Labor Market Consequences of Pay-equity Laws*

Md Moshi Ul Alam †
Queen's University
[Job Market Paper]

Steven F. Lehrer
Queen's University
NBER

Nuno Sousa Pereira
University of Porto

November 3, 2023

Regularly updated. Click here for the most recent version.

Abstract

Pay equity laws exist in many countries, but studying their causal impacts have been challenging due to uniform exposure. In 2018, Portugal announced a pay-equity policy targeting firms with over 250 employees, imposing fines on those with gender wage gaps exceeding five percent. Using administrative employer-employee data and an event study design, we study the immediate labor market impact of this pay equity policy and uncover significant unintended consequences. Specifically, for firms with pre-existing wage gaps exceeding five percent, the gap decreased by an average of 13%, mainly due to reduced male wage growth. In contrast, firms with gaps below five percent experienced an increase in the wage gap by more than 25% mainly due to larger reductions in female wage growth. Moreover, among a small proportion of workers who are not covered by collective bargaining agreements, the law reduced wage gaps by one-fifth, driven by increased female wage growth. Notably, the number of women experiencing reductions in wage growth was an order of magnitude larger than those who saw increased wage growth. We delve into the mechanisms driving these outcomes, and explore the policy impacts to firm value added.

*We thank Christopher Taber, Jeffrey Smith and Jesse Gregory for insightful discussions. We also thank comments from Michael Baker, Aloysius Siow, Ismael Mourifié, Nahim Bin Zahur, and seminar participants in Queen's University, University of Wisconsin-Madison and those in the Queen's-Toronto Labor Conference.
Alam and Lehrer acknowledge support from the SSHRC Insight Development Grant.

†Corresponding author. Email: alam.m@queensu.ca

1 Introduction

Our understanding of gender inequalities in labor markets has significantly advanced over the past two decades, shedding light on both the changes that have occurred and the underlying explanations (Goldin, 2017). Nevertheless, numerous gender disparities persist, and research indicates that the bulk of the current earnings gap is now between men and women in the same jobs, and exists even in countries that have introduced pay equity laws that are designed to ensure "equal pay for equal work". Despite the prevalence of pay equity laws,[1] there is scant evidence regarding their impact on wage levels and the wage distribution. This because in most countries, pay equity is enshrined as a Constitutional Right, or is a federally mandated law, leaving researchers with little to no variation to exploit. Our paper aims to bridge this gap in the literature by examining the evolution of workers' wages following the announcement of legislation in 2018 that bolstered pay equity policies in Portugal. Notably, this legislation applied exclusively to firms with over 250 employees providing us with a unique opportunity to study the causal impact of pay equity laws on wages.

Portuguese workers had Constitutional Right to equal pay for equal work since 1976. But there were ambiguity on repercussions on firms on having gender wage gaps, and in general on enforcement. On August 21, 2018, the government announced Law 60/2018 which promoted *"equal pay between men and women for equal work"* in firms employing more than 250 workers. Identical to a recent EU recommendation to its member countries on achieving pay equity, the Portuguese government enforced the law on these firms by imposing fines on those with "unjustified" gender wage gaps above five percent. In extreme cases, firms could be banned from public procurement auctions or even have their licenses suspended. We document evidence of widely differential impacts on firms who were above this target wage gap and on those who were below. As such, our paper presents a cautionary tale on the potential unintended consequences of such laws.

We use administrative matched employer-employee data on the universe of private sector workers in Portugal between 2014 and 2019 to examine the impact of this legislation. First, we

[1] Various forms of pay equity laws and corresponding amendments have existed in most countries since the 1970s and as early as the 1940s. Examples include Austria (1979), Belgium (1999, 1975), Bulgaria (Labour Code), Czech Republic (2006, 2014), Denmark (1976), Finland (1995), France (1946, Labour Code), Germany (1949), Greece (1975, 1984), Hungary (Labour Code), Iceland (1961, 1976, 2008, 2017), Ireland (1998, 1974, 1977), Israel (1998, 1996), Italy (Constitution, 1977, 1991), Latvia (Labour Code), Liechtenstein (Civil Code), Lithuania (Labour Code), Luxembourg (1981, 1974), Malta (Constitution, Equality Act), Netherlands (Constitution, 1994), Norway (1978), Poland (1997, 1952), Portugal (Constitution, 1997), Romania (Constitution), Slovakia (Constitution), Spain (Constitution, Workers' Statute), Sweden (1980), the USA (1963), and the UK (1970, 1983, 1975, 1986, 2010).

find no systemic evidence that firms employing more than 250 workers in the pre-law periods adjust their size and bunch to the left of the law threshold. We also show that any employment changes that happened in large firms in the post-policy period are no different from the pre-policy periods, and could thus be interpreted as employment shocks and not deliberate adjustment of size in response to the policy. Second, we provide evidence that firms do not change the number of job-tiles or the gender composition within jobs in order to circumvent the law. Motivated by this lack of evidence on firm manipulation, we identify and estimate the average treatment effect of the law on the gender wage gap in treated firms among workers producing equal work using an event study design.

The richness of our data permits 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. Our empirical analysis additionally exploits institutional features of the Portuguese labor market that influence wage setting processes. Similar to other European countries, 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). In Portugal, CBAs set industry-wide job-title specific wage floors, but firms have large flexibility in adding idiosyncratic "wage cushions" on top of these floors (Card & Cardoso 2022, Card, Cardoso & Kline 2016). Given the substantial differences in the wage setting process (Card & Cardoso 2022), we separately estimate the effect of the law for subgroups of workers defined by CBA coverage.

We identify the causal effect of the law by exploiting the variation in firm-size over time and estimate it using an event-study design conditional on "equal work" defined above. The average treatment effect of the law on the treated, is identified under the assumption that in absence of the pay equity law, the evolution of gender-specific wages of workers producing equal work in treated firms would have been similar to the evolution of gender-specific wages of workers producing equal work in untreated firms, conditional on factors that determine for wages. Considering the non-linear cost of the law among treated firms, contingent on the baseline gender wage gap, we estimate the law's effect separately for firms with above and below the target five percent gender wage gap in the year preceding the policy implementation.

Among workers working in firms above the target wage gap, we find that the pay equity law reduced gender disparities. In these firms the conditional gender wage gap reduced by

13.35% from 6.3% 2017 to 5.5% in 2019. This effect was largely driven by lower wage growth of male workers. In contrast, workers, working in firms below the target gender wage gap, experienced an increment in gender wage gaps. We estimate that in these firms the gender wage gap increased by at least 30.3% from 3% to 3.9% . This widening of gender disparities was primarily driven by larger reductions in wage growth of female workers. Thus, the law aiming to promote equality in pay, reduced gender disparities in firms above their target wage gap, but it also inadvertently led to widening of wage gaps in firms previously under their target wage gap. We discuss later in the paper, how pooling all workers together would have completely masked the unintended consequences of the law.

Non-CBA workers—though comprising only 15% of the workforce—are important because they are among the most gender inequitable group of workers in Portugal, as in other European countries. Typically, collective bargaining and, or unions have been documented to provide an oversight that could dampen gender disparities (Bruns 2019). Among non-CBA workers working in firms above the target wage gap, the law reduced the conditional gender wage gap by 21% from 9.7% to 7.6% in two years. This large reduction from an already high baseline was almost entirely driven by larger wage growth of female workers. Important institutional details of wage setting explains these patterns which we delineate later in the paper. Among non-CBA workers, in the remaining firms, the conditional base gender wage gap was 4.3%. Being so close to the target wage gap, the law did not have any effect on the wages of these non-CBA workers in firms under the target wage gap.

Overall a simple back of the envelope calculation shows that on average while one in fifty female workers (primarily non-CBA workers) experienced increased wage growths, one in five experienced reductions. We explore various mechanisms which could explain these consequences in such firms. The key insight lies in the law's non-linear enforcement structure on treated firms, clarifying that significant penalties would apply only when gender wage gaps exceeded 5%, whereas before the law came in there was ambiguity to the consequences of gender pay gaps. This resolution of uncertainty, could have encouraged risk-averse firms, which had previously maintained lower gender wage gaps due to the uncertainty of regulatory repercussions, to now widen these gaps up towards the target gap without facing fines. Firms could have also offered non-wage amenities to female workers as compensating differentials, allowing them to lower female wage growth while keeping them at the similar utility levels. Also, the introduction of this law could have influenced moderately discriminatory

firms—those with initial gender wage gaps below 5%—to increase these disparities. The reduced expected cost of discrimination post-law potentially emboldened such firms to widen gender wage gaps, especially since labor market frictions might prevent workers from easily sorting away to more equitable firms. These mechanisms either separately or in some combination could plausibly explain the unintended consequences of this pay-equity law.

Within the reduced form framework we are unable to directly identify the effects of the policy on employment because the potential outcomes of employment are not independent of treatment status given that firm size by itself defines treatment. Also, we cannot restrict the sample to firms who never moved from one side of the firm size threshold to the other as it would induce endogenous sample selection in absence of the knowledge of the counterfactual firm size. However, we provide suggestive evidence by estimating the impact of the law on firm value added using additional administrative data on firms' business records. We find that annual firm value added—a measure of firm performance (Guiso, Pistaferri & Schivardi 2005, Lamadon, Mogstad & Setzler 2022)—fell by 2pp because of the law. Under some assumptions, which we discuss later in the paper, a drop in firm value added suggests a drop in employment.

We conduct a host of robustness exercises to test the sensitivity of our results. Firms near the 250-worker threshold, could fluctuate across the threshold multiple times within a year due to churn and in general employment shocks. Hence, firm size being observable once a year raises concern on bias resulting from potential measurement error in treatment status. To address this concern, we re-estimate our empirical model(s) excluding firms who employ in between 200 and 300 workers, finding that our results to be robust to this restriction. Our results are unaffected by endogenous mobility of workers, if any. Estimating our empirical model for the sample of workers who never switched firms in the entire sample period, we find the results are unaffected. Allowing for more flexible estimation strategies do not impact our overall conclusions. Finally, we find the intent-to-treat effects of the policy are very similar to our main results which report the average treatment effect on the treated.

We contribute to the vast literature of gender inequality in the labor market (Blau & Kahn 2017, Goldin, Kerr, Olivetti & Barth 2017, Goldin 2014), by providing—to the best of our knowledge—one of the first causal evidence on pay-equity laws enabled by the rich variation in the implementation of the law across firms of different sizes and over time. Related to our work, Baker & Fortin (2004) is the only paper which studies a pay-equity legislation implemented all across Ontario, Canada in the early 1990s. In absence of any variation within

Ontario, they use workers of Quebec as a comparison group, and find no significant impacts on the gender wage gap. However, they document considerable lapses in compliance which could have led to selection into treatment based on unobservables. Additionally, measurement error in survey data on wages could have contributed towards statistically insignificant results by increasing the standard errors of the estimates of interest. In comparison, we improve in three distinct ways. First, we have a cleaner counterfactual group enabled by the policy variation. Second, we have accurate information on wages enabled by administrative payroll data. This alleviates concerns of measurement error in the outcomes of interest. Third, near perfect compliance of firms enable us to make causal statements associated with the impact of the law.

A different, yet seemingly-related literature on policies designed to reduce wage inequality focuses on evaluating efficacy of *pay-transparency policies* which are substantively different from pay-equity policies.[2] Pay transparency policies require employers to disclose information about compensation structures by demographic groups. The goal of these policies are to eliminate information asymmetry within the firm, allowing the possibility for employees to better understand and negotiate their wages (Cullen & Pakzad-Hurson 2023). While pay transparency policies have helped in reducing gender wage gap by reducing wage growth of men, a consistent evidence and consequent criticism of such policies is that they have failed to boost female wage growth (Baker, Halberstam, Kroft, Mas & Messacar 2023, Bennedsen, Simintzi, Tsoutsoura & Wolfenzon 2022, Perez-Truglia 2020).[3,4] The reason potentially lies in one of the inherent mechanism through which a pay-transparency policy operates. By construction, pay transparency puts the onus on underpaid workers to use the information on wage structures revealed because of the policy, to bargain for better wages with their employer. This is an additional friction, especially burdensome on females given well-documented gender differences in bargaining (Roussille 2021, Card, Cardoso & Kline 2016, Biasi & Sarsons 2022, Hall & Krueger 2012). In contrast, pay-equity policies aim at reducing wage inequality within the firm by putting the onus directly on the employer—failure to comply has legal consequences

[2]Examples include 1996 pay transparency law of Canada for jobs in the public sector. On June 9 2006, the Danish government announced Act 562 which requires firms employing more than 50 workers to report gender based disaggregated statistics within each of its six digit occupation code, and made available to its employees. In UK starting 2018, firms employing more than 250 workers are required to publish gender gaps in mean and median hourly pay along with their proportion of female hires. Similar policy was also announced in Austria in 2011.

[3]An exception is Gulyas, Seitz & Sinha (2023) who find no impact of a pay transparency policy on gender wage gap in Austria.

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

for the employer—unlike pay-transparency policies which only hold the firm directly liable for information dissipation and not wage inequality. In light of this, our paper presents new evidence, with significant policy implication, of reduction in gender wage gap, driven by boosting wage growth of female workers among the most inequitable group (non-CBA workers), while cautioning the unintended consequences as discussed above.

The rest of the paper is divided as follows. We first describe the institutional details explaining the pay equity law and wage setting in the Portuguese labor market which informs the definition of *'equal work'*. We then describe the data, and present the descriptive statistics. In the next section we present evidence that firms do not systematically circumvent the law, which then allows us to define treatment and the empirical strategy the estimate the impact of the law. Next we delineate the identification assumptions followed by the event study framework for estimation. We then present results followed by their discussion of the mechanisms driving the results. We conclude with a discussion on policy implications.

2 Institutional details

2.1 Pay-equity law in Portugal

All Portuguese workers had Constitutional Rights to equal pay since 1976. However, there was no clear guidance on enforcement and repercussions to having gender disparities were at best ambiguous, until 2018 when Law 60/2018 was announced. On Aug 21, 2018 the Portuguese government announced Law 60/2018 which required equal pay for equal work done in firms employing more than 250 employees aiming to promote gender pay equality.[5] If a pay gap is discovered or reported, government authorities would notify the firms in question.[6] Those

[5]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.

[6]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 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.

firms would then have 120 days to justify or correct the wage gaps, else would be subject to fines, banned from participating in auctions and public tenders for up to two years, or even license revocation. The law though applicable on firms with more than 250 workers was enforced if the gender gap exceeded 5%. 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. This level of *'accepted'* gender wage gap of five percent was in line with Article 9 of the report on joint pay assessment by the European Union which has recently recommended all its member countries (in which pay equity legislations exist) to impose fines on firms which had more than five percent of hourly pay gaps.

