Do firms respond to gender pay gap transparency?*

Morten Bennedsen† Elena Simintzi‡ Margarita Tsoutsoura§

Daniel Wolfenzon¶

November 5 2018

Abstract

We examine whether pay transparency closes the gender pay gap and affects firm outcomes. The paper exploits a 2006 legislation change in Denmark that requires firms to provide gender dis-aggregated wage statistics. Using detailed employee-employer administrative data we find that the law has an effect in reducing the gender pay gap, primarily through slowing the wage growth for male employees. This effect is more pronounced for firms whose managers have more daughters, presumably due to the effect of daughters on managerial preferences, and for industries with higher gender pay differentials pre-treatment. Such changes in firm wage policies following the passage of the law are associated with negative outcomes on overall firm productivity, but also with a reduction in firm wage bill, resulting in no significant effects on firm profitability.

*We thank Daniel Ferreira (discussant), Camille Hebert (discussant) and Rebecca Zarutskie (discussant). We are grateful for excellent comments from seminar participants at UC San Diego, IESE Barcelona, Columbia Business School, Cornell University, Darden, INSEAD, UIC, LBS Summer Finance Symposium, Stanford SITE conference, CEPR Incentives, Management and Organization 2018 conference, and the Corporate Finance Beyond Public Companies conference. We thank Brian J. Lee 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).

†Niels Bohr Professor, University of Copenhagen and André and Rosalie Hoffmann Professor, INSEAD

‡Kenan-Flagler School of Business, University of North Carolina

§Cornell University and NBER

¶Columbia University and NBER

I Introduction

Gender pay disparities characterize labor markets in most developed countries (OECD, 2015).[1] When a man earns 100 dollars, a woman earns 77 in US (Goldin (2014)), 78.5 dollars in Germany, 79 dollars in the UK and 83.8 on average across EU 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 change in Denmark to study how transparency through gender based wage statistics may affect firm wage policy and outcomes.

There is an ongoing debate about 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 and confidentiality.[4] The effect of transparency on gender pay disparities is an open empirical question, as it is unclear whether transparency will incentivize firms to respond by adjusting their compensation policies and, if they do so, by which margins.

---

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

[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 U.S. government in 2016 required large companies to report salary data broken down by gender, starting in 2017 but the rule got halted 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 key to 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

Studying this question empirically requires addressing two key challenges: finding exogenous variation in transparency at the firm level as well access to wage data at the individual level. To address these difficulties, 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. Under the 2006 law, 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. In addition, we use data at the employee-firm level from the Danish Statistics matched employee-employer administrative dataset. We employ a difference-in-differences approach, where treated firms employ 35-50 employees on average from 2003 to 2005, the years prior to the introduction of the law, and control firms employ 20-34 workers on average. Using detailed worker-firm data, we then compare the change in employee and firm outcomes around the passage of the law for treated firms relative to control firms.

Our sample firms pay their male employees a 18.9% wage premium before the regulation is introduced. This gender pay differential is not driven by differences in demographics, work experience, macro trends or selection into specific occupations as we are able to absorb such variation using detailed controls in our specifications. We find that transparency lowers the gender wage gap: it is reduced by 1.4 percentage points in treated relative to control firms, or a 7% reduction relative to the pre-regulation wage gap. Uncovering the source of adjustment, we show that wages for all employees (both male and female) increase over time; however, male wages in treated firms increase by less and female wages in treated firms (weakly) increase by more, thus contributing to an overall decline in the male wage premium for treated firms following the law change.

To further account for important drivers of gender pay disparities (Blau and Kahn, 2017), not necessarily related to pay discrimination, such as differences in skills, selection of women in certain occupations, industries or firms, we employ the full capacity of the firm-worker administrative data and include interacted firm and individual fixed effects in our specifications, in addition to individual time-varying controls and year fixed effects.

We find that wages of male employees in treated firms are lower by 1.7 percentage points relative to male employees in control firms and this reduction in wages is statistically significant. On the contrary, we find a positive but not significant relation for female workers. Overall, we show that men's wages grow by 2 percentage points less relative to women in treated as opposed to control firms following the law.

