Do firms respond to gender pay gap disclosure?*

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Abstract

We examine whether pay transparency closes the gender pay gap in firms 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 with better governance, whose managers are more likely to have preferences similar to those of women, 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 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 inspire 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] The opponents of pay transparency argue that disclosing gender pay comes as a challenge to firms as it lacks practical utility, it increases administrative burden and it violates privacy and confidentiality.[4]

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[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. In Iceland, firms with more than 25 employees must obtain a gender wage equality certificate that documents that men and women receive the same wage for the same work. An executive order signed by the U.S. government in 2016 required large companies to report salary data broken down by gender, starting in 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

The effect of transparency on gender pay disparities is a priori ambiguous. It is theoretically unclear whether transparency will incentivize firms to respond by adjusting their compensation policies and, if they do so, by which margins. At the same time, an empirical investigation requires addressing two key challenges: finding variation in transparency at the firm level as well as data on employee wages. To address these difficulties, we use data at the employee-firm level from the Danish Statistics matched employee-employer administrative dataset and exploit a 2006 legislation change 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.

We employ a difference-in-difference 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, that is statistically significant for both treated and control firms. 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 results in lower gender gap after the regulation: the gender gap for treated firms is reduced by 1.4 percentage points relative to the 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 (?), 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% 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% 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.

In interpreting the magnitude of our results, note that we include in the treatment group all the firms with more than 35 employees and do not restrict this group to the firms that actually complied. In order to fully comply, firms needed to have more than 35 employees and at least ten men and ten women within a given occupation. However, survey evidence suggests that firms were not familiar with the second criterion or they were confused about what it meant. Effectively, we perform an intention to treat analysis. Therefore, the differences we observe between treatment and control groups can be attributed to direct effect of compliance of the subset of firms that did so and perhaps to some anticipatory measures that non-compliers took.

To observe to what extent the average effect we estimate is the direct effect of compliance or it captures anticipatory behavior, we collect data on the treated firms that reported wage statistics by gender in 2007, the first year firms report data. We observe that both groups of treated firms, those that we know reported wage statistics in 2007 and those that likely did not, increase male wages at a lower rate as compared to controls. However, the 5% reduction in male wages for those firms that immediately complied (as compared to 1.6% for the rest) suggests that disclosure accelerates wage adjustments towards closing the gender pay gap.

We argue that firm level characteristics, managerial preferences and industry conditions are non-mutually exclusive factors that can help explain the way firms adjust to demand for increased transparency following the regulation. We start from the fact that corporate governance is a key determinant in firms' ability to swiftly adapt to changing conditions and hypothesize that better governed firms are more likely to demonstrate a greater response to the regulation. In support for this channel, we find that treated firms with more independent directors on their board tend to pay female employees relatively more, as compared to the group of controls.

Second, consistent with the idea that managerial styles affect corporate policies (?), we argue that managerial preferences may affect how managers respond to the demand for more pay equity in the workplace. We use the finding in the literature that men parenting daughters are more likely to adopt preferences more similar to those of females because they have experienced a higher degree of "female socialization" (?). 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.

Third, firms' responses may depend on the pre-law gender pay inequality in the industry. Our intuition is that it might not be optimal for a firm alone to fix pay disparities as relatively lowering wages can hurt employee morale (?). The law allows firms to coordinate in changing their compensation policies, perturbing an equilibrium outcome where female employees are under-compensated relative to their male peers. Consistent with this intuition, we see a stronger adjustment if within occupation inequality at the industry is higher prior to the law introduction.

Besides documenting that pay transparency against gender pay discrimination is effective in changing compensation within firms, we provide evidence for some unintended consequences of the law on 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 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 (?). 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. A null result on profitability renders no support for critics of the law who argue that disclosing pay gaps may be particularly costly to implement and can thus hurt firm profits.

The paper contributes to a growing literature studying how wage disparities within the firm may affect employee or firm outcomes. For example, **?** 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. **?** uses a sample of California government workers and **?** 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 or they derive conclusions from the public sector. In addition, our study is the first study with a specific focus on gender disparities—an issue of debate.

Our paper contributes to a growing literature on gender and organizations that point to biases facing women in the professional workforce. **?** show that female advisers face harsher outcomes following misconduct, but this effect is mitigated for firms with more female executives. **?** show that gender barriers tend to discourage women from working in finance. **?** show that female division managers are allocated less capital, especially in firms where CEOs grew up in male-dominated families. **?** 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.

The paper also relates to the vast literature that studies the sources of gender pay disparities.[5] 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.

II The Law

On June 9th 2016, Denmark adopted Act no. 562 of 9 June 2006 which created requirement for firms to report gender based dis-aggregated statistics. The goal of the law was "to promote visibility and information about wage differentials." The law stated that an employer with a minimum of 35 employees and at least 10 employees of each gender within a 6-digit DISCO code (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.[6] The statistics must be made available to the employees through the employee representatives. The new provisions came into force on 1 January 2007.