Although the law did not specify an objective definition of "equal work",[7] in practice, it is enforced at the job-title level because it is at that level where wages are most comparable. A pay gap would be considered justified if explained through differences in observable characteristics of workers, such as education and tenure at the firm.[8] Typically, since most workers are covered by a CBA, any firm justifying wage differences using such characteristics would effectively need to justify the differences in wage cushions that it offers to different workers on top of the identical wage floor determined by the CBA under which these workers working in the same job-title are covered. As such the law effectively aimed at achieving gender wage equality also based on characteristics that could plausibly matter for productivity and hence the total wages received by these workers, excluding those received as remuneration for overtime work.

2.2 Wage setting in Portugal

Wage setting in Portugal, and in most of continental Europe, mimics a two-tier bargaining structure—primarily industry-wide collective bargaining of job-title specific wage floors, and worker-firm level bargaining of wage cushions—allowing for some degree of both centralization and decentralization in wage setting. Such two-tier bargaining system is common in many developed countries such as Austria, Belgium, Italy, Netherlands, Spain, France, and the Scandinavian countries with cross-country variation in the degree of centralization. As Bhuller,

[7] At best, 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.

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

Moene, Mogstad & Vestad (2022) note, unions and collective bargaining are not synonymous concepts outside North America.[9]

In Portugal, the vertical centralization of wage-setting happens primarily via industry-wide collective bargaining agreements. Around 85% of workers in the private sector are governed by collective bargaining agreements, which are primarily industry-wide. Additionally, workers under the same job-title within a firm but working in different plants could have different CBAs accounting for regional differences.[10] Yet, employers in Portugal have considerable flexibility to pay idiosyncratic wage premiums to individual employees, on and above the collectively bargained wage floors. These "wage cushions" (also termed as *wage drift* in the literature) are common, vary by firm and worker characteristics, and change with changes in wage floors in Portugal (Card & Cardoso 2022).[11] On top of the wage floor and the wage cushion, workers in Portugal typically receive regular earnings supplement which are payments such as meal allowances.[12] Additionally, for employees that have signed a sectoral or a firm-specific bargaining contract, the normal hours of working are a part of the collective agreement.

3 Data sources

We use the *Quadros de Pessoal* data (henceforth QP) 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 at the end of October of each year by the Ministry of Employment in Portugal from all firms which has at least one paid employee. The data contains firm level, establishment level and worker level information. At the firm level the QP contains information on region of operation, establishments, number of workers, industry of operation and volume of

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

[10] 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).

[11] Card & Cardoso (2022) document that wage cushions on top of the industry-wide wage floors are typically larger for males than female workers.

[12] Within a given sectoral agreement, more productive firms (average VA per worker) have some but little flexibility to assign their workers to higher floor categories.

annual sales. At the worker level, the QP contains 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 for our purposes, the QP contains the job title of each worker which is our measure of defining what constitutes equal work across workers of different gender 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 recorsd which we can link to the QP data. Firm value added is usually 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.

3.1 Sample selection

In order to facilitate comparison with existing literature using the QP we mostly follow Card, Cardoso & Kline (2016) in constructing our sample. We remove any unpaid family labor, keep workers of age in between 19 and 65. We construct hourly wages by dividing the sum of the base salary (wage floor + wage cushion) and regular earning supplements by the normal hours of work.[13] We normalize all monetary measures in our data to 2019 euros. We keep firms with at least 5 males and 5 female workers, such that gender wage gap within firms is well-defined. We remove the two largest firms in Portugal. Our results are robust to using the entire sample. This leaves us with 35,809 firm-years and 6,613,573 worker-years with 48% female worker-years.[14] The value added information in the IBAS is only available for firms which are corporations and are not owned by sole proprietors. Corresponding to our sample of workers we are able to match 88% of firms. The remaining 12% of firms are mostly untreated firms closer to the lower end of the firm size distribution where most sole proprietors exist.

3.2 Summary statistics

We begin with a descriptive overview of the data in the year prior to the announcement of the law. In Table 1 we report the summary statistics of worker characteristics, and disaggregate it by gender. The 2017 sample of 1516180 workers is 48% female. Half of the labor force are

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

[14] 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.

employed in firms employing more than 250 workers. While on average 95% male workers work full-time that share at 89% for female workers. The share of female workers working full-time jobs in Portugal is relatively higher compared to other countries, as has been documented by other studies (Card, Cardoso & Kline 2016) using the QP data, enabling the study of gender disparity relatively easier. The average worker in 2017 worked for 156 hours in a month, earned real log hourly real wage of 1.83. 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. The average age in the sample is around 40 years and is similar for males and females. The average experience at a firm is 8.2 years with females at 8 years and males slightly higher at 8.5 years. Around 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 2. Small firms comprise 46.8% of female workers while in large firms the share is close to half. 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 higher hourly real wage rate. Another important difference is that 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 3. In 2017, around 61% of workers worked in firms with gender wage gaps above five percent[15] 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.

[15]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.

4 Evidence on firm responses

Firms could have responded to the introduction of the pay-equity law by endogenously choosing their firm size in order to avoid the law. Also, firms could have changed the gender composition of their workers within jobs in order to circumvent the law. This evidence is essential in order to define treatment, correspondingly lay-out appropriate assumptions which can identify the parameters of interest given the variation the law provides, and consequently describe the empirical strategy. If large firms systematically reduced their size in order to avoid the law, then we cannot identify any parameter of interest of the causal effect of the law beyond the intent-to-treat effects. In this section, we sow that there is no evidence on firms systematically adjusting size to avoid to this law.

4.1 Distribution of firm size over time

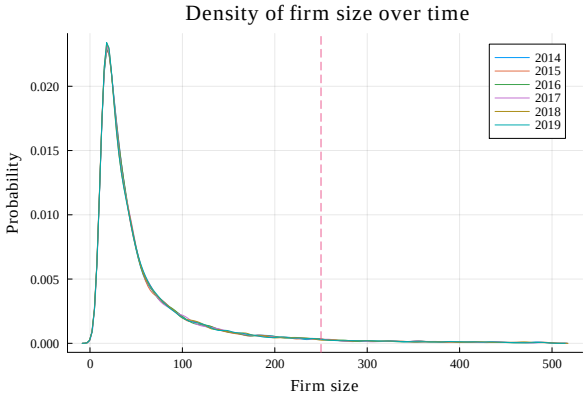
First, we show that the distribution of firm size over time exhibits no systemic bunching on the left of the 250 threshold in the years after the policy. In Figure 1, we plot and overlay the density of firm size in each year from 2014 to 2019. In Figure 2, we plot and overlay the histogram of firm size in each year from 2014 to 2019. In Appendix Figure 13 we plot the corresponding empirical CDFs. 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 is the first in line to suggest that firms did not endogenously respond to the law by reducing their firm size in order to avoid the law. We bolster this evidence in the next subsection by examining how employment shocks vary over time and across firms of different sizes.

4.2 Employment changes in firms of size around 250 over time

Large firms could have tried to avoid the policy by systemically reducing employment such that their size falls below 250. If that was the case, we would observe a jump in the proportion of firms employing less than 250 workers in the post-law periods who had employed more than 250 workers in the pre-law periods.

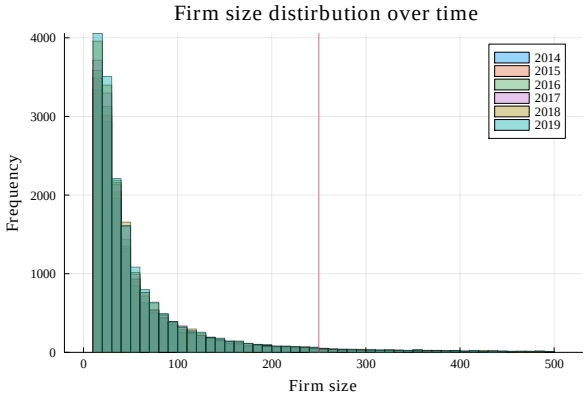
In Figure 3, on the right axis we measure the total number of firms in each year plotted in bars, and on the right axis we measure proportion of firms. The red line plots the evolution of

Figure 1: Densities of firm size over time



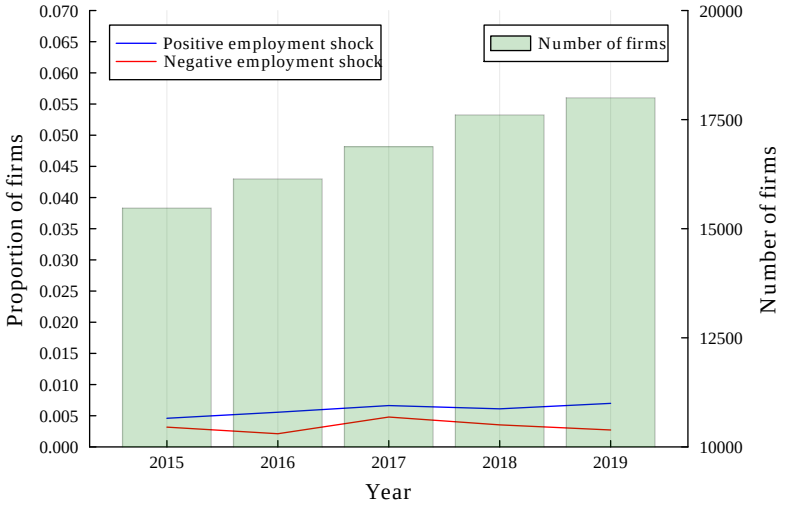
Notes: This figure plots the kernel density of firm size for each year in between 2014 and 2019, and overlay them on top of one another. The vertical red line represents the firm size of 250. The pay equity law was announced in 2018.

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 over time around firm size 250

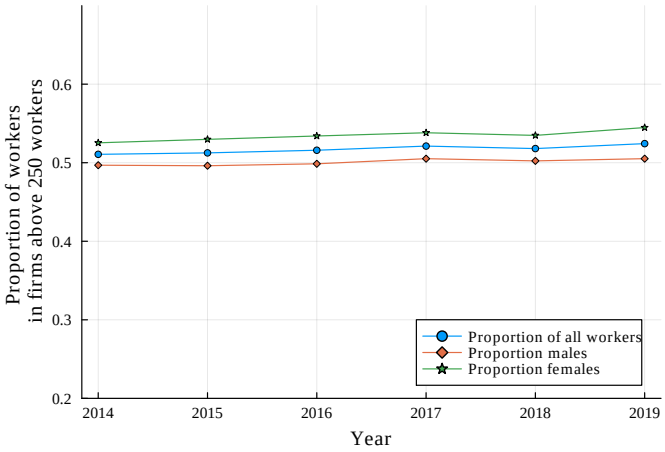


Notes: This figure plots the proportion of firms receiving employment shocks and the number of firms over the years 2014-2019. The red line plots the proportion of firms employing less than 250 workers in each year who had employed more than 250 workers in the year before after receiving a negative employment shock. The blue line plots the proportion of firms employing more than 250 workers in each year who had employed less than 250 workers in the year before after receiving a positive employment. The left axis measures the proportion of firms, and the right axis measures the total number of firms in each year plotted in bars.

the proportion of firms employing less than 250 workers in each year who had employed more than 250 workers in the year before. We observe that not only are the proportions very small (less than half a percent on average), but also there is no change in the evolution of this proportion. In particular, we do not observe a jump in the proportion of firms employing less than 250 workers in the post-law periods and had employed more than 250 workers in the pre-law periods. If anything, there is a modest decline. We refer to this line as a negative employment shock. For completeness, we also plot in blue, a positive employment shock which is the proportion of firms employing more than 250 workers in each year and had employed less than

250 workers in the year before. We observe that there is no systemic change in this proportion as well. 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.

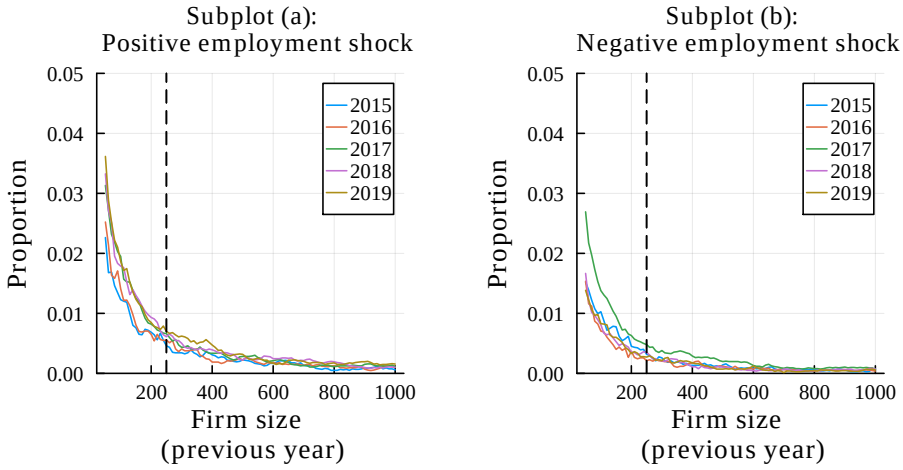
Figure 4: Proportion of workers working in firms employing above 250 workers over time



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.

4.3 Employment changes in firms of various sizes over time

Figure 5: Employment shocks

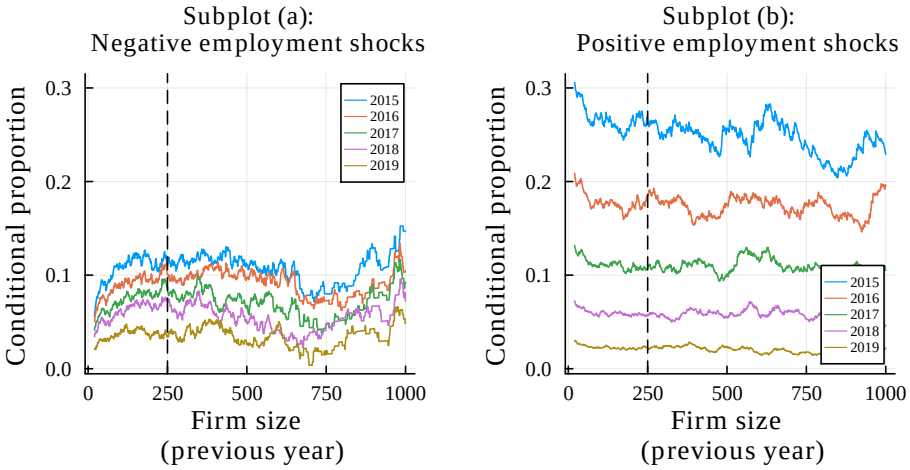


Notes: This figure plots the proportion of firms receiving employment shocks (on the y-axis) for a given firm size in the last year (on the x-axis) over the years 2014-2019. The left sub-plot plots the proportion of firms receiving a positive employment shock and expanded their size relative to the last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

Firms regularly experience employment shocks. Such employment shocks will change firm sizes over the years. In Figure 5 we plot two sub-plots to show what proportion of firms are impacted by employment changes from 2014 to 2019 for firms of different sizes. In Figure 5-

(a), we plot the proportion of all firms who had their size fall below—negative employment shock—the size that they had employed in the previous year (on the x-axis). The lines of different colors represent different years in each sub-plot. For example, the blue line at firm-size 250 on the x-axis and 0.005 on the y-axis represents that firms who had employed 250 workers in 2014 and had their size fell below 250 in 2015 constitute 0.5% of all firms. In Figure 5-(b) we plot the same except for when the firm experienced a positive employment shock and had their firm size increase relative to the previous year.