We provide additional analysis that further supports a causal interpretation. First, we estimate the effect of the law by-year and find no evidence of pre-treatment trends. Second, we perform placebo tests using alternative employee size cutoffs to define treatment and find no significant effects. Third, we show our results are robust to estimating our analysis 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 changes in firm labor demand, further alleviating concerns that time-varying differences between treated and control firms are driving our findings. Fourth, we get similar results when we use hourly wages as our compensation measure or when we also consider non-salary compensation components, such as bonus compensation.

Besides documenting that pay transparency against gender pay discrimination is effective in changing compensation within firms, we provide evidence on how the law affected employee reallocation. Using the same empirical design at the firm level, we show that treated firms hire more female employees as compared to control firms in specifications that include firm and year fixed effects. This is in line with an argument that the supply pool of female employees increases as gender pay transparency improves and thereby gender pay gap closes. On the contrary, we do not find a statistically significant effect on female employees' departures following the law passage. We also find that the law has spillover effects on promotion decisions to the favor of female employees. We find that women tend to be promoted from the bottom of the hierarchy to more senior positions, while we do not find any significant change in promotions for male employees.

In additional tests, we examine the implications of gender pay transparency on firm outcomes. For one, our findings suggest that the law resulted in lower wage growth for treated firms as firms pay relatively lower wages to male employees. Indeed, we confirm our finding at the firm level where we show a negative and significant effect on treated firms' wage bills, which are lower by 2.8%, as compared to control firms. Moreover, the law significantly affects employee productivity. A priori, the effect on productivity is ambiguous. If information on gender pay gaps will lower 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 will create 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 controls following the passage of the law. As such, the negative effect on productivity is offset by firms' lower wage costs, resulting in no significantly different effects on firm profitability.

Finally, we explore potential mechanisms explaining firm responses to the law. We argue that managerial preferences and pre-law industry gender pay differentials are non-mutually exclusive factors that can help explain the way firms adjust to increased transparency following the regulation. Fist, consistent with the idea that managerial styles affect corporate policies (Bertrand and Schoar, 2003), we argue that managerial preferences may affect how managers respond to the regulation for transparency on pay equity in the workplace. We use the finding in the literature that men parenting daughters are more likely to adopt pro-women preferences because they have experienced a higher degree of "female socialization" (Dahl, Dezső, and Ross, 2012). Indeed, we show that managers with more daughters exhibit higher sensitivity to the law passage closing the gender pay gap by increasing female compensation relatively more.

Second, firms' responses may depend on the pre-law gender pay inequality if increased transparency led to greater accountability. Using the pre-law within occupation gender pay inequality in the industry, as a proxy for pre-law within occupation firm level wage differentials, we see a stronger adjustment if within occupation inequality at the industry is higher prior to the law introduction.

The paper contributes to the literature on the effects of mandated pay transparency. Using government employees in California Card et al. (2012) shows that after government employee salaries are published online, aggregate worker satisfaction drops. 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. Both of these studies focus on the public sector sector.[5] We provide the first evidence based on private firms.

Moreover, the paper relates to a growing literature studying how wage disparities within the firm may affect employee or firm outcomes. For example, 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. Breza et al. (2018) uses 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. However, these papers do not link wage transparency to firm outcomes. In addition, our study is the first study with a specific focus on gender disparities—an issue of debate.

The paper also relates to the vast literature that studies the sources of gender pay disparities.[6] Although most work in the area has focused on determinants of the gender pay gap, there is limited evidence on what might be the possible solutions. After accounting for the common drivers of gender pay gap in the literature, we provide support for pay transparency as a potential avenue to mitigate gender pay gaps within organizations.

Our paper contributes to a growing literature on gender and organizations that point

[5]The mechanisms that affect wage setting in the public sector might be different than those affecting wages in the private sector, 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.

[6]See, for example, Goldin (2014) and Blau and Kahn (2017) for a review of the literature.

