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Pay Transparency, Wage Inequality, and the Role of Collective Bargaining*

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Abstract

We study the impacts of firm-level pay transparency policy under different collective bargaining settings. We show that firms react to pay transparency requirements by reducing gender gaps, as intended by policy makers, but also by hiring fewer workers to lower-paid positions, which results in overall wage compression. Gender gap reductions are concentrated in firms under collective bargaining agreements, while hiring reductions and wage compression are concentrated in firms less exposed to collective bargaining, where employers have more discretion over individual wages. These results suggest that the effects of pay transparency on workplace inequality depend on labor market institutions.

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Keywords: pay transparency, inequality, work satisfaction, discrimination, collective bargaining

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I. Introduction

Pay transparency has become a widely adopted policy tool to address wage discrimination in the workplace (OECD, 2023). By reducing information frictions, these policies increase pressure on employers to reduce unjustified pay disparities. At the same time, it may also induce potentially unanticipated changes in workforce composition through hiring and separations. For example, firms may strategically choose to change their hiring decisions to avoid conflicts over wages in the workplace, while workers may threaten to leave if they consider their pay to be unfair. Which margin dominates has consequences for who benefits from these policies and whether they reduce wage inequality. The institutional environment is also likely to play a central role in shaping these responses. In particular, collective bargaining affects firms' discretion over individual pay, workers' bargaining power, and overall wage inequality.¹ Despite the widespread adoption of pay transparency legislation across countries, there is limited evidence on how the institutional environment such as collective bargaining influences the effects of pay transparency on wage-setting and worker composition.

We provide new evidence on the effects of pay transparency using a reform that increased firm-level pay transparency in Finland. In particular, we examine how the effects of pay transparency operate through changes in worker composition, which groups of workers are most affected, and how these responses vary across bargaining regimes. Our identification strategy exploits a reform that required firms above a size threshold to disclose internal pay reports to employees. These reports contained information on the wages of men and women, as well as their distribution across job categories. Because the policy applied only to firms with at least 30 employees, we can compare the trajectories of otherwise similar workers and firms that were either subject to the policy or exempt from it. Importantly, we observe collective bargaining agreement (CBA) coverage by firm. The substantial variation in coverage allows us to provide novel evidence on how the effects of pay transparency differ across bargaining regimes.

We combine multiple administrative and survey datasets that allow us to observe hourly wages

¹For a review of the role of collective bargaining across countries, see Jäger et al. (2025)

from employer personnel records, worker characteristics and preferences, and worker flows across firms. Our detailed data enables us to distinguish between changes driven by wage-setting within firms and those arising from shifts in worker composition, as well as to assess how firm management and institutional features such as collective bargaining shape these responses. We also assess how pay transparency affects worker perceptions of discrimination and work satisfaction, and also evaluate its impact on firm performance.

To examine the salience of the reform to workers, we use survey data across workplaces. We show that after the implementation of the reform, a substantially larger fraction of workers in the exposed firms reported that their employers had prepared equality plans and had taken additional measures to address gender wage gaps in the workplace. This is an important signal that the policy induced changes in workplaces as intended and that it was salient to workers.

We then investigate changes in wage levels, within-firm wage distributions, and worker composition. We find that the policy increased hourly wages and compressed the wage distribution. Our difference-in-differences estimates suggest that the policy raised wages at the firm level by around 0.8 percentage points over the five years following the reform. This effect is primarily driven by increases at the bottom of the wage distribution, while we observe little impact at the top. As a result, the 90/10 wage percentile ratio decreases by around 1.2 percentage points. We show that changes in worker composition are a key explanation for these effects as firms reduce hiring of new workers, in particular into lower-paid positions. We find a 1.5 p.p. reduction in hiring to the lowest quartile of the firm's wage distribution. At the same time, we find no increase in average separation rates, nor any differential effects on separations across the firm's wage distribution.

Consistent with the main policy objective, we show that the pay transparency mandate was effective in narrowing gender wage gaps. Our preferred estimates, which account for worker composition, suggest that the within-firm gender wage gap narrowed by 0.9 to 1.1 percentage points. This corresponds to around 5.3% to 7.6% of the initial gender wage gap among control and treated firms prior to the policy. The narrowing of the wage gap can be primarily explained by increased promotion rates among women. The estimated effects on the narrowing of the gender wage gap

due to promotions (0.9 to 1 p.p.) are comparable to the effects on the average gender wage gap in models accounting for worker composition (0.9 to 1.1 p.p.), indicating promotions as a key mechanisms through which the policy improved the compensation of female workers and reduced inequality.

Next, we show that collective bargaining agreements play an important role in determining the effectiveness of pay transparency legislation and the margins of adjustment. We find that wage compression and hiring responses are most pronounced among firms outside the bargaining system. These firms are characterized by higher firm-level discretion in setting the pay for individual workers and by greater pre-policy wage dispersion in our data. In contrast, we find that the gender gap reductions are concentrated in organized firms. These firms have more compressed wage distributions and their baseline wage rates are typically determined by job category and are thus better known among workers. Therefore, the pay transparency policy is mostly informative about the gender disparity within job categories (horizontal inequality) as opposed to between job categories (vertical inequality) within these firms, as well as gender differences in assignments to job categories.

Finally, our results do not suggest significant trade-offs between pay transparency and worker satisfaction or firm productivity in the longer term. Using survey data, we show that the policy led to a decline in perceived discrimination among workers, while we find no evidence of decreased worker satisfaction three years after the introduction of the policy. We also find no evidence of adverse effects on firm productivity.

Our work is closely linked to the literature examining how pay transparency policies and labor market institutions interact. Evidence across countries and states suggests that the impact of pay transparency legislation on wages is muted in settings where collective bargaining rates are high (Cullen and Pakzad-Hurson, 2023). We contribute to this literature by using a design that allows us to explore the importance of differences in CBA coverage across firms. We show that collective bargaining is an important factor explaining whether firms respond to transparency requirements through adjustments in hiring or through wage setting and promotion of worker groups susceptible

to discrimination (in our setting, female workers).

Second, our study contributes to the literature examining the effects of pay transparency on wage distributions at the workplace. [Mas \(2017\)](#) provides evidence that salaries of highly paid city managers in California declined following public disclosure of their compensation. Our analysis complements and extends this strand of research by covering the whole within-employer wage distribution and by focusing on the private sector, where wage-setting incentives may differ from the public sector. [Gulyas et al. \(2023\)](#) and [Brütt and Yuan \(2022\)](#) analyze establishment- and firm-level wage variances but do not find significant effects. We show that pay transparency compresses the wage distribution and that these effects are driven by firms' adjustments along the hiring margin, with large negative effects on recruitment into lower-paid positions.

One distinctive feature of our study compared to previous work is that we examine whether the pay transparency reform is salient to workers and effectively implemented by employers. Examining this is important as, for instance, [Gulyas et al. \(2023\)](#) argue that the limited impact of the Austrian reform may be due to a lack of sufficient incentives for employers to provide information on and adjust wages. We show that the Finnish reform generates clear changes in workplace practices and worker awareness. This helps explain why we find effects on both the wage distributions and hiring.

Third, our work also contributes to the literature examining the impacts of transparency policies on gender wage gaps (e.g., [Castilla, 2015](#); [Maxwell et al., 2019](#); [Burn and Kettler, 2019](#); [Böheim and Gust, 2021](#); [Bennedsen et al., 2022](#); [Obloj and Zenger, 2022](#); [Brütt and Yuan, 2022](#); [Blundell et al., 2023](#); [Baker et al., 2023](#); [Gulyas et al., 2023](#)). Our estimated decrease of 0.9 to 1.1 p.p. in the gender wage gap in Finland represents a more moderate decline relative to previous studies, where the decrease ranges between 2 and 3 p.p. (in settings where the policy is found to be effective). Beyond the first-order effect on the gender wage gap, we are also able to show that there is a decline in perceived gender discrimination among workers, following the introduction of pay transparency. Furthermore, we show that the narrowing of the gender gap takes place primarily in firms within the collective bargaining system and that managers' gender and education are important determinants

of gender gap responses.

Fourth, our study is closely related to the literature on worker satisfaction, which shows that increasing awareness of pay inequality can lower morale and increase employee turnover in the short run ([Card et al., 2012](#); [Breza et al., 2018](#); [Dube et al., 2019](#)). In our setting, pay transparency does not lead to lower morale or higher separation rates within the firm in the long run. One possible explanation, supported by our results, is that greater transparency improved women’s career progression. Hiring fewer low-paid workers may also have helped mitigate potential adverse effects within the firm, which could explain why some firms responded to the policy at this margin.²

More broadly, our study is linked to the literature examining the role of firms in shaping wage distributions and gender inequality, which suggests that women tend to sort into firms that, on average, pay lower wages.³ Our results suggest that firms’ responses to transparency policies aiming to reduce gender wage gaps involve a significant adjustment through worker composition. In particular, our results highlight the role of firms’ hiring decisions in driving this sorting pattern.⁴

The paper is organized as follows. Section II describes the institutional setting and pay transparency policy in Finland. Section III provides details of the research design and data sources, and reports summary statistics. The results for wage distributions and sorting are presented in Section IV, while Section V provides results for gender inequality. Sections VI and VII examine the role of collective bargaining institutions and other mechanisms. Section VIII discusses how our estimates compare with the previous literature and Section IX concludes.

²Our results also speak to the literature that evaluates how much people care about their rank in the income distribution. Individuals are concerned about inequality in the groups that matter to them and it has been shown that the workplace is one of the most relevant groups for income comparisons ([Hvidberg et al., 2023](#); [Xu et al., 2025](#)). Thus, it may be somewhat surprising that we find no negative effects on either separation rates or employee morale in our context. On the other hand, firms’ quick reactions in reducing the gender wage gap and in adjusting the hiring margin to improve within-firm equality show that they are aware that revealing their employees’ relative income position may create dissatisfaction and lead to negative firm outcomes.

³See e.g., [Card et al., 2016](#); [Bruns, 2019](#); [Casarico and Lattanzio, 2024](#); [Morchio and Moser, 2024](#); [Palladino et al., 2025](#).

⁴In related studies on sorting, [Bamieh and Ziegler \(2025\)](#) find little evidence that mandatory minimum wage offers in job postings affect gender sorting into better-paying occupations, while [Roussille \(2024\)](#) finds that in the context of engineering jobs posted salaries in job ads reduced the gender-gap at the point of hiring. [Arnold et al. \(2025\)](#), in the context of US state-level transparency reforms for job postings, show that salary information is indeed more likely to appear in job postings and that wages of posted jobs significantly increase post-reform. [Skoda \(2022\)](#) also finds increases in the wages of newly hired workers, after a mandated reform to provide pay information in job ads.

II. Pay Transparency Reform and Institutional Setting

We study the consequences of firm-level pay transparency legislation that was introduced through amendments to the Gender Equality Act in 2005 (HE 194/2004). Prior to the reform, all employers were obliged to promote equality in workplaces and employers with at least 30 employees had to prepare an equality plan on how to promote gender equality in the workplace, but specific actions to be implemented were not defined. The new law in 2005 obligated employers with at least 30 employees to include in their equality plans a wage report. The wage report needed to contain a breakdown of the placement of women and men across different job categories and a report of the wages of women and men, including gender pay gaps by job categories.

Information in wage reports allowed employees to learn about their position in the firm's wage distribution and to uncover potential wage disparities – both among workers in similar job categories (horizontal inequality) and across different job categories (vertical inequality).⁵ The reporting also reveals information on the number of workers by job category and gender, which provides further information on gender distributions across job categories and pay levels. The purpose of the report is to uncover potential gender discrimination in wages, both within and across job categories, as well as disparities in realized career progression between men and women.

There are both legal and non-legal reasons why firms might want to comply with the pay transparency requirement. The Ombudsman for Equality had the authority to impose a conditional fine on employers who neglect their gender equality obligations and to set a deadline by which the equality plan must be prepared. If necessary, the employer could be compelled to comply under the threat of the conditional fine taking effect.⁶ An employer who violated the prohibition of discrimination was also liable to pay compensation to the discriminated party. Moreover, firms may also want to avoid reputational concerns from not complying with the regulation.

Wage setting institutions in Finland. In Finland, collective agreements between employer and

⁵Appendix Table A1 shows a wage report template.

⁶In Finland, conditional fines (*uhkasakko*) are administrative sanctions designed to compel compliance with legal obligations. The amount of such fines is determined based on the nature and extent of the obligation and the financial capacity of the obligated party (Act on Conditional Fines 1113/1990).

employee associations have been prevalent, occasionally replaced by industry-level agreements (Uusitalo and Vartiainen, 2009; Calmfors, 2025). The agreements set occupation- and job-specific wage floors and address other issues related to labor relations, including working conditions, work hours, and amenities. However, firms are essentially unrestricted in paying wage premiums above the agreed wage floors. Collective bargaining agreements covered around 90 percent of all workers and 85 percent of private sector workers in 2008.⁷

III. Research Design and Data

III.A. Empirical Setting

In our empirical analysis, we use variation in pay transparency across firms due to the 2005 policy change that required firms with at least 30 employees to report average wages by job category and gender to their employees. In particular, we compare firms that were mandated to report wages to their employees (≥ 30 employees) to firms that were not (< 30 employees). Our baseline model is the following difference-in-differences or event-study specification:

$$y_{ijt} = \sum_{\substack{k=-5 \\ k \neq -1}}^5 \beta_k \mathbb{1}_{(t-2005=k)} \mathbb{1}(\# \text{Emp}_{j,2004} \geq 30) + \theta_j + \lambda_t + \gamma_i + \epsilon_{ijt}, \quad (1)$$

where y_{ijt} is our outcome of interest for worker i in firm j at time t ; $\mathbb{1}_{(t-2005=k)}$ are event-time dummies, k indexing years from the reform; $\mathbb{1}(\# \text{Emp}_{j,2004} \geq 30)$ is an indicator for treatment status, based on employment prior to the reform; λ_t absorbs time trends; and θ_j are firm-level fixed effects, which control for time-invariant components in firm-level characteristics, such as in working conditions or quality of management. In addition, to understand the role of worker composition, we also consider specifications that include worker-level fixed effects γ_i . We cluster standard errors at the firm level.

Our research design compares the development of smaller and larger firms across the firm size cutoff. This approach is commonly used in recent studies evaluating the effects of a variety of firm

⁷Työehtosopimusten kattavuus Suomessa vuonna 2008, Lasse Ahlainen, MEE Publications 28/2011.

policies (e.g., [Guceri and Liu, 2019](#); [Benzarti et al., 2020](#); [Bennedsen et al., 2022](#); [Blundell et al., 2023](#); [Gulyas et al., 2023](#)). It relies on the assumption that, in the absence of the reform, firms above and below the 30-employee cutoff would have experienced parallel trends in outcomes. To ensure that parallel trends are a reasonable assumption in our context, we constrain our baseline sample to firms closer to the cutoff. More specifically, we use firms with 15-29 employees as the control group and firms with 30-55 employees as the treatment group. We show that outcomes in these two groups, both at the worker and firm level, develop similarly prior to the reform, lending credibility to the empirical strategy.

III.B. Data Sources

To measure hourly wages, worker mobility and promotions, we use data from the annual Structure of Earnings Survey (SES) covering the years 2000 through 2010. The data include more than 50% of employees in Finland, excluding some of the smallest employers with less than 10 employees. A major benefit of the data is that they include worker-level information on salaries and work hours, allowing us to measure hourly wages.⁸ Another advantage is that surveyed employers provide information on all of their employees, which allows us to accurately measure within-firm wage distributions. Moreover, the data allow us to identify the firms where workers are employed and follow their transitions across firms over time. We also use information on workers' detailed occupation codes to measure promotions. We rank occupations by assigning to each 5-digit occupation the average hourly wage in the pre-reform period. This measure allows us to track occupational mobility in terms of expected wages.

