shows how employment shocks vary over time conditional on employing

[21] In 2017, around 924,000 workers worked in firms with gender wage gaps above five percent, while around 592,000 workers worked in firms with gender gaps below five percent.

[22] In Appendix Figure 14 we plot the corresponding empirical CDFs.

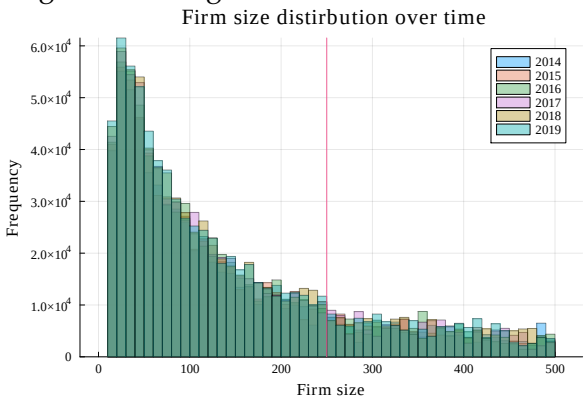
[23] Specifically, for firms who employed in between 200 and 300 workers before the policy, we estimate $log(size_{jt}) = \sum_{s \neq 2017} \alpha_s D_j * \mathbb{I}[t = s] + \theta_j + \theta_{industry,t} + \epsilon_{jt}$ where D_j is the indicator for whether firm j's size in 2017 was above 250, $\theta_j,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

Figure 1: Densities of firm size over time



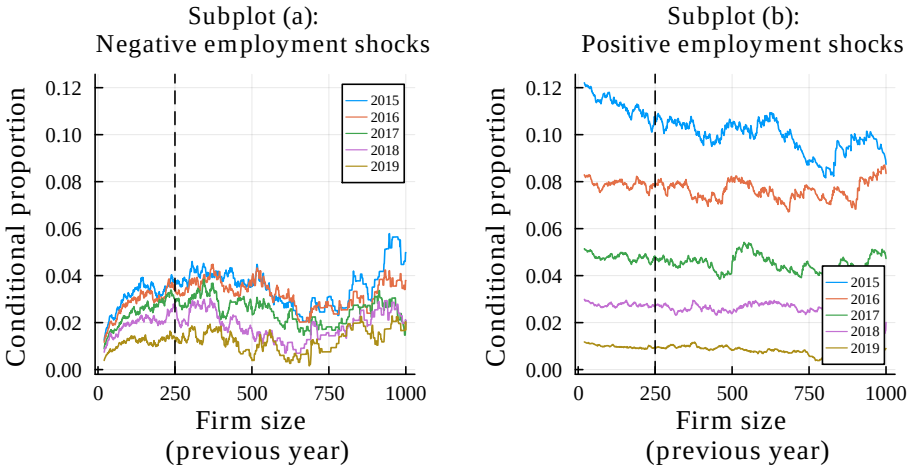
Notes: This figure plots the kernel density of firm size for each year in between 2014 and 2019, and overlay them on top of one another. The vertical red line represents the firm size of 250. The pay equity law was announced in 2018.

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.

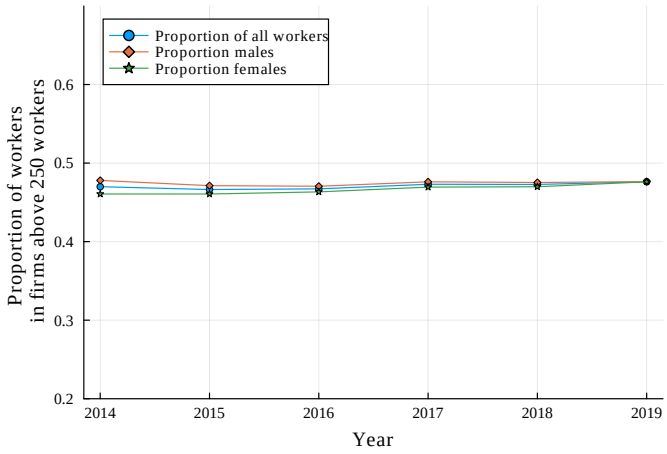
Figure 3: Employment shocks conditional on firm size



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

a given number of workers in the preceding year. In Figure 3-(a), for any given year, a point on that year's line represents on the x-axis a firm size in the previous year and on the y-axis represents the proportion of firms of that size which experienced a negative employment shock and had their size drop below their prior year's size on the x-axis. Figure 3-(b) shows the same except for when the firm experienced a positive employment shock and had their firm size increase relative to the previous year. We accompany this evidence with Figure 4 which shows that the proportion of the workforce working in large and small firms are very similar over time.

Figure 4: Proportion of workers working in firms employing above 250 workers over time

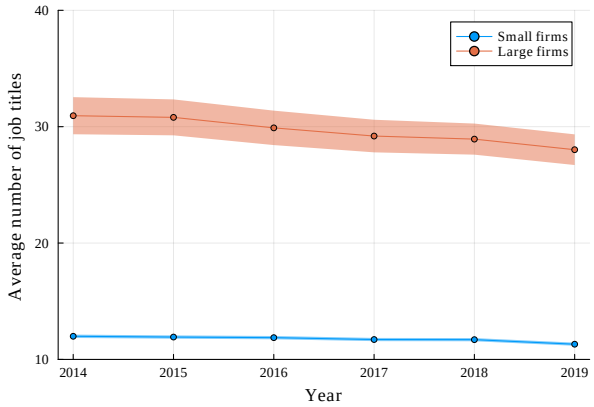


Notes: This figure plots the proportion of workers working in firms employing above 250 workers over time, for all workers and also by gender of workers. The pay equity law was announced in 2018.

The primary observation we want to highlight here is that there is no evidence of systemic change in employment in firms employing more than 250 workers (or of any size up to a thousand workers) relative to how changes in employment occurred in similarly sized firms over time. These figures also show that the proportion of firms employing workers around the policy threshold of 250 workers which experience employment shocks are fairly similar across years. If anything employment shocks have been reducing over time. But these reductions too exhibit no systemically different pattern than how they were reducing before the policy years. In addition, these employment shocks are not substantially any different around the threshold of 250 workers than it is around any other threshold up to 700 workers. If firms were at all responding to the policy then we should see a spike in the proportion and number of firms who received a negative employment shock in the years of 2018 and 2019, and a consequent drop in positive employment shocks. Consequently, those employment changes would exhibit different patterns in the post-policy years compared to the pre-policy years and could no longer be interpreted as mere shocks. However, as shown in 3 we find no discernible systemic pattern in which firms seem to respond to the policy. This suggests that these changes in employment can be interpreted as shocks and as such are exogenous to the policy and firms are not systematically choosing their size to avoid the policy. Hence, we can use firm size as a valid measure to define treatment.

4.3 Job titles over time

Figure 5: 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.

In Figure 5 we plot the average number of job titles in firms employing above 250 workers and firms employing below 250 workers over time. The shaded regions represent the 95% confidence bands. We observe that the average number of job titles in these two types of firms did not change much over time. In particular, we do not see any evidence on the number of job-titles being adjusted by large firms in order to work around the law. This is unsurprising because job-titles and job-title specific wage floors are set by collective bargaining agreements which are primarily industry-wide.

5 Identification and estimation

5.1 Defining "equal work"

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

The key advantage of the QP data is that it has information on the job-title of the worker and the Collective Bargaining Agreement (CBA) that each worker is covered by. This information is typically absent in most matched employer-employee datasets but crucial in defining "equal work". The CBA are typically industry-wide contracts between the firm and the union which specifies the wage floor for each job title. Within CBAs firms have flexibility to add cushions on top of CBA specified wage floor. These wage cushions vary with changes in wage floors negotiated in the industry-wide CBAs. Hence, our preferred definition of "equal work" is workers of the same job title in the same occupation and covered by the same CBA within a firm.[24]

[24] A single CBA can cover multiple firms and CBAs are renegotiated every 2-3 years. However, we do not know which year each CBA is updated, and the QP also does not contain the negotiated CBA specific wage floors. Hence, in the estimations we also allow for unrestricted variation of each CBA with time.

In addition, these firm-occupation-CBA specific job title fixed effects would also control from any unobserved differences between how different firms systematically adds wage cushions on top of industry-wide CBA specified wage floors.

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

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

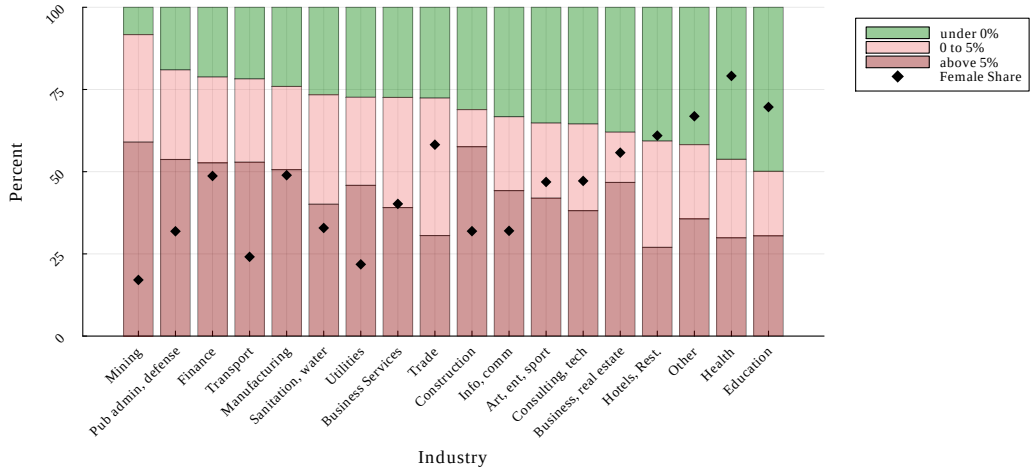
From an econometric perspective, using a time-invariant treatment definition prevents the inclusion of firm-specific time-invariant characteristics. This limitation hinders the analyst's ability to capture systemic differences based on firm size. Consequently, the estimates heavily depend on the assumption that firms are similar in both observed and unobserved dimensions relative to the threshold. Moreover, as discussed in Section 8, this comparison excludes the possibility that firms experiencing churn may be more susceptible to changes in treatment status, potentially introducing bias due to misclassification.

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

In order to have a well-defined gender wage gap within job titles, we require variation in the gender of workers within job titles. In this section, we document that in our data 43% of workers work in jobs where all workers are of the same gender. This highlights the extent of gender segregation within job titles. Consequently, we are only able to construct average pre-policy gender wage gaps for the remaining 57% of workers, and can only identify the causal effect of the law on this subset of workers. However, in latter sections we show that the law

had no impact on the gender composition in jobs. This is consistent with the previous sections where we found no evidence that firms endogenously changed their size in order to avoid the law and the discussion of the presence of CBAs. Next, in Figure 6 we show that within firms job-titles with gender variation in workers differ in their gender wage gaps.[25]

Figure 6: 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.

5.4 Identification

To estimate the causal impacts of the policy, we employ an event-study framework. This approach compares within firm occupation CBA job title cells, the differences in wages between male and female workers in treated firms to differences in wages in their counterparts in untreated firms over time. We make the standard assumptions of sharp design, no anticipation and conditional parallel trends. We formally specify these assumptions in Appendix B.1.

In our context, the conditional parallel trends assume that the average differences in wages between male and female workers producing work of equal value 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 in the triple difference event study framework.

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. 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 under the assumption of SUTVA. This requires the implicit

[25] Additionally, in Figure 6 we show that industries with higher shares of female (male) workers are also associated with higher (lower) shares of negative gender wage gaps.

assumption that the policy does not affect the labor market outcomes of workers in the control group and treatment status of a firm is not affected by the treatment status of other firms.

5.5 Estimation: event-study framework

We use an event-study approach to estimate the ATT of the law on the gender wage gap. We build on the framework of Bailey et al. (2024), extending it to compare wages between male and female workers within job roles. The richness of the data enables us to precisely define and analyze cases of equal work. Intuitively, this strategy compares the difference in wages between male and female workers in treated firms to their counterparts in untreated firms over time.[26] Our main event study specification can be expressed as

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

$\theta_{equalwork(j(i))}$ 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.[27] To flexibly account for differential aspirations of cohorts by gender we control for a gender by year of birth fixed effect $\theta_{g(i),b(i)}$ similar to Goldin (2006, 2002), Bailey et al. (2024). Additionally, since the timing of the renegotiation of each CBA is unobserved in the data, we allow for a flexible CBA by year fixed effect.[28] $\delta_{ind}, \delta_{CBA \times t}$ are industry, and time-specific CBA fixed effects respectively. We include a time-specific CBA fixed effect to account for time-variant unobserved changes in CBA which changed the wage floor across firms over time. Card & Cardoso (2022) document that CBAs are renegotiated on average every one or two years depending on firm, and/or industry profitability. This could account for unobserved differences in profitability between large and small over years, which in turn could affect wage cushions and thus wages. Allowing for further flexibility

[26] Note that the effects on employment are not identified within the design-based framework. This is because the outcome—in this case employment—itself affects probability of treatment. In other words, the potential outcomes of employment are not independent of the treatment status and thus will induce simultaneity bias. Consequently, it is not straightforward to make claims on suggestive evidence on worker productivity by looking at the impact of the policy on employment and the volume of sales or profits. In our data 97.3% of firms always employed either less than, or always employed more than 250 workers. It might be tempting to think that we can estimate the effect of the law on such firms, but we are then conditioning on post-treatment outcome to subset the data which leads to endogenous sample selection. We do not know 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.