In order to facilitate the reporting from companies, if the company so wishes, the Statistical Bureau would process the data and deliver a gender-specific statistic for the firm.[7] The costs of the Statistical Bureau's processing of the data are paid by the Ministry of Employment. Thus the firm can obtain the gender-specific statistics that it has to disclose free of charge. The law also offers an alternative choice to employers by permitting to replace gender based wage statistics with an internal report on equal pay. This report is to contain a description of the conditions that are important for

[5] See, for example, **?** and **?** for a review of the literature.

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

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

determining the wage and establish an action plan for equal pay for work of equal value to be implemented within a three-year period.

II.1 Timing and implementation of the Law

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

On December 7, 2005, the Minister for Employment submitted a proposal to Parliament to amend the Equal Pay Act. The proposal was adopted by Act no. 562 of 9 June 2006, thus amending the Equal Pay Act. As we mention above the proposal stated that an employer with a minimum of 35 employees and at least 10 employees of each gender within a 6-digit DISCO code shall each year prepare gender-segregated wage statistics. The proposal took most observers by surprise and it was approved over a short time. It was generally viewed as an attempt of the government to get a better standing among female voters.

From January 1, 2007, all firms that satisfied the criteria of the law should either provide the gender based wage statistic or prepare a report on gender wage to be shared with the employees and the employee representatives. The statistics were not made available online or to the general public.

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 include in the treatment group all the firms with more than 35 employees and do not restrict this group to the firms that actually complied. Effectively, we perform an intention to treat analysis. Therefore, the differences we observe between treatment and control groups can be attributed to direct effect of compliance of the subset of firms that did so and perhaps to some anticipatory measures that non-compliers in the treatment group took.

The reason we design our empirical strategy around the 35 threshold and do not take into account the other criterion mentioned in the legislation that firms should have at least ten male and ten female employees in one six-digit DISCO code, is that firms that did not satisfy the second criterion still complied with the law. In interviews with Statistics Denmark, the organization in charge of collecting wage data, we have been told that "firms did not have DISCO code information or were confused about it". Indeed, according to DA, the main employer organization in Denmark, all compliers met the 35 employee threshold criterion but 35% did not meet the second criterion. In addition, this is consistent with how the law was interpreted more widely. The description of the law in publications by law firms, the EU, or even academics, in which only the requirement of the threshold of 35 employees is mentioned.[8] Given these facts, constructing the treatment and control

[8]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."

groups using both criteria would include a large number of compliers in the control group. Using the 35 employee criterion alone and using an intent to treat analysis is free of these problems.

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:

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

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. The coefficient of interest δ captures the differential effect of the law on wages of male and female individuals at treated firms, as compared to the group of controls. 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:

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).

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

where i, and t index firms and years; post takes a value of 1 for 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. 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 also exclude individuals who move to a different firm in that given year. 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.

V Results

V.1 Wage Pay Gap

We begin by examining whether demand for greater transparency following the law has an effect on the gender pay gap. In Table 2, we estimate the male wage premium around the law reform for the treated and control group separately. In Panel A, we start from a simple specification, following **?**, where we only control for year fixed effects to absorb aggregate macroeconomic shocks. We observe that there is a common trend towards a lower male wage premium for all firms, except the reduction for the treated group is higher by 1.6 percentage points relative to the control group.

In Panel B, we add controls for time-varying individual characteristics (age, experience, education), following **?**, as well as industry and occupation fixed effects as inter-industry pay differentials or selection into different occupations are known to be important determinants of gender pay gaps. We continue to observe a similar pattern. The male wage premium for the treated group is 20.2% prior to the regulation and it falls to 18.9% after the law, a 2.4 percentage point reduction. For the control group, the male wage premium falls from 17.9% to 17.2%. Thus the gender gap is reduced by 1.4 percentage points relative to the control group, or a 7% reduction relative to the pre-disclosure gender wage gap. The effect is also statistically significant at the 5% level. We find that the estimated difference-in-difference effect strengthens in both magnitudes and statistical significance, after controlling for time-invariant individual characteristics by including person fixed effects (Panel C), or firm fixed effects in order to examine the within-firm change in gender pay gap around the regulation (Panel D).

V.2 Wages

How did firms adjust their compensation policies to close the gender pay gap? To address this question, Table 3 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 are demeaned at the individual level to account for differences in individual characteristics in the two employee groups. 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% 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 4 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 3. The effect is statistically significant at the 1% level and economically important. Treated male wages are reduced by 1.67% 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% 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. **?**; **?**). 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 1, 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.

As explained in Section III, our analysis is designed to consider the effect for those firms that complied and those that anticipate to comply around the 35 employee threshold. To assess to what extent the estimated magnitudes capture the direct effect of disclosure versus anticipatory effects, we collect data on the treated firms of our sample that we know reported wages statistics in 2007, the first year firms would report statistics on pay by gender. DA has this list as it was tasked by the Danish government to help its members comply with the law. However, firms did not have to ask DA to help them prepare the statistics and as such firms that complied could be ommitted from this list.

