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JEL Classification: N/A

Keywords: Gender pay gap, transparency, Difference-in-Discontinuities

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Do Firms Respond to Gender Pay Gap Transparency?*

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April 2020

Abstract

We examine the effect of pay transparency on gender pay gap and firm outcomes. This paper exploits a 2006 legislation change in Denmark that requires firms to provide gender disaggregated wage statistics. Using detailed employee-employer administrative data and a difference-in-differences and difference-in-discontinuities designs, we find the law reduces the gender pay gap, primarily by slowing the wage growth for male employees. The gender pay gap declines by approximately two percentage points, or a 13% reduction relative to the pre-legislation mean. Despite the reduction of the overall wage bill, the wage-transparency mandate does not affect firm profitability, likely because of the offsetting effect of reduced firm productivity.

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I Introduction

Gender pay disparities characterize labor markets in most developed countries (Goldin, 2014; Blau and Kahn, 2017). When a man earns $100, a woman earns $77 in the US (Goldin, 2014), $78.5 in Germany, $79 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.[1] However, evidence on the effect of transparency on gender pay disparities on employee and firm outcomes is limited. In this paper, we draw insights from a regulation in Denmark that increased transparency by requiring companies to inform employees of average wages by gender and occupation.

The debate about the consequences of disclosing gender wage gaps is ongoing. 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 secrecy about pay contributes significantly to unequal pay for women.[2] Opponents of pay transparency argue disclosing gender pay comes as a challenge to firms because it lacks practical utility, increases administrative burden, and violates employee privacy.[3]

The effect of transparency on the gender pay gap and firm outcomes is ultimately an empirical question. Whether transparency will provide sufficient incentives for firms

[1] In the UK, 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 the median salary for a group of comparable employees in firms with more than 200 employees. An executive order signed by the US government in 2016 required large companies to report salary data broken down by gender starting in 2017, but the succeeding administration overturned the rule.

[2] 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 60% of employees are discouraged or prohibited from sharing wage information, and concludes pay secrecy is an important determinant of the gender gap in earnings (IWPR, 2014).

[3] 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

to adjust their compensation policies is unclear. Moreover, these wage adjustments might have unintended consequences for firm outcomes, such as firm productivity and, eventually, profitability.

Studying this question empirically requires addressing two key challenges: finding exogenous variation in wage transparency at the firm level and obtaining information on wages and employment at the individual level. For the source of exogenous variation, we exploit a 2006 legislation change in Denmark that requires firms with more than 35 employees to report salary data broken down by gender for employee groups large enough for individual's anonymity to be protected. Firms must inform their employees of wage gaps between men and women and explain the design of the statistics and the wage concept used. For data on wages and employment at the individual level, we use administrative records from the Danish Statistics matched employee-employer dataset.

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

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

In terms of the effect of transparency on firm compensation policies, we find that after

the passage of the law, wages of male employees in treated firms grow 1.7 percentage points slower than wages of male employees in control firms. The effect is statistically significant at the 1% level and economically important. By contrast, female wages in treated firms increase by 0.3 percentage points more than for female employees in control firms, although this difference is not statistically significant. Examining the dynamic effects, we find no evidence of pre-treatment trends, whereas we observe a sharp drop in male wages in treated firms in 2006, which increasingly persists until up to year 2008.

Moving to a firm-level analysis, we confirm the reduction of the wage gap. We find the average ratio of male to female wages (gender pay gap) declines by 1.9 percentage points in treated firms relative to control firms, following the law passage, a 13% reduction relative to the pre-treatment mean. We also show the average decline is more pronounced at the bottom and the middle of the wage distribution.

We also find similar results when we employ a difference-in-discontinuities design (diff-in-disc) following (Grembi, Nannicini, and Troiano, 2016). This approach takes the difference between the pre-treatment and post-treatment discontinuity at 35 employees. As such, the diff-in-disc allows us to difference out the effect of any potential pre-existing discontinuity at 35 employees. In addition, by focusing only on the effect at the threshold, this method circumvents concerns with the diff-in-diff method that the control firms might not be a good counterfactual for treated firms. Using this approach, we find that male employee wages grow by 1.1 percentage points less for firms above the threshold, and this difference is significant at the 1% level. Consistent with our diff-in-diff analysis, the decline in male wages starts in 2006 and gradually increases until the end of the sample. By contrast, we find a positive but insignificant change for female employee wages following the policy change.

After documenting the effect of the law on employee wages, we investigate how transparency affects employee reallocation and promotions. We show treated firms hire more female employees than do control firms. This finding is in line with an argument that female employees might be more willing to seek employment in firms where the gender

pay gap is reduced. We also find male employees are less likely to join treated firms, consistent with the fact that wage policies are less attractive for male employees in these firms following the legislation. We do not find that female or male employees are more likely to leave treated firms after the law passage. We also find the law has effects on promotion decisions that favor female employees. We find women are more likely to be promoted to higher-paid positions within the firm, whereas we do not find any significant change in the promotion probability for male employees.

In additional tests, we examine the implications of gender pay transparency on wage bill, firm productivity, and profits. Consistent with our employee-level analysis, we find the average wage per employee is reduced in treated firms relative to control firms. Despite the reduction in the average wages, we do not find any effect on firm profitability, which could be potentially explained by a decline in labor productivity, because previous research has shown that employees who learn they are earning less than their peers become less productive (Akerlof and Yellen, 1990; Breza, Kaur, and Shamdasani, 2018). We find a negative effect on productivity in some of our specifications.

The paper contributes to the literature on the effects of pay transparency. Breza, Kaur, and Shamdasani (2018) use a sample of workers in an Indian manufacturing plant to show information on peers' salaries generates negative feelings, reduces job satisfaction, and negatively affects labor productivity.[4] Cullen and Perez-Truglia (2017) and Cullen and Perez-Truglia (2018) show employees have misperceptions about the salaries of their peers and their superiors and change their behavior when they learn their true salaries. In the context of mandated pay disclosure on the public sector, Card, Mas, Moretti, and Saez (2012) use a sample of government employees in California to show that after government employee salaries are published online, aggregate worker satisfaction drops. These studies mostly focus on job satisfaction and employee productivity and not on wages or firm outcomes, as we do in this paper.

[4]Perez-Truglia (2016) shows how online access of the general public to tax income information in Norway increases relative well-being and life satisfaction for the rich. This finding suggests the disclosure regulation might have a direct effect on employee well-being.

Focusing on how transparency affects wage setting in organizations, Cullen and Pakzad-Hurson (2019) argue that higher transparency results in more equal but lower wages, thereby resulting in higher profits for employers. They test the predictions of their model using data from online markets for low-skill, temporary jobs and an online field experiment. Mas (2017) shows 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. These papers focus on the effect of transparency on the distribution of wages, and their main focus is not on the gender wage gap. In addition, these papers analyze the wages of either temporary employees or public sector employees, and wage setting for regular employment contracts in the private sector is likely different. For example, in the public sector, public pressure and public aversion to high compensation or inequalities might play a larger role than in the private sector.

In a recent paper, Baker, Halberstam, Kroft, Mas, and Messacar (2019) show that disclosing the wages of university faculty in Canada reduced the gender pay gap, driven primarily by universities where faculty are unionized. In contrast in this paper we provide evidence based on private firms.

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

Our paper lastly contributes to a growing literature on gender and organizations that points to biases that women face in the professional workforce. Hospido, Laeven, and Lamo (2019) show gender differences exist in career progression and promotions in central

banking. Egan, Matvos, and Seru (2017) show female advisers face harsher outcomes following misconduct, but this effect is mitigated in firms with more female executives. Adams and Ragunathan (2017) show gender barriers tend to discourage women from working in finance. Duchin, Simutin, and Sosyura (2018) show female division managers are allocated less capital, especially in firms where CEOs grew up in male-dominated families. Tate and Yang (2015) show male leadership cultivates a less female-friendly culture within firms. Our findings suggest regulatory mandates on pay transparency, as a means to overcome biases against women in the workforce, may be effective in closing the gender pay gap.

II The Law

On June 9, 2006, Denmark adopted Act no. 562, which required firms to report gender-based disaggregated statistics. The goal of the law was "to promote visibility and information about wage differentials."[5] The law stated that an employer with a minimum of 35 employees and at least 10 employees of each gender within an occupation classification code (six-digit DISCO code) shall each year prepare gender-segregated wage statistics for the purpose of consulting and informing the employees of wage gaps between men and women in the firm.[6] The statistics had to be made available to the employees through the employee representatives; they did not need to be made available to the general public. The law also offered an alternative choice to employers by permitting them to replace gender-based wage statistics with an internal report on equal pay. This report had to include a description of the conditions that are important for determining wages, and establish an action plan for equal pay to be implemented.

During the campaign for the snap election in February 2005, the government promised

[5]Indeed, in a survey conducted thee years after the law implementation, companies stated that "the law has sharpened their attention about pay and any pay gaps" (Holt and Larsen, 2011).

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

to introduce a law that would require firms with more than 35 employees to produce gender-disaggregated wage statistics.[7] This campaign promise was generally viewed as an attempt by the government to get a better standing among female voters (NordjyskeStiftstidende, 2007). The campaign promise surprised most observers because the same administration had stalled a similar proposal years earlier.

After winning the election in February, little discussion addressed fulfilling this promise, but eight months after the re-election, in October 2005, the government announced it would introduce a bill to amend the Equal Pay Act. The Ministry of Economics introduced the bill to Parliament in December 2005. The proposal was adopted on June 2006, and the new provisions came into force in January 2007. In Appendix II, we provide additional details on the timeline and implementation of the law.

III How Might Transparency Affect the Gender Pay Gap?

We now discuss mechanisms through which transparency may affect firms' gender pay gap and worker outcomes. An extensive literature provides evidence of the gender pay gap and explores potential causes. (Goldin (2014) and Blau and Kahn (2017) provide a detailed survey of the literature). A large portion of the gender pay gap can be explained by several observable characteristics such as education, experience, hours worked, occupation or industry, and geographical location, among others. Blau and Kahn (2017) find 91.6% of the gender pay gap in 2010 in the US can be attributed to these observable factors. In our empirical analysis, we do not capture changes in the gender wage gap driven by these observable factors, because, as we explain in section V.1, we control for observable characteristics and exploit within-firm-employee variation.

The gender pay gap might be driven by unobservable factors (the "unexplained" component) such as differences in risk aversion or bargaining power between male and

[7]The snap election was announced on January 18, one year earlier than scheduled, and took place on February 8.

female employees. Below, we discuss some of these explanations with respect to our results.

A number of studies propose the differences in bargaining power and the willingness to bargain between men and women contribute to the gender pay gap. Hall and Krueger (2012) find women are roughly half as likely as men to bargain for wages. Babcock and Laschever (2003) find women are less informed of the market value of their work than men and are less likely to negotiate. Transparency, by increasing the information of employees about their peers' wages, affects the bargaining process. In a recent paper, Cullen and Pakzad-Hurson (2019) provide a dynamic bargaining model of wage negotiations as a function of transparency. In their model, when transparency is high, firms are reluctant to offer high wages to an employee, because doing so will affect negotiation with other employees. As a result, overall wages decline and dispersion in wages is reduced.

Employees might bargain harder not only because knowledge of their peers' salary is useful in negotiation, but also because they dislike unequal wages. Cullen and Perez-Truglia (2017) and Cullen and Perez-Truglia (2018) show employees have large misperceptions about the salary of their peers. Once transparency is increased, employees who realize the gender pay gap is higher than they thought might be encouraged to put more pressure on firms to reduce it.

Alternatively, managers could be averse to being associated with unequal compensation. As a result, they act to reduce the gender gap when compensation is known. Indeed, the rationale behind the UK introducing wage-transparency regulation in 2017 was supposed to work by shaming firms into action. Although in Denmark, firms are not required to make salaries public as in the UK, this same mechanism can be at work, because all employees as well as some external stakeholders learn about the wages.

In addition, the closing of the gender wage gap once information about wages is available to employees might be profit maximizing. Prior literature shows employees who learn they are earning less than their peers become less productive (Akerlof and Yellen, 1990; Breza, Kaur, and Shamdasani, 2018). Hence, an optimal response of managers

to increased transparency is to adjust wages and reduce the discrepancies to avoid the lower effort provision (Cullen and Pakzad-Hurson, 2019).

The above mechanisms are related to transparency leading to a change in the information environment. An alternative mechanism is that firms learn the intentions of the government. Specifically, the enactment of the law might create expectations that additional legislation would be related to gender pay equality in the future, causing firms to reduce the gender pay gap in anticipation.

IV Data and Summary Statistics

IV.1 Data and sample description

Our main dataset is the matched employer-employee dataset from the Integrated Database for Labor Market Research (IDA database) at Statistics Denmark. In addition to the employer's identification number (CVR), and employee identification number (CPR), the IDA dataset contains detailed information for employees' compensation, demographics, and occupation. For compensation, we have information on employees' wages and bonuses. Furthermore, for each employee, we observe their age, gender, and education, as well as their position in the firm hierarchy.