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 for firms with more female executives. Adams and Ragunathan (2017) show that gender barriers tend to discourage women from working in finance. Duchin, Shamir, Patriat, Kim, Vitek, Sapiro, and Harel (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 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 on June 2006. 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 (occupation classification 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.[7] 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 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 the wage and establish an action plan for equal pay to be implemented within a three-year period. The law establishes that non-compliance by firms would be punished by a fine. The new provisions came

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

into force on January 2007.

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 the year prior to the introduction of the law and the control firms are those that employ 20-34 workers. We are taking a narrow window around the 35-employee cutoff so that we compare firms of similar size.

We design our empirical strategy around the 35 threshold and do not take into account the criterion that firms 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 Association "some firms may still internalize the law even when do not satisfy the second criterion (DISCO)."[8]. 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 EU and the ILO only mentions the requirement of the threshold of 35 employees.[9]

We use a panel of employee-firm-years to test whether disclosure of information on wages by gender 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:

[8] Private communication

[9] 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).

$$
\begin{aligned}
log(wage)_{ijt} &= \alpha_{ij} + \alpha_t + \gamma_1 X_{it} + \gamma_2 X_{ijt} + \beta_1 I(Treated_{ijt}) + \beta_2 I(Post_{ijt}) \\
&+ \beta_3 I(Male_{ijt}) + \beta_4 I(Treated_{ijt} \times Post_{ijt}) \\
&+ \beta_5 I(Treated_{ijt} \times Male_{ijt}) + \beta_6 I(Post_{ijt} \times Male_{ijt}) \\
&+ \delta\, I(Treated_{ijt} \times Post_{ijt} \times Male_{ijt}) + \varepsilon_{ijt} \qquad (1)
\end{aligned}
$$

where i, j, 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; X_{it} and X_{ijt} capture time-varying firm- and individual-level control variables, respectively. We add controls for time-varying individual characteristics (age, experience, education), following Blau and Kahn (2017). α_t is year fixed effects to absorb aggregate macroeconomic shocks. We also include interacted individual firm fixed effects, α_{ij}. The individual times firm fixed effects allows us to control for time-invariant person characteristics, time invariant firm characteristics as well as the match between firms and workers. Effectively we are able to identify the effect on wages, clean of compositional changes at the firm. Our empirical strategy allows to account for important drivers of gender pay disparities (Blau and Kahn, 2017), not necessarily related to pay discrimination, such as differences in skills, selection of women in certain occupations, industries or firms. Standard errors are clustered at the firm level. We start our sample in 2003 to provide sufficient years to estimate the baseline effect for each firm-employee and end in 2008 to avoid overlap of our sample with the financial crisis which had economy-wide effects on wages.

We also examine the effect of the law on firm outcomes, such as hiring decisions, productivity and profitability. Using instead a panel of firm-years, we estimate OLS regressions of the following form:

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

where i, 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; X_{it} captures time-varying firm-level control variables. The coefficient of interest δ captures the differential effect of the law on the dependent variables for treated and control firms. α_t is year fixed effects to absorb aggregate macroeconomic shocks. We also include firm fixed effects, α_i 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 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 detailed information for employees' compensation, demographics and occupation. For compensation we have information on employees' wage and bonus. Furthermore, for each employee we observe their age, gender and education as well as their position in the organization.

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, excluding CEOs and board directors. We drop firms in industries unaffected by the policy (farming, gardening, forestry and fisheries). We require firms to have financial information which results in dropping 0.8% of firm years over our sample period.

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 thousand ($34.4), while for the control group is $53 thousand ($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 or control group hold a college degree.

Treated firms are larger than control firms by definition. For example, the average treated firm has 42 employees pre-treatment, assets of $7.2 million and sales of $11.68 million as compared to 26 employees, $6.1 million in assets and $7.73 million in sales 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.

Panel B2 shows the pre-law wage premiums[10] for treated and control firms. We estimate the male wage premium both without controls and with adding year fixed effects, time-varying individual characteristics (age, experience, education), control for sales, occupation fixed effects and industry fixed effects. In our stricter specification we observe that the male wage premium is 20.2% for the treated group and 17.9% for the control group prior to the regulation.

