NBER WORKING PAPER SERIES

DO FIRMS RESPOND TO GENDER PAY GAP TRANSPARENCY?

Morten Bennedsen
Elena Simintzi
Margarita Tsoutsoura
Daniel Wolfenzon

Working Paper 25435
http://www.nber.org/papers/w25435

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
January 2018

We thank our discussants Daniel Ferreira, Camille Hebert, Karin Thorburn, and Rebecca Zarutskie. We are grateful for excellent comments from seminar participants at UC San Diego, IESE Barcelona, Columbia Business School, Cornell University, Darden, INSEAD, UIC, UCLA, ASU, and conference participants at LBS Summer Finance Symposium, Stanford SITE conference, CEPR Incentives, Management and Organization 2018 conference, Colorado Finance Summit, Swedish House of Finance and AFFECT conference, and the Corporate Finance Beyond Public Companies conference. We thank Brian J. Lee and Jiacheng Yan for excellent research assistance, He Zhang for data management, and help with data understanding from Mona Larsen (VIVE), Helle Holt (VIVE) and Maria Boysen (Statistic Office Denmark). We are grateful for financial support from the Danish National Research Foundation (Niels Bohr Professorship). The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Do Firms Respond to Gender Pay Gap Transparency?
Morten Bennedsen, Elena Simintzi, Margarita Tsoutsoura, and Daniel Wolfenzon
NBER Working Paper No. 25435
January 2018
JEL No. G18,G28,J16

ABSTRACT

We examine the effect of pay transparency on gender pay gap and firm outcomes. This paper exploits a 2006 legislation change in Denmark that requires firms to provide gender dis-aggregated wage statistics. Using detailed employee-employer administrative data and a difference-in-differences approach, we find that the law reduces the gender pay gap, primarily by slowing the wage growth for male employees. The gender pay gap declines by approximately two percentage points, or a 7% reduction relative to the pre-legislation mean. In addition, the wage transparency mandate causes a reduction in firm productivity and in the overall wage bill, leaving firm profitability unchanged.

Morten Bennedsen
Department of Economics
INSEAD
Fontainebleau
France
Morten.BENNEDSEN@insead.edu

Elena Simintzi
University of North Carolina at Chapel Hill
Kenan-Flagler Business School
Chapel Hill, NC
United States
Elena_Simintzi@kenan-flagler.unc.edu

Margarita Tsoutsoura
SC Johnson College of Business
Cornell University
Ithaca, NY 14853
and NBER
tsoutsoura@cornell.edu

Daniel Wolfenzon
Graduate School of Business
Columbia University
Uris Hall, Room 808
3022 Broadway
New York, NY 10027
and NBER
dw2382@columbia.edu

I Introduction

Gender pay disparities characterize labor markets in most developed countries.[1] When a man earns 100 dollars, a woman earns 77 in the United States (Goldin, 2014), 78.5 dollars in Germany, 79 dollars in the United Kingdom, and 83.8 on average across European Union countries (Eurostat, 2016). Recent proposals across many countries focus on pay transparency to promote equal pay.[2] However, evidence on the effect of transparency on gender pay disparities on employee and firm outcomes is limited. In this paper, we draw insights from a regulation in Denmark that increased transparency by requiring companies to inform employees of average wages by gender and occupation.

There is an ongoing debate about the consequences of disclosing gender wage gaps. Governments often propose transparency as a tool to encourage firms to reduce the wage gap between men and women. Unions and employee groups representing women also seem to believe that secrecy on pay contributes significantly to unequal pay for women.[3] Opponents of pay transparency argue that disclosing gender pay comes as a challenge to firms as it lacks practical utility, increases administrative burden, and violates employee privacy.[4]

The effect of transparency on the gender pay gap and firm outcomes is ultimately

[1]See, for example, Goldin (2014) and Blau and Kahn (2017).

[2]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. An executive order signed by the US government in 2016 required large companies to report salary data broken down by gender starting in 2017, but the rule was overturned by the succeeding administration.

[3]AFL-CIO runs a petition campaign as a response to the halt of the equal pay initiative that would have required large corporations to report pay data by gender to the Equal Employment Opportunity Commission. https://actionnetwork.org/petitions/tell-the-eeoc-we-need-the-equal-pay-data-collection?source=website. The Institute for Women's Policy Research in a survey documents that 60% of employees are discouraged or prohibited from sharing wage information and concludes that pay secrecy is an important determinant of gender gap in earnings (IWPR, 2014).

[4]See, for example, a letter representing employers against a bill in California that requires large firms in the state to file reports detailing the gender pay gap for people working in the same position. http://blob.capitoltrack.com/17blobs/e3526ab2-1360-4461-a1d3-b0580abe6172

an empirical question. It is unclear whether transparency will provide sufficient incentives for firms to adjust their compensation policies. Moreover, these wage adjustments might have unintended consequences for firm outcomes, such as firm productivity and, eventually, profitability.

Studying this question empirically requires addressing two key challenges: finding exogenous variation in wage transparency at the firm level and obtaining information on wages and employment at the individual level. For the source of exogenous variation, we 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. Firms have the duty to inform their employees of wage gaps between men and women and explain the design of the statistics and the wage concept used. For data on wages and employment at the individual level, we use administrative records from the Danish Statistics matched employee-employer dataset.

In our research design, we compare firms above the 35-employee threshold to firms below. Because firm size can influence wage dynamics and firm outcomes, our sample includes only firms in a narrow band around 35 employees. Specifically, we estimate a difference-in-differences model where treated firms are those that employ 35-50 employees prior to the introduction of the law and the control firms are those with 20-34 workers.

In terms of the effect of transparency on firm compensation policies, we find that after the passage of the law, wages of male employees in treated firms grow 1.67 percentage points slower than wages of male employees in control firms. The effect is statistically significant at the 1% level and economically important. On the contrary, female wages in treated firms increases by 0.28 percentage points more relative to female employees in control firms, although this difference is not statistically significant. These results imply that the wage gap in treated firms closes by roughly 2 percentage points more than in control firms, or 7% relative to the pre-treatment mean.

In our specifications we control for a variety of time-varying firm and individual characteristics (age, work experience, firm size), year fixed effects, and interacted individual and firm fixed effects. By including the latter fixed effects, we control for time-invariant person characteristics, time invariant firm characteristics, and the match between firms and workers. In essence, these fixed effects allow us to compare the *same employee at the same firm* before and after the regulation.

We provide additional analysis that further supports a causal interpretation of our results. First, we estimate the effect of the law by year and find no evidence of pre-treatment trends. Second, we explore whether a contemporaneous factor other than the aforementioned law drives our results. We note that it is difficult to come up with such a factor, as it would need to differentially affect male and female wages in large versus small firms. In any case, we perform placebo tests using alternative employee size cutoffs to define treatment and find no significant effects. This test further mitigates concerns that a different factor that affected wages around the same time of this law drives our results, as this factor would not only need to differentially affect wages of male and female employees in large and small firms, but also affect firms exactly at the 35 employee cutoff and not below. Third, we show our results are robust to estimating our specifications within firm-years by including interacted firm and year fixed effects. As such, we absorb any time-varying shocks at the firm level that may be correlated with wages. Fourth, we get similar results when we use hourly wages as our compensation measure, indicating that our results on wages are not driven by differential changes in hours worked of men and women. Finally, we repeat our analysis using total compensation (wage plus bonus payment) and get similar estimates. This test alleviates concerns that companies offset the change in wages by adjusting bonuses.

After documenting the effect of the law on employee wages, we investigate how transparency affects firm level outcomes, specifically employee reallocation, firm productivity, and profits. We show that treated firms hire more female employees as compared to control firms. This is in line with an argument that the supply of female employees increases as the gender pay gap closes in these firms. Moreover, we do not find that female employees are more likely to leave treated firms after the law passage.

We also find that the law has spillover effects on promotion decisions that favor female employees. We find that women are more likely to be promoted from the bottom of the hierarchy to more senior positions, while we do not find any significant change in the promotion probability for male employees.

In additional tests, we examine the implications of gender pay transparency on productivity, wage bill, and profits. *A priori*, the effect on productivity is ambiguous. If information on gender pay gaps lowers job satisfaction for those employees paid below their reference group—either because female employees learn of the pay gaps, or because male employees are dissatisfied with firms giving them lower pay increases as a response to the law—then we should expect to see a negative effect on firm productivity (Akerlof and Yellen, 1990). If, instead, the reduction in wage disparities creates a sentiment of fairness among workers, employee productivity may increase. We present evidence suggesting that productivity (measured as the logarithm of sales over employees) drops by 2.5% relative to control firms following the passage of the law.