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

We start with the universe of limited-liability firms in Denmark and their employees included in the IDA dataset. For ease of comparison, for the employee-level outcomes,

we focus on full-time workers, excluding CEOs and boards of directors. We drop firms in industries unaffected by the policy (farming, gardening, forestry, and fishery). We require firms to have financial information, which results in dropping 0.8% of firm-years in the sample. We end up with a panel of employee-firm-years over the 2003-2008 period, which we use to test whether transparency on wages by gender has real effects on firms' compensation policies.

We perform a difference-in-difference-in-differences analysis where we compare the effect of the regulation on male wages (i.e., difference between the change in male wages in treated firms relative to control firms) to its effect on female wages. We define treated firms to be firms that employ 35-50 employees prior to the introduction of the law, and control firms to be those that employ 20-34 workers. We take a narrow window around the 35-employee cutoff so that the control firms are close in size to the treated firms, and hence likely to be a valid counterfactual.

We define treated and control groups based on the 35-employee threshold and do not take into account the criterion that firms should have at least 10 male and 10 female employees in one six-digit DISCO code. The reason is that firms do not typically have DISCO code information. As a result, firms in practice often do not follow the occupational-codes recommendation to report disaggregated wage statistics, or ignore them altogether.[8] This observation is consistent with the information we obtained from the Confederation of Danish Employers (DA), the largest employer association, covering 24,200 employers and about a third of Danish employees in the private sector. In fact, according to the DA, 35% of firms that reported gender disaggregated wage statistics through them did not satisfy the second criterion; yet, all of these firms had more than 35 employees. In addition, this finding is consistent with how the law was interpreted more widely. The description of the law by the EU and the International Labor Organization (ILO) only mentions the criterion that firms above the 35-employee threshold must

[8]In Appendix II, we provide a number of examples of firms that did not follow the DISCO requirement.

comply.[9]

IV.2 Summary statistics

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.[10] The average annual (hourly) wage for employees in the treated firms is \$54,500 (\$34.4), whereas for the control group it is \$53,000 (\$33.5). The average employee in the sample is 40 years old and has 17 years of work experience in both treated and control groups. On average, 25% of employees in treated and control groups hold a college degree. Consistent with the well-documented employer size-wage effect (Brown and Medoff, 1989; Idson and Oi, 1999), the average individual wage in treated firms is higher than that in control firms. However, the average employee is similar in terms of other observable characteristics between treated and control firms.

Treated firms are larger than control firms by construction. For example, as shown in Panel B, the average treated firm has 42 employees pre-treatment, assets of \$7.2 million, sales of \$11.68 million, and pays total wages of \$2.3 million compared to 26 employees, \$5.8 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% of employees, on average, who are male.

Panel B also shows the "unexplained" part of the pre-law gender wage gap for treated

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

[10]In Internet Appendix Table IA1, we present the industry composition for the overall sample, as well as for the treated and control groups separately. Our sample spans all major industry categories, whereas both groups are equally represented across all industries around the 35-employee cutoff.

and control firms.[11] When we do not control for observables, we find, on average, a gender pay gap of 18.7% prior to the regulation. When we instead control for observables, we estimate, on average, a pre-treatment gender pay gap of 15.4%, which is not statistically significant between treated and control firms.[12]

In Table 2, we show univariate tests that demonstrate the main effect of the regulation on wages. We calculate 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. To control for compositional changes, we keep only observations in which the employee works at the same firm as he did in 2005.

Wages increase for all employees, irrespective of their gender in both the treated and the control group. However, male employee wages grow by 1.65 percentage points less in treated firms than in control firms, and this difference is statistically significant at the 1% level. By contrast, we find no significant differences in female wage growth between treated and control firms. These univariate comparisons suggest the reform requiring wage transparency resulted in a 1.96-percentage-points-lower wage growth for male employees than for female employees.

Another interesting observation is the rate at which the wage gap changes in treated and control firms. In control firms, the wage growth rate of male and female employees is similar. The difference is -0.0012, but it is not statistically significant. That is, no change in the wage gap occurs in control firms. However, in treated firms, the growth

[11] We calculate the gender pay gap as the estimate of the coefficient (γ_j) on a male dummy in the following log wage regression estimated at the individual level in our baseline sample over 2003-2005: $log\,wage_{ijt} = \alpha_j + \sum_{n=1}^{N} \gamma_n \times 1(n = j) \times male_i + \alpha_k + \alpha_t + \beta X_i + \epsilon_{ijt}$, where i is an individual, j is a firm, k is an occupation, N is the total number of firms, and t represents a year. α_j, α_k, and α_t are firm, occupation, and year fixed effects, respectively. X_i consists of individual controls that include age, work experience, education, and number of children. Standard errors are two-way clustered on the person and firm level.

[12] To examine whether our estimates are in line with other studies for Denmark, we run an additional analysis for the overall population in Denmark in 2005, where we find an average "unexplained" gender pay gap of 12%. This finding is consistent with the estimates of the gender pay gap for Denmark by (Kleven, Landais, and Søgaard, 2018; Gallen, Lesner, and Vejlin, 2019). Gallen, Lesner, and Vejlin (2019) find an unexplained gender pay gap in 2010 of approximately 12%. Kleven, Landais, and Søgaard (2018) report a raw (no controls) gender pay gap of about 20%.

rate of male employees is lower than that of female employees. The difference is -2.08 percentage points, and it is significant at the 1% level. The fact that male wages grow more slowly than female wages in treated firms implies a reduction in the gender pay gap of around two percentage points. This reduction is economically meaningful. The level of the "unexplained" pay gap prior to the reform was 15.4% based on the mean salaries of men and women. Thus, the pay gap is reduced by about 13% following the law.

An alternative way to visualize the effect of the law on wages is to focus on the discontinuity created by the law at 35 employees. For each employee, we construct the change in log wage from 2005 to 2008. We averaged across all employees in firms with a given employee size. We plot this average separately for male and females in Appendix Figure A1. We also fit a third-order polynomial on each side of the discontinuity. We observe a clear discontinuous drop at the 35-employee threshold for male employees, but not for female employees. This observation is consistent with the results in Table 2.

V Effect of Transparency on Wages

V.1 DIDID estimates

We next turn to a multivariate regression analysis and estimate the effect of disclosing gender pay disparities on individual wages. We estimate the following OLS regression in which the coefficient of interest is δ:[13]

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

[13]Equation 1 is our preferred specification. In the tables, we also report diff-in-diff specifications separately for male and female employees.

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;[14] *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 $Male_i$, $Treated_j$, $Post_t$, and $Treated_{ij} \times Male_i$ are not shown, because their coefficients are absorbed by the fixed effects. X_{jt} and Z_{ijt} capture time-varying firm- and individual-level control variables, respectively. X_{jt} controls for firm size proxied by sales (log-transformed). Z_{it} controls for time-varying individual characteristics (age, work experience), following Blau and Kahn (2017). α_t is year fixed effects to absorb aggregate macroeconomic shocks.

We also include interacted individual and firm fixed effects, α_{ij}. By including these fixed effects, we control for time-invariant person characteristics (e.g., skill, education), time-invariant firm characteristics, and the match between firms and workers.[15] Essentially, we compare the *same employee at the same firm* before and after the regulation. That is, our estimation results are free of composition effects.[16] Standard errors are double clustered at the individual and the firm level.[17]

Table 3 reports the results. Column 1 compares the change in wages of male employees in treated firms relative to control firms. Column 2 repeats this analysis comparing instead wages for female employees. We find that wages of male employees in treated firms grow 1.7 percentage points more slowly than wages of male employees in control firms. This magnitude is similar to that in our univariate results in Table 2. The effect

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

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

[16]To further confirm that compositional changes are not explaining our findings, we repeat our analysis in Internet Appendix Table IA2 by keeping only observations in which the employee works at the same firm as he did in 2005.

[17]In our empirical strategy, we measure differences between control and treatment groups. General equilibrium effects might attenuate our measurement of the true effect of the regulation. For example, if a large number of treated firms lower wages for male employees, wages for male employees could decrease in control firms as well, as the outside option of the latter decreases. In Internet Appendix Table IA3, we test for spillover effects and do not find supportive evidence.

is statistically significant at the 1% level. On the contrary, we find a positive, but not significant, coefficient on treated firms' female wages relative to control firms in column 2. In column 3, we estimate the triple-differences model of equation 1 that compares the effect of the law on wages of male relative to female employees. The triple-difference coefficient shows male wage growth is two percentage points lower than female wage growth, and the effect is statistically significant at the 1% level. In column 4, we additionally control for interacted firm×year-fixed effects that absorb any time-varying changes at the firm level that could be driving our results.[18] The triple-difference coefficient is not qualitatively affected.

In columns 5-8, we repeat our estimation, additionally controlling for individual time-varying characteristics (age and experience) and firm size (proxied by logarithm of sales) to account for the well-documented employer size-wage effect whereby larger firms pay higher wages (e.g., Brown and Medoff, 1989; Idson and Oi, 1999). Including firm size is important in our setting given that the treated group includes larger firms by construction. At the same time, acknowledging that firm size is likely an endogenous control that could potentially bias our estimates is important. The fact that the estimated coefficients remain virtually unchanged after controlling for firm size is reassuring.

One potential concern with our result is that it could be driven by differential changes in working hours, with men reducing their hours more than women while the compensation per hour remains the same. Prior literature (e.g., Card, Mas, Moretti, and Saez, 2012; Cullen and Perez-Truglia, 2017; Breza, Kaur, and Shamdasani, 2018) show employees change the number of hours worked when they receive feedback about their relative salary. To address this concern, in Internet Appendix Table IA4 Panel A, we replicate Table 3 using hours worked as the outcome variable. Indeed, we find a negative and significant effect on hours worked for both male and female employees in treated firms relative to controls. The estimated coefficient for female employees is larger than for males, although the difference is not statistically significant. This result indicates a re-

[18] Firm×year fixed effects subsume the coefficient on $Treated x Post$.

duction in male employees working hours relative to female employees' working hours does not explain our results. In addition, in Panel B, we use hourly wages as the dependent variable. We show hourly wages grow by 1.2 percentage points less for male employees in treated firms as compared to control firms (column 1), whereas the triple-difference coefficient shows male wage growth is 1.3 percentage points lower than female wage growth (column 3).

Another potential concern is that firms may offset the slower growth of male wages by an increase in bonuses. We address this issue in Internet Appendix Table IA5, where we estimate the effect of the law on total compensation (wages and bonus). Because this effect is only operative for employees who receive performance pay, we split the sample into employees who never received a bonus in our sample period and those who did. In the overall sample as well as in the two sub-samples, we find the estimated triple-difference coefficient is negative and large in magnitude.

Using data at the employee level, our analyses shows the regulation results in a significant drop in male employees' wage growth and no effect on females wages. Taken together, these results imply a reduction in the gender pay gap. We confirm this reduction using gender pay gap at the firm level. Table 4 presents estimates from a diff-in-diff specification in which the dependent variable is the firm-level wage gap measured at different percentiles of the wage distribution. To control for compositional changes in the firm-level analysis, we condition this analysis on individuals who work at the same firm they did in 2005 (as in Table 2). In columns 1 and 2, we use the average wages of male and female employees to measure the wage gap. We find a 1.9-percentage-point decline in treated firms' gender pay gap following the regulation change, as compared to controls—a similar estimate to our individual-level analysis. This drop is equivalent to a 13% reduction in the pre-treatment "unexplained" gender pay gap.

We further evaluate whether this average effect is driven by a few employees at the top of the wage distribution or is more evenly distributed throughout the organization. We thus compute the ratio of male to female wages at the firm level at the 10^{th}, 50^{th},

and 90th percentiles of the wage distribution. We present the results in columns 3-8, Table 4. When comparing wages by gender at the bottom of the wage distribution, we find a large and significant decline equal to 3.7 percentage points that is statistically significant at the 1% level. We also find a 1.9-percentage-point decline when we consider the gender pay ratio based on median wages, significant at the 5% level. The decline is weaker and not statistically significant when we instead consider gender pay differences at the 90th percentile of the wage distribution; however, the estimated magnitude of a 1.4-percentage-point decline is economically large. Overall, this analysis suggests the regulation affected all levels of the organization, but it is particularly strong toward the bottom and middle of the wage distribution.

V.1.1 Dynamics

A first-order concern with the diff-in-diff analysis is whether wages follow differential trends in small and large firms. Differential trends alone, however, would not explain our results, because our estimated effect on wages is concentrated on male employees (as opposed to all employees). To drive our findings, an omitted variable would need to not only 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.

Figure 1 (a) shows year-by-year coefficient estimates for male employees between 2003 and 2008, omitting 2005, the year the law was announced. We find no significant difference in the evolution of male wages between treated and control groups prior to the adoption of the law. We observe a sharp drop in the wages of male employees in treated firms relative to control firms starting in 2006 (0.9 percentage points, significant at the 1% level), which increasingly persists up to 2008 (1.38 percentage points in 2008, significant at the 1% level). In Figure 1 (b), we look instead at the year-by-year estimates on female wages and observe no significant changes. Consistent with our previous findings, Figure 1 (c) shows the triple-difference coefficient between male and female employees is insignificant prior to the law, drops in 2006 and 2007 (1.14 percentage points and 2.17

percentage points, respectively), and remains at that level in 2008. These results support the identifying assumption that wages in treated and control firms were following parallel trends prior to the law and that the effects only appear after the law was implemented.