For firms that are part of an employer organization, the wage data also provides information on whether each employee is covered by a collective bargaining agreement (CBA). This information allows us to construct a firm-level indicator of CBA status equal to one if a firm has any employees with a CBA code, and zero otherwise. We refer to firms with any employees with CBA codes as organized, and firms without as unorganized. The advantage of these data is that we observe for

⁸Hourly wages are deflated by the consumer price index and winsorized annually at the 99.5th percentile of the distribution to reduce the influence of outliers.

all employers whether they are operating within the collective bargaining system or not.⁹

To measure firm size and performance, we use data from the Firm Registry and Financial Statements (FS) panels. These data cover around 95-98% of all firms in Finland during our sample period. We first use these data to identify the number of employees in each firm, which determines their liability to introduce pay transparency policies. Second, we use measures of firm productivity as outcomes in a firm-level analysis. Our primary measures of firm productivity are sales and value added per worker, which can be constructed for most firms. Our baseline approach is to use all firms in the panel data, but we replicate our results also with firms whose workers appear in the wage data.

To examine the salience of the policy to workers and its impacts on worker satisfaction and perceived discrimination, we use the Quality of Working Life Survey (QWLS). The survey targets 15-64 year old employees who work at least five hours per week, and it is conducted every five years. The sample contains more than 4000 respondents per wave. We focus on two waves in 2003 and 2008 which contain questions that directly relate to firm-level measures relevant for pay transparency policies. In particular, the respondents are asked i) whether their employer has prepared an equality plan and ii) whether their employer has implemented measures to address gender wage gaps. In addition, to measure worker satisfaction, the survey asks how satisfied the respondents are with their current job. The survey also includes personal identifiers for each respondent which allows us to link them to their employer.

III.C. Sample Selection and Summary Statistics

For the difference-in-differences analysis at the 30-employee cutoff, we construct two samples. In the first sample, the treatment group includes firms that had 30-55 employees and the control group

⁹We note that some workers in unorganized firms may be covered by CBAs due to their general applicability, but we do not observe this. Thus, in the sample of unorganized firms, all workers do not necessarily fall outside CBAs, but they are more likely to do so. In our sample, 74 percent of workers are observed to be covered by CBAs in 2004, while estimates published by the Ministry of Economic Affairs and Employment suggest that overall 85 percent of private sector workers are covered by CBAs, though these estimates also include uncertainty (*Työehtosopimusten kattavuus Suomessa vuonna 2008*, Lasse Ahlainen, MEE Publications 28/2011). Smaller firms are over-represented in our sample, which likely explains this difference.

includes those with 15-29 employees in 2004, one year before the introduction of the policy.¹⁰ We then follow these firms over the years around the policy reform to assess effects at the firm level. We refer to this as the "*Firm Sample*". This sample allows us to examine effects on firm-level wage distributions and to assess the role of worker composition in explaining the effects.

Second, we construct a sample of all workers who were employed either in a treatment or a control group firm in 2004 and follow their trajectories over time, irrespective of their employer in other years. We refer to this as the "*Worker Sample*". This sample provides us with an alternative way to control for worker-level sorting and to analyze the extent to which exposed workers switch between firms and occupations. In specifications using this sample, we always include firm fixed effects based on the worker's initial 2004 employer firm.¹¹

Summary statistics for the year 2004 are reported in Appendix Table A2.¹² Firms in the treatment group are, on average, larger and have more employees than firms in the control group by construction. Employees in the treatment group firms earn 16.45 euros per hour, while employees in the control group earn 15.81 euros per hour, on average. Treated firms have a higher proportion of employees with hourly contracts (28% vs. 23%), while control firms have a higher proportion of employees on monthly contracts (77% vs. 72%). We also observe more men in the treatment group than in the control group firms (51% vs. 46%).

III.D. Worker Exposure to the Reform and Perceptions of Anti-Discrimination Measures

A key question for the effectiveness of the pay transparency reform is whether the potential firm-level measures induced by it were salient to employees. Appendix Figure A1 shows that after the introduction of the policy the fraction of workers reporting that equality plans had been prepared at their workplace more than doubled from 12% to 26%, and the fraction of workers reporting

¹⁰To limit the influence of outliers with large changes in the number of employees before the policy reform, we restrict our baseline sample to firms with an average number of employees between 15 and 55 in the pre-policy period 2000-2004. We also show results for alternative definitions of the treatment and control groups, discussed in Sections IV and V.

¹¹This allows us to track labor market trajectories of workers in this sample over time, irrespective of the firm they work at after 2004.

¹²We note that the Firm and Worker Samples include the same firms and workers in 2004.

measures to reduce gender wage gaps rose by 40 percent, from 20% to 28% . Table 1 shows that these changes are driven by workers in firms from the treatment group (≥ 30 employees) rather than by workers in firms from the control group (< 30 employees).

Appendix Figure A1 also reports the extent to which firms in the treatment and control groups are subject to the pay transparency mandate over time. In the first year the policy was in effect, nearly all firms in the treatment group were subject to the mandate (96 percent) while only 10 percent of firms in the control group were.¹³ This gap converges somewhat over time as firms' sizes change, leading to a change in some firms' exposure to the policy over the study period. Yet, the gap remains substantial at around 60 p.p., even five years after the reform.¹⁴

IV. The Impact of Pay Transparency on Wages and Worker Composition

In this section, we present our main results on the impacts of the pay transparency reform on wages and the wage distribution. We also study how the policy affects worker composition and how hiring and separations contribute to the estimated wage effects.

IV.A. Wages and the Wage Distribution

Panel A in Figure 1 shows the effect of the pay transparency mandate on average wages based on our baseline specification in Eq. 1. Wage trajectories in the treatment and control groups are similar prior to the policy reform, lending credibility to the parallel trends assumption. We find that average wages in the treatment group gradually increase compared to the control group after the reform. By 2010, average hourly wages are 1.3 p.p. higher in the treatment group than in the control group firms. The difference-in-differences estimate pooling the pre- and post-reform years is 0.8 p.p. (Column 1 in Panel A of Table 2).

Panel B of Figure 1 shows that the positive impact on average wages is driven by a significant increase at the bottom of the firm-level wage distribution while there is little change at the top.

¹³The difference-in-differences estimate of the difference is 82 p.p.

¹⁴Appendix Figure A2 shows that firms appear not to have strategically adjusted their employee counts, indicated by smooth distributions of the number of employees both before and after the policy.

The 10th percentile increases by 1.45 p.p. while the effect on the 90th percentile is 0.18 p.p. and not statistically significant (Columns 2 and 3 in Panel A of Table 2). The reduction in the 90/10 percentile ratio is 1.2 p.p. (Column 1 in Panel B of Table 2). Thus the reform led to a compression of the wage distribution, with most of the compression emerging at the bottom of the distribution: the 50/10 percentile wage ratio drops by 0.85 p.p. while the 90/50 ratio drops by 0.36 p.p.¹⁵

We next decompose within-firm wage dispersion, measured by standard deviation, into its within- and between-occupation components, which allows us to assess the impacts of the reform on horizontal and vertical inequality. Since firms were required to report wages and inequality both within and across occupational categories, there could plausibly be changes along both margins. Panel C of Table 2 shows that 40 percent of the decrease in wage variance is driven by compression within 1-digit occupations and 60 percent by compression across occupations, suggesting that both horizontal and vertical inequality declined within firms. Consistent with firms and workers responding to the disclosed information, the wage effect is larger in firms with higher pre-policy inequality, although estimated with relatively low precision (Column 1 in Appendix Table A3). The impacts are more pronounced in firms with higher initial horizontal inequality, while they vary little with the level of initial vertical inequality (Columns 2 and 3).

Taken together, these results suggest that the pay transparency mandate led to an increase in wages and compression of the wage distribution within firms. While wages compress roughly at the same rate within and between occupations, wages rise more in firms with higher initial within-occupation inequality, which the policy targeted primarily.

IV.B. Worker Composition

We next assess the extent to which the effects on wages and firm-level wage distributions are driven by worker composition. The results are shown in Figure 2. First, we add worker fixed effects to our baseline model in Eq. 1. This controls for within-firm changes in the composition of workers by unobserved characteristics affecting wages. This flattens the pattern of estimates,

¹⁵In Panels A and B of Figure A3, we show event-study graphs for the 90/10 ratio, indicating that trends between the treatment and control groups are very similar prior to the reform.

with the pooled difference-in-differences estimate becoming statistically insignificant at 0.1 p.p., which is 0.7 p.p. lower than in our baseline model that does not control for worker fixed effects (Columns 1 and 2 in Appendix Table A4). Second, we use the Worker Sample, following workers who were initially employed in the treatment and control firms in 2004, irrespective of their current employer. This gives very similar results to the worker fixed effects model for the baseline sample, with consistent zero effects and pooled difference-in-differences point estimates of zero both with and without worker fixed effects (Columns 3 and 4 in Appendix Table A4). Together, these results indicate that the positive effect on mean wage at the firm level is to a large extent driven by worker composition.¹⁶

To better understand the drivers of these compositional changes, we next examine worker separations and hiring on average and across the wage distribution.

Worker separations. To measure separations, we follow workers who were initially employed in the sample firms (Worker Sample) and examine their likelihood to move to other firms over time. Panel A of Figure 3 shows no evidence that pay transparency increases separation rates. On the contrary, we find that separation rates decline by 2 p.p. immediately after the policy reform and later converge back to pre-policy levels.

Worker separations can influence the wage structure if they are heterogeneous across the wage distribution. However, in Panel B of Figure 3, we show that the estimates for worker separations are small, statistically insignificant, and similar in magnitude across wage deciles, based on workers' position in the wage distribution of their employer firm before the reform in 2004. The point estimates are mainly negative, meaning fewer exits across the wage distribution, and their magnitude is smallest for the workers below the 10th and above 90th percentiles. Overall, these results suggest that worker exits are not the main mechanism for the observed changes in the wage distributions following the introduction of pay transparency.

Hiring. Firm responses to pay transparency requirements can also operate through hiring. The hiring margin may be particularly important because firms may have more control over hiring than

¹⁶In Appendix Table A5, we show that the results are very similar for both models when we use alternative definitions of treatment and control groups.

separations, which are limited by employment protection.¹⁷

To assess the impacts on hiring, we follow firms in the treatment and control groups over time and measure the fraction of new hires relative to pre-policy employment. Panel C of Figure 3 shows the results. We find that treated firms hire fewer new workers than the control firms, with a persistent negative effect after the policy change. The difference-in-differences estimate for the fraction of new hires relative to employment in 2004 is -2.4 p.p. (Column 4 in Appendix Table A6). Panel D of Figure 3 displays the effects on hiring by the firm's 2004 wage quartiles, showing that treated firms hire fewer workers into the lower-paid positions compared to the control firms. The effects are negative and statistically significant for the other quartiles as well, but the point estimate is two to three times larger for the bottom quartile (1.5 p.p. for the bottom quartile and 0.5 p.p. for the remaining three quartiles, on average). We also find a reduction in employment, largely explained by fewer new hires (Appendix Table A6). Overall, the results suggest that the main mechanisms through which firms' wage distributions shift and wages compress is the disproportionate reduction in hiring to lower-paid positions.

V. The Impact of Pay Transparency on Gender Equality

Since the new pay transparency requirements were motivated by concerns about gender inequality and included specific provisions to reduce unjustified wage gaps, we next examine their effects separately on male and female wages, and on gender wage disparities. We further investigate the potential channels through which the transparency mandate may have operated. In particular, we assess whether there were differential impacts on promotions, hiring, and separations by gender.

V.A. Gender Wage Gap

We start by presenting results for wage responses by gender, without controlling for worker composition. Panel A of Figure 4 shows that average wages increase for both male and female workers in treatment group firms compared to the control group after the reform. Women's wages increase

¹⁷In Finland, firms can terminate permanent job contracts if they need to downsize production, and the separation process is relatively strictly regulated by law.

more than men's, although the estimates are less precisely estimated for men. For women, wages are 0.9 p.p. ($p < 0.01$) higher in treatment group firms compared to the control group, while the effect on men's wages is 0.4 p.p. and statistically insignificant ($p = 0.30$) (Columns 1 and 2 in Panel A of Table 3). The difference-in-differences point estimate indicates a decline of 0.3 p.p. in the gender wage gap, but this effect is not statistically significant at conventional levels.

Panel B of Figure 4 shows wage effects by wage decile, separately for men and women. We find a clear negative gradient for both groups. Women's wage increases are driven by the bottom of the wage distribution while there are no changes at the top. For men, the gradient is qualitatively similar, but steeper. The reduction in the 90/10 wage percentile ratio is 1.0 p.p. for women and 2.1 p.p. for men (Panels B and C of Appendix Table A7), indicating that wages compress among both female and male workers, but more so among men.¹⁸

In section IV.A., we showed that the effects on firm-level wage distributions are to a significant degree driven by sorting rather than wage setting. To account for changes in worker composition and to isolate the effect of wage setting on gender gaps, Panels C and D in Figure 4 control for worker fixed effects. These results show that female wages rose and male wages fell after the introduction of the policy, leading to a significant reduction in the gender wage gap. The difference-in-differences estimates suggest that women's wages increase by 0.5 p.p. and men's wages decrease by 0.4 p.p., resulting in a 0.9 p.p. decline in the gender wage gap (Columns 4-6 in Panel A of Table 3). Panel B shows that the reduction in the gender wage gap is very similar when we use the Worker Sample, following workers initially in the control and treatment firms (0.4 p.p. increase for women and 0.7 p.p. decrease for men, resulting in a 1.1 p.p. decline in the gap, columns 4-6 in Panel B of Table 3) or if we use alternative definitions of the treatment and control groups (Appendix Tables A8 and A9). The magnitude of these estimates corresponds to around 5.3% to 7.6% of the initial gender wage gap within firms in 2004, and to around 53% to 71% percent of the reduction in it between 2004 and 2010.¹⁹

¹⁸Appendix Figure A3 shows the corresponding event-study graphs, supporting the parallel trends assumption.

¹⁹Appendix Figure A4 shows economy-wide gender wage gaps over the sample period, calculated from the full wage survey data (including also all workers outside our firm-size window). We note that the pay transparency reform affected the majority of workers in this data because employers with at least 30 employees cover around 90% of the

To explore the source of differences between estimates that do and do not control for worker composition, we next evaluate the role of separations and hiring. Panels A and B of Appendix Figure A5 shows results for separations by gender around the reform and across wage deciles, based on firms' pre-policy wage distributions in 2004. Separation rates for men decline temporarily after the reform, but the effects do not persist in the long term, and there is little impact on separations among women, on average.²⁰ For most deciles, the pattern of estimates is very similar, with women no more likely to exit the firm than men. However, at the lowest decile, the estimate for women is positive and statistically significant, suggesting that the lowest-paid women are more likely to leave the firm than men when pay transparency is introduced. Separations of the lowest-paid women also partly explain the differences in the gender wage gap estimates between specifications that condition on sorting and those that do not.

Panels C and D of Appendix Figure A5 show the results for hiring by gender around the reform and across wage deciles. Hiring of both new male and female workers declines, with no statistically significant effect heterogeneity by gender.²¹ Moreover, the reduction in hiring is very similar for both men and women across the wage distribution, emerging to a large extent in the bottom quartile.

While the differences in the estimates by gender for the hiring and separation margins are statistically insignificant, the combination of lower point estimate for separations among men and lower point estimate for hiring among women translates into a larger drop in the number of female workers (Appendix Table A6 Columns 2 and 3).

In Appendix Figure A6, we show that the reduction in the gender gap is driven by workers in managerial and professional occupations. These white-collar jobs are typically characterized by salary-based contracts, larger pre-policy gender wage gaps, and higher degree of subjective evaluation in wage setting. In contrast, we see no changes in gender wage gaps for routine-task-intensive and male-dominated blue-collar occupations.

total employment in the data.

²⁰Appendix Table A10 shows that while the difference-in-differences point estimates are insignificant across samples, their magnitudes suggest a slightly larger reduction in separation rates for men.

²¹Columns 5 and 6 in Appendix Table A6 show that hiring of women decreases by 1.5 p.p. and of men by 1.1 p.p. These estimates are not statistically different from each other.