[10] The male wage premium is the estimate of the coefficient on a male dummy in a log wage regression

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. Wages increase for all employees, irrespective of their gender, in both the treated and the control group. However, male employee wages grow by less in treated firms as compared to control firms and the difference in the average increase between the treated and the control groups (-0.0156) is statistically significant at the 1% level. Similarly, both treated and control firms' female employees receive higher wages on average following the reform. The increase is higher, however, for the female employees in the treated firms as compared to control firms, although this difference is not statistically significant. These univariate comparisons suggest that the reform results in lower wage growth for male employees and higher (although not statistically significant) wage growth for female employees. This results in about 2 percentage points lower wage increase for male versus female employees in treated relative to control firms.

We next turn to our regression analysis and use detailed employee-firm wage data to account for the possibility that compositional changes at the firm level may affect the observed differences in wages. We thus 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 as compared 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 relatively less as compared to male employees in control firms following the passage of the law, similar to our univariate results in Table 2. The effect is statistically significant at the 1% level and economically important. Treated male wages are reduced by 1.67 percentage points more relative to male wages in the control group. 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 employees as compared to wages of female employees following the passage of the law relatively to a group of control firms. The triple difference coefficient shows that male wages grow by 2 percentage points less then female wages in treated versus control firms and the effect is statistically significant at the 1% level.

In columns 4-6, we repeat our estimation additionally controlling for firm size (proxied by logarithm of sales) to control for the well documented employer size-wage effect (e.g. Brown and Medoff (1989); Idson and 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. Including firm size is also important in our setting given the treated group includes by construction larger firms. The estimated coefficients remain virtually unchanged after controlling for firm size. In Internet Appendix Table IA1, we repeat our analysis including in the sample only individuals that worked for the firm at least one year in the pre-law period and one year in the post period. This test further addresses concerns that changes in the composition of a firm's employees are driving our results. The estimated coefficients are qualitatively similar, except the positive effect on female wages is now weakly statistically significant.

V.2 Identification concerns

Our analysis allows us to absorb a lot of unobserved variation by including detailed individual controls and interacted person and firm fixed effects. Moreover, the fact that our estimated effect on wages is concentrated on male employees (as opposed to all employees) mitigates identification concerns related to omitted variables which might correlate with firm size, e.g. firms of different sizes adjusting their compensation differently to their investment opportunity sets. To drive our findings, an omitted variable would not only need to be correlated with wages, but also differentially affect male wages across different firms. In this section, we provide further evidence that our results are consistent with a causal interpretation.

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 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 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.

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 20-35 employees in the 2003-2005 period and placebo control firms those firms with 5-19 employees. In column 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 as described by the law.

Moreover, we repeat our baseline analysis additionally controlling for interacted firm and year fixed effects. These controls allow us to absorb any time-varying changes at the firm level that could be driving our results. To include firm-year fixed effects, we need variation within firm-year and as such, we can only repeat specifications similar to those in column 3, Table 3, where we provide a triple difference estimate comparing the effect of the law between male and female employees in treated versus control firms. Table IA2, in the Internet Appendix, repeats estimates in Tables 3 and 4 and returns very similar estimates when we additionally control for firm level shocks.

One potential concern with the interpretation of our findings could be that the law resulted in men working less hours in treated firms as this could mechanically improve the gender wage statistics. To examine whether this alternative interpretation might be true, we replicate Table 3 using instead employee hourly wages. In Internet Appendix IA3, 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 require 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 in bracketed hours worked and it is capped, which is not the case for our baseline wages measure. Moreover, we examine the possibility that the reduction in salary may be (partially) offset by relatively higher bonuses offered to male workers in treated firms. Thus, in Internet Appendix Table IA4 we estimate the effect of the law on employee total compensation (wage and bonus payment). Bonus payments do not seem to materially affect our estimates as shown by the coefficients in Internet Appendix Table IA4.