[27] Note that we do not include any worker fixed effects in this estimation. This is because the policy gives us variation to compare between workers of different genders, and not within worker. Also including a worker fixed effect will subsume the time-invariant dummy of the gender of the worker. Consequently, we cannot identify the base gender wage gap in small firms in the year prior to policy implementation.

[28] Card & Cardoso (2022) document that CBAs in Portugal are typically renegotiated every two years on average. Additionally, the change in the wage floor upon renegotiation does not completely pass through to total wages as firms adjust wage cushions in response.

by incorporating industry by time and occupation by time fixed effects does not change our results.

$\mathcal{S} \equiv \{2014 : 2019\} \setminus \{2017\}$ is the set of years except the year prior to policy announcement. The primary parameters of interest are $\{\alpha_s\}_s$ that represent the change in gender-wage gap between large and small firms in year s relative to year 2017. In the pre-policy periods of $s <$ 2017—with α_{2017} normalized to zero—ideally α_s should not exhibit any statistical differences from zero, serving as a test for parallel trends in the evolution of gender wage gap between large and small firms before the policy was implemented. The 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 $s < 2017$, serve as a test for parallel trends in the evolution of the within-gender wage gap between large and small firms before the policy was implemented. It is important to note that while it still maybe possible that the between gender parallel trends hold i.e. α_s is statistically indistinguishable from zero for all $s < 2017$, the within gender parallel trends might fail in equal magnitude for both males and females. In such a situation although one can make causal claims on the effect of the law on the gender wage gap, the evidence will remain silent to make causal claims on the mechanisms of how did the wages of males and females evolve because of the policy. The parameter ψ represents the wage gap between female workers working in large and small firms in the base-year. Note that ψ is identified only from firms who either become large from small, or small from being large within the sample period, given that we have equal work fixed effects which are a subset of firm fixed effects. If firms never moved on either side of the 250 worker firm size threshold, ψ would not be identified. The parameter τ represents the gender wage gap between large and small firms in the base-year.

6 Results

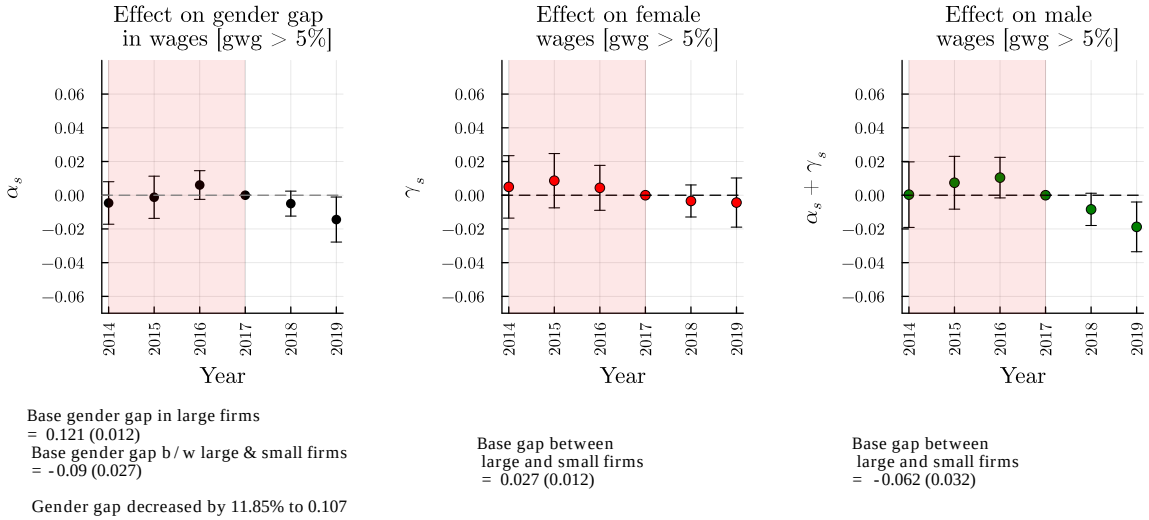
In this section, we present the results jointly estimating the impact of the law on gender wage gaps, female wages, and thereby on male wages and separately for subgroups defined based on whether their firm had a pre-existing wage gap exceeding 5%. Firms with baseline gender wage gaps above five percent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

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

Figure 7 plots the policy impact for workers in treated firms with jobs where the baseline gender wage gaps wereabove 5%, with the full set of coefficient estimates reported in column 3 of Table 2.

In the first subplot of Figure 7 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 the pay equity law reduced the

Figure 7: Jobs where average pre-policy gender wage gap was above five percent



conditional gender wage gap in these firms from 12.1% by 0.8pp (p-value = 0.003) to 10.7% on average. This represents a 11.85%reduction in the gender wage gap within two years of the law. In this figure, we do not observe a statistical difference in the evolution of the gender wage gap between large and small firms prior to the announcement of the law between 2014 and 2016 relative to how they differed in 2017. This increases our confidence in the validity of the conditional parallel trends assumption.

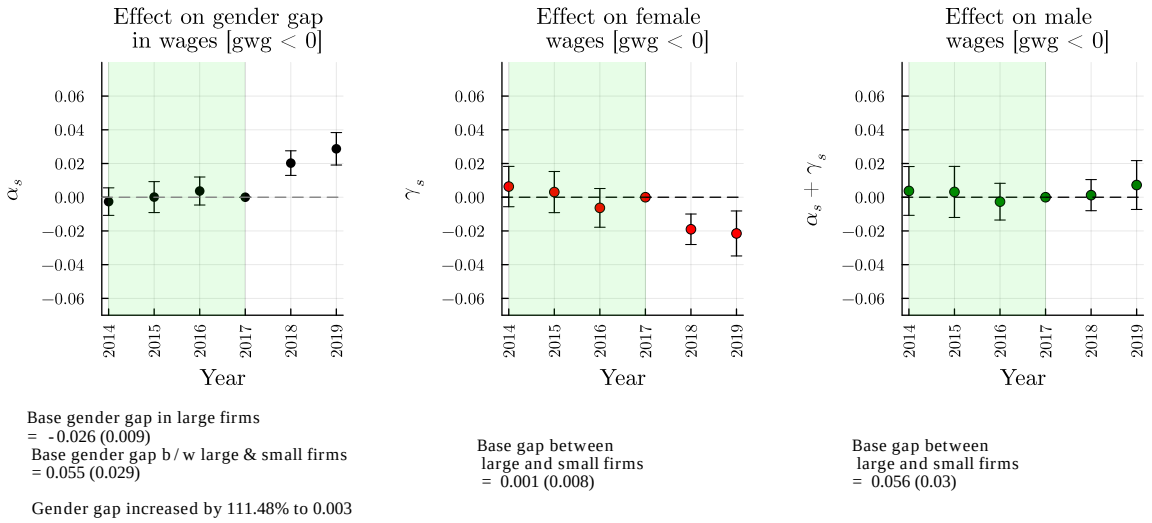
The second and third panels of Figure 7 present estimates of α's and (α +γ)'s, respectively. These estimates capture the effect of Law 60/2018 on the female wage gap between large and small firms relative to the gap in 2017 and the corresponding gap for male wages. We observe that the overall reduction in the gender wage gap in the first panel of Figure 7 was driven primarily by a larger reduction in male wage growth without impacting female wage growth. Specifically, male wage growth fell by around 1.9pp in two years after announcement of the law.

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

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

We find that the pay equity law increased the gender wage gap in these jobs by 2.0-2.9pp on average as shown in the first panel of Figure 8 and column 1 of Table 2. As before we do not observe a statistical difference in the evolution of the gender wage gap between large and small firms prior to the announcement of the law between 2014 and 2016 relative to how they differed in 2017. This increase was driven almost exclusively by a reduction in female wage growth who in these jobs outearned their male co-workers. Females in these jobs on average experienced a drop in their wage growth by 1.9-2.1pp. We find neither statistically nor economically significant effects on male wage growth.

Figure 8: Jobs where average pre-policy gender wage gap was below zero



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

Next, we present the results for workers in treated firms with jobs where the baseline gender wage gap was in between zero and five percent reported in column 2 of Table 2. Given the enforcement rules, these firms would not have to make any changes to comply with Law 60/2018. However, among these firms, beginning in 2018, any uncertainty about the consequences, including financial penalties from having a gender wage disparity, was essentially removed by the legislation.

The first panel of Figure 9 shows that the pay equity law significantly increased the conditional gender wage gap in these firms from 2.1% by 1.1pp in 2018 (p-value = 0.0004) and 1.4pp (p-value = 0.008) in 2019 to 3.5% on average. This is equivalent to a 66.6% increase in the gender wage gap within two years of the law. We also observe that between 2014 and 2016, there is no statistical difference in the evolution of the gender wage gap between large and small firms, relative to how they differed in 2017—providing confidence in our conditional parallel trends assumption.

The increase in the overall gender wage gap in Figure 9 is due to a larger decline in female wage growth relative to male wage growth, as shown when comparing 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). In contrast, the reduction in male wage growth was close to half that of their female counterparts, at 1.3pp -1.4pp .

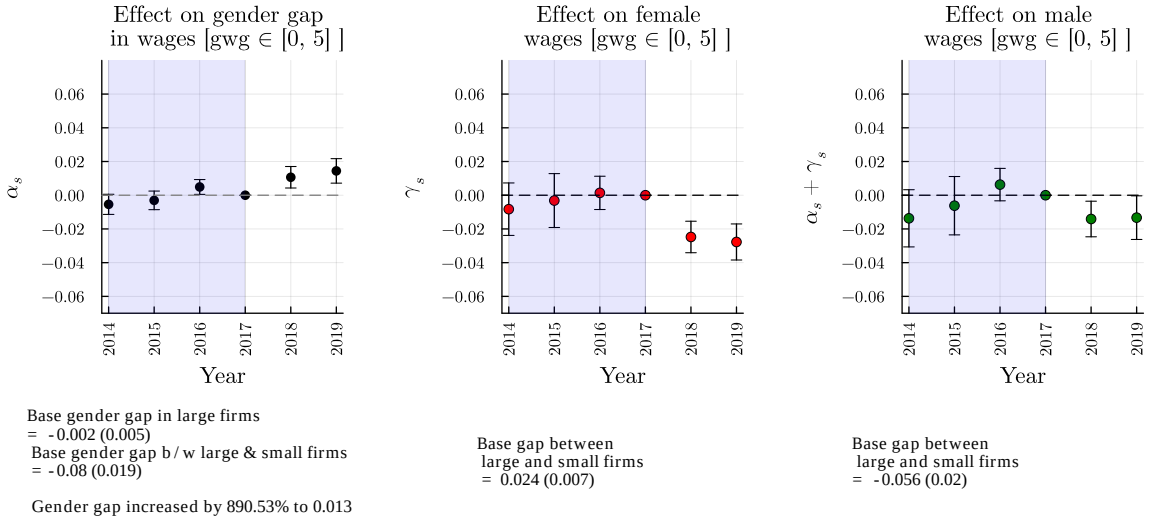
To probe this further in the next subsection we uncover further heterogeneity in how these effects differ by male and female dominated industries.

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

We define an industry to be dominated by a gender, if the average share of workers of that gender within jobs in the industry is above 50%.[29] We discuss the estimates of the impact of

[29]Male dominated industries include Construction, Manufacturing, Transport, Finance, Mining, Sanitation and Water, Art, entertainment and sport, Information and communication, Consulting and technology, Public admin-

Figure 9: Jobs where average pre-policy gender wage gap was between zero and five percent



the law on female and on male dominated industries on workers wages. The main observation from these estimates is that the unintended consequences of the law are more pronounced in industries with above median male share whereas the intended consequences are more pronounced in industries with above median share of female workers.

