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“Disclosing the Gender Pay Gap”

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

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

Do firms respond to gender pay gap transparency?*

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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.

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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 in Denmark that increased transparency by requiring companies to inform all employees on average wages by gender and occupation.

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.⁴

The effect of transparency on the gender pay gap and firm outcomes is in the end an empirical question. It is unclear whether transparency will provide sufficient incentives

¹<http://www.oecd.org/gender/data/genderwagegap.htm>

²In 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.

³AFL-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).

⁴See 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>

for firms to adjust their compensation policies. Moreover, these wage adjustments might have unintended consequences for firm outcomes such as firm productivity and ultimately profits.

Studying this question empirically requires addressing two key challenges: finding exogenous variation in transparency at the firm level as well information on wage data at the individual level. 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. We obtain wage and employment records at the individual level from the Danish Statistics matched employee-employer administrative dataset.

In our research design we compare firms above the 35 employee threshold ("treated firms") to firms below ("control firms"). 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-difference model where 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. Using detailed worker-firm data, we then compare the change in employee and firm outcomes around the passage of the law.

In terms of the effect of transparency on firm compensation policies, 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. The effect is statistically significant at the 1% level and economically important. On the contrary, female wages in treatment and control firms grow by roughly the same rate. These results imply that the wage gap in treatment firms closes by roughly 2 percentage points more than in control firms.

Our preferred specification is one in which we control for a variety of time-varying firm and individual characteristics (age, work experience, firm size), year fixed effects as well as interacted individual and firm fixed effects. By including these fixed effects we

control for time-invariant person characteristics, time invariant firm characteristics, as well as the match between firms and workers. These fixed effects allows 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. 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.

After documenting the effects 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 pool of female employees increases as the gender pay gap closes in these firms. 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.

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.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) 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. However, these papers do not link wage transparency to firm outcomes. In addition, our study is the first study

⁵The 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.

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 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 Raganathan (2017) 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 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 an occupation classification code (6-digit DISCO code) shall each year prepare gender-segregated wage statistics for the purpose of consulting and informing the employees of wage gaps between men and women in the

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

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. The law establishes that non-compliance by firms would be punished by a fine. The new provisions came 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 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 the control firms are close in size to the treatment 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 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")⁸, some firms complied with the law even when 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

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

⁸DA 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.

Union (EU) and the International Labor Organization (ILO) only mentions the criterion that firms above the 35 employee threshold must comply.⁹

We use a panel of employee-firm-years to test whether transparency 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 by estimating the following OLS regression:

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

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; $Treated$ 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_{it} and Z_{ijt} capture time-varying firm- and individual-level control variables, respectively. X_{it} controls for firm size proxied by sales (log-transformed). Z_{ijt} 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, time invariant firm characteristics, as well as the match between firms and workers. Essentially, we compare the *same employee at the same firm* before and after the regulation. This empirical strategy

⁹European 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."

controls for differences in skills, occupation, and industries that previous literature has identified as important drivers of gender pay disparities (Blau and Kahn, 2017).

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

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

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). 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_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’ wage and bonus. 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 public and private 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 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, as shown in Panel B1, 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.

Panel B2 shows the pre-law wage premiums for treated and control firms.¹⁰ We estimate the male wage premium both without controls and with adding year fixed effects, individual characteristics (age, work 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.

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 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. Similarly, female wages in all firms in the sample increase following the reform, but there is no significant differences between treated and control firms. These univariate comparisons suggest that the reform results in a 2 percentage points lower wage growth for male employees than female employees.

Another interesting observation from the table is the comparison between the growth rate of male versus female wages in the treatment and control firms. In control firms, the

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

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 treatment 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 slower than female wages in treatment firms implies a reduction in the gender pay gap of around 2 percentage points which is meaningful relative to the level of the pay gap prior to the reform of 25.7%.

We next turn to our 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 lower by 2 percentage points 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 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

by construction larger firms. The estimated coefficients remain virtually unchanged after controlling for firm size.¹¹

V.2 Hours worked and bonus payments

Our main result is that male wages in treated firms grow slower. While this is consistent with the firms slowing the growth rate of male employee compensation, there could be potential alternative explanations for this fact. One is that this reduction in wages is driven by men in treated firms working fewer hours while the compensation per hour remains the same. The other is that treated firms reduce male wages but offset this reduction by increasing the bonuses.

To examine the first alternative, 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 Sogaard (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 do not materially affect our estimates.

V.3 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.