While a reduction in productivity negatively affects profits, our results suggest that it is likely that the average employee wage (measured as the logarithm of total wage bill over employees) is reduced in treated firms, which will have the opposite effect on profits. Indeed, we find a negative and significant effect on treated firms' average wage, which is lower by 2.8%, as compared to control firms.

Finally, we examine the effect of the transparency law on firms' profits. The direction of this effect depends on which outcome (decrease productivity or lower wages) dominates. We find that the negative effect on productivity is offset by firms' lower wage costs, resulting in no significantly different effect on firm profitability.

The paper contributes to the literature on the effects of pay transparency. Breza, Kaur, and Shamdasani (2018) use a sample of workers in an Indian manufacturing plant to show that information on how much peers are earning relative to one's own salary might generate negative feelings and reduce job satisfaction.[5] In the context of mandated

[5]Perez-Truglia (2016) shows how online access of the general public to tax income information in

pay disclosure on the public sector, Card, Mas, Moretti, and Saez (2012) use a sample of government employees in California to show that after government employee salaries are published online, aggregate worker satisfaction drops. However, these studies focus on job satisfaction and not on wages or firm outcomes.

Focusing on how transparency affects wage setting in organizations, Mas (2017) shows that top earners in municipal jobs experience a drop in wages following the public disclosure of wages, which he argues is primarily due to public aversion to visibly exorbitant salaries. Yet, this paper analyzes wages in the public sector and it is likely that wage setting in the private sector might be different. For example, in the public sector, public pressure and public aversion to high compensation or inequalities might play a larger role than in the private sector. We provide the first evidence of the effect of mandated pay transparency on wages based on private firms. In addition, our study is the first one to focus on the effect of mandated transparency on gender disparities—an issue of debate.

A related literature examines the effect of information sharing on executive compensation. Shue (2013) finds that exchange of information through peer interactions affects managerial pay. Mas (2016) uses data from the Great Depression to find that a mandated pay disclosure of executive compensation led to an increase in the average CEO pay relative to other highly-paid executives in the firm. More generally, Hermalin and Weisbach (2012) argue that an increase in disclosure requirements about the firm can affect firm value and CEO compensation. While these papers focus on executives, we are interested in how transparency can affect wage and firm outcomes throughout the organization.

Our paper also contributes to a growing literature on gender and organizations that point to biases facing women in the professional workforce. Egan, Matvos, and Seru (2017) show that female advisers face harsher outcomes following misconduct, but this effect is mitigated in firms with more female executives. Adams and Ragunathan (2017)

Norway increases relative well-being and life satisfaction for the rich.

show that gender barriers tend to discourage women from working in finance. Duchin, Simutin, and Sosyura (2018) show that female division managers are allocated less capital, especially in firms where CEOs grew up in male-dominated families. Tate and Yang (2015) show that male leadership cultivates a less female-friendly culture within firms. Our findings suggest that regulatory mandates on pay transparency, as a means to overcome biases against women in the workforce, may be effective in closing the gender pay gap.

II The Law

On June 9$^{\text{th}}$ 2016, Denmark adopted Act no. 562 that created the 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 an occupation classification code (six-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.[6] The statistics had to be made available to the employees through the employee representatives; they did not need to be made available to the general public. The law also offered an alternative choice to employers by permitting them to replace gender-based wage statistics with an internal report on equal pay. This report had to include a description of the conditions that are important for determining wages and establish an action plan for equal pay to be implemented.

Passage of the law was unexpected, and it was approved over a short time. On December 7, 2005, with elections looming in 2007, the Ministry of Economics introduced a proposal to Parliament to amend the Equal Pay Act. The proposal was adopted on June 2006, and the new provisions came into force on January 2007. The proposal surprised most observers since the same administration had stalled a similar proposal

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

years earlier. The introduction of this law was generally viewed as an attempt of the government to get a better standing among female voters.

III Empirical Design

To estimate the effect of gender-pay transparency on employee pay and other firm outcomes, we employ a difference-in-differences approach. Our treated firms are firms that employ 35-50 employees prior to the introduction of the law, and the control firms are those that employ 20-34 workers. We take a narrow window around the 35-employee cutoff so that the control firms are close in size to the treated firms and, hence, likely to be a valid counterfactual.

We design our empirical strategy around the 35 threshold and do not take into account the criterion that firms should have at least ten male and ten female employees in one six-digit DISCO code. The reason is that firms do not typically have DISCO code information. According to the Danish Employer Confederation (DA),[7] some firms complied with the law even when they did not satisfy the second criterion (DISCO). In fact, 35% of firms that reported gender disaggregated wage statistics with the DA did not satisfy the second criterion; yet, all of them had more than 35 employees. In addition, this is consistent with how the law was interpreted more widely. The description of the law by the European Union (EU) and the International Labor Organization (ILO) only mentions the criterion that firms above the 35 employee threshold must comply.[8]

We use a panel of employee-firm-years to test whether transparency on wages by gender has real effects on firms' compensation policies. We compare the effect of the

[7]DA represents more than 28,000 firms in Denmark. Its main activities are coordination of collective agreements, employment policy, occupational health and safety, and other labor-related issues.

[8]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). 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."

regulation on male wages (e.g. difference in growth rates in male wages in treated and control firms) to its effect on female wages by estimating the following OLS regression in which the coefficient of interest is δ:

$$\begin{aligned} log(wage)_{ijt} &= \alpha_{ij} + \alpha_t + \gamma_1 X_{jt} + \gamma_2 Z_{it} \\ &+ \beta_1 I(Treated_{ij} \times Post_t) \\ &+ \beta_2 I(Treated_{ij} \times Male_i) + \beta_3 I(Post_t \times Male_i) \\ &+ \delta\, I(Treated_{ij} \times Post_t \times Male_i) + \varepsilon_{ijt}, \end{aligned} \tag{1}$$

where j, i, and t index firms, individuals, and years; *Post* takes a value of 1 for 2006, 2007, and 2008 and a value of 0 for years 2003, 2004 and 2005;[9] *Treated* takes a value of 1 for firms that employ 35-50 employees prior to the introduction of the law and 0 for firms that employ 20-34 workers. The terms $Male_i$, $Treated_j$, and $Post_t$ are not shown because their coefficients are absorbed by the fixed effects. X_{jt} and Z_{ijt} capture time-varying firm- and individual-level control variables, respectively. X_{jt} controls for firm size proxied by sales (log-transformed). Z_{it} controls for time-varying individual characteristics (age, work experience), following Blau and Kahn (2017). α_t is year fixed effects to absorb aggregate macroeconomic shocks.

We also include interacted individual and firm fixed effects, α_{ij}. By including these fixed effects we control for time-invariant person characteristics (e.g. skill, education), time invariant firm characteristics, and the match between firms and workers.[10] Essentially, we compare the *same employee at the same firm* before and after the regulation. That is, our estimation results are free of composition effects.[11]

We start our sample in 2003 to provide sufficient years to estimate the baseline effect

[9] The results are robust if we drop 2006, the year in which the law was passed.

[10] Individual fixed effects largely overlap with occupation fixed effects, and therefore our estimates remain unchanged when we additionally control for occupation fixed effects.

[11] We repeat our analysis by limiting our sample of employees to those who worked with the firm at least one year before the law and one full year after. Our results are robust (Internet Appendix Table IA1).

for each firm-employee group and end in 2008 to avoid overlap of our sample with the financial crisis. Standard errors are clustered at the firm level.

We also examine the effect of the law on firm outcomes, such as hiring decisions, productivity, and profitability. Using a panel of firm-years, we estimate OLS regressions of the following form, in which the coefficient of interest δ captures the differential effect of the law on the dependent variables for treated and control firms:

$$Y_{jt} = \alpha_j + \alpha_t + \gamma X_{jt} + \delta\, I(Treated_j \times Post_t) + \varepsilon_{jt} \quad (2)$$

where j and t index firms and years; $Post$ takes a value of 1 for 2006, 2007, and 2008 and a value of 0 for years 2003, 2004, and 2005. The terms $Treated_j$ and $Post_t$ are not shown because their coefficients are absorbed by the fixed effects. X_{jt} controls for firm size proxied by sales (log-transformed). α_t is year fixed effects to absorb aggregate macroeconomic shocks. We also include firm fixed effects, α_j, to control for time-invariant firm characteristics. 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 Labor 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 detailed information for employees' compensation, demographics, and occupation. For compensation, we have information on employees' wages and bonuses. Furthermore, for each employee, we observe their age, gender, and education, as well as their position in the firm hierarchy.