VI Pay by Hierarchy, Hiring and Promotions

Does increased transparency affect all employees in the firm, or do we observe 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 the results for managerial employees in the high hierarchy levels; columns 4-6 instead present results for non-managerial employees in lower hierarchy levels. It can be observed that there is no significant relationship on pay for either men or women at the top, while there is a strong and significant effect for all other employees.[11] 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.

Although the law seems to have a clear effect on wages, as intended by the regulator, this might not be the only response by firms. Changes in the way similar employees of different gender are compensated might affect the demand or supply for those employees, resulting in differences in hiring or departure rates. Alternatively, the law mandate for fairer practices may have spillover effects on other firm decisions such as employee promotions. We examine the effect of the law passage on each of these different outcomes next.

We start by computing hiring rates for female employees at the three hierarchy levels within the firm, as in Table 6. Thus, *Joining rate* is the total number of female employees joining the firm-hierarchy in a given year t normalized by the total number of employees joining. (By construction, hiring rates for men and women sum up to one and thus,

[11] In unreported results we replicate this analysis defining firm hierarchies based on worker's occupations following Caliendo, Monte, and Rossi-Hansberg (2015) and Friedrich (2015). Hierarchical layers group occupations in a systematic way in four layers to focus on vertical relationships between top managers, middle managers, supervisors, and workers. Using this alternative measure of firm hierarchies, we find consistent results that significance comes from the non-managerial layers.

we only present hiring rates for female employees). We thus 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 results in Panel A, Table 7. Conditional on hiring, we find that treated firms hire a higher share of women in the intermediate hierarchy levels, which is statistically significant at the 5% level. We also find an economically large effect for low hierarchy levels although this effect is statistically noisier, and we observe no difference at the top. The pre-law average female joining rate is 37% and 43.6% for intermediate and low hierarchy levels respectively, and the joining rate of women increased by 4.4 percentage points and 2.5 percentage points, respectively. This finding is consistent with the fact that firms are able to attract more female employees in positions where they offer a fairer compensation.

Similarly, we define departure rates as the number of female employees leaving in given firm-hierarchy-year normalized by the total number of employees leaving in that firm-hierarchy-year. Our goal is to capture voluntary departures from the firm rather than firings. Therefore from our measure we exclude departures where the employee remained 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 weak, this evidence suggests that women are more likely to leave from positions where there was no adjustment in pay towards closing the gender pay gap. Overall, these results suggest that women participation rates increase in those occupations where male wage premium is reduced.

To examine firm promotion decisions, we define a dummy that takes a value of 1 if a given individual is promoted to a higher hierarchical level. The measure is thus meaningful for the intermediate and low level employees. Table 8 present 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 controls. The promotion probability before the reform is 2.2% for males and 2% for females, and although it does not change for males, it increases by 1.2 percentage points for female employees. 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

We next explore whether the effect of the law on gender pay and employee reallocation also affects firm productivity and profits. In doing so, we need to caution that the average treatment effect we are able to estimate may be driven by both compensation or compositional changes at the firm level as a result of the law, limiting our ability to pin down the precise mechanism or responses by different employee groups within the firm.

We perform our analysis at the firm level in a specification with firm and year fixed effects. We report the results in Table 9, with and without controlling for firm size. In columns 1-2, we examine the effect of the law on firm productivity of treated firms as compared to the group of controls. The effect on productivity is theoretically ambiguous. If information on gender pay gap will lower job satisfaction of female employees, that should negatively impact their productivity. A similar effect should be observed if male employees get dissatisfied with firms' lowering their wages relative to their peers. However, if increased transparency and firms' responses create a sentiment of fairness among employees, then productivity should be positively impacted. 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 controls, and this reduction is statistically significant at the 5% level.

However, this drop in productivity seems to be exactly offset by the lower wages due to the fact that firms respond to the law by lowering male employee wages. Indeed, columns 3 and 4, Table 9 show that the average wage per employee (log-transformed) is reduced by 2.8%, canceling out the negative productivity effect. Note 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.

These results can explain why we do not observe any significant effect on firm profitability, in columns 7-8, Table 9. Because of the accounting identity, the effect on profits must reflect some combination of the decrease in costs but also of the decrease in revenues. Using profit per employee, as our measure of profitability, we find no effect of the law on firm profits. A null result on profitability renders no support for critics of the law who argue that disclosing pay gaps may be particularly costly to firm profits.