¹¹Our results are robust to considering instead a sample of employees conditional on working with the firm at least one year before the law and one full year after (Internet Appendix Table IA1).

A first order concern is whether wages follow differential trends in small and large firms. This, however, will 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 on either male or female wages between treated and control groups prior to the adoption of the law. However, male wages decline in treatment 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 men and women) were following parallel trends prior to the law and that our effects only show 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 to be firms with 20-35 employees prior to the law and placebo control firms 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 as 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 3,¹² 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 we obtain 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 the results for managerial employees in the high hierarchy levels; columns 4-9 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

¹²Firm-year fixed effects subsume the coefficient on *Treated \times Post*.

effect for all other employees.¹³ 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 or supply for 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 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. *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, 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 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. One possible explanation is 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 a

¹³In unreported results we replicate this analysis defining firm hierarchies based on worker's occupations following Caliendo, Monte, and Rossi-Hansberg (2015) and Friedrich (2015) and find similar results.

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, 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 statistically insignificant, 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 affects firm productivity and profits. 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 *a priori* unambiguous. 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 lower wage growth relative to their peers. However, if increased transparency and firms' responses create a sentiment of fairness among employees, then productivity should be positively impacted. Although the law may differentially affect productivity of males versus females, our estimates capture the average effect of the law on firm productivity as 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 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.

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 might be explained by the offsetting effects of lower employee productivity and wages.¹⁴

¹⁴In 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 wages is not contemporaneous but rather precedes that of productivity by one year suggesting a causal link.

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 managerial 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. Our first measure, *Female Managers*, is the fraction of women in the top management team. Given that only 20% of top managers in our sample are women, we construct a proxy for male managers preferences towards pro-women policies. This second measure would allow 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, Dezsó, and Ross, 2012).¹⁵ 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 IA6 and Table 10 we augment our baseline specifications by interacting $Treated \times Post$ with *Female Managers* and *Female Child*. In Table IA6, we find weak

¹⁵Examples 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.

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. In Table 10, 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 percentage points 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 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.¹⁶ This suggest preferences of the management team play a role in the way firm responded to the regulation.

Second, we consider the role of the pre-existing gender 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. Therefore we use the 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 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).

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.

¹⁶In unreported regressions, we confirm that these same set of firms offered higher wages to women, but not to men, in the pre-treatment period.

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%. 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, 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.

Investigating empirically 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.