To further explore the adjustment of the gender wage gap within and between job categories, we expand our analysis to larger firms with 30 to 250 employees in Appendix B. In this sample, all firms are affected by the transparency policy, but for firms with higher pre-policy gender wage gaps, the new transparency mandate can be expected to be more binding than for firms with lower initial gender wage gaps. The benefit of using larger firms is that it allows us to explore gender wage gaps not just within firms but across job categories. This is supported by our analysis showing that horizontal firm-level inequality predicts wage adjustments after the reform (Appendix Table A3). This sample also allows us to examine whether the difference-in-differences results estimated across the 30-employee cutoff generalize to a broader firm population exposed to the policy. We find a 0.7 p.p. decline in the gender wage gap in a firm with 10 p.p. higher initial gender inequality, driven by a positive impact on female wages and a negative impact on male wages. Overall, the results for these larger firms are consistent with our baseline results for gender wage gaps. These estimates also suggest that the gender gap results estimated across the 30-employee cutoff extend to the wider population of firms exposed to the policy, some of which are outside the size window of our baseline sample.

V.B. Promotions and the Gender Wage Gap

To further understand the mechanism behind the reductions in gender wage gaps, we next turn to promotions. To measure promotions, we link individuals' current occupation codes to expected wages using average hourly wage by detailed 5-digit occupation in the pre-policy year 2004. Results in Panel E of Table 3 show that, after the reform, women initially working in treated firms are promoted to positions with higher expected wages compared to women initially in control firms, while there is no such effect for men. The estimates are very similar across specifications with and without worker fixed effects. The difference-in-differences estimates suggest that promotions due to the reform induced a 0.8 p.p. wage gain for women, while the corresponding effect for men is small and negative.²² Together, these effects led to a 0.9 to 1 p.p. reduction in the gender gap in

²²We show event-study estimates for promotions by gender and the gender gap in Appendix Figure A7.

promotions (Columns 3 and 6), which is around 9-10 percent of the pre-policy within-firm gender gap.

Overall, the results for promotions suggest that the wage convergence between women and men induced by the reform is to a significant degree driven by promotions of female workers to higher-paid positions. The estimated effects on the narrowing of the gender wage gap due to promotions (0.9 to 1 p.p.) are comparable to the effects on the average gender wage gap in models accounting for worker composition (0.9 to 1.1 p.p.), indicating promotions as a key mechanisms through which the policy improved the compensation of female workers and reduced inequality.

V.C. Magnitude of the Gender Wage Gap Reduction

To interpret the magnitude of our results, we compare our estimated effect on the gender wage gap to changes in the economy-wide gender wage gap during the sample period. Prior to the reform in 2004, the raw gap was around 21 percent and the within-firm gap was around 17 percent (Appendix Figure A4).²³ After the reform, we see a substantial and continuous decline in the within-firm gap of 1.7 p.p. between 2004 and 2010, while the raw gap increases by 0.9 p.p. Specifications in Table 3 conditioning on worker composition suggest that the gender wage gap declined by 0.9 to 1.1 p.p. among the treatment group firms due to the reform. These effects correspond to 5.3% to 7.6% of the initial gender wage gap within firms in 2004, and between 53% to 71% percent of the reduction in it between 2004 and 2010.

VI. The Role of Labor Market Institutions

The consequences of pay transparency can critically depend on the bargaining setting between employers and employees (Cullen and Pakzad-Hurson, 2023; Cullen, 2024). In this section, we leverage firm-level information about collective bargaining agreements (CBA) to test how the effects of pay transparency depend on the bargaining setting.

²³Economy-wide gender wage gaps are calculated from the full wage survey data, including all workers in the survey, also those who are working for firms outside the employee range used to define our baseline sample. The pre-policy gender wage gap is 16% in the Firm and Worker samples, which are limited to firms with 15–55 employees in 2000-2004, on average.

Panels A and B in Figure 5 show the effects by CBA coverage for wages and the wage distribution, without controlling for worker composition. Interestingly, we find that the positive effect on wages and the compression of the bottom half of the wage distribution are driven largely by unorganized firms. The difference-in-differences estimates suggest that the wage increases by 2.5 p.p. among unorganized firms and only by 0.4 p.p. among the organized firms (Column 1 in Table 5). When we control for worker composition with worker fixed effects, the magnitude of the estimates declines and become statistically insignificant (0.7 p.p. for unorganized and -0.1 p.p. for organized in Column 5), suggesting that changes in worker composition are important among the unorganized firms.

Panels C and D in Figure 5 provide evidence on the link between bargaining institutions and the sorting mechanism. Both the reduction in hiring into lower-paid positions and the decrease in separation rates are driven by the unorganized firms. In particular, the drop in hiring into the bottom wage quartile of unorganised firms is -4.5 p.p. while it is -1.1 p.p. for CBA firms. Thus, the negative impact on hiring that we observe at the bottom of the wage distribution is three times larger for unorganised relative to CBA firms. In contrast, separations in unorganized firms are decreasing across the wage distribution by an average of -5.3 p.p., but remain flat and statistically insignificant in CBA firms.

Table 5 shows that there is also important heterogeneity in the wage effects by gender depending on whether the employer is organized or not. In particular, we find that gender wage gaps decline in the organized firms but not in the unorganized firms: the gaps decline 2.5 p.p. more among the organized firms than among the unorganized firms (Column 4), and this difference remains at 1.6 p.p. when we add worker fixed effects (Column 8), with both coefficients being significant at the 10% level. Moreover, for the unorganized firms, the estimated effect on the gender gap is relatively small, positive, and statistically insignificant (0.3 p.p. in the model controlling for worker fixed effects).²⁴

These results are in line with the view that labor market institutions matter for the effective-

²⁴Appendix Table A11 shows that the estimates on the gender wage gap are similar in the Worker Sample where we follow workers rather than firms.

ness of pay transparency policies. Our analysis uncovers an unintended consequence of the policy, whereby firms outside the collective bargaining institutions respond to more stringent pay transparency requirements – and the demands to correct unjust wage gaps within the firm – by recruiting fewer lower-paid employees. This response might be partly explained by higher wage dispersion and more individual-level wage bargaining within firms not bounded by CBA wage floors. At the same time, firms within the collective bargaining system seem to respond to the policy by reducing gender wage gaps within the firm, despite the fact that they have smaller gender gaps to begin with, while their responses at the hiring and separations margins are relatively small. One interpretation of our results is that pay transparency policies reduce gender wage gaps most effectively when both sides, the employees and employers, are organized.

VII. Additional Results and Mechanisms

In this section, we discuss the effects of the policy on worker satisfaction and beliefs about discrimination, the role of managers in shaping responses to the pay transparency mandate, and the impacts on firm productivity.

VII.A. Perceived Discrimination and Worker Satisfaction

Table 4 shows the effect of the reform on perceived discrimination in wage setting and hiring against women (Panel A) and men (Panel B), based on data from the Quality of Working Life Survey. Baseline pre-policy perceived discrimination against women is higher than against men (0.068 vs. 0.018), which is consistent with the pre-existing gender wage gaps. Following the reform, women in exposed firms are less likely to report discrimination against women (Column 4 in Panel A), while men are more likely to report it (Column 3 in Panel A). Overall, the gender gap in reporting discrimination against women significantly decreased after the reform in affected firms by 6.6 p.p., driven by a 4.5 p.p. decrease among women and a 2.1 p.p. increase among men. These results are in line with the idea that among women, the policy reduced perceived inequality because it reduced gender wage gaps and increased promotions of female workers, while among

men, it may have increased the salience and awareness of gender inequality. We find no evidence of perceived discrimination against men (Panel B) or work satisfaction (Panel C) being significantly affected by the reform, although wage growth was slower among men.

VII.B. Firm Productivity

From the firm’s point of view, pay transparency may reduce overall productivity by making it harder to compensate for hard-to-observe effort or to attract talent. At the same time, learning about vertical inequality within the firm can increase effort and productivity among the lower-paid workers (Cullen and Perez-Truglia, 2022; Deserranno et al., 2025). Moreover, pay transparency may reduce effort by lowering worker morale, although this does not appear to be a key mechanism in our setting.

To test the impact of the policy on productivity, we estimate event-study models for sales and value added per worker at the firm level. Figure 6 shows that, prior to the reform, firms in the treatment and control groups are on similar trends, and we see no significant changes in either measure after the reform. The difference-in-differences point estimates are 1.3 p.p. for sales per worker and -1.1 p.p. for value added per worker in the Firm Register - FS sample (Columns 1 and 3 in Panel A of Appendix Table A12).²⁵ The results are similar, although less precise, with around 70% larger standard errors, when the sample is restricted to firms observed in the Wage Survey data (Panel B). These results suggest limited firm-level productivity effects from pay transparency legislation, even though it narrowed gender wage gaps and affected wage distributions and firms’ hiring decisions.

VII.C. The Role of Managers

Managers’ backgrounds and attitudes can play an important role in wage setting and gender equality (e.g., Acemoglu et al., 2022; Ronchi and Smith, 2024). In this section, we examine whether managers’ backgrounds affect the degree to which pay transparency policies translate into lower

²⁵The results are very similar when we control for industry-year fixed effects that capture differential trends in productivity across industries.

inequality. We estimate the effects of the policy by the gender and educational composition of managers in the firm, using as outcomes wages of workers who were in non-managerial positions prior to the policy. In our data, 22 percent of managers are women and 15 percent have college degrees.

Table 6 shows the estimates of the impact of the reform by the share of female and highly-educated managers. In Panel A, the estimated effect on wages for all workers is larger in firms with a larger share of female managers, although the coefficient on the interaction term is not statistically significant (Columns 1 and 5). While managers' gender matters little for women's wage setting (Columns 2 and 6), male managers tend to reduce the wages of male workers when pay transparency policies are implemented, whereas female managers appear to shield men from downward wage pressures (Columns 3 and 7); the point estimate of the response for male wages is negative among male managers while it is positive among female managers (in Column 7, -0.012 vs. 0.010). As a result, the reductions in gender gaps are driven by responses among male managers (-0.015 , $p\text{-value} < 0.05$). While the gender gap was somewhat smaller prior to the policy in firms with a larger share of female managers, which might have reduced the pressure to equalize wages, this difference does not explain the opposite responses by male and female managers to stricter pay transparency requirements in terms of male wages.²⁶

In Panel B, we present similar estimates for the share of managers with a higher education degree. We find that wage effects tend to increase with managers' education (Columns 1 and 5). This effect is now entirely driven by wages of female workers (Columns 2 and 6): less-educated managers adjust female wages only a little, with small, negative, and insignificant point estimate, while highly-educated managers increase wages of female workers by 2.6 p.p. more than less-educated managers.²⁷

The finding that male managers respond to the policy by reducing gender wage gaps is reas-

²⁶The pre-policy within-firm gender wage gap was 16.7 p.p. in firms with no female managers and 11.7 p.p. in firms where all managers are women.

²⁷The pre-policy gender wage gap was significant in firms without highly-educated managers (15.8 p.p.), although lower than in firms where all managers hold a higher degree (24.3 p.p.). The higher initial gap might have increased the pressure to equalize wages more for the latter group, which could partly explain the larger responses for them.

asuring, as the majority of managers are men. This result is in line with the idea that men may disproportionately benefit from having male managers and that the pay transparency mandate allows to correct for biases that may arise due to homophilic preferences (e.g. [Cullen and Perez-Truglia, 2023](#)). Our finding that female managers do not reduce gender gaps, on average, may suggest that female managers may not experience similar social pressure as male managers to reduce gender wage gaps when gender disparities are made salient within the firm. It is also consistent with prior research suggesting that the representation of women in decision-making positions may not always be sufficient for improving gender equality (e.g., [Bagues et al., 2017](#); [Bertrand et al., 2019](#); [Deschamps, 2024](#)). Finally, our results suggest that managers’ education plays an important role in ensuring that transparency mandates translate into lower gender inequality.

VIII. Discussion

The consequences of the Finnish pay transparency mandate can be directly compared with the impact of similar policies in other countries ([Obloj and Zenger, 2022](#); [Cullen and Pakzad-Hurson, 2023](#); [Blundell et al., 2023](#); [Bennedsen et al., 2022](#); [Baker et al., 2023](#); [Gulyas et al., 2023](#)). Overall, our findings are broadly consistent with the literature in showing that pay transparency reduces gender wage inequality. In Finland, the policy reduced the gender wage gap by 0.9-1.1 p.p., representing a more moderate decline relative to previous studies, where estimated effects range between 2 and 3 p.p. when the policy is effective. A useful benchmark is Denmark, which has the most comparable policy design and institutional setting to Finland. There, the gender wage gap decreased by 2.0 p.p ([Bennedsen et al., 2022](#)), placing our estimates at the lower end of the range of effects estimated in similar contexts. However, we uncover more symmetric wage adjustments by gender: while some prior studies find that reductions in gender gaps are driven by declines in men’s wages ([Duchini et al., 2024](#); [Bennedsen et al., 2023](#)), we find that the narrowing of the gender wage gap is a result of both a decline in men’s wages and an increase in women’s wages.

In Finland, the relatively high CBA coverage along with substantial variation across employers provides a useful setting for examining how pay transparency interacts with labor market institu-

tions. This is particularly relevant because existing evidence suggests that the differential impact of pay transparency across countries is systematically correlated with the level of the country's CBA coverage (Cullen and Pakzad-Hurson, 2023). In addition, Baker et al. (2023) show that gender wage gaps declined among unionised college professors following the introduction of pay transparency in Canada. We complement this work by extending the analysis to the private sector, which is a major target of these policies. Although Finnish collective bargaining takes place at the broader sectoral level and not at the employer-level as in Canada, the evidence points to the same direction: collective bargaining is important for pay transparency to effectively reduce gender wage gaps.

Our results by CBA coverage are also crucial for understanding how the policy affected average wages. Cullen and Pakzad-Hurson (2023) argue that introducing pay transparency between workers in the same occupation and firm will decrease average wages, by limiting workers' ability to bargain individually. This implies that the initial level of workers' bargaining power is an important determinant of whether transparency policies lead to wage reductions. For Finland, we find no evidence of such negative effects on average wages at the firm level. Once we control for worker composition within firms we find a precisely estimated zero effect. This contrasts with evidence from countries such as Canada, the United States and the United Kingdom, where individual bargaining power is generally weaker and where pay transparency has been found to put downward pressure on average wages (Cullen and Pakzad-Hurson, 2023; Mas, 2017; Obloj and Zenger, 2022; Baker et al., 2023; Blundell et al., 2023). Taken together, these findings suggest that strong collective bargaining institutions can mitigate the potential negative wage effects of pay transparency while still promoting reductions in the gender wage gap.

A widely discussed concern in the literature on pay transparency is its potential negative impact on worker morale, which may in turn lead to increased separations and lower firm productivity (Card et al., 2012; Cohn et al., 2014; Mas, 2017; Breza et al., 2018; Dube et al., 2019; Bennedsen et al., 2022; Gulyas et al., 2023; Blundell et al., 2023). We do not find evidence for any of these mechanisms. Worker morale is not significantly affected and separations do not increase in treated firms. We also do not find any evidence of reduced productivity among treated firms. Our results

on separations are similar to the Danish study ([Bennedsen et al., 2022](#)), which is closest to our setting in terms of policy design, institutional context, and the availability of linked employer-employee data on the entire economy. At the same time, our findings are different from some prior studies focusing on specific workers groups, which document increases in separations or job search intentions.²⁸

A novel and potentially concerning finding of our study highlights how pay transparency affects firms' hiring strategies and the central role of collective bargaining institutions in shaping this response. We find that treated firms reduce hiring into the lowest-paid positions within the firm. This response is concentrated in firms that are less exposed to collective bargaining agreements (CBAs). The reduction in hiring into low-paid positions may benefit incumbent workers but could disadvantage new labor market entrants.

IX. Conclusions

We study the effects of a pay transparency mandate that required employers to provide information on average wages by job category and gender. We show that the policy was salient to employees and successful in achieving its main objective of reducing gender inequality in workplaces. Women's wages were set closer to those of their male peers, and they gained access to higher-paid positions within firms. Pay transparency also led to lower perceptions of discrimination among employees and had no negative impacts on worker satisfaction or firm productivity.