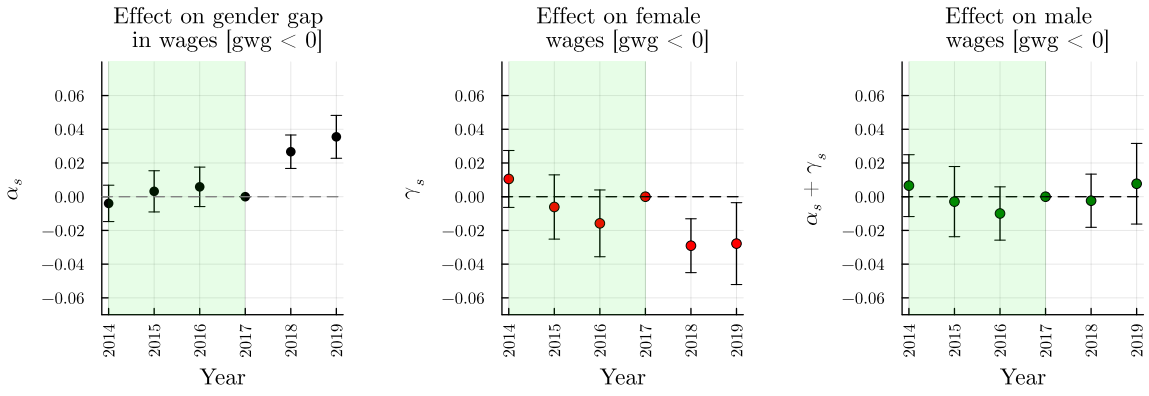
In jobs where the average pre-policy gender wage gaps were under 0% as shown in Figures 10 there are no discernible differences in the impact of the law on gender wage gaps and growth of female and male wages. In contrast, in jobs where the average pre-policy gender wage gaps were above 5% as shown in Figures 11, we find that the law had a more pronounced impact on industries with above median share of female workers, in comparison to industries with above median share of male workers where we do see a trend toward slower male wage growth but they are statistically insignificant at the 95% confidence level. Thus, the intended consequences of the law are more pronounced in industries with above median share of female workers.

On the other hand, in jobs where the average pre-policy gender wage gaps were in between 0 and 5% as shown in Figure 12, we find that the unintended consequence of the law in ramping up gender wage gaps through higher reduction in female wage growth is both more substantially and statistically significant in industries with above median share of male workers. In contrast, industries with above median share of female workers see an almost equal reduction in male and female wage growth which result in no statistical difference in the impact of the law on the gender wage gaps in treated firms in these industries. Thus, the unintended consequences of the law are more pronounced in industries with above median share of male workers.

istration and defense, Business services and Utilities. Female dominated industries include Health, Education, Hotels and restaurants, Business and real estate, Trade, and Other services.

Figure 10: Jobs where average pre-policy gender wage gaps were under 0%

(a) Industries with above median share of male workers



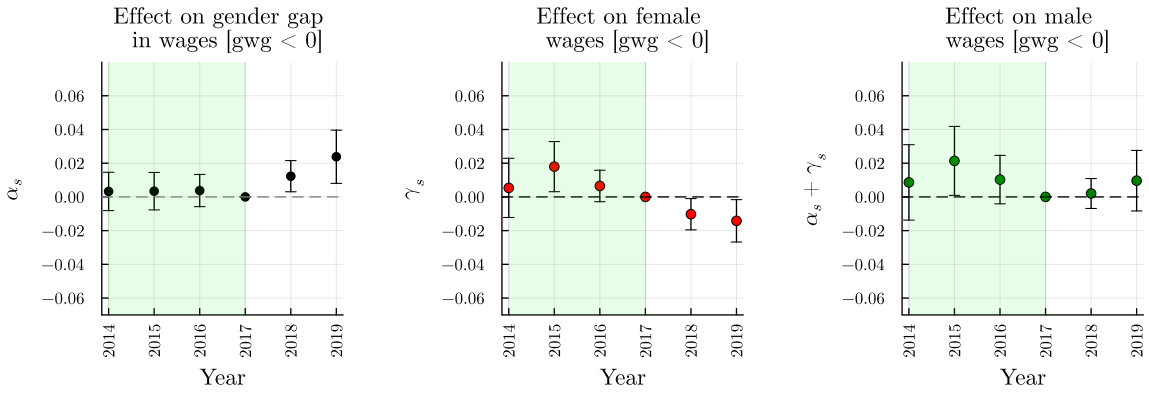
Base gender gap in large firms
= -0.04 (0.021)
Base gender gap b/w large & small firms
= 0.028 (0.043)

Gender gap increased by 89.87% to -0.004

Base gap between large and small firms
= 0.009 (0.012)

Base gap between large and small firms
= 0.037 (0.044)

(b) Industries with above median share of female workers



Base gender gap in large firms
= -0.017 (0.006)
Base gender gap b/w large & small firms
= 0.099 (0.037)

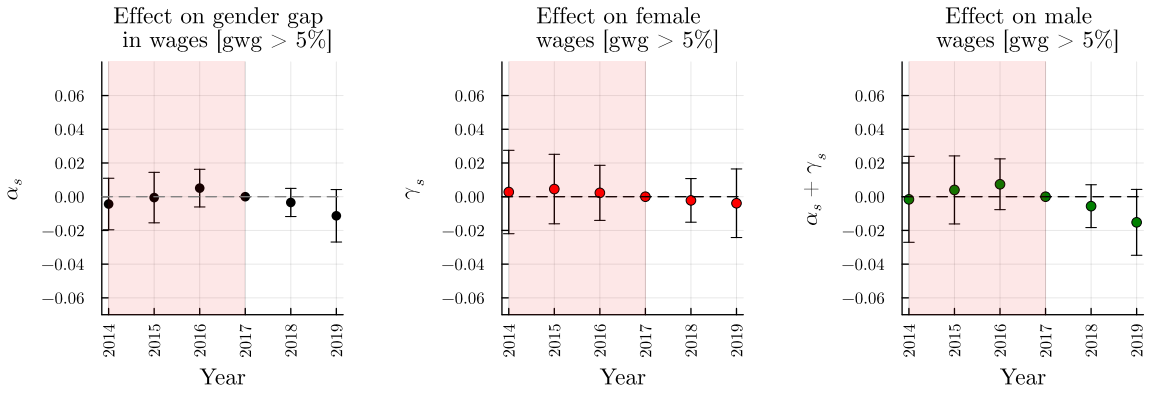
Gender gap increased by 141.19% to 0.007

Base gap between large and small firms
= -0.019 (0.011)

Base gap between large and small firms
= 0.08 (0.038)

Figure 11: Jobs where average pre-policy gender wage gaps were above 5%

(a) Industries with above median share of male workers



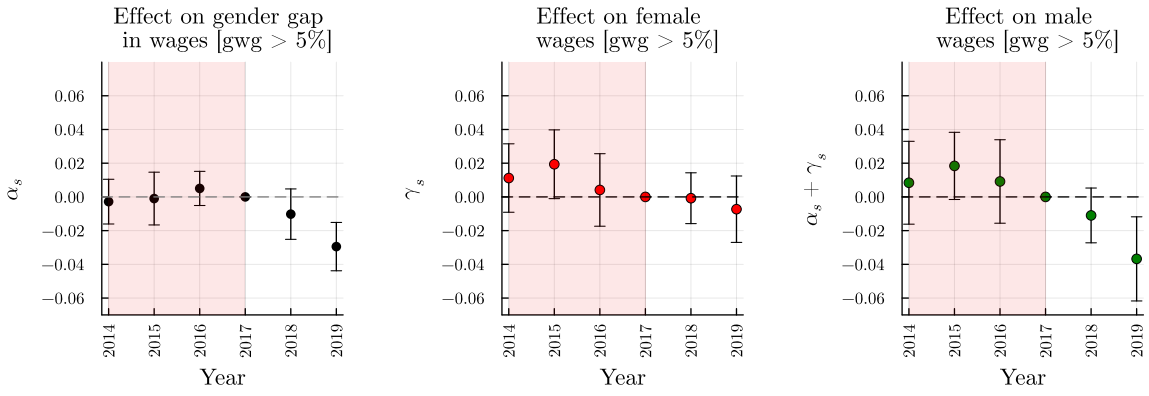
Base gender gap in large firms = 0.142 (0.016)
Base gender gap b/w large & small firms = -0.054 (0.032)

Gender gap decreased by 7.66% to 0.131

Base gap between large and small firms = 0.041 (0.012)

Base gap between large and small firms = -0.012 (0.037)

(b) Industries with above median share of female workers



Base gender gap in large firms = 0.084 (0.018)
Base gender gap b/w large & small firms = -0.194 (0.055)

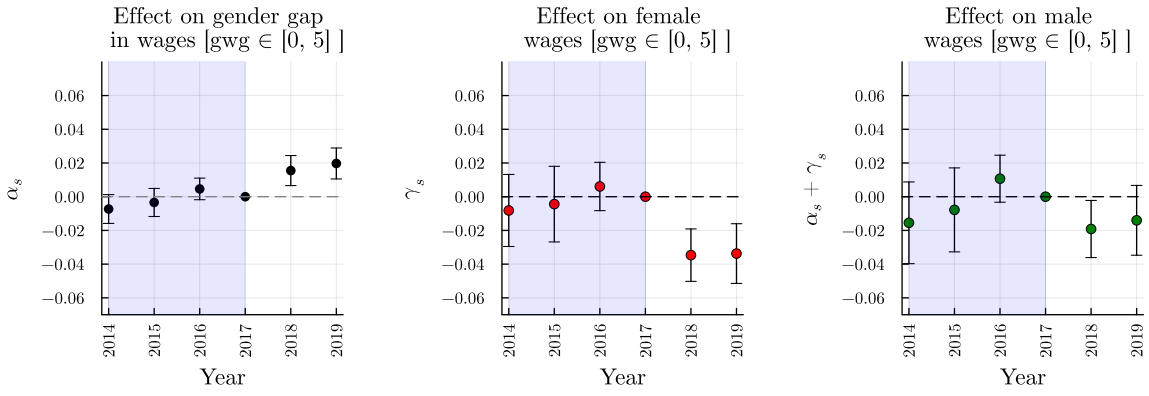
Gender gap decreased by 34.65% to 0.055

Base gap between large and small firms = -0.005 (0.025)

Base gap between large and small firms = -0.199 (0.06)

Figure 12: Jobs where average pre-policy gender wage gaps were in between 0 and 5%

(a) Industries with above median share of male workers



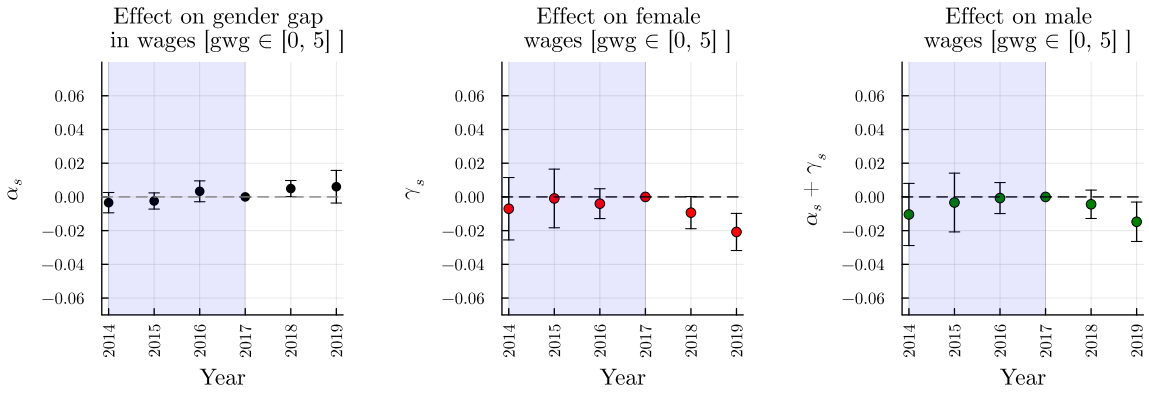
Base gender gap in large firms
= -0.003 (0.009)
Base gender gap b/w large & small firms
= -0.081 (0.022)

Gender gap increased by 566.5% to 0.016

Base gap between large and small firms
= 0.035 (0.009)

Base gap between large and small firms
= -0.046 (0.024)

(b) Industries with above median share of female workers



Base gender gap in large firms
= 0.004 (0.005)
Base gender gap b/w large & small firms
= -0.096 (0.033)