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Table 1: Summary Statistics

This table reports summary statistics for the employee-level (Panel A) and firm level (Panel B1) 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 workers. 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. Panel B2 shows the pre-law wage premiums, estimated as the coefficient on a male dummy in a log wage regression. Full specification controls include year fixed effects, individual characteristics (age, work experience, education), firm sales, occupation fixed effects and industry fixed effects. 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
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
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 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 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 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. The employee-year level observations were kept based on year 2005 employment. 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*** (0.0041)	0.1059*** (0.0032)	-0.0144*** (0.0052)
Female	0.1144*** (0.0046)	0.1115*** (0.0037)	0.0029 (0.0059)
DD/DD/DDD	-0.0229*** (0.0053)	-0.0056 (0.0044)	-0.0173** (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. Person controls include employee 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*** (0.0039)	0.0028 (0.0045)	0.0028 (0.0044)	-0.0142*** (0.0035)	0.0039 (0.0042)	0.0040 (0.0042)
Male × Post			-0.0022 (0.0034)			-0.0030 (0.0033)
Treated × Post × Male			-0.0195*** (0.0052)			-0.0185*** (0.0050)
log(Sales)				0.0217*** (0.0028)	0.0195*** (0.0035)	0.0210*** (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 \times Year ₂₀₀₄	-0.0001 (0.0035)	-0.0076 (0.0051)	-0.0077 (0.0051)
Treated \times Year ₂₀₀₅	-0.0054 (0.0040)	-0.0059 (0.0057)	-0.0060 (0.0057)
Treated \times Year ₂₀₀₆	-0.0140*** (0.0047)	-0.0033 (0.0062)	-0.0033 (0.0062)
Treated \times Year ₂₀₀₇	-0.0193*** (0.0053)	0.0016 (0.0066)	0.0017 (0.0066)
Treated \times Year ₂₀₀₈	-0.0185*** (0.0059)	-0.0004 (0.0072)	-0.0004 (0.0071)
Male \times Treated \times Year ₂₀₀₄			0.0075 (0.0060)
Male \times Treated \times Year ₂₀₀₅			0.0005 (0.0066)
Male \times Treated \times Year ₂₀₀₆			-0.0110 (0.0072)
Male \times Treated \times Year ₂₀₀₇			-0.0213*** (0.0078)
Male \times Treated \times Year ₂₀₀₈			-0.0185** (0.0084)
log(Sales)	0.0218*** (0.0028)	0.0196*** (0.0035)	0.0210*** (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 1-3, the placebo treatment group includes firms with 20-35 employees prior to the law and the placebo control group includes firms with 5-19 in the pre-treatment years 2003-2005. 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
Treated _p × Post	0.0039 (0.0037)	0.0017 (0.0040)	0.0017 (0.0040)	-0.0009 (0.0045)	-0.0018 (0.0056)	-0.0017 (0.0055)	0.0019 (0.0072)	0.0011 (0.0067)	0.0009 (0.0066)
Male × Post			-0.0062 (0.0040)			-0.0223*** (0.0039)			-0.0198*** (0.0051)
Treated _p × Post × Male			0.0022 (0.0051)			0.0011 (0.0063)			0.0007 (0.0089)
log(Sales)	0.0210*** (0.0026)	0.0215*** (0.0030)	0.0212*** (0.0022)	0.0204*** (0.0035)	0.0149*** (0.0038)	0.0182*** (0.0031)	0.0405** (0.0174)	0.0151*** (0.0048)	0.0291*** (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 \times Post	-0.0108 (0.0081)	-0.0017 (0.0132)	-0.0008 (0.0130)	-0.0208*** (0.0056)	0.0055 (0.0071)	0.0057 (0.0070)	-0.0106** (0.0046)	0.0029 (0.0054)	0.0028 (0.0054)
Male \times Post			-0.0190* (0.0104)			0.0104* (0.0056)			-0.0063 (0.0044)
Treated \times Post \times Male			-0.0101 (0.0145)			-0.0268*** (0.0083)			-0.0137** (0.0066)
log(Sales)	0.0228*** (0.0054)	0.0190** (0.0092)	0.0220*** (0.0055)	0.0220*** (0.0040)	0.0178*** (0.0059)	0.0206*** (0.0037)	0.0209*** (0.0044)	0.0210*** (0.0043)	0.0209*** (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 \times Post	0.0091 (0.0248)	0.0088 (0.0251)	0.0423** (0.0216)	0.0441** (0.0217)	0.0257 (0.0192)	0.0248 (0.0192)
log(Sales)		0.0018 (0.0160)		-0.0079 (0.0145)		0.0201 (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 \times Post	0.0216 (0.0242)	0.0175 (0.0243)	0.0077 (0.0208)	0.0078 (0.0208)	-0.0138 (0.0185)	-0.0130 (0.0185)
log(Sales)		0.0149 (0.0149)		-0.0041 (0.0148)		0.0151 (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 \times Post	0.0067 (0.0047)	-0.0019 (0.0042)	-0.0021 (0.0042)	0.0019 (0.0042)	0.0116** (0.0050)	0.0115** (0.0050)
Male \times Post			0.0011 (0.0034)			0.0010 (0.0032)
Treated \times Post \times Male			0.0087 (0.0060)			-0.0097* (0.0052)
log(Sales)	-0.0027 (0.0024)	-0.0023 (0.0023)	-0.0025 (0.0019)	-0.0018 (0.0034)	-0.0020 (0.0039)	-0.0019 (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. 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 \times 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
Treated×Post	-0.0184*** (0.0050)	-0.0059 (0.0058)	-0.0057 (0.0057)
Post×Female Child	0.0071 (0.0054)	-0.0025 (0.0058)	-0.0026 (0.0057)
Treated×Post×Female Child	-0.0085 (0.0080)	0.0158* (0.0088)	0.0157* (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*** (0.0030)	0.0197*** (0.0035)	0.0206*** (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: 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*** (0.0037)	0.0055 (0.0043)	0.0056 (0.0043)
Post×Ind. Gender Gap	-0.0017 (0.0040)	-0.0012 (0.0046)	-0.0016 (0.0045)
Treated×Post×Ind. Gender Gap	-0.0032 (0.0059)	0.0125** (0.0060)	0.0126** (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*** (0.0028)	0.0193*** (0.0035)	0.0208*** (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 A.

Table A1: Definitions of Variables

Variable	Definition
<i>Firm-level variables</i>	
Treated firms	An indicator variable that takes the value 1 for firms that employ 35-50 employees prior to the introduction of the law and 0 for firms that employ 20-34 workers.
Sales	Measured in real USD. Source is KOB.
Firm age	Firm age is 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 information comes from the administrative-matched employer-employee dataset (IDA)
<i>Employee-level variables</i>	
Male	An indicator variable that takes the value 1 if an individual 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 used 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.