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 around the same time as the disclosure law. In this 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 perform several placebo tests where the treatment threshold takes each of the values between 15 and 100 employees, excluding the range of 20-50 employees. We plot the average placebo coefficients in Figure 1, Panels (b), (d), and (f), together with our baseline coefficient estimates shown in Panels (a), (c), and (e). We show the average placebo coefficients are close to zero both before and after the law in all cases, mitigating concerns that a different factor that affected wages around the same time of this law drives our results, because this factor would need to affect firms exactly at the 35-employee cutoff.

V.2 Difference-in-Discontinuities estimates

A remaining concern with our diff-in-diff analysis is whether the control group, which by definition includes smaller firms, is a good counterfactual. To address this concern, we follow Grembi, Nannicini, and Troiano (2016) and employ a diff-in-disc design.

This approach takes the difference between the pre-treatment and post-treatment discontinuity at 35 employees. As such, the diff-in-disc allows us to difference out the effect of any potential pre-existing discontinuity at 35 employees.[19] In contrast to a traditional RDD design, the diff-in-disc design examines whether the gender pay gap disclosure legislation led to a change in the discontinuity around the 35-employee cutoff. As such, it can identify the effect of the 2006 regulation on gender pay gap from the

[19]For example, since 1986, firms with more than 35 employees are required to have a work council (a body in which representatives of workers and management discuss issues), which could potentially affect salaries.

effect of any potential pre-existing regulation that uses the same cutoff. Specifically, we estimate the following for male and female employees separately:

$$\begin{aligned} Log\,Wage_{ijt} &= \alpha_i + \alpha_1 Emp_{jt} + Over35_{jt}(\beta_0 + \beta_1 Emp_{jt}) \\ &+ Post_t\Big[\gamma_0 + \gamma_1 Emp_{jt} + Over35_{jt}(\delta_0 + \delta_1 Emp_{jt})\Big] + \epsilon_{ijt} \end{aligned} \tag{2}$$

where i indexes employees, j indexes firms, and t indexes years. Emp is the number of employees minus 35, $Over35$ denotes the number of employees over the 35 threshold,[20] and $Post$ takes a value of 1 for 2006, 2007, and 2008 and a value of 0 for years 2003, 2004, and 2005. Number of employees is based on $t-1$. We include individual fixed effects (α_i's) to reduce the residual variance due to unobserved factors affecting wages (Lee and Lemieux, 2010). The coefficient δ_0 is the diff-in-disc estimator and identifies the treatment effect of the pay-gap disclosure. Standard errors are double clustered at the individual and firm level.

Table 5 presents the main diff-in-disc results. In column 1, we show the law has a negative effect on wage growth for male employees for firms above the 35-employee cutoff. Male employee wages grow less by 1.1 percentage points for firms above the threshold, and this difference is significant at the 1% level. By contrast, a positive but not significant change exists for female employee wages following the policy change, as shown in column 2.

Figure 2 presents the coefficients of year-by-year diff-in-disc regressions for male and female employee wages. The coefficients shown are estimates of the change in the discontinuity around the 35-employee threshold between a given year and the base year (2005). In Panel A, no significant change occurs in the discontinuity in years 2003 to 2005 for male employee wages. The discontinuity jumps downwards in 2006 and gradually widens over time (-1.2 percentage points in 2006, -1.5 percentage points in 2007,

[20] In Internet Appendix Table IA6, we show qualitatively similar results when we use year t-2 instead to define the running variable.

and -1.8 percentage points in 2008). In Panel B, we show no significant change in the discontinuity for female employees.

We present several robustness checks for our diff-in-disc analysis. First, we test whether firms manipulate employee size to avoid having to comply with the law. In Internet Appendix Figure IA1, we show the null hypothesis of continuity of the density around 35 employees cannot be rejected either before or after the policy change, suggesting firms do not systematically manipulate their size. Second, in Internet Appendix Table IA7, we test for the balancing of covariates at the individual (age, education, gender, work experience, number of children) and firm (female share, profits (in $), assets (in $), sales (in $)) levels in 2005. None of these characteristics display a statistically significant jump around the threshold in 2005. Third, we check the sensitivity of the diff-in-disc estimates to the bandwidth chosen. The results in Table 5 use a bandwidth of 65 (so that we estimate equation 2 using firms with zero to 100 employees). However, in Internet Appendix Figure IA2, we show the estimates are very stable along a wide range of bandwidth choices.[21]

VI Joining, Leaving, and Promotions

Our results establish that the law has an effect on wages. However, this effect might not be the only response by firms. Changes in the way similar employees of different genders are compensated might affect the willingness of firms to hire employees of different genders or the propensity of employees to seek employment in firms with different gender pay gaps. These would result 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 next examine the effect of the law's passage on each of these different outcomes.

We start by examining the effect of the law on the joining and leaving rates for

[21] We show the stability of the coefficients up to a bandwidth of 110, which is the optimal bandwidth using the method in Calonico, Cattaneo, and Titiunik (2014).

female and male employees separately. To this end, we estimate a diff-in-diff model at the firm level with the outcome variable defined as the percentage of female (male) employees joining the firm over total employment and the percentage of female (male) employees leaving the firm over total employment. We present the results in Table 6 in a specification with firm and year fixed effects. In Panel A, we find a positive and statistically significant coefficient at the 1% level on female employees joining, but no significant effect on female employees leaving the treated firms. The magnitudes we estimate are large. The pre-law average of female employees hiring share is 5%. Our estimates indicate the law causes female hiring rates to increase by 0.8 percentage points. One possible interpretation of these results is that firms are able to attract more female employees because they offer relatively fairer compensation.

By contrast, in Panel B, we find a negative and significant effect on male employees joining the firm, consistent with the fact that male wage growth in treated firms is lower following the law, and thus these firms are less attractive to male employees. The results are significant at the 10% level and economically large. Relative to the pre-treatment average male hiring share of 8.5%, male hiring rates decrease by 0.8 percentage points. We continue to find an insignificant effect on male employees' departures.

In Appendix Table A2, we present the year-by-year coefficient estimates for all four dependent variables. Across all specifications, we find no significant pre-trends prior to the legislation. In addition, the estimated coefficients on joining rates for female employees (Panel A) and male employees (Panel B) in treated firms become significant in 2007. Moreover, we find no significant effects on employees leaving around the regulation change.[22]

These results highlight the fact that the composition of employees changes in treated firms following the regulation. An interesting question is whether firms are "gaming" the system by hiring more highly paid women and/or firing more highly paid men to report

[22]In Appendix Table A3, we report the main diff-in-disc specification for female and male joiners, and leavers, shares. We find results consistent with the diff-in-diff analysis, albeit the result on female joiners' share is not statistically significant.

smaller gender wage gaps. We explore this hypothesis in Internet Appendix Table IA8 and find no support for this hypothesis.

We next examine whether the law affected firm promotion decisions. We define a promotion to be any transitions from a lower- to a higher-paying occupation code, where we rank occupation codes based on average wages computed in 2002. Table 7 presents the results. Column 1 shows male employees are less likely to be promoted to higher-paid positions within the organization, although the effect is not statistically significant. Column 2 shows instead that female employees in treated firms are more likely to be promoted after the passage of the law than are female employees in control firms. Columns 3 and 4 show the triple-difference coefficient is statistically significant and economically large with a probability of promotion of 1.2 to 1.6 percentage points higher for females than for males. Columns 5-8 show the coefficient estimates remain unchanged after controlling for firm size and individual-level controls.

We also present robustness for the promotion results in the Appendix. Table A4 shows the diff-in-diff year-by-year estimates, and presents consistent evidence of no pre-trends prior to the law and a significant differential effect in 2007.[23] Overall, these results indicate that the law also improved female employees' ability to climb up the corporate ladder.

VII Firm Performance

In this section, we explore whether the law on gender pay affects firm-level outcomes. Specifically, we examine its effect on average wage, firm productivity, and profits. We perform a diff-in-diff analysis at the firm level in a specification with firm and year fixed effects, and report the results in Table 8.

In columns 1-2, we analyze the effect of the law on average wages of treated firms

[23]In Appendix Table A5, we report the main diff-in-disc specification for our promotion results. We confirm the absence of any statistically significant effect on male promotions, and a positive and statistically significant effect for female promotions following the legislation.

as compared to the group of control firms. Although our main result is that the law reduced the growth rate of male wages, the average wage at treated firms may still remain constant or even increase due to composition effects (e.g., treated firms might hire high-wage individuals after the passage of the law). We confirm the average wage per employee (log-transformed) is reduced by 2.8%. We only observe a negative and significant effect on employee wages and not on other labor costs, such as pensions and other social security costs, in columns 3-4, because the latter are not directly impacted by the regulation.

In columns 5-6, we examine the effects of the law on firm productivity. The effect on productivity is *a priori* ambiguous. If information on gender pay gap lowers job satisfaction of female employees, it might negatively affect their productivity (Akerlof and Yellen, 1990). A similar effect should be observed if male employees are dissatisfied with lower wage growth relative to their peers. However, if increased transparency and firms' responses create a sentiment of fairness among employees, productivity might be positively affected. Although the law may differentially affect the productivity of male versus female employees, our estimates capture the average effect of the law on firm productivity, because we do not have data on productivity at the individual employee level. We observe that, on average, productivity (measured as the log-transformed sales per employee) drops by 2.7% in treated firms following the regulation as compared to control firms, and this reduction is statistically significant at the 5% level.

Lastly, in columns 7-8, we estimate the effect of the law on firm profitability, measured as profits per employee. Because profits can take negative values and thus cannot be log-transformed, we estimate a Poisson regression model (after left-censoring profits at 0).[24] We find no effect on firm profits, which can be explained by the offsetting effects of lower employee productivity and wages.

In Appendix Table A6, we present the year-by-year coefficient estimates. We find no

[24]In unreported regressions, we find similar results using OLS estimation and using profits as the dependent variable.

significant effects in any years prior to the legislation across all measures, which provides evidence in favor of the parallel-trends assumption. Consistent with the individual-level analysis presented in Figure 1, average employee wages in treated firms start declining in 2006 and continue to do so until 2008. Interestingly, the drop in average wages precedes the drop in productivity by one year. None of the coefficients of the pension & other labor cost variable and, most importantly, profits are statistically significant before or after the law.

We further present results using a diff-in-disc analysis. We thus examine whether firms above the threshold deferentially respond to the policy change after the law in terms of average wages, pension & other labor costs, employee productivity, and profits. We present this analysis in Appendix Table A7. Consistent with the earlier findings, we find a drop in average wages for firms above the threshold post legislation. Nevertheless, the drop in productivity is much smaller in economic magnitude and not statistically significant. Finally, we confirm the result that profits are unaffected.

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 discuss cases where we might expect to find a more pronounced reduction in gender pay gaps.

We start by considering the role of pre-existing gender pay inequality at the firm. We first estimate the "unexplained" part of the gender pay gap at the firm level over 2003-2005, following the methodology detailed in section IV.2. We next classify firms into deciles of gender pay gap, and we asses whether firms with higher pre-treatment gender pay gap inequality exhibit larger effects.

We present the results in Appendix Table A8. In column 1, we find the drop in the growth rate of male wages gets larger in magnitude for firms with higher pre-treatment gender pay gaps. Moving from the lowest decile of the pre-treatment gender pay gap

distribution to the highest decile, we observe a 3.8-percentage-point-larger drop in the wage growth of male employees. By contrast, we find no significant effect of the regulation on female employees across deciles. In column 3, the differential effect is large in magnitude but not statistically significant, but it becomes significant at the 5% level after controlling for firm-year fixed effects. In sum, our results suggest firms with higher gender pay inequality close the gender gap more aggressively. This finding might be due to the fact that transparency leads to an increase in accountability in these firms.

Note the relation between the pre-treatment gap and the strength of the response is a priori ambiguous. For example, firms with managers with pro-women preferences might both have a smaller gender pay gap and react more strongly to the disclosure regulation.

Another example is the relation between the pre-treatment wage gap and the strength of the response varying by employee characteristics. For example, the fact that having children penalizes women in terms of wages is well known (e.g., Bertrand, Goldin, and Katz, 2010; Kleven, Landais, and Søgaard, 2018).[25] In Appendix Table A9, we show this same characteristic also affects the strength of the reaction to the regulation. Specifically, we show that male employees with children experience a lower decline in wages than men without children. As a result, the pre-treatment wage gap is higher among employees with children, and yet it does not close as much for this group.

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 firm gender wage gap.

[25]In unreported analysis, we confirm in our pre-treatment sample that the gender pay gap is bigger among the group of employees with children.

Empirically investigating the effect of gender pay transparency as a measure to reduce gender pay discrimination within firms is challenging because it requires finding both exogenous variation in transparency and detailed information on 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 both a difference-in-differences and a difference-in-discontinuities design, we find changes in compensation within firms. Specifically, male employees experience slower wage growth than 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.

