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# Women in Finance Conference 2018

## "Disclosing the Gender Pay Gap"

9:15 am -10:00 am

**Presenter:** Elena Simintzi, Assistant Professor of Finance, Kenan-Flagler Business School University of North Carolina

**Discussant:** Karin Thorburn, Professor of Finance at Norwegian School of Economics

# Disclosing the Gender Pay Gap*

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**Abstract**

We exploit a 2006 legislation change in Denmark that requires firms to provide gender dis-aggregated wage statistics to study the effect of transparency on gender pay disparities on employee and firm outcomes. Using detailed employee-employer administrative data we find that the policy has an effect in changing compensation within firms, through slowing the growth of compensation for male employees. Furthermore, we find that such changes in firm wage policies following the passage of the law are associated with negative outcomes on overall firm productivity.

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# I Introduction

Gender pay disparities characterize labor markets in most developed countries (OECD, 2015).[1] When a man earns 100 dollar, a woman earns 78.5 dollars in Germany, 79 dollars in UK and 83.8 on average across EU countries (Eurostat 2016). Recently many countries have introduced various forms of pay transparency as a method to reduce gender pay disparities. In the United Kingdom, employers of firms with more than 250 employees have to publish gender based wage statistics from April 2018. In Germany, employees have the right to know median salary for a group of comparable employees in firms with more than 200 employees. In Iceland, firms with more than 25 employees must obtain a gender wage equality certificate that documents that men and women receive the same wage for the same work. An executive order signed by the U.S. government in 2016 required large companies to report salary data broken down by gender, starting in 2018. Evidence on the effect of transparency on gender pay disparities on employee and firm outcomes is limited. In this paper, we fill this void and document real effects of increased transparency of within firm gender pay disparities on firm wage policy and outcomes.

There is an ongoing heated debate about such transparency policies. Governments propose transparency as a tool to inspire firms to reduce the wage gap between men and women. Transparency on gender pay comes as a challenge to firms as information disclosure on pay is not a common practice and is often discouraged.[2] [3] Employers, who are against transparency, argue that it is a huge burden on firms to produce

[1] http://www.oecd.org/gender/data/genderwagegap.htm

[2] According to survey evidence, 62% of women and 60% of men working for private employers in the U.S. report that wage and salary information is secret and about half of all workers (51 percent of women and 47 percent of men) report that the discussion of wage and salary information is either discouraged or prohibited and/or could lead to punishment (Institute for Women's Policy Research (IWPR, 2014)

[3] Google reacted negatively to an initiative by a few Google's employees to share their salaries through an internal, online system: http://www.payscale.com/career-news/2015/08/what-you-should-know-about-pay-transparency-in-the-workplace

the statistic, that it is essentially an example of naming and shaming firms, that it violates the privacy of individual workers and that wage determination shall be left to employers and employees and not be dictated by governments. The transparency proposals are also criticized from groups that work for more gender wage equality as being symbolic in the sense that few firms are covered and there are no consequences for firms that reveal gender wage differences.

While some studies have examined how information on peers' earnings can affect individuals' job satisfaction (Card et al. (2012); Breza et al. (2018)) and the compensation structure in organizations (Mas & Moretti (2009)), most of these papers do not link wage transparency to firm outcomes or they derive conclusions from the public sector. Therefore, little is known how pay transparency can affect firm compensation policies and firm outcomes.

The effect of disclosing gender pay disparities on male and female employee compensation is an open empirical question. Such disclosure might provide incentives for managers to close the gender wage gap by increasing female employee wages or by reducing male employee compensation. Alternatively, disclosure might provide insufficient incentives to change firm compensation policies.

The effect of this disclosure on firm outcomes is also a priori ambiguous. On the one hand, such information may be detrimental to employee productivity since it can lower job satisfaction for those employees paid below their reference group. Employee productivity can also be affected if the reduction in the gender wage gap is achieved by reducing the wage growth rate of male employees. Moreover, the potential for an increase in turnover from dissatisfied employees might be another cost the firm could bear. On the other hand, the reduction in wage disparities might create a sentiment of fairness among workers leading to improved employee productivity and firm outcomes.

Empirical investigation of pay transparency rules as a measure to reduce pay discrimination within firms is challenging as it requires finding variation in transparency

at the firm level as well as data on employee wages. To address these difficulties, we use data at the employee-firm level from the Danish Statistics matched employee-employer administrative dataset and exploit a 2006 legislation change in Denmark that requires firms with more than 35 employees to report salary data broken down by gender for employee groups large enough so that anonymity of individuals can be protected. Under the 2006 law, firms have the duty to inform all their employees of wage gaps between men and women and explain the design of the statistics and the wage concept used. Alternatively, the firm may chose not to provide statistics but instead to discuss an action plan to address wage disparities with employee representatives.

To estimate the effect of the law on various firm outcomes, we employ a difference-in-difference approach. Our treated firms are firms that employ 35-50 employees on average from 2003 to 2005, the years prior to the introduction of the law, and the control firms are those that employ 20-34 workers on average. We then compare the change in employee and firm outcomes around the passage of the law for treated firms relative to control firms.

Using detailed worker-firm data, we show that wages of male employees in treated firms are lower by 1.3% relative to male employees in control firms and this reduction in wages is statistically significant at the 1% level. On the contrary, we find no statistically or economically significant relation for female workers in treated relative to control firms. These regressions include firm-employee fixed effects, thereby exploiting variation within a given employee at a given firm, and year fixed effects.

In interpreting the magnitude of these results, note that, in the first few years following the passage of the law, compliance was far from perfect. According to Holt and Larsen (2011), only 1/3 of firms that were required to comply actually did so. It is important to note that we include in the treatment group all the firms with more than 35 employees and do not restrict this group to the firms that actually complied. Effectively, we perform an intention to treat analysis. Therefore, the differences

we observe between treatment and control groups can be attributed to direct effect of compliance of the subset of firms that did so and perhaps to some anticipatory measure that non-compliers took.

These results suggest that policies encouraging pay transparency against gender pay discrimination are effective in changing compensation within firms. We show that adjustments in workers' compensation take effect through relatively lower pay of male employees. We next show that the information disclosure on gender pay differences and the resulting changes in relative pay within firms have also an effect on employee reallocation. Using the same empirical design at the firm level, we show that treated firms experience an increase in the joining rate of women employees as compared to control firms in specifications that include firm and year fixed effects.[4]

These changes in wages and hiring decisions following the passage of the law seem to affect employee productivity. We show that treated firms have lower productivity (measured as the logarithm of sales over assets or the logarithm of sales over employees) relative to controls following the passage of the law. The results are statistically and economically significant: treated firms have lower sales to asset ratio (sales over employee) by 3.4% (2.3%) as compared to control firms. This negative effect on firm productivity suggests that lower wage growth for men can hurt their performance.