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 limited liability firms in Denmark and their employees included in the IDA dataset. For ease of comparison, for the employee-level outcomes we focus on full-time workers, excluding CEOs and boards of directors. We drop firms in industries unaffected by the policy (farming, gardening, forestry, and fishery). We require firms to have financial information which results in dropping 0.8% of firm years in the sample.

Table 1 presents summary statistics for the treated and control firms in our sample over the 2003-2005 period prior to the law passage. Panel A presents employee-level characteristics and Panel B presents firm-level characteristics. The average annual (hourly) wage for employees in the treated firms is $55,000 ($34.4), while for the control group it is $53,000 ($33.5). The average employee in the sample is 40 years old and has 17 years of work experience in both treated and control groups. On average, 25% of employees in treated and control groups hold a college degree. Consistent with the well-documented employer size-wage effect (Brown and Medoff, 1989; Idson and Oi, 1999), the average individual wage in treated firms is higher than that in control firms. However, the average employee is similar in terms of other observable characteristics between treated and control firms.

Treated firms are larger than control firms by construction. For example, as shown in Panel B, the average treated firm has 42 employees pre-treatment, assets of $7.2 million, sales of $11.68 million, and pays total wages of $2.3 million as compared to 26 employees, $6.1 million in assets, $7.73 million in sales, and $1.4 million in wages for control firms. However, firms are similar in terms of their pre-treatment productivity, cost structures, and the gender composition of their employees with 70% male employees on average.

V Results

V.1 Wages

Our goal is to identify the effect of transparency on firm compensation policies and the relative pay of men and women. Before we present our OLS results, we show univariate tests that demonstrate the main effect. Table 2 presents the average log wage in years 2006-2008 minus the average log wage in 2003-2005, the three years prior to the passage of the law. In order to control for compositional changes, we keep only observations in which the employee works at the same firm as he did in 2005.

Wages increase for all employees, irrespective of their gender in both the treated and the control group. However, male employee wages grow by 1.44 percentage points less in treated firms as compared to control firms, and this difference is statistically significant at the 1% level. In contrast, there is no significant differences in female wage growth between treated and control firms. These univariate comparisons suggest that the reform requiring wage transparency resulted in a 1.73 percentage points lower wage growth for male employees than female employees.

Another interesting observation is the rate at which the wage gap changes in treated and control firms. In control firms, the wage growth rate of male and female employees is similar. The difference is -0.0056, but it is not statistically significant. That is, there is no change in the wage gap in control firms. However, in treated firms, the growth rate of male employees is lower than that of female employees. The difference is -2.29 percentage points, and it is significant at the 1% level. The fact that male wages grow more slowly than female wages in treated firms implies a reduction in the gender pay gap of around 2 percentage points. This reduction is economically meaningful. The level of the pay gap prior to the reform was 26% based on the mean salaries of men and women.[12] Thus, the pay gap is reduced by about 7% following the law.

We next turn to our multivariate regression analysis and estimate the effect of disclosing gender pay disparities on wages of a given individual within a treated firm as compared to an individual in a control firm. Table 3 reports the results. In our regressions, we include firm-individual fixed effects to control for firm and individual time-invariant characteristics and the match between firms and employees and year fixed effects to absorb macroeconomic shocks.

Column 1 compares the effect of the law on wages of male employees in treated firms relative to male employees in control firms. Column 2 repeats this analysis comparing instead wages for female employees. We find that wages of male employees in treated firms grow by 1.67 percentage points slower than wages of male employees in control firms. This magnitude is similar to that in our univariate results in Table 2. The effect is statistically significant at the 1% level and economically important. On the contrary, we find a positive, but not significant, coefficient on treated firms' female wages relative to control firms in column 2. In a triple-differences estimation in column 3, we compare the effect of the law on wages of male relative to female employees. The triple-difference coefficient shows that male wage growth is 2 percentage points lower than female wage growth, and the effect is statistically significant at the 1% level.

In columns 4-6, we repeat our estimation additionally controlling for individual time-varying characteristics to account for time-varying differences between employees in our treated and control firms and firm size (proxied by logarithm of sales) to account for the well-documented employer size-wage effect that larger firms pay higher wages (e.g. Brown and Medoff, 1989; Idson and Oi, 1999). Including firm size is important in our setting given the treated group includes larger firms by construction. The estimated coefficients remain virtually unchanged after controlling for firm size.

[12]The level of the pay gap in our sample is similar to the one reported by Kleven, Landais, and Søgaard (2018) (Figure 1b).

V.2 Hours worked and bonus payments

Our main result is that male wages in treated firms grow more slowly than in control firms. In this section we show that this result is not driven by a reduction in male employees' working hours (while the compensation per hour remains the same), and it is not offset by an increase in bonuses to male employees.

To examine the first point, we replicate Table 3 using employee hourly wages as the outcome variable. In Internet Appendix IA2, we show that the results are similar both in terms of economic and statistical significance. The measure of hourly wages comes from a mandated pension scheme introduced in 1964—Arbejdsmarkedets Tillaegspension (ATP)—that requires all employers to contribute on behalf of their employees based on individual hours worked. One caveat, however, as explained in Kleven, Landais, and Søgaard (2018), is that this ATP-based measure of hourly wages is based on bracketed hours worked, and it is capped, which is not the case for our baseline wages measure.

To address the second concern, in Internet Appendix Table IA3 we estimate the effect of the law on employee total compensation (wage plus bonus payment). Including bonus payments does not materially affect our estimates.

V.3 Identification concerns

In this section we address several concerns with the causal interpretation of our results. A first order concern is whether wages follow differential trends in small and large firms. This alone, however, would not explain our results since our estimated effect on wages is concentrated on male employees (as opposed to all employees). To drive our findings, an omitted variable would not only need to be correlated with size, but also differentially affect male and female wages. To explore this possibility, we analyze the dynamics of male and female wages. Table 4 shows year-by-year coefficients for male (column 1) and female (column 2) employees before and after the passage of the law. We find no significant difference in the evolution of either male or female wages between treated and control groups prior to the adoption of the law. However, male wages decline in treated firms relative to control firms immediately after the passage of the law, while female wages do not seem affected by 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 2.1 percentage points and 1.9 percentage points, respectively), as compared to female wage growth in treated versus control firms, while there is no significant difference pre-treatment. These results show that wages in treated and control firms (both for men and women) were following parallel trends prior to the law and that the effects only appear after the law was implemented. The results of Table 4 further reinforce the causal interpretation of our findings.

A different concern is that some other factor (e.g., another law) differentially affected the wages of men and women in large and small firms exactly around the same time as the disclosure law. If this were the case, we would still observe parallel trends, but our main results could be potentially explained by this factor rather than the disclosure law we are studying. To address this concern, 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 as firms with 20-35 employees prior to the law and placebo control firms as those firms with 5-19 employees. In columns 4-6, we use 50 employees as the cutoff, and thus, placebo treated firms are those firms with 50-65 employees pre-treatment and placebo control firms are firms with 35-49 employees pre-treatment. In columns 7-9, we instead use a cutoff of 65 employees, and thus, placebo treated firms are those firms with 65-80 employees pre-treatment and placebo control firms are firms with 50-64 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 described by the law. These results mitigate concerns that a different factor that affected wages around the same time of this law drives our results as this factor would need to affect firms exactly at the 35 employee cutoff.

Moreover, we repeat our baseline analysis additionally controlling for interacted-firm and year-fixed effects in Internet Appendix Table IA4. These controls allow us to absorb any time-varying changes at the firm level that could be driving our results. When we include firm-year fixed effects, we can only repeat specifications similar to those in column 3, Table 3,[13] where we provide a triple difference estimate comparing the effect of the law between male and female employees in treated versus control firms. The results are very similar to our main results.

VI Pay by Hierarchy, Hiring and Promotions

To get a better understanding of how firms adjusted their compensation policy following the law, we study whether there are asymmetric responses by firms depending on employee hierarchy. In Table 6, we examine the effect of the law on pay for managerial employees at the top of the hierarchy and for employees in non-managerial positions at lower-hierarchy levels. IDA database provides information on the primary working position of the employee and whether the employee is high-level employee, intermediate-level employee, or low-level employee. Columns 1-3 show that the law had no impact on wages of employees at the high hierarchy level. However, the results in columns 4-9 show that the law negatively affected the wage growth of non-managerial male employees. Yet, the wage growth of non-managerial female employees was not affected.[14] These results are consistent with the fact that the law is more likely to apply to employees compensated based on wages and not performance pay.

