Pay Transparency and Cracks in the Glass Ceiling*

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Abstract

Each year since 2018, more than 10,000 UK firms have to publicly disclose their gender pay gap and gender wage distributions. This paper studies how this transparency policy affects the occupational outcomes and wages of male and female workers. Theoretically, pay transparency represents an information shock that alters the bargaining power of male and female employees vis-à-vis the firm in an asymmetric way. As women are currently underpaid, this shock may improve women's relative outcomes. We test these theoretical predictions using a difference-in-difference strategy that exploits the variation in the UK mandate across firm size and time. Our results show that pay transparency increases the probability that women are hired in above-median-wage occupations by 5 percent compared to the pre-policy mean. Additionally, it leads to a 2 percent decrease in male real hourly pay in treated firms compared to control ones. Combining the difference-in-difference strategy with a text analysis of job listings, we also find suggestive evidence that treated firms in industries with a high gender pay gap become more likely to post wage information than control ones.

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1 Introduction

The 4th of April 2018 was the first deadline for more than 10,000 UK firms to publish statistics on their gender pay gaps. Up until that time, less than 3 percent of UK firms had ever publicly disclosed this information (Downing et al. 2015). The following day, all national British newspapers commented on the figures. The second deadline fell in April 2019, and again drew significant media attention (*Financial Times* 2019, *BBC* 2018, *The Guardian* 2018, *Financial Times* 2018).[1]

While the UK is the only country in which some companies are required to publish their gender pay gaps publicly, many countries are adopting pay transparency policies. All have the declared objective of reducing the gender pay gap.[2] The argument for these initiatives goes as follows: pay transparency is an information shock that asymmetrically alters the bargaining power of male and female employees vis-à-vis the firm because women are paid less on average (Cullen and Perez-Truglia 2018b). Due to the potential negative effects on firm reputation, the shock also incentivizes targeted firms to hire more women in better paid positions, and discourages the promotion of male employees. In turn, this could translate into improved outcomes for women relative to men.

This paper tests these theoretical predictions in the UK setting. The British government passed the *Equality Act 2010 (Gender Pay Gap Information) Regulations 2017* in February 2017. The act mandates that all firms registered in Great Britain with at least 250 employees have to publish, on a dedicated government website, a series of indicators that include percentage mean and median gender hourly pay differentials, and the gender composition along the wage distribution. If a firm has at least 250 employees by the end of each financial year (April), it will have to provide these figures by the end of the following financial year. According to the government, all firms that were required to comply with the law did so during its first two years of operation.

To identify the impact of this policy on wages and occupational outcomes of male and female

[1] In mid March 2020, just two weeks before the publication deadline, due to the COVID-19 outbreak, the requirement to publish gender pay gap indicators was removed. By that point only half of the targeted companies had published their data (*Financial Times* 2020).

[2] Following the recommendations of the European Commission, Denmark, Italy, France, and Germany introduced transparency laws, for instance.

workers, we use a difference-in-difference strategy that exploits the variation across firm size and over time in the application of the government mandate. To avoid capturing any potential impact of this policy on firm size, we define the treatment status based on the firms' number of employees prior to the introduction of the mandate. To enhance comparability, we restrict the sample to firms with +/-50 employees from the 250 threshold.

To conduct this analysis, we use the Annual Survey of Hours and Earnings (ASHE) from 2012 to 2019. This is an annual employer survey covering 1 percent of the UK workforce, and designed to be representative of the employee population. Crucially for us, it provides information on gender, number of employees, firm and individual identifiers, wage components, hours worked, tenure, occupation, and industry.

Our analysis delivers two key findings. First, the mandate changes the occupational composition of the pool of female employees in treated firms compared to control ones, by increasing the probability that women work in above-median-wage occupations by 5 percent relative to the pre-policy mean. As this effect comes from newly hired women, so far it has failed to translate into a visible increase in women's salaries. However, our second finding is that pay transparency leads to pay compression from above: the mandate leads to a 2 percent decrease in male real hourly wages in treated firms relative to control ones following its introduction.