We repeat the analysis in Table 4, except we separate treated firms into those included in the Danish Employer Association (DA) and Statistic Denmark (DST) list that reported statistics and those that are not part of the list. Table 5 reports the results. Similar to our baseline analysis, we observe that for both group of firms male wage growth for treated firms is relatively reduced, while there is no effect on female wages in either case. The effect, however, for firms that we certainly know disclosed wage statistics is economically larger supporting our hypothesis that pay disclosure is an effective way of mitigating gender pay gaps. The larger magnitudes on compliers also suggest that the intention to treat analysis, if anything, results in downward biased 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. 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 6 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% and 1.9%, 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 7, 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 4, where we provide a triple difference estimate comparing the effect of the law between male and female employees in treated versus control firms. Table 2, in the Internet Appendix, repeats estimates in Tables 4, 5, 6 and 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 4 using instead employee hourly wages. In Internet Appendix 3A, 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 **?**, 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 Table 3B 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 Table **??**.

V.4 Mechanisms

We next explore potential channels explaining firm's response to the increased demand for transparency following the passage of the law. We propose three non-mutually exclusive mechanisms: 1) firm ability to adapt; 2) managerial preferences; and 3) pre-law industry gender pay differentials.

Our first mechanism is motivated by the fact that the law mandates transparency on gender pay gaps but does not mandate firms to change the way they compensate their employees. We argue that firms are more likely to respond if they are inept in adjusting to changing conditions. We start from the fact that better governed firms are better able to adapt to new challenges and as such we expect them to respond more to the legislation. We proxy for good governance using the share of board members on the board that are independent. As such, *Ind. Board* is an indicator variable that takes the value of 1 if the ratio of independent directors is above the sample median, and 0 otherwise. In Table **??**, we augment our baseline specifications by interacting $Treated \times Post$ with *Ind. Board* and find that better governed firms increase female compensation by 1.5% more, as compared to controls. This results in closing the gender pay gap by 1.3% more for treated firms with better corporate governance, although this estimate is not statistically significant.

Second, 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 preferences more similar to those of females (Warner, 1991; Warner and Steel, 1999; Oswald and Powdthavee, 2010; Washington, 2008; Glynn and Sen, 2014; Cronqvist and Yu, 2017; Dahl, Dezső, and Ross, 2012).[9] As such, managers with daughters should not only be more likely to follow fairer pay practices towards women,[10] 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.[11] 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 Dummy* 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 Dummy*. Table **??** 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 **??**, 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 Dummy* measure based on the first-born child of the firm's top managers, which is arguably a more exogenous child gender measure (Bennedsen et al, 2007; Cronqvist and Yu, 2017). The estimated coefficients are similar to those reported in Table **??**.

[9]Examples that support the female socialization hypothesis abound in the social sciences literature. Washington (2008) and Glynn and Sen (2014) 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.

[10]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.

[11]20% of top managers in our sample are women.

Third, we consider the role of the industry pre-existing gender pay inequality. Even if firms or managers may be willing to fix gender pay disparities, they may be limited in their ability to do so unilaterally if all other firms in the industry pay unequal wages. For example, lowering pay to male employees below that of their peers in other firms may hurt their morale (Akerlof and Yellen, 1990). Such deviations from the "norm" may be facilitated, however, by the law as it allows firms to coordinate in changing the way they compensate employees towards fixing gender pay disparities.

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 **??**, 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 multiple mechanisms at play that can plausibly explain the observed response by firms to the demand for higher transparency on gender pay. A caveat of this analysis, however, is that we cannot assess the importance of each channel due to differences in the power of our empirical proxies.

VI Pay by Hierarchy, Hiring and Promotions

Does increased demand for transparency affect all employees in the firm, or do we observe asymmetric responses by firms depending on employee hierarchy? In Table 8, 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.[12] 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 increasing demand 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 8. Thus, *Joining rate* is the total number of female employees joining the firm-hierarchy in a given year t normalized by the total

[12]In unreported results we replicate this analysis defining firm hierarchies based on worker's occupations following **?** and **?**. 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.

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 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 9. 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% and 2.5%, 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 9, 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 10 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% 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 productivity and profits

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 **??**, 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 **??** 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 **??**. 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.

VIII Conclusion

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

Investigating empirically the effect of pay transparency rules as a measure to reduce pay discrimination within firms is challenging as it requires finding variation in transparency rules but also having detailed information of employee wages. We overcome these hurdles by exploiting a 2006 regulation in Denmark that 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 demand for transparency.