Our results establish that the law has an effect on wages, as intended by the regulator. However, this might not be the only response by firms. Changes in the way similar employees of different gender are compensated might affect the demand for or supply of those employees, resulting in differences in hiring or departure rates. Moreover, the law mandate for fairer practices may have spillover effects on other firm decisions, such as

[13] Firm-year fixed effects subsume the coefficient on $TreatedxPost$.

[14] In unreported results, we replicate this analysis defining firm hierarchies based on workers' occupations following Caliendo, Monte, and Rossi-Hansberg (2015) and Friedrich (2015) and find similar results.

employee promotions. We next examine the effect of the law's passage on each of these different outcomes.

We start by computing hiring rates for female employees at the three hierarchy levels described above. *Joining rate* is the share of female employees joining the firm in a given hierarchy level in a given year, t. (By construction, hiring rates for men and women sum up to one, and thus, we only present hiring rates for female employees). We compare hiring rates for women in treated versus control firms in a given hierarchy level following the policy change in a specification with firm and year fixed effects. We present the results in Panel A, Table 7. Conditional on hiring, we find no differential effect of the law in the high-hierarchy levels. We find that treated firms hire a higher share of women in the intermediate-hierarchy levels. This result is statistically significant at the 5% level. We also find an economically-large effect for low hierarchy levels, although this effect is noisier and not statistically significant. The magnitudes we estimate are large. The pre-law average of *Joining rate* is 37% and 43.6% for intermediate and low hierarchy levels, respectively. Our estimates indicate that the law causes female joining rates to increase by 4.4 percentage points and 2.5 percentage points, respectively. One possible interpretation of these results is that firms are able to attract more female employees in positions where they offer more fair compensation.

Similarly, we define departure rates as the share of female employees leaving the firm from a given firm-hierarchy-year. Our goal is to capture voluntary departures from the firm rather than firings. Therefore, we exclude departures in which the employee remains unemployed for more than a year. In Panel B, Table 7, we find no statistically significant change in departure rates of males or females across firm hierarchies. Interestingly, however, the departure rate for high-level female employees is economically large. Although statistically insignificant, this evidence suggests that women are more likely to leave positions that did not adjust wages to reduce the gender pay gap. Overall, these results suggest that women participation rates increase in positions in which the male wage premium is reduced.

To examine firm promotion decisions, we define a dummy variable that takes a value of 1 if a given individual is promoted to a higher hierarchical level within the firm. The measure is thus meaningful for the intermediate and low level employees. Table 8 presents the results. Columns 1-3 show that, for intermediate-level employees, there is no change in their propensity to get promoted to the highest hierarchy level after the passage of the law. Columns 4-6 show instead that low-level female employees are more likely to be promoted to higher hierarchy levels in treated firms after the passage of the law, as compared to those in control firms. The promotion probability before the reform is 2.2% for males and 2% for females, and, although it does not change for males, the probability increases by 1.2 percentage points for female employees after the reform. These results complement our previous findings indicating that the law did not only have the intended consequences of "fixing" gender pay disparities within the firm, but also improved female employees' ability to climb up the corporate ladder.

VII Firm Performance

Although the law targets wages, it is possible that it has unintended consequences on firm-level outcomes. In this section we explore whether the effects of the law on gender pay affect firm productivity, wages, and profits. We perform our analysis at the firm level in a specification with firm and year fixed effects as described in Equation 2. We report the results in Table 9.

In columns 1-2, we examine the effects of the law on firm productivity of treated firms as compared to the group of control firms. The effect on productivity is *a priori* ambiguous. If information on gender pay gap lowers job satisfaction of female employees, it might negatively impact their productivity (Akerlof and Yellen, 1990). A similar effect should be observed if male employees are dissatisfied with lower wage growth relative to their peers. However, if increased transparency and firms' responses create a sentiment of fairness among employees, then productivity might be positively impacted. Although the law may differentially affect productivity of male versus female employees, our estimates capture the average effect of the law on firm productivity as we do not have data on productivity at the individual employee level. We observe that, on average, productivity (measured as the log transformed sales per employee) drops by 2.5% in treated firms following the regulation as compared to control firms, and this reduction is statistically significant at the 5% level.

Next we analyze the effect of the law on average wages. While our main result is that the law reduced the growth rate of male wages, it is still possible that the average wage at treated firms remains constant or even increases due to composition effects (e.g. treated firms might hire high-wage individuals after the passage of the law). Columns 3 and 4, Table 9, show that the average wage per employee (log-transformed) is reduced by 2.8%. We only observe a negative and significant effect on employee wages and not on other labor costs, such as pensions and other social security costs, as the latter are not directly impacted by the regulation.

In columns 7-8, Table 9, we estimate the effect of the law on firm profitability, measured as profits per employee. We find no effect on firm profits, which can be explained by the offsetting effects of lower employee productivity and wages.[15]

VIII Heterogeneity in the Effect of Transparency on the Gender Wage Gap

Having established the effect of transparency on wage setting in firms, we next study how managerial characteristics and pre-existing inequality affect the degree to which companies adjust.

We start with managerial characteristics. To ensure that we take into account the characteristics of the managers involved in setting employee wages, we define the man-

[15] In Internet Appendix Table IA5, we present by-year estimates of Table 9. We find no significant effects pre-treatment for firm productivity and wages, while results become significant following the law passage. Interestingly, the significant effect on firm wages is not contemporaneous, but rather precedes that of productivity by one year.

agerial team as the top five earners in the firm in the period prior to the law.

We focus on the degree to which managerial preferences are pro-women. Since only 20% of top managers in our sample are women, we construct a proxy for male managers preferences towards pro-women policies. This measure allows us to use variation in preferences over the majority of the managers in our sample. To create this proxy, we start from the finding in the literature that men parenting daughters are more likely to adopt pro-women preferences (Warner, 1991; Warner and Steel, 1999; Oswald and Powdthavee, 2010; Washington, 2008; Glynn and Sen, 2015; Cronqvist and Yu, 2017; Dahl, Dezső, and Ross, 2012).[16] We define a variable to be 1 if a male manager has more daughters than sons, 0.5 if they have as many daughters as sons, and 0 otherwise. We average this variable for each firm's managerial team and define $Female\ Child$ to be 1 if the firm average is above the sample median and 0 otherwise.

In Table 10, we augment our baseline specifications by interacting $Treated \times Post$ with $Female\ Child$. As we saw in our main result, column 1 shows that the law has a negative impact on the growth rate of male wages. Moreover, when the management team has pro-women preferences, this reduction in growth is more pronounced. In terms of female wages, column 2 shows they are not affected by the law unless the management team has pro-women preferences. In the latter case, female wages grow 1.58 percentage points faster than where the management team has no pro-women preferences. Putting these results together, the wage gap closes more in firms in which the management team exhibit pro-women preferences.[17] Note in unreported regressions, we confirm that these same set

[16]Examples that support the female socialization hypothesis abound in the social sciences literature. Washington (2008) and Glynn and Sen (2015) find that having a daughter increases the propensity to vote liberally for members of the US Congress or federal judges, respectively. Oswald and Powdthavee (2010) show, more generally, that parents with daughters tend to be politically more left-oriented. Cronqvist and Yu (2017) show that CEOs with daughters are more likely to make corporate social responsible decisions, especially related to issues concerning diversity, the environment, and employee relations.

[17]We also consider the fraction of women in the top management team ($Female\ Managers$) as an alternative measure for pro women managerial preferences. In Table IA6, we find weak evidence that the effect is larger for firms with more women on the top management. However, we do not capture statistically significant results possibly due to the low share of female managers in the average firm.

of firms offered higher wages to women, but not to men, in the pre-treatment period. In Internet Appendix Table IA7, we instead construct the *Female Child* measure based on the first-born child of the firm's top managers, which is arguably a more exogenous child-gender measure (Bennedsen, Nielsen, Perez-Gonzalez, and Wolfenzon, 2007; Cronqvist and Yu, 2017). The estimated coefficients are similar to those reported in Table 10.

In sum, our results show that female wages increase following the passage of the law in firms with managers parenting daughters. This suggests that the preferences of the management team play a role in the way firms responded to the regulation.

Second, we consider the role of the pre-existing gender pay inequality. We use the pre-law within occupation gender pay inequality in the industry, measured as the median log difference in wages by gender at the industry-occupation-year level and averaged over the pre-treatment period.