The key assumption implicit in the above argument is that, conditional on controls, treated and control firms are effectively randomly assigned. To empirically support our argument that there are no systematic differences between our treated and control firms, we conduct several tests. First, we compare several firm characteristics for our treated and control samples for 2005 and show that they are similar pre-treatment. Second, we perform a dynamic analysis and find no evidence of pre-treatment trends. Third, we create placebo tests using alternative employee size cutoffs to define treatment.

[4]Note the changes in wages we observe are not driven by changes in employee composition at a given firm as they are estimated in a panel of employees-firms using within employee-firm variation.

The paper contributes to a growing literature studying how wage disparities within the firm may affect employee or firm outcomes. Card et al. (2012) with a sample of California government workers and Breza et al. (2018) using a sample of workers in an Indian manufacturing plant show that knowing how much peers are earning, relative to your own salary, might generate negative feelings and reduce job satisfaction. Mueller et al. (2017) find that firms with higher pay inequality exhibit larger equity returns suggesting that differences in pay inequality across firms are a reflection of differences in managerial talent.

The paper also adds to the literature on the sources of gender pay disparities. Card et al. (2015) explore the effects of the firm-specific pay premium on gender wage gap using Portuguese worker-firm data. Blau & Kahn (2003) analyze 22 countries and find that higher male wage compression and lower female net labor supply are associated with lower gender pay gaps. Del Bono & Vuri (2011) using Italian data find that a significant component of the gender wage gap might derive from the different wage returns to job changes between genders.

# II The Law

On June 9th 2016 Denmark adopted Act no. 562 of 9 June 2006 which created requirement for firms to report gender based dis-aggregated statistics. The goal of the law was "to promote visibility and information about wage differentials." The law stated that an employer with a minimum of 35 employees and at least 10 employees of each gender within a 6-digit DISCO code shall each year prepare gender-segregated wage statistics for the purpose of consulting and informing the employees of wage gaps between men and women in the firm.[5] The statistics must be made available to the employees through the employee representatives. The new provisions came into force on 1 January 2007. In order to facilitate the reporting from companies, if

[5]The requirement does not extend to companies in the fields of farming, gardening, forestry and fisheries.

the company so wishes, the Statistical Bureau would process the data and deliver a gender-specific statistic for the firm.[6] The costs of the Statistical Bureau's processing of the data are paid by the Ministry of Employment.

The law also offers an alternative choice to employers by permitting to replace gender based wage statistics with an internal report on equal pay. This report is to contain a description of the conditions that are important for determining the wage and establish an action plan for equal pay for work of equal value to be implemented within a three-year period. Yet, according to Holt and Larsen (2011), very few (almost none) of the firms implemented this alternative.

## II.1 Timing and implementation of the Law

Passage of the law was unexpected. In the spring of 2001, the then social democratic government proposed an amendment of the existing Danish Equal Pay Act with a goal to introduce a duty for employers with more than 10 employees to produce gender-specific wage statistics for the firm. The Act was passed in June 2001, but it was provided that the provisions on a duty to provide gender-specific information should not come into force until 1 July 2002 in order to give the employers a possibility to prepare for fulfilling the duty. However, the government changed in Spring 2002 and the new right-wing government never implemented the law. They argued that it would increase the burden of administration for firm managers and that it did not protect individual workers, since their wages could be exposed in the dis-aggregated statistic in particular in smaller firms. Members of the new government also expressed the view that firms wage policy is a firm matter and the government should not be involved in that. As a way of postponing the law, it was left to the discretion of the Minister for Employment to decide when (and if) the provision should come into force.

[6]There is already a duty on the part of the employers to report their wages to Denmark's Statistical Bureau for statistical purposes, partly because of Denmark's duty to provide statistical information to the EU.

However, on December 7, 2005, the Minister for Employment submitted a proposal to Parliament to amend the Equal Pay Act. The proposal was adopted by Act no. 562 of 9 June 2006, thus amending the Equal Pay Act. The proposal was submitted two months before a general election and took most observers by surprise. It was generally viewed as an attempt of the government to get a better standing among female voters.

From January 1, 2007, all firms that satisfied the criteria of the law should either provide the gender based wage statistic or prepare the report on gender wage to be shared with the employees.

# III Empirical Design

To estimate the effect of the law on various employee and firm outcomes, we employ a difference-in-difference approach. Our treated firms are firms that employ 35-50 employees the year prior to the introduction of the law and the control firms are those that employ 20-34 workers. We then compare the change in firm outcomes around the passage of the law for treated firms relative to control firms. The key assumption is that firms in the control group are a good counterfactual for those in the treated group. That is, firm outcomes of the control group can be taken as the outcomes of the treated group in the absence of the law. We provide several tests below to ensure that this assumption is likely to hold in our setting.

We note that, in the first few years following the passage of the law, compliance was far from perfect. The implementation of the law was evaluated in a report in 2011 (Holt and Larsen 2011). The authors use numbers from 2009. The report estimates that 2,497 firms satisfied the conditions of the law. The authors surveyed approximately 20% of these firms and found that only a third of the firms provide gender-based wage statistic data to Statistic Denmark and almost no firms used the alternative option of making an internal report. Yet, we include in the treatment group all the firms with more than 35 employees and do not restrict this group to the

firms that actually complied. Effectively, we perform an intention to treat analysis. Therefore, the differences we observe between treatment and control groups can be attributed to direct effect of compliance of the subset of firms that did so and perhaps to some anticipatory measures that non-compliers in the treatment group took.

There are two criteria that firms need to satisfy to be subject to the requirements of the law. The first criterion is that firms need to employ more than 35 people. The second criterion is that firms have at least 10 male and 10 female employees in one single 6-digit DISCO code. While at first it might be seem that the appropriate way to construct the treatment group is to include only firms that satisfy both criteria, we decided to include all firms with more than 35 employees. The reason is that many firms that did not satisfy the second criterion still complied with the law. It seems that it was difficult for firms to determine whether they had 10 male and 10 females employees in the same disco code as, according to surveys, firms did not have disco code information or were confused about it. In addition, this is consistent with the description of the law in publications by law firms, the EU, or even academics, in which only the requirement of the threshold of 35 employees is mentioned.[7] We gathered additional data to test this explanation. DA, the main employer association in Denmark, provided us with a list of firms that complied with the law. DA has this list as it was tasked by the Danish government to help its members comply with the law. In this list, all firms that complied have more than 35 employees, yet 35% of these compliers did not satisfy the second criterion. Given these facts, constructing the treatment and control groups using both criteria will include a large number of compliers in the control group. Using the 35 employee criterion and using an intent to treat analysis is free of these problems.