A series of event study exercises show that these results do not capture pre-policy differential trends in the outcomes of interest between treated and control groups. With additional robustness checks, we exclude that our estimates capture the impact of time shocks affecting firms above and below the 250 threshold differently. First, our estimates are unchanged in triple-difference regressions that account for within-group time shocks common to male and female employees. Second, local difference-in-discontinuity specifications that control for firm-size specific time shocks deliver the same results as our difference-in-difference model. Third, we estimate placebo regressions, pretending that the policy binds at different firm size thresholds, and find no significant effect of placebo policies. Finally, we check that our estimates are not sensitive to the choice of the estimation sample around the 250 cutoff, and that they are robust to the year consider to define treatment status.

To delve into the mechanisms driving the estimated effects, we follow two main directions. First, to understand how treated firms may have been able to attract more women, we turn to analyze their hiring strategies, exploiting a unique data set compiled by Burning Glass Techonologies (BGT hereafter) and collecting online job listings from 2012 onward for the entire UK territory. Many studies document a gender gap in bargaining skills in favor of men (Leibbrandt and List 2015, Bowles et al. 2007, Babcock et al. 2003.). Accordingly, the gender pay gap is also larger in jobs where negotiation rules are unclear. In BGT less than 30 percent of vacancies contain information on wages. In light of these stylized facts, we test whether treated firms change wage posting decisions following the introduction of the pay transparency policy, and find suggestive evidence that firms belonging to industries with a high gender pay gap increase the probability of posting wages by one third, relative to the control group.

Second, we study the role played by reputation in triggering firms' response to the pay transparency policy. To this aim, we focus on the reaction of the stock market following the publication of gender pay gap indicators by firms listed on the London Stock Exchange - representing around 10 percent of firms targeted by the mandate and one third of listed firms. This analysis indicates that, in the first year of the mandate, firms' cumulative abnormal returns decrease by up to 65 basis points in the aftermath of the publication. While this effect fades away after five days from the publication, it shows that firms publishing gender pay gap indicators are under the scrutiny of investors. In turn, this suggests that the reputation motive may be a potential channel to explain why firms reacted to the policy.

Finally, we also check whether this policy affects employees' retention. Recent evidence shows that disclosing information on peers' salaries may hurt job satisfaction and increase job search intentions of low-paid employees paid (Perez-Truglia 2020, Dube et al. 2019, Breza et al. 2018, Cullen and Perez-Truglia 2018a, Card et al. 2012). In the UK, publishing gender pay gap indicators may discourage female employees, and be perceived as a threat by male employees. As such, it could weaken employees' satisfaction and increase quitting. At the same time, if firms respond by promoting gender equality, a more egalitarian environment may increase employees' retention. Based on our estimates, at least in the short run, none of these two forces seem to prevail.

Overall, this paper provides several contributions to different strands of literature. First, it adds to the growing number of studies from the economic and management literature analyzing the impact of pay transparency policies on personnel management decisions and the gender pay gap (Gulyas et al. 2020, Baker et al. 2019, Bennedsen et al. 2019, Burn and Kettler 2019, Mas 2017). The closest studies to ours are Gulyas et al. (2020), Baker et al. (2019), and Bennedsen et al. (2019). Baker et al. (2019) studies the effect on the gender pay gap of a Canadian law imposing that public sector organizations publish employees' salaries above a certain pay threshold, while Gulyas et al. (2020) and Bennedsen et al. (2019) analyze the impact on the gender pay gap of a 2011 Austrian law and a 2006 Danish law, respectively, mandating private firms to provide employees' representative pay measures by gender and occupation. Both Baker et al. (2019) and Bennedsen et al. (2019) find that transparency leads to pay compression from above, while Gulyas et al. (2020) find no impact on individual wages and the gender pay gap. Relative to these studies, the UK mandate has two unique features that could help us improve our understanding of the effects of pay transparency. First, it requires the publication of the percentage gender pay gap, rather than pay levels by gender. In the latter case, both male and female workers' bargaining power may increase, as all employees acquire information on gender differentials, but also on one's own gender pay. In contrast, in the UK, this second channel is shut down. Second, the public disclosure of the information, coupled with extensive media attention, magnifies the information shock and stimulates behavioral responses. Finally, studying the UK context helps us understand whether the findings for Denmark and Canada may extend to less egalitarian countries, such as the UK (OECD 2019).