VII.1 Mechanisms

We next explore potential channels explaining firm's response to the mandate for transparency following the passage of the law. We propose two non-mutually exclusive mechanisms: 1) managerial preferences; and 2) pre-law gender pay differentials.

First, we propose that managerial styles may affect the way firms respond to the law, as they have been shown to affect corporate policies (Bertrand and Schoar, 2003). To create a proxy for managerial preferences that would favor women, 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).[12] As such, managers with daughters should not only be more likely

[12] 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 U.S. 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.

to follow fairer pay practices towards women,[13] but they should also exhibit greater sensitivity to the law passage.

To test this, we consider a firm's managerial team to be the top five earners in the firm in the 2003-2005 period. We exclude female managers and consider (up to) five male managers.[14] We then 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 the above categorical variable for each firm's managerial team and define *Female Child* to be one if the firm average is above the sample median, 0 otherwise. We augment our baseline specifications by interacting *Treated* $\times$ *Post* with *Female Child.* Table 10 presents the results. We find that firms where male managers have more daughters pay higher wages to their female employees following the law passage relative to controls, while we observe no significant difference for male employees. In column 3, Table 10, we show that this results in closing the gender pay gap by 2.4% more for treated firms whose managers tend to be more favorable to women, as compared to controls, and this effect is statistically significant at the 5% level. In Internet Appendix Table **??**, 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.

Second, we consider the role of the pre-existing gender pay inequality.Increased transparency might lead to increased accountability thus firms with higher pre-existing inequality might be more responsive to reduce pay inequality. Ideally, we would would like to have a measure of gender pay inequality at the firm level but this measure would be very noisy given that firms are unlikely to have a large number of men and women in the same occupation so that we can have a meaningful measure. Therefore we use the

[13] In unreported regressions, we confirm that managers that have more daughters tend to offer higher wages to women in the pre-treatment period, but not to men, and this difference is statistically significant.

[14] 20% of top managers in our sample are women.

pre-law within occupation gender pay inequality in the industry, as a proxy for pre-law within occupation firm level wage differentials

To this end, we define gender pay gaps at the industry-occupation level by computing the median log difference in wages by gender at the industry-occupation-year level and averaging over the pre-treatment period (2003-2005). We augment our baseline specification by interacting $Treated \times Post$ with *Ind. Gender Gap*, the pre-treatment industry-occupation gender pay differential. In Table 11, we show that treated firms in industries with high gender disparities pre-treatment pay relatively lower wages to male employees, although this difference is not statistically significant. In contrast, they pay female employees more relative to controls, 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%. Most importantly, in column 3, we show that gender pay gaps close by more when pre-treatment inequality is higher. Specifically, a one standard deviation increase in *Ind. Gender Gap* is associated with a 1.6% reduction in the gender pay gap.

In sum, these results suggest different mechanisms at play that can plausibly explain the observed response by firms to the increased transparency on gender pay. A caveat of this analysis, however, is that these tests are suggestive and we cannot assess the relative importance of each of these mechanisms.

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 exogenous 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 requires certain companies 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. We argue that firm, industry and managerial characteristics play a non-mutually exclusive role in explaining firms' response to the increased transparency.

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.

Bertrand, M., Schoar, A., 2003. Managing with style: the effect of managers on firm policies. Quarterly Journal of Economics 118, 1169–1208.

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.

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 CEOâĂŹs Children Affect the Wages of His Employees. Administrative Science Quarterly 57, 669–693.

Duchin, Y., Shamir, R. R., Patriat, R., Kim, J., Vitek, J. L., Sapiro, G., Harel, N., 2018. Patient-specific anatomical model for deep brain stimulation based on 7 Tesla MRI. PLOS ONE 13, 1–23.

Egan, M. L., Matvos, G., Seru, A., 2017. When Harry Fired Sally: The Double Standard in Punishing Misconduct. Working Paper 23242, 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.