[7]ILO describes the law as:"employers employing 35 or more workers are required to prepare annually gender-disaggregated statistics or, alternatively, an equal pay report and action plan"; European commission directorate for internal policies issued a report on policies on Gender Equality in Denmark describing the law:"Since 2007, companies with 35 employees or more should carry out gender-disaggregated pay statistics and elaborate status reports on the efforts to promote equal pay in the workplace."(European Commission 2015)

We use a panel of employee-firm-years to test whether revelation of information on wages has real effects on firms' compensation policies. We, thus, compare firms with employees just above the employee threshold defined by the law with those just below the threshold. We estimate OLS regressions of the following form:

$$\begin{aligned} log(wage)_{ijt} &= \alpha_{ij} + \alpha_t + \gamma X_{it} + \beta_1 I(Treated_{ijt}) + \beta_2 I(Post_{ijt}) \\ &+ \beta_3 I(Male_{ijt}) + \beta_4 I(Treated_{ijt} \times Post_{ijt}) \\ &+ \beta_5 I(Treated_{ijt} \times Male_{ijt}) + \beta_6 I(Post_{ijt} \times Male_{ijt}) \\ &+ \delta\, I(Treated_{ijt} \times Post_{ijt} \times Male_{ijt}) + \varepsilon_{ijt} \end{aligned} \tag{1}$$

where i, j, and t index firms, individuals and years; post takes a value of 1 for a two-year period following the law; $X_{it}$ captures time-varying firm-level control variables. The coefficient of interest $\delta$ captures the differential effect of the law on wages of male and female individuals at a given firm. Standard errors are clustered at the firm level. We start our sample in 2004 to provide sufficient years to estimate the baseline effect for each firm-employee and end in 2008 to avoid any overlap of our sample with the financial crisis which had economy-wide effects on wages.

Having established the effect of the law on wages, we next examine the effect of the law on firm productivity and hiring decisions. Using a panel of firm-years, we estimate OLS regressions of the following form:

$$\begin{aligned} Y_{it} &= \alpha_i + \alpha_t + \gamma X_{it} + \beta_1 I(Treated_{it}) + \beta_2 I(Post_{it}) \\ &+ \delta\, I(Treated_{it} \times Post_{it}) + \varepsilon_{it} \end{aligned} \tag{2}$$

where i, and t index firms and years; post takes a value of 1 for a two-year period following the law; $X_{it}$ captures time-varying firm-level control variables. The coefficient of interest $\delta$ captures the differential effect of the law on the dependent variables. Standard errors are clustered at the firm level.

# IV Data and Sample Description

## IV.1 Data sources

Our main dataset is the matched employer-employee dataset from the Integrated Database for Labour Market Research (IDA database) at Statistics Denmark. In addition to the employer's identification number (CVR), and employee identification number (CPR), the IDA dataset contains employees' wage, demographic information, such as age and gender and the employee's position in the organization. The position in the firm is based on the Danish occupational code (DISCO), defined based on the international standard classification of occupations (ISCO). We use data for the period 2004 to 2008.

This information is combined with firm-level outcomes from the Danish Business Register. This dataset covers all firms incorporated in Denmark and includes the information these firms are required to file with the Ministry of Economics and Business Affairs, including the value of total assets, number of employees and revenues. Even though most firms in this dataset are privately held, external accountants audit firm financial information in compliance with Danish corporate law. We link information in the firm-level dataset to the the matched employer-employee dataset using the firm identifier (CVR number).

## IV.2 Sample construction and summary statistics

We start with the universe of public and private limited liability firms in Denmark and their employees included in the Integrated Database for Labour Market Research. For ease of comparison, for the employee level outcomes we focus on full-time workers.

Table 1 presents summary statistics for the treated and control firms in our sample over the 2003-2005 period. Panel A presents employee level characteristics and Panel B presents firm level characteristics. The average wage for employees in the control group is 237,283 DKK (39,370 USD), while for treated firms is 242,540 DKK (40,242

USD). The average employee in the sample is 36 years old in both treated and control groups. Treated and control groups are also similar in terms of the gender composition of their employees with 66% male employees on average. Treated firms are larger than control firms in terms of assets, sales and employees as by construction treated firms are chosen to be firms with 35-50 employees pre-treatment as compared to 20-34 employees for control firms. Indeed, the average treated firm has 41 employees pre-treatment, assets of 44,662,000 DKK (7,412,000 USD) and sales of 75,186,000 DKK (12,477,000 USD) as compared to 25 employees, 31,504,000 DKK (5,228,000 USD) in assets and 49,708,000 DKK (8,249,000 USD) in sales for control firms. However, firms are similar in terms of their productivity pre-treatment with sales over asset (sales over employee) equal to 2.51 (2,082 DKK) for control firms and 2.39 (2,025 DKK) for treated firms.

# V Results

## V.1 Wages

In this section, we examine whether the law has an effect on firm compensation policies consistent with the intended consequence of "fixing" gender wage disparities within firms. We use detailed employee-firm wage data to avoid the possibility that compositional changes at the firm level affect the observed differences in wages and we estimate the effect of information revelation on gender pay disparities on wages of a given individual within a given treated firm as compared to individuals in control firms.

Table 2 presents the results of the OLS estimation. In all our regressions, we include firm-individual fixed effects to control for firm and individual specific characteristics and the match between firms and employees and year fixed effects to capture macroeconomic shocks. Column 1 compares the effect of the law on wages of male employees in treated firms as compared to male employees in control firms. Column

2 repeats this analysis comparing wages for female employees. We find that wages of male employees in treated firms grow relatively less as compared to male employees in control firms following the passage of the law. The effect is statistically significant at the 1% level and economically important. Treated male wages are reduced by 1.13% more relative to male wages in the control group. On the contrary, we find no differential effect on female wages between treated and control firms in column 2. To test whether the wage growth of male and female employees are statistically different from each other, we perform a triple differences estimation in column 3. We thus compare the effect of the law on wages of male employees as compared to wages of female employees following the passage of the law relatively to a group of controls. The triple differences coefficient is negative and statistically significant at the 5% level showing a reduction on wages of male employees as compared to female employees following the passage of the law.

We repeat our estimation in columns 4-6, where we additionally control for firm size (proxied by logarithm of sales) to control for the well documented employer size-wage effect (e.g. Brown & Medoff (1989); Idson & Oi (1999)). Note the employer size-wage effect would predict a positive effect for wages of larger firms (indeed the coefficient for sales is positively correlated with wages and significant at the 1% level)—the opposite from our findings that the law negatively affects wage growth for larger firms. The estimated coefficients remain virtually unchanged after controlling for firm size.

Our results are robust to including industry times year fixed effects in our regression specifications to control for changes in investment opportunities (Table A1). Although bonuses consist of only 1.7% of the average worker's total compensation, we further examine whether the reduction in base level wages we capture can be partially offset by relatively higher bonuses offered to male workers in treated firms. Thus, in columns 4-6, Table A2, we use bonuses as our dependent variable and fail to capture any statistical significance on bonuses.