Figure 6: 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.

In Figure 6 we show how employment shocks vary over years conditional on employing a given number of workers the year before. The lines of different colors represent different years in each sub-plot. In Figure 6-(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 last year's size on the x-axis.[16] Figure 6-(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.

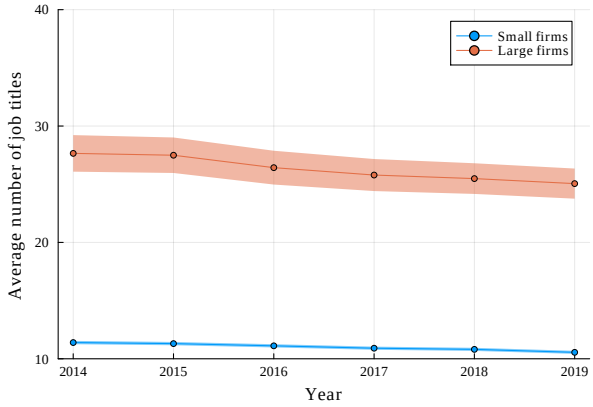
The primary observation we want to highlight here is that there is no systemic change in employment in firms employing more than 250 workers (or of any size up to a thousand workers) relative to how changes in employment occurred in similarly sized firms over time. These

[16]For example, the point on the blue line at 250 on the x-axis and 0.12 on the y-axis represents that 12% of firms which had 250 workers in 2014 experienced a negative employment shock and had their firm size drop below 250 in 2015.

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 Figure 5 and 6 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.

4.4 Job titles over time

Figure 7: 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 7 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 primarily because most 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. 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.[17]

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. Firm-specific CBA fixed effects could also be used as a measure of equal work done.

5.2 Treatment definition

In this section we first explain how we define treatment which we will later interact with time dummies to construct a standard event study estimation framework to estimate the impact of the policy. Let $j(i,t)$ represent the firm in which worker i worked at time t. We define treatment for a given time period t as a dummy variable which takes a value 1 if the number of workers

[17] 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 the firm where worker i is employed at time t is greater than or equal to 250.

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

In other words, this definition is equivalent to a dummy of whether a firm is large in any given time period. Observe this definition of treatment is consistent with the evidence we show in the previous section where we do not find that firms systematically attempt to avoid by endogenously choosing firm size. Additionally, this 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 policy on the treated.

It is essential to highlight that our definition of treatment differs from most of the pay transparency literature where the policy rule to disclose pay structures within the firm also follows a threshold criterion based on firm size. The pay transparency literature assumes that firm size is endogenous to the policy, but does not always test for it. Thus, treatment is defined as a dummy variable for whether the firm size was beyond the specified threshold in the year prior to policy implementation. Consequently, the parameters identified in the pay transparency literature are intent-to-treat effects. By construction, such a treatment definition is time-invariant. Hence, any kind of firm fixed effect will not allow the researcher to include a dummy for a large firm to capture any systemic effect of a large firm, since that will be subsumed within the firm fixed effect. This restriction forces comparison of firms which are close to the policy size threshold. In a latter section of the paper, on additional exercises and robustness checks we argue that firms close to the policy threshold may be most prone to changes in treatment status because of churn and thus estimates could be prone to measurement error. Additionally, this implicitly makes an assumption on the underlying data generating process. In particular, this assumes that firms can almost freely choose their size. This is in contrast to the labor market monopsony literature (Card, Cardoso, Heining & Kline 2018, Card 2022, Lamadon, Mogstad & Setzler 2022) where the firm size is determined in equilibrium given the labor supply curve each firm faces and the wage schedule it offers given its production function.

5.3 Identification

In estimating the causal impacts of the policy, we use an event-study framework. We compare the differences in wages between male and female workers in treated firms to differences in wages in their counterparts in untreated firms over time in an event-study framework. In doing so 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 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.

5.4 Estimation: event-study framework

5.4.1 Worker level estimations

The worker-firm level estimating equations to estimate the average treatment effect on the treated, take the following generic form:

$$
\begin{aligned}
y_{ijt} = & \sum_{s \in \mathcal{S}} \alpha_s * D_{j(i,s)s} \times \mathbb{1}[t = s] \times Male_i \; + \\
& \sum_{s \in \mathcal{S}} \gamma_s * D_{j(i,s)s} \times \mathbb{1}[t = s] \; + \; \sum_{s \in \mathcal{S}} \phi_s * Male_i \times \mathbb{1}[t = s] \; + \; \tau * D_{j(i,t)t} \times Male_i \; + \\
& \psi D_{j(i,t)t} \; + \; \delta Male_i \; + \; \theta_t \; + \; X'_{ij(i,t)t}\beta \; + \; \mathbb{FE} \; + \; e_{ijt}
\end{aligned}
$$

where $\mathcal{S} \equiv \{2014 : 2019\} \setminus \{2017\}$ is the set of years except the year prior to policy announcement and $\mathbb{FE}$ contains a specified set of fixed effects which includes the equal work fixed effect within a firm where worker i works. 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.[18] An example is $\mathbb{FE} \equiv \{\theta_{equalwork(j(i))}, \delta_{CBA\times t}, \delta_{ind}\}$ where $\theta_{equalwork(j(i))}$ is the equal work fixed effect within a firm where worker i works $\delta_{ind}, \delta_{CBA\times t}$ are industry, and time-specific CBA fixed effects respectively. We use a few different ways of defining $\theta_{equalwork(j(i))}$. Our most preferred and flexible definition defines workers producing equal work as those who work in the same firm, under the same CBA, in the same occupation and with the same job-title. In particular, $\theta_{equalwork(j(i))} \equiv \theta_{firm_j\times CBA_i\times occupation_i\times job-title_i}$.[19] Our results are robust to other less flexible definitions like defining equal work as the set of workers who work in the same firm, occupation and job-title, as well as defining it as the set of workers who work in the same firm, occupation, level of qualification and CBA.

We also 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.

Another estimating equation is one where we assume that the time effects for males and females are not different from each other is the following:

$$y_{ijt} = \sum_{s\in\mathcal{S}} \alpha_s * D_{j(i,s)s} \times \mathbb{1}[t = s] \times Male_i \;+\; \sum_{s\in\mathcal{S}} \gamma_s * D_{j(i,s)s} \times \mathbb{1}[t = s] \;+\; \tau * D_{j(i,t)t} \times Male_i \;+$$
$$\psi D_{j(i,t)t} \;+\; \delta Male_i \;+\; \theta_t \;+\; X'_{ij(i,t)t}\beta \;+\; \mathbb{FE} \;+\; e_{ijt}$$

Our results are robust to this assumption.

It is useful to discuss what each of these coefficients represent. The primary parameters of interest are $\{\alpha_s\}_s$. The parameter α_s represent the change in gender-wage gap between large

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

[19] 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.

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 estimates of α_s in the post-policy years provide us the estimate of the impact of the policy on the gender-wage gap in large firms, relative to small firms in year s compared to the base-year. 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 policy 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.

5.4.2 Firm level estimations

The firm-level estimating equations take the following generic form:

$$Y_{jt} = \sum_{s \in \mathcal{S}} \alpha_s * D_{js} \times \mathbb{1}[t = s] \;+\; \gamma * D_{jt} \;+\; \theta_t \;+\; \mathbb{FE} \;+\; X'_{jt}\beta \;+\; u_{jt}$$

In our preferred specification, $\mathbb{FE} \equiv \{\theta_j, \theta_{industry \times t}\}$.[20] In X_{jt}, we control for the firm's age and the location of the firm.[21] We investigate the effect of the policy on firm value added and

[20] Our results are robust to alternate though less flexible definitions of $\mathbb{FE}$ such as $\mathbb{FE} \equiv \{\theta_j, \theta_{industry}\}$.

[21] Location of firms follow the geographical demarcations as per Nomenclature of Territorial Units for Statistics

the variance of wages within the firm.

Effects on employment are not identified within the design-based framework

As we estimated causal effects of the policy on the wages of workers, we cannot directly estimate causal effects of the policy on the firm employment for all workers. 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 policy 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.

6 Results

We report the results on the impact of the policy on various worker level outcomes through event-study plots and tables of the estimates. For a given outcome variable, each worker level event-study plot consists of three sub-plots where we plot the jointly estimated effects of the policy on the gender gap, on female and on male outcomes. Below each sub-plot we report the corresponding estimated conditional base gaps in the outcome variable for the year 2017, relative to which each plotted estimate are to be interpreted. In the left-most sub-plot we plot the estimates of α_s which represents the effect of the policy on the gender gap in large firms relative to small firms as compared to their difference in the base year 2017. In the middle sub-plot we plot the estimates of γ_s which represent the effect of the policy on the female wage gap between large and small firms relative to the gap in 2017. In the right-most sub-plot, we plot the estimates of $\alpha_s + \gamma_s$ with their standard errors computed using the Delta Method, which represent the effect of the policy on the male wage gap between large and small firms

(NUTS) 2 regions.

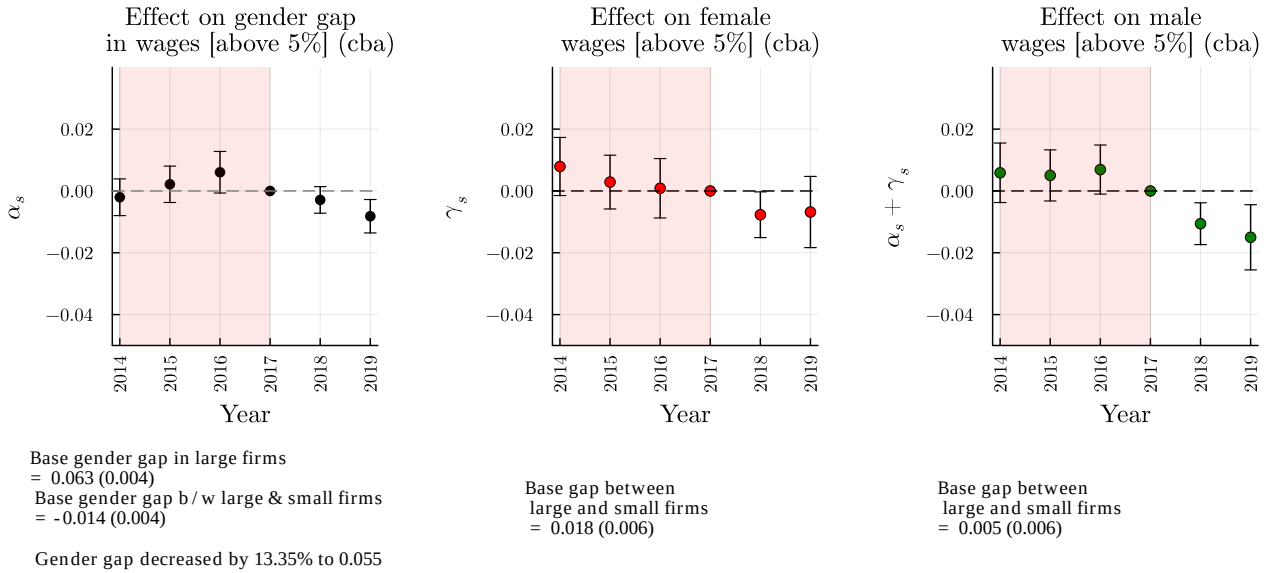
relative to the gap in 2017. We estimate the policy effects on wages for various sub-samples of workers. All our estimations control for the age of the worker in bins, their education, their qualifications, the region in which the firm is located, their experience, type of their contract and their nationality.

We begin discussing our estimates of the average treatment effect on the treated in firms with baseline gender wage gaps above five percent followed by those below five percent. Within each of these groups we discuss the results on workers covered by a collective bargaining agreement followed by those who were not. We then discuss the results on the overall sample of workers, and show that how pooling all treated firms and all treated workers, not only ignores the differences in the non-linear costs imposed by the law on different types of firms, and the wage setting processes, but most importantly masks the overall heterogeneity in the effects. We then discuss potential mechanisms driving these results.

6.1 Firms with baseline gender wage gap above five percent

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. We begin by presenting the results of the impact of the policy on the gender wage gaps, female and male wages in Figure 8 and estimates reported in column 1 of Table 4.

Figure 8: Workers in firms above five percent of baseline gender wage gap



Notes: This figure plots the ATT estimates from the event study specification estimated on the sample of workers in firms above five percent of baseline gender wage gap. 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.

In the years before the law 2014-2016, we find no statistical difference in the evolution of the gender wage gap between large and small firms, relative to how they differed in 2017—reassuringly providing us confidence in our conditional parallel trends assumption. In the left most subplot of Figure 8 we show that the pay equity law reduced the conditional gender wage gap in these firms from 6.3% by 0.8pp (p-value = 0.003) to 5.5% on average. This is equivalent to a 13.35% reduction in the gender wage gap within two years of the law.