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 **?** and **?** 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.
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		
	Observations	Mean	S.D.	P50	Mean	S.D.	P50	Mean	S.D.	P50
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Wage (thous. $)	66,195	53.80	23.71	50.52	54.59	23.82	50.99	53.27	23.59	50.17
Hourly Wage ($)	66,188	33.92	15.09	31.17	34.41	15.38	31.46	33.54	14.82	30.95
Bonus (thous. $)	65,958	1.18	3.04	0	1.15	3.11	0	1.21	3.00	0.028
Age (years)	67,574	39.79	10.77	38.50	39.90	10.63	39	39.70	10.85	38.50
Male (%)	67,749	0.64	0.48	1	0.64	0.48	1	0.64	0.48	1
College degree (%)	66,158	0.25	0.43	0	0.25	0.44	0	0.24	0.43	0
Work Experience (years)	67,824	17.23	10.36	16.19	17.34	10.29	16.40	17.14	10.40	16.02

Table 1: [Continued] Summary Statistics

Panel B - Firm-Level Characteristics

	All				Treated			Control		
	Observations	Mean	S.D.	P50	Mean	S.D.	P50	Mean	S.D.	P50
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Assets (mil. $)	3,956	6.44	23.74	3.22	7.20	13.23	4.64	6.07	27.46	2.72
Sales (mil. $)	3,956	9.03	9.64	5.81	11.68	10.74	7.97	7.73	8.77	4.80
Sales/Employee (mil. $)	3,956	0.28	0.30	0.18	0.27	0.25	0.18	0.28	0.33	0.17
Employment	4,005	31.12	8.49	29	41.67	4.37	41.33	25.97	4.12	25.50
Female Share (%)	3,998	0.30	0.21	0.25	0.29	0.21	0.24	0.30	0.21	0.25
Profits (mil. $)	3,957	0.23	0.68	0.13	0.27	0.74	0.19	0.21	0.64	0.11
Profits/Employee (mil. $)	3,957	0.007	0.018	0.004	0.006	0.017	0.005	0.007	0.019	0.004
Wages (mil. $)	3,950	1.70	0.70	1.56	2.26	0.69	2.20	1.43	0.52	1.34
Wage/Employee (mil. $)	3,950	0.051	0.017	0.049	0.051	0.016	0.050	0.051	0.017	0.049
Pension & Soc. Sec. (mil. $)	3,950	0.135	0.082	0.118	0.179	0.091	0.168	0.114	0.068	0.101
Pension & Soc. Sec./Employee (mil. $)	3,950	0.004	0.002	0.004	0.004	0.002	0.004	0.004	0.002	0.004

Table 2: Change in the Male Wage Premium Around the Disclosure Law: Difference-in-Differences (DD)

The table presents changes in the male wage premium around the disclosure law. The male wage premium is the estimate of the coefficient on a male dummy in a log wage regression. Panel A includes year fixed effects and controls for firm sales. Panel B includes year fixed effects, industry and occupation fixed effects and controls for education (college degree or not), age (quartile of age), experience (quartile of experience) and firm sales. Panel C additionally includes person fixed effects. Panel D includes year, person, occupation and firm fixed effects, as well as controls for education, age, experience and sales. 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). Pre-law spans years 2003-2005 and post-law years 2006-2008. ***, and ** correspond to statistical significance at the 1% and 5% levels, respectively. Standard errors are clustered at the firm level.

Panel A - No Human Capital Controls

	Pre-law	Post-law	Difference
Male Wage Premium	0.253***	0.224***	-0.031***
Treated	(0.0091)	(0.0094)	(0.0070)
Male Wage Premium	0.222***	0.207***	-0.015***
Control	(0.0066)	(0.0065)	(0.0055)
Difference-in-Differences			-0.016*
			(0.0089)

Panel B - Human Capital Controls

	Pre-law	Post-law	Difference
Male Wage Premium	0.202***	0.189***	-0.024***
Treated	(0.0063)	(0.0065)	(0.0049)
Male Wage Premium	0.179***	0.172***	-0.009**
Control	(0.0054)	(0.0051)	(0.0043)
Difference-in-Differences			-0.014**
			(0.0066)

Table 2: [Continued] Change in the Male Wage Premium Around the Disclosure Law: Difference-in-Differences (DD)

Panel C - Person FE and Human Capital Controls			
	Pre-law	Post-law	Difference
Male Wage Premium	.	.	-0.021***
Treated	(omitted)	(omitted)	(0.0043)
Male Wage Premium	.	.	-0.002
Control	(omitted)	(omitted)	(0.0038)
Difference-in-Differences			-0.018***
			(0.0057)

Panel D - Person FE, Firm FE and Human Capital Controls			
	Pre-law	Post-law	Difference
Male Wage Premium	.	.	-0.021***
Treated	(omitted)	(omitted)	(0.0043)
Male Wage Premium	.	.	-0.002
Control	(omitted)	(omitted)	(0.0038)
Difference-in-Differences			-0.018***
			(0.0057)

Table 3: Change in Compensation Policy Around the Disclosure Law: Difference-in-Difference-in-Differences (DDD)

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 and demeaned at the individual level. *** 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 4: Gender Pay Gap Disclosure and Employee Wages