Our findings improve understanding of the contexts in which pay transparency policies are most effective in reducing gender wage gaps. We show that employers operating within the collective bargaining system respond particularly strongly to the policy by reducing gender wage gaps, while their responses at the hiring and separation margins are small. At the same time, unorganized firms reduce hiring into lower-paid positions, while gender wage gaps do not decline among these firms, even though they have larger initial gender wage gaps. Taken together, these findings raise concerns that in settings with weak labor market institutions, pay transparency policies may worsen

²⁸For example, [Mas \(2017\)](#) examines separations by public sector managers and [Card et al. \(2012\)](#) job search intentions of employees at a university.

the prospects of new labor market entrants and less advantaged groups. They also have important implications for the design of pay transparency policies, highlighting the importance of features that limit unintended consequences arising from firms' hiring responses.

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Figures

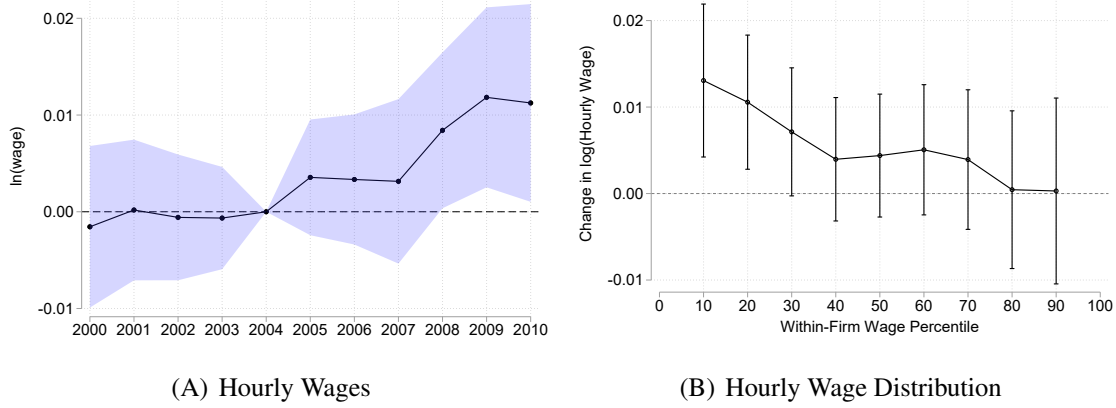


Figure 1: THE IMPACT OF PAY TRANSPARENCY ON WAGES AND WAGE DISTRIBUTION

Note: This figure shows event-study and difference-in-differences estimates of the effects of the 2005 pay-transparency reform on wages. Panel A presents event-study estimates of the impact on log wages in firms mandated to increase pay transparency relative to firms not subject to the mandate. Panel B presents difference-in-differences estimates for wage percentiles of the firm-level wage distribution. All specifications include firm and year fixed effects. Standard errors are clustered at the firm level. Shaded areas in Panel A and vertical lines in Panel B show 95% confidence intervals.

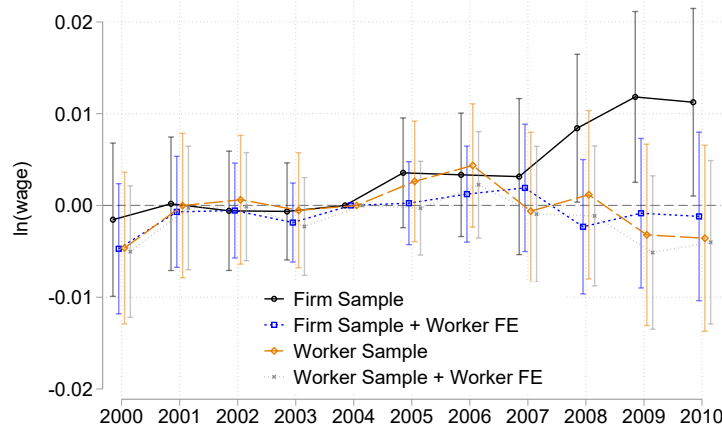
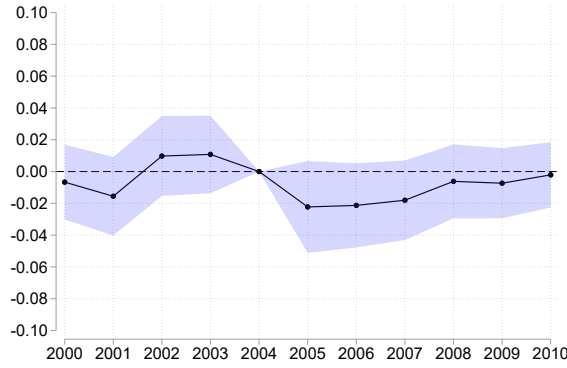
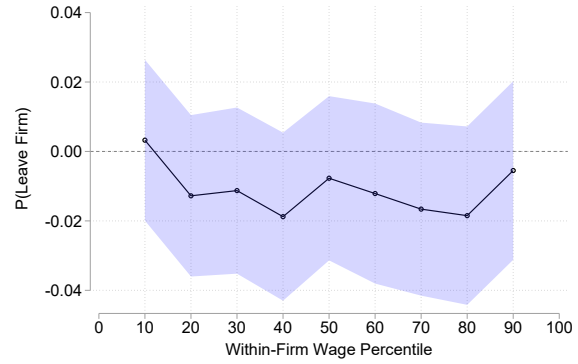


Figure 2: CONTRIBUTION OF WORKER COMPOSITION TO WAGE EFFECTS

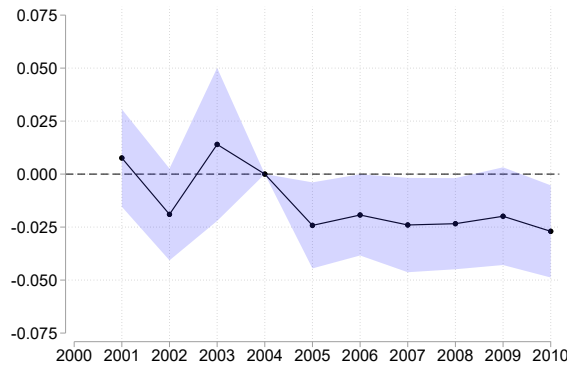
Note: This figure shows event-study estimates of the impact of pay transparency on wages when we adjust for worker composition. We replicate the baseline estimates using the Firm Sample (solid black line) and for three specifications that account for worker composition: (i) adding worker fixed effects to the baseline specification (blue dotted line), (ii) using the Worker Sample, which follows individuals employed in treatment and control firms in 2004 regardless of subsequent mobility (orange dashed line), and (iii) the Worker Sample with worker fixed effects (grey dotted line). Standard errors are clustered at the firm level. Vertical lines show 95% confidence intervals.



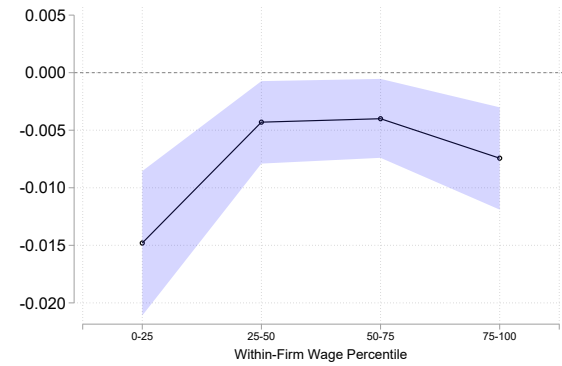
(A) Separations



(B) Separations by Wage Decile



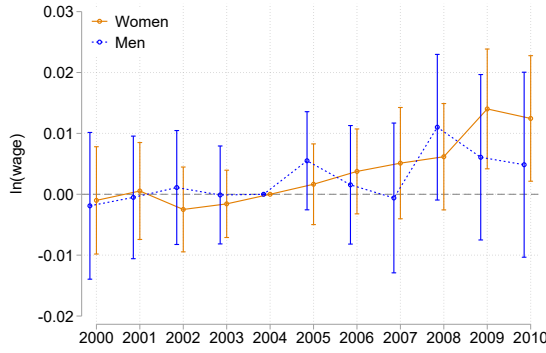
(C) Hiring



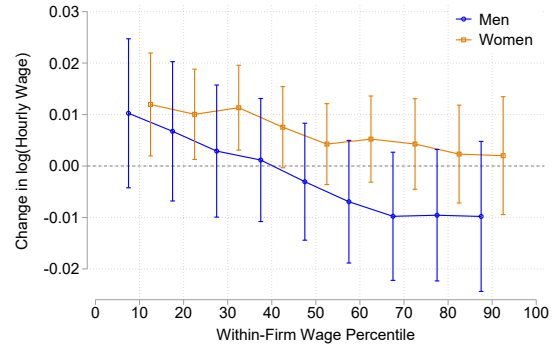
(D) Hiring by Wage Quartile

Figure 3: THE IMPACT OF PAY TRANSPARENCY ON WORKER SEPARATIONS AND HIRING

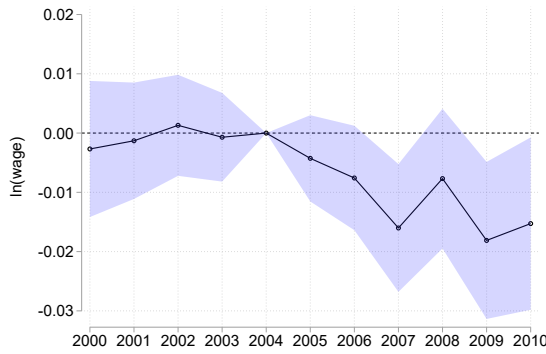
Note: This figure shows event-study and difference-in-differences estimates of the effects of pay transparency on worker separations and hiring. Panel A reports event-study estimates for separations among workers employed in sample firms in 2004. Panel B reports difference-in-differences estimates for separations by the worker's 2004 wage decile. Panel C reports event-study estimates of hiring relative to the number of pre-policy workers at the firm level. Panel D shows difference-in-difference estimates of hiring since 2004 relative to the number of workers at the firm level by 2004 wage quartile. All specifications include firm and year fixed effects. Standard errors are clustered at the firm level. Shaded areas show 95% confidence intervals.



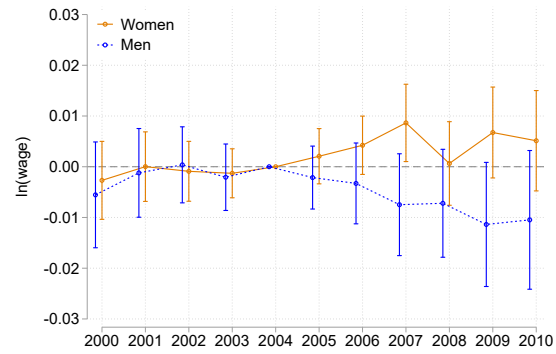
(A) Wages by Gender



(B) Wage Distribution by Gender



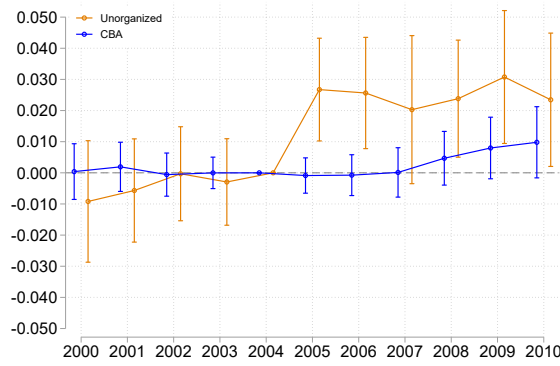
(C) Gender Wage Gap Controlling for Sorting



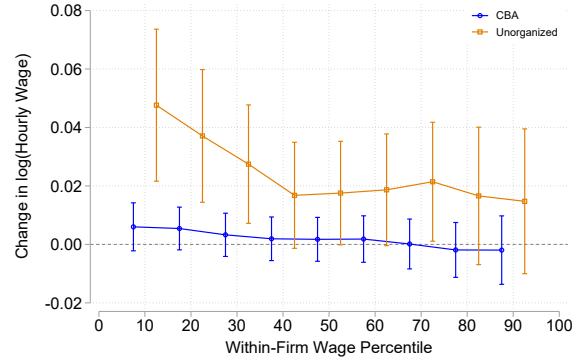
(D) Wages by Gender Controlling for Sorting

Figure 4: THE IMPACT OF PAY TRANSPARENCY ON THE GENDER WAGE GAP

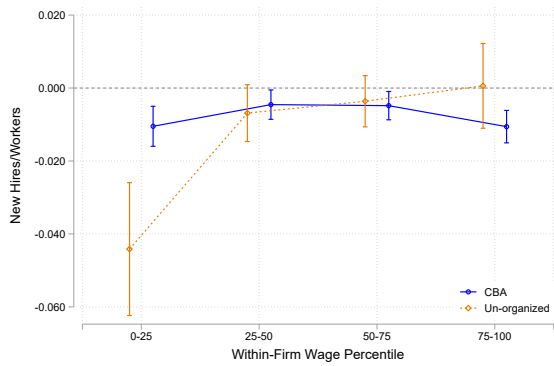
Note: This figure shows difference-in-differences and event-study estimates of the impact of pay transparency on the gender wage gap and on wages by gender. Panel A presents event-study estimates for the baseline model by gender, controlling for firm fixed effects. Panel B shows difference-in-differences estimates for wage percentiles of the firm wage distribution by gender, controlling for firm fixed effects. Panel C shows event-study estimates of the gender wage gap, controlling for firm and worker fixed effects. Panel D shows event-study estimates for the baseline model by gender, controlling for firm and worker fixed effects. All specifications include year fixed effects. Standard errors are clustered at the firm level. Shaded areas (vertical lines) show 95% confidence intervals.



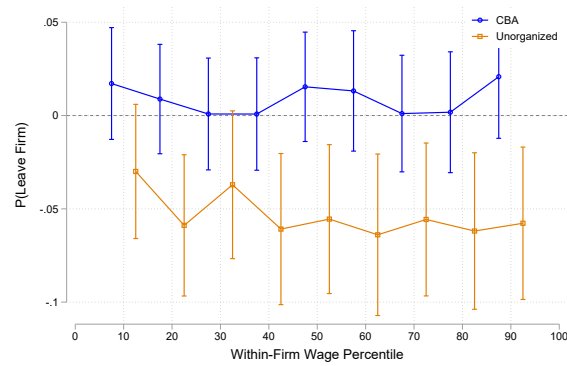
(A) Wages by CBA



(B) Wage Distribution by CBA



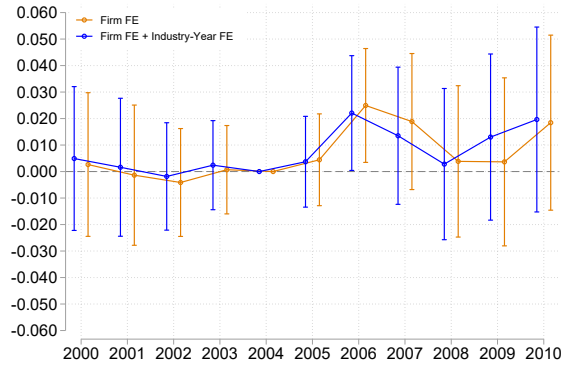
(C) Hiring by CBA



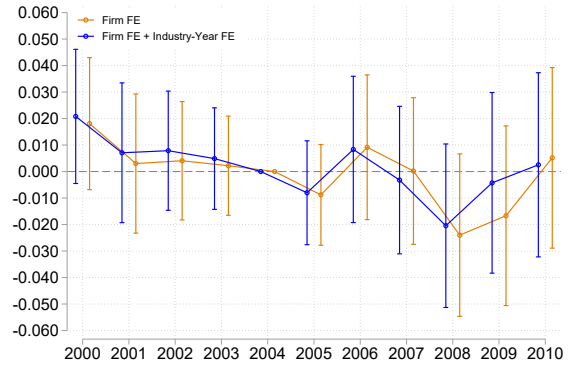
(D) Separations by CBA

Figure 5: WAGE EFFECTS AND SORTING BY COLLECTIVE BARGAINING STATUS

Note: This figure shows event-study and difference-in-difference estimates for the impact of pay transparency on wages, hiring, and separations by firm's pre-reform collective bargaining agreement (CBA) status, measured in 2004. Panel A shows event-study estimates for the baseline model. Panel B shows difference-in-difference estimates on wage percentiles of the firm wage distribution. Panel C shows difference-in-difference estimates of hiring since 2004 relative to the number of workers at the firm level by 2004 wage quartiles. Panel D shows difference-in-differences estimates for separations by the workers' wage decile measured in 2004. All estimations include firm and year fixed effects. Vertical lines show the 95% confidence intervals.