One caveat of our analysis is that our estimates are the local average treatment effect for firms around the 35-employee-cutoff, and they do not necessarily apply to larger or smaller firms. Yet the number of firms in the 20-to 50- employees range is significant (around 20% of firms and 16% of all employees in the private sector belong in this firm employee size range).

Although the regulation had the effect of reducing the gender wage gap, it also had the unintended consequence of reducing the average employee wage at the firm. Policymakers should thus weigh the welfare implications when designing regulation to reduce the gender wage gap.

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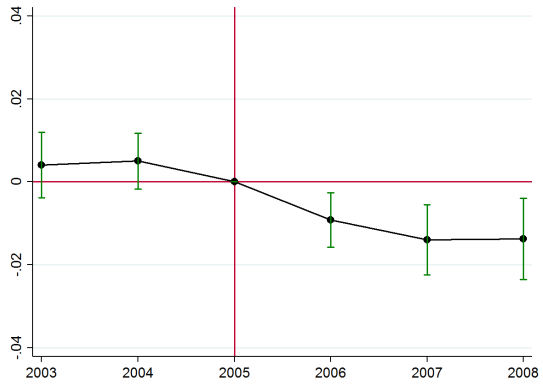
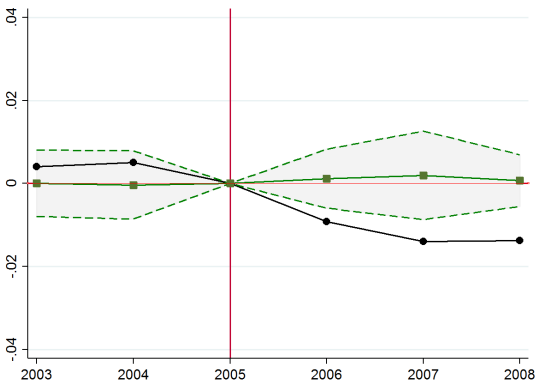
Figure 1: Diff-in-diff Dynamics and Placebo Estimates

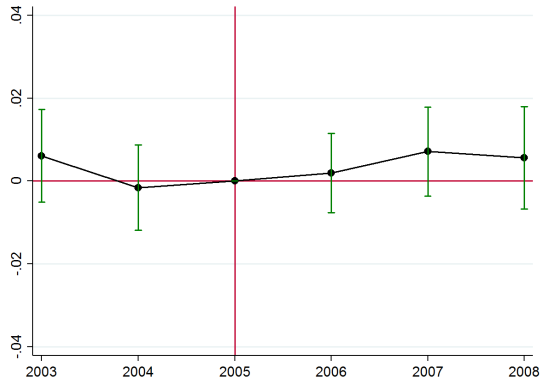
The black solid lines in panels (a) and (b) show the diff-in-diff year-by-year coefficient estimates for male employee wages. The black solid lines in panels (c) and (d) show the diff-in-diff year-by-year coefficient estimates for female employee wages. The black solid lines in panels (e) and (f) show the triple differences year-by-year coefficient estimates on all employee wages. Year 2005 is omitted. In panels (b), (d), and (f), the solid green lines represent the average coefficient estimates of placebo tests. The treatment threshold for the placebo tests is defined from 15 to 100 employees, excluding 20-50 and the green dashed lines represent the 95% confidence interval for the placebo estimates. In panels (a), (c), and (e), the vertical lines represent 95% confidence intervals.



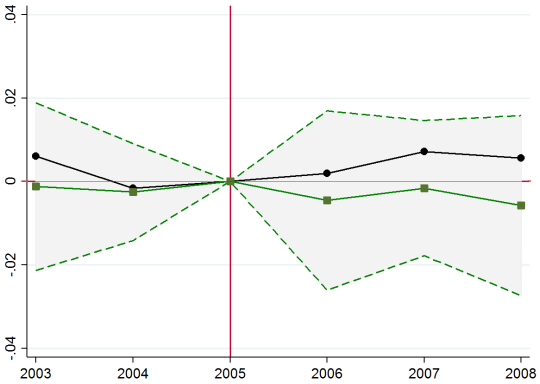
(a) Male Log(Wage)



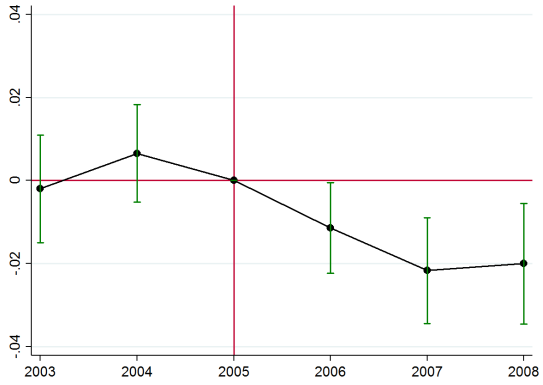
(b) Male Log(Wage)



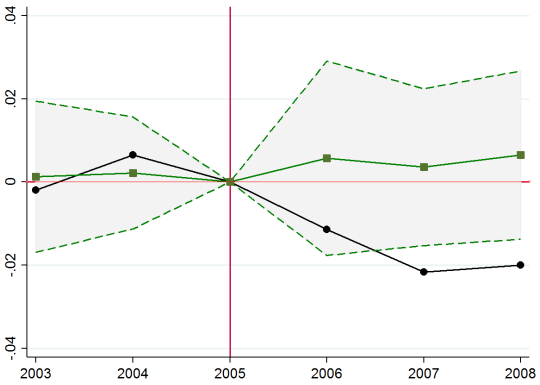
(c) Female Log(Wage)



(d) Female Log(Wage)



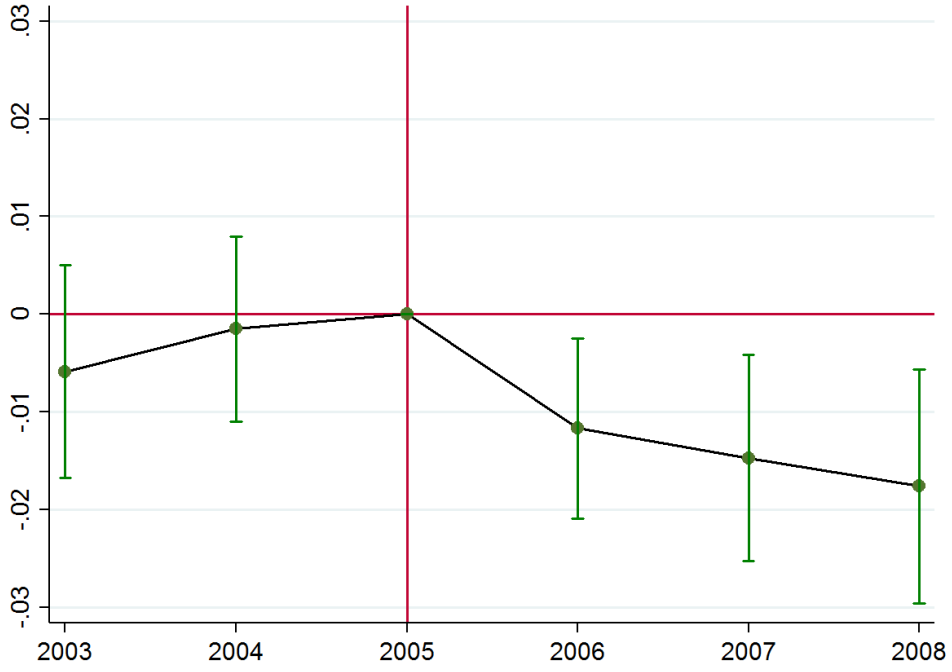
(e) All Employees Log(Wage)



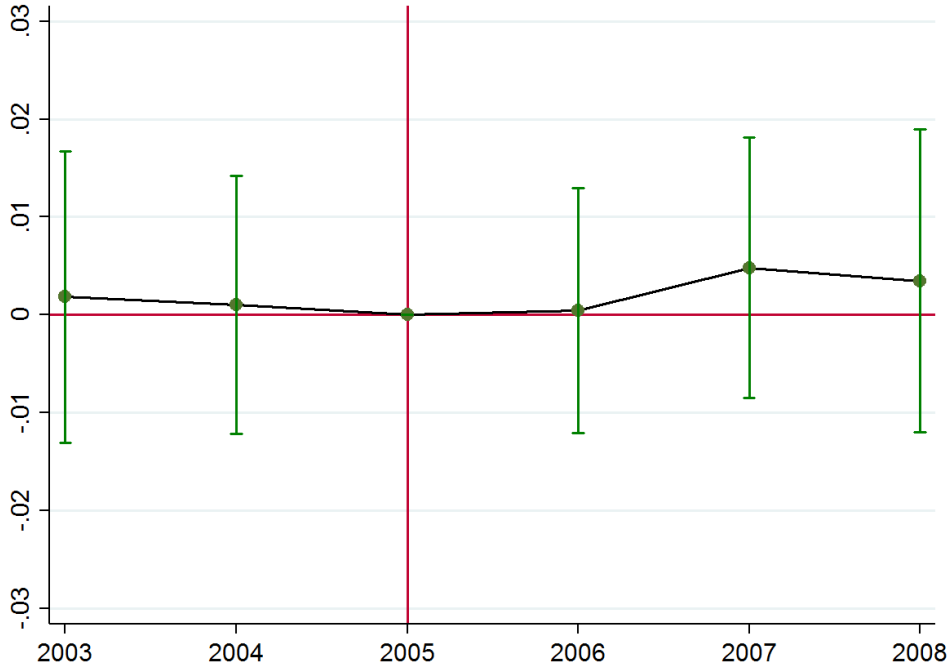
(f) All Employees Log(Wage)

Figure 2: Diff-in-Disc Dynamics

The black solid lines show the difference-in-disc year-by-year coefficient estimates for male (panel a) and female (panel b) employee wages. Year 2005 is omitted. The vertical lines represent 95% confidence intervals.



(a) Male Log(Wage)



(b) Female Log(Wage)

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 35-50 employees prior to the introduction of the law, and controls are those with 20-34 employees. The variables are averaged over the pre-law years 2003-2005. The table reports unconditional means, standard deviations, and p-values of the differences in means between treated and control groups pre-treatment. For the conversion from DKK to USD, we use the spot exchange rate at the year-end. Detailed descriptions of the variables are given in Table A1. Firm-level variables are winsorized at 1%. For the t-test, standard errors are clustered at the firm level in Panel A.

Panel A - Employee-Level Characteristics

	All (Treated & Control)			Treated		Control		t-test
	Observations	Mean	S.D.	Mean	S.D.	Mean	S.D.	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Wage (thous. $)	67,823	53.74	23.72	54.50	23.77	53.17	23.63	0.020
Hourly Wage ($)	67,816	33.90	15.10	34.37	15.38	33.53	14.81	0.014
Bonus (thous. $)	67,575	1.17	3.02	1.13	3.06	1.21	3.00	0.232
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

Panel B - Firm-Level Characteristics

	All (Treated & Control)			Treated		Control		t-test
	Observations	Mean	S.D.	Mean	S.D.	Mean	S.D.	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Assets (mil. $)	3,956	6.26	20.46	7.20	13.23	5.81	23.19	0.016
Sales (mil. $)	3,956	9.03	9.64	11.68	10.74	7.73	8.77	0.000
Employment	4,005	31.12	8.49	41.67	4.37	25.97	4.12	0.000
Profits (mil. $)	3,957	0.25	1.88	0.26	1.49	0.25	2.04	0.960
Total Wages (mil. $)	3,950	1.70	0.70	2.26	0.69	1.43	0.52	0.000
Pension & Soc. Sec. (mil. $)	3,950	0.135	0.082	0.179	0.091	0.114	0.068	0.000
Sales/Employee (mil. $)	3,956	0.28	0.30	0.27	0.25	0.28	0.33	0.156
Profits/Employee (mil. $)	3,957	0.007	0.021	0.006	0.020	0.007	0.022	0.101
Wage/Employee (mil. $)	3,950	0.051	0.017	0.051	0.016	0.051	0.017	0.923
Pension & Soc. Sec./Employee (mil. $)	3,950	0.004	0.002	0.004	0.002	0.004	0.002	0.819
Female Share (%)	3,998	0.30	0.21	0.29	0.21	0.30	0.21	0.153
Gender Pay Gap with controls	2,310	0.154	0.278	0.160	0.245	0.150	0.296	0.409
Gender Pay Gap without controls	2,331	0.187	0.319	0.193	0.270	0.184	0.343	0.458