Estimates on the effect of the law on female and male wages show that this reduction in gender wage gap was on average driven by a larger reduction in male wage growth relative to female wage growth. Male wage growth fell by 1.1pp in 2018 and by 1.5pp on average in treated firms in 2019. Female wage growth fell by 0.8pp in 2018 and 0.7pp in 2019 though statistically insignificant at the 95% confidence level.[22]

6.2 Firms with baseline gender wage gap below five percent

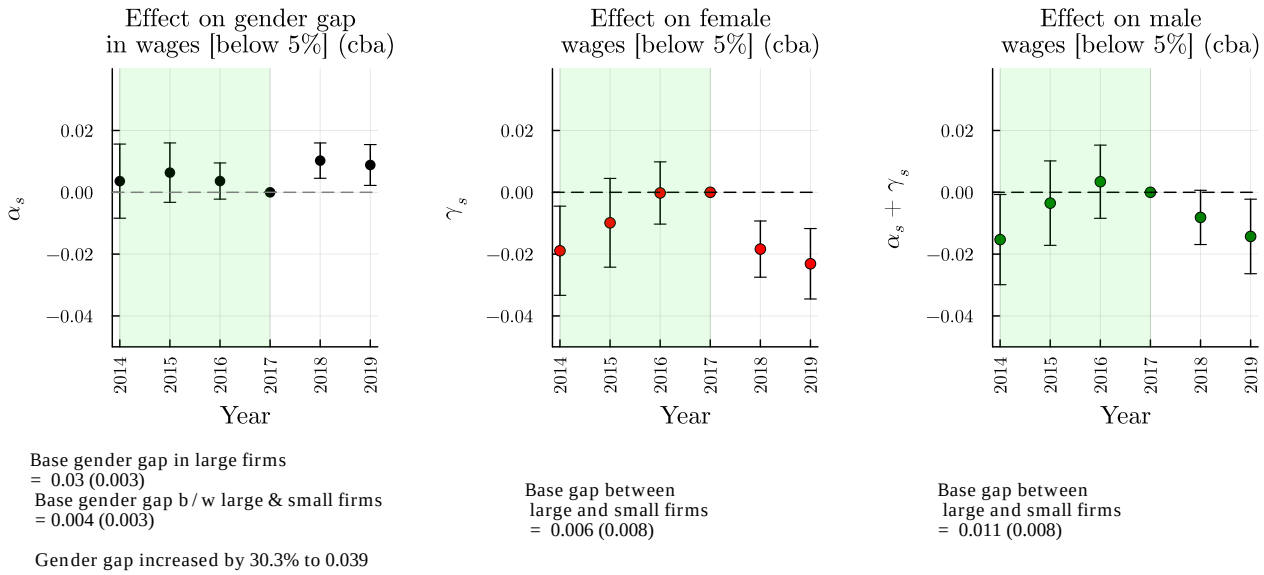
Firms employing more than 250 workers, but with baseline gender wage gaps though are treated. Given the enforcement rules they need not change existing wage disparities among its workers. However, among these firms, any uncertainty on penalties from having wage disparities were essentially removed by the law starting 2018.

In the years before the law 2014-2016, we find no statistical difference in the evolution of the gender wage gap between treated and untreated firms, relative to how they differed in 2017—reassuringly providing us confidence in our conditional parallel trends assumption. In the left most subplot of Figure 9 we show that the pay equity law increased the conditional gender wage gap in these firms from 3% by 1pp in 2018 (p-value = 0.0004) and 0.9pp (p-value = 0.008) in 2019 to 3.9% on average. This is equivalent to a 30.3% increment in the gender wage gap within two years of the law. We find that this increment in the gender wage gap resulted from a larger decline in female wage growth relative to male wage growth. Female wage growth in treated firms fell by at least 1.8pp -2.3pp (p-value < 0.0001). In contrast, the reduction in male wage growth was close to half that of their female counterparts, at 0.8pp -1.4pp .

In the sub-plots depicting the estimates on male and female wages, the seeming failure in parallel trends in the year of 2014 actually makes our estimates on the effect of the policy

[22] As discussed before, the conditional base gap in female wages between large and firms is identified off firms who at any point of time during the sample period move to either side of the threshold from previously being a small or a large firm respectively because of exogenous employment shocks. Otherwise, this effect would have been subsumed by the equal work fixed effect which is invariant within the firm.

Figure 9: Workers in firms below five percent of baseline gender wage gap



Notes: This figure plots the ATT estimates from the event study specification estimated on the sample of workers in firms below five percent of baseline gender wage gap. 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.

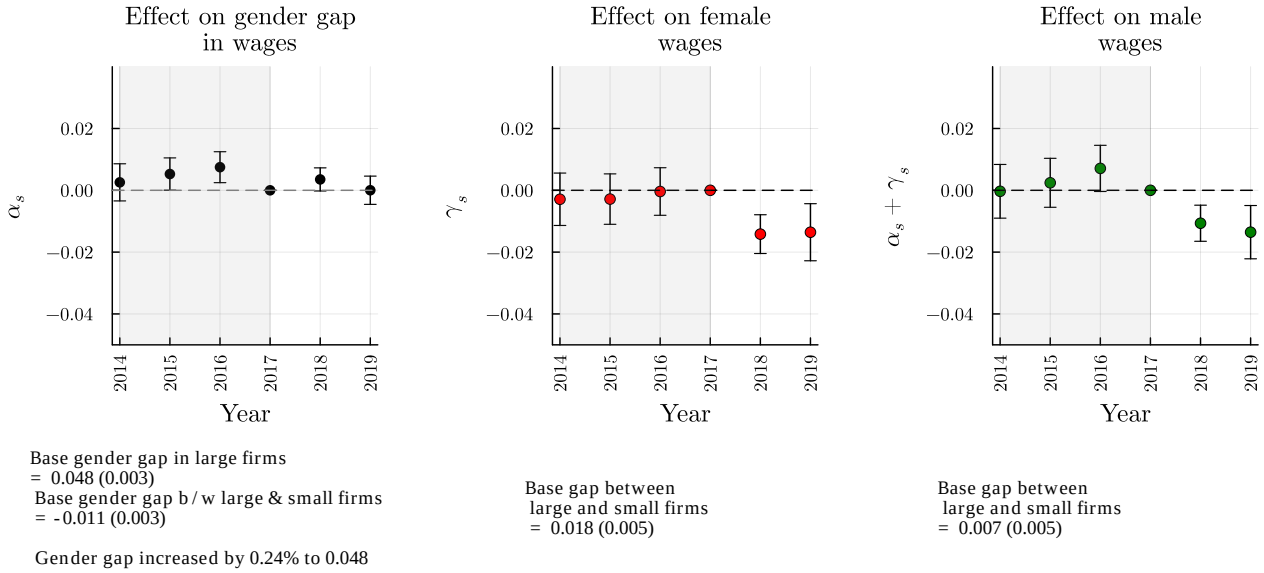
on male and female wages, a lower bound. In 2014, both male and female wage growths in treated firms were lower than in untreated firms. However, we also see that both male and female wages in treated firms were trending upwards in the years before the law, with no statistically significant differences in 2015 and 2016. If the trends had continued as they did from 2014 to 2016, our estimate on the effect of the policy on male and female wages are lower bounds following Rambachan & Roth (2023) in the Appendix. Furthermore, the larger 'failure' of parallel trends for female wages in 2014, in conjunction with its opposite trends in the year after the law, implies that had the trends continued similarly in the years after the law, then female wages would have dropped even more than our estimates, than they would drop for male workers. This implies that the increment in gender wage gap in treated firms who were under the five percent threshold would be even larger than our estimates, making our estimate of an increment in the gender wage gap by 30%, a lower bound.

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

In this subsection we show that if we pool all workers together, the estimates will not reveal the differential wage dynamics on male and female workers in treated firms by their baseline gender wage gap. We report the estimates in column 3 of Table 4 and plot them in Figure 10.

In the years before and after the law, we find no impact of the pay equity law on the evo-

Figure 10: Workers in all firms



Notes: This figure plots the ATT estimates from the event study specification estimated on the sample of workers in all firms above five percent of gender wage gap. 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.

lution 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 entire effect of how 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. Furthermore, we see an equal drop in wage growth of male and female workers. This too masks the differential impact the law has on gender-specific wage growths, where female wage growth primarily fell in firms who were already under the target gender wage gap, but the male wage growth fell primarily in firms who were above the target gender wage gap prior to the announcement of the law.

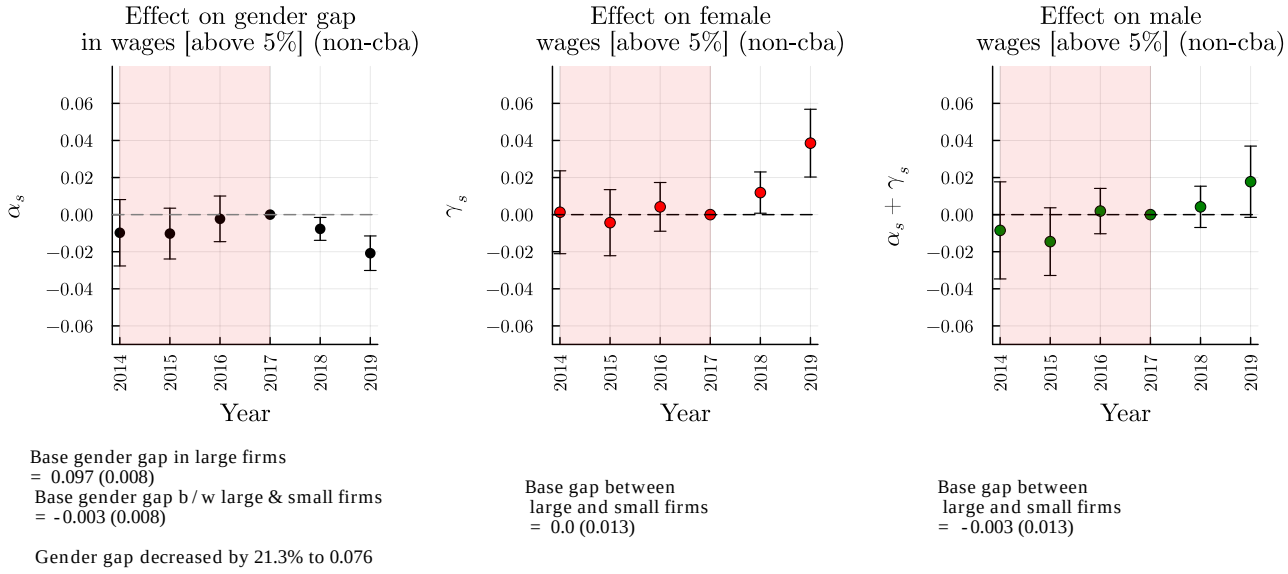
6.4 Impact of pay-equity law on non-CBA workers

As discussed earlier workers not covered by a CBA comprise a small proportion—15% of the workforce. Among the treated firms, this proportion is similar, around 16% . As explained in the institutional details, this group needs to be examined differently because of the differences in the wage setting process for workers in this category which primarily involves individual worker-firm bargaining. In addition, this group of workers need separate attention, given existing evidence of higher gender inequality among workers not covered by CBAs.

6.4.1 Firms with more than 5% of gender wage gap

We present the results of the effect of the law on firms with more than 5% gender wage gap in Figure 11 and Table 5. Consistent with existing evidence (Bruns 2019) we find that in firms with larger than 5% of gender wage gap, the conditional base gender gap among workers not covered by any CBA is quite high at 9.7%. This gap is one and a half times larger than the gap among workers covered by a CBA. We find that the policy had the largest impact on the gender wage gap among these workers. In the periods before the law we do not find any differences in the evolution of gender wage gaps among treated and untreated firms. In two years of policy implementation, the gender wage gap reduced from 9.7% ins 2017 to 7.6% in 2019. These estimates correspond to closing the gender wage gap among non-CBA workers by more than a fifth. The other two sub-plots in Figure 11 shows that the effect is primarily driven by a lager wage growth among female workers. In 2017, female workers in untreated firms earned similar on average to female workers in treated firms. In two years of the announcement of the law, non-CBA female workers experienced 3.8pp increase in their wage growth in treated firms relative to untreated firms on average.

Figure 11: Non-CBA workers in firms above five percent of baseline gender wage gap



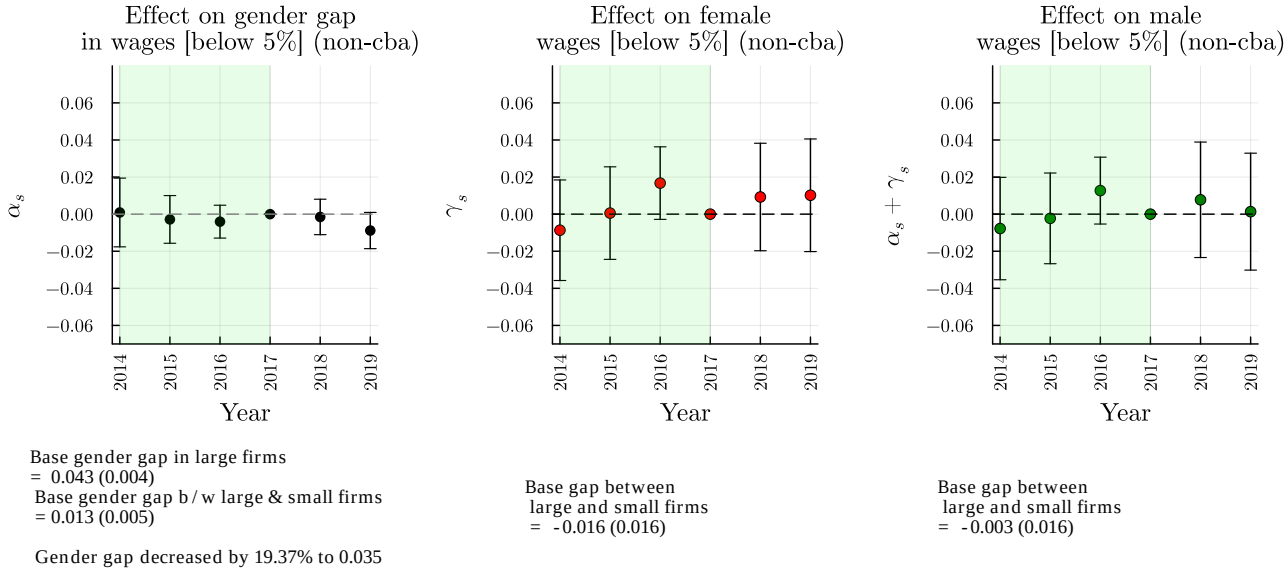
Notes: This figure plots the ATT estimates from the event study specification estimated on the sample of workers not covered by any CBA in firms above five percent of baseline gender wage gap. We plot the ATT estimates α_s, γ_s and $\alpha_s + \gamma_s$ of the effect of the 2018 pay equity law on gender 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.

6.4.2 Firms with less than 5% of gender wage gap

We present the results of the effect of the law on workers not covered by any CBA and working in firms with less than 5% gender wage gap in Figure 12 and column 2 of Table 5.

Since this group of workers belong to the most inequitable group, the conditional baseline gender wage gap was still quite high at 4.3%. This gap is 1.26 times larger than the gap among workers covered by a CBA working in firms with baseline gender wage gaps under five percent. In the periods before the law we do not find any differences in the evolution of gender wage gaps among treated and untreated firms. We find that the law had almost no impact on these group of workers and there are no discernible impacts on male and female workers as we see in the other two subplots of Figure 12 shows that the effect is primarily driven by a lager wage growth among female workers. In 2017, female workers in untreated firms earned similar on average to female workers in treated firms. In two years of the announcement of the law, non-CBA female workers experienced 3.8pp increase in their wage growth in treated firms relative to untreated firms on average.

Figure 12: Non-CBA workers in firms below five percent of baseline gender wage gap



Notes: This figure plots the ATT estimates from the event study specification estimated on the sample of workers not covered by any CBA agreement in all firms below five percent of gender wage gap. 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.