(A) Sales Per Worker



(B) Value Added per Worker

Figure 6: FIRM PRODUCTIVITY

Note: This figure shows event-study estimates of the impact of pay transparency on firm productivity, as measured by the firms' sales per worker (Panel A) and value added per worker (Panel B). Estimations include firm and year fixed effects (orange) and additional year-industry interactions (blue). Errors are clustered at the firm level. Vertical lines show 95% confidence intervals.

Tables

Table 1: SALIENCE OF PAY TRANSPARENCY POLICY TO WORKERS

	(1) All	(2) All	(3) Men	(4) Women	(5) All
Panel A: P(Equality Plan at Workplace)					
Post	0.075*** (0.010)	0.001 (0.017)	0.013 (0.022)	-0.017 (0.026)	0.013 (0.022)
$\mathbb{1}(\#Emp \geq 30) \times Post$		0.114*** (0.021)	0.103*** (0.027)	0.130*** (0.032)	0.103*** (0.027)
$\mathbb{1}(\#Emp \geq 30) \times Post \times Female$					0.026 (0.042)
Pre-policy Mean	0.075	0.075	0.077	0.072	0.075
Observations	4242	4242	2522	1720	4242
Panel B: P(Measures to Address Gender Wage Gaps)					
Post	0.042*** (0.014)	0.009 (0.024)	0.030 (0.032)	-0.025 (0.036)	0.025 (0.031)
$\mathbb{1}(\#Emp \geq 30) \times Post$		0.050* (0.029)	0.021 (0.038)	0.095** (0.044)	0.024 (0.038)
$\mathbb{1}(\#Emp \geq 30) \times Post \times Female$					0.071 (0.059)
Pre-policy Mean	0.254	0.254	0.279	0.215	0.254
Observations	4145	4145	2447	1698	4145

Note: This table shows difference-in-differences estimates for the salience of the pay transparency policy to workers and for the degree to which workers perceived increased measures to address gender wage gaps at the workplace, using individual-level data from the 2003 and 2008 waves of the Quality of Working Life Survey. Estimates are from a difference-in-difference model without firm or worker fixed effects: $y_{it} = \alpha + \rho Post + \beta Post \times \mathbb{1}(\#Emp_{j(i),2004} \geq 30) + \gamma \mathbb{1}(\#Emp_{j(i),2004} \geq 30) + \epsilon_{it}$. The sample includes all survey respondents (not restricted by pre-reform firm size). Panel A shows the estimates for a binary outcome equal to one if a worker reports that the employer has prepared an equality plan for the workplace and zero otherwise. Panel B shows the estimates for a binary outcome equal to one if the worker reports that the employer has implemented measures to address gender wage gaps at the workplace and zero otherwise. Robust standard errors are in parentheses. Significance levels: * < 10% ** < 5% *** < 1%.

Table 2: THE IMPACT OF PAY TRANSPARENCY ON THE WAGE DISTRIBUTION

Panel A. Hourly Wage			
	Mean	10th Percentile	90th Percentile
$1(\#Emp \geq 30) \times Post$	0.008** (0.003)	0.0145*** (0.0045)	0.0018 (0.0054)
Observations	570,571	24,240	24,240
Panel B. Within-Firm Inequality: Percentile Ratios			
	90/10 Wage Ratio	50/10 Wage Ratio	90/50 Wage Ratio
$1(\#Emp \geq 30) \times Post$	-0.0121** (0.00594)	-0.00853** (0.00379)	-0.00357 (0.00445)
Observations	24,240	24,240	24,240
Panel C. Within-Firm Inequality: Standard Deviation			
	Total Inequality	Horizontal Inequality	Vertical Inequality
$1(\#Emp \geq 30) \times Post$	-0.0045** (0.0020)	-0.0018 (0.0018)	-0.0027 (0.0033)
Outcome Mean	0.2397	0.1638	0.2446
Observations	22,444	22,444	20,846

Note: This table shows the difference-in-differences estimates on the within-firm wage distribution. Panel A shows estimates for the within-firm mean wage and for the 90th and 10th wage percentiles. Panel B shows estimates for the within-firm 90/10, 50/10, and 90/50 wage percentile ratios. Panel C shows estimates for the standard deviations of wages at the firm level as well as for standard deviation within (horizontal) and across occupations (vertical) within the firm. All specifications control for firm fixed effects and year dummies. Standard errors are clustered at the firm level. Significance levels: * < 10% ** < 5% *** < 1%.

Table 3: GENDER WAGE GAP AND PROMOTIONS BY GENDER

	Firm FE			Firm FE + Worker FE		
	Women (1)	Men (2)	All (3)	Women (4)	Men (5)	All (6)
Panel A: log(Hourly Wage), Firm Sample						
1(#Emp ≥ 30) × Post	0.009*** (0.003)	0.004 (0.004)	0.009*** (0.004)	0.005* (0.003)	-0.004 (0.004)	0.005 (0.003)
1(#Emp ≥ 30) × Post × Male			-0.003 (0.005)			-0.009** (0.004)
Worker FE	No	No	No	Yes	Yes	Yes
Pre-Policy Gender Gap			-0.161*** (0.003)			
Observations	309,013	261,507	570,571	309,013	261,507	570,571
Panel B: log(Hourly Wage), Worker Sample						
1(#Emp ≥ 30) × Post	0.005* (0.003)	-0.007 (0.004)	0.006** (0.003)	0.004 (0.003)	-0.007** (0.003)	0.004 (0.003)
1(#Emp ≥ 30) × Post × Male			-0.013*** (0.005)			-0.011*** (0.004)
Worker FE	No	No	No	Yes	Yes	Yes
Pre-Policy Gender Gap			-0.166*** (0.004)			
Observations	296,013	255,029	551,042	296,013	255,029	551,042
Panel C: Occupation Score, Worker Sample						
1(#Emp ≥ 30) × Post	0.008*** (0.003)	-0.001 (0.003)	0.009*** (0.003)	0.008*** (0.002)	-0.002 (0.003)	0.008*** (0.002)
1(#Emp ≥ 30) × Post × Male			-0.009*** (0.004)			-0.010*** (0.003)
Worker FE	No	No	No	Yes	Yes	Yes
Pre-Policy Gender Gap			-0.102*** (0.003)			
Observations	252,478	222,601	475,079	252,478	222,601	475,079

Note: This table shows difference-in-differences estimates for hourly wages and promotions by gender. Panel A shows estimates for the baseline Firm Sample, Panel B for the baseline Worker Sample. Panel C shows the impact of pay transparency on the occupation score, which is constructed by assigning to each 4-digit occupation the average hourly wage in the pre-reform period. It tracks occupational mobility in terms of expected wages. We control for year and firm fixed effects in all specifications, and in Columns 4-6 we add worker fixed effects to control for sorting (estimates based on the Worker Sample are not affected by sorting in either model). The pre-policy gender wage gap in Column 3 is calculated from a regression controlling for firm and year fixed effects, using the sample indicated by the panel title and restricted to the pre-reform years 2000-2004. Significance levels: * < 10% ** < 5% *** < 1%.

Table 4: PERCEIVED DISCRIMINATION AND JOB SATISFACTION

	(1) All	(2) All	(3) Men	(4) Women	(5) All
Panel A: P(Discrimination Against Women)					
Post	-0.003 (0.007)	-0.000 (0.011)	-0.008 (0.010)	0.013 (0.022)	-0.007 (0.011)
$\mathbb{1}(\#Emp \geq 30) \times Post$		-0.005 (0.014)	0.021 (0.014)	-0.045 (0.028)	0.021 (0.014)
$\mathbb{1}(\#Emp \geq 30) \times Post \times Female$					-0.066** (0.031)
Pre-policy Mean	0.068	0.068	0.036	0.117	0.068
Observations	4967	4967	2946	2021	4967
Panel B: P(Discrimination Against Men)					
Post	0.003 (0.004)	0.007 (0.006)	0.015* (0.008)	-0.007 (0.010)	0.015* (0.008)
$\mathbb{1}(\#Emp \geq 30) \times Post$		-0.005 (0.008)	-0.005 (0.011)	-0.005 (0.013)	-0.005 (0.011)
$\mathbb{1}(\#Emp \geq 30) \times Post \times Female$					0.001 (0.017)
Pre-policy Mean	0.018	0.018	0.016	0.023	0.018
Observations	4967	4967	2946	2021	4967
Panel C: Workplace Satisfaction					
Post	-0.029*** (0.009)	-0.032** (0.015)	-0.039** (0.020)	-0.021 (0.023)	-0.039** (0.020)
$\mathbb{1}(\#Emp \geq 30) \times Post$		0.004 (0.018)	0.005 (0.024)	0.004 (0.028)	0.005 (0.024)
$\mathbb{1}(\#Emp \geq 30) \times Post \times Female$					-0.000 (0.037)
Pre-policy Mean	0.909	0.909	0.911	0.906	0.909
Observations	4967	4967	2946	2021	4967

Note: This table shows difference-in-differences estimates on the effects of the pay transparency mandate on perceived gender discrimination and job satisfaction. We use individual-level data from the 2003 and 2008 waves of the Quality of Working Life Survey. We estimate a simplified version of the baseline difference-in-difference model without firm fixed effects: $y_{it} = \alpha + \rho Post + \beta Post \times \mathbb{1}(\#Emp_{j(i),2004} \geq 30) + \gamma \mathbb{1}(\#Emp_{j(i),2004} \geq 30) + \epsilon_{it}$. The sample includes all survey respondents (not restricted by pre-reform firm size). Panel A shows the estimates for perceived discrimination against women at the workplace. Panel B shows the estimates for perceived discrimination against men at the workplace. Panel C shows the estimates for job satisfaction. Robust standard errors in parentheses. Significance levels: * < 10% ** < 5% *** < 1%.

Table 5: WAGE SETTING BY FIRM ORGANISATION AND CBAS

	log(Hourly Wages)							
	All (1)	Women (2)	Men (3)	All (4)	All (5)	Women (6)	Men (7)	All (8)
$1(\#Emp \geq 30) \times Post$	0.025*** (0.008)	0.015** (0.007)	0.034*** (0.011)	0.017** (0.008)	0.007 (0.005)	0.006 (0.005)	0.008 (0.008)	0.005 (0.005)
$1(\#Emp \geq 30) \times Post \times CBA$	-0.021** (0.008)	-0.006 (0.008)	-0.038*** (0.012)	-0.009 (0.009)	-0.008 (0.006)	-0.001 (0.006)	-0.017* (0.009)	-0.001 (0.006)
$1(\#Emp \geq 30) \times Post \times Male$				0.015 (0.012)				0.003 (0.009)
$1(\#Emp \geq 30) \times Post \times CBA \times Male$				-0.025* (0.013)				-0.016* (0.010)
<i>N</i>	570,571	309,025	261,507	570,571	570,571	309,013	261,507	570,571
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Worker FE	No	No	No	No	Yes	Yes	Yes	Yes
Outcome Mean	2.739	2.664	2.827	2.739	2.739	2.664	2.827	2.739
% Organised	0.739	0.744	0.733	0.739	0.739	0.744	0.733	0.739
Pre-policy coef. on Female	-0.200*** (0.007)							
Pre-policy coef. on Female $\times$ CBA	0.056*** (0.008)							

Note: This table shows heterogeneity in the difference-in-differences estimates by collective bargaining agreement (CBA) status. CBA status is a binary indicator equal to one if the firm is a member of an employer association and zero otherwise. The estimations use the Firm Sample, with Columns 6-8 including worker fixed effects to control for worker composition. The pre-policy gender wage gap is calculated from the Firm Sample, using the pre-reform years 2000-2004 and controlling for firm and year fixed effects. We control for firm and year fixed effects in all specifications. Results for the Worker Sample are reported in Appendix Table A11. Significance levels: * < 10% ** < 5% *** < 1%.

Table 6: WAGE SETTING BY MANAGER CHARACTERISTICS

	log(Hourly Wages)							
	(1) All	(2) Women	(3) Men	(4) All	(5) All	(6) Women	(7) Men	(8) All
Panel A: Manager Gender								
$1(\#Emp \geq 30) \times Post$	-0.003 (0.005)	0.004 (0.005)	-0.013* (0.007)	0.004 (0.005)	-0.004 (0.004)	0.002 (0.005)	-0.012** (0.006)	0.002 (0.005)
$1(\#Emp \geq 30) \times Post \times Fem. Manager$	0.008 (0.008)	-0.002 (0.008)	0.025* (0.013)	-0.001 (0.008)	0.010 (0.007)	0.002 (0.008)	0.022** (0.011)	0.002 (0.008)
$1(\#Emp \geq 30) \times Post \times Male$				-0.014* (0.007)				-0.015** (0.006)
$1(\#Emp \geq 30) \times Post \times Fem. Manager \times Male$				0.021 (0.014)				0.019* (0.011)
<i>N</i>	350083	191036	159047	350083	350083	191036	159047	350083
Worker FE	No	No	No	No	Yes	Yes	Yes	Yes
Outcome Mean	2.755	2.686	2.839	2.755	2.755	2.686	2.839	2.755
% Female Managers	0.22				0.22			
Panel B: Manager Education								
$1(\#Emp \geq 30) \times Post$	-0.004 (0.005)	-0.005 (0.004)	-0.007 (0.007)	-0.005 (0.004)	-0.006 (0.004)	-0.006 (0.004)	-0.007 (0.006)	-0.006 (0.004)
$1(\#Emp \geq 30) \times Post \times High Edu. Manager$	0.015* (0.009)	0.026*** (0.009)	0.003 (0.013)	0.028*** (0.010)	0.020** (0.008)	0.031*** (0.009)	0.003 (0.012)	0.031*** (0.009)
$1(\#Emp \geq 30) \times Post \times Male$				0.000 (0.007)				-0.001 (0.006)
$1(\#Emp \geq 30) \times Post \times High Edu. Manager \times Male$				-0.029** (0.015)				-0.028*** (0.013)
<i>N</i>	350083	191036	159047	350083	350083	191036	159047	350083
Worker FE	No	No	No	No	Yes	Yes	Yes	Yes
Outcome Mean	2.755	2.686	2.839	2.755	2.755	2.686	2.839	2.755
% Highly Educated Managers	0.15				0.15			

Note: This table shows heterogeneity in difference-in-differences estimates by the share of female (Panel A) and highly-educated managers (Panel B). The estimation is based on the Worker Sample and includes workers that are not themselves in managerial positions prior to the reform. Results for the Firm Sample can be found in Appendix Table A13. The shares of female and highly educated managers are shown for the pre-policy year 2004. We control for firm and year fixed effects in all specifications. Significance levels: * < 10% ** < 5% *** < 1%.

A. Appendix Tables and Figures



Figure A1: SALIENCE OF PAY TRANSPARENCY MEASURES AND DIFFERENCES IN PAY TRANSPARENCY MANDATE

Note: Panels A and B show the prevalence of anti-discrimination measures within workplaces, using data from the Quality of Working Life Survey. Panel A reports the proportion of employees whose workplace prepared a Gender Equality Plan, and Panel B reports the proportion of workers whose workplace implemented measures to reduce the gender wage gap. Panels C and D show the persistence of treatment assignment over time. Panel C plots the probability that a firm is affected by the pay transparency mandate after 2004, where treated firms have ≥ 30 employees in 2004 and control firms have < 30 employees. Panel D reports difference-in-differences estimates following Equation 1, where the outcome is an binary indicator for a worker being employed in a firm bound by the mandate in the post-reform years. Panels C and D are based on the baseline Firm Sample. Shaded areas denote 95% confidence intervals.