In Table 11 we augment our baseline specification by interacting $Treated \times Post$ with *Ind. Gender Gap*, the pre-treatment industry-occupation gender pay differential. *Ind. Gender Gap* is standardized to have mean zero and a standard deviation of one. We show that treated firms in industries with high pre-law gender gap increase male wages by less, although this difference is not statistically significant. In contrast, they increase female employee wages more relative to control firms, and this difference is both statistically and economically significant. A one-standard-deviation increase in pre-treatment industry gender pay gaps is associated with an increase in female wages by 1.25 percentage points. Most importantly, in column 3, we show that gender pay gaps reduce more when pre-treatment inequality is higher. Specifically, a one-standard-deviation increase in *Ind. Gender Gap* is associated with a 1.6 percentage-points reduction in the gender pay gap.

In sum, our results show that firms with higher gender pay inequality close the gender gap more aggressively. This might be due to the fact that transparency leads to an increase in accountability in these firms.

IX Conclusion

The gender pay gap has been at the epicentre of a heated debate among academics and policy makers. Recently, governments around the world have proposed transparency as a tool to nudge firms to reduce the wage gap between men and women. This paper is the first systematic study of the role of disclosure of gender-based statistics on the gender wage gap.

Empirically investigating the effect of gender pay transparency as a measure to reduce gender pay discrimination within firms is challenging as it requires finding both exogenous variation in transparency and detailed information of employee wages. We overcome these hurdles by exploiting a 2006 regulation in Denmark that requires certain companies to report gender-segregated wage statistics. Using detailed employee-firm matched administrative data and employing a difference-in-difference methodology, we find changes in compensation within firms. Specifically, male employees experience slower wage growth relative to female employees. Moreover, we find that companies subject to the regulation are more likely to hire and promote more women. We also find a negative impact on firm productivity, but no significant effects on firm profits.

References

Adams, R. B., Ragunathan, V., 2017. Lehman sisters. Available at SSRN.

Akerlof, G. A., Yellen, J. L., 1990. The fair wage-effort hypothesis and unemployment. The Quarterly Journal of Economics 105, 255–283.

Bennedsen, M., Nielsen, K. M., Perez-Gonzalez, F., Wolfenzon, D., 2007. Inside the family firm: The role of families in succession decisions and performance. The Quarterly Journal of Economics 122, 647–691.

Blau, F. D., Kahn, L. M., 2017. The gender wage gap: Extent, trends, and explanations. Journal of Economic Literature 55, 789–865.

Breza, E., Kaur, S., Shamdasani, Y., 2018. The morale effects of pay inequality. The Quarterly Journal of Economics 133, 611–663.

Brown, C., Medoff, J., 1989. The employer size-wage effect. Journal of Political Economy 97, 1027–1059.

Caliendo, L., Monte, F., Rossi-Hansberg, E., 2015. The anatomy of French production hierarchies. Journal of Political Economy 123, 809–852.

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

Cronqvist, H., Yu, F., 2017. Shaped by their daughters: Executives, female socialization, and corporate social responsibility. Journal of Financial Economics 126, 543–562.

Dahl, M. S., Dezső, C. L., Ross, D. G., 2012. Fatherhood and managerial style: How a male CEO's children affect the wages of his employees. Administrative Science Quarterly 57, 669–693.

Duchin, R., Simutin, M., Sosyura, D., 2018. The origins and real effects of the gender gap: Evidence from CEOs' formative years. SSRN Electronic Journal.

Egan, M. L., Matvos, G., Seru, A., 2017. When Harry fired Sally: The double standard in punishing misconduct. Working paper, National Bureau of Economic Research.

Friedrich, B., 2015. Trade shocks, firm hierarchies and wage inequality. Economics working papers, Department of Economics and Business Economics, Aarhus University.

Glynn, A., Sen, M., 2015. Identifying judicial empathy: Does having daughters cause judges to rule for women's issues? American Journal of Political Science 59, 37–54.

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

Hermalin, B. E., Weisbach, M. S., 2012. Information disclosure and corporate governance. The Journal of Finance 67, 195–233.

Idson, T. L., Oi, W. Y., 1999. Workers are more productive in large firms. American Economic Review 89, 104–108.

Kleven, H., Landais, C., Søgaard, J. E., 2018. Children and gender inequality: Evidence from Denmark. Working Paper. National Bureau of Economic Research.

Mas, A., 2016. Does disclosure affect CEO pay setting? Evidence from the passage of the 1934 Securities and Exchange Act. Working Paper.

Mas, A., 2017. Does transparency lead to pay compression? Journal of Political Economy 125, 1683–1721.

Oswald, A. J., Powdthavee, N., 2010. Daughters and left-wing voting. The Review of Economics and Statistics 92, 213–227.

Perez-Truglia, R., 2016. The effects of income transparency on well-being: Evidence from a natural experiment. Available at SSRN.

Shue, K., 2013. Executive networks and firm policies: Evidence from the random assignment of MBA peers. The Review of Financial Studies 26, 1401–1442.

Tate, G., Yang, L., 2015. Female leadership and gender equity: Evidence from plant closure. Journal of Financial Economics 117, 77–97.

Warner, R. L., 1991. Does the sex of your children matter? Support for feminism among women and men in the United States and Canada. Journal of Marriage and Family 53, 1051–1056.

Warner, R. L., Steel, B. S., 1999. Child rearing as a mechanism for social change: The relationship of child gender to parents' commitment to gender equity. Gender and Society 13, 503–517.

Washington, E. L., 2008. Female socialization: How daughters affect their legislator fathers. American Economic Review 98, 311–32.

Table 1: Summary Statistics

This table reports summary statistics for the employee-level (Panel A) and firm level (Panel B) variables for all firms in our sample and for treated and control firms separately. Treated firms are those that employ 35-50 employees prior to the introduction of the law and controls are those that employ 20-34 employees. The variables are averaged over the pre-law years 2003-2005. The table reports unconditional means, standard deviations, and p-values of the differences in means between treated and control groups pre-treatment. For the conversion from DKK to USD we use the spot exchange rate at the year-end. Firm-level variables are winsorized at 1%.

Panel A - Employee-Level Characteristics

	All			Treated		Control		t-test
	Observations	Mean	S.D.	Mean	S.D.	Mean	S.D.	p-value
Wage (thous. $)	66,195	53.80	23.71	54.59	23.82	53.27	23.59	0.020
Hourly Wage ($)	66,188	33.92	15.09	34.41	15.38	33.54	14.82	0.013
Bonus (thous. $)	65,958	1.18	3.04	1.15	3.11	1.21	3.00	0.375
Age (years)	67,574	39.79	10.77	39.90	10.63	39.70	10.85	0.326
Male (%)	67,749	0.64	0.48	0.64	0.48	0.64	0.48	0.860
College degree (%)	66,158	0.25	0.43	0.25	0.44	0.24	0.43	0.213
Work Experience (years)	67,824	17.23	10.36	17.34	10.29	17.14	10.40	0.347

Table 1: [Continued] Summary Statistics

Panel B - Firm-Level Characteristics

	All			Treated		Control		t-test
	Observations	Mean	S.D.	Mean	S.D.	Mean	S.D.	p-value
Assets (mil. $)	3,956	6.44	23.74	7.20	13.23	6.07	27.46	0.079
Sales (mil. $)	3,956	9.03	9.64	11.68	10.74	7.73	8.77	0.000
Sales/Employee (mil. $)	3,956	0.28	0.30	0.27	0.25	0.28	0.33	0.156
Employment	4,005	31.12	8.49	41.67	4.37	25.97	4.12	0.000
Female Share (%)	3,998	0.30	0.21	0.29	0.21	0.30	0.21	0.153
Profits (mil. $)	3,957	0.25	1.88	0.26	1.49	0.25	2.04	0.960
Profits/Employee (mil. $)	3,957	0.007	0.021	0.006	0.020	0.007	0.022	0.101
Total Wages (mil. $)	3,950	1.70	0.70	2.26	0.69	1.43	0.52	0.000
Wage/Employee (mil. $)	3,950	0.051	0.017	0.051	0.016	0.051	0.017	0.923
Pension & Soc. Sec. (mil. $)	3,950	0.135	0.082	0.179	0.091	0.114	0.068	0.000
Pension & Soc. Sec./Employee (mil. $)	3,950	0.004	0.002	0.004	0.002	0.004	0.002	0.819

Table 2: Univariate Test: Change in Compensation Policy Around the Disclosure Law

This table reports the difference in average wage around the disclosure law for male and female employees in treated and control firms. To compute the average wage before and after the reform, we keep only observations in which the employee works at the same firm he did in 2005. Column (1) pertains to employees of firms in the treated group and column (2) pertains to employees of control firms. Column (3) presents the difference between column (1) and column (2) (difference-in-differences). The first row reports the difference of average male wage between the post-law (2006-2008) and pre-law (2003-2005) periods for the treated (column 1) and control groups (column 2), and the difference between column 1 and column 2 (column 3). The second row similarly reports the first and second difference for the average female wage. The difference-in-difference-in-differences result represents the difference between the change in the male wages and female wages around the disclosure law in treated versus control firms. Detailed descriptions of the variables are given in Table A1. The wages are log-transformed. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the firm level.

log Wage (3-year avg after – 3-year avg before)	Treated	Control	Dif-in-Dif (DD)
Male	0.0915***	0.1059***	-0.0144***
	(0.0041)	(0.0032)	(0.0052)
Female	0.1144***	0.1115***	0.0029
	(0.0046)	(0.0037)	(0.0059)
DD/DD/DDD	-0.0229***	-0.0056	-0.0173**
	(0.0053)	(0.0044)	(0.0069)

Table 3: Gender Pay Gap Disclosure and Employee Wages

This table reports the effects of gender pay gap disclosure on employee wages. The dependent variable is employee annual wage (log transformed). Person controls include employee work experience and age. Detailed descriptions of the variables are given in Table A1. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the firm level.