6.5 Comparing effects across workers covered and not covered by any CBA

Existing evidence shows that workers who are not covered by collective bargaining agreements tend to have larger gender wage gaps (Bruns 2019). Well documented evidence on gender gaps in bargaining could explain these large disparities. In comparison, workers covered by any CBA have their wage floors fixed by their job-title specific CBA which are gender-neutral. Hence, any variation in wages comes from the wage cushions that firms idiosyncratically add on top of the floor. Card & Cardoso (2022) document that these cushions tend to be larger for males than females. Thus, the fact that we see much lower baseline gender wage gaps among workers covered by CBAs is not surprising. Given the large initial disparities among non-CBA workers, we see the reduction in gender wage gap operating through increment in female wage growth. In contrast, among CBA workers with smaller baseline wage disparities, the reduction in gender wage gaps operated through slowing down of male wage growth.

Lowering larger wage disparities among non-CBA workers, by slowing down wage growth of males would require a substantial drop in their wage growth. This could have in turn lead to larger turnover among male non-CBA workers, which the firms may have found harder to replace, given that these workers tend to be more educated and hence plausibly more skilled. We see that in the data, non-CBA workers tend to be more educated. Consequently, non-CBA male workers may have substantially high bargaining power, which could have countered any effort of the firm to reduce their wage growth. In absence of this channel, the only way for firms to reduce gender disparity among these most inequitable group of workers had to come through increment of female wage growth.

We find no effects of the policy on non-CBA workers working in firms with baseline gender wage gap under five percent. This is primarily because the conditional gender wage gap among these workers was very close to the target gender wage gap. Hence, for these firms, the available margins of wage adjustments were small.

7 Additional exercises and robustness checks

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

7.1 Intent-to-treat effects

In this subsection we discuss the intent-to-treat effects of the policy. To do so we re-define treatment as the dummy of whether a firm had more than 250 workers in the year prior to the policy implementation. Under this definition, the indicator of being a large firm is time-invariant. Specifically, the firm in which worker i is working in 2017 is a treated firm if it had employed more than 250 workers in 2017. We define the treatment dummy as follows.

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

Given this definition of treatment, we can estimate the intent to treat effects of the policy on the outcomes of interest using the following equation in an event-study framework. However, 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 we will not be able to identify the effect of a large firm because that will be subsumed by the fixed effects at the company level. 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. Given this we estimate:

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

All other variables follow the same definitions as before.

We report the estimates of the event studies in Figures 14 and 15 and for non-CBA workers in Figures 22 and 23 and in Tables... We find that the ITT is not largely different from the ATT. This is primarily driven by the fact that in our sample only 1.73% of firms move on either side of the 250 worker firm size threshold over the entire sample period.

7.2 Potential measurement error in treatment

In the data, we observe the firm size at one point of time in the year. However, within any given year firms with size close to the threshold could be moved to the left and the right of the threshold of 250 workers, because of regular churn and employment shocks in general,

independent of the law. This will result in these firms being exposed to different treatment status within the same year. This plausible irregularity in treatment status, would induce measurement error in treatment. Consequently, our estimates of the impact of the law would be attenuated down towards zero, driven by extent to which how large is the underlying variance in unobserved differential treatment status of firms within a year. To address this potential concern, we note that it is unlikely that firms who are far away from the threshold of 250 workers would be subject to such exogenous employment shocks which would change their treatment status. Hence, to address this concern, we remove firms employing in between 200 and 300 workers and re-estimate our empirical models on the remaining sample of firms. We find that our results are robust to this restriction. We report these results in Figures 16 and 17 and for non-CBA workers in Figures 24 and 25

7.3 Potential concern on endogenous mobility

Mobility of workers could be associated with higher wage growth independent of the policy. Workers who move usually do so because of an associated wage increment, or expectation thereof, or because of some non-pecuniary compensating differential. It could be that the policy induced more females to sort into larger firms in expectation of higher wage growth or reduced mobility of existing female workers in large firms. Our results maybe affected if the policy induced differential mobility relative to the pre-policy periods. To test how much our results, if any, are driven by differential mobility of workers, we test the sensitivity of our results by estimating the empirical model after restricting the sample to only those workers who did not change firms in the sample period. We find that the results are similar to those discussed in the prior sections where we included all workers in our estimation. We report these results in Figures 18 and 19 and for non-CBA workers in Figures 26 and 27. This result also suggests that the policy by itself did not induce substantial differential mobility of workers in the short-run. Consequently, mobility in short run if any is plausibly unlikely to be systematically endogenous to the pay equity law. This gives us confidence of comparisons of workers of similar types before and after the policy and that our results are robust to mobility of workers.

7.4 Full-time workers

Full time workers 92% of our sample. Firms could have passed through the burden of the law on part-time workers if their bargaining power were plausibly lower. This would raise the

concern if our results were driven by the disproportionate impact of the law in treated firms on part-time workers. To address this concern we re-estimate our model only on full-time workers. Restricting our sample to only full-time workers does not change our results. We report these results in Figures 20 and 21 and for non-CBA workers in Figures 28 and 29.

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

In the results on the causal impact of the policy, we show that on average firms which had gender wage gaps below five percent had increased their gender wage gaps because of the 2018 pay equity law. The underlying reasons for this unintended consequence of the law revolves around the enforcement of the law.

Portuguese workers have enjoyed Constitutional Rights to pay equity since 1976. However, clear enforcement rules for pay equity were absent until the 2018 pay-equity law. This law specified that firms with a gender wage gap exceeding 5% could face fines unless they addressed these disparities. By doing so, it dispelled uncertainties about potential penalties in presence of different levels of gender pay gaps. Specifically, the law made it clear that the expected costs of having wage disparities would be significant only if the gaps surpassed five percent. This non-linear shift in the anticipated costs of the law to the firms, primarily explains the results we presented earlier: firms with a gender wage gap over 5% reduced their gap, whereas those with gaps below 5% saw an increase. The probable mechanisms we'll discuss are anchored in this non-linear cost structure related to gender pay gap. Though in absence of rich additional data, it is hard to separately identify the relative importance of each mechanism, discussion of them is essential since they could lead to different welfare implications.

8.1 Risk aversion

Before the implementation of the 2018 pay-equity law, firms with higher levels of risk aversion could have been more likely to maintain smaller gender wage gaps because of the uncertainties associated with potential repercussions from an undefined regulatory environment. In such a context, risk averse firms would err on the side of caution, minimizing potential areas of contention like wage disparities. The 2018 pay-equity law, however, introduced a clear benchmark by specifying a 5% gender wage gap. This clear demarcation effectively resolved

the earlier uncertainties that might have constrained the actions of risk-averse firms. While previously they might have been wary of approaching or exceeding an undefined wage gap limit, they could now easily adjust their wage policies and avoid any penalty as long as their gender wage gaps remain under the specified 5% threshold.

8.2 Compensating differentials

Firms with wage gaps below the 5% limit could offer non-wage amenities as compensating differentials to lower wage growth of female employees. Given a large literature on gender differences in valuation of non-wage amenities, it is plausible that female workers would still be willing to supply labor at lower wage growth rates, if offered better or more non-wage amenities. Thus, firms could plausibly increase their wage gaps by offering additional amenities or benefits to female workers, arguing that the overall compensation, when considering non-wage amenities, remains equitable. As long as the cost to provide these amenities is lower than the wage bill saved by the firms, this approach would allow firms to make direct wage adjustments while remaining under 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), or observable data on non-wage amenities (Dey & Flinn 2005), or exogenous variations thereof (Mas & Pallais (2017), Wiswall & Zafar (2018), Alam et al. (2023)).

8.3 Taste based discrimination

Given the vast evidence on labor markets not being competitive but rather monopsonistic, the long run existence of discriminatory firms is plausible. The equilibrium gender wage gaps in such firms result from trading off firm preferences to discriminate (Becker 1957) and their expected cost of discrimination. Moderately discriminatory firms—those at the bottom of the discrimination distribution—could have gender wage gaps below five percent before 2018. After the enforcement rule of the 2018 pay equity law, the expected cost to discriminate falls for such firms. As a result, such firms could now increase the gender wage gap by reducing the female wage growth. It is also important to note that labor market frictions could also restrict workers from switching to their most preferred employer in 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 be another explanation of why we see gender

wage gaps go up in firms who were under the 5 percent gender wage gap before 2018. It is also important to highlight that the 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 under five percent are discriminatory.

Understand 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.

9 Conclusion

Pay equity laws exist in many countries, but studying their causal impacts have been challenging due to uniform exposure. We examine the effects of a pay equity law in Portugal, aimed at promoting wage equality for "equal work" among genders in firms employing more than 250 workers, enforceable by imposing fines on those with more than five percent gender wage gap. In this paper, we have presented a cautionary tale on unintended consequences of pay equity laws and their enforcement, if implemented without paying attention to the existing distribution of gender wage gaps. Using detailed matched employer-employee data, in an event-study design, we estimate the impact of the law, and document large unintended consequences. In firms above five percent wage gaps—employing a little above half of the treated workforce—the gap reduced by 10% driven by a larger reduction in male wage growth. However, in the remaining firms—those with gaps under five percent—the wage gap increased driven by larger reductions in female wage growth. Separately, among the most inequitable group—workers not covered by any collective bargaining agreement making up fifteen percent of the workforce—the law reduced wage gaps by a fifth through increased female wage growth. Yet, back of the envelope calculations reveal that while wages of one in fifty women grew, wags of one in five women shrunk.

On March 2021, the EU has recommended its member countries to impose fines on firms with gender wage gaps exceeding five percent. Our paper presents evidence that such policies could have large unintended consequences. A law which primarily targets firms above a certain well-defined gender wage gap, removes almost all uncertainty for firms with lower gaps

unlike in the years prior to this law, where the firms were uncertain about the costs of having wage disparities. Our paper underscores the importance of pay-equity laws while presenting a cautionary tale of unintended effects which could be large and remain masked in average effects.

References

Addison, J. T., Portugal, P. & de Almeida Vilares, H. (2023), 'Union membership density and wages: The role of worker, firm, and job-title heterogeneity', *Journal of Econometrics* **233**(2), 612–632.

Alam, M. M. U., Mookerjee, M. & Roy, S. (2023), Worker side discrimination: Beliefs and preferences-evidence from an information experiment with jobseekers. Working paper.

Baker, M. & Fortin, N. M. (2004), 'Comparable worth in a decentralized labour market: the case of ontario', *Canadian Journal of Economics* **37**(4), 850–878.

Baker, M., Halberstam, Y., Kroft, K., Mas, A. & Messacar, D. (2023), 'Pay transparency and the gender gap', *American Economic Journal: Applied Economics* **15**(2), 157–183.

Bennedsen, M., Simintzi, E., Tsoutsoura, M. & Wolfenzon, D. (2022), 'Do firms respond to gender pay gap transparency?', *The Journal of Finance* **77**(4), 2051–2091.

Bhuller, M., Moene, K. O., Mogstad, M. & Vestad, O. L. (2022), 'Facts and fantasies about wage setting and collective bargaining', *Journal of Economic Perspectives* **36**(4), 29–52.

Biasi, B. & Sarsons, H. (2022), 'Flexible wages, bargaining, and the gender gap', *The Quarterly Journal of Economics* **137**(1), 215–266.

Blau, F. D. & Kahn, L. M. (2017), 'The gender wage gap: Extent, trends, and explanations', *Journal of economic literature* **55**(3), 789–865.

Breza, E., Kaur, S. & Shamdasani, Y. (2018), 'The morale effects of pay inequality', *The Quarterly Journal of Economics* **133**(2), 611–663.

Bruns, B. (2019), 'Changes in workplace heterogeneity and how they widen the gender wage gap', *American Economic Journal: Applied Economics* **11**(2), 74–113.

Card, D. (2022), 'Who set your wage?', *American Economic Review* **112**(4), 1075–1090.

Card, D. & Cardoso, A. R. (2022), 'Wage flexibility under sectoral bargaining', *Journal of the European Economic Association* **20**(5), 2013–2061.

Card, D., Cardoso, A. R., Heining, J. & Kline, P. (2018), 'Firms and labor market inequality: Evidence and some theory', *Journal of Labor Economics* **36**(S1), S13–S70.

Card, D., Cardoso, A. R. & Kline, P. (2016), 'Bargaining, sorting, and the gender wage gap: Quantifying the impact of firms on the relative pay of women', *The Quarterly journal of economics* **131**(2), 633–686.

Card, D., Mas, A., Moretti, E. & Saez, E. (2012), 'Inequality at work: The effect of peer salaries on job satisfaction', *American Economic Review* **102**(6), 2981–3003.

Cullen, Z. B. & Pakzad-Hurson, B. (2023), 'Equilibrium effects of pay transparency', *Econometrica* **91**(3), 765–802.

Cullen, Z. & Perez-Truglia, R. (2022), 'How much does your boss make? the effects of salary comparisons', *Journal of Political Economy* **130**(3), 766–822.

Dey, M. S. & Flinn, C. J. (2005), 'An equilibrium model of health insurance provision and wage determination', *Econometrica* **73**(2), 571–627.

Goldin, C. (2014), 'A grand gender convergence: Its last chapter', *American Economic Review* **104**(4), 1091–1119.

Goldin, C., Kerr, S. P., Olivetti, C. & Barth, E. (2017), 'The expanding gender earnings gap: Evidence from the lehd-2000 census', *American Economic Review* **107**(5), 110–114.

Guiso, L., Pistaferri, L. & Schivardi, F. (2005), 'Insurance within the firm', *Journal of Political Economy* **113**(5), 1054–1087.

Gulyas, A., Seitz, S. & Sinha, S. (2023), 'Does pay transparency affect the gender wage gap? evidence from austria', *American Economic Journal: Economic Policy* **15**(2), 236–255.

Hall, R. E. & Krueger, A. B. (2012), 'Evidence on the incidence of wage posting, wage bargaining, and on-the-job search', *American Economic Journal: Macroeconomics* **4**(4), 56–67.

Lamadon, T., Mogstad, M. & Setzler, B. (2022), 'Imperfect competition, compensating differentials, and rent sharing in the us labor market', *American Economic Review* **112**(1), 169–212.

Mas, A. & Pallais, A. (2017), 'Valuing alternative work arrangements', *American Economic Review* **107**(12), 3722–3759.

Perez-Truglia, R. (2020), 'The effects of income transparency on well-being: Evidence from a natural experiment', *American Economic Review* **110**(4), 1019–1054.

Rambachan, A. & Roth, J. (2023), 'A more credible approach to parallel trends', *Review of Economic Studies* p. rdad018.