Given the law does not directly apply to employees paid according to performance, we repeat our analysis examining the effect of the law on gender pay differentials at three hierarchy levels within the firm. Higher and intermediate level hierarchies tend to include more employees paid on performance, while lower level hierarchies tend to include blue collar workers whose compensation depends exclusively on hourly wages. Columns 1-3, Table 3, show the results for employees in the highest hierarchy level; columns 4-6 present results for the middle hierarchy level; columns 7-9 show results for the lowest hierarchy level. It can be observed that there is no significant relationship on pay for either men or women in hierarchy levels 1 or 2, while there is a strong and significant effect for hierarchy level 3.

## V.2 Robustness

Our difference-in-differences analysis is subject to the common criticism that omitted variables and reverse causality pose a potential challenge to identification. In this section, we discuss several pieces of evidence to address such concerns.

First, the fact that Table 2 shows that our estimated effect on wages is concentrated at male employees (as opposed to all employees) mitigates identification concerns. Furthermore, Table 3 shows that the estimated effect on male wages is driven specifically by the lowest hierarchy level in the firm and not by *any* hierarchy level, consistent with the fact that the law is more likely to apply to employees compensated based on hourly wages. To drive our findings, an omitted variable would not only need to be correlated with male wages, but also differentially affect male wages across different firm-level hierarchies.

Next, Table 4 shows year-by-year coefficients for male (column 1) and female (column 2) employees before and after the passage of the law and find no significant difference in the evolution of wages at treated and control groups prior to the adoption of the law. Column 3 presents year-by-year estimates of the triple interaction coefficients and also shows that male wage growth is significantly lower in 2007 and

2008 by 1.2% and 4%, respectively, as compared to female wage growth in treated versus control firms, while there is no significant difference pre-treatment.

In order to further alleviate the concern that an omitted variable differentially lowers male wages at larger firms, we create placebo tests where we use alternative employee size cutoffs to define treatment. In columns 1-3, Table 5, we define placebo treated firms to be firms with 50-65 employees in the 2003-2005 period and placebo control firms those firms with 35-49 employees. In column 4-6, we use 65 employees as the cutoff and thus, placebo treated firms are those firms with 65-80 employees pre-treatment and placebo control firms are firms with 50-64 employees pre-treatment. In columns 7-9, we instead use a cutoff of 80 employees and thus placebo treated firms are those firms with 80-95 employees pre-treatment and placebo control firms are firms with 65-79 employees. We are unable to replicate our baseline findings when considering these alternative cutoffs, consistent with the fact that the effect is unique to the 35 employee cutoff as described by the law.

As noted earlier, in our baseline results we focus on firms above 35 employees and we ignore the law requirement that firms should have at least 10 male and 10 female workers in a given 6-digit DISCO code, based on the fact that a lot of compliers do not seem to satisfy this criterion. As such, the magnitudes can be attributed to the direct effect of compliance through reporting gender wage statistics as well as to some anticipatory measures that non-compliers took. As an additional robustness result, we consider a list of firms that report gender based wage statistic to the Danish Employer Association (DA) and Statistic Denmark (DST) in 2007 and 2008. These are firms that we know that complied with the law. Figure 1 plots the male wage premium by year for the list of firms that report to DST. Consistent with our baseline results, we observe a decline in the male wage premium post-2006 that is also statistically significant, while there is no differential effect between wages of male and female employees of firms that reported to DST in the years prior to the passage of the law.

# VI Firm Productivity

Having established the effect of the law on "fixing" gender wage differentials , we next examine whether these changes in firm wage policy may affect employee productivity. The effect on employee productivity is not *a priori* unambiguous. On the one hand, reducing wage growth of male employees in treated firms as compared to control firms might negatively impact productivity of male workers who see their pay being reduced relative to the years prior to the law or relative to comparable employees in control firms (Akerlof & Yellen (1990); Campbell III & Kamlani (1997)). This negative effect should be even stronger if male workers' higher wages reflect higher productivity rather than biases shown to often lead to pay differentials.

On the other hand, enhanced transparency within the firm may have positive effects on firm outcomes if employees feel transparency and the subsequent effects on wages creates a fairer workplace. Even if positive productivity effects are confined to females' higher efforts, there can be positive spillover effects through the positive impact of the more productive employees on the productivity of their peers (Kandel & Lazear (1992); Mas & Moretti (2009)).

In the absence of data on individual employee productivity, we examine the overall effect of the law on firm productivity. Thus, our estimates capture the net effect of the law on productivity as we cannot rule out that the law may differentially affect productivity of males versus females. Table 8 presents the results. In columns 1-2, we define firm productivity using the logarithm of sales normalized by assets. Column 1 controls for firm and year fixed effects; column 2 additionally controls for firm size to control for the fact that productivity may follow differential trends in firms of different sizes. The effect on productivity is negative, statistically significant at the 1% level and economically large. Following the passage of the law, the logarithm of sales over asset in treated firms is 3.4% lower as compared to control firms. In columns 3-4, we repeat these estimations defining productivity as sales over employees (log-transformed). Our results are robust to using this alternative measure of productivity.

In Table 9, we show that there are no pre-treatment trends in sales over assets (columns 1-2) or in sales over employees (columns 3-4). On the contrary, there is a significant negative effect on productivity in 2007 and 2008, namely the same years for which we observe the decline in wage growth for male employees. Moreover, Table 10 shows we cannot replicate the same negative effects in productivity when we consider a cutoff of 50 employees in a placebo test, instead of 35 in our baseline analysis.[8]

# VII Firm Employee Composition

We showed that revelation of information on gender pay differentials has real effects on firms' wage policies. In this section, we examine whether fairness in pay across genders also affects employee composition.

To study changes in workers' gender composition, we examine the within firm share of female employees in firms affected by the law as compared to control firms. Table 6 presents the results. In column 1, we control for firm and year fixed effects but no other controls; column 2 additionally controls for firm size as gender reallocation may be easier in larger firms. The diff-in-diff coefficient is positive and significant both statistically and economically. Female share increases by 4.4% in treated firms as compared to control firms following the passage of the law. These results suggest that the law has important reallocation effects with treated firms showing a relatively higher preference for female employees over males.

Furthermore in Table 7 we explore the joining and leaving rates of female employees. We find that the higher female participation in treated firms seems to be driven by increased joining rates for females employees in treated firms. We do not find a change in leaving rates. Moreover, in unreported regressions we repeat the analysis for male employees and we do not find any change in joining or leaving rates for male

[8]In unreported regressions, we show these results are robust to considering 65 or 80 employees as the employee size cutoff.

employees in treated firms.

# VIII Conclusion

Reducing the gender pay gap has been at the epicentre of a heated debate among academics and policy makers. Recently, governments around the world proposed transparency as a tool to inspire firms to reduce the wage gap between men and women. Nevertheless, there is no systematic study that examines the effects of increased transparency of within firm gender pay disparities on firm wage policy and outcomes.