More broadly, our study contributes to the analysis of policies aimed at tackling the gender pay gap. As policies such as gender quotas and paternity leave have been proven to have a negligible impact so far, it seems especially important to assess the role of other interventions such as pay transparency (Antecol et al. 2018, Wasserman 2019, Bertrand et al. 2019, Ekberg et al. 2013).

The paper proceeds as follows. Section 2 describes the institutional setting and the UK transparency policy. Section 3 discusses the identification strategy. Section 4 describes the different sources of data used in the empirical analysis. Section 5 illustrates the main results. Section 6 reports the results of a battery of robustness checks. Section 7 discusses the potential mechanisms behind the main results. Section 8 concludes.

2 Institutional setting

In 2015, the UK government launched a process of consultations with employers to enhance pay transparency. At that time, the average gender pay gap for all employees in the UK stood at 19.1 percent. Moreover, women made up only 34 percent of managers, directors, and senior officials (Government Equalities Office 2015). According to the government's view, "greater transparency will encourage employers and employees to consider what more can be done to close any pay gaps. Moreover, employers with a positive story to tell will attract the best talent" (Government Equalities Office 2015).

In February 2017, this process resulted in the passing of the *Equality Act 2010 (Gender Pay Gap Information) Regulations 2017*. This mandate imposes that all firms registered in Great Britain that have at least 250 employees should publish gender pay gap indicators both on their own website and on a dedicated website managed by the Government Equalities Office (GEO hereafter).[3],[4]

The timing of publication works as follows: if a firm has at least 250 employees by the end of the current financial year (April), it will have to provide gender pay indicators by the end of

[3]This legislation does not apply to Northern Ireland.

[4]The mandate applies to both private and public sector firms. Yet, note that public sector firms were already subject to some transparency measures. According to regulations introduced in 2011, public bodies in England with over 150 employees were required to publish information annually on the diversity of their workforce, though no gender pay gap information. The Welsh regulations, also introduced in 2011, require public bodies to publish the number of men and women employees broken down by pay. Public authorities are also required to make arrangements for identifying and collecting (but not necessarily publishing) information about differences between pay of people by protected characteristics, such as gender or ethnicity. Where any difference can be linked to a protected characteristic, public authorities should set equality objectives to address the causes of any differences. Finally, only Scottish public organisations with 20 or more employees were required since 2012 to publish information on the gender pay gap.

the following financial year. Firms must themselves calculate their number of employees under detailed guidelines provided by the government. Importantly, they have to adopt an extended definition of employee, which includes agency workers. Partners of firms are also included in the definition of employees, but should not enter in the calculation of the indicators. Finally, part-time workers have the same weight as full-time ones in the calculations.

The indicators that firms have to report include: the overall mean and median gender hourly pay gap, expressed in percentage terms; the overall mean and median gender bonus gap; the proportion of male and female employees who receive any bonus pay; the proportion of male and female employees in each quartile of the company wage distribution. Table 1 provides sample means of these indicators for the two years firms had to publish them so far. The mean gender pay gap is just below 15 percent and decreases by 1 percent between 2017/2018 and 2018/2019. The median gender gap is smaller in both years and slightly increases over time, suggesting that the decrease in the mean gap is driven by a drop in extreme values. Both the mean and the bonus gap are smaller but it is worth noting from the standard deviation that some firms mistakenly reported the level gap rather than a percentage, making it difficult to interpret these mean values.[5] The share of women receiving bonus pay is smaller than that of men in both years, and the ratio remains stable over time. The gender ratio along the wage distribution is balanced at the bottom, but the share of women shrinks in the upper part of the distribution. Yet, it increases by around 1 percent over the two years. Finally, figure 1 also shows that the mean gender hourly pay gap is larger in firms that have a lower share of women at the top of the wage distribution. From now on, we will refer to these data as the GEO data.

Three other features of this policy are important to understand in the UK context. First, the policy does not impose sanctions on firms that do not improve their gender pay gap over time. However, the Equality and Human Rights Commission, the enforcement body responsible for this regulation, can issue court orders and unlimited fines for firms that do not comply with the regulations that mandate disclosure of pay gaps. Up to now, all firms deemed to comply have

[5] When excluding the bottom and top 1 percent, the mean bonus gap stands at 23.22 in 2017/18 and 23.76 in the second year.

published their gender pay gap indicators. Figure 2 reports the distribution of submission dates for the two years the mandate has been in place. While some firms visibly do not meet the deadline, the majority publish their data in the last month before deadline.