This table reports the effects of gender pay gap disclosure on employee wages. Estimated coefficients are from least squares regressions. The dependent variable is employee annual wage. 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. Post = 0 for years 2003-2005, and Post = 1 for years 2006-2008. The above regressions exclude CEOs and board members and firms in industries unaffected by the policy. Education is defined as a binary variable that is one if the employee is a college graduate. Controls for age (age quartiles) and experience (experience quartiles) are also included. ***, **, 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***	0.00276	0.00278	-0.0142***	0.00386	0.00401
	(0.00386)	(0.00446)	(0.00444)	(0.00351)	(0.00422)	(0.00418)
Male × Post			-0.00219			-0.00300
			(0.00344)			(0.00329)
Treated × Post × Male			-0.0195***			-0.0185***
			(0.00524)			(0.00498)
log(Sales)				0.0217***	0.0195***	0.0210***
				(0.00281)	(0.00353)	(0.00252)
Observations	145852	79532	225384	145262	79027	224289
R^2	0.868	0.827	0.866	0.871	0.828	0.868
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
No. Firms	3785	3659	4012	3778	3651	4005

Table 5: Gender Gap Revelation and DST Compilers

The sample restrictions and variable definitions follow Table 4. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Male	Female	All	Male	Female	All
DST Treated × Post	-0.0504**	-0.000755	-0.00182	-0.0448**	-0.00143	-0.00225
	(0.0196)	(0.0173)	(0.0172)	(0.0190)	(0.0150)	(0.0146)
Non-DST Treated × Post	-0.0164***	0.00283	0.00288	-0.0139***	0.00397	0.00414
	(0.00387)	(0.00450)	(0.00448)	(0.00352)	(0.00426)	(0.00422)
Male × Post			-0.00219			-0.00300
			(0.00344)			(0.00329)
DST Treated × Male × Post			-0.0477*			-0.0425*
			(0.0264)			(0.0241)
Non-DST Treated × Male × Post			-0.0193***			-0.0183***
			(0.00527)			(0.00501)
log(Sales)				0.0217***	0.0196***	0.0210***
				(0.00281)	(0.00353)	(0.00253)
Observations	145852	79532	225384	145262	79027	224289
R^2	0.868	0.827	0.866	0.871	0.828	0.868
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

Table 6: Gender Gap Revelation and Subsequent Employee Wages, Treatment by Year

This table reports the $Treated \times Year$ effects for years 2004-2008 on employee wage. The sample restrictions and variable definitions follow Table 4. $Male \times Year$ terms were reported but then omitted here for brevity. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Male	Female	All
Treated × Year_{2004}	-0.000141	-0.00764	-0.00769
	(0.00353)	(0.00511)	(0.00511)
Treated × Year_{2005}	-0.00540	-0.00590	-0.00604
	(0.00400)	(0.00568)	(0.00566)
Treated × Year_{2006}	-0.0140***	-0.00329	-0.00328
	(0.00468)	(0.00622)	(0.00619)
Treated × Year_{2007}	-0.0193***	0.00160	0.00166
	(0.00529)	(0.00664)	(0.00660)
Treated × Year_{2008}	-0.0185***	-0.000438	-0.000385
	(0.00593)	(0.00715)	(0.00709)
Male × Treated × Year_{2004}			0.00748
			(0.00602)
Male × Treated × Year_{2005}			0.000533
			(0.00661)
Male × Treated × Year_{2006}			-0.0110
			(0.00719)
Male × Treated × Year_{2007}			-0.0213***
			(0.00775)
Male × Treated × Year_{2008}			-0.0185**
			(0.00838)
log(Sales)	0.0218***	0.0196***	0.0210***
	(0.00282)	(0.00354)	(0.00253)
Observations	145262	79027	224289
R^2	0.871	0.828	0.868
Person-Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Person Controls	Yes	Yes	Yes

Table 7: Gender Gap Revelation and Subsequent Employee Wages, Placebo Treatment

This table reports the effects of placebo treatment on employee wage. For columns 1-2, the placebo treatment group includes firms with average employment of 20-35 in the pre-treatment years 2003-2005, and the placebo control group includes firms with average employment of 5-19 in the pre-treatment period. For columns 3-4, the ranges are 50-65 and 35-49, respectively. For columns 5-6, the placebo treatment group includes the range 65-80 and the control group includes the range 50-64. The sample restrictions and variable definitions follow Table 4. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	20 Cutoff			50 Cutoff			65 Cutoff		
	Male	Female	All	Male	Female	All	Male	Female	All
Treated$_p$ × Post	0.00391	0.00172	0.00171	-0.000890	-0.00176	-0.00173	0.00193	0.00105	0.000954
	(0.00370)	(0.00396)	(0.00395)	(0.00447)	(0.00556)	(0.00549)	(0.00719)	(0.00671)	(0.00661)
Male × Post			-0.00618			-0.0223***			-0.0198***
			(0.00397)			(0.00391)			(0.00506)
Treated$_p$ × Post × Male			0.00220			0.00105			0.000736
			(0.00509)			(0.00626)			(0.00888)
log(Sales)	0.0210***	0.0215***	0.0212***	0.0204***	0.0149***	0.0182***	0.0405**	0.0151***	0.0291***
	(0.00262)	(0.00296)	(0.00221)	(0.00354)	(0.00381)	(0.00313)	(0.0174)	(0.00481)	(0.0112)
Observations	148573	88160	236733	104098	56899	160997	72578	41786	114364
R^2	0.865	0.827	0.863	0.875	0.822	0.871	0.862	0.815	0.859
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