Investigating empirically the effect of pay transparency rules as a measure to reduce pay discrimination within firms is challenging as it requires finding variation in transparency rules but also having detailed information of employee wages. We overcome these hurdles by exploiting a 2006 regulation in Denmark that required companies with a minimum of 35 employees and at least 10 employees of each gender within the same occupational category to report gender-segregated wage statistics. Using detailed employee-firm matched administrative data and using a difference-in-difference methodology we find changes in compensation within firms. Specifically male employees experience slower wage growth relative to female employees. Furthermore we find that increased transparency led to higher share of female employees. Finally we examine the effects of these changes on firm productivity. We find that affected firms have lower productivity relative to control firms. Our results contribute to the debate on the effect of gender pay gap transparency on gender pay inequality within firms.

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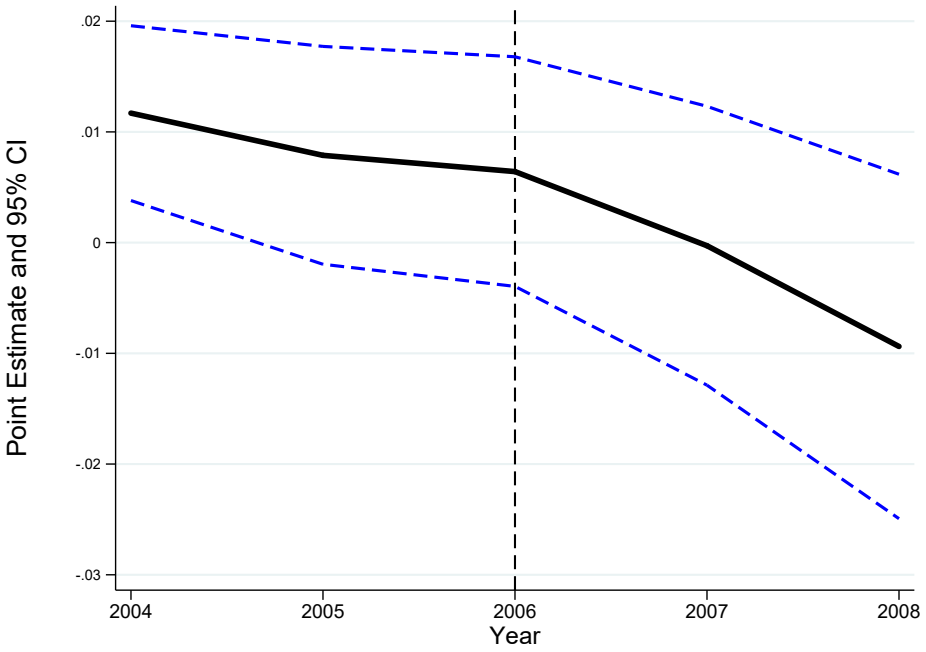
Figure 1: Gender Gap Revelation and the Subsequent Male Wage Premium




*Note:* This figure illustrates the male wage premium by year, defined as $Male \times Year$ in the following regression: $log(Wage) = \sum \beta_t \left(Male \times Year_t\right) + \theta\, log(Sales)_t + \varepsilon$. The sample includes the group of firms that appear in the DST list from 2007 to 2008, excluding CEOs and board members, firms in industries unaffected by the policy, and firms that have one or fewer average pre-treatment male employees or female employees. The blue dashed lines denote the 95% confidence interval

Table 1: Summary Statistics

**Employee-Level Characteristics**

| | Control | | | | | | Treated | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Observations | Mean | S.D. | P25 | Median | P75 | Observations | Mean | S.D. | P25 | Median | P75 |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Wage (DKK) | 106,256 | 237,283 | 156,809 | 119,140 | 241,666 | 320,607 | 84,262 | 242,540 | 159,066 | 125,861 | 246,018 | 324,149 |
| Bonus (DKK) | 66,291 | 5,865 | 15,268 | 0 | 0 | 3,667 | 55,814 | 5,385 | 14,852 | 0 | 0 | 2,953 |
| Income (DKK) | 106,241 | 235,765 | 150,503 | 119,699 | 240,469 | 318,347 | 84,249 | 240,662 | 151,696 | 126,640 | 244,365 | 321,662 |
| Age | 103,101 | 36.15 | 12.76 | 26 | 35 | 45 | 82,117 | 36.6 | 12.76 | 26 | 36 | 46 |
| Male | 106,017 | 0.660 | 0.474 | 0 | 1 | 1 | 84,136 | 0.661 | 0.474 | 0 | 1 | 1 |

**Firm-Level Characteristics**

| | Control | | | | | | Treated | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Observations | Mean | S.D. | P25 | Median | P75 | Observations | Mean | S.D. | P25 | Median | P75 |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Assets (DKK thousands) | 2,880 | 31,504 | 60,020 | 9,395 | 17,829 | 30,827 | 1,410 | 44,662 | 60,938 | 16,987 | 30,013 | 49,703 |
| Sales (DKK thousands) | 2,941 | 49,708 | 58,169 | 20,330 | 31,332 | 56,637 | 1,446 | 75,186 | 70,090 | 35,375 | 52,238 | 89,888 |
| Sales per Asset | 2,831 | 2.51 | 3.76 | 1.30 | 1.99 | 2.92 | 1,411 | 2.39 | 2.33 | 1.30 | 1.89 | 2.81 |
| Sales per Employee (DKK thousands) | 2,941 | 2,082 | 3,097 | 844 | 1,249 | 2,300 | 1,446 | 2,025 | 4,920 | 894 | 1,279 | 2,186 |
| Employment | 2,999 | 25.37 | 4.31 | 22 | 25 | 28.7 | 1,448 | 40.7 | 5.05 | 37 | 40.5 | 44.7 |
| Female Share | 2,999 | 0.325 | 0.218 | 0.137 | 0.269 | 0.481 | 1,448 | 0.328 | 0.232 | 0.127 | 0.277 | 0.489 |

*Notes:* This table reports the summary statistics for the variables considered in the paper. The employee-level variables are averaged over the pre-treatment years 2003-2005. The firm-level variables are also averaged over 2003-2005. The variables with Assets, Sales, and Sales per Employee are reported in thousands.