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 24219, National Bureau of Economic Research.

Mas, A., Moretti, E., 2009. Peers at work. American Economic Review 99, 112–45.

Mueller, H. M., Ouimet, P. P., Simintzi, E., 2017. Within-firm pay inequality. Review of Financial Studies 30, 3605–3635.

Oswald, A. J., Powdthavee, N., 2010. Daughters and Left-Wing Voting. The Review of Economics and Statistics 92, 213–227.

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.

Appendix: Definitions of Variables

Variable	Definition
Firm-level variables	
Treated firms	An indicator variable that takes the value 1 for firms with average employment between 35 and 50 in the pre-disclosure period (2003-2005) and 0 otherwise
Control firms	An indicator variable that takes the value 1 for firms with average employment between 35 and 50 in the pre-disclosure period (2003-2005) and 0 otherwise
Assets	Measured in real USD. The source is KOB.
Sales	Measured in real USD.Source is KOB.
Firm age	Firm age based on the firm foundation date. The information source is the business registry.
Hierarchy	We follow Caliendo, Monte, and Rossi-Hansberg (2015) and Friedrich (2015) in constructing a measure on how hierarchical a firm is. The measure is based on the number of different occupational layers represented by workers in a firm. We use workers' occupations as reported in the Danish occupational code DISCO. The source is IDA
Employee-level variables	
Male	An indicator variable that takes the value 1 if the person is male, and 0 otherwise. The source is the Danish Civil Registration System.
Age	Employee age. The source is the Danish Civil Registration System. It is recoded into quartiles when using as a regression control.
Experience	Employee's number of years worked. It is recoded into quartiles when using as a regression control.
No. of children	The number of the employee's living children. The source is the Danish Civil Registration System.
Wage	Total annual wage of the employee. The information comes from the administrative-matched employer-employee dataset (IDA).
College degree	An indicator variable that takes the value 1 if an employee has completed a bachelor's degree. The variable is constructed based on information from the official Danish registry.
Promotion	An indicator variable that takes the value 1 if the employee got a promotion that year, and 0 otherwise. The promotion variable is constructed based on information of employee position from IDA.
Separation	An indicator variable that takes the value 1 if the employee left the company that year, and 0 otherwise. The separation variable is constructed based on information from IDA.

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 with average employment between 35 and 50 and controls are those with average employment between 20 and 34, in the pre-law period (2003-2005). The variables are averaged over the pre-law years 2003-2005. The table reports unconditional means, medians and standard deviations. For the conversion from DKK to USD we use the spot exchange rate at the year-end. Firm-level variables (except employment and female shares) 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 B1 - 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
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

Panel B2 - Pre-law Male Wage Premium

	All	Treated	Control	Difference
Male Wage Premium	0.240***	0.257***	0.226***	0.031***
(No Controls)	(0.0056)	(0.0094)	(0.0068)	(0.0115)
Male Wage Premium	0.189***	0.202***	0.179***	0.019***
(Full Specification Controls)	(0.0041)	(0.0063)	(0.0054)	(0.0072)

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. Column (1) pertain 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 control (Column 1) and treated group (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 third row reports the difference-in-difference-in-differences result, the difference between the change in the male wages and female wages around the disclosure law, in treated versus control firms. Firms with average employment between 35 and 50 in the pre-law period (2003-2005) are identified as treated, and firms with 20-34 are identified as the control group. The wages are log-transformed. *** corresponds to statistical significance at the 1% level. 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	0.0810***	0.0967***	-0.0157***
	(0.0034)	(0.0029)	(0.0044)
Female	0.1015***	0.0981***	0.0034
	(0.0040)	(0.0033)	(0.0051)
Difference-in-Difference-in-Differences (DDD)			-0.0190***
			(0.0061)