	Male	Female	All	Male	Female	All
Treated × Post	-0.0167***	0.0028	0.0028	-0.0142***	0.0039	0.0040
	(0.0039)	(0.0045)	(0.0044)	(0.0035)	(0.0042)	(0.0042)
Male × Post			-0.0022			-0.0030
			(0.0034)			(0.0033)
Treated × Post × Male			-0.0195***			-0.0185***
			(0.0052)			(0.0050)
Firm size				0.0217***	0.0195***	0.0210***
				(0.0028)	(0.0035)	(0.0025)
Person-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Person Controls	No	No	No	Yes	Yes	Yes
Observations	145,852	79,532	225,384	145,262	79,027	224,289
R^2	0.868	0.827	0.866	0.871	0.828	0.868

Table 4: Gender Pay Gap Disclosure and Employee Wages: Treatment by Year

This table reports the $Treated \times Year$ effects of gender pay gap disclosure on employee wages. The sample and variable definitions follow Table 3. $Male \times Year$ terms are estimated but omitted for brevity. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the firm level.

	Male	Female	All
Treated × Year_{2004}	-0.0001	-0.0076	-0.0077
	(0.0035)	(0.0051)	(0.0051)
Treated × Year_{2005}	-0.0054	-0.0059	-0.0060
	(0.0040)	(0.0057)	(0.0057)
Treated × Year_{2006}	-0.0140***	-0.0033	-0.0033
	(0.0047)	(0.0062)	(0.0062)
Treated × Year_{2007}	-0.0193***	0.0016	0.0017
	(0.0053)	(0.0066)	(0.0066)
Treated × Year_{2008}	-0.0185***	-0.0004	-0.0004
	(0.0059)	(0.0072)	(0.0071)
Male × Treated × Year_{2004}			0.0075
			(0.0060)
Male × Treated × Year_{2005}			0.0005
			(0.0066)
Male × Treated × Year_{2006}			-0.0110
			(0.0072)
Male × Treated × Year_{2007}			-0.0213***
			(0.0078)
Male × Treated × Year_{2008}			-0.0185**
			(0.0084)
Firm size	0.0218***	0.0196***	0.0210***
	(0.0028)	(0.0035)	(0.0025)
Person-Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Person Controls	Yes	Yes	Yes
Observations	145,262	79,027	224,289
R^2	0.871	0.828	0.868

Table 5: Gender Pay Gap Disclosure and Employee Wages: Placebo Tests

This table reports a placebo estimation of gender pay gap disclosure on employee wages, using alternative employment cutoffs to define treatment. In columns 1-3, the placebo treatment group includes firms with 20-35 employees prior to the law and the placebo control group includes firms with 5-19 employees in pre-treatment years. In columns 4-6, the ranges are 50-65 and 35-49 employees, respectively. In columns 7-9, the ranges are 65-80 and 50-64 employees, respectively. The sample and variable definitions follow Table 3. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the firm level.

	20 Cutoff			50 Cutoff			65 Cutoff		
	Male	Female	All	Male	Female	All	Male	Female	All
$\text{Treated}_p \times \text{Post}$	0.0039	0.0017	0.0017	-0.0009	-0.0018	-0.0017	0.0019	0.0011	0.0009
	(0.0037)	(0.0040)	(0.0040)	(0.0045)	(0.0056)	(0.0055)	(0.0072)	(0.0067)	(0.0066)
Male × Post			-0.0062			-0.0223***			-0.0198***
			(0.0040)			(0.0039)			(0.0051)
$\text{Treated}_p \times \text{Post} \times \text{Male}$			0.0022			0.0011			0.0007
			(0.0051)			(0.0063)			(0.0089)
Firm size	0.0210***	0.0215***	0.0212***	0.0204***	0.0149***	0.0182***	0.0405**	0.0151***	0.0291***
	(0.0026)	(0.0030)	(0.0022)	(0.0035)	(0.0038)	(0.0031)	(0.0174)	(0.0048)	(0.0112)
Person-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Person Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	148,573	88,160	236,733	104,098	56,899	160,997	72,578	41,786	114,364
R^2	0.865	0.827	0.863	0.875	0.822	0.871	0.862	0.815	0.859

Table 6: Gender Pay Gap Disclosure and Employee Wages by Hierarchy

This table reports the effects of gender pay gap disclosure on employee wages, by employee position in the firm hierarchy. The sample and variable definitions follow Table 3. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the firm level.

	High-level			Intermediate-level			Lower-level		
	Male	Female	All	Male	Female	All	Male	Female	All
Treated × Post	-0.0108	-0.0017	-0.0008	-0.0208***	0.0055	0.0057	-0.0106**	0.0029	0.0028
	(0.0081)	(0.0132)	(0.0130)	(0.0056)	(0.0071)	(0.0070)	(0.0046)	(0.0054)	(0.0054)
Male × Post			-0.0190*			0.0104*			-0.0063
			(0.0104)			(0.0056)			(0.0044)
Treated × Post × Male			-0.0101			-0.0268***			-0.0137**
			(0.0145)			(0.0083)			(0.0066)
Firm size	0.0228***	0.0190**	0.0220***	0.0220***	0.0178***	0.0206***	0.0209***	0.0210***	0.0209***
	(0.0054)	(0.0092)	(0.0055)	(0.0040)	(0.0059)	(0.0037)	(0.0044)	(0.0043)	(0.0034)
Person-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Person Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	33,647	9,146	42,793	45,901	27,056	72,957	61,136	39,663	100,799
R^2	0.829	0.805	0.829	0.849	0.799	0.849	0.856	0.810	0.847

Table 7: Gender Pay Gap Disclosure and Employee Hiring

This table reports the effects of gender pay gap disclosure on the firm's joining rate and leaving rate of employees. In Panel A, *Joining Rate* is defined as ($\frac{\text{\# female employees joining in year t}}{\text{\# total employees joining in year t}}$). In Panel B, *Leaving Rate* is defined as ($\frac{\text{\# female employees leaving in year t}}{\text{\# total employees leaving in year t}}$). The sample is defined at the firm level. Detailed descriptions of the variables are given in Table A1. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the firm level.

	Panel A - Joining Rate					
	High-level		Intermediate-level		Lower-level	
Treated × Post	0.0091	0.0088	0.0423**	0.0441**	0.0257	0.0248
	(0.0248)	(0.0251)	(0.0216)	(0.0217)	(0.0192)	(0.0192)
Firm size		0.0018		-0.0079		0.0201
		(0.0160)		(0.0145)		(0.0137)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,221	3,208	5,391	5,373	7,046	7,035
R^2	0.500	0.500	0.533	0.533	0.555	0.555

	Panel B - Leaving Rate					
	High-level		Intermediate-level		Lower-level	
Treated × Post	0.0216	0.0175	0.0077	0.0078	-0.0138	-0.0130
	(0.0242)	(0.0243)	(0.0208)	(0.0208)	(0.0185)	(0.0185)
Firm size		0.0149		-0.0041		0.0151
		(0.0149)		(0.0148)		(0.0145)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,698	3,673	5,753	5,735	7,840	7,825
R^2	0.465	0.467	0.516	0.517	0.564	0.564

Table 8: Gender Pay Gap Disclosure and Employee Promotion

This table reports the effects of gender pay gap disclosure on employee promotion likelihood. We consider an employee promoted if he/she changes from a lower to higher hierarchy level in the given year in the firm, represented by 1, and assign 0 otherwise. Columns 1-3 show the results for employees who were in the intermediate hierarchy level in the previous year, and columns 4-6 show the results for those who were in the low hierarchy level in the previous year. The sample and variable definitions follow Table 3. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the firm level.