Table 2: Gender Gap Revelation and Subsequent Employee Wages, All Employees

| **Panel A** | | | | | | |
|---|---|---|---|---|---|---|
| | Male | Female | All | Male | Female | All |
| Treated × Post | 0.00439 | -0.0289*** | -0.0291*** | -0.000640 | -0.00920 | -0.0111 |
| | (0.00580) | (0.00976) | (0.00966) | (0.00432) | (0.00739) | (0.00738) |
| Male | | | 0.196*** | | | 0.197*** |
| | | | (0.00738) | | | (0.00504) |
| Male × Post | | | -0.0270*** | | | -0.0177*** |
| | | | (0.00677) | | | (0.00517) |
| Treated × Male | | | -0.0264** | | | -0.0117 |
| | | | (0.0120) | | | (0.00825) |
| Treated × Post × Male | | | 0.0331*** | | | 0.0109 |
| | | | (0.0100) | | | (0.00785) |
| log(Sales) | | | | 0.0222*** | 0.0193*** | 0.0206*** |
| | | | | (0.00612) | (0.00746) | (0.00541) |
| Education level | | | | 0.221*** | 0.242*** | 0.227*** |
| | | | | (0.00702) | (0.00785) | (0.00615) |
| Observations | 403241 | 195701 | 598968 | 377895 | 180853 | 558780 |
| $R^2$ | 0.303 | 0.331 | 0.318 | 0.493 | 0.532 | 0.515 |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Age Control | No | No | No | Yes | Yes | Yes |
| Exp. Control | No | No | No | Yes | Yes | Yes |
| No. Firms | 4227 | 4210 | 4239 | 4220 | 4198 | 4233 |

| **Panel B** | | | | | | |
|---|---|---|---|---|---|---|
| | Male | Female | All | Male | Female | All |
| Treated × Post | -0.0113*** | 0.00122 | 0.00130 | -0.0105*** | 0.00124 | 0.00182 |
| | (0.00376) | (0.00547) | (0.00545) | (0.00357) | (0.00521) | (0.00519) |
| Male × Post | | | -0.00621 | | | -0.00647* |
| | | | (0.00388) | | | (0.00387) |
| Treated × Post × Male | | | -0.0127** | | | -0.0125** |
| | | | (0.00590) | | | (0.00570) |
| log(Sales) | | | | 0.0274*** | 0.0237*** | 0.0261*** |
| | | | | (0.00609) | (0.00453) | (0.00463) |
| Observations | 325552 | 147295 | 472847 | 320470 | 144076 | 464546 |
| $R^2$ | 0.910 | 0.901 | 0.910 | 0.894 | 0.886 | 0.895 |
| Person-Firm FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Age Control | No | No | No | Yes | Yes | Yes |
| Exp. Control | No | No | No | Yes | Yes | Yes |
| No. Firms | 4011 | 3985 | 4020 | 4005 | 3980 | 4016 |

*Notes:* This table reports the effects of gender-separated wage revelation on employee wage. The standard errors are clustered at the firm-level. The firms with average employment between 35 and 50 in the pre-treatment period 2003-2005 are identified as treated, and firms with 20-34 are identified as the control group. Post = 0 for years 2004-2006, and Post = 1 for years 2007-2008. The above regressions exclude CEOs and board members, firms in industries unaffected by the policy, and firms that have one or fewer average pre-treatment male employees or female employees. Education is defined as a binary variable that is one if the employee is a college graduate. Age controls and Experience controls are included as a categorical variable divided into four quartiles. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 3: Gender Gap Revelation and Subsequent Employee Wages by Hierarchy

| | High-level | | | Intermediate-level | | | Low-level | | |
|---|---|---|---|---|---|---|---|---|---|
| | Male | Female | All | Male | Female | All | Male | Female | All |
| Treated × Post | -0.00581 | -0.00822 | -0.00671 | -0.00212 | 0.00147 | 0.00233 | -0.0134*** | 0.00372 | 0.00404 |
| | (0.00763) | (0.0150) | (0.0148) | (0.00686) | (0.00771) | (0.00770) | (0.00396) | (0.00656) | (0.00653) |
| Male × Post | | | -0.0145 | | | -0.00878 | | | -0.00610 |
| | | | (0.0106) | | | (0.00709) | | | (0.00472) |
| Treated × Post × Male | | | 0.000744 | | | -0.00475 | | | -0.0176** |
| | | | (0.0157) | | | (0.00971) | | | (0.00705) |
| log(Sales) | 0.0188*** | 0.0215 | 0.0195*** | 0.0134 | 0.00843 | 0.0118 | 0.0362*** | 0.0282*** | 0.0334*** |
| | (0.00541) | (0.0147) | (0.00634) | (0.0129) | (0.00562) | (0.00891) | (0.00522) | (0.00625) | (0.00437) |
| Observations | 30910 | 8070 | 38980 | 55299 | 29809 | 85108 | 240689 | 103528 | 344217 |
| $R^2$ | 0.878 | 0.853 | 0.878 | 0.865 | 0.833 | 0.865 | 0.890 | 0.883 | 0.892 |
| Person-Firm FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Age Control | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Exp. Control | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| No. Firms | 2530 | 1063 | 2666 | 2666 | 2362 | 3096 | 3868 | 3843 | 3987 |

*Notes:* This table reports the effects of gender-separated wage revelation on employee wage, by employee position in the firm. The sample restrictions and variable definitions follow Table 2. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 4: Gender Gap Revelation and Subsequent Employee Wages, Treatment by Year

| | Male | Female | All |
|---|---|---|---|
| Treated × $\text{Year}_{2005}$ | 0.000923 | 0.000101 | 0.00230 |
| | (0.00347) | (0.00558) | (0.00463) |
| Treated × $\text{Year}_{2006}$ | -0.00267 | -0.00843 | -0.00146 |
| | (0.00410) | (0.00637) | (0.00530) |
| Treated × $\text{Year}_{2007}$ | -0.00827* | -0.00352 | 0.000538 |
| | (0.00490) | (0.00713) | (0.00606) |
| Treated × $\text{Year}_{2008}$ | -0.0159*** | -0.00142 | 0.0158** |
| | (0.00562) | (0.00856) | (0.00748) |
| log(Sales) | 0.0275*** | 0.0237*** | 0.0261*** |
| | (0.00609) | (0.00453) | (0.00463) |
| Male × Treated × $\text{Year}_{2005}$ | | | -0.00222 |
| | | | (0.00469) |
| Male × Treated × $\text{Year}_{2006}$ | | | -0.00419 |
| | | | (0.00545) |
| Male × Treated × $\text{Year}_{2007}$ | | | -0.0102* |
| | | | (0.00596) |
| Male × Treated × $\text{Year}_{2008}$ | | | -0.0386*** |
| | | | (0.00720) |
| Observations | 320470 | 144076 | 464546 |
| $R^2$ | 0.894 | 0.886 | 0.895 |
| Person-Firm FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Age Control | Yes | Yes | Yes |
| Exp. Control | Yes | Yes | Yes |
| No. Firms | 4005 | 3980 | 4016 |