Internet Appendix to
"Do firms respond to gender pay gap disclosure?"

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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 \times Post	-0.0097*** (0.0028)	0.0058* (0.0033)	0.0059* (0.0033)
Male \times Post			-0.0002 (0.0026)
Treated \times Post \times Male			-0.0159*** (0.0039)
Log(Sales)	0.0183*** (0.0023)	0.0144*** (0.0029)	0.0170*** (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: 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 \times Post	-0.0130*** (0.0031)	0.0001 (0.0034)	0.0001 (0.0034)	-0.0116*** (0.0030)	0.0008 (0.0033)	0.0008 (0.0033)
Male \times Post			0.0078*** (0.0026)			0.0074*** (0.0026)
Treated \times Post \times Male			-0.0130*** (0.0039)			-0.0125*** (0.0039)
log(Sales)				0.0131*** (0.0026)	0.0139*** (0.0035)	0.0135*** (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 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 \times Post	-0.0146*** (0.0040)	0.0030 (0.0046)	0.0030 (0.0046)	-0.0120*** (0.0037)	0.0041 (0.0044)	0.0043 (0.0044)
Male \times Post			-0.0020 (0.0036)			-0.0028 (0.0034)
Treated \times Post \times Male			-0.0176*** (0.0054)			-0.0166*** (0.0051)
log(Sales)				0.0234*** (0.0030)	0.0206*** (0.0037)	0.0224*** (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 our baseline specification and the *Treated* × *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 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'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. 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 \times Year ₂₀₀₄	-0.0008 (0.0137)	-0.0039 (0.0108)	-0.0023 (0.0041)	-0.0014 (0.0042)	0.0206 (0.0293)	0.0210 (0.0291)	-0.0725 (3.661)	-0.433 (3.669)
Treated \times Year ₂₀₀₅	0.0005 (0.0159)	-0.0102 (0.0135)	-0.0076 (0.0061)	-0.0058 (0.0060)	0.0314 (0.0323)	0.0298 (0.0324)	-2.886 (4.504)	-3.494 (4.471)
Treated \times Year ₂₀₀₆	-0.0045 (0.0176)	-0.0113 (0.0152)	-0.0193*** (0.0069)	-0.0181*** (0.0067)	0.0441 (0.0341)	0.0414 (0.0339)	6.197 (5.201)	5.453 (5.200)
Treated \times Year ₂₀₀₇	-0.0395** (0.0183)	-0.0419** (0.0168)	-0.0379*** (0.0081)	-0.0376*** (0.0076)	0.0079 (0.0332)	0.0064 (0.0331)	4.477 (5.500)	4.031 (5.470)
Treated \times Year ₂₀₀₈	-0.0327* (0.0192)	-0.0364** (0.0183)	-0.0386*** (0.0091)	-0.0371*** (0.0085)	-0.0071 (0.0364)	-0.0088 (0.0364)	4.837 (6.202)	4.735 (6.071)
log(Sales)		0.4050*** (0.0224)		-0.1040*** (0.0113)		0.1010*** (0.0187)		26.27*** (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: Fraction of Female Managers

This table is similar to Table 10, except focusing on the proportion of female managers. *Female Managers*, is a dummy that takes the value 1 for firms with above median fraction of women in the top management team, and 0 otherwise. ***, **, 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*** (0.0043)	0.0041 (0.0055)	0.0044 (0.0054)
Post×Female Managers	0.0116** (0.0047)	-0.0025 (0.0055)	-0.0028 (0.0054)
Treated×Post×Female Managers	-0.0052 (0.0074)	-0.0008 (0.0086)	-0.0010 (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)
Log(Sales)	0.0220*** (0.0028)	0.0195*** (0.0035)	0.0211*** (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: Top 5 Earners Female First Child Ratios

This table repeats Table 10, except focusing on managers' first child to construct *Female Child* 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
Treated×Post	-0.0221*** (0.0051)	-0.0084 (0.0059)	-0.0080 (0.0059)
Post×Female Child Ratio	0.0011 (0.0054)	-0.0047 (0.0057)	-0.0047 (0.0057)
Treated×Post×Female Child Ratio	-0.0001 (0.0079)	0.0203** (0.0087)	0.0199** (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*** (0.0030)	0.0197*** (0.0035)	0.0206*** (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