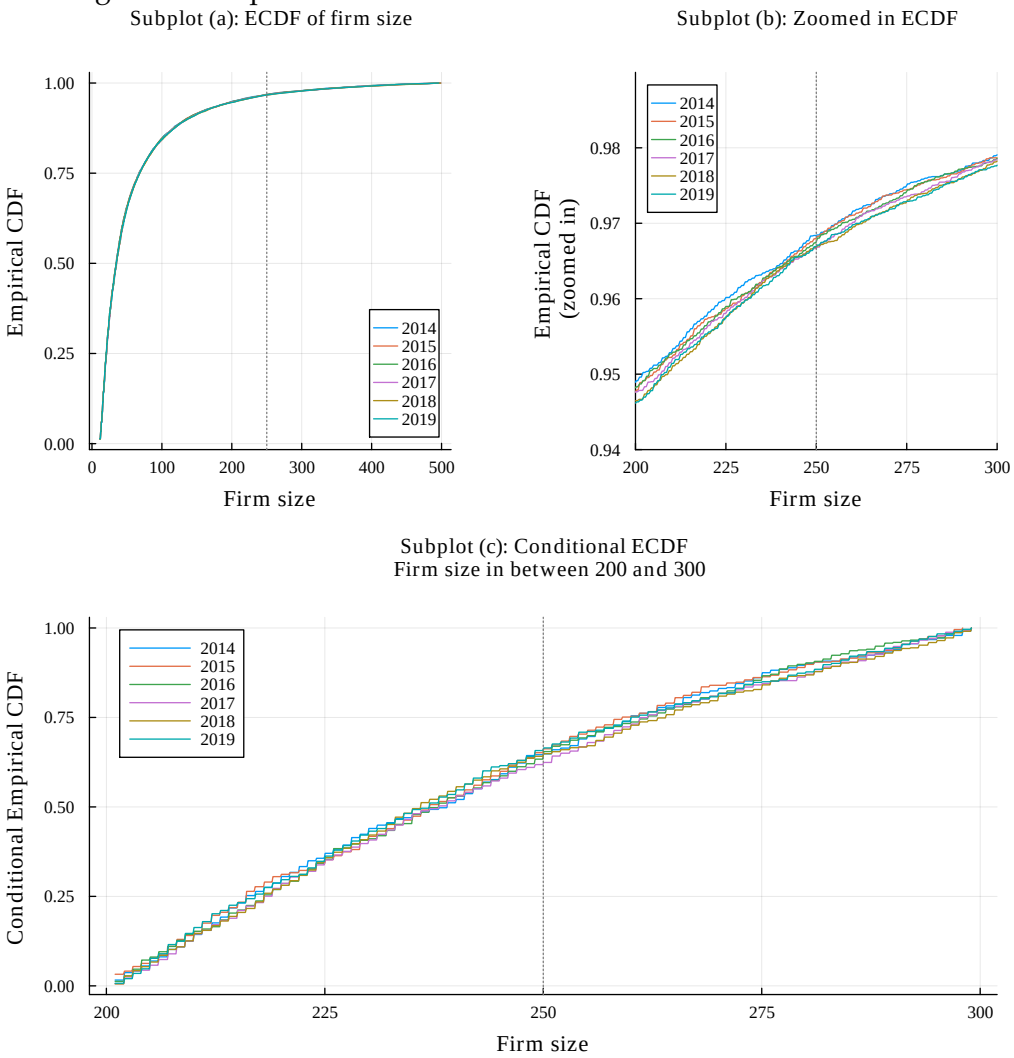
Roussille, N. (2021), 'The central role of the ask gap in gender pay inequality', *Working paper, University of California, Berkeley* .

Wiswall, M. & Zafar, B. (2018), 'Preference for the workplace, investment in human capital, and gender', *The Quarterly Journal of Economics* **133**(1), 457–507.

A Appendix-A

A.1 Tables and Figures

Figure 13: Empirical Cumulative Distribution of firm size over time



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

Table 1: Summary statistics: All workers and by gender in 2017

	All			Female			Male		
	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N
Female	0.480	0.500	1516180	1.000	0.000	727368	0.000	0.000	788812
Firm size > 250	0.500	0.500	1516180	0.512	0.500	727368	0.489	0.500	788812
Monthly hours	156.221	35.998	1516180	152.250	39.133	727368	159.884	32.414	788812
Monthly wage	1194.810	1539.666	1516180	1022.730	832.610	727368	1353.486	1965.901	788812
Log hourly wage	1.832	0.549	1516180	1.735	0.506	727368	1.922	0.571	788812
Age	39.856	11.126	1516180	39.772	10.990	727368	39.934	11.250	788812
Tenure at firm	8.226	9.484	1516114	7.953	9.131	727345	8.478	9.792	788769
Full-time	0.924	0.265	1516180	0.890	0.313	727368	0.955	0.207	788812
Not covered by CBA	0.170	0.375	1516180	0.171	0.376	727368	0.169	0.374	788812

Table 2: Summary statistics: By firm size in 2017

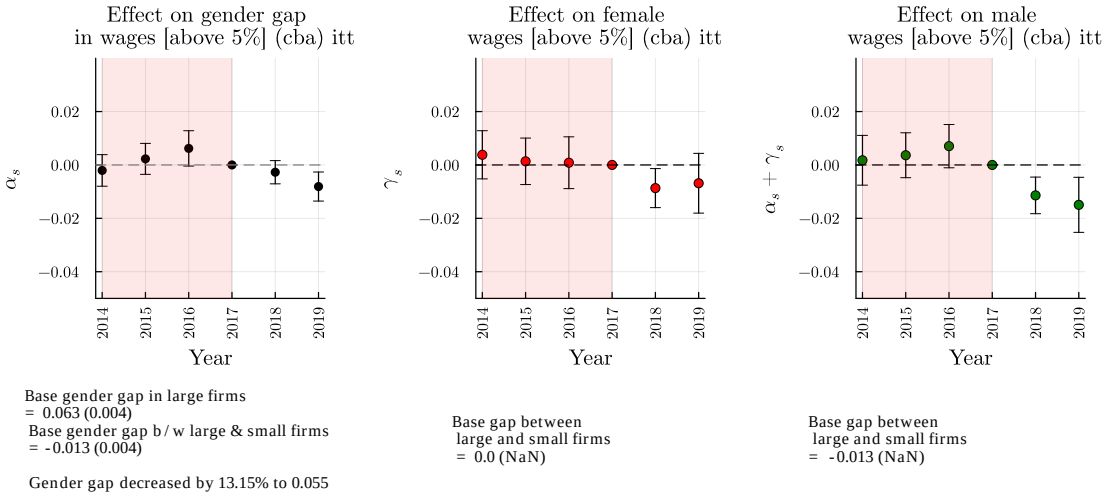
	Firm size below 250			Firm size above 250		
	Mean	Std. Dev.	N	Mean	Std. Dev.	N
Female	0.468	0.499	757983	0.491	0.500	758197
Firm size > 250	0.000	0.000	757983	1.000	0.000	758197
Monthly hours	161.443	29.705	757983	151.001	40.678	758197
Monthly wage	1138.490	1415.906	757983	1251.114	1652.244	758197
Log hourly wage	1.784	0.512	757983	1.880	0.579	758197
Age	40.248	11.137	757983	39.465	11.101	758197
Tenure at firm	7.937	9.260	757934	8.516	9.694	758180
Full-time	0.961	0.195	757983	0.888	0.316	758197
Not covered by CBA	0.131	0.338	757983	0.208	0.406	758197

Table 3: Summary statistics: By gender wage gap in 2017

	Above 5% GWG			Below 5% GWG		
	Mean	Std. Dev.	N	Mean	Std. Dev.	N
Female	0.547	0.498	924193	0.374	0.484	591987
Firm size > 250	0.481	0.500	924193	0.530	0.499	591987
Monthly hours	157.161	34.101	924193	154.755	38.729	591987
Monthly wage	1262.345	1831.878	924193	1089.376	902.363	591987
Log hourly wage	1.864	0.575	924193	1.783	0.501	591987
Age	40.327	10.989	924193	39.121	11.298	591987
Tenure at firm	8.672	9.486	924146	7.531	9.439	591968
Full-time	0.926	0.262	924193	0.921	0.269	591987
Not covered by CBA	0.166	0.372	924193	0.175	0.380	591987

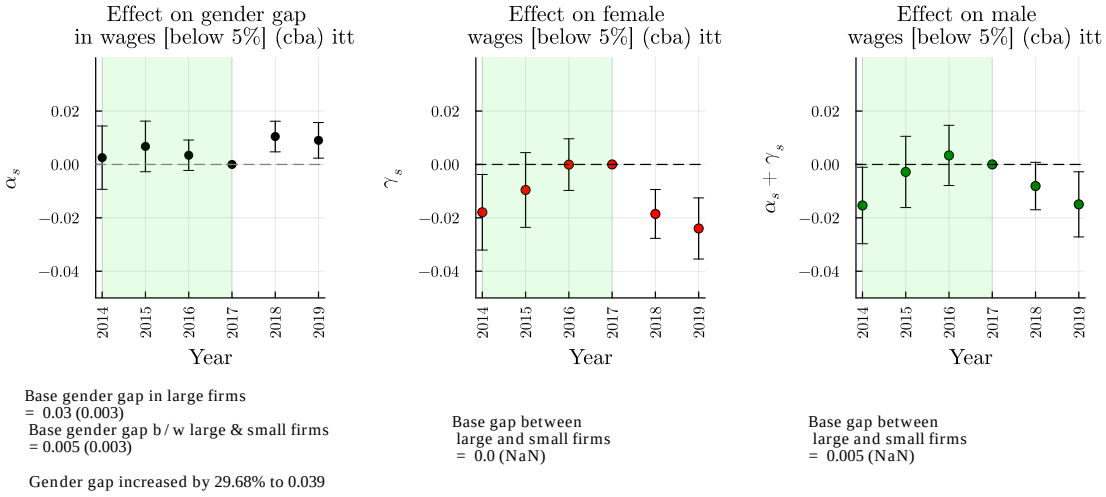
A.2 Intent-to-treat effects

Figure 14: ITT: Workers in firms above five percent of baseline gender wage gap



Notes: This figure plots the ITT estimates from the event study specification estimated on the sample of workers in firms above five percent of baseline gender wage gap. We plot the ITT 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.

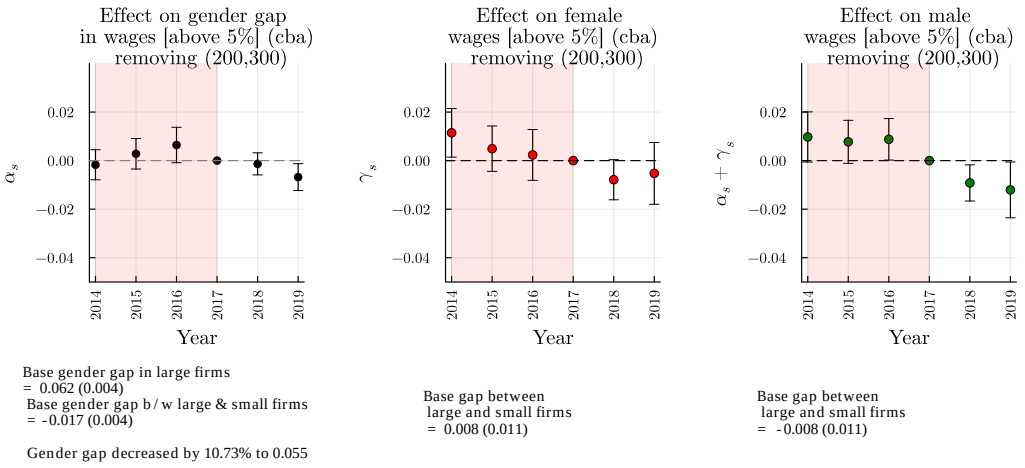
Figure 15: ITT: Workers in firms below five percent of baseline gender wage gap



Notes: This figure plots the ITT estimates from the event study specification estimated on the sample of workers in firms below five percent of baseline gender wage gap. We plot the ITT 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.

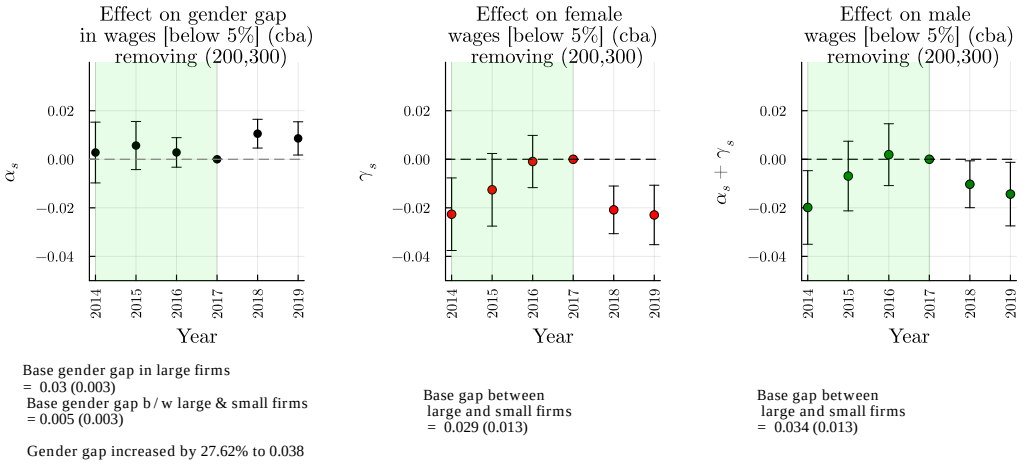
A.3 Removing firms employing in between 200 and 300 workers

Figure 16: Workers in firms above five percent of baseline gender wage gap



Notes: This figure plots the ATT estimates from the event study specification estimated on the sample of workers in firms above five percent of baseline gender wage gap after removing all firms that employ in between 200 and 300 workers. 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.

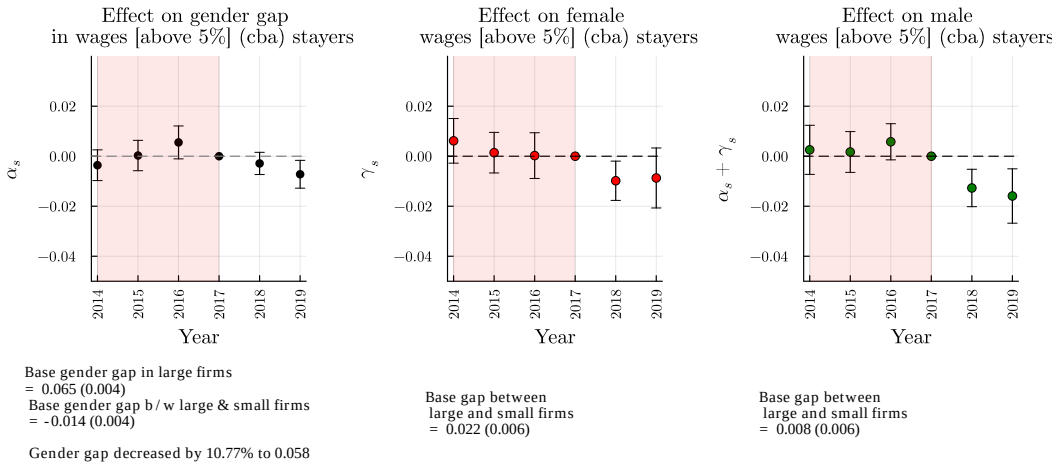
Figure 17: Workers in firms below five percent of baseline gender wage gap



Notes: This figure plots the ATT estimates from the event study specification estimated on the sample of workers in firms below five percent of baseline gender wage gap after removing all firms that employ in between 200 and 300 workers. 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.