Gender gap increased by 167.84% to 0.01

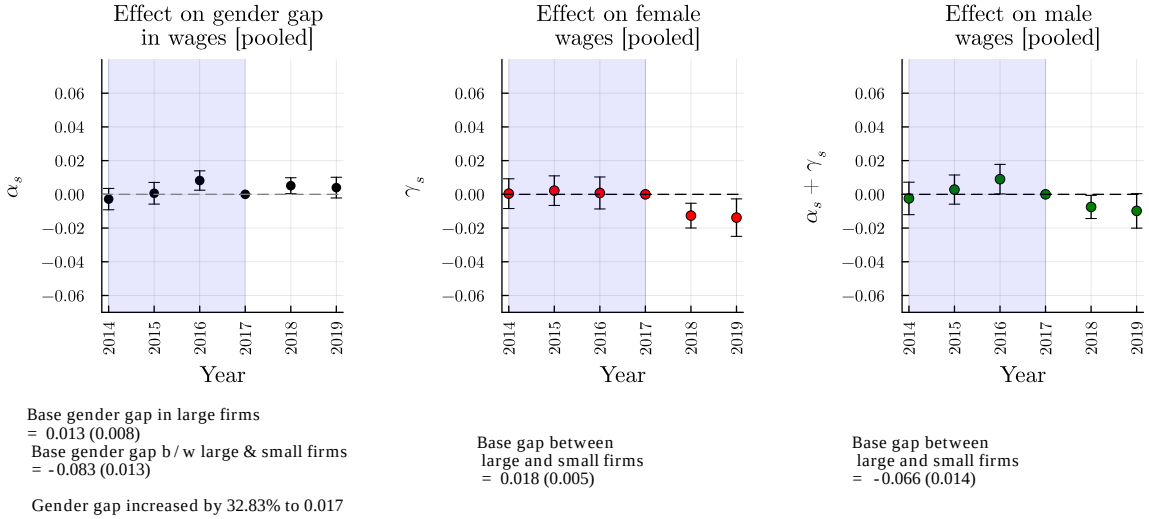
Base gap between large and small firms
= 0.021 (0.013)

Base gap between large and small firms
= -0.075 (0.035)

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

If we estimated equation 1 by pooling the above subsamples together, then not surprisingly a different policy message would have emerged. By pooling the subsamples, we would restrict the wage dynamics for male and female workers in treated firms to be identical once ruling out differential policy responses to the baseline gender wage gaps in jobs within firms. For completeness, Figure 13 plots the time-varying estimates of the policy effect with the full set of coefficients appearing in column 3 of Table 2. Examining figure 13, we observe that in the years prior to and post policy, we find no impact of the pay equity law on the evolution of the gender wage gap between treated and untreated firms when all firms are pooled together with the conditional base gender wage gap being around 4.8% in 2017. This shows that the law impacts gender wage gaps almost oppositely for firms above and below the target wage gap of five percent is masked when we pool all workers together.

Figure 13: Wage growth of all workers pooled together



Furthermore, the second and third panels of Figure 13 show that the wage growth for male and female workers in treated firms experience an almost equal drop after Law 60/2018 is introduced, masking the differential impact the law had on gender-specific wage growth presented in the corresponding panels of Figures 7 and 9. This econometric evidence where the overall heterogenity is almost entirely masked supports our use of subsamples in estimating the underlying policy effects depending on initial gaps.

6.6 Intent-to-treat effects

To identify the intent to treat effects of the pay equity law, we redefine treatment ensuring 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. Specifically, the firm in which worker i is working in 2017 is now redefined to be treated if it had employed more than 250 workers in 2017. 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.[30]

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

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

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

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. This estimating equation differs from equation 1 and we cannot obtain an estimate of the conditional wage gap between large and small firms in 2017, since a large firm effect is not separately identified.

6.7 Additional exercises and robustness checks

In this subsection, we discuss some additional concerns and provide evidence to show that our main results are robust to these concerns.

6.7.1 Impact on hours worked by male and female workers

Evolution in hourly wages could potentially be driven by changes in hours worked by workers. To address this concern, we estimate the impact of the law on hours worked by workers. We report the ATT and ITT estimates respectively in Figures 17 and 18 and in tables 4 and 5. We find no 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 any changes in hourly wages.

6.7.2 Impact on gender composition within jobs

Firms could have also repsonded 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 10 and Figure 22. We find precise null effects on the gender composition within jobs. This removes 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.

[30] Some parameters, by construction will not be identified in this setup. Observe that compared to the estimation of the average treatment on the treated, in this case since we will not be able to identify the effect of a large firm. Hence, for both male and female workers, we will not have an estimate for their respective conditional gap in wages between large and small firms in 2017.

6.7.3 Potential measurement error in treatment

In the data, we observe firm size at a single point in time during the year. However, within any given year, firms with sizes close to 250 workers could move to the left or right of the 250-worker threshold due to regular churn and unanticipated employment shocks, independent of the law. This 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, driven by the extent of the underlying variance in unobserved differential treatment statuses of firms within a year.

To address this potential concern, we remove firms employing between 240 and 260 workers and re-estimate equation 1 on the remaining sample of firms. We argue that it is unlikely for firms far from the 250-worker threshold to be subject to exogenous employment shocks that would change their treatment status. Our results are reported in Table 7 and Figure 20. Notice that our main findings are robust to the exclusion of observations that may be subject to misclassification. Last, we also verified and found robustness to other exclusions including those working in firms with between 220 and 280 workers in Table 8 Figure 21.

6.7.4 Potential concern on endogenous mobility

Our results may be affected if the policy induced gender differential mobility relative to the pre-policy periods. After all, workers who move often do so because of an associated wage increment, an expectation thereof, or due to some non-pecuniary compensating differential. 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 assess the sensitivity of our results by estimating the empirical model after restricting the sample to only those workers who did not change firms during the entire sample period (2014-2019). The results are reported in Figure 19 and Table 6. Notice that our main results, which included all workers in our estimation, are similar in sign, magnitude, and statistical significance, illustrating that endogenous mobility, if any, is not driving our main findings.

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

We discuss three mechanisms that may underlie our main results, each stemming from the non-linear cost structure imposed by fines if a firm's gender wage gap both exceeded 5% and remained unaddressed. The enforcement of Law 60/2018 was strict and immediate,[31] removing any uncertainties about potential penalties for having gender pay disparities. As a result, this non-linear shift in anticipated costs influenced firms' wage-setting policies. Although data

[31] Portuguese workers had Constitutional Rights to pay equity since 1976. However, clear enforcement rules for pay equity were absent until the 2018 pay-equity law.

limitations prevent us from separately identifying the relative importance of each mechanism, we discuss each one in turn due to their distinct welfare implications.

7.1 Risk aversion

Prior to the implementation of Law 60/2018, firms with higher levels of risk aversion 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 potential areas of contention such as wage disparities. However, the policy by establishing a salient threshold for the gender wage gap limit, any the uncertainties that had previously constrained the actions of risk-averse firms were resolved. Whereas these firms might have been cautious about approaching or exceeding an undefined wage gap threshold, they can now more easily adjust their wage policies to avoid penalties, as long as their gender wage gaps remain below the explicitly specified 5% threshold.

7.2 Compensating differentials

Given the extensive literature on gender differences in the valuation of non-wage amenities (see e.g., Wiswall & Zafar (2018)), it is plausible that female workers might still be willing to supply labor at lower wage growth rates if offered better or more non-wage amenities. Therefore, firms could potentially increase their wage gaps by offering additional amenities or benefits to female workers, arguing that overall compensation remains equitable when considering non-wage amenities. As long as the cost of providing these amenities is lower than the wage bill saved by the firms, this approach would allow firms to make direct wage adjustments while staying within the target gender wage gap. However, the contribution of such time-varying unobserved amenities towards wage inequality is difficult to identify without additional exogenous product market variations (Lamadon, Mogstad & Setzler 2022), observable data on non-wage amenities (Dey & Flinn 2005), or exogenous variations thereof (Mas & Pallais (2017), Wiswall & Zafar (2018), Alam et al. (2023)).

7.3 Taste based discrimination

In perfectly competitive labor markets, discriminatory firms exit the market because labor can move freely to their preferred employer in absence of any friction. However, given the growing evidence 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, the equilibrium gender wage gaps in such firms 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 could restrict 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 a reduction in male wage growth—albeit much smaller than that of their female coworkers—strongly suggests that not all firms with baseline gender wage gaps below 5% were discriminatory.

Understanding the dominant underlying mechanism is important to make welfare statements on the consequence of these laws. For example, if the mechanism of compensating differentials were dominant then it would have different welfare implication than if the mechanism of taste based discrimination were dominant. Future research could work towards separating these the underlying mechanisms.

8 Conclusion

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

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

Our findings reveal substantial variation in policy impacts that aggregate firm-level analyses would obscure. In jobs with pre-existing gender wage gaps exceeding 5%, the policy reduced the gap by 12% through reduced male wage growth. In a small fraction of jobs where women initially outearned men, the policy successfully eliminated the conditional gender wage gap. Conversely, jobs with baseline gaps between 0% and 5% experienced increased gender wage disparities, driven by reduced wage growth for women. This unintended consequence was more pronounced in male-dominated industries, whereas female-dominated industries saw the intended effects.

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

A limitation of our study is the absence of worker productivity data which limits us from analyzing wage disparities conditional on worker productivity. Nevertheless, we find it un-

likely that firms with higher initial gender gaps also had corresponding productivity gaps. Thus, our results likely reflect factors beyond productivity differences.

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

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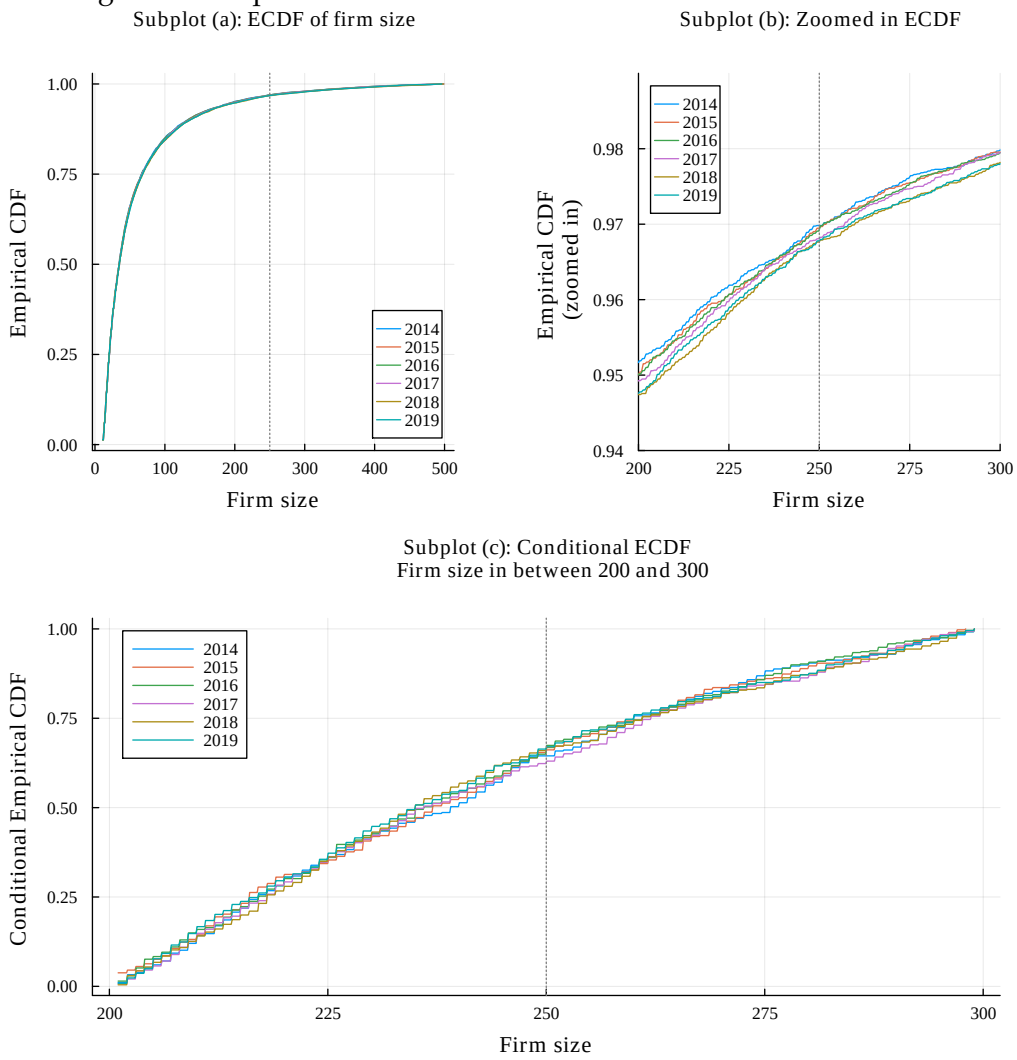
A Appendix-A

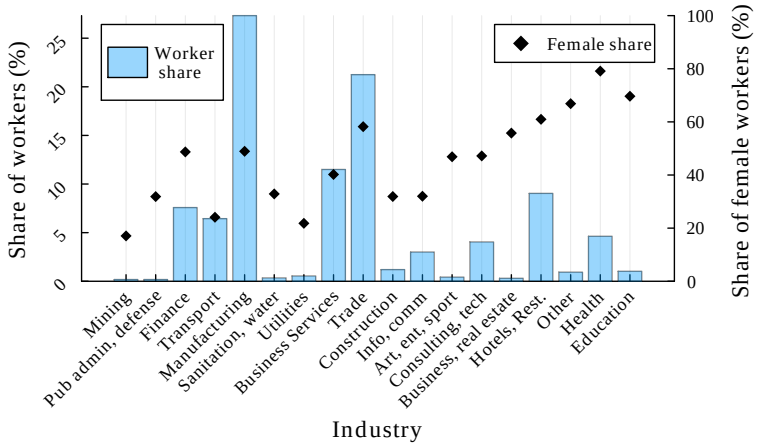
A.1 Tables and Figures

Figure 14: Empirical Cumulative Distribution of firm size over time



Notes: In Figure 14-(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 14-(b). In Figure 14-(c) we plot the conditional ECDF by conditioning on firm size being in between 200 and 300 workers.