	Intermediate-level			Low-level		
	Male	Female	All	Male	Female	All
Treated × Post	0.0067	-0.0019	-0.0021	0.0019	0.0116**	0.0115**
	(0.0047)	(0.0042)	(0.0042)	(0.0042)	(0.0050)	(0.0050)
Male × Post			0.0011			0.0010
			(0.0034)			(0.0032)
Treated × Post × Male			0.0087			-0.0097*
			(0.0060)			(0.0052)
Firm size	-0.0027	-0.0023	-0.0025	-0.0018	-0.0020	-0.0019
	(0.0024)	(0.0023)	(0.0019)	(0.0034)	(0.0039)	(0.0029)
Person-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Person Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	35,166	19,907	55,073	52,382	33,398	85,780
R^2	0.429	0.380	0.417	0.522	0.527	0.524

Table 9: Gender Pay Gap Disclosure and Firm Performance

This table reports the effects of gender pay gap disclosure on firm performance. In columns 1-2, the dependent variable is the logarithm of sales per employee; in columns 3-4, the dependent variable is the logarithm of wages per employee; in columns 5-6, the dependent variable is the logarithm of pension and social security expenses per employee; in columns 7-8, the dependent variable is profits per employee. The analysis is at the firm level. Detailed descriptions of the variables are given in Table A1. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the firm level.

	log(Sales/employees)		log(Wage/employees)		log(Pension & Soc.Sec./employees)		Profits/employees	
Treated × Post	-0.0250**	-0.0246**	-0.0282***	-0.0281***	-0.0018	-0.0034	6.22	6.11
	(0.0120)	(0.0112)	(0.0056)	(0.0053)	(0.0207)	(0.0207)	(3.79)	(3.73)
Firm size		0.4050***		-0.1040***		0.1010***		26.26***
		(0.0224)		(0.0113)		(0.0186)		(5.19)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	22,414	22,391	22,429	22,391	22,374	22,351	21,602	21,564
R^2	0.845	0.879	0.849	0.863	0.544	0.547	0.621	0.625

Table 10: Heterogeneity: Managerial Preferences

This table reports the effects of gender pay gap disclosure on employee wages depending on whether managers have pro-women preferences. We define a firm's managerial team as the top five earners in the firm pre-treatment. We define a variable to be 1 if a male manager has more daughters than sons, 0.5 if they have as many daughters as sons, and 0 otherwise. We average this variable for each firm's managerial team and define *Female Child* to be 1 if the firm average is above the sample median and 0 otherwise. *Treated*×*Female Child* and *Male*×*Female Child* are absorbed by the Person-Firm FE. The sample and variable definitions follow Table 3. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the firm level.

	Male	Female	All
Treated×Post	-0.0184***	-0.0059	-0.0057
	(0.0050)	(0.0058)	(0.0057)
Post×Female Child	0.0071	-0.0025	-0.0026
	(0.0054)	(0.0058)	(0.0057)
Treated×Post×Female Child	-0.0085	0.0158*	0.0157*
	(0.0080)	(0.0088)	(0.0087)
Male×Post			0.0058
			(0.0046)
Treated×Post×Male			-0.0131*
			(0.0068)
Post×Male×Female Child			0.0097
			(0.0073)
Treated×Post×Male×Female Child			-0.0243**
			(0.0109)
Firm size	0.0211***	0.0197***	0.0206***
	(0.0030)	(0.0035)	(0.0026)
Person-Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Person Controls	Yes	Yes	Yes
Observations	122,266	74,516	196,782
R^2	0.851	0.815	0.848

Table 11: Heterogeneity: Industry Gender Pay Gap

This table reports the effects of gender pay gap disclosure on employee wages depending on pre-treatment industry gender pay gap. We define *Ind. Gender Gap* at the industry-occupation level by computing the median log difference in wages by gender at the industry-occupation level in the pre-treatment period. *Ind. Gender Gap* is standardized to have mean zero and a standard deviation of one. *Ind. Gender Gap*, *Treated×Ind.Gender Gap*, *Male×Ind.Gender Gap* are estimated, but not reported, for brevity. The sample and variable definitions follow Table 3. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the firm level.

	Male	Female	All
Treated×Post	-0.0146***	0.0055	0.0056
	(0.0037)	(0.0043)	(0.0043)
Post×Ind. Gender Gap	-0.0017	-0.0012	-0.0016
	(0.0040)	(0.0046)	(0.0045)
Treated×Post×Ind. Gender Gap	-0.0032	0.0125**	0.0126**
	(0.0059)	(0.0060)	(0.0060)
Male×Post			-0.0020
			(0.0034)
Treated×Post×Male			-0.0205***
			(0.0051)
Treated×Male×Ind. Gender Gap			-0.0100
			(0.0165)
Post×Male×Ind. Gender Gap			-0.0003
			(0.0058)
Treated×Post×Male×Ind. Gender Gap			-0.0159**
			(0.0079)
Firm size	0.0217***	0.0193***	0.0208***
	(0.0028)	(0.0035)	(0.0026)
Person-Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Person Controls	Yes	Yes	Yes
Observations	138,576	77,609	216,185
R^2	0.871	0.828	0.868

Appendix Table A1: Variable Definitions

Variable	**Definition**
Firm-level variables	
Firm size	It is the logarithm of sales. Sales are measured in real USD.
log(Sales/employees)	It is the logarithm of sales per employee. Sales are measured in real USD. Number of employees is based on employment data provided by Statistics Denmark (DST).
log(Wage/employees)	It is the logarithm of the total wage bill divided by number of employees. The information on wages comes from DST. Number of employees is based on employment data provided by DST.
log(Pension & Soc.Sec./employees)	It is the logarithm of pension and social security expenses per employee. The source of data for pensions, social security expenses, and number of employees is DST.
Profits/employees	It is net income per employee. Number of employees is based on the employment data provided by DST.
Female Child	To construct the variable, we follow the steps below. We first define a firm's managerial team as the top five earners in the firm pre-treatment. We then define a variable to be 1 if males in the managerial team have more daughters than sons, 0.5 if they have as many daughters as sons, and 0 otherwise. We average this variable for each firm's managerial team and define *Female Child* to be 1 if the firm average is above the sample median and 0 otherwise. In Table IA7, we construct the variable based on the gender of the first-born child.
Female Managers	It is an indicator variable that takes the value 1 if the firm's female manager ratio exceeds the sample median and 0 if otherwise.
Ind. Gender Gap	It is defined at the industry-occupation level by computing the median log difference in wages by gender at the industry-occupation level in the pre-treatment period. It is standardized to have mean zero and a standard deviation of one.
Joining Rate	It is defined as $(\frac{\text{\# female employees joining in year t}}{\text{\# total employees joining in year t}})$. An employee is considered to have joined the firm in a given year if he/she appears in the firm's employment data that year.
Leaving Rate	It is defined as $(\frac{\text{\# female employees leaving in year t}}{\text{\# total employees leaving in year t}})$. An employee is considered to have left the firm in a given year if it is the last year the employee appears in the firm's employment data and the employee does not remain unemployed for more than one year after that.

Appendix Table A1: Variable Definitions [cont.]

Variable	**Definition**
Employee-level variables	
Male	It is an indicator variable that takes the value 1 if an individual is male and 0 otherwise. The source is the Danish Civil Registration System.
Wage	It is total annual wage of the employee (log-transformed). The information on wages comes from DST.
Hourly Wage	It is hourly wage payment. The measure of hourly wages comes from a mandated pension scheme introduced in 1964—Arbejdsmarkedets Tillaegspension (ATP)—that requires all employers to contribute on behalf of their employees based on individual hours worked.
Bonus	It is irregular payments including bonus, grants, commissions, etc.
Age	It is the employee age recoded into quartiles. The source is the Danish Civil Registration System.
College degree	It is an indicator variable that takes the value 1 if an employee has completed a bachelor's degree and 0 otherwise. The variable is constructed based on information from the official Danish registry.
Work Experience	It is an employee's number of years worked recoded into quartiles.
Promotion	It is an indicator variable that takes the value 1 if an employee is promoted to a higher hierarchy level in the firm in a given year and 0 otherwise. The promotion variable is constructed based on information regarding employee position from IDA.

Internet Appendix to

"Do Firms Respond to Gender Pay Gap Disclosure?"