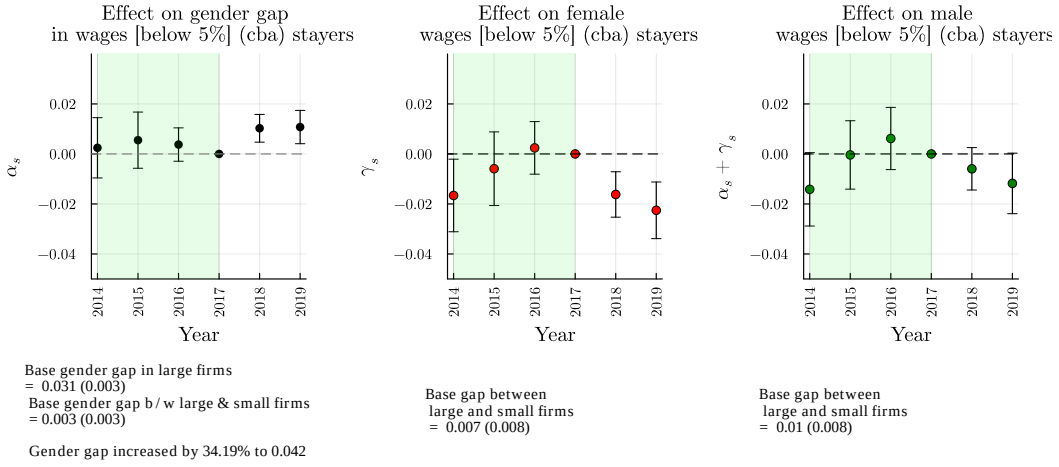
A.4 Stayers

Figure 18: Stayers: Workers in firms above five percent of baseline gender wage gap



Notes: This figure plots the ATT estimates from the event study specification estimated on the sample of workers in firms above five percent of baseline gender wage gap who did not switch firms in the sample period. 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.

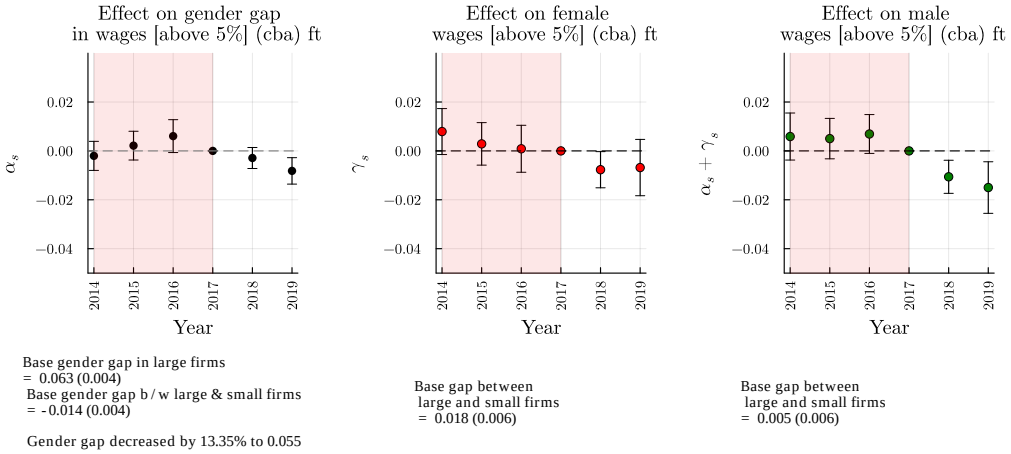
Figure 19: Stayers: Workers in firms below five percent of baseline gender wage gap



Notes: This figure plots the ATT estimates from the event study specification estimated on the sample of workers in firms below five percent of baseline gender wage gap who did not switch firms in the sample period. 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.

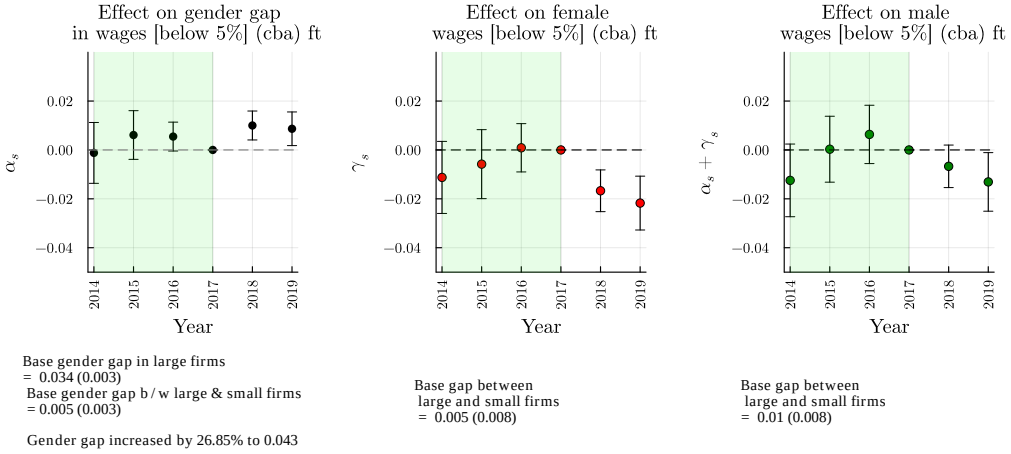
A.5 Full-time workers

Figure 20: Full-time : Workers in firms above five percent of baseline gender wage gap



Notes: This figure plots the ATT estimates from the event study specification estimated on the sample of workers in firms above five percent of baseline gender wage gap who work full-time.. 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 21: Full-time : Workers in firms below five percent of baseline gender wage gap



Notes: This figure plots the ATT estimates from the event study specification estimated on the sample of workers in firms below five percent of baseline gender wage gap who work full-time.. 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.

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

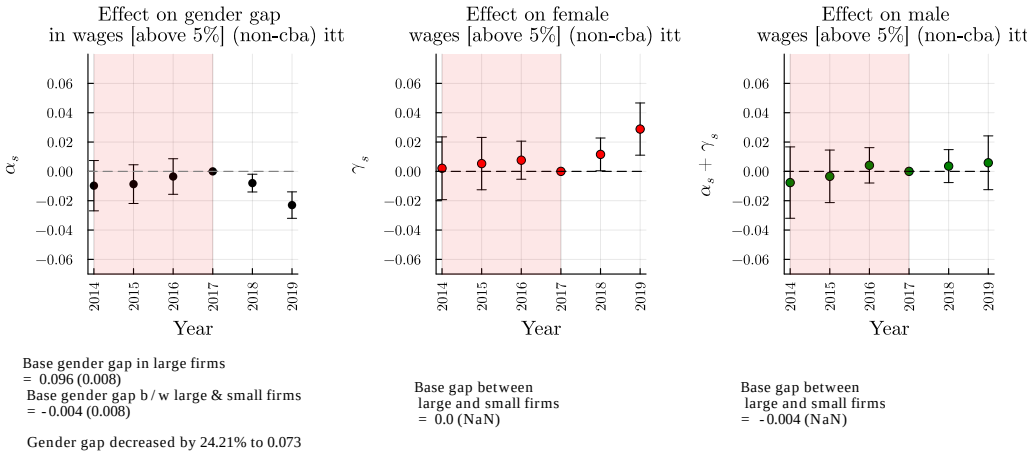
	Above 5% GWG	Below 5% GWG	Pooled
	(1)	(1)	(1)
D_{jt}	0.018***	0.006	0.018***
	(0.006)	(0.008)	(0.005)
Male	0.077***	0.025***	0.059***
	(0.001)	(0.001)	(0.001)
$D_{jt} \times 1[t = 2019]$	-0.007	-0.023***	-0.014***
	(0.006)	(0.006)	(0.005)
$D_{jt} \times 1[t = 2018]$	-0.008**	-0.018***	-0.014***
	(0.004)	(0.005)	(0.003)
$D_{jt} \times 1[t = 2016]$	8.689e-04	-2.163e-04	-3.920e-04
	(0.005)	(0.005)	(0.004)
$D_{jt} \times 1[t = 2015]$	0.003	-0.010	-0.003
	(0.004)	(0.007)	(0.004)
$D_{jt} \times 1[t = 2014]$	0.008*	-0.019**	-0.003
	(0.005)	(0.007)	(0.004)
$D_{jt} \times$ Male	-0.014***	0.004	-0.011***
	(0.004)	(0.003)	(0.003)
$D_{jt} \times 1[t = 2019] \times$ Male	-0.008***	0.009***	1.987e-05
	(0.003)	(0.003)	(0.002)
$D_{jt} \times 1[t = 2018] \times$ Male	-0.003	0.010***	0.004*
	(0.002)	(0.003)	(0.002)
$D_{jt} \times 1[t = 2016] \times$ Male	0.006*	0.004	0.007***
	(0.003)	(0.003)	(0.003)
$D_{jt} \times 1[t = 2015] \times$ Male	0.002	0.006	0.005**
	(0.003)	(0.005)	(0.003)
$D_{jt} \times 1[t = 2014] \times$ Male	-0.002	0.004	0.003
	(0.003)	(0.006)	(0.003)
Equal work FE	✓	✓	✓
Industry FE	✓	✓	✓
CBA-year FE	✓	✓	✓
Dependent mean (all)	1.841	1.788	1.832
Dependent mean (untreated 2017)	1.791	1.707	1.784
N	4218524	2567474	7156399
R^2	0.903	0.874	0.891

Notes: The table above shows the 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.

A.6 Non-CBA workers

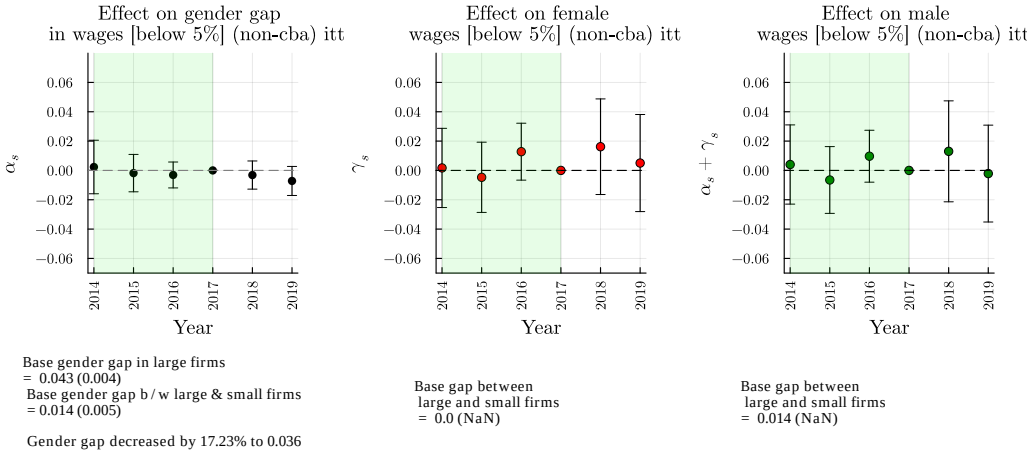
A.6.1 Non-CBA workers: Intent-to-Treat effects

Figure 22: ITT: Non-CBA Workers in firms above five percent of baseline gender wage gap



Notes: This figure plots the ITT estimates from the event study specification estimated on the sample of non-CBA workers in firms above five percent of baseline gender wage gap. We plot the ITT 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.

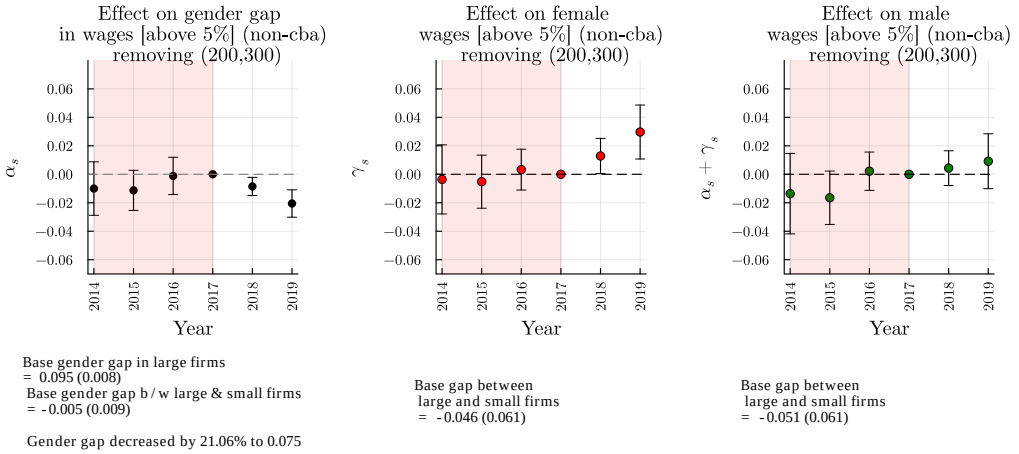
Figure 23: ITT: Non-CBA Workers in firms below five percent of baseline gender wage gap



Notes: This figure plots the ITT estimates from the event study specification estimated on the sample of non-CBA workers in firms below five percent of baseline gender wage gap. We plot the ITT 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.

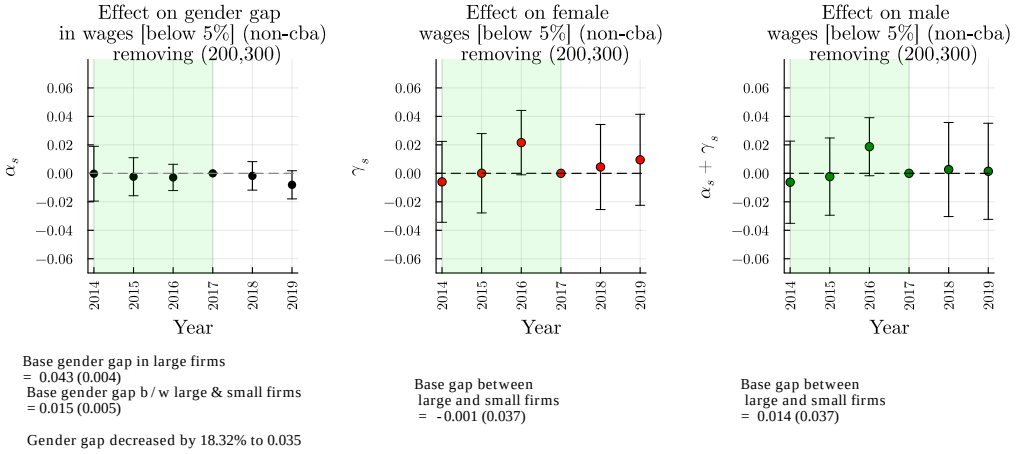
A.6.2 Non-CBA workers: Removing firms employing in between 200 and 300 workers

Figure 24: Non-CBA Workers in firms above five percent of baseline gender wage gap



Notes: This figure plots the ATT estimates from the event study specification estimated on the sample of non-CBA workers in firms above five percent of baseline gender wage gap after removing all firms that employ in between 200 and 300 workers. 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.