Figure 15: Share of all workers and female across industries



Notes: This figure plots the share of all workers by industry on the left y-axis represented by the blue bars, and the share of female workers on the right y-axis represented by the black dots. The industries are sorted in the descending order of the share of workers in jobs with positive gender wage gaps as in Figure 6.

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

Panel A: All workers and by gender

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

Panel B: By firm size

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

Panel C: By pre-policy gender wage gap

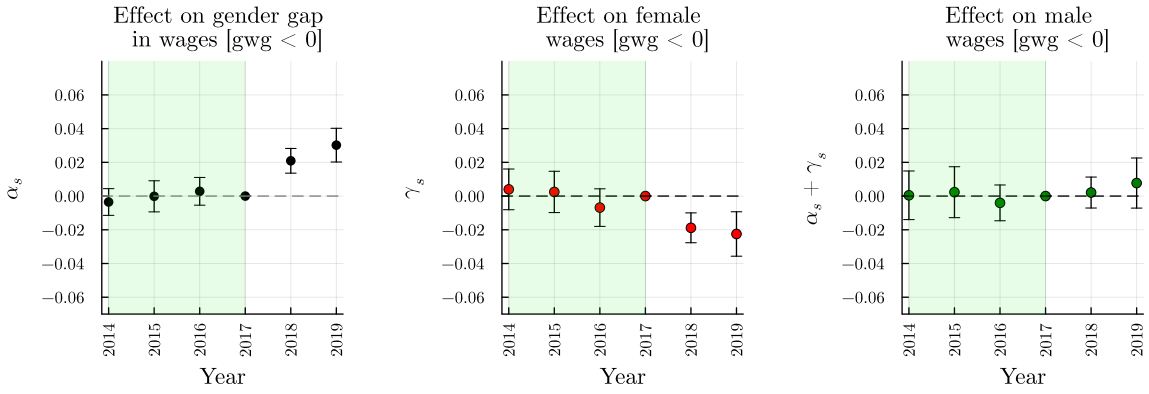
	Above 5% GWG			Above 0% below 5% GWG			Below 0% GWG		
	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N
Female	0.452	0.498	848,777	0.529	0.499	620,754	0.542	0.498	591,613
Firm size > 250	0.527	0.499	848,777	0.621	0.485	620,754	0.443	0.497	591,613
Monthly hours	168.024	8.935	848,777	168.658	9.211	620,754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

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

A.2 Intent-to-treat effects

Figure 16: Event-study ITT estimates on wages

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



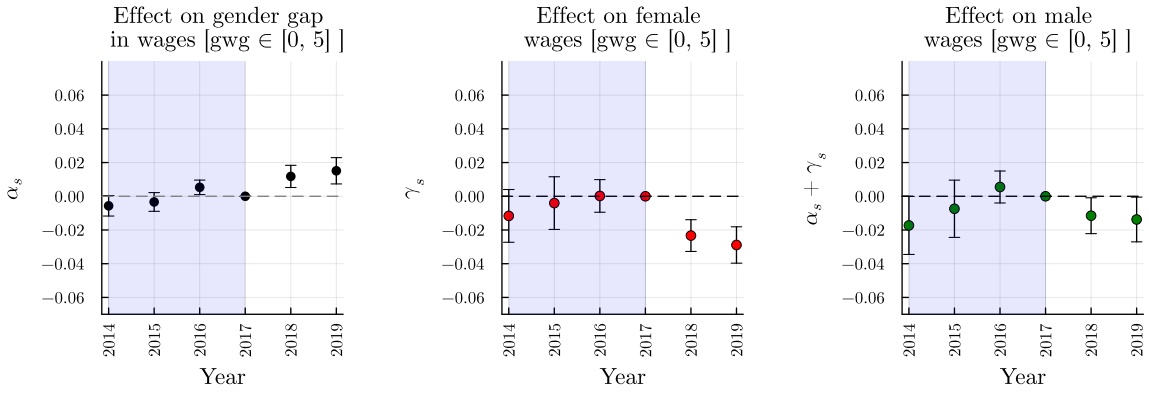
Base gender gap in large firms = -0.025 (0.009)
Base gender gap b/w large & small firms = 0.056 (0.029)

Gender gap increased by 119.67% to 0.005

Base gap between large and small firms = 0.0 (NaN)

Base gap between large and small firms = 0.056 (NaN)

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



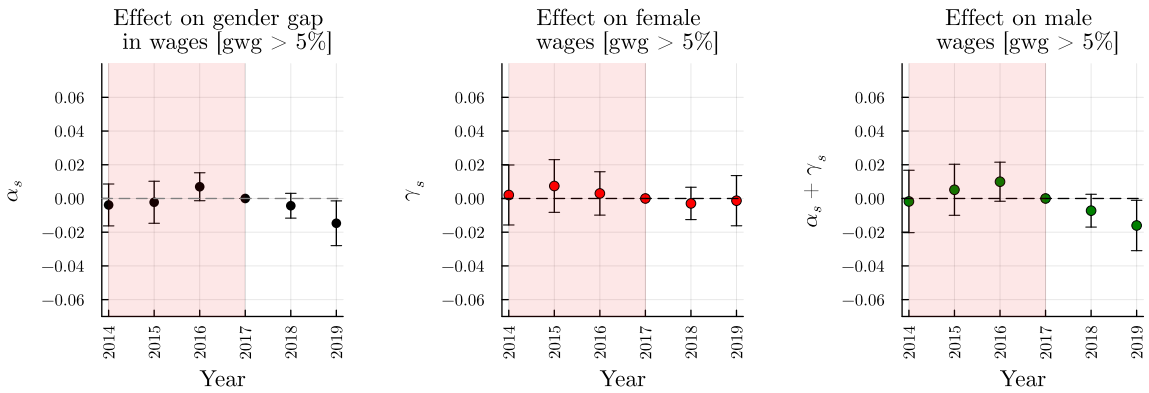
Base gender gap in large firms = -0.002 (0.005)
Base gender gap b/w large & small firms = -0.08 (0.019)

Gender gap increased by 816.94% to 0.013

Base gap between large and small firms = 0.0 (NaN)

Base gap between large and small firms = -0.08 (NaN)

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



Base gender gap in large firms = 0.12 (0.012)
Base gender gap b/w large & small firms = -0.09 (0.027)

Gender gap decreased by 12.6% to 0.105

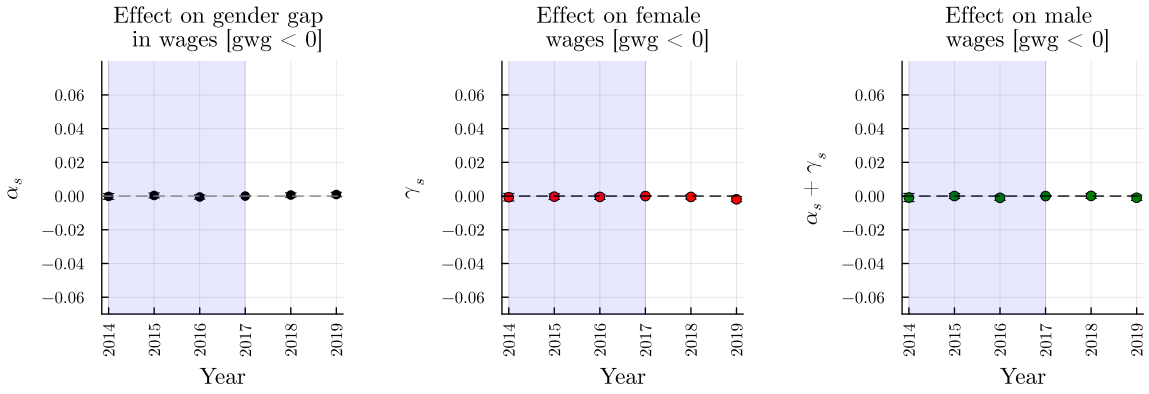
Base gap between large and small firms = 0.0 (NaN)

Base gap between large and small firms = -0.09 (NaN)

Notes: This figure plots the ATT estimates from the event study specification estimated on the sample of workers in jobs with average pre-policy gender wage gaps below zero percent. We plot the ATT estimates α_s, γ_s and $\alpha_s + \gamma_s$ of the effect of the 2018 pay equity law on gender wage gaps, female wages and male wages in the left, middle and the right subplot respectively for each year s, along with their 95% confidence intervals. We report the respective conditional base gap below each subplot. The shaded regions represent the years prior to the announcement of the law.

Figure 17: Event-study ATT estimates on hours worked

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



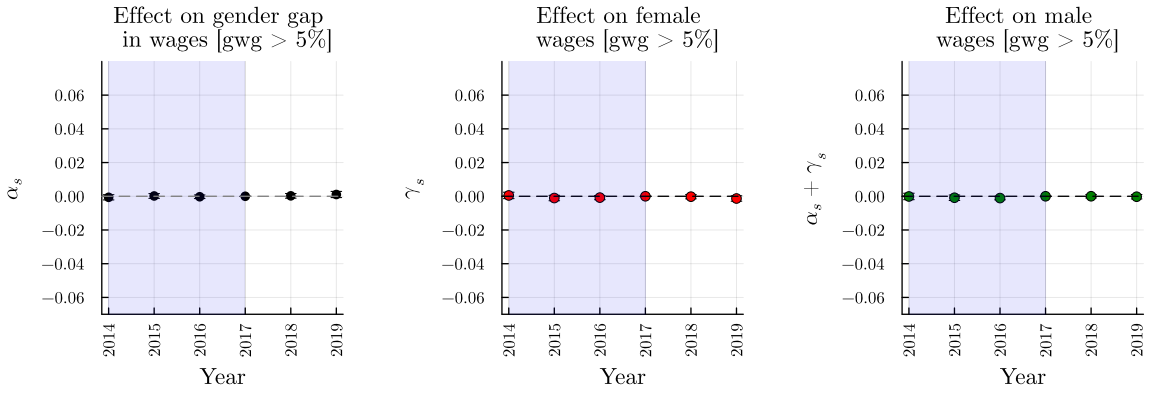
Base gender gap in large firms = -0.004 (0.005)
Base gender gap b/w large & small firms = -0.009 (0.006)

Gender gap increased by 19.48% to -0.003

Base gap between large and small firms = 0.001 (0.001)

Base gap between large and small firms = -0.008 (0.006)

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



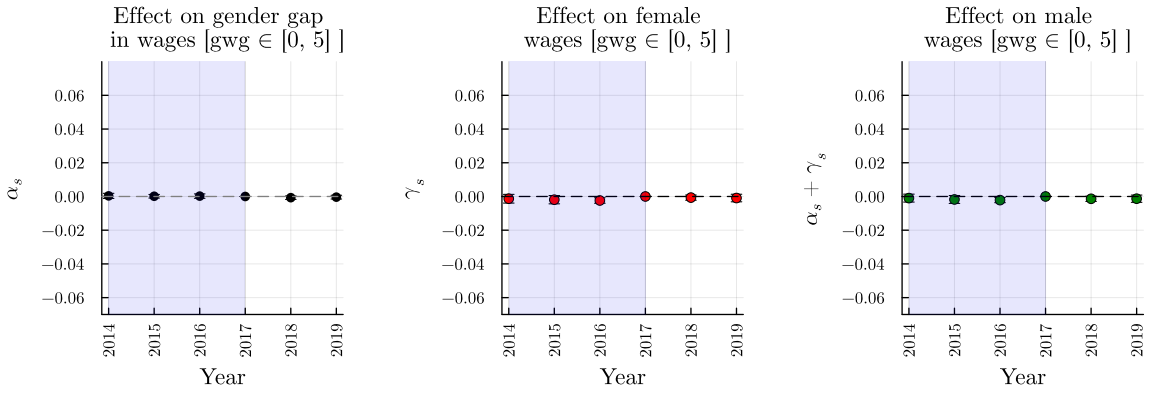
Base gender gap in large firms = -0.008 (0.004)
Base gender gap b/w large & small firms = -0.008 (0.005)