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. Treated firms are those with average employment between 35 and 50 and controls are those with average employment between 20 and 34, in the pre-law period (2003-2005). Post is 0 for years 2003-2005 and 1 for years 2006-2008. Person controls include employee experience and age. All variables are defined in the Appendix. ***, **, 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)
log(Sales)				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}			0.00748
			(0.00602)
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)
log(Sales)	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. In columns 1-2, the placebo treatment group includes firms with average employment of 20-35 and the placebo control group includes firms with average employment of 5-19 in the pre-treatment years 2003-2005. In columns 3-4, the ranges are 50-65 and 35-49 employees, respectively. In columns 5-6, 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)
log(Sales)	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)
log(Sales)	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. All variables are defined in the Appendix. ***, **, 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)
log(Sales)		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	0.00765	0.00779	-0.0138	-0.0130
	(0.0242)	(0.0243)	(0.0208)	(0.0208)	(0.0185)	(0.0185)
log(Sales)		0.0149		-0.00406		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 is promoted if he/she works at a higher hierarchy level that year, and 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)
log(Sales)	-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's 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 & Social Security expenses per employee; in columns 7-8, the dependent variable is profits per employee. The sample is defined at the firm level. All variables are defined in the Appendix. ***, **, 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)
log(Sales)		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: Mechanisms: Managerial Preferences

This table reports the effects of gender pay gap disclosure and its interaction with whether managers have pro-women preferences on employee wages. We exclude female managers and consider (up to) five male managers in the pre-treatment (2003-2005) period. 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. *Female Child* is an indicator that takes a value of 1 if the firm average exceeds the sample median, and 0 otherwise. 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
Tested×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)
Log(Sales)	0.0211***	0.0197***	0.0206***
	(0.0030)	(0.0035)	(0.0026)
Person-Frim 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: Mechanisms: Industry Gender Pay Gap

This table reports the effects of gender pay gap disclosure and its interaction with pre-treatment industry gender pay gap on employee wages. We define *Ind. Gender Gap* at the industry-occupation level by computing the median log difference in wages by gender at the industry-occupation-year level and averaging over the pre-treatment period (2003-2005). *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)
log(Sales)	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

Online 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 our baseline specification including in the sample only employees that were working for the firm at least one full year before the law and one full year after. Years where employees joined or left are not counted as full years. The 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	-0.0097***	0.0058*	0.0059*
	(0.0028)	(0.0033)	(0.0033)
Male × Post			-0.0002
			(0.0026)
Treated × Post × Male			-0.0159***
			(0.0039)
Log(Sales)	0.0183***	0.0144***	0.0170***
	(0.0023)	(0.0029)	(0.0021)
Person-Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Person Controls	No	No	No
Observations	94,118	49,451	143,569
R^2	0.933	0.894	0.929

Table IA2: Robustness: Firm-Year Fixed Effects

This table repeats our baseline specification and the $Treated \times Year$ effects of gender pay gap additionally controlling for firm-year fixed effects. The sample restrictions 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.

	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 IA3: Robustness: Employee Hourly Wage

This table reports the effects of gender pay gap disclosure on employee hourly wages. 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	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)
log(Sales)				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 IA4: Robustness: Employee Wages and Bonus Payments

This table reports the effects of gender pay gap disclosure on employee wages and bonus payments. 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	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)
log(Sales)				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 IA5: Top 5 Earners Female First Child Ratios

This table repeats Table 10, except focusing on managers' first child to construct *Female Chile* 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
Tested×Post	-0.0221***	-0.0084	-0.0080
	(0.0051)	(0.0059)	(0.0059)
Post×Female Child Ratio	0.0011	-0.0047	-0.0047
	(0.0054)	(0.0057)	(0.0057)
Treated×Post×Female Child Ratio	-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 Ratio			0.0059
			(0.0073)
Treated×Post×Male×Female Child Ratio			-0.0197*
			(0.0107)
Log(Sales)	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

Table IA6: Gender Pay Gap Disclosure and Firm Performance: Dynamics

This table is the by-year version of Table 11: Gender Pay Gap Disclosure and Firm Performance. This table reports the effects of gender pay gap disclosure on firm's 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 & Social Security expenses per employee; in columns 7-8, the dependent variable is profits per employee. The sample is defined at the firm level. All variables are defined in the Appendix. ***, **, 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)
log(Sales)		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