Morten Bennedsen, Elena Simintzi, Margarita Tsoutsoura, and Daniel Wolfenzon

Table IA1: Robustness: Sample Restriction to Control for Employee Composition Changes

This table repeats Table 3, except it only includes in the sample employees who were working for the firm at least one full year before the law and one full year after. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the firm level.

	Male	Female	All	Male	Female	All
Treated × Post	-0.0110***	0.0052	0.0052	-0.0097***	0.0058*	0.0059*
	(0.0029)	(0.0034)	(0.0034)	(0.0028)	(0.0033)	(0.0033)
Male × Post			-0.0002			-0.0002
			(0.0027)			(0.0026)
Treated × Post × Male			-0.0162***			-0.0159***
			(0.0040)			(0.0039)
Firm size				0.0183***	0.0144***	0.0170***
				(0.0023)	(0.0029)	(0.0021)
Person-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Person Controls	No	No	No	Yes	Yes	Yes
Observations	94,332	49,661	143,993	94,118	49,451	143,569
R^2	0.931	0.893	0.927	0.933	0.894	0.929

Table IA2: Robustness: Employee Hourly Wage

This table repeats Table 3, except it uses employee hourly wages as dependent variable. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the firm level.

	Male	Female	All	Male	Female	All
Treated × Post	-0.0130***	0.0001	0.0001	-0.0116***	0.0008	0.0008
	(0.0031)	(0.0034)	(0.0034)	(0.0030)	(0.0033)	(0.0033)
Male × Post			0.0078***			0.0074***
			(0.0026)			(0.0026)
Treated × Post × Male			-0.0130***			-0.0125***
			(0.0039)			(0.0039)
Firm size				0.0131***	0.0139***	0.0135***
				(0.0026)	(0.0035)	(0.0026)
Person-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Person Controls	No	No	No	Yes	Yes	Yes
Observations	153,062	83,895	236,957	152,460	83,372	235,832
R^2	0.906	0.884	0.907	0.907	0.886	0.908

Table IA3: Robustness: Employee Wages and Bonus Payments

This table repeats Table 3, except it uses as dependent variable employee wages and bonus payments. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the firm level.

	Male	Female	All	Male	Female	All
Treated × Post	-0.0146***	0.0030	0.0030	-0.0120***	0.0041	0.0043
	(0.0040)	(0.0046)	(0.0046)	(0.0037)	(0.0044)	(0.0044)
Male × Post			-0.0020			-0.0028
			(0.0036)			(0.0034)
Treated × Post × Male			-0.0176***			-0.0166***
			(0.0054)			(0.0051)
Firm size				0.0234***	0.0206***	0.0224***
				(0.0030)	(0.0037)	(0.0027)
Person-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Person Controls	No	No	No	Yes	Yes	Yes
Observations	144,811	79,001	223,812	144,235	78,510	222,745
R^2	0.866	0.828	0.865	0.869	0.829	0.867

Table IA4: Robustness: Firm-Year Fixed Effects

This table repeats column 3 of Table 3 and column 3 of Table 4 additionally controlling for firm-year fixed effects. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the firm level.

	Baseline	Treatment by year
	All	All
Male × Post	-0.0044	
	(0.0035)	
Treated × Post × Male	-0.0143***	
	(0.0051)	
Male × Treated × Year_{2004}		0.0048
		(0.0066)
Male × Treated × Year_{2005}		-0.0072
		(0.0073)
Male × Treated × Year_{2006}		-0.0116
		(0.0077)
Male × Treated × Year_{2007}		-0.0202**
		(0.0083)
Male × Treated × Year_{2008}		-0.0200**
		(0.0088)
Person-Firm FE	Yes	Yes
Year FE	Yes	Yes
Firm-Year FE	Yes	Yes
Person Controls	Yes	Yes
Observations	222,529	222,529
R^2	0.885	0.885

Table IA5: Gender Pay Gap Disclosure and Firm Performance: Dynamics

This table is the by-year version of Table 11. This table reports the effects of gender pay gap disclosure on firm performance. In columns 1-2, the dependent variable is the logarithm of sales per employee; in columns 3-4, the dependent variable is the logarithm of wages per employee; in columns 5-6, the dependent variable is the logarithm of pension and social security expenses per employee; in columns 7-8, the dependent variable is profits per employee. The sample is defined at the firm level. Detailed descriptions of the variables are given in Table A1. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the firm level.

	log(Sales/employees)		log(Wage/employees)		log(Pension & Soc.Sec./employees)		Profits/employees	
Treated × $Year_{2004}$	-0.0008	-0.0039	-0.0023	-0.0014	0.0206	0.0210	-0.0725	-0.433
	(0.0137)	(0.0108)	(0.0041)	(0.0042)	(0.0293)	(0.0291)	(3.661)	(3.669)
Treated × $Year_{2005}$	0.0005	-0.0102	-0.0076	-0.0058	0.0314	0.0298	-2.886	-3.494
	(0.0159)	(0.0135)	(0.0061)	(0.0060)	(0.0323)	(0.0324)	(4.504)	(4.471)
Treated × $Year_{2006}$	-0.0045	-0.0113	-0.0193***	-0.0181***	0.0441	0.0414	6.197	5.453
	(0.0176)	(0.0152)	(0.0069)	(0.0067)	(0.0341)	(0.0339)	(5.201)	(5.200)
Treated × $Year_{2007}$	-0.0395**	-0.0419**	-0.0379***	-0.0376***	0.0079	0.0064	4.477	4.031
	(0.0183)	(0.0168)	(0.0081)	(0.0076)	(0.0332)	(0.0331)	(5.500)	(5.470)
Treated × $Year_{2008}$	-0.0327*	-0.0364**	-0.0386***	-0.0371***	-0.0071	-0.0088	4.837	4.735
	(0.0192)	(0.0183)	(0.0091)	(0.0085)	(0.0364)	(0.0364)	(6.202)	(6.071)
Firm size		0.4050***		-0.1040***		0.1010***		26.27***
		(0.0224)		(0.0113)		(0.0187)		(5.189)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	22,414	22,391	22,429	22,391	22,374	22,351	21,602	21,564
R^2	0.845	0.879	0.849	0.863	0.544	0.547	0.621	0.625

Table IA6: Heterogeneity: Fraction of Female Managers

This table is similar to Table 10, except it focuses on the proportion of female managers. *Female Managers* is a dummy variable that takes the value 1 for firms with an above-median fraction of women in the top management team and 0 otherwise. *Treated×Female Managers* and *Male×Female Managers* are absorbed by the Person-Firm FE. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the firm level.

	Male	Female	All
Treated×Post	-0.0122***	0.0041	0.0044
	(0.0043)	(0.0055)	(0.0054)
Post×Female Managers	0.0116**	-0.0025	-0.0028
	(0.0047)	(0.0055)	(0.0054)
Treated×Post×Female Managers	-0.0052	-0.0008	-0.0010
	(0.0074)	(0.0086)	(0.0085)
Post×Male			-0.0082*
			(0.0043)
Treated×Post×Male			-0.0168***
			(0.0063)
Post×Male×Female Managers			0.0144**
			(0.0066)
Treated×Post×Male×Female Managers			-0.0042
			(0.0102)
Firm size	0.0220***	0.0195***	0.0211***
	(0.0028)	(0.0035)	(0.0025)
Person-Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Person Controls	Yes	Yes	Yes
Observations	145,262	79,027	224,289
R^2	0.871	0.828	0.868

Table IA7: Heterogeneity: Female First Child

This table is similar to Table 10, except focusing on managers' first-born child to construct *Female Child* variable. *Treated×Female Child* and *Male×Female Child* are absorbed by the Person-Firm FE. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively. Standard errors are clustered at the firm level.

	Male	Female	All
Treated×Post	-0.0221***	-0.0084	-0.0080
	(0.0051)	(0.0059)	(0.0059)
Post×Female Child	0.0011	-0.0047	-0.0047
	(0.0054)	(0.0057)	(0.0057)
Treated×Post×Female Child	-0.0001	0.0203**	0.0199**
	(0.0079)	(0.0087)	(0.0087)
Post×Male			0.0074
			(0.0049)
Treated×Post×Male			-0.0146**
			(0.0071)
Post×Male×Female Child			0.0059
			(0.0073)
Treated×Post×Male×Female Child			-0.0197*
			(0.0107)
Firm size	0.0211***	0.0197***	0.0206***
	(0.0030)	(0.0035)	(0.0026)
Person-Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Person Controls	Yes	Yes	Yes
Observations	122,232	74,528	196,760
R^2	0.851	0.816	0.848