Second, this policy is likely to represent an information shock both inside and outside the firm. According to a survey addressed on behalf of GEO, out of 855 private and non-profit firms with at least 150 employees, only one third of firms have ever computed their gender pay gap, and just 3 percent have made these figures publicly available. Moreover, up to 13 percent declared that staff are discouraged from talking about it and 3 percent reported that their contracts include a clause on pay secrecy (Downing et al. 2015).

Finally, this policy is salient. Not only are the figures publicly available on a government website, but, as noted in the introduction, they also receive extensive media attention each year that they are published. Importantly, figure 3 shows that google searches for the term gender pay gap also spike around each year's deadline, indicating that this policy has attracted significant public interest.

3 Identification strategy

To identify the impact of the 2017 transparency policy on wages, occupational outcomes and firm-level outcomes, we exploit the variation across firm size and over time in its implementation. Specifically, we estimate a difference-in-difference model that compares the evolution of the outcomes of interest in firms whose size is slightly larger (treated group) or smaller (control) than the 250-employee cutoff. As firm size can be endogenously determined, we define treatment status based on firm size in 2015, prior to the start of the consultation process to implement the mandate. To enhance comparability between treatment and control group, in the main specification we consider firms with $+/-50$ employees from the 250 threshold.[6] When studying employees' outcomes, our baseline regression model is as follows:

[6] In the next section, we show that our results are robust both to the use of a different year to define the treatment status, and to the bandwidth chosen to construct the estimation sample.

$$Y_{ijt} \quad = \quad \alpha_j + \theta_t + \beta_0 TreatedFirm_j * Post_t + X'_{it}\pi + Z'_{jt}\delta + u_{ijt} \tag{1}$$

where i is an employee working in firm j, having 200-300 employees, in year t, with t comprised between 2012 to 2019.[7] The outcome Y_{ijt} is either a measure of occupation held, job mobility, pay (hourly or weekly wages, bonuses or allowances), or hours worked. As for the regressors, α_j are firm fixed effects that capture the impact of firm-specific time-invariant characteristics such as industry, or firm culture.[8] θ_t are year fixed effects that control for time shocks common to all firms such as electoral cycles. $TreatedFirm_j$ is a dummy equal to one if a firm has at least 250 employees in 2015, and $Post_t$ is a dummy equal to one from 2017 onward. The vector X_{it} includes individual controls. In regressions analyzing how the policy affects the composition of firms' workforce, individual controls are limited to age and age squared. When considering wages, we control for individual fixed effects to take into account compositional effects. In what follows, we also compare the results of specifications where the vector Z_{jt} contains different time-varying firm-level controls, such as region-specific time shocks, industry linear trends, or measures of product-market concentration, such as interaction terms between the 2011 industry-level Herfindahl–Hirschman index and year fixed effects. Our main coefficient of interest is β_0 which, conditional on the validity of this identification strategy, should capture any deviation from a parallel evolution in the outcome of interest between the treatment and the control group due to the introduction of the mandate. Finally, all regressions are weighted with Labor Force Survey weights, though in the appendix we show that our results do not depend on this choice. As for standard errors, they are clustered at the firm level, though in the appendix we also present specifications with other clustering groups such as firm size, or firm size times industry.

As our hypothesis is that this policy will affect differently men and women, we will estimate

[7] As explained in section 4, we choose this time window because it is the maximum number of years over which we observe all outcomes of interest.

[8] Both industry and firm culture can change over time, for instance if firms become multi-product, or hire a new CEO. Yet, it seems plausible to assume that these characteristics will be constant over the period of time considered.

this regression separately by gender. All regression tables will also report the p-value of the t-test on the equality of coefficients for men and women.