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. Treated individuals are employed in firms with 35-50 employees prior to the introduction of the law, and control individuals are employed in firms with 20-34 employees. Column 3 presents the difference between column 1 and column 2. The first row reports the difference in average male wage between the post-law (2006-2008) and pre-law (2003-2005) periods for the treated (column 1) and control groups (column 2), and the difference between column 1 and column 2 (column 3). The second row similarly reports the first and second difference for the average female wage. The difference-in-difference-in-differences result represents the difference between the change in the male wages and female wages around the disclosure law in treated versus control firms. Standard errors are clustered at the firm level and reported in parentheses. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

log Wage (3-year avg after – 3-year avg before)	Treated	Control	DD/DD/DDD
	(1)	(2)	(3)
Male	0.0885	0.1050	-0.0165***
			(0.0044)
Female	0.1093	0.1062	0.0031
			(0.0055)
DD/DD/DDD	-0.0208***	-0.0012	-0.0196***
	(0.0048)	(0.0042)	(0.0063)

Table 3: Gender Pay Gap Disclosure and Employee Wages

This table reports the effects of gender pay gap disclosure on employee wages. Each column presents coefficients from a different multivariate regression. Columns 1 and 5 (2 and 6) are estimated using only male (female) employees. All other columns use the entire sample. The dependent variable is employee annual wage (log-transformed). *Treated* is a dummy that takes the value 1 for individuals working in firms with 35-50 employees before the introduction of the law and 0 for employees in firms with 20-34 employees. *Post* takes a value of 1 for 2006, 2007, and 2008, and a value of 0 for years 2003, 2004, and 2005. Individual controls include employee work experience and age. Firm controls include sales (log-transformed). Detailed descriptions of the variables are given in Table A1. Standard errors are double clustered at the individual and the firm level and reported in parentheses. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Male	Female	All	All	Male	Female	All	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated x Post	-0.0167***	0.0028	0.0028		-0.0152***	0.0036	0.0036	
	(0.0039)	(0.0045)	(0.0044)		(0.0037)	(0.0044)	(0.0043)	
Male x Post			-0.0022	-0.0041			-0.0033	-0.0046
			(0.0034)	(0.0036)			(0.0034)	(0.0035)
Treated x Post x Male			-0.0195***	-0.0148***			-0.0190***	-0.0144***
			(0.0052)	(0.0053)			(0.0052)	(0.0053)
Person-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Firm x Year FE	No	No	No	Yes	No	No	No	Yes
Firm Controls	No	No	No	No	Yes	Yes	Yes	No
Individual Controls	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.868	0.827	0.866	0.884	0.868	0.826	0.866	0.883
N	145,852	79,532	225,384	223,624	145,262	79,027	224,289	222,529

Table 4: Gender Pay Gap Disclosure and Firm Level Gender Pay Gap

This table reports the effects of gender pay gap disclosure on the firm-level gender pay gap. Gender pay gap is defined as the log ratio of male over female wages at the mean (columns 1-2), the 10^{th} (columns 3-4), the 50^{th} (columns 5-6), and 90^{th} (columns 7-8) percentile of the wage distribution. *Treated* is a dummy that takes the value 1 for firms with 35-50 employees before the introduction of the law, and 0 for firms with 20-34 employees. *Post* takes a value of 1 for 2006, 2007, and 2008, and a value of 0 for years 2003, 2004, and 2005. Firm controls include sales (log-transformed). To address composition changes, we keep only observations in which the employee works at the same firm he did in 2005 as in Table 2. Standard errors are clustered at the the firm level and reported in parentheses. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Mean	Mean	10%	10%	50%	50%	90%	90%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated x Post	-0.0188**	-0.0190**	-0.0377**	-0.0373**	-0.0188**	-0.0188**	-0.0139	-0.0143
	(0.0076)	(0.0076)	(0.0152)	(0.0153)	(0.0079)	(0.0079)	(0.0088)	(0.0088)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.701	0.701	0.513	0.514	0.711	0.711	0.742	0.742
N	13,291	13,263	13,291	13,263	13,291	13,263	13,291	13,263

Table 5: Gender Pay Gap Disclosure and Employee Wages: Diff-in-Disc Estimates

The table shows the diff-in-disc estimates of the impact of the law on male and female wages. We include firms between 1 and 100 employees (i.e., bandwidth of 65) for the years 2003-2008. The dependent variable is wages (log transformed). The table presents estimates of the local linear regression in equation 2 following Grembi, Nannicini, and Troiano (2016). Employment is measured in year $T-1$. Standard errors are double clustered at the firm and individual level and reported in parentheses. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Male	Female
	(1)	(2)
Diff-in-Disc	-0.0114***	0.0032
	(0.0043)	(0.0052)
Person FE	Yes	Yes
R^2	0.850	0.803
N	328,659	191,461

Table 6: Gender Pay Gap Disclosure and Joining and Leaving Rates

This table reports the estimated coefficients from a diff-in-diff model at the firm level with the outcome variable defined as the percentage of female (male) employees joining the firm over total employment and the percentage of female (male) employees leaving the firm over total employment. Panel A refers to female employees, and Panel B to male employees. *Treated* is a dummy that takes the value of 1 for firms with 35-50 employees before the introduction of the law and 0 for firms with 20-34 employees. *Post* takes a value of 1 for 2006, 2007, and 2008, and a value of 0 for years 2003, 2004, and 2005. Firm controls include sales (log-transformed). Standard errors are clustered at the firm level and reported in parentheses. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Panel A: Female			
	Joiners / Emp	Joiners / Emp	Leavers / Emp	Leavers / Emp
	(1)	(2)	(3)	(4)
Treated x Post	0.0084***	0.0083***	-0.0014	-0.0014
	(0.0029)	(0.0029)	(0.0033)	(0.0033)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	No	Yes
R^2	0.408	0.409	0.369	0.370
N	20,203	20,159	20,203	20,159
	Panel B: Male			
	Joiners / Emp	Joiners / Emp	Leavers / Emp	Leavers / Emp
	(1)	(2)	(3)	(4)
Treated x Post	-0.0072*	-0.0076*	-0.0027	-0.0023
	(0.0042)	(0.0042)	(0.0046)	(0.0046)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	No	Yes
R^2	0.388	0.388	0.363	0.365
N	20,203	20,159	20,203	20,159

Table 7: Gender Pay Gap Disclosure and Employee Promotion

This table reports the estimated coefficients from a diff-in-diff model at the individual level with promotion as the outcome variable. We define promotion to be 1 if an employee transitions from a lower- to a higher-paying occupation code, and 0 otherwise. We rank occupation codes based on average wages computed in the population of Danish firms with employment between 0 and 100 in 2002. *Treated* is a dummy that takes the value 1 for individuals working in firms with 35-50 employees before the introduction of the law and 0 for employees in firms with 20-34 employees. *Post* takes a value of 1 for 2006, 2007, and 2008 and a value of 0 for years 2003, 2004, and 2005. Individual controls include employee work experience and age. Firm controls include sales (log-transformed). Standard errors are double clustered at the individual and the firm level and reported in parentheses. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Male	Female	All	All	Male	Female	All	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated x Post	-0.0057	0.0103**	0.0102**		-0.0058	0.0104**	0.0104**	
	(0.0044)	(0.0045)	(0.0045)		(0.0044)	(0.0045)	(0.0045)	
Male x Post			0.0027	0.0046			0.0030	0.0047
			(0.0034)	(0.0037)			(0.0034)	(0.0037)
Treated x Post x Male			-0.016***	-0.012**			-0.016***	-0.012**
			(0.003)	(0.030)			(0.002)	(0.027)
Person-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Firm x Year FE	No	No	No	Yes	No	No	No	Yes
Firm Controls	No	No	No	No	Yes	Yes	Yes	No
Individual Controls	No	No	No	No	Yes	Yes	Yes	Yes
R^2	0.311	0.314	0.312	0.481	0.312	0.314	0.312	0.481
N	99,615	51,971	151,586	150,844	99,307	51,707	151,014	150,268

Table 8: Gender Pay Gap Disclosure and Firm Outcomes

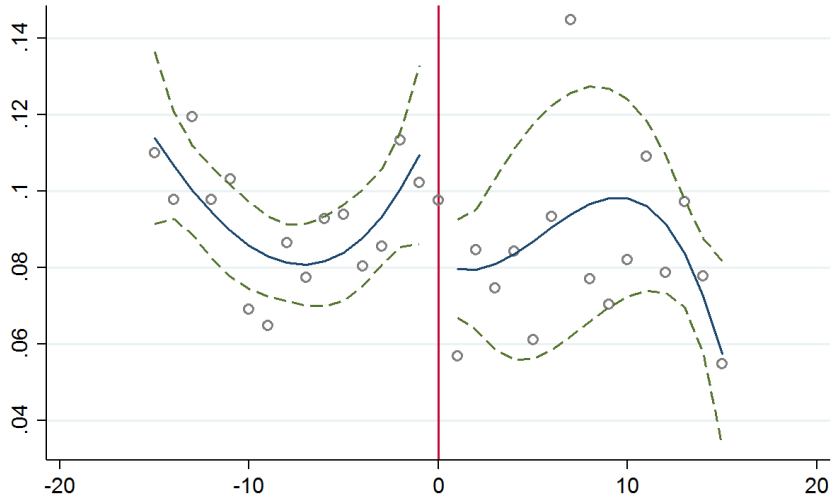
This table reports the effects of gender pay gap disclosure on firm outcomes. The dependent variables are the logarithm of wages per employee (columns 1-2), the logarithm of pension and social security expenses per employee (columns 3-4), the logarithm of sales per employee (columns 5-6), and profits per employee censored at 0 (columns 7-8). We estimate a diff-in-diff model using OLS (columns 1-6) and a Poisson left-censored regression (columns 7-8). Firm controls include sales (log-transformed). Detailed descriptions of the variables are given in Table A1. Standard errors are clustered at the firm level and reported in parentheses. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Wages per Employee		Labor Cost per Employee		Sales per Employee		Profits per Employee	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated x Post	-0.0284***	-0.0281***	-0.0063	-0.0076	-0.0290**	-0.0265**	0.0122	0.0137
	(0.0054)	(0.0050)	(0.0215)	(0.0214)	(0.0118)	(0.0110)	(0.0412)	(0.0418)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	No	Yes	No	Yes	No	Yes
Estimation Method	OLS	OLS	OLS	OLS	OLS	OLS	Censored Poisson	Censored Poisson
R^2	0.851	0.865	0.531	0.534	0.841	0.875	.	.
N	24,241	24,177	24,178	24,137	24,216	24,177	23,387	23,326

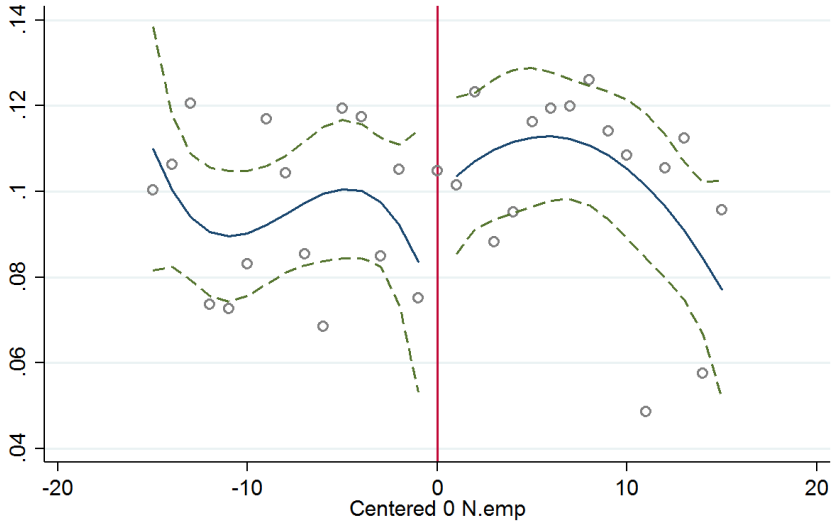
I Appendix

Figure A1: Log Wage Difference by Firm Number of Employees

Vertical axis shows employee's log wage difference between 2008 and 2005 for male (a) and female (b) employees. Horizontal axis shows firm employee size minus 35. The employment number is based on year T-1. The central line is a third order polynomial fit; the dotted lines represent the 95% confidence interval. Scatter points are average changes in individual log wages between 2008 and 2005 for firms with a given employee size.