*Notes:* This table reports the $Treated \times Year$ effects for years 2005-2008 on employee wage. The sample restrictions and variable definitions follow Table 2. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 5: Gender Gap Revelation and Subsequent Employee Wages, Placebo Treatment

| | 50 Cutoff | | | 65 Cutoff | | | 80 Cutoff | | |
|---|---|---|---|---|---|---|---|---|---|
| | Male | Female | All | Male | Female | All | Male | Female | All |
| Treated$_p$ × Post | -0.00172 | -0.00215 | -0.00230 | -0.00564 | -0.0101 | -0.0103 | -0.00369 | -0.00388 | -0.00385 |
| | (0.00437) | (0.00650) | (0.00647) | (0.00512) | (0.00793) | (0.00788) | (0.00573) | (0.00901) | (0.00896) |
| Male × Post | | | -0.0217*** | | | -0.0213*** | | | -0.0169*** |
| | | | (0.00444) | | | (0.00526) | | | (0.00653) |
| Treated$_p$ × Post × Male | | | 0.000579 | | | 0.00472 | | | 0.0000478 |
| | | | (0.00687) | | | (0.00835) | | | (0.00941) |
| log(Sales) | 0.0354*** | 0.0330*** | 0.0345*** | 0.0381*** | 0.0403*** | 0.0388*** | 0.0404*** | 0.0600*** | 0.0466*** |
| | (0.00475) | (0.00655) | (0.00466) | (0.00699) | (0.00924) | (0.00659) | (0.00941) | (0.0140) | (0.00908) |
| Observations | 220384 | 99622 | 320006 | 166003 | 74529 | 240532 | 134566 | 60494 | 195060 |
| $R^2$ | 0.890 | 0.880 | 0.891 | 0.884 | 0.874 | 0.886 | 0.884 | 0.872 | 0.885 |
| Person-Firm FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Age Control | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Exp. Control | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| No. Firms | 1793 | 1784 | 1797 | 994 | 991 | 996 | 660 | 655 | 661 |

*Notes:* This table reports the effects of placebo treatment on employee wage. For columns 1-2, the placebo treatment group includes firms with average employment of 51-65 in the pre-treatment years 2003-2005, and the placebo control group includes firms with average employment of 36-50 in the pre-treatment period. For columns 3-4, the ranges are 66-80 and 51-65, respectively. For columns 5-6, the placebo treatment group includes the range 81-95 and the control group includes the range 66-80. The sample restrictions and variable definitions follow Table 2. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 6: Gender Gap Revelation and Female Share

| | (1) | (2) |
|---|---|---|
| Treated × Post | 0.00484* | 0.00441* |
| | (0.00258) | (0.00258) |
| log(Sales) | | 0.00262 |
| | | (0.00470) |
| Observations | 18847 | 18798 |
| $R^2$ | 0.927 | 0.927 |
| Firm FE | Yes | Yes |
| Year FE | Yes | Yes |
| No. Firms | 4035 | 4029 |

*Notes:* This table reports the effects of gender-separated wage revelation on the female share of employment at the firm. The sample restrictions and variable definitions follow Table 8. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 7: Gender Gap Revelation and Female Employee Joining and Leaving

**All Employees**

| | Male Join Rate | | Leave Rate | | Female Join Rate | | Female Leave Rate | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Treated × Post | 0.00158 | 0.00158 | 0.00271* | 0.00247* | 0.00374 | 0.00353 | 0.00137 | 0.00132 |
| | (0.00339) | (0.00340) | (0.00151) | (0.00148) | (0.00247) | (0.00247) | (0.00121) | (0.00119) |
| log(Sales) | | 0.00372 | | 0.0184*** | | 0.00691** | | 0.0107*** |
| | | (0.00380) | | (0.00182) | | (0.00296) | | (0.00132) |
| Observations | 18847 | 18798 | 18847 | 18798 | 18847 | 18798 | 18847 | 18798 |
| $R^2$ | 0.510 | 0.509 | 0.576 | 0.585 | 0.729 | 0.730 | 0.678 | 0.683 |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| No. Firms | 4035 | 4029 | 4035 | 4029 | 4035 | 4029 | 4035 | 4029 |

**Low-Level Employees**

| | Male Join Rate | | Male Leave Rate | | Female Join Rate | | Female Leave Rate | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Treated × Post | 0.00162 | 0.00177 | 0.00309* | 0.00290* | 0.00938*** | 0.00941*** | 0.00245 | 0.00240 |
| | (0.00411) | (0.00412) | (0.00171) | (0.00170) | (0.00340) | (0.00340) | (0.00169) | (0.00167) |
| log(Sales) | | 0.00785* | | 0.0185*** | | 0.00308 | | 0.0113*** |
| | | (0.00421) | | (0.00208) | | (0.00419) | | (0.00172) |
| Observations | 18685 | 18641 | 18685 | 18641 | 18685 | 18641 | 18685 | 18641 |
| $R^2$ | 0.463 | 0.464 | 0.521 | 0.528 | 0.637 | 0.637 | 0.558 | 0.561 |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| No. Firms | 4020 | 4015 | 4020 | 4015 | 4020 | 4015 | 4020 | 4015 |

*Notes:* This table reports the effects of gender-separated wage revelation on the firm's joining rate and leaving rate of employees, defined as $\frac{\text{\# male (female) employees joining (leaving) in year t}}{\text{\# total employees in year t}}$. The sample restrictions and variable definitions follow Table 8. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 8: Gender Gap Revelation and Firm Productivity

| | log(Sales/Asset) | | log(Sales/Employee) | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Treated × Post | -0.0343** | -0.0399*** | -0.0233* | -0.0278** |
| | (0.0139) | (0.0119) | (0.0130) | (0.0122) |
| log(Sales) | | 0.706*** | | 0.514*** |
| | | (0.0168) | | (0.0215) |
| Observations | 17999 | 17999 | 18798 | 18798 |
| $R^2$ | 0.827 | 0.902 | 0.849 | 0.886 |
| Firm FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| No. Firms | 3951 | 3951 | 4029 | 4029 |

*Notes:* This table reports the effects of gender-separated wage revelation on firm productivity. The standard errors are clustered at the firm-level. The firms with average employment between 35 and 50 in the pre-treatment period 2003-2005 are identified as treated, and firms with 20-34 are identified as the control group. Post = 0 for years 2004-2006, and Post = 1 for years 2007-2008. The above regressions exclude firms in industries unaffected by the policy, and firms that have one or fewer average pre-treatment male employees or female employees. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 9: Gender Gap Revelation and Firm Productivity, Treatment by Year

| | log(Sales/Asset) | | log(Sales/Employee) | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Treated × $\text{Year}_{2005}$ | 0.000305 | -0.00482 | -0.00191 | 0.00156 |
| | (0.0170) | (0.0115) | (0.0157) | (0.0122) |
| Treated × $\text{Year}_{2006}$ | -0.0193 | -0.0230 | -0.0157 | -0.0119 |
| | (0.0190) | (0.0143) | (0.0175) | (0.0153) |
| Treated × $\text{Year}_{2007}$ | -0.0479** | -0.0530*** | -0.0390** | -0.0401** |
| | (0.0203) | (0.0164) | (0.0180) | (0.0163) |
| Treated × $\text{Year}_{2008}$ | -0.0333 | -0.0455** | -0.0188 | -0.0219 |
| | (0.0216) | (0.0182) | (0.0193) | (0.0180) |
| log(Sales) | | 0.706*** | | 0.514*** |
| | | (0.0168) | | (0.0215) |
| Observations | 17999 | 17999 | 18798 | 18798 |
| $R^2$ | 0.827 | 0.902 | 0.849 | 0.887 |
| Firm FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| No. Firms | 3951 | 3951 | 4029 | 4029 |