Gender gap increased by 11.58% to -0.007

Base gap between large and small firms = 0.001 (0.001)

Base gap between large and small firms = -0.007 (0.005)

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



Base gender gap in large firms = -0.004 (0.002)
Base gender gap b/w large & small firms = 0.008 (0.007)

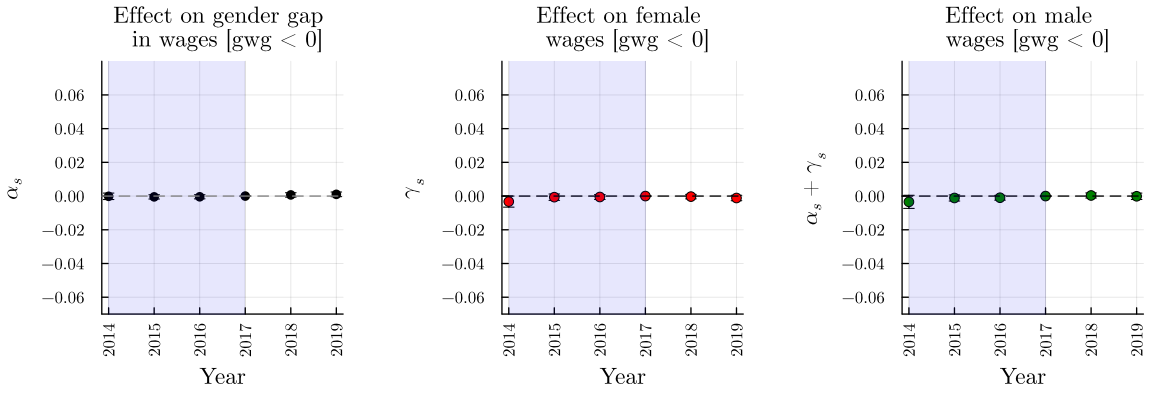
Gender gap decreased by 4.95% to -0.004

Base gap between large and small firms = -0.0 (0.001)

Base gap between large and small firms = 0.008 (0.007)

Figure 18: Event-study ITT estimates on hours worked

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



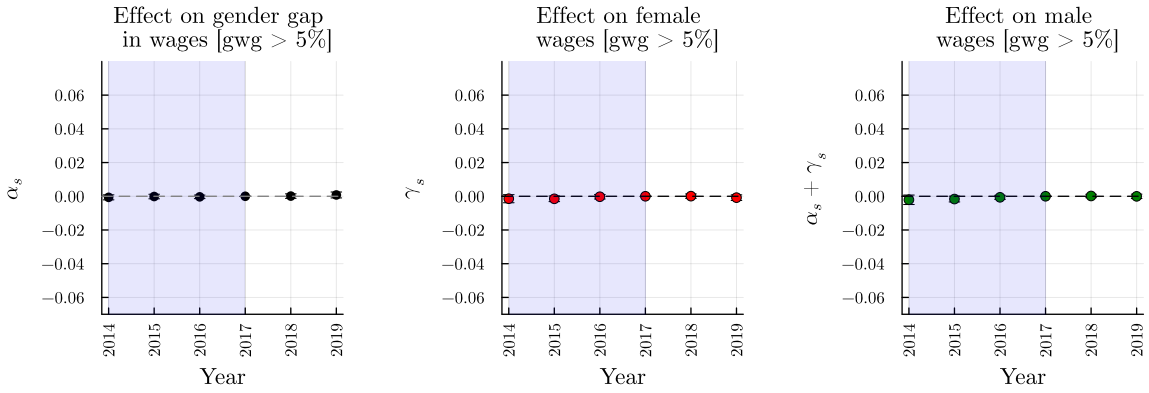
Base gender gap in large firms = -0.004 (0.005)
Base gender gap b/w large & small firms = -0.009 (0.006)

Gender gap increased by 17.64% to -0.003

Base gap between large and small firms = 0.0 (NaN)

Base gap between large and small firms = -0.009 (NaN)

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



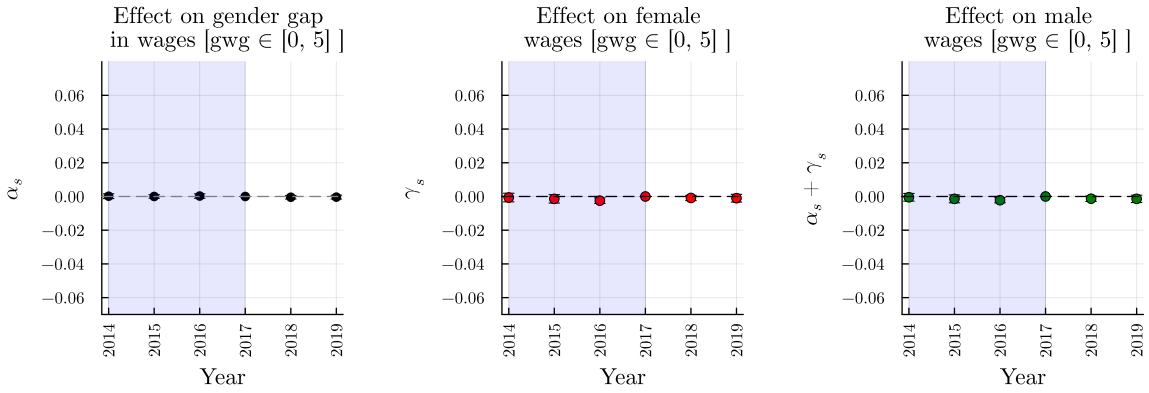
Base gender gap in large firms = -0.008 (0.004)
Base gender gap b/w large & small firms = -0.007 (0.005)

Gender gap increased by 9.25% to -0.007

Base gap between large and small firms = 0.0 (NaN)

Base gap between large and small firms = -0.007 (NaN)

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



Base gender gap in large firms = -0.004 (0.002)
Base gender gap b/w large & small firms = 0.008 (0.007)

Gender gap decreased by 5.76% to -0.004

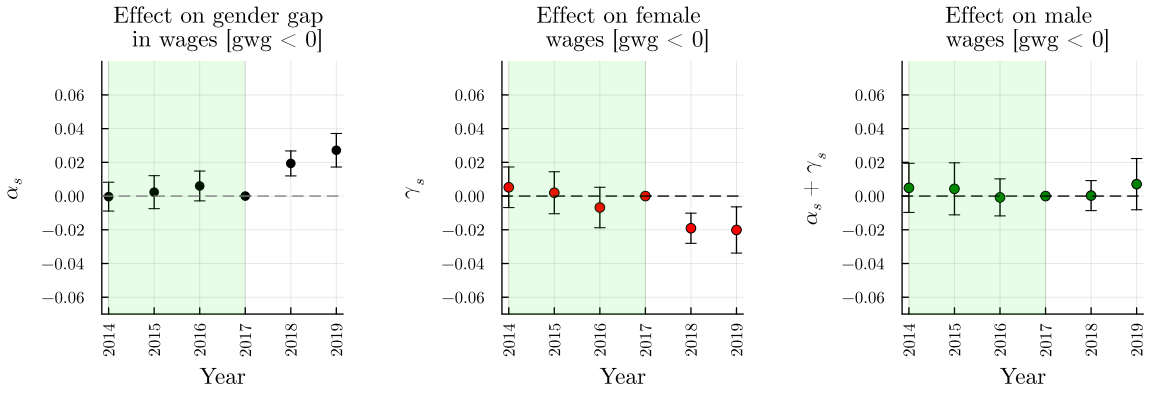
Base gap between large and small firms = 0.0 (NaN)

Base gap between large and small firms = 0.008 (NaN)

A.3 Stayers

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

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



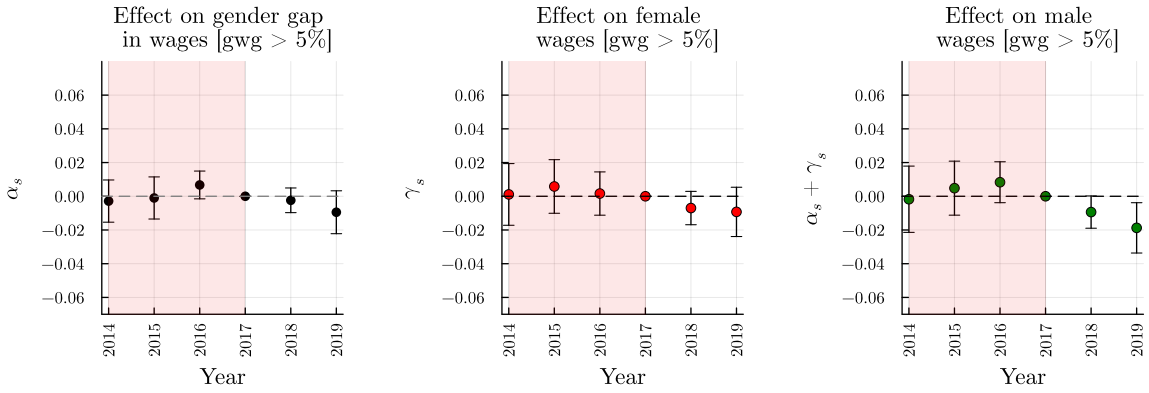
Base gender gap in large firms = -0.021 (0.009)
Base gender gap b/w large & small firms = 0.063 (0.029)

Gender gap increased by 128.61% to 0.006

Base gap between large and small firms = 0.001 (0.008)

Base gap between large and small firms = 0.064 (0.03)

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



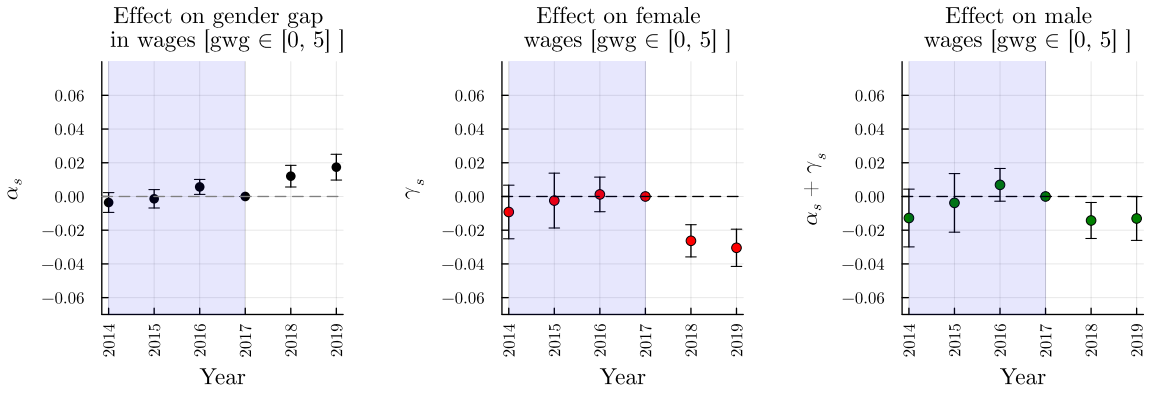
Base gender gap in large firms = 0.132 (0.013)
Base gender gap b/w large & small firms = -0.077 (0.029)

Gender gap decreased by 6.88% to 0.123

Base gap between large and small firms = 0.034 (0.012)

Base gap between large and small firms = -0.043 (0.033)

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



Base gender gap in large firms = 0.001 (0.006)
Base gender gap b/w large & small firms = -0.077 (0.019)