(a) Log Wage Difference 2008-2005 Male



(b) Log Wage Difference 2008-2005 Female

Appendix Table A1: Variable Definitions

Variable	**Definition**
Firm-level variables	
Firm size	It is the logarithm of sales. Sales are measured in real USD.
Sales per Employee	It is the logarithm of sales per employee. Sales are measured in real USD. Number of employees is based on employment data provided by Statistics Denmark (DST).
Wages per Employee	It is the logarithm of the total wage bill divided by number of employees. The information on wages comes from DST. Number of employees is based on employment data provided by DST.
Labor Cost per Employee	It is the logarithm of pension and social security expenses per employee. The source of data for pensions, social security expenses, and number of employees is DST.
Profits per Employee	It is net income per employee. Number of employees is based on the employment data provided by DST.
Gender Pay Gap	Measures the gender pay gap at the firm level pre-treatment. To construct the variable, we run the following regression at the individual level in our baseline sample over 2003-2005: $log\,wage_{ijt} = \alpha_j + \sum_{n=1}^{N} \gamma_n \times 1(n = j) \times male_i + \alpha_k + \alpha_t + \beta X_i + \epsilon_{ijt}$ where i is an individual, j is a firm, k is an occupation, N is the total number of firms, and t represents a year. α_j, α_k, and α_t are firm, occupation, and year fixed effects, respectively. X_i consists of individual controls including age, work experience, education, and number of children. Standard errors are double clustered at the individual and the firm level. The coefficient γ_j captures the gender pay gap at the firm-level. We compute the pay gap with and without including controls. When we include controls, we capture the "unexplained" part of the gender pay gap, because we estimate it after controlling for observable characteristics.
Have Children	It is an indicator variable that takes the value of 1 if an individual has children, and 0 otherwise.
Pre_Gap	It is an indicator variable that takes values from 1 to 10 that correspond to the lowest and highest deciles, respectively, of the pre-treatment "unexplained" firm-level gender pay gap.
Joiners/Emp	It is defined as the percentage of female (male) employees joining the firm in a given year, normalized by total employment. An employee is considered to have joined the firm in a given year if he/she appears in the firm's employment data that year.
Leavers/Emp	It is defined as the percentage of female (male) employees leaving the firm in a given year, normalized by total employment. An employee is considered to have left the firm in a given year if it is the last year the employee appears in the firm's employment data and the employee does not remain unemployed for more than one year after that. To capture voluntary departures from the firm rather than firings, we exclude departures in which the employee remains unemployed for more than a year.

Variable	Definition
Employee-level variables	
Male	It is an indicator variable that takes the value of 1 if an individual is male, and 0 otherwise. The source is the Danish Civil Registration System.
Wage	It is total annual wage of the employee (log-transformed), "measured" on December 31 of the year. The information on wages comes from DST.
Age	It is the employee age recoded into quartiles. The source is the Danish Civil Registration System.
Work Experience	It is an employee's number of years worked recoded into quartiles.
Promotion	It is an indicator variable that takes the value of 1 if an employee is promoted to a higher-paying occupation code in the firm in a given year, and 0 otherwise. We rank occupation codes based on average wages computed in 2002 for the population of Danish firms with 0-100 employees in 2002 (the year prior to the start of our sample). The promotion variable is constructed based on information from IDA.

Appendix Table A2: Gender Pay Gap Disclosure and Joining and Leaving Rates: Dynamics

This table reports the year-by-year coefficient estimates of the effects of gender pay gap disclosure on joining and leaving rates. The dependent variables are defined as the percentage of female (male) employees joining the firm over total employment and the percentage of female (male) employees leaving the firm over total employment. Panel A refers to female employees, and Panel B to male employees. Firm controls include sales (log-transformed). Year 2005 is omitted. Detailed descriptions of the variables are given in Table A1. Standard errors are clustered at the firm level and reported in parentheses. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Female				
	Joiners / Emp	Joiners / Emp	Leavers / Emp	Leavers / Emp
	(1)	(2)	(3)	(4)
Treated 2003	-0.0030	-0.0027	0.0037	0.0034
	(0.0042)	(0.0042)	(0.0044)	(0.0044)
Treated 2004	0.0013	0.0013	-0.0005	-0.0000
	(0.0042)	(0.0042)	(0.0042)	(0.0042)
Treated 2006	0.0039	0.0038	-0.0021	-0.0018
	(0.0041)	(0.0041)	(0.0048)	(0.0048)
Treated 2007	0.0104**	0.0102**	0.0063	0.0063
	(0.0047)	(0.0047)	(0.0055)	(0.0055)
Treated 2008	0.0098**	0.0102**	-0.0055	-0.0055
	(0.0048)	(0.0048)	(0.0055)	(0.0055)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	No	Yes
R^2	0.409	0.409	0.369	0.370
N	20,203	20,159	20,203	20,159

Panel B: Male

	Joiners / Emp	Joiners / Emp	Leavers / Emp	Leavers / Emp
	(1)	(2)	(3)	(4)
Treated 2003	-0.0042	-0.0038	0.0021	0.0015
	(0.0063)	(0.0063)	(0.0068)	(0.0068)
Treated 2004	0.0034	0.0036	0.0018	0.0014
	(0.0062)	(0.0062)	(0.0066)	(0.0066)
Treated 2006	-0.0089	-0.0089	-0.0075	-0.0079
	(0.0058)	(0.0058)	(0.0066)	(0.0066)
Treated 2007	-0.0135**	-0.0137**	-0.0002	-0.0006
	(0.0061)	(0.0061)	(0.0075)	(0.0075)
Treated 2008	0.0007	0.0002	0.0044	0.0053
	(0.0066)	(0.0065)	(0.0080)	(0.0080)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	No	Yes
R^2	0.388	0.388	0.363	0.365
N	20,203	20,159	20,203	20,159

Appendix Table A3: Gender Pay Gap Disclosure and Joining and Leaving Rates: Diff-in-Disc Estimates

The table shows the diff-in-disc estimates of the impact of the law on joining and leaving rates at the firm level following Grembi, Nannicini, and Troiano (2016). We include firms with 1-100 employees (i.e., bandwidth of 65) for the years 2003-2008. Employment is measured in year $T-1$. The dependent variables are the percentage of female (male) employees joining the firm over total employment and the percentage of female (male) employees leaving the firm over total employment. Columns 1-2 refer to female employees, and columns 3-4 refer to male employees. Detailed descriptions of the variables are given in Table A1. Standard errors are clustered at the firm level and reported in parentheses. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Female		Male	
	Joiners	Leavers	Joiners	Leavers
	(1)	(2)	(3)	(4)
Diff-in-Disc	0.0045	0.0051	-0.0095**	0.0080
	(0.0036)	(0.0044)	(0.0045)	(0.0059)
Firm FE	Yes	Yes	Yes	Yes
R^2	0.443	0.384	0.445	0.381
N	46,973	46,973	46,973	46,973

Appendix Table A4: Gender Pay Gap Disclosure and Promotions: Dynamics

This table reports the year-by-year coefficient estimates of the effects of gender pay gap disclosure on employee promotions. Promotion is an indicator equal to 1 if an employee transitions from a lower- to a higher-paying occupation code, and 0 otherwise. Individual controls include employee work experience and age. Firm controls include sales (log-transformed). Detailed descriptions of the variables are given in Table A1. Year 2005 is omitted. Standard errors are double clustered at the individual and the firm level and reported in parentheses. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	All	All	All	All
	(1)	(2)	(3)	(4)
Male x Treated x Year03	-0.0016	0.0059	-0.0011	0.0067
	(0.0078)	(0.0079)	(0.0078)	(0.0079)
Male x Treated x Year04	0.0118	0.0041	0.0116	0.0039
	(0.0087)	(0.0082)	(0.0088)	(0.0082)
Male x Treated x Year06	-0.0089	-0.0048	-0.0092	-0.0049
	(0.0081)	(0.0080)	(0.0081)	(0.0080)
Male x Treated x Year07	-0.0198**	-0.0172*	-0.0199**	-0.0170*
	(0.0095)	(0.0091)	(0.0095)	(0.0091)
Male x Treated x Year08	-0.0107	-0.0074	-0.0112	-0.0076
	(0.0081)	(0.0085)	(0.0081)	(0.0086)
Person-Firm FE	Yes	Yes	Yes	Yes
Firm-Year FE	No	Yes	No	Yes
Year FE	Yes	No	Yes	No
Firm Controls	No	No	Yes	No
Individual Controls	No	No	Yes	Yes
R^2	0.313	0.481	0.313	0.481
N	151,586	150,844	151,014	150,268

Appendix Table A5: Gender Pay Gap Disclosure and Promotions: Diff-in-Disc Estimates

The table shows the difference-in-discontinuity estimates of the impact of the law on employee promotions following Grembi, Nannicini, and Troiano (2016). We include firms with 1-100 employees (i.e., bandwidth of 65) for the years 2003-2008. Employment is measured in year $T-1$. The dependent variable is 1 if an employee transitions from a lower- to a higher-paying occupation code, and 0 otherwise. We rank occupation codes based on average wages computed in the population of Danish firms with 0-100 employees in 2002. Column 1 refers to male employees, and column 2 refers to female employees. Detailed descriptions of the variables are given in Table A1. Standard errors are double clustered at the individual and the firm level and reported in parentheses. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Male	Female
	(1)	(2)
Diff-in-Disc	0.0057	0.0112*
	(0.0060)	(0.0063)
Person FE	Yes	Yes
R^2	0.314	0.321
N	216,074	119,098

Appendix Table A6: Gender Pay Gap Disclosure and Firm Outcomes: Dynamics

This table reports the year-by-year coefficient estimates of the effects of gender pay gap disclosure on firm outcomes. The dependent variables are the logarithm of wages per employee (columns 1-2), the logarithm of pension and social security expenses per employee (columns 3-4), the logarithm of sales per employee (columns 5-6), and profits per employee censored at 0 (columns 7-8). We estimate a diff-in-diff model using OLS (columns 1-6) and a Poisson left-censored regression (columns 7-8). Firm controls include sales (log-transformed). Year 2005 is omitted. Detailed descriptions of the variables are given in Table A1. Standard errors are clustered at the firm level and reported in parentheses. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Wages per Employee		Labor Cost per Employee		Sales per Employee		Profits per Employee	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated 2003	0.0029	0.0018	-0.0246	-0.0243	-0.0062	-0.0058	0.0266	0.0483
	(0.0063)	(0.0062)	(0.0336)	(0.0337)	(0.0160)	(0.0139)	(0.0528)	(0.0543)
Treated 2004	0.0042	0.0036	-0.0076	-0.0050	-0.0036	0.0027	0.0332	0.0367
	(0.0056)	(0.0055)	(0.0295)	(0.0295)	(0.0148)	(0.0122)	(0.0462)	(0.0471)
Treated 2006	-0.0135**	-0.0137**	0.0090	0.0089	-0.0112	-0.0084	0.0563	0.0691
	(0.0057)	(0.0056)	(0.0252)	(0.0251)	(0.0147)	(0.0123)	(0.0440)	(0.0449)
Treated 2007	-0.0310***	-0.0327***	-0.0194	-0.0196	-0.0506***	-0.0404***	0.0202	0.0299
	(0.0072)	(0.0069)	(0.0288)	(0.0289)	(0.0165)	(0.0149)	(0.0496)	(0.0517)
Treated 2008	-0.0350***	-0.0337***	-0.0428	-0.0436	-0.0361**	-0.0351**	0.0105	0.0137
	(0.0081)	(0.0074)	(0.0323)	(0.0323)	(0.0178)	(0.0164)	(0.0633)	(0.0647)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.851	0.865	0.531	0.534	0.841	0.875	.	.
N	24,241	24,177	24,178	24,137	24,216	24,177	23,387	23,326

Appendix Table A7: Gender Pay Gap Disclosure and Firm Outcomes: Diff-in-Disc Estimates

The table shows the diff-in-disc estimates of the impact of the law on firm outcomes following Grembi, Nannicini, and Troiano (2016). We include firms with 1-100 employees (i.e., bandwidth of 65) for the years 2003-2008. Employment is measured in year $T-1$. The dependent variables are the logarithm of wages per employee (column 1), the logarithm of pension and social security expenses per employee (column 2), the logarithm of sales per employee (column 3), and profits per employee (column 4). Detailed descriptions of the variables are given in Table A1. Standard errors are clustered at the firm level and reported in parentheses. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Employee Number in Year T-1			
	Wages / Emp.	Labor Cost / Emp.	Sales / Emp.	Profits / Emp.
	(1)	(2)	(3)	(4)
Diff-in-Disc	-0.0083*	0.0356	-0.0022	132.4744
	(0.0044)	(0.0241)	(0.0152)	(102.5967)
Firm FE	Yes	Yes	Yes	Yes
R^2	0.934	0.608	0.864	0.809
N	46,974	46,485	46,862	45,477

Appendix Table A8: Diff-in-diff Heterogeneity: Firm Gender Pay Gap

This table reports the effects of gender pay gap disclosure on employee wages, depending on the firm pre-treatment level of the gender pay gap. *pre_gap* is an indicator that takes values from 1 to 10 for the lowest and highest deciles, respectively, of the firm-level pre-treatment "unexplained" gender pay gap. Each column presents coefficients from a different multivariate regression. Column 1 (2) is estimated using only male (female) employees. Columns 3 and 4 use the entire sample. The dependent variable is employee annual wage (log transformed). Treated is a dummy that takes the value of 1 for individuals working in firms with 35-50 employees before the introduction of the law, and 0 for individuals in firms with 20-34 employees. Post takes a value of 1 for 2006, 2007, and 2008, and a value of 0 for years 2003, 2004, and 2005. Detailed descriptions of the variables are given in Table A1. Standard errors are double clustered at the individual and the firm level and reported in parentheses. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Male	Female	All	All
	(1)	(2)	(3)	(4)
Treated x Post	0.0012	-0.0010	-0.0012	
	(0.0129)	(0.0144)	(0.0144)	
Post x *pre_gap*	0.0011	0.0041***	0.0041***	
	(0.0014)	(0.0015)	(0.0015)	
Male x Post			0.0176*	0.0111
			(0.0100)	(0.0094)
Treated x Post x Male			0.0026	0.0216
			(0.0171)	(0.0153)
Male x Post x *pre_gap*			-0.0030	-0.0032*
			(0.0019)	(0.0018)
Treated x Post x *pre_gap*	-0.0038*	-0.0006	-0.0006	
	(0.0022)	(0.0024)	(0.0024)	
Treated x Post x Male x *pre_gap*			-0.0033	-0.0053**
			(0.0030)	(0.0027)
Person x Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No
Firm x Year FE	No	No	No	Yes
R^2	0.877	0.829	0.869	0.885
N	69,035	44,655	113,690	113,499