*Notes:* This table reports the $Treated \times Year$ effects for years 2005-2008 on firm productivity. The sample restrictions and variable definitions follow Table 8. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 10: Gender Gap Revelation and Firm Productivity, Placebo Treatment

| | log(Sales/Asset) | | log(Sales/Employee) | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Treated$_p$ × Post | 0.0357 | 0.00930 | 0.0262 | 0.0101 |
| | (0.0239) | (0.0190) | (0.0220) | (0.0192) |
| log(Sales) | | 0.687*** | | 0.488*** |
| | | (0.0294) | | (0.0599) |
| Observations | 8071 | 8071 | 8404 | 8404 |
| $R^2$ | 0.835 | 0.904 | 0.855 | 0.890 |
| Firm FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| No. Firms | 1766 | 1766 | 1801 | 1801 |

*Notes:* This table reports the effects of placebo treatment on firm productivity. The placebo treatment group includes firms with average employment of 51-65 in the pre-treatment years 2003-2005, and the placebo control group includes firms with average employment of 36-50 in the pre-treatment period. The above regressions exclude firms in industries unaffected by the policy, and firms that have one or fewer average pre-treatment male employees or female employees. The standard errors are clustered at the firm-level. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

# IX Appendix

Table A1: Gender Gap Revelation and Subsequent Employee Wages, All Employees, with Industry-Year Fixed Effects

| | Male | Female | All |
|---|---|---|---|
| Treated × Post | -0.00845** | 0.00427 | 0.00466 |
| | (0.00377) | (0.00542) | (0.00539) |
| Male × Post | | | -0.00448 |
| | | | (0.00390) |
| Treated × Post × Male | | | -0.0132** |
| | | | (0.00586) |
| log(Sales) | 0.0292*** | 0.0248*** | 0.0278*** |
| | (0.00617) | (0.00473) | (0.00472) |
| Observations | 325000 | 146811 | 471811 |
| $R^2$ | 0.910 | 0.902 | 0.910 |
| Person-Firm FE | Yes | Yes | Yes |
| Industry-Year FE | Yes | Yes | Yes |
| No. Firms | 4007 | 3981 | 4016 |

*Notes:* This table reports the effects of gender-separated wage revelation on employee wage as in Table 2, but with industry-year fixed effects instead of year fixed effects. The standard errors are clustered at the firm-level. The firms with average employment between 35 and 50 in the pre-treatment period 2003-2005 are identified as treated, and firms with 20-34 are identified as the control group. Post = 0 for years 2004-2006, and Post = 1 for years 2007-2008. The above regressions exclude CEOs and board members, firms in industries unaffected by the policy, and firms that have one or fewer average pre-treatment male employees or female employees. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A2: Gender Gap Revelation and Subsequent Employee Bonus, All Employees

| | Bonus | | |
|---|---|---|---|
| | Male | Female | All |
| Treated × Post | 0.0666 | 0.0955 | 0.0911 |
| | (0.131) | (0.131) | (0.131) |
| Male × Post | | | 0.0542 |
| | | | (0.0815) |
| Treated × Post × Male | | | -0.0234 |
| | | | (0.131) |
| log(Sales) | 0.145 | 0.304*** | 0.199** |
| | (0.0895) | (0.0959) | (0.0808) |
| Observations | 195131 | 83131 | 278262 |
| $R^2$ | 0.702 | 0.690 | 0.699 |
| Person-Firm FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Age Control | Yes | Yes | Yes |
| Exp. Control | Yes | Yes | Yes |
| No. Firms | 3579 | 3364 | 3676 |

*Notes:* This table reports the effects of gender-separated wage revelation on employee bonus. The standard errors are clustered at the firm-level. The firms with average employment between 35 and 50 in the pre-treatment period 2003-2005 are identified as treated, and firms with 20-34 are identified as the control group. Post = 0 for years 2004-2006, and Post = 1 for years 2007-2008. The above regressions exclude CEOs and board members, firms in industries unaffected by the policy, and firms that have one or fewer average pre-treatment male employees or female employees. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A3: Gender Gap Revelation and Female Employee Joining and Leaving, High- and Intermediate-Level Employees

**High-Level Employees**

| | Male Join Rate | | Male Leave Rate | | Female Join Rate | | Female Leave Rate | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Treated × Post | 0.00609 | 0.00669 | 0.00462 | 0.00459 | 0.00557 | 0.00548 | -0.00217 | -0.00200 |
| | (0.00835) | (0.00835) | (0.00438) | (0.00438) | (0.00481) | (0.00480) | (0.00264) | (0.00265) |
| log(Sales) | | 0.00663 | | 0.0145*** | | 0.00195 | | 0.00176 |
| | | (0.00829) | | (0.00452) | | (0.00426) | | (0.00236) |
| Observations | 11557 | 11528 | 11557 | 11528 | 11557 | 11528 | 11557 | 11528 |
| $R^2$ | 0.413 | 0.413 | 0.352 | 0.354 | 0.391 | 0.392 | 0.351 | 0.352 |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| No. Firms | 2805 | 2801 | 2805 | 2801 | 2805 | 2801 | 2805 | 2801 |

**Intermediate-Level Employees**

| | Male Join Rate | | Male Leave Rate | | Female Join Rate | | Female Leave Rate | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Treated × Post | -0.00420 | -0.00415 | -0.00224 | -0.00220 | 0.00653 | 0.00650 | 0.00201 | 0.00207 |
| | (0.00779) | (0.00780) | (0.00376) | (0.00376) | (0.00641) | (0.00643) | (0.00331) | (0.00331) |
| log(Sales) | | -0.00346 | | 0.0108*** | | 0.00843* | | 0.00973*** |
| | | (0.00660) | | (0.00343) | | (0.00506) | | (0.00290) |
| Observations | 14229 | 14189 | 14229 | 14189 | 14229 | 14189 | 14229 | 14189 |
| $R^2$ | 0.388 | 0.388 | 0.370 | 0.369 | 0.459 | 0.460 | 0.428 | 0.429 |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| No. Firms | 3350 | 3346 | 3350 | 3346 | 3350 | 3346 | 3350 | 3346 |

*Notes:* This table reports the effects of gender-separated wage revelation on the firm's joining rate and leaving rate of employees, defined as $\frac{\text{\# male (female) employees joining (leaving) in year t}}{\text{\# total employees in year t}}$. The sample restrictions and variable definitions follow Table 8. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.