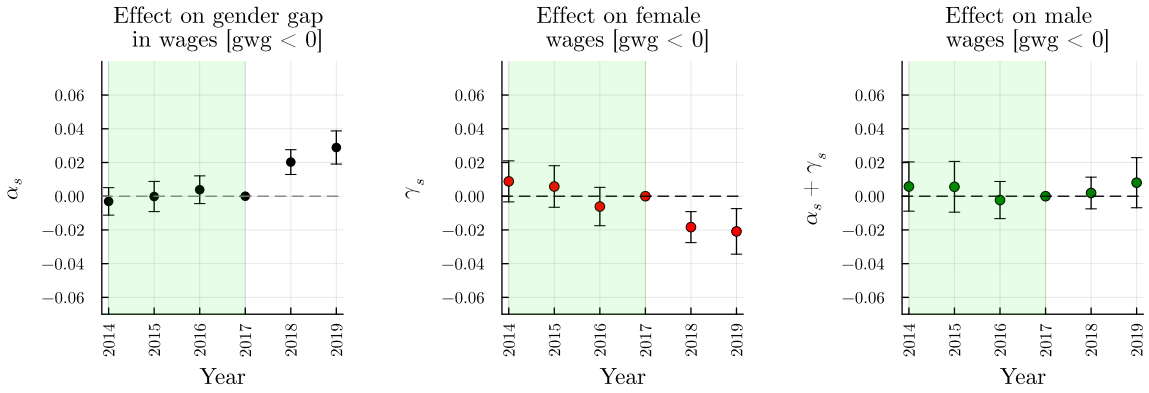
Gender gap increased by 3170.86% to 0.018

Base gap between large and small firms = 0.029 (0.007)

Base gap between large and small firms = -0.047 (0.02)

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

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



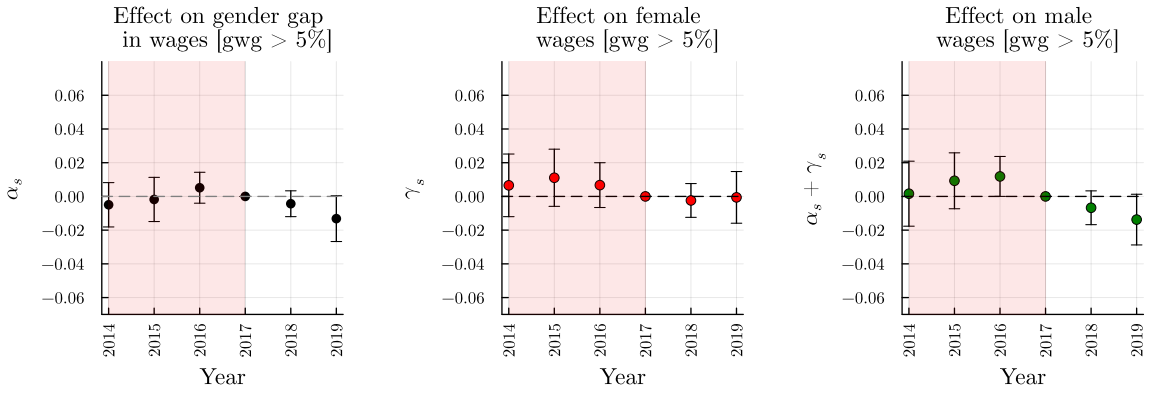
Base gender gap in large firms = -0.028 (0.009)
Base gender gap b/w large & small firms = 0.053 (0.03)

Gender gap increased by 100.0% to 0.0

Base gap between large and small firms = 0.004 (0.014)

Base gap between large and small firms = 0.056 (0.033)

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



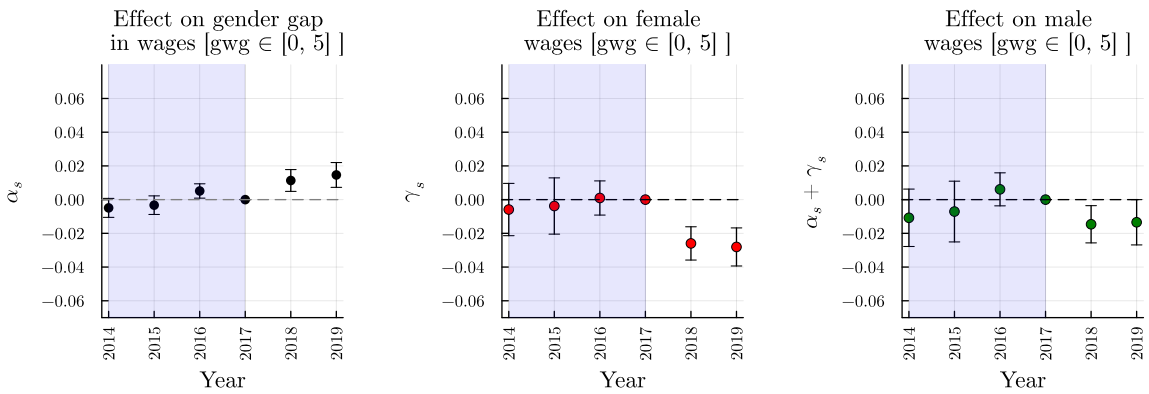
Base gender gap in large firms = 0.124 (0.012)
Base gender gap b/w large & small firms = -0.084 (0.028)

Gender gap decreased by 10.4% to 0.111

Base gap between large and small firms = 0.031 (0.016)

Base gap between large and small firms = -0.053 (0.034)

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



Base gender gap in large firms = -0.003 (0.005)
Base gender gap b/w large & small firms = -0.087 (0.019)

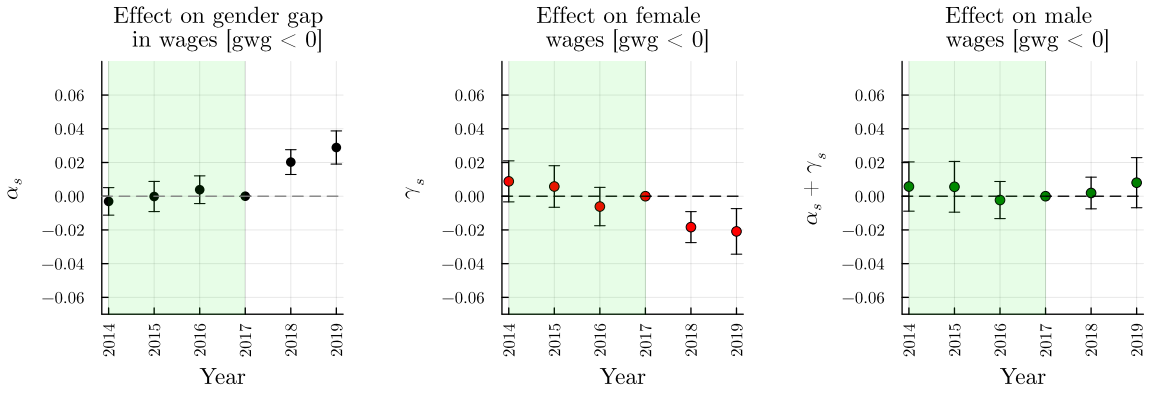
Gender gap increased by 511.87% to 0.012

Base gap between large and small firms = 0.026 (0.009)

Base gap between large and small firms = -0.06 (0.022)

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

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



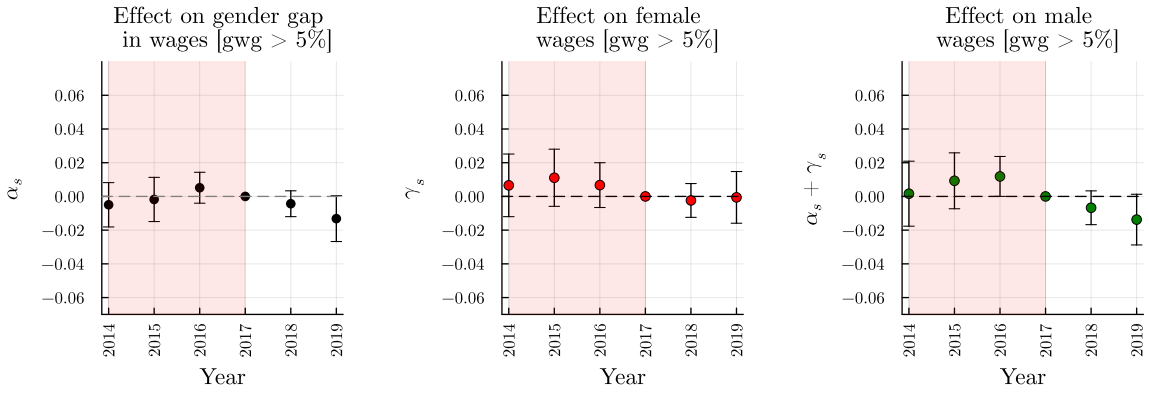
Base gender gap in large firms = -0.028 (0.009)
Base gender gap b/w large & small firms = 0.053 (0.03)

Gender gap increased by 100.0% to 0.0

Base gap between large and small firms = 0.004 (0.014)

Base gap between large and small firms = 0.056 (0.033)

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



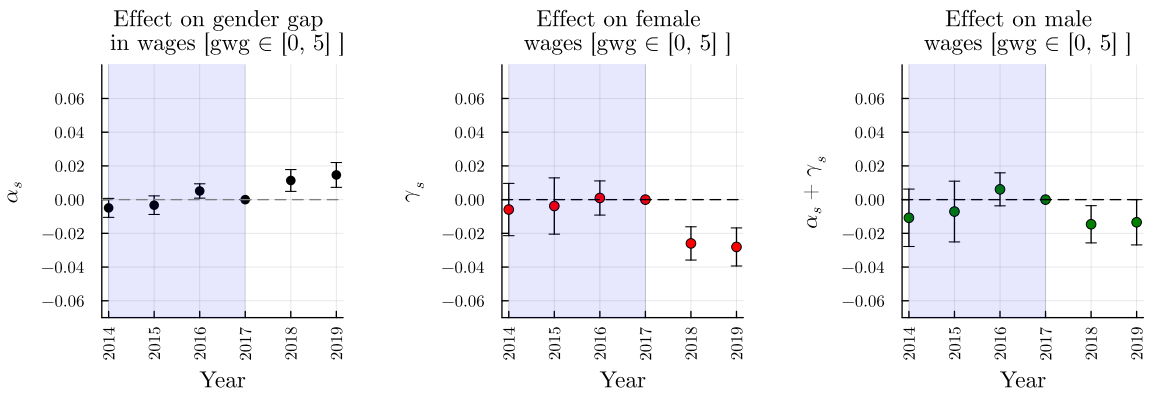
Base gender gap in large firms = 0.124 (0.012)
Base gender gap b/w large & small firms = -0.084 (0.028)

Gender gap decreased by 10.4% to 0.111

Base gap between large and small firms = 0.031 (0.016)

Base gap between large and small firms = -0.053 (0.034)

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



Base gender gap in large firms = -0.003 (0.005)
Base gender gap b/w large & small firms = -0.087 (0.019)

Gender gap increased by 511.87% to 0.012

Base gap between large and small firms = 0.026 (0.009)

Base gap between large and small firms = -0.06 (0.022)

A.4 Estimation tables of robustness and additional exercises

Table 2: ATT Estimates of the effect of the pay-equity law on log hourly wages

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

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

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

	$GWG_{prepolicy} < 0\%$	$GWG_{prepolicy} \in [0\%, 5\%]$	$GWG_{prepolicy} > 5\%$	Pooled
	(1)	(1)	(1)	(1)
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 6: Estimates of the effect of the pay-equity law on log hourly wages of stayers

	$GWG_{prepolicy} < 0\%$	$GWG_{prepolicy} \in [0\%, 5\%]$	$GWG_{prepolicy} > 5\%$	Pooled
	(1)	(1)	(1)	(1)
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 7: Estimates of the effect of the pay-equity law on log hourly wages of workers in firms not employing in between 240 and 260 workers