Appendix Table A9: Diff-in-Diff Heterogeneity: The Effect of Having Children

This table reports the effects of gender pay gap disclosure on employee wages depending on whether they have children. Have Children is an indicator variable that takes the value of 1 if an individual has children, and 0 if otherwise. Each column presents coefficients from a different multivariate regression. Column 1 (2) is estimated using only male (female) employees. Columns 3 and 4 use the entire sample. The dependent variable is employee annual wage (log transformed). Treated is a dummy that takes the value of 1 for individuals working in firms with 35-50 employees before the introduction of the law, and 0 for individuals in firms with 20-34 employees. Post takes a value of 1 for 2006, 2007, and 2008, and 0 for years 2003, 2004, and 2005. Detailed descriptions of the variables are given in Table A1. Standard errors are double clustered at the individual and the firm level and reported in parentheses. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Male	Female	All	All
	(1)	(2)	(3)	(4)
Treated x Post	-0.0324***	0.0128	0.0129	
	(0.0085)	(0.0116)	(0.0116)	
Post x Have Children	-0.1105***	-0.0476***	-0.0469***	-0.0342***
	(0.0054)	(0.0078)	(0.0078)	(0.0077)
Treated x Post x Have Children	0.0233***	-0.0126	-0.0127	-0.0120
	(0.0085)	(0.0121)	(0.0120)	(0.0112)
Male x Post			0.0428***	0.0404 ***
			(0.0085)	(0.0086)
Treated x Post x Male			-0.0453***	-0.0399***
			(0.0134)	(0.0127)
Male x Post x Have Children			-0.0638***	-0.0620***
			(0.0091)	(0.0091)
Treated x Post x Male x Have Children			0.0359**	0.0339**
			(0.0141)	(0.0134)
Person-Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No
Firm x Year FE	No	No	No	Yes
R^2	0.870	0.827	0.867	0.885
N	145,852	79,532	225,384	223,624

II Law Details

This section provides more information regarding the law. We include a timeline describing the law introduction and discuss the law implementation and compliance. We gathered information from several sources, including the original law, news articles, an evaluation survey conducted by the Ministry of Employment, and a major employer organization. Finally, we verified the information with a Danish law firm that specializes in labor law.

II.1 Timeline

Below, we present a detailed timeline of the law introduction:

- January 2005: A snap election was announced on January 18, 2005, one year earlier than scheduled, and took place on February 8, 2005. Although the governing party was regarded as the front-runner, uncertainty existed regarding the reelection. The day before the election, the two main polls predicted reelection with 52.4% and 52.9% of the votes, respectively.
- February 2005: During the campaign for the snap election, the leading governing party, Venstre, promised to reduce the gender pay gap by introducing a law that would require firms with more than 35 employees to produce gender-disaggregated wage statistics.
- October 2005: The re-elected government announced it would introduce a bill to amend the Equal Pay Act.
- December 2005: Prime Minister Anders Fogh Rasmussen presented at his weekly press conference the government's bill on gender-disaggregated wage statistics and also introduced the bill to the Parliament.
- June 2006: The proposal was voted and adopted on June 9, 2006. The proposal

passed by a majority of 24 that voted it through (out of 179 members of parliament).

- January 2007: The new provisions came into force. The first gender-based wage statistic produced by companies should cover 12 months starting no later than January 1st, 2007.

II.2 Implementation

The initial announcement related to the legislation took place during the electoral campaign in February 2005 and was widely reported by news agencies.[26]. Although this announcement emphasized only the 35-employee cutoff, the actual law stated that an employer with a minimum of 35 employees and at least 10 employees of each gender within an occupation classification code (six-digit DISCO code) must 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. Firms needed to disclose once per year for every year they met the requirements of the law. The law did not specify the exact wage measure the firm had to construct (but it specified it had to be at the occupation and gender level). The firm could decide what wage statistic to compute but had to explain in a report how the wage statistic was constructed and had to make sure the wage statistic was transparent to "allow comparison of women's and men's wages in a meaningful way". Moreover, firms needed to consistently use the same wage statistic throughout their report to allow comparability.

To obtain information on the wage statistic that firms more commonly used in practice as well as the way firms implemented the policy, we gathered information from two sources: the Danish Employer Confederation (DA) and an evaluation study that the Ministry for Employment conducted (SFI report, Holt and Larsen, 2011) in 2009, three years after the law passage.[27] The SFI evaluation report was based on a questionnaire

[26]e.g., Politiken (2005); Bureau (2005).

[27]The Ministry for Employment commissioned The Danish National Centre for Social Research (SFI)

survey sent to management and employee representatives in 740 companies that, according to Statistics Denmark, were covered by the law.[28] We also reached out to one of Denmark's largest employer associations, the Danish Employer Confederation (DA), in order to obtain more information on what statistic firms more commonly constructed. According to these sources, the most common statistics were the percentage differences in wages and mean wages for men and women in the same occupation.

The statistics had to be provided by gender and occupation. However, companies expressed in the SFI evaluation report "that the DISCO codes are difficult to work with". To get around the issue that the 10-10 requirement may not be satisfied, one survey respondent mentioned that firms tend to design their own groups: "What they [HR] do is that, for example, suppose there is a large IT department with 30 people -22 men and 8 women. Since there is not 10 of each, they are grouped with another department (or similar)." Another firm mentioned it reported each individual's wage statistics (i.e, not following the DISCO group recommendation).

The gender wage statistics are typically made available to both management and employee representatives. The management and the employee representatives are then responsible for informing the employees and giving them the opportunity to express their views on the statistics. Most companies informed representatives and employees through the collaboration committee meeting between leaders and employees (Samarbejds Udvalg/ Medbestemmelse og MEdindflydels) or the HR department.

Finally, in terms of enforcement, according to the law, "If a company does not meet the legal requirements, the employer can be punishable by a fine. Anyone will be in the position to report to the police if one thinks that an employer does not comply with the law. The police are then responsible to investigate and determine if there are

to conduct an evaluation of the law.

[28]The collection of data consisted of an online form with phone follow-up. The response rate was 61 percent.

grounds to press charges". To the best of our knowledge, no company has been fined.[29] Nevertheless, according to newspaper articles, employer organizations and their legal advisors were threatening with lawsuits if firms did not comply with the law (Politiken, 2007).

[29]However, a fine is only made public if a business does not accept it, in which case the matter will be settled in court.

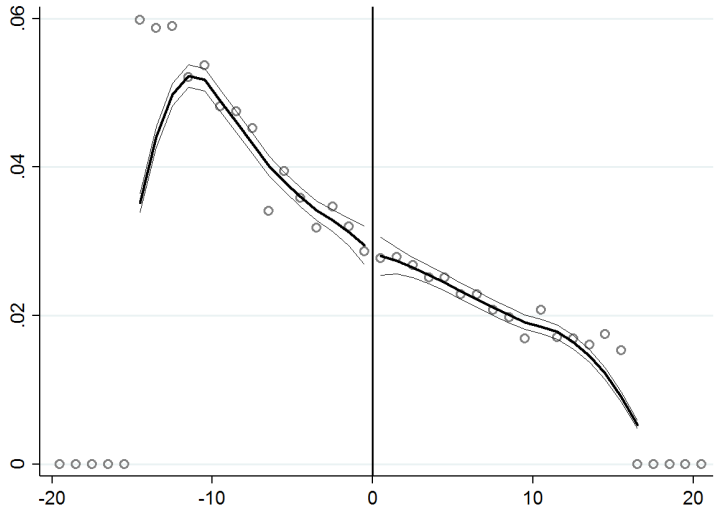
Internet Appendix to

"Do Firms Respond to Gender Pay Gap Disclosure?"

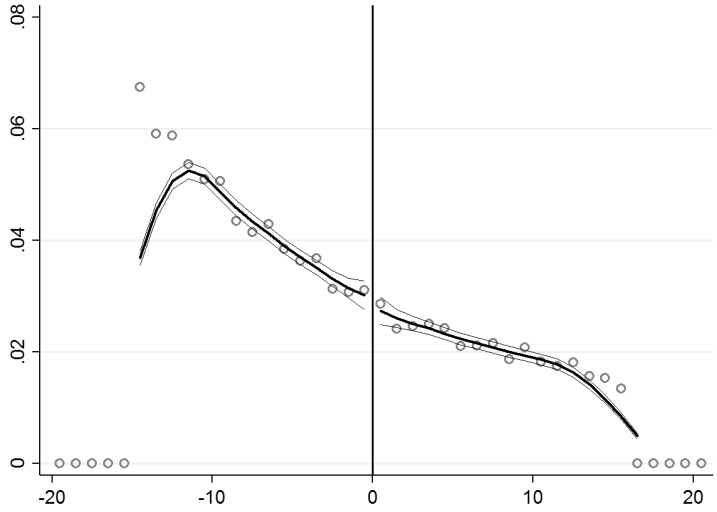
Morten Bennedsen, Elena Simintzi, Margarita Tsoutsoura, and Daniel Wolfenzon

Figure IA1: McCrary Plots before and after the Law

The plots present results of the McCrary (2008) test, which tests for the continuity in the densities of the running variable at the 35-employee cutoff. Sub-figure (a) presents the plot for years 2003-2005, and sub-figure (b) for years 2006-2008.



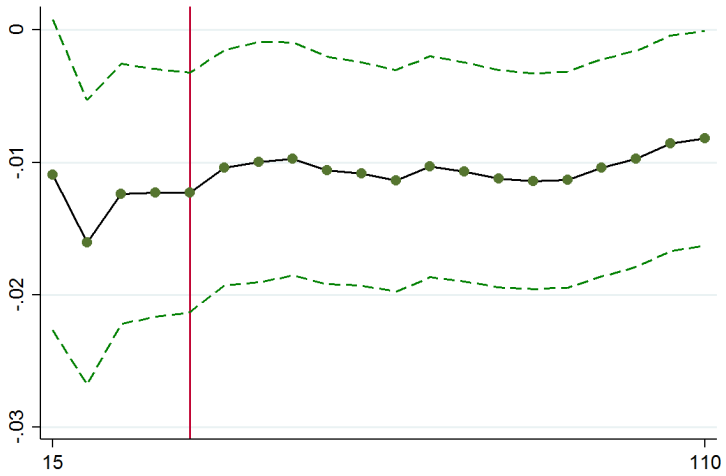
(a) McCrary 2003 - 2005



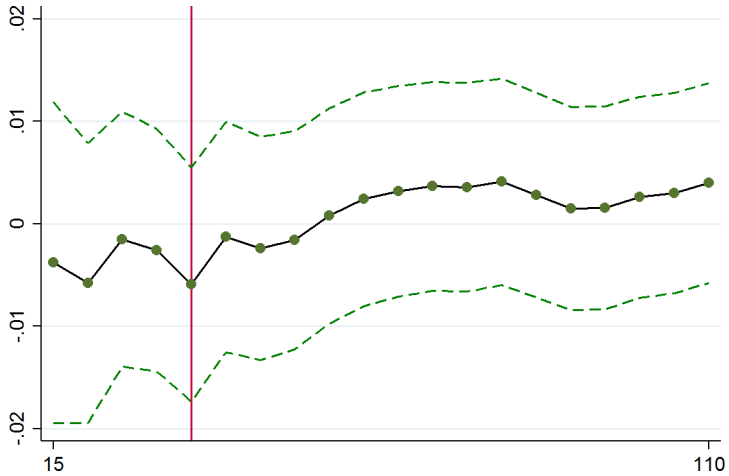
(b) McCrary 2006 - 2008

Figure IA2: Diff-in-Disc Estimates by Bandwidth

We repeat the main diff-in-disc estimates of Table 5 for bandwidths 15-110 for males (a) and females (b) separately. The solid line plots the estimated coefficients, and the dotted lines represent the 95% confidence interval.



(a) RDD Pre Post Bandwidth Stabilization Male



(b) RDD Pre Post Bandwidth Stabilization Female

Table IA1: Diff-in-Diff Sample: Pre-Treatment Industry Distribution

This table reports the industry composition of our diff-in-diff sample based on the unique number of firms in 2005 (one year prior to the law passage). We classify firms into 9 industries according to the Statistics Denmark industry classification code: 1. Agriculture, fishing, quarrying; 2. Manufacturing; 3. Electricity, gas and water supply; 4. Construction; 5. Wholesale and retail trade, hotels, restaurants; 6. Transport, post and telecommunications; 7. Finance and business activities; 8. Public and personal services; 9. Activity not stated.