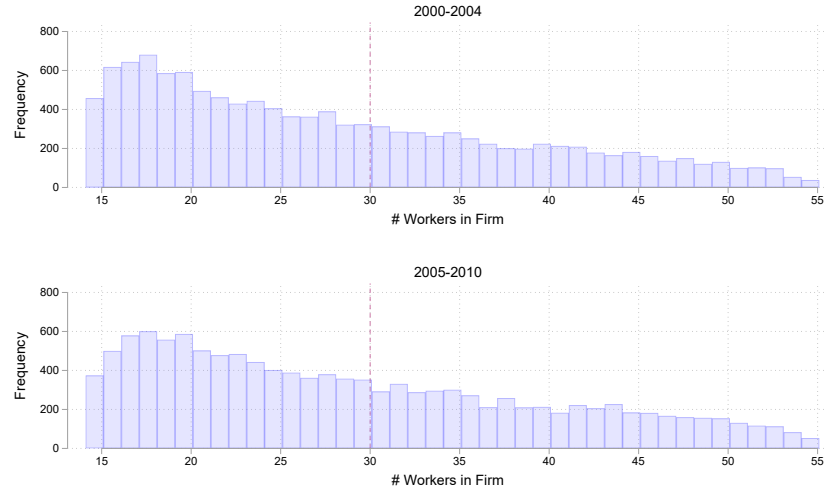
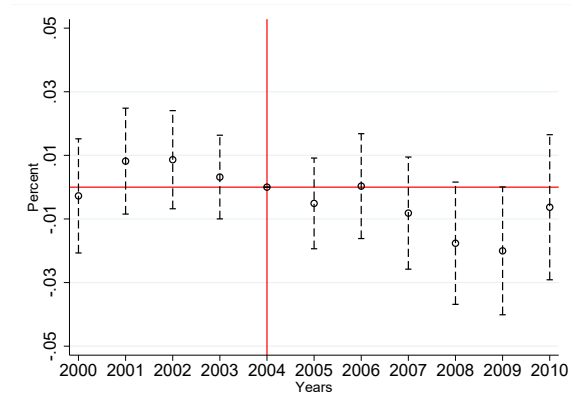
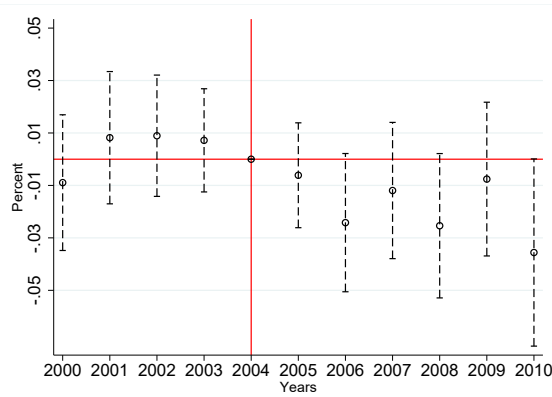


Figure A2: FIRM SIZE DISTRIBUTION BEFORE AND AFTER THE REFORM

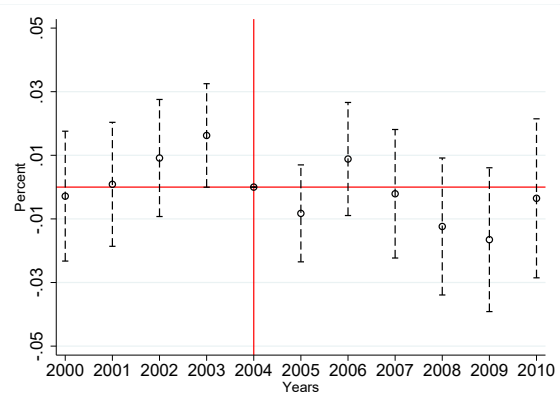
Note: This figure shows the distribution of firms weighted by the number of employees around the policy cutoff of 30 employees. Panel A shows the distribution prior to the reform in years 2000-2004 and Panel B after the reform in years 2005-2010.



(A) 90-10 Ratio, All Workers



(B) 90-10 Ratio, Male Workers



(C) 90-10 Ratio, Female Workers

Figure A3: EVENT-STUDY ESTIMATES FOR 90/10 WAGE PERCENTILE GAP

Note: This figure shows event-study estimates for the within-firm 90/10 wage percentile ratio. Panel A shows the overall effect, and Panels B and C show results separately by gender. All specifications include firm and year fixed effects. Standard errors are clustered at the firm level, and vertical lines show 95% confidence intervals

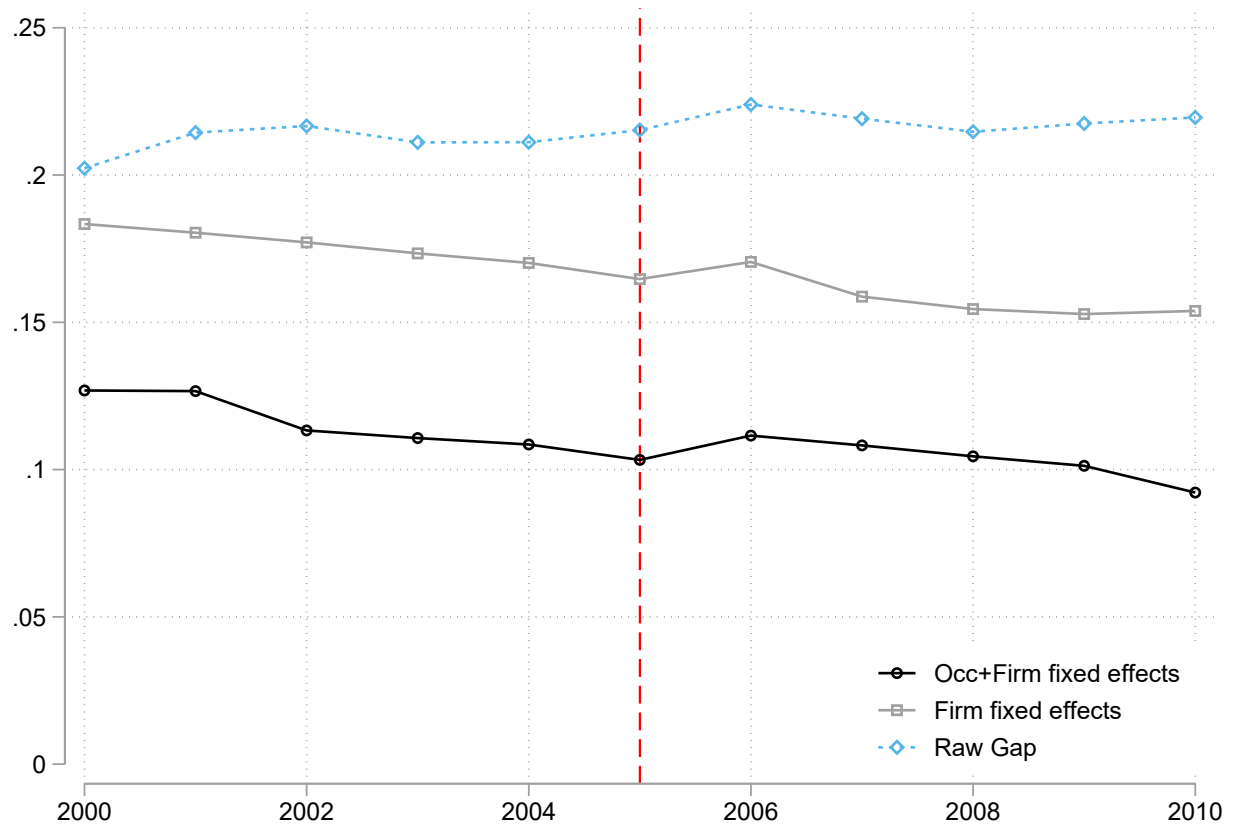
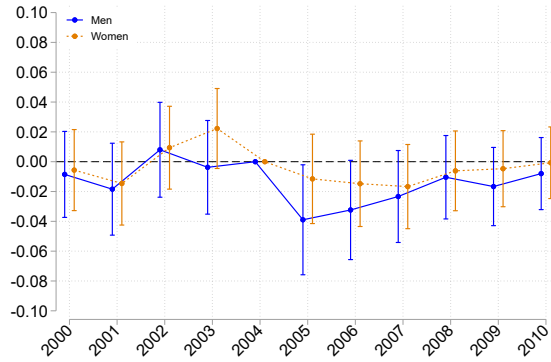
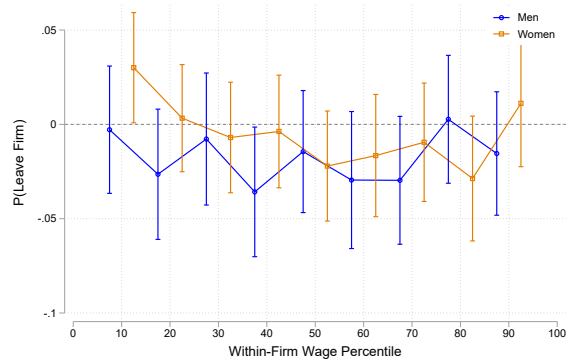


Figure A4: EVOLUTION OF THE GENDER WAGE GAP

Note: This figure shows the gender wage gap over time for all firms and workers in the Structure of Earnings Survey. Each point represents a coefficient from a separate regression of $\log(\text{hourly wages})$ on a female dummy. Diamonds show the raw gender wage gap; squares show estimates controlling for firm fixed effects; and circles show estimates controlling for both firm and occupation fixed effects. Prior to the reform in 2004, the raw gap was 21 percent and the within-firm gap was 17 percent. After the reform, we see a decline in the within-firm gap of 1.7 p.p. between 2004 and 2010, while the raw gap increases by 0.9 p.p.



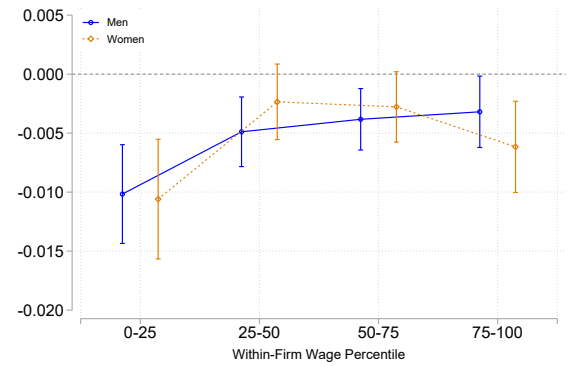
(A) Worker Separations by Gender



(B) Worker Separations by Wage Decile and Gender



(C) Hiring by Gender



(D) Hiring by Wage Quartile and Gender

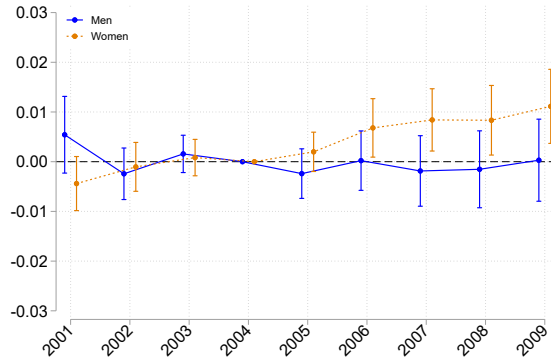
Figure A5: THE IMPACT OF PAY TRANSPARENCY ON WORKER SEPARATIONS AND HIRING BY GENDER

Note: This figure presents event-study and difference-in-differences estimates of the effects of pay transparency on worker separations and hiring by gender. Panel A shows estimates for worker separations by gender; Panel B shows difference-in-differences estimates for separations by wage decile and gender; Panel C shows event-study estimates for hiring by gender; Panel D shows difference-in-differences estimates for hiring by wage quartile and gender. All specifications include firm and year fixed effects. Standard errors are clustered at the firm level, and vertical lines show 95% confidence intervals.

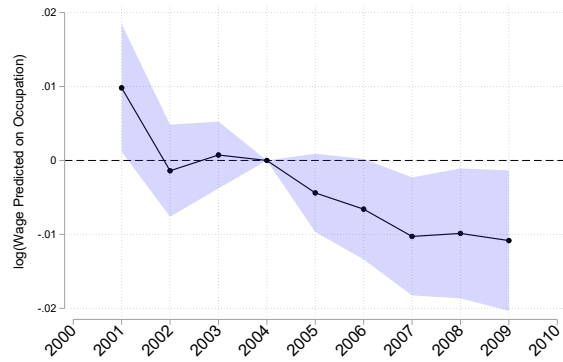


Figure A6: EFFECTS ON GENDER WAGE GAP BY 1-DIGIT ISCO OCCUPATIONS

Note: This figure shows difference-in-differences estimates of the effect of pay transparency on the gender wage gap by occupation. All specifications include year fixed effects as well as worker fixed effects to account for worker composition. Standard errors are clustered at the firm level, and horizontal lines show 95% confidence intervals.



(A) Promotions by gender



(B) Gender-Gap in Promotions

Figure A7: THE IMPACT OF PAY TRANSPARENCY ON PROMOTIONS

Note: This figure shows event-study estimates of the effects of the 2005 pay-transparency reform on promotions. The outcome is the occupation-specific expected wage measured before the reform (occupation score), which varies as an individual's occupation code changes. We use the Worker Sample which follows individuals who were employed in treated and control firms in 2004. Panel A reports effects on promotions by gender, and Panel B reports effects on the gender gap in promotions. All specifications include firm, year, and worker fixed effects. Standard errors are clustered at the firm level, and shaded areas and vertical lines show 95% confidence intervals.

Table A1: PAY REPORT TEMPLATE

	Number of Workers		Base wage (euros)		Base wage gap, %	Total wage (euros)		Total gap %
	Women	Men	Women	Men		Women	Men	
Occupation A								
Occupation B								
Occupation C								
or:								
Pay Grade A								
Pay Grade B								
Pay Grade C								

Note: This table shows a template for pay reporting from 2006 by a large employer association. The pay report provides information on wages of men and women across job categories either using occupation groups or pay grades. The report also includes information on the distribution of men and women across job categories. Link: <https://www.kt.fi/sites/default/files/media/document/yleiskirje0601-esimerkki-kuntasektorin-pakkakartoituksesta-liite-2.pdf>

Table A2: PRE-REFORM CHARACTERISTICS OF WORKERS

	Mean	15-29 Employees	30-55 Employees	Observations
	(1)	(2)	(3)	(4)
Individual-level means				
Men	0.49 [0.50]	0.46 [0.50]	0.51 [0.50]	83,332
Age	39.62 [12.34]	39.66 [12.51]	39.59 [12.20]	83,332
Hourly Wage	16.16 [7.11]	15.81 [6.65]	16.45 [7.45]	80,038
Hourly Wage (Men)	17.78 [8.30]	17.43 [7.83]	18.03 [8.62]	38,196
Hourly Wage (Women)	14.68 [5.40]	14.50 [5.16]	14.85 [5.61]	41,842
Occupation Score	2.78 [0.27]	2.77 [0.26]	2.78 [0.27]	82,402
Occupation Score (Men)	2.83 [0.27]	2.83 [0.26]	2.84 [0.27]	40,214
Occupation Score (Women)	2.72 [0.25]	2.71 [0.24]	2.73 [0.25]	42,188
Private Sector	0.96 [0.19]	0.97 [0.16]	0.95 [0.21]	83,332
Salary Contract	0.74 [0.44]	0.77 [0.42]	0.72 [0.45]	83,332
Hourly Contract	0.26 [0.44]	0.23 [0.42]	0.28 [0.45]	83,332
Bachelor's Degree	0.14 [0.34]	0.14 [0.34]	0.14 [0.34]	83,332
CBA coverage	0.69 [0.46]	0.71 [0.45]	0.67 [0.47]	83,332
Female Manager Share	0.33 [0.39]	0.35 [0.42]	0.32 [0.37]	54,371
High Edu. Manager Share	0.26 [0.36]	0.26 [0.38]	0.26 [0.35]	54,371
# Employees	32.39 [11.00]	22.11 [4.21]	40.88 [6.83]	83,332
Firm-level means				
CBA coverage	0.65 [0.60]	0.68 [0.66]	0.61 [0.50]	3,089
Female Manager Share	0.22 [0.36]	0.26 [0.39]	0.18 [0.31]	1,937
High Edu. Manager Share	0.15 [0.30]	0.18 [0.33]	0.13 [0.26]	1,937
# Employees	28.80 [10.56]	21.33 [4.19]	39.81 [6.78]	3,089

Note: This table reports characteristics of workers and firms in the baseline Firm and Worker Samples in 2004, which is the last pre-policy year. We note that the two samples include the same firms and workers in this year. Standard deviations are in brackets.