Table 8: Gender Gap Revelation and Subsequent 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 restrictions and variable definitions follow Table 4. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	High-level			Intermediate-level			Lower-level		
	Male	Female	All	Male	Female	All	Male	Female	All
Treated × Post	-0.0108	-0.00174	-0.000834	-0.0208***	0.00554	0.00574	-0.0106**	0.00290	0.00275
	(0.00805)	(0.0132)	(0.0130)	(0.00556)	(0.00709)	(0.00700)	(0.00460)	(0.00541)	(0.00537)
Male × Post			-0.0190*			0.0104*			-0.00628
			(0.0104)			(0.00559)			(0.00435)
Treated × Post × Male			-0.0101			-0.0268***			-0.0137**
			(0.0145)			(0.00831)			(0.00661)
log(Sales)	0.0228***	0.0190**	0.0220***	0.0220***	0.0178***	0.0206***	0.0209***	0.0210***	0.0209***
	(0.00538)	(0.00919)	(0.00551)	(0.00402)	(0.00585)	(0.00367)	(0.00436)	(0.00425)	(0.00335)
Observations	33647	9146	42793	45901	27056	72957	61136	39663	100799
R^2	0.829	0.805	0.829	0.849	0.799	0.849	0.856	0.810	0.847
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

Table 9: Gender Gap Revelation and Female Employee Joining and Leaving

This table reports the effects of gender-separated wage revelation on the firm's joining rate and leaving rate of employees, defined as $\frac{\text{\# female employees joining (leaving) in year t}}{\text{\# total employees joining (leaving) in year t}}$. The sample restrictions and variable definitions follow Table 11. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A - Joining Rate

	High-level (1)	(2)	Intermediate-level (3)	(4)	Low-level (5)	(6)
Treated × Post	0.00913	0.00880	0.0423**	0.0441**	0.0257	0.0248
	(0.0248)	(0.0251)	(0.0216)	(0.0217)	(0.0192)	(0.0192)
log(Sales)		0.00178		-0.00794		0.0201
		(0.0160)		(0.0145)		(0.0137)
Observations	3221	3208	5391	5373	7046	7035
R^2	0.500	0.500	0.533	0.533	0.555	0.555
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B - Leaving Rate

	High-level (1)	(2)	Intermediate-level (3)	(4)	Low-level (5)	(6)
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)
Observations	3698	3673	5753	5735	7840	7825
R^2	0.465	0.467	0.516	0.517	0.564	0.564
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 10: Gender Gap Revelation and Promotion

This table reports the effects of gender-separated wage revelation on employee promotion likelihood, defined as a movement to a higher hierarchy level. 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 from the low hierarchy. The sample restrictions and variable definitions follow Table 4. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Intermediate-level			Low-level		
	Male	Female	All	Male	Female	All
Treated × Post	0.00674	-0.00197	-0.00205	0.00189	0.0116**	0.0115**
	(0.00471)	(0.00423)	(0.00424)	(0.00419)	(0.00504)	(0.00504)
Male × Post			0.00110			0.00101
			(0.00341)			(0.00320)
Treated × Post × Male			0.00873			-0.00974*
			(0.00604)			(0.00523)
log(Sales)	-0.00268	-0.00230	-0.00254	-0.00179	-0.00199	-0.00190
	(0.00244)	(0.00227)	(0.00186)	(0.00343)	(0.00387)	(0.00291)
Observations	35166	19907	55073	52382	33398	85780
R^2	0.429	0.380	0.417	0.522	0.527	0.524
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

Table 11: Gender Gap Revelation and Firm Outcomes

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

	log(Sales/Emp)		log(Wage/Emp)		log(Pension and other/Emp)		Profits/Emp	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated × Post	-0.0250**	-0.0246**	-0.0282***	-0.0281***	-0.00179	-0.00335	6.218	6.107
	(0.0120)	(0.0112)	(0.00560)	(0.00525)	(0.0207)	(0.0207)	(3.787)	(3.731)
log(Sales)		0.405***		-0.104***		0.101***		26.26***
		(0.0224)		(0.0113)		(0.0186)		(5.188)
Observations	22414	22391	22429	22391	22374	22351	21602	21564
R^2	0.845	0.879	0.849	0.863	0.544	0.547	0.621	0.625
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 12: Top 5 Earners More Female Child Ratios

The wages are in logs. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Male	Female	All
Tested×Post	-0.0184***	-0.00587	-0.00569
	(0.00500)	(0.00575)	(0.00571)
Post×Female Child Ratio	0.00706	-0.00247	-0.00258
	(0.00543)	(0.00579)	(0.00573)
Treated×Post×Female Child Ratio	-0.00853	0.0158*	0.0157*
	(0.00795)	(0.00882)	(0.00874)
Post×Male			0.00578
			(0.00461)
Treated×Post×Male			-0.0131*
			(0.00681)
Post×Male×Female Child Ratio			0.00967
			(0.00734)
Treated×Post×Male×Female Child Ratio			-0.0243**
			(0.0109)
Log(Sales)	0.0211***	0.0197***	0.0206***
	(0.00298)	(0.00351)	(0.00258)
Observations	122266	74516	196782
R^2	0.851	0.815	0.848
Person-Frim FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Person Controls	Yes	Yes	Yes