	All		Treated		Control	
9-Cat Industry	No. of Firms	%	No. of Firms	%	No. of Firms	%
	(1)	(2)	(3)	(4)	(5)	(6)
2: Manufacturing	1,008	28.83	374	32.13	634	27.19
4: Construction	423	12.10	143	12.29	280	12.01
5: Retail	1,374	39.30	427	36.68	947	40.61
6: Telecom	186	5.32	71	6.10	115	4.93
7: Finance	505	14.45	149	12.80	356	15.27
Total	3,496	100	1,164	100	2,332	100

Table IA2: Diff-in-Diff: Sample Restriction to Control for Employee Composition Changes

This table repeats Table 3, except the sample includes only observations in which the employee works at the same firm as he did in 2005. Standard errors are double clustered at the individual and the firm level and reported in parentheses. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Male	Female	All	All	Male	Female	All	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated x Post	-0.0152***	0.0024	0.0024		-0.0146***	0.0031	0.0031	
	(0.0039)	(0.0045)	(0.0045)		(0.0038)	(0.0044)	(0.0044)	
Male x Post			-0.0029	-0.0053			-0.0038	-0.0057
			(0.0035)	(0.0036)			(0.0034)	(0.0035)
Treated x Post x Male			-0.0176***	-0.0131**			-0.0178***	-0.0129**
			(0.0052)	(0.0052)			(0.0052)	(0.0052)
Person x Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Firm x Year FE	No	No	No	Yes	No	No	No	Yes
Individual Controls	No	No	No	No	Yes	Yes	Yes	Yes
Firm Controls	No	No	No	No	Yes	Yes	Yes	No
R^2	0.871	0.828	0.869	0.889	0.870	0.827	0.869	0.888
N	110,034	58,672	168,706	166,865	109,747	58,398	168,145	166,307

Table IA3: Diff-in-Diff: Industry Treatment Intensity

This table reports whether gender pay gap disclosure has spillover effects on control firms. We measure whether control firms operate in industries with high treatment intensity. We classify industries by the percentage of employees working on firms with more than 35 employees pre-treatment. We define Industry Treatment Intensity to be 1 if the firm operates in an industry where the percentage of employees treated in the industry is above the sample median, and 0 otherwise. The sample includes only control firms, those firms with 20-34 employees. The dependent variable is wages (log-transformed). Columns 1 and 5 (2 and 6) are estimated using only male (female) employees. All other columns use the entire sample. Post takes a value of 1 for 2006, 2007, and 2008, and 0 for years 2003, 2004, and 2005. Individual controls include employee work experience and age. Firm controls include sales (log-transformed). We classify firms into 27 industries according to the Statistics Denmark industry classification code. Standard errors are double clustered at the individual and the firm level and reported in parentheses. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Male	Female	All	All	Male	Female	All	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Industry Treatment Intensity x Post	-0.0375*	-0.0388*	-0.0377		-0.0421**	-0.0395*	-0.0370*	
	(0.0202)	(0.0235)	(0.0234)		(0.0177)	(0.0227)	(0.0225)	
Male x Post			-0.0024	0.0000			0.0004	0.0015
			(0.0173)	(0.0172)			(0.0164)	(0.0168)
Industry Treatment Intensity x Male			0.0591	-0.0166			0.0316	-0.0376
			(0.0695)	(0.0719)			(0.0649)	(0.0723)
Industry Treatment Intensity x Post x Male			0.0001	-0.0070			-0.0049	-0.0094
			(0.0293)	(0.0287)			(0.0276)	(0.0281)
Person x Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Firm x Year FE	No	No	No	Yes	No	No	No	Yes
Individual Controls	No	No	No	No	Yes	Yes	Yes	Yes
Firm Controls	No	No	No	No	Yes	Yes	Yes	No
R^2	0.860	0.827	0.860	0.881	0.865	0.829	0.863	0.883
N	81,755	44,773	126,528	125,186	81,399	44,468	125,867	124,526

Table IA4: Diff-in-Diff: Employee Hours Worked and Hourly Wages

This table repeats Table 3, but uses employee hours worked (log-transformed) in Panel A, and hourly wages (log-transformed) in Panel B, as the dependent variables. The measure of hourly wages comes from a mandated pension scheme—Arbejdsmarkedets Tillaegspension (ATP)—that requires all employers to contribute on behalf of their employees based on individual hours worked. Standard errors are double clustered at the individual and the firm level and reported in parentheses. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Employee Hours Worked (log-transformed)

	Male	Female	All	All	Male	Female	All	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated x Post	-0.0167**	-0.0215**	-0.0215**		-0.0148*	-0.0206**	-0.0206**	
	(0.0078)	(0.0094)	(0.0094)		(0.0077)	(0.0093)	(0.0093)	
Male x Post			-0.0095	-0.0027			-0.0102	-0.0026
			(0.0071)	(0.0041)			(0.0071)	(0.0041)
Treated x Post x Male			0.0049	0.0052			0.0058	0.0054
			(0.0093)	(0.0063)			(0.0092)	(0.0062)
Person-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Firm x Year FE	No	No	No	Yes	No	No	No	Yes
Individual Controls	No	No	No	No	Yes	Yes	Yes	Yes
Firm Controls	No	No	No	No	Yes	Yes	Yes	No
R^2	0.419	0.490	0.455	0.640	0.421	0.492	0.456	0.641
N	145,855	79,532	225,387	223,627	145,265	79,027	224,292	222,532

Panel B: Employee Hourly Wages (log-transformed)

	Male	Female	All	All	Male	Female	All	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated x Post	-0.0116***	0.0018	0.0018		-0.0106***	0.0023	0.0023	
	(0.0030)	(0.0033)	(0.0033)		(0.0030)	(0.0033)	(0.0033)	
Male x Post			0.0080***	0.0092***			0.0074***	0.0090***
			(0.0026)	(0.0025)			(0.0026)	(0.0025)
Treated x Post x Male			-0.0133***	-0.0126***			-0.0130***	-0.0125***
			(0.0039)	(0.0038)			(0.0039)	(0.0038)
Person x Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Firm x Year FE	No	No	No	Yes	No	No	No	Yes
Individual Controls	No	No	No	No	Yes	Yes	Yes	Yes
Firm Controls	No	No	No	No	Yes	Yes	Yes	No
R^2	0.912	0.892	0.913	0.931	0.912	0.893	0.913	0.931
N	145,720	79,465	225,185	223,424	145,131	78,963	224,094	222,333

Table IA5: Diff-in-Diff: Employee Total Compensation

This table repeats Table 3 for two subsets of employees: those who received bonus compensation at least one year in our sample period (Panel A) and those who never received a bonus in our sample period (Panel B). In Panel A, the dependent variable is employee total compensation, wages, and bonuses (log-transformed). In Panel B, the dependent variable is employee wages. The employee bonus variable includes irregular payments including bonus, grants, commissions, etc. Standard errors are double clustered at the individual and the firm level and reported in parentheses. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Ever Received Bonus

	Male	Female	All	All	Male	Female	All	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated x Post	-0.0128***	0.0009	0.0008		-0.0099**	0.0032	0.0035	
	(0.0045)	(0.0054)	(0.0054)		(0.0041)	(0.0051)	(0.0050)	
Male x Post			-0.0004	-0.0014			0.0003	-0.0005
			(0.0041)	(0.0042)			(0.0039)	(0.0040)
Treated x Post x Male			-0.0137**	-0.0125**			-0.0137**	-0.0129**
			(0.0061)	(0.0060)			(0.0058)	(0.0058)
Person-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Firm x Year FE	No	No	No	Yes	No	No	No	Yes
Individual Controls	No	No	No	No	Yes	Yes	Yes	Yes
Firm Controls	No	No	No	No	Yes	Yes	Yes	No
R^2	0.867	0.834	0.868	0.890	0.872	0.838	0.872	0.893
N	104,484	54,298	158,782	156,175	104,077	53,948	158,025	155,424

Panel B: Never Received Bonus

	Male	Female	All	All	Male	Female	All	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated x Post	-0.0224***	0.0032	0.0034		-0.0189***	0.0031	0.0031	
	(0.0071)	(0.0083)	(0.0082)		(0.0063)	(0.0078)	(0.0077)	
Male x Post			-0.0051	-0.0090			-0.0074	-0.0110
			(0.0068)	(0.0075)			(0.0065)	(0.0071)
Treated x Post x Male			-0.0257**	-0.0158			-0.0223**	-0.0127
			(0.0101)	(0.0109)			(0.0095)	(0.0104)
Person-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Firm x Year FE	No	No	No	Yes	No	No	No	Yes
Individual Controls	No	No	No	No	Yes	Yes	Yes	Yes
Firm Controls	No	No	No	No	Yes	Yes	Yes	No
R^2	0.861	0.811	0.855	0.887	0.864	0.812	0.858	0.888
N	48,524	28,773	77,297	74,102	48,323	28,583	76,906	73,732

Table IA6: Diff-in-Disc Robustness: T-1 and T-2

We repeat the main diff-in-disc estimates of Table 5 in columns 1-2. In columns 3-4, we instead define employment based on years T-2. Standard errors are double clustered at the individual and the firm level and reported in parentheses. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	T-1		T-2	
	Male	Female	Male	Female
	(1)	(2)	(3)	(4)
Diff-in-Disc	-0.0114***	0.0032	-0.0133***	-0.0010
	(0.0043)	(0.0052)	(0.0046)	(0.0054)
Firm FE	Yes	Yes	Yes	Yes
R^2	0.850	0.803	0.850	0.805
N	328,659	191,461	330,433	190,904

Table IA7: Covariate Balance

We estimate an RDD model with individual (panel A) and firm (panel B) characteristics as outcomes. The model fits a different third-degree polynomial on employment at each side of the discontinuity. Data are from the period before the implementation of the law (2003-2005). We include firms with 1-100 employees (i.e., bandwidth of 65). Employee number is defined as of year T-1. Standard errors are double clustered at firm and employee levels for individual covariates in Panel A and clustered at the firm level for firm covariates in Panel B. Standard errors are reported in parentheses. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Panel A: Individual-Level Covariate Balance				
	Age	Education	Gender	Work Experience	Has Child
	(1)	(2)	(3)	(4)	(5)
Over 35 Employees	-0.3174	-0.0152	0.0044	-0.2452	-0.0330
	(0.3697)	(0.0193)	(0.0161)	(0.3685)	(0.0292)
Year FE	Yes	Yes	Yes	Yes	Yes
R^2	0.001	0.002	0.001	0.001	0.001
N	290,854	285,349	291,472	291,638	291,639

	Panel B: Firm-Level Covariate Balance			
	Profits	Female Proportion	Assets	Sales
	(1)	(2)	(3)	(4)
Over 35 Employees	220.4412	-0.0157	2372.3869	-783.3038
	(289.8334)	(0.0160)	(2212.7116)	(1145.1933)
R^2	0.000	0.016	0.014	0.122
N	22,777	23,460	23,460	23,460

Table IA8: Diff-in-Diff: Wages of Joiners and Leavers

The table reports the effects of gender pay gap disclosure on whether high- or low-paid employees leave and join the firm. Each column presents coefficients from a different multivariate regression. Columns 1 and 4 (2 and 5) are estimated using only male (female) employees. Columns 3 and 6 use the entire sample. The dependent variable in columns 1-3 is the employee annual wage (log transformed) the year before leaving the firm (T-1). The dependent variable in columns 4-6 is the employee annual wage (log transformed) the year after joining the firm (T+1). Treated is a dummy that takes the value of 1 for individuals working in firms with 35-50 employees before the introduction of the law, and 0 for individuals in firms with 20-34 employees. Post takes a value of 1 for 2006, 2007, and 2008 and 0 for years 2003, 2004, and 2005. Standard errors are double clustered at the firm level and reported in parentheses. ***, **, and * correspond to statistical significance at the 1%, 5%, and 10% levels, respectively.

	Leavers			Joiners		
	Male	Female	All	Male	Female	All
	(1)	(2)	(3)	(4)	(5)	(6)
Post	0.0353***	0.0409***	0.0409***	0.0432***	0.0690***	0.0690***
	(0.0114)	(0.0131)	(0.0131)	(0.0118)	(0.0123)	(0.0123)
Treated	0.0371**	0.0095	0.0095	0.0398**	0.0247	0.0247
	(0.0160)	(0.0177)	(0.0177)	(0.0164)	(0.0176)	(0.0176)
Treated x Post	-0.0058	-0.0049	-0.0049	-0.0206	-0.0337*	-0.0337*
	(0.0191)	(0.0214)	(0.0214)	(0.0180)	(0.0197)	(0.0197)
Male			0.2246***			0.2045***
			(0.0118)			(0.0116)
Post x Male			-0.0055			-0.0258*
			(0.0152)			(0.0147)
Male x Treated			0.0276			0.0151
			(0.0191)			(0.0190)
Treated x Post x Male			-0.0009			0.0131
			(0.0248)			(0.0219)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.003	0.002	0.067	0.004	0.007	0.070
N	17,933	10,082	28,015	16,515	10,072	26,587