	$GWG_{prepolicy} < 0\%$	$GWG_{prepolicy} \in [0\%, 5\%]$	$GWG_{prepolicy} > 5\%$	Pooled
	(1)	(1)	(1)	(1)
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 8: 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)	(1)	(1)	(1)
D_{jt}	0.004	0.026***	0.031*	0.024***
	(0.014)	(0.009)	(0.016)	(0.009)
Male	-0.081***	0.084***	0.207***	0.099***
	(0.028)	(0.019)	(0.025)	(0.011)
Female wages in 2019	-0.021***	-0.028***	-0.00	-0.014**
	(0.007)	(0.006)	(0.008)	(0.006)
Female wages in 2018	-0.018***	-0.026***	-0.002	-0.013***
	(0.005)	(0.005)	(0.005)	(0.004)
Female wages in 2016	-0.006	0.00	0.007	0.00
	(0.006)	(0.005)	(0.007)	(0.005)
Female wages in 2015	0.006	-0.004	0.011	0.003
	(0.006)	(0.009)	(0.009)	(0.005)
Female wages in 2014	0.009	-0.006	0.007	0.002
	(0.006)	(0.008)	(0.009)	(0.005)
$D_{jt} \times$ Male	0.025***	-0.008***	-0.060***	-0.011***
	(0.004)	(0.002)	(0.011)	(0.004)
Gender wage gap in 2019	0.029***	0.015***	-0.013*	0.005
	(0.005)	(0.004)	(0.007)	(0.003)
Gender wage gap in 2018	0.020***	0.011***	-0.004	0.006**
	(0.004)	(0.003)	(0.004)	(0.003)
Gender wage gap in 2016	0.004	0.005**	0.005	0.008**
	(0.004)	(0.002)	(0.005)	(0.003)
Gender wage gap in 2015	-0.00	-0.003	-0.002	0.00
	(0.005)	(0.003)	(0.007)	(0.003)
Gender wage gap in 2014	-0.003	-0.005*	-0.005	-0.003
	(0.004)	(0.003)	(0.007)	(0.003)
Male wages in 2019	0.008	-0.013*	-0.014	-0.009
	(0.009)	(0.007)	(0.01)	(0.007)
Male wages in 2018	0.002	-0.015**	-0.007	-0.007
	(0.006)	(0.006)	(0.006)	(0.005)
Male wages in 2016	-0.002	0.006	0.012	0.008
	(0.007)	(0.006)	(0.008)	(0.006)
Male wages in 2015	0.006	-0.007	0.009	0.003
	(0.008)	(0.009)	(0.011)	(0.006)
Male wages in 2014	0.006	-0.011	0.002	-0.001
	(0.007)	(0.008)	(0.012)	(0.006)
Equal work FE	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓
CBA-year FE	✓	✓	✓	✓
Dependent mean (all)	1.788	1.699	1.975	1.811
Dependent mean (untreated 2017)	1.741	1.604	1.861	1.75
N	796,192	1,274,004	1,156,034	4,960,076
R^2	0.908	0.918	0.884	0.903

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

B Appendix-B

B.1 Identifying Assumptions

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

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

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

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

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

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

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

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

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

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

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

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

C Appendix-C

C.1 Overview of pay equity laws in different countries

Pay equity laws have a long history, evolving over multiple decades to address gender wage disparities across various countries. France, with a long-standing commitment to gender equality, incorporated pay equity into its Labour Code in 1946, making it one of the earliest adopters. Germany's constitution of 1949 also laid the foundation for gender pay equity. In the United States, the Equal Pay Act of 1963 was one of the earliest comprehensive laws aimed at abolishing wage discrimination based on gender. Canada implemented its first pay equity law in 1996 for jobs in the public sector, contributing to a broader global effort toward gender pay equity. The United Kingdom followed with the Equal Pay Act of 1970, which later evolved through amendments in 1975, 1983, 1986, and 2010 to strengthen its provisions. The Nordic countries have been particularly progressive, with Sweden introducing its first Equal Pay Act in 1980 and Iceland continuously refining its laws from 1961 through to its most recent amendment in 2017, making it the first country to enforce equal pay through certification requirements for employers. In other parts of Europe, countries like Austria (1979), Belgium (1975, 1999), and Denmark (1976) passed their laws, progressively amending them to align with EU directives. Similarly, in Southern Europe, Portugal enshrined pay equity in its 1976 constitution, while Spain's Workers' Statute provides the legal framework. Eastern European countries such as Poland and Romania integrated pay equity into their constitutions post-World War II, reflecting the broader socialist commitments to gender equality. The Czech Republic and Slovakia, emerging from the Czechoslovak state, adopted specific pay equity laws and constitutional protections after 1992. These examples demonstrate the varied yet convergent global commitment to eliminating gender wage disparities through legislative action.

D Relation of our work to existing literature

This paper contributes to the vast literature that both documents and provides an understanding of gender inequality in the labor market (see e.g., Blau & Kahn (2017), Goldin, Kerr, Olivetti & Barth (2017), Goldin (2014); among others). To the best of our knowledge, only Baker & Fortin (2004) present evidence on the causal impact of a pay-equity legislation implemented all across Ontario, Canada in the early 1990s. While they do not find evidence of a significant impact of pay equity legislation on wages, that is suggested to be due to few firms complying with the legislation.[32] Our empirical setting differs sharply not only by the near perfect degree of firm compliance with the law, but also from having more accurate information on employee wages and exploiting policy variation across firms of different sizes over time.

Our work is closely related to Bailey et al. (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 em-

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

ploy 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 et al. (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.[33] The richness of our data allows us also to mitigate many other potential concerns which we discuss later. Significantly, our analysis is set apart by the Portuguese government's enforcement strategy, which imposed penalties on companies with gender wage disparities over five percent. This contrasts with the 1964 US Equal Pay Act, which lacked such regulatory teeth, offering no such explicit benchmarks for acceptable gender wage differentials. Additionally, the degree to which occupational segregation influences gender wage gaps has diminished since the 1960s, reflecting broader shifts in the determinants of wage disparities in recent years.[34]

E Further details on the 2018 pay equity law in Portugal

Employees or union representatives may also lodge a complaint with relevant authorities. The Committee on Equality in Labor and Employment was empowered to issue a statement on the existence of gender-based discrimination on the basis of a worker's or union representative's request, in accordance with Article 6 of the law. The Commission for Equality in Labor and Employment (*Comissão para a Igualdade no Trabalho e no Emprego*)-CITE is the primary government authority responsible for the implementation and enforcement of the law. CITE is tasked with delivering a conclusive judgment on any allegation of gender discrimination in context of the law, initiated by an employee or trade union representative(s). The written claim must identify the opposite-gender worker(s) concerned, and CITE must then inform the employer within 10 days. The employer will have 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. CITE has a 60-day period to

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

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

present its provisional technical view, and if discrimination is detected, the employer has 120 days to either justify the evidence or outline corrective actions. 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.[35]

F Results on firm level and firm-job level outcomes

Table 9: 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)	(1)	(1)	(1)
new$D_j \times 1[t = 2019]$	0.014	0.020*	0.005	0.011
	(0.011)	(0.012)	(0.012)	(0.010)
new$D_j \times 1[t = 2018]$	-0.004	-0.002	0.006	0.004
	(0.010)	(0.012)	(0.009)	(0.008)
new$D_j \times 1[t = 2016]$	0.006	0.005	0.005	0.009
	(0.010)	(0.013)	(0.010)	(0.009)
new$D_j \times 1[t = 2015]$	-0.004	0.015	-0.002	0.006
	(0.012)	(0.014)	(0.012)	(0.010)
new$D_j \times 1[t = 2014]$	-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

[35] Article 3 of the law specifies responsible government authorities to carry-out statistical comparison of the pay gaps between men and women by company, occupation and qualification levels. But the article does not objectively define this combination of identifiers to be used a measure of equal work, and neither is it specified to be used in the enforcement of the law.

Table 10: Estimates of the effect of the pay-equity law on share of within job female workers

	$GWG_{prepolicy} < 0\%$	$GWG_{prepolicy} \in [0\%, 5\%]$	$GWG_{prepolicy} > 5\%$	Pooled
	(1)	(1)	(1)	(1)
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

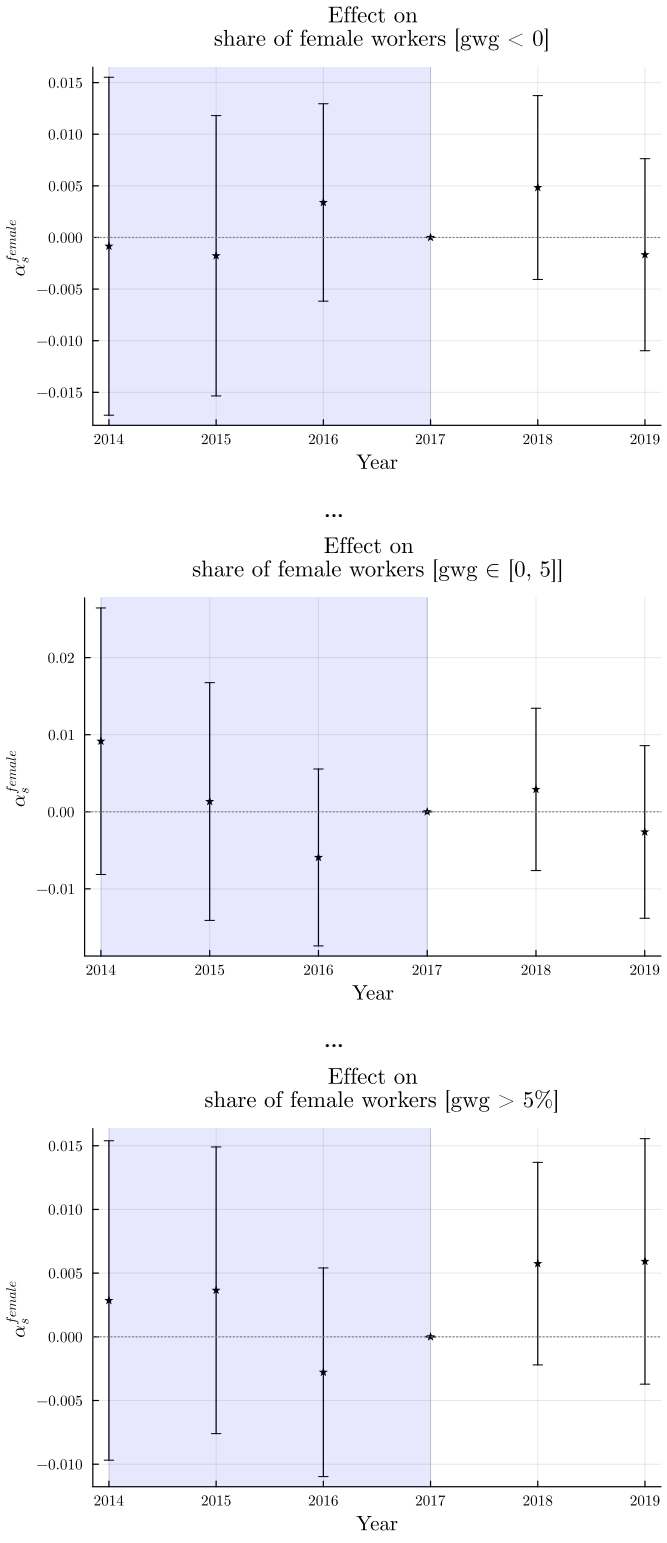
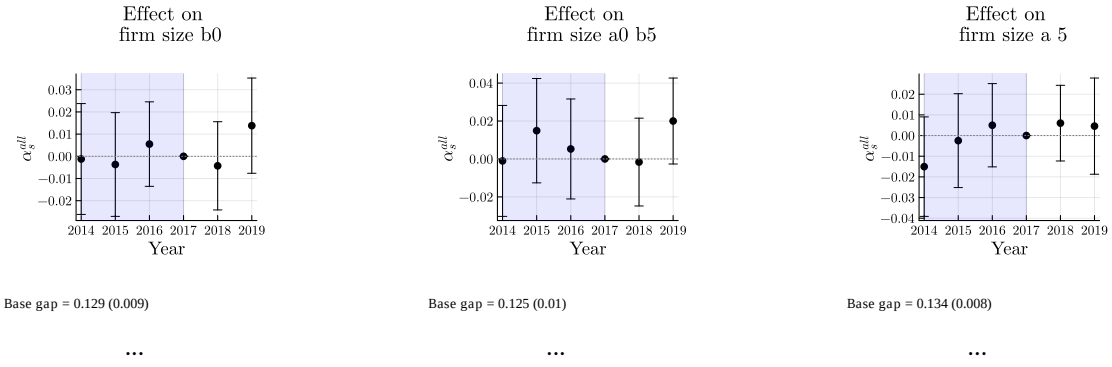


Figure 22: Impact of the law on the within job share of female workers

Figure 23: Impact of the law on firm size



Base gap = 0.129 (0.009)

Base gap = 0.125 (0.01)

Base gap = 0.134 (0.008)

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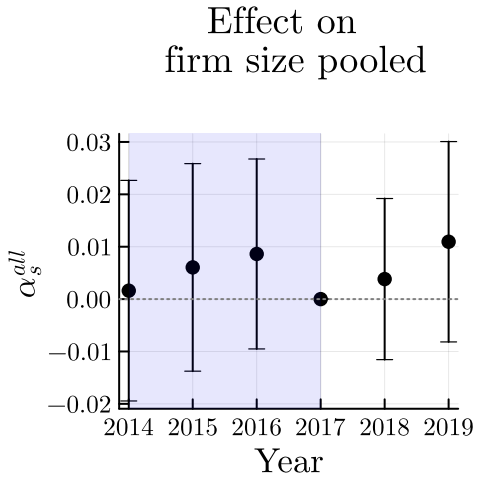
...



Base gap = 0.124 (0.007)

Figure 24: ITT estimates on firm size employing between 200 and 300 workers pre-policy