Table 13: Board Independence Interaction

Independent board members are defined as board members that are not from management families or control families and are not employees. The regression used the above-or-below-median board independence rate dummy variable. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Male	Female	All
Treated×Post	-0.0154***	-0.00710	-0.00698
	(0.00548)	(0.00667)	(0.00659)
Post×Indp. Board	-0.00447	-0.00468	-0.00514
	(0.00481)	(0.00582)	(0.00575)
Treated×Post×Indp. Board	0.00201	0.0149*	0.0149*
	(0.00721)	(0.00862)	(0.00851)
Male×Post			-0.00339
			(0.00561)
Treated×Post×Male			-0.00883
			(0.00790)
Post×Male×Indp. Board			0.000674
			(0.00700)
Treated×Post×Male×Indp. Board			-0.0127
			(0.0102)
log(Sales)	0.0216***	0.0187***	0.0206***
	(0.00286)	(0.00352)	(0.00255)
Observations	139356	74696	214052
R^2	0.869	0.826	0.866
Person-Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Person Controls	Yes	Yes	Yes

Table 14: Industry Occupation Level Gender Wage Difference Interaction

Wage Diff is the standardized industry 1-digit-DISCO level gender median log wage difference. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Male	Female	All
Wage Diff	0.0127*	-0.00650	-0.00639
	(0.00659)	(0.00809)	(0.00807)
Treated×Post	-0.0146***	0.00551	0.00562
	(0.00365)	(0.00429)	(0.00425)
Treated×Wage Diff	-0.0244***	-0.0141	-0.0145
	(0.00940)	(0.0147)	(0.0146)
Post×Wage Diff	-0.00173	-0.00123	-0.00158
	(0.00398)	(0.00456)	(0.00452)
Treated×Post×Wage Diff	-0.00319	0.0125**	0.0126**
	(0.00591)	(0.00603)	(0.00600)
Male×Post			-0.00196
			(0.00338)
Male×Wage Diff			0.0194**
			(0.00934)
Treated×Post×Male			-0.0205***
			(0.00510)
Treated×Male×Wage Diff			-0.0100
			(0.0165)
Post×Male×Wage Diff			-0.000328
			(0.00583)
Treated×Post×Male×Wage Diff			-0.0159**
			(0.00792)
log(Sales)	0.0217***	0.0193***	0.0208***
	(0.00284)	(0.00351)	(0.00255)
Observations	138576	77609	216185
R^2	0.871	0.828	0.868
Person-Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Person Controls	Yes	Yes	Yes

Online Appendix to

"Do firms respond to gender pay gap disclosure?"

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

Table 1: Stayers baseline regression

An individual is a stayer if he/she stayed in the same firm at least one year before the law one year after the law, not counting joining/leaving years. The wages are in logs. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Male	Female	All
Treated × Post	-0.00971***	0.00578*	0.00589*
	(0.00279)	(0.00333)	(0.00332)
Male × Post			-0.000204
			(0.00257)
Treated × Post × Male			-0.0159***
			(0.00393)
Log(Sales)	0.0183***	0.0144***	0.0170***
	(0.00229)	(0.00293)	(0.00208)
Observations	94118	49451	143569
R^2	0.933	0.894	0.929
Person-Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Person Controls	No	No	No

Table 2: Selected Results with Firm-Year Fixed Effect

This table reports three columns of selected results with firm-year fixed effect included. The sample restrictions and variable definitions follow Table 4. $Male \times Year$ terms were reported but then omitted here for brevity. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Baseline All	DST compliers All	Treatment by year All
Male × Post	-0.00439	-0.00439	
	(0.00347)	(0.00347)	
Treated × Post × Male	-0.0143***		
	(0.00514)		
DST Treated × Post × Male		-0.0463*	
		(0.0264)	
Non-DST Treated × Post × Male		-0.0137***	
		(0.00517)	
Male × Treated × $Year_{2004}$			0.00482
			(0.00659)
Male × Treated × $Year_{2005}$			-0.00715
			(0.00728)
Male × Treated × $Year_{2006}$			-0.0116
			(0.00770)
Male × Treated × $Year_{2007}$			-0.0202**
			(0.00832)
Male × Treated × $Year_{2008}$			-0.0200**
			(0.00877)
Observations	222529	222529	222529
R^2	0.885	0.885	0.885
Person-Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Firm-Year FE	Yes	Yes	Yes
Person Controls	Yes	Yes	Yes

Table 3A: Gender Gap Revelation and Subsequent Employee Hourly Wage

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

	Male	Female	All	Male	Female	All
Treated × Post	-0.0130***	0.0000786	0.0000954	-0.0116***	0.000783	0.000784
	(0.00313)	(0.00343)	(0.00343)	(0.00303)	(0.00332)	(0.00331)
Male × Post			0.00782***			0.00740***
			(0.00264)			(0.00257)
Treated × Post × Male			-0.0130***			-0.0125***
			(0.00395)			(0.00385)
log(Sales)				0.0131***	0.0139***	0.0135***
				(0.00258)	(0.00350)	(0.00259)
Observations	153062	83895	236957	152460	83372	235832
R^2	0.906	0.884	0.907	0.907	0.886	0.908
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