4 Data

To study the overall effect of this government mandate on the outcomes of interest, we make use of several sources of data, including individual-level data on pay and occupational outcomes, firm-level data on job vacancies and stock prices. Here we first introduce the main data used to measure employees' outcomes. This is the Annual Survey of Hours and Earnings (ASHE), an employer survey covering 1 percent of the UK workforce, conducted every year, and designed to be representative of the employee population.[9] The ASHE sample is drawn from National Insurance records for working individuals, and their respective employers are required by law to complete the survey. Specifically, ASHE asks employers to report data on wages, paid hours of work, tenure in the firm, and pensions arrangements for the selected employees, all measured in April. Other variables relating to age, occupation and industrial classification, and firm size are also available. Once a worker enters the survey, he/she is followed even when changing employer, though the individual is not observed when unemployed or out of the labor force. In practice, ASHE is an unbalanced panel data set at the employee level.

From ASHE, we create the following variables. First, to measure occupational outcomes and workers' flows, we proceed as follows. We consider a dummy equal to one if a worker is employed in an occupation whose median wage is in the top two quartiles of the pre-policy wage distribution (2012-2016). This includes skilled-trades, administrative, technical, and professional and managerial occupations. For brevity, we refer to this as "working in above-median-wage occupations". We create a dummy variable that is equal to one if the worker has changed job in the last year (ASHE provides a categorical variable to measure this). We also consider months of tenure in the firm, though this is missing for around 3 percent of the estimation sample. And, finally, we consider a

[9] Office for National Statistics. (2019). Annual Survey of Hours and Earnings, 1997-2018: Secure Access. [data collection]. 14th Edition. UK Data Service. SN: 6689, http://doi.org/10.5255/UKDA-SN-6689-13.

dummy variable that is equal to one if the employees leaves the firm in $t + 1$. By construction, this variable is missing in the last year of data. As for pay measures, the main variable of interest is log real hourly pay, including bonuses and allowances, but excluding overtime pay; next, we consider log basic real hourly wage, bonuses and allowances separately. To study the impact of the policy on bonuses and allowances, we consider the inverse hyperbolic sine transformation to account for the fact that many workers do not receive any bonus or allowance. Then, we consider log real weekly pay, and weekly hours worked, distinguishing between contractual hours and overtime.

In the empirical analysis, we consider data over the period 2012-2019. We chose this time window for two reasons. First, data on firm job advertisements that we will use in the analysis of mechanisms are only available from 2012 onward. Second, the ONS' occupational classification changes in 2010, and the variable following the new classification is only available from 2012 onward in the employees' data set. However, as soon as new waves of ASHE will become available, we will add them to the estimation period.

Table 2 provides summary statistics for the main outcomes, measured in the pre-treatment period. Several things are worth noting. First, the profile of workers in treated and control firms is remarkably similar. Second, focusing on the treatment group (columns 1 and 3), there is a six percent gender gap in the probability of working in above-median-wage occupations. Next, the unconditional percentage hourly pay gap amounts to 18 percent. There is also a large gender gap both in the probability of receiving allowances or bonuses (35 and 33 percent respectively), and a huge one in the amount received (around 60 and 75 percent). Men are also more likely to work in the private sector than women - though this share is already 80 percent. Finally, it is worth noticing that among both men and women, only one third of workers is covered by a collective agreement. This figure is important to consider when thinking about the mechanisms through which the policy may affect wage and occupational outcomes. In principle, pay transparency may induce women, and especially those covered by collective agreements to put pressure on employers to obtain promotions or wage increases. Yet, with such a low share of women covered, it is unlikely that this channel will be important in triggering firms' response.

5 Main findings

This section illustrates our key findings. First, we present the results on occupational outcomes and job mobility, then we move to the analysis of wages, considering both different pay measures and various components of wages.

5.1 Occupational outcomes and job mobility

Figure 4 introduces the analysis on occupational outcomes. In particular, it shows the row trends in the variable "above-median-wage occupation" over the period 2012-2019 for employees working in treated firms (250 to 300 employees) and in control ones (200 to 249 employees). The top graph reports the trends for men, while the bottom one refers to women. We can observe two things from these figures. First, the evolution of this variable in the pre-policy period seems to be comparable across treatment and control groups, both for male and female employees. Second, while the top graph suggests that male occupational distribution has not been affected by the policy, the bottom graph suggests that treated firms may have changed the composition of their female workforce after the introduction of the policy, by increasing the share of women in above-median-wage occupations.