Table A3: IMPACT ON WAGES BY INITIAL WAGE INEQUALITY

	log(Hourly Wages)		
	(1)	(2)	(3)
$\mathbb{1}(\#Emp \geq 30) \times Post$	-0.0084 (0.0088)	-0.0079 (0.0084)	0.0045 (0.0060)
$\mathbb{1}(\#Emp \geq 30) \times Post \times Total\ Inequality$	0.0610 (0.0386)		
$\mathbb{1}(\#Emp \geq 30) \times Post \times Horizontal\ Inequality$		0.0900* (0.0528)	
$\mathbb{1}(\#Emp \geq 30) \times Post \times Vertical\ Inequality$			0.0063 (0.0258)
Observations	521,876	521,876	521,876
Ineq. mean	0.244	0.165	0.226
Worker FE	No	No	No

Note: This table shows difference-in-differences estimates of the effect of the pay transparency mandate on average wages by pre-policy firm-level wage inequality. Pre-policy inequality is measured as the standard deviation of wages at the firm level in 2004. We decompose total wage inequality into horizontal (within 1-digit occupation) and vertical (between 1-digit occupation) components. All specifications include firm and year fixed effects. Standard errors are clustered at the firm level. Significance levels: * < 10% ** < 5% *** < 1%.

Table A4: WORKER COMPOSITION AND WAGE EFFECTS

	log(Hourly Wage)			
	Firm Sample		Worker Sample	
	(1)	(2)	(3)	(4)
$\mathbb{1}(\#Emp \geq 30) \times Post$	0.008** (0.003)	0.001 (0.003)	0.001 (0.003)	-0.000 (0.003)
Worker FE	No	Yes	No	Yes
Outcome Mean	2.739	2.739	2.752	2.752
Observations	570,571	570,571	551,042	551,042

Note: This table shows difference-in-differences estimates of wage effects for specifications that do (Column 1) and do not (Columns 2-4) account for worker composition across firms. Column 1 reports estimates using the baseline Firm Sample and controlling for firm and year fixed effects. Column 2 adds worker fixed effects to control for sorting. Columns 3 and 4 report analogous estimates using the Worker Sample, which eliminates the contribution of sorting by following workers who were initially employed by treatment or control firms in 2004, regardless of their subsequent mobility. Standard errors are clustered at the firm level. Significance levels: * < 10% ** < 5% *** < 1%.

Table A5: ROBUSTNESS - WORKER COMPOSITION AND WAGE EFFECTS FOR ALTERNATIVE DEFINITIONS OF THE TREATMENT AND CONTROL GROUPS

	log(Hourly Wage)			
	Firm Sample		Worker Sample	
	(1)	(2)	(3)	(4)
Panel A: Baseline (15-55 employees)				
$\mathbb{1}(\#Emp \geq 30) \times Post$	0.008*** (0.003)	0.001 (0.003)	0.001 (0.003)	-0.000 (0.003)
Person FE	No	Yes	No	Yes
Outcome Mean	2.739	2.739	2.752	2.752
Observations	570,571	570,571	551,042	551,042
Panel B: Expanded Baseline (10-60 employees)				
$\mathbb{1}(\#Emp \geq 30) \times Post$	0.007*** (0.003)	0.001 (0.002)	0.002 (0.002)	0.001 (0.002)
Person FE	No	Yes	No	Yes
Outcome Mean	2.734	2.734	2.747	2.747
Observations	748,780	748,780	722,630	722,630
Panel C: 10-55 employees				
$\mathbb{1}(\#Emp \geq 30) \times Post$	0.008*** (0.003)	0.002 (0.002)	0.002 (0.003)	0.002 (0.002)
Person FE	No	Yes	No	Yes
Outcome Mean	2.730	2.730	2.743	2.743
Observations	689,486	689,486	667,810	667,810
Panel D: 15-50 employees				
$\mathbb{1}(\#Emp \geq 30) \times Post$	0.006** (0.003)	0.000 (0.003)	0.000 (0.003)	-0.001 (0.003)
Person FE	No	Yes	No	Yes
Outcome Mean	2.735	2.735	2.749	2.749
Observations	508,683	508,683	493,695	493,695
Panel E: 15-19 vs. 30-55 employees (excluding firms with 20-29 employees)				
$\mathbb{1}(\#Emp \geq 30) \times Post$	0.009** (0.004)	0.001 (0.003)	-0.004 (0.003)	-0.001 (0.003)
Person FE	No	Yes	No	Yes
Outcome Mean	2.750	2.750	2.759	2.759
Observations	429,970	429,970	400,649	400,649

Note: This table shows difference-in-differences estimates of the effect of the pay transparency mandate on hourly wages using samples with alternative definitions of the treatment and control groups. Panel A reports results for the baseline bandwidth of 15–55 employees, Panel B for an expanded bandwidth of 10–60 employees, and Panels C and D for bandwidths of 10-55 and 15-50 employees, respectively. Panel E removes firms with 20–29 employees from the baseline. Columns 1–2 follow firms in the treatment and control groups (Firm Sample), and Columns 3–4 follow workers initially employed in treatment and control group firms (Worker Sample). All specifications include firm and year fixed effects. Standard errors are clustered at the firm level. Significance levels: * < 10% ** < 5% *** < 1%.

Table A6: FIRM SIZE AND HIRING BY GENDER

	Number of Workers			New Hires		
	All (1)	Women (2)	Men (3)	All (4)	Women (5)	Men (6)
$\mathbb{1}(\#Emp \geq 30) \times Post$	-0.048*** (0.012)	-0.034*** (0.008)	-0.013* (0.007)	-0.024*** (0.006)	-0.015*** (0.004)	-0.011*** (0.003)
Outcome Mean	0.885	0.493	0.392	0.172	0.101	0.080
Observations	24,261	24,261	24,261	24,261	24,261	24,261

Note: This table shows difference-in-differences estimates of the effect of the pay transparency mandate on firm size and hiring. Columns 1 and 4 report effects on the total number of workers and new hires relative to firm size in 2004, respectively. Columns 2–3 and 5–6 report effects separately by gender. All specifications include firm and year fixed effects. Standard errors are clustered at the firm level. Significance levels: * < 10% ** < 5% *** < 1%.

Table A7: IMPACT ON WITHIN-FIRM INEQUALITY BY GENDER

Panel A. All Workers

	90/10 Wage Ratio	50/10 Wage Ratio	90/50 Wage Ratio
1 (#Emp $\geq$ 30) X Post	-0.0121** (0.00594)	-0.00853** (0.00379)	-0.00357 (0.00445)
Observations	24240	24240	24240

Panel B. Female Workers

	90/10 Wage Ratio	50/10 Wage Ratio	90/50 Wage Ratio
1 (#Emp $\geq$ 30) X Post	-0.00979 (0.00660)	-0.00763* (0.00413)	-0.00216 (0.00489)
Observations	22881	22881	22881

Panel C. Male Workers

	90/10 Wage Ratio	50/10 Wage Ratio	90/50 Wage Ratio
1 (#Emp $\geq$ 30) X Post	-0.0205** (0.00916)	-0.0137** (0.00574)	-0.00688 (0.00644)
Observations	22547	22547	22547

Note: This table shows difference-in-differences estimates of the effect of the pay-transparency mandate on within-firm wage inequality using 90/10 (Column 1), 50/10 (Column 2), and 90/50 (Column 3) wage percentile ratios. Panel A reports estimates for all workers, and Panels B and C report estimates separately by gender. All specifications include firm and year fixed effects. Standard errors are clustered at the firm level. Significance levels: * < 10% ** < 5% *** < 1%.

Table A8: ROBUSTNESS - GENDER WAGE GAP FOR ALTERNATIVE DEFINITIONS OF THE TREATMENT AND CONTROL GROUPS (FIRM SAMPLE)

	log(Hourly Wage)					
	Firm FE			Firm FE + Worker FE		
	Women (1)	Men (2)	All (3)	Women (4)	Men (5)	All (6)
Panel A: Baseline (15-55 employees)						
1(#Emp ≥ 30) × Post	0.009*** (0.003)	0.004 (0.004)	0.009*** (0.004)	0.005* (0.003)	-0.004 (0.004)	0.005 (0.003)
1(#Emp ≥ 30) × Post × Male			-0.003 (0.005)			-0.009** (0.004)
Worker FE	No	No	No	Yes	Yes	Yes
Pre-Policy Gender Gap			-0.161*** (0.003)			
Observations	309,013	261,507	570,571	309,013	261,507	570,571
Panel B: Expanded Baseline (10 - 60 employees)						
1(#Emp ≥ 30) × Post	0.009*** (0.003)	0.003 (0.004)	0.008*** (0.003)	0.004 (0.003)	-0.004 (0.003)	0.004 (0.003)
1(#Emp ≥ 30) × Post × Male			-0.001 (0.005)			-0.007** (0.003)
Worker FE	No	No	No	Yes	Yes	Yes
Pre-Policy Gender Gap			-0.162*** (0.003)			
Observations	412,785	335,881	748,780	412,785	335,881	748,780
Panel C: 10 - 55 employees						
1(#Emp ≥ 30) × Post	0.009*** (0.003)	0.005 (0.004)	0.009*** (0.003)	0.005* (0.003)	-0.003 (0.003)	0.005* (0.003)
1(#Emp ≥ 30) × Post × Male			-0.002 (0.005)			-0.008** (0.004)
Worker FE	No	No	No	Yes	Yes	Yes
Pre-Policy Gender Gap			-0.162*** (0.003)			
Observations	385,397	303,978	689,486	385,397	303,978	689,486
Panel D: 15 - 50 employees						
1(#Emp ≥ 30) × Post	0.007** (0.003)	0.004 (0.004)	0.006* (0.004)	0.002 (0.003)	-0.004 (0.004)	0.002 (0.003)
1(#Emp ≥ 30) × Post × Male			0.001 (0.005)			-0.006 (0.004)
Worker FE	No	No	No	Yes	Yes	Yes
Pre-Policy Gender Gap			-0.161*** (0.004)			
Observations	278,255	230,385	508,683	278,255	230,385	508,683
Panel E: 15-19 vs. 30-55 employees (excluding firms with 20-29 employees)						
1(#Emp ≥ 30) × Post	0.010** (0.004)	0.005 (0.005)	0.012*** (0.004)	0.005 (0.003)	-0.005 (0.004)	0.004 (0.003)
1(#Emp ≥ 30) × Post × Male			-0.007 (0.006)			-0.010** (0.005)
Worker FE	No	No	No	Yes	Yes	Yes
Pre-Policy Gender Gap			-0.160*** (0.004)			
Observations	230,044	199,891	429,970	230,044	199,891	429,970

Note: This table shows difference-in-differences estimates of the effect of the pay transparency mandate on the gender wage gap using alternative definitions of the treatment and control groups for the Firm Sample. Panels A-E for alternative group definitions mirror those in Appendix Table A5. All specifications include firm and year fixed effects. Standard errors are clustered at the firm level. Significance levels: * < 10% ** < 5% *** < 1%.

Table A9: ROBUSTNESS - GENDER WAGE GAP FOR ALTERNATIVE DEFINITIONS OF THE TREATMENT AND CONTROL GROUPS (WORKER SAMPLE)

	log(Hourly Wage)					
	Firm FE			Firm FE + Worker FE		
	Women (1)	Men (2)	All (3)	Women (4)	Men (5)	All (6)
Panel A: Baseline (15-55 employees)						
1(#Emp ≥ 30) × Post	0.005* (0.003)	-0.007 (0.004)	0.006** (0.003)	0.004 (0.003)	-0.007** (0.003)	0.004 (0.003)
1(#Emp ≥ 30) × Post × Male			-0.013*** (0.005)			-0.011*** (0.004)
Worker FE	No	No	No	Yes	Yes	Yes
Pre-Policy Gender Gap			-0.166*** (0.004)			
Observations	296,013	255,029	551,042	296,013	255,029	551,042
Panel B: Expanded Baseline (10 - 60 employees)						
1(#Emp ≥ 30) × Post	0.006** (0.002)	-0.007*** (0.003)	0.006*** (0.002)	0.005** (0.002)	-0.007*** (0.003)	0.005** (0.002)
1(#Emp ≥ 30) × Post × Male			-0.013*** (0.004)			-0.011*** (0.003)
Worker FE	No	No	No	Yes	Yes	Yes
Pre-Policy Gender Gap			-0.168*** (0.003)			
Observations	396,659	325,653	722,312	396,659	325,653	722,312
Panel C: 10 - 55 employees						
1(#Emp ≥ 30) × Post	0.007*** (0.002)	-0.008** (0.003)	0.008*** (0.003)	0.006*** (0.002)	-0.007** (0.003)	0.006** (0.002)
1(#Emp ≥ 30) × Post × Male			-0.015*** (0.004)			-0.013*** (0.003)
Worker FE	No	No	No	Yes	Yes	Yes
Pre-Policy Gender Gap			-0.166*** (0.003)			
Observations	371,704	295,806	667,510	371,704	295,806	667,510
Panel D: 15 - 50 employees						
1(#Emp ≥ 30) × Post	0.003 (0.003)	-0.006* (0.003)	0.003 (0.003)	0.002 (0.002)	-0.006* (0.003)	0.002 (0.002)
1(#Emp ≥ 30) × Post × Male			-0.009* (0.005)			-0.008** (0.003)
Worker FE	No	No	No	Yes	Yes	Yes
Pre-Policy Gender Gap			-0.167*** (0.004)			
Observations	268,330	225,146	493,476	268,330	225,146	493,476
Panel E: 15-19 vs. 30-55 employees (excluding firms with 20-29 employees)						
1(#Emp ≥ 30) × Post	0.003 (0.003)	-0.016*** (0.004)	0.004 (0.004)	0.004 (0.003)	-0.010*** (0.004)	0.002 (0.003)
1(#Emp ≥ 30) × Post × Male			-0.021*** (0.006)			-0.014** (0.004)
Worker FE	No	No	No	Yes	Yes	Yes
Pre-Policy Gender Gap			-0.165*** (0.004)			
Observations	213,390	187,064	400,454	213,390	187,064	400,454

Note: This table shows difference-in-differences estimates of the effect of the pay-transparency mandate on the gender wage gap using alternative definitions of the treatment and control groups for the Worker Sample. Panels A-E for alternative group definitions mirror those in Appendix Table A5. All specifications include firm and year fixed effects. Standard errors are clustered at the firm level. Significance levels: * < 10% ** < 5% *** < 1%.