Table 3B: Gender Gap Revelation and Subsequent Employee Bonus and Wage, All Employees

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

	Male	Female	All	Male	Female	All
Treated × Post	-0.0146***	0.00296	0.00300	-0.0120***	0.00408	0.00425
	(0.00399)	(0.00464)	(0.00463)	(0.00365)	(0.00442)	(0.00437)
Male × Post			-0.00197			-0.00281
			(0.00358)			(0.00342)
Treated × Post × Male			-0.0176***			-0.0166***
			(0.00537)			(0.00512)
log(Sales)				0.0234***	0.0206***	0.0224***
				(0.00296)	(0.00367)	(0.00267)
Observations	144811	79001	223812	144235	78510	222745
R^2	0.866	0.828	0.865	0.869	0.829	0.867
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

Table 4: Gender Gap Revelation and Subsequent Employee Wages, Treatment by Year 2004 - 2010

This table reports the $Treated \times Year$ effects for years 2004-2010 on employee wage. The sample restrictions and variable definitions follow Table 4. $Male \times Year$ terms were reported but then omitted here for brevity. All three columns have person-firm FE, year FE, age control and experience control. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Male	Female	All
Treated × $Year_{2004}$	0.00130	-0.00731	-0.00744
	(0.00363)	(0.00512)	(0.00512)
Treated × $Year_{2005}$	-0.00464	-0.00557	-0.00574
	(0.00403)	(0.00575)	(0.00574)
Treated × $Year_{2006}$	-0.0118**	-0.00401	-0.00433
	(0.00471)	(0.00625)	(0.00622)
Treated × $Year_{2007}$	-0.0161***	-0.000199	-0.0000435
	(0.00528)	(0.00661)	(0.00656)
Treated × $Year_{2008}$	-0.0157**	-0.00146	-0.00115
	(0.00569)	(0.00673)	(0.00668)
Treated × $Year_{2009}$	-0.0173***	-0.00558	-0.00560
	(0.00643)	(0.00719)	(0.00710)
Treated × $Year_{2010}$	-0.0109	-0.00817	-0.00820
	(0.00868)	(0.00774)	(0.00765)
Male × Treated × $Year_{2004}$			0.00867
			(0.00595)
Male × Treated × $Year_{2005}$			0.00100
			(0.00664)
Male × Treated × $Year_{2006}$			-0.00801
			(0.00717)
Male × Treated × $Year_{2007}$			-0.0164**
			(0.00763)
Male × Treated × $Year_{2008}$			-0.0150*
			(0.00794)
Male × Treated × $Year_{2009}$			-0.0121
			(0.00840)
Male × Treated × $Year_{2010}$			-0.00317
			(0.00986)
log(Sales)	0.0232***	0.0187***	0.0216***
	(0.00311)	(0.00333)	(0.00275)
Observations	195384	107215	302599
R^2	0.860	0.820	0.859

Table 5: Top 5 Earners Female First Child Ratios

The wages are in logs. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Male	Female	All
Tested×Post	-0.0221***	-0.00837	-0.00804
	(0.00513)	(0.00594)	(0.00589)
Post×Female Child Ratio	0.00113	-0.00471	-0.00472
	(0.00543)	(0.00572)	(0.00566)
Treated×Post×Female Child Ratio	-0.0000738	0.0203**	0.0199**
	(0.00791)	(0.00874)	(0.00865)
Post×Male			0.00744
			(0.00485)
Treated×Post×Male			-0.0146**
			(0.00707)
Post×Male×Female Child Ratio			0.00586
			(0.00725)
Treated×Post×Male×Female Child Ratio			-0.0197*
			(0.0107)
Log(Sales)	0.0211***	0.0197***	0.0206***
	(0.00298)	(0.00352)	(0.00259)
Observations	122232	74528	196760
R^2	0.851	0.816	0.848
Person-Frim FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Person Controls	Yes	Yes	Yes

Table 6: Pre-treatment Top 5 Earners More Daughter Ratio Wage Regression

More Daughter = 1, if an employee's firm level more daughter ratio among the top 5 earners is in the top 50%. The wages are in logs. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Male	Female	All
More Daughter	0.0107	0.0167**	0.0156**
	(0.00750)	(0.00765)	(0.00778)
Education Level	0.0864***	0.131***	0.106***
	(0.00747)	(0.00783)	(0.00584)
Male			0.139***
			(0.00554)
Male×More Daughter			-0.00363
			(0.00839)
Log(Sales)	0.0665***	0.0542***	0.0617***
	(0.00622)	(0.00508)	(0.00484)
Observations	42166	28210	70414
R^2	0.422	0.340	0.411
Industry Control	Yes	Yes	Yes
Person Controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
DISCO Control	Yes	Yes	Yes