Table 3 turns to the regression analysis. Panel A reports the estimates of β_0 for men, while Panel B focuses on women, and each column refers to a different specification. Column 1 reports the estimates of the baseline specification, which controls for firm and year fixed effects. According to these results, the mandate increases the probability that women work in above-median-wage occupations by 3 percentage points - or 5 percent relative to the pre-policy mean reported at the bottom of the table. In contrast, the policy does not seem to affect the occupational distribution of men. Column 2 adds individual controls for age and age squared, but the results are practically unchanged. Column 3 further includes year times region fixed effects to control for local labor market time shocks, and once again the results are barely affected.[10] Columns 4 to 6 add different

[10]We consider NUTS2 regions here, corresponding to 8 areas in the UK.

industry/firm-level controls. Specifically, column 4 includes industry linear time trends, column 5 includes interaction terms between the 2011 industry-level Herfindahl-Hirschman index for product market concentration interacted with year fixed effects, and column 6 includes interaction terms between firm 2011 output level and year fixed effects. None of these controls affect the estimates of β_0 for either men or women. Thus, as the results are very similar across specifications, in what follows we take the specification of column 3 as our benchmark specification.[11]

Table 4 complements these results by analyzing the impact on job mobility. Specifically, the first column reports the impact on the probability of working in above-median-wage occupations, column 2 displays the impact on the probability of having joined the firm in the last year, column 3 focuses on months of tenure in the firm, and column 4 reports the effects on the probability of leaving the firm in $t + 1$. The results in columns 2 and 3 suggest that the positive impact on women's occupational outcomes comes from the newly hired women. Column 4 shows instead that the policy has no effect on the probability of leaving the firm for either men or women.

As the policy does not affect men's occupational outcome and job mobility, the first implication of this table is that the overall gender composition should have changed in treated firms following this policy. While we cannot test this implication with the current available data, we will be able to do so upon gaining access to the Workplace Employment Relationship Survey for the years 2011 and 2018. This will allow us to measure the share of women in treated and control firms both before and after the introduction of pay transparency.

The second implication of these results is that pay transparency does not seem to affect retention rates in this context. Yet, as suggested by the "fair wage-effort hypothesis" (Akerlof and Yellen 1990), it will be important to continue monitoring this outcome as the publication of the gender pay gap indicators, coupled with firms' responses, may affect effort levels and retention rates of those workers who perceive that they are treated unfairly by their employer. Moreover, upon getting access to the Annual Business Survey, we will also study the impact of this policy on

[11] In appendix, table A1, we further show that this result seems to be driven by a increase in the womens' probability of working in occupations in the middle tercile, administrative and skilled trade occupations, and by a contemporaneous decrease in their likelihood of working in low-paid occupations, namely personal, sales, elementary, and plant and machine-operative occupations.

labor productivity.

5.2 Wages

Figure 5 shows the raw trends in the variable "log real hourly pay" over the period 2012-2019 for employees working in treated firms (250 to 300 employees) and in control ones (200 to 249 employees). As above, the top graph reports the trends for men, while the bottom one refers to women. Two things may be observed from these figures. First, the evolution of real hourly pay in the pre-policy period seems to be comparable across treatment and control groups, both for male and female employees. Second, the top graph suggests that male real hourly pay of employees working in treated firms may have dropped after the introduction of the mandate. As for women, it does not appear that the policy has visibly affected their real wages.

Table 5 reports the estimates of the difference-in-difference model for this outcome. Panel A reports the estimates of β_0 for men, while Panel B focuses on women. Each column refers to a different specification. Column 1 presents the estimates from the baseline specification, with firm, year and individual fixed effects. According to these results, the transparency policy decreases men's real hourly pay by around 2 percent in treated firms relative to control ones after the introduction of the mandate, with this effect being significant at 5 percent. In contrast, the policy does not seem to have an effect on female real wages. Column 2 adds firm times individual fixed effects. As results are practically unchanged, this indicates that the drop in men' real wages is actually a within-firm-within-individual effect, meaning that it is experienced by individuals who were already employed at the firm before the introduction of the mandate. Column 3 adds year times region fixed effects to the baseline specification. Point estimates slightly increase but the significance level does not change. Next, as above, columns 4 to 6 add different industry/firm-level controls to the specification of column 3, but the main conclusions of the analysis are unchanged: pay transparency leads to pay compression from above. Importantly, as indicated by the p-value of the t-test on the equality of coefficients for men and women, the effects by gender are statistically different. In other words, this policy leads to a significant reduction of the gender pay gap, amounting to around 15 percent of the pre-policy mean.[12]