Table A10: PAY TRANSPARENCY AND SEPARATIONS

	P(Leave firm)							
	All (1)	Women (2)	Men (3)	All (4)	All (5)	Women (6)	Men (7)	All (8)
Treated×Post	-0.013 (0.012)	-0.011 (0.011)	-0.017 (0.015)	-0.011 (0.011)	-0.013 (0.012)	-0.011 (0.011)	-0.017 (0.015)	-0.011 (0.011)
Treated × Post × Male				-0.006 (0.013)				-0.006 (0.013)
Worker FE	no	no	no	no	yes	yes	yes	yes
Outcome Mean	0.533	0.513	0.554	0.533	0.533	0.513	0.554	0.533
Observations	915,612	465,258	450,354	915,612	915,612	465,258	450,354	915,612

Note: This table shows difference-in-differences estimates of the effect of the pay-transparency mandate on the probability that a worker leaves the firm in which she or he was employed in 2004. The table reports estimates for the Worker Sample. All specifications include firm fixed effects. Standard errors are clustered at the firm level. Significance levels: * < 10% ** < 5% *** < 1%.

Table A11: WAGE SETTING BY FIRM ORGANIZATION AND CBAs (WORKER SAMPLE)

	log(Hourly Wages)							
	All (1)	Women (2)	Men (3)	All (4)	All (5)	Women (6)	Men (7)	All (8)
$1(\#Emp \geq 30) \times Post$	0.008 (0.006)	0.003 (0.006)	0.012 (0.010)	0.005 (0.006)	0.005 (0.005)	0.003 (0.005)	0.006 (0.007)	0.003 (0.005)
$1(\#Emp \geq 30) \times Post \times CBA$	-0.010 (0.007)	0.002 (0.007)	-0.022** (0.010)	-0.001 (0.007)	-0.007 (0.005)	0.000 (0.006)	-0.016** (0.008)	-0.000 (0.006)
$1(\#Emp \geq 30) \times Post \times Male$				0.003 (0.011)				0.003 (0.008)
$1(\#Emp \geq 30) \times Post \times CBA \times Male$				-0.018 (0.009)				-0.016* (0.009)
<i>N</i>	471,876	254,924	216,952	471,876	471,876	254,924	216,952	471,876
Worker FE	No	No	No	No	Yes	Yes	Yes	Yes
Outcome Mean	2.746	2.672	2.832	2.746	2.746	2.672	2.832	2.746
% Organised (proxy)	0.752	0.757	0.746	0.752	0.752	0.757	0.746	0.752
Pre-policy coef. on Female	-0.211*** (0.007)							
Pre-policy coef. on Female $\times$ CBA	0.060*** (0.008)							

Note: This table shows heterogeneity in difference-in-differences estimates by collective bargaining agreement (CBA) status, mirroring specifications in Table 5 but using the Worker Sample. All specifications include firm and year fixed effects. Standard errors are clustered at the firm level. Significance levels: * < 10% ** < 5% *** < 1%.

Table A12: THE IMPACT ON FIRM PRODUCTIVITY

	log(Sales per Worker)		log(Value Added per Worker)	
	(1)	(2)	(3)	(4)
Panel A: All Firms in Firm Registry				
1 (#Emp $\geq$ 30) X Post	0.013 (0.010)	0.011 (0.010)	-0.011 (0.010)	-0.012 (0.010)
Firm FE	Yes	Yes	Yes	Yes
Industry-Year FE	No	Yes	No	Yes
Observations	71,203	71,177	70,572	70,538
Panel B: Firms in the Wage Data				
1 (#Emp $\geq$ 30) X Post	0.018 (0.017)	0.010 (0.017)	-0.010 (0.017)	-0.019 (0.017)
Firm FE	Yes	Yes	Yes	Yes
Industry-Year FE	No	Yes	No	Yes
Observations	22,491	22,462	22,369	22,330

Note: This table shows difference-in-differences estimates of the effect of the pay transparency mandate on firm productivity. In Panel A, Columns 1–2 report effects on sales per worker, and Columns 3–4 report effects on value added per worker, using specifications with firm fixed effects and industry-year fixed effects. Panel B reports analogous estimates for firms included in the Structure of Earnings Survey. Standard errors are clustered at the firm level. Significance levels: * < 10% ** < 5% *** < 1%.

Table A13: WAGE SETTING BY MANAGER CHARACTERISTICS (FIRM SAMPLE)

	log(Hourly Wages)							
	(1) All	(2) Women	(3) Men	(4) All	(5) All	(6) Women	(7) Men	(8) All
Panel A: Manager Gender								
1(#Emp ≥ 30) × Post	0.001 (0.004)	0.006 (0.005)	-0.007 (0.006)	0.005 (0.006)	0.001 (0.004)	0.008 (0.005)	-0.008 (0.005)	0.007 (0.005)
1(#Emp ≥ 30) × Post × Fem. Manager	0.010 (0.007)	0.007 (0.008)	0.011 (0.012)	0.008 (0.009)	0.009 (0.007)	0.001 (0.008)	0.020* (0.011)	0.002 (0.008)
1(#Emp ≥ 30) × Post × Male				-0.006 (0.006)				-0.008 (0.005)
1(#Emp ≥ 30) × Post × Fem. Manager × Male				0.011 (0.013)				0.020* (0.011)
<i>N</i>	265279	146897	118328	265279	265279	146889	118312	265279
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Worker FE	No	No	No	No	Yes	Yes	Yes	Yes
Outcome Mean	2.749	2.684	2.829	2.749	2.749	2.684	2.829	2.749
% Female Managers	0.22				0.22			
Panel B: Manager Education								
1(#Emp ≥ 30) × Post	0.001 (0.004)	0.003 (0.004)	-0.004 (0.006)	0.002 (0.004)	-0.001 (0.004)	0.000 (0.004)	-0.003 (0.005)	0.000 (0.004)
1(#Emp ≥ 30) × Post × High Edu. Manager	0.013 (0.009)	0.018* (0.010)	-0.000 (0.013)	0.019* (0.011)	0.016* (0.009)	0.023** (0.011)	0.003 (0.012)	0.023** (0.011)
1(#Emp ≥ 30) × Post × Male				-0.005 (0.007)				-0.003 (0.006)
1(#Emp ≥ 30) × Post × Male × High Edu. Manager				-0.013 (0.016)				-0.019 (0.012)
<i>N</i>	265279	146897	118328	265279	265279	146889	118312	265279
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Worker FE	No	No	No	No	Yes	Yes	Yes	Yes
Outcome Mean	2.749	2.684	2.829	2.749	2.749	2.684	2.829	2.749
% Highly Educated Managers	0.15				0.15			

Note: This table shows heterogeneity in difference-in-differences estimates by manager characteristics, mirroring specifications in Table 6 but using the Firm Sample. Panel A reports estimates by manager gender, and Panel B reports estimates by manager education. The shares of female and highly educated managers are shown for the pre-policy year 2004. All specifications include firm and year fixed effects. Standard errors are clustered at the firm level. Significance levels: * < 10% ** < 5% *** < 1%.

B. Effects on the Gender Wage Gap in Large Firms

To further explore the adjustment of the gender wage gap within and between job categories, we expand our analysis to larger firms with 30 to 250 employees, which we call the "*Large Firm Sample*". In this sample, all firms are affected by the transparency policy, but for firms with higher pre-policy gender wage gaps, the new transparency mandate can be expected to be more binding than for firms with lower initial inequality. This sample also allows us to examine whether the difference-in-differences results estimated across the 30-employee cutoff generalize to this broader firm population exposed to the policy. Motivated by this, we estimate the following triple-difference event-study regression:

$$y_{ijt} = \sum_{\substack{k=-5 \\ k \neq -1}}^5 \beta_k \mathbb{1}_{(t-2005=k)} \times \text{Firm-Occup GWG}_j^{\text{pre-reform}} \times \text{Male} + \rho X_{ijt} + \theta_j + \lambda_t + \gamma_i + \epsilon_{ijt}, \quad (2)$$

where y_{ijt} is log wage for worker i in firm j at time t ; $\mathbb{1}_{(t-2005=k)}$ are event-time dummies, k indexing years from the reform; Firm-Occup $\text{GWG}_j^{\text{pre-reform}}$ is the average within-occupation gender wage gap in firm j , calculated for the pre-reform years (2000-2004) based on wages of men and women in the same 1-digit occupation; λ_t absorbs time trends; θ_j includes firm-level fixed effects; and X_{ijt} includes the two-way interactions between event time and gender. In this framework, an estimate of β_k recovers the differential change in gender wage gaps, over k years after the reform, between firms with higher and lower initial gender inequality. For a firm with a 10 p.p. higher initial gender wage gap, the gender wage gap changes due to the reform by $\beta_k * 0.10$, compared to a firm with a 10 p.p. lower initial gap. In order to understand the role of worker composition, we also consider specifications with worker level fixed-effects γ_i . As before, we cluster standard errors at the firm level.

Column 3 in Panel A of Table A14 shows that, when not controlling for sorting, the within-firm gender wage gap narrowed by around 2.2 p.p. in a firm with 10 p.p. higher initial inequality. This effect is driven by a 1.1 p.p. increase in female wages and a 0.8 p.p. decrease in male wages. Controlling for sorting by including worker fixed effects mutes both the positive impact on female wages and the negative impact on male wages, but the estimates are statistically significant and the overall pattern is similar, with a 0.7 p.p. decline in the gender wage gap in a firm with 10 p.p. higher initial inequality (Columns 4-6). In Panels B and C of Table A14 we show a validation exercise of the design on the baseline *Firm sample*. For ease of comparison we transform the variable Firm-Occup $\text{GWG}_j^{\text{pre-reform}}$ into a dummy equal to one if the within-firm within-occupation cell is above the median. The coefficient of interest is on the quadruple diff-in-diff in Panel C. The coefficient is close to zero and statistically insignificant. In other words, we cannot directly validate that firms with higher pre-policy gender wage gaps in our baseline sample experience higher decreases in the gender wage gap post-reform, relative to firms with smaller pre-policy gender wage gaps. Nevertheless, we note that the within-firm within-1-digit-occupation cells in firms with 15-50 employees will be very small and hence lead to limited statistical power for calculating differences.

Figure A8 shows graphically the event-study estimates for Equation 2 conditioning on worker fixed effects. We observe very similar pre-trends between firms with high and low pre-policy

inequality (Panel A), lending credibility to this alternative research design and alleviating concerns about reversion to the mean among firms with high initial gender wage gaps. Event-study estimates separately for male and female wages show how higher wage growth for women relative to men generates the effects (Panel B).

Overall, the results for the large firms, which are based on a complementary source of variation, are consistent with our baseline results for gender wage gaps, suggesting that the policy led to the narrowing of gender wage gaps within firms. These estimates also suggest that the gender gap results estimated across the 30-employee cutoff extend to the wider population of firms exposed to the policy, some of which are outside the size window of our baseline sample.

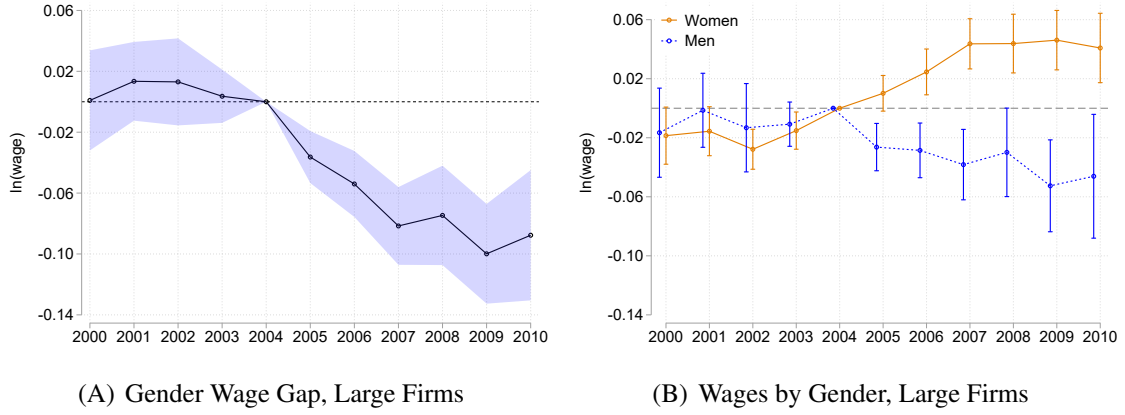


Figure A8: IMPACTS BY THE INITIAL GENDER WAGE GAP, LARGE FIRMS

Note: This figure shows the impact of the reform on large firms with 30-250 employees, based on Eq. 2. Panel A plots the estimate of β_k on the interaction term $\mathbb{1}_{(t-2005=k)} \times \text{Firm-Occup GWG}_j^{2004} \times \text{Male}$, where $\text{Firm-Occup GWG}_j^{2004}$ is the average within-occupation gender wage gap in the firm in the pre-reform years (2000-2004). For example, an estimate of -0.10 means that the gender wage gap declined by one percentage point more in a firm with ten percentage point higher initial gender wage gap ($-0.10 \times 0.10 = -0.01$). Panel B plots the corresponding estimates by gender for the interaction term $\mathbb{1}_{(t-2005=k)} \times \text{Firm-Occup GWG}_j^{2004}$. All estimations include firm, worker, and year fixed effects. Shaded areas (vertical lines) show the 95% confidence intervals.

Table A14: GENDER WAGE GAP IN LARGE FIRMS

	Firm FE			Firm FE + Worker FE		
	Women (1)	Men (2)	All (3)	Women (4)	Men (5)	All (6)
Panel A: log(Hourly Wage), Large Firms						
Firm-occup GWG $\times$ Post	0.110*** (0.013)	-0.080*** (0.016)	0.129*** (0.014)	0.045*** (0.007)	-0.027*** (0.009)	0.044*** (0.007)
Firm-occup GWG $\times$ Post $\times$ Male			-0.217*** (0.019)			-0.071*** (0.010)
Worker FE	No	No	No	Yes	Yes	Yes
Pre-Policy Gender Gap			-0.175*** (0.003)			
Observations	656,590	760,816	1,417,443	656,590	760,816	1,417,443
Panel B: log(Hourly Wage), Large Firms						
Post $\times$ High Firm-Occupation GWG (Pre)	0.029*** (0.004)	-0.018*** (0.005)	0.034*** (0.004)	0.017*** (0.002)	-0.004 (0.003)	0.017*** (0.002)
Post $\times$ High Firm-Occupation GWG (Pre) $\times$ Male			-0.053*** (0.005)			-0.020*** (0.003)
Worker FE	No	No	No	Yes	Yes	Yes
Observations	656,590	760,816	1,417,443	656,590	760,816	1,417,443
Panel C: log(Hourly Wage), Firm Sample						
$1(\#Emp \geq 30) \times$ Post	0.009 (0.006)	0.006 (0.012)	0.009 (0.006)	0.000 (0.005)	-0.008 (0.006)	0.000 (0.005)
$1(\#Emp \geq 30) \times$ Post $\times$ High Firm-Occupation GWG (Pre)	-0.002 (0.010)	0.003 (0.018)	-0.002 (0.010)	0.007 (0.007)	0.012 (0.008)	0.007 (0.007)
$1(\#Emp \geq 30) \times$ Post $\times$ Male			-0.001 (0.010)			-0.009 (0.006)
$1(\#Emp \geq 30) \times$ Post $\times$ High Firm-Occupation GWG (Pre) $\times$ Male			0.004 (0.016)			0.006 (0.008)
Worker FE	No	No	No	Yes	Yes	Yes
Observations	224,517	179,372	403,934	224,517	179,372	403,934

Note: This table shows difference-in-differences estimates for hourly wages by gender. Panels A-B show estimates for large firms (50-250 employees). We control for year and firm fixed effects in all specifications, and in Columns 4-6 we add worker fixed effects to control for sorting (estimates based on the Worker Sample are not affected by sorting in either model). The pre-policy gender wage gap Panel A Column 3 is calculated from a regression controlling for firm and year fixed effects, using the sample indicated by the panel title and restricted to the pre-reform years 2000-2004. The variable "Firm-occup GWG" in Panel A is the average within-firm, within-occupation gender wage gap, calculated for the pre-reform years 2000-2004 and based on wages of men and women in the same 1-digit occupation. The variable "High Firm-Occupation GWG (Pre)" in Panel B and E is a dummy equal to one if the within-firm, within-occupation gender wage gap was above the median in pre-reform years 2000-2004. Panel C shows estimates for the baseline sample (15-55 employees). Significance levels: * < 10% ** < 5% *** < 1%.