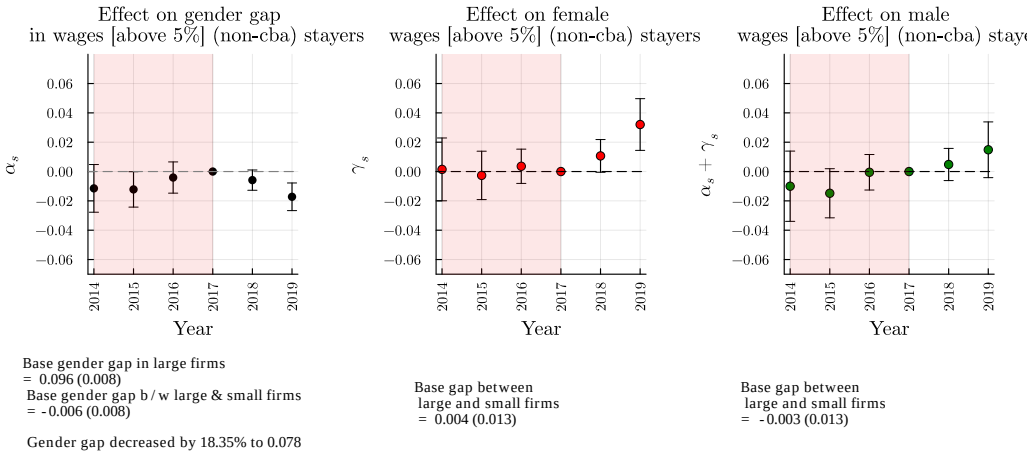
Figure 25: on-CBA Workers in firms below five percent of baseline gender wage gap



Notes: This figure plots the ATT estimates from the event study specification estimated on the sample of non-CBA workers in firms above five percent of baseline gender wage gap after removing all firms that employ in between 200 and 300 workers. 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.

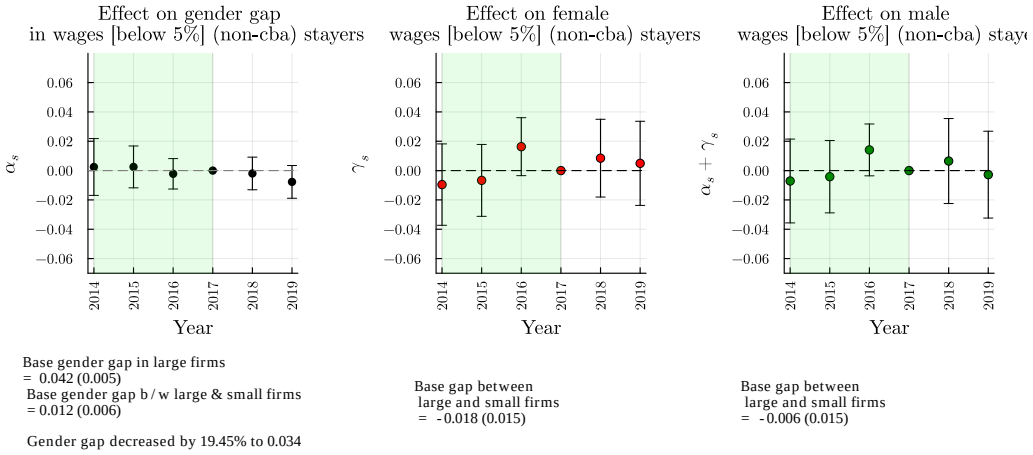
A.6.3 Non-CBA workers: Stayers

Figure 26: Stayers: Non-CBA Workers in firms above five percent of baseline gender wage gap



Notes: This figure plots the ATT estimates from the event study specification estimated on the sample of non-CBA workers in firms above five percent of baseline gender wage gap who did not switch firms in the sample period. 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.

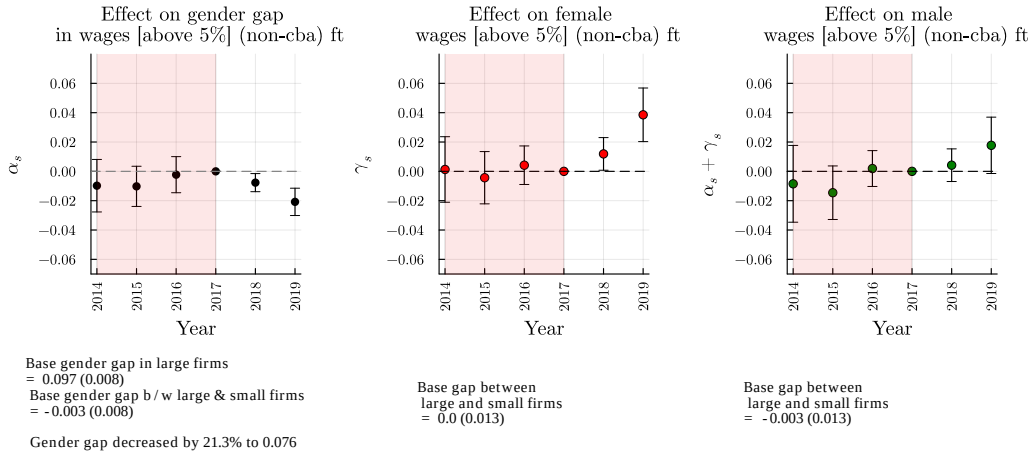
Figure 27: Stayers: Non-CBA Workers in firms below five percent of baseline gender wage gap



Notes: This figure plots the ATT estimates from the event study specification estimated on the sample ofnon-CBA workers in firms above five percent of baseline gender wage gap who did not switch firms in the sample period. 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.

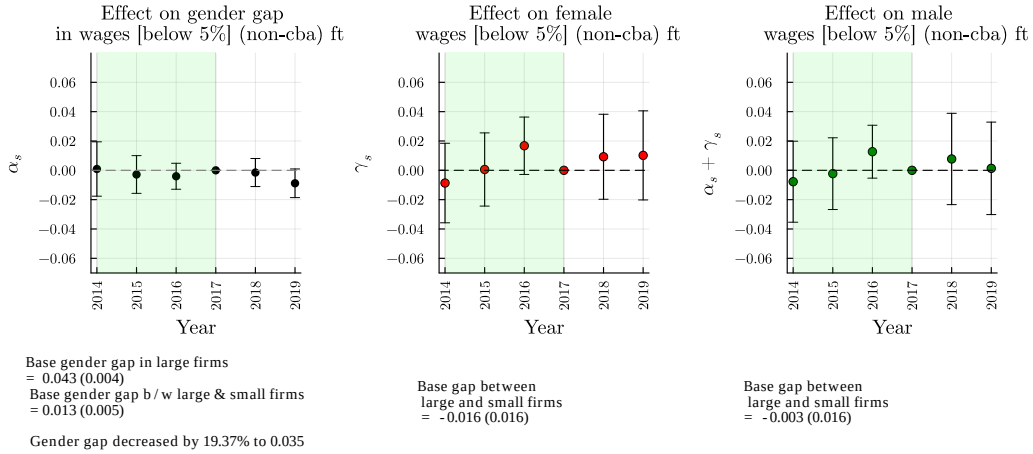
A.6.4 Non-CBA workers: Full-time

Figure 28: Full-time: Non-CBA workers in firms above five percent of baseline gender wage gap



Notes: This figure plots the ATT estimates from the event study specification estimated on the sample of non-CBA workers in firms above five percent of baseline gender wage gap who work full-time. 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 29: Full-time: Non-CBA workers in firms below five percent of baseline gender wage gap



Notes: This figure plots the ATT estimates from the event study specification estimated on the sample of non-CBA workers in firms above five percent of baseline gender wage gap who work full-time. 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.

Table 5: Estimates of the effect of the pay-equity law on log hourly wages of non-CBA workers

	Above 5% GWG: Non-CBA	Below 5% GWG: Non-CBA
	(1)	(1)
D_{jt}	3.360e-04	-0.016
	(0.013)	(0.016)
Male	0.099***	0.031***
	(0.003)	(0.003)
$D_{jt} \times 1[t = 2019]$	0.039***	0.010
	(0.009)	(0.015)
$D_{jt} \times 1[t = 2018]$	0.012**	0.009
	(0.006)	(0.015)
$D_{jt} \times 1[t = 2016]$	0.004	0.017*
	(0.007)	(0.010)
$D_{jt} \times 1[t = 2015]$	-0.004	5.680e-04
	(0.009)	(0.013)
$D_{jt} \times 1[t = 2014]$	0.001	-0.009
	(0.011)	(0.014)
$D_{jt} \times$ Male	-0.003	0.013**
	(0.008)	(0.005)
$D_{jt} \times 1[t = 2019] \times$ Male	-0.021***	-0.009*
	(0.005)	(0.005)
$D_{jt} \times 1[t = 2018] \times$ Male	-0.008**	-0.002
	(0.003)	(0.005)
$D_{jt} \times 1[t = 2016] \times$ Male	-0.002	-0.004
	(0.006)	(0.005)
$D_{jt} \times 1[t = 2015] \times$ Male	-0.010	-0.003
	(0.007)	(0.007)
$D_{jt} \times 1[t = 2014] \times$ Male	-0.010	9.013e-04
	(0.009)	(0.009)
Equal work FE	✓	✓
Year FE	✓	✓
Industry-year FE	✓	✓
Dependent mean (all)	1.975	1.757
Dependent mean (untreated 2017)	2.013	1.825
N	784291	532422
R^2	0.810	0.796

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 policy 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 Other Summary Statistics

In Table 6 we report the summary statistics by firm size threshold of 250 workers and by gender of workers. A few key differences to note are the gender gap in monthly hours worked is more than twice as high in large firms (9.92 hours) relative to small firms (3.82), but the monthly unadjusted raw gross real wage gap in large firms is 376.26 euros while it is 279.02 euros in small firm. These two lead to a higher gender wage gap in log hourly wages in small firms (0.16 log points) than in large firms (0.20 log points). Although the share full-time workers by gender in small firms is not too different (94.9% females and 97.1% males) the share of female full time workers (82.8%) is smaller than that of male full-time workers (93.3%). The share of female workers in small firms not covered by a CBA is 12%, that of male workers is 14%, while it is relatively similar by gender in large firms (19.7% of females and 18.5% of males).

Table 6: Summary statistics: By firm size and gender of workers in 2017

	Firm size below 250						Firm size above 250					
	Female			Male			Female			Male		
	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N
Female	1.000	0.000	354853	0.000	0.000	403130	1.000	0.000	372515	0.000	0.000	385682
Firm size > 250	0.000	0.000	354853	0.000	0.000	403130	1.000	0.000	372515	1.000	0.000	385682
Monthly hours	159.408	31.310	354853	163.234	28.095	403130	145.430	44.283	372515	156.382	36.054	385682
Monthly wage	990.096	740.935	354853	1269.114	1802.723	403130	1053.816	910.316	372515	1441.675	2119.513	385682
Log hourly wage	1.695	0.470	354853	1.862	0.534	403130	1.772	0.536	372515	1.985	0.600	385682
Age	40.231	10.917	354853	40.263	11.327	403130	39.334	11.041	372515	39.591	11.159	385682
Tenure at firm	8.077	9.227	354838	7.814	9.287	403096	7.836	9.037	372507	9.173	10.246	385673
Full-time	0.949	0.220	354853	0.971	0.168	403130	0.834	0.372	372515	0.939	0.239	385682
Not covered by CBA	0.120	0.325	354853	0.141	0.348	403130	0.219	0.414	372515	0.198	0.398	385682

Table 7: Summary statistics: By firm size and gender wage gap in 2017

	Firm size below 250						Firm size above 250					
	Above 5% GWG			Below 5% GWG			Above 5% GWG			Below 5% GWG		
	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N
Female	0.511	0.500	479708	0.394	0.489	278275	0.586	0.493	444485	0.357	0.479	313712
Firm size > 250	0.000	0.000	479708	0.000	0.000	278275	1.000	0.000	444485	1.000	0.000	313712
Monthly hours	162.202	28.261	479708	160.136	31.998	278275	151.721	38.715	444485	149.981	43.286	313712
Monthly wage	1205.427	1679.178	479708	1023.101	760.963	278275	1323.774	1981.701	444485	1148.164	1007.738	313712
Log hourly wage	1.818	0.540	479708	1.724	0.454	278275	1.912	0.607	444485	1.835	0.533	313712
Age	40.526	11.028	479708	39.769	11.306	278275	40.113	10.943	444485	38.547	11.259	313712
Tenure at firm	8.360	9.429	479673	7.208	8.914	278261	9.009	9.536	444473	7.817	9.872	313707
Full-time	0.965	0.183	479708	0.952	0.213	278275	0.883	0.321	444485	0.894	0.308	313712
Not covered by CBA	0.123	0.329	479708	0.145	0.352	278275	0.212	0.409	444485	0.203	0.402	313712

C.2 CBA coverage in the data

Workers who are not covered by a CBA comprise 17% of our sample. These workers tend to be more educated and younger in age. These workers are not particularly concentrated in any particular industry. CBA coverage primarily varies at the firm-level and in some cases within firms as well. In 2017, 11% of firms had none of their workers covered by any CBA, 84.5% of firms had all their workers covered under some CBA agreement, and the remaining 4.5% of firms had some workers covered by a CBA and some who are not. The distribution of CBA coverage also varies with firm size. Firms with less than 250 workers are more likely to have all workers covered under any CBA (at 85.71%) than firms with more than 250 workers (68.62%). Additionally, firms with more than 250 workers are more likely to have none of their workers covered under any CBA (at 14.07%) than firms with less than 250 workers (at 11.07%). While on the other hand, firms with more than 250 workers are more likely to have some of their workers covered under any CBA (at 17.36%) than firms with less than 250 workers (at 3.22%). In 98.3% of firms, workers under the same job-title have the same CBA. In the rest of the firms there is variation of CBAs within the same job-title. In particular, multi-plant firms tend to have region-specific CBAs for the same job-title across different plants. Also, larger firms tend to be covered by multiple CBAs.