Tables 6 and 7 further unpack the effects on male hourly wages. First, table 6 shows that weekly wages, rather than hours worked, are the margin of adjustment. Second, table 7 shows that the changes brought by the policy are mainly due to contractual wages rather than allowances and bonuses. Taken together, these results suggest that the slowdown of male real hourly wages may be explained by a decrease in the probability of being promoted, though the data we have do not allow us to measure this precisely as it does not include job level information (only occupational information).

The last point that is worth discussing concerns the effect on women's pay. In light of the results on occupational outcomes, we could have expected to see an increase in women's wages. Two factors may explain why this effect has not materialized. First, both treated and control firms may have decided to raise women's wages if competing in the same labor market. Yet, in figure 5, we do not see any sharp increase in women's wages after the introduction of pay transparency in either the treatment or the control group. An alternative explanation may have to do with compositional effects. As treated firms are hiring more women in above-median-wage occupations relative to the control group, the average woman in treated firms becomes less experienced and potentially of lower ability compared to those in the control group. We believe that this is a very likely explanation for why we fail to see an increase in their wages.

6 Robustness checks

The validity of our identification strategy depends on three assumptions. First, the evolution of the outcomes of interest is comparable in treated and control firms prior to the introduction of the policy, the usual parallel-trend assumption. Second, our estimates do not capture the effect of other time shocks coinciding with the introduction of pay transparency and affecting differently firms on

[12] According to the estimates shown in table 5, the transparency policy reduces male real hourly wages by 2.8 percent relative to a pre-treatment mean of 16.92, that is 47 cents. The row pre-policy gender hourly pay gap amounts to 3.03 pounds. Thus, the policy leads to a reduction of 0.47/3.03 or 15.5 percent.

the two sides of the 250-employees cutoff. Third, the results do not depend on the size of the bandwidth considered around the policy cutoff, nor do they depend on the year chosen to define the treatment status.

Parallel-trend assumption. To support the validity of the parallel-trend assumption, we perform event-study exercises. Specifically we augment regression 1 with the leads and lags of the mandate, as follows:

$$Y_{ijt} \quad = \quad \alpha_j + \theta_t + \sum_{k=2012}^{2019} \beta_k (TreatedFirm_j * \theta_k) + X'_{it}\pi + Z'_{jt}\delta + u_{ijt} \tag{2}$$

Figures 6 and 7 report the estimates of the β_k on the probability of working in above-median-wage occupations, and log real hourly pay. In each figure, the top graph refers to men, while the bottom one refers to women. Note that 2017 is taken as the reference year. The leads of the mandate are insignificant for both variables, and genders, which strongly supports the hypothesis that the evolution of the outcomes of interest is comparable across treated and control firms before the introduction of the mandate. On the other hand, the effect on women's probability of working in above-median-wage occupations is visible already in the first post-mandate year and increases over the second year. As for the negative effect on male hourly pay, this becomes clearly visible and significant in the second year of the treatment period.

Contemporaneous shocks. To make sure that our estimates do not capture the effect of other phenomena occurring in 2018 and affecting treated and control firms differently, we perform three robustness checks. First, table 8 compares the estimates from the difference-in-difference model to those of a triple-difference model with the gender dimension as the third difference. As such, this alternative specification controls for within-group time shocks that are common to male and female employees. The table reads as follows. The first three columns refer to the outcome "working in above-median-wage occupations", while columns 4-6 focus on log real hourly pay. For each outcome, the first column reports the estimates of the difference-in-difference model for men, the second columns the effects on women, while the third one reports the estimates from the triple-difference model. At the bottom of columns 3 and 6, we also report the p-value on the t-test for the overall effect on women, i.e. the sum of the male coefficients plus the differential effect on women. Remarkably, the estimates from the triple difference model are practically indistinguishable from those of the difference-in-difference model, both in the case of the occupational outcomes and wages. The only difference is that in column 6, the coefficient on the differential effect of the policy on men and women's wages is marginally insignificant. Yet, the overall effect on women is null and insignificant.

We next perform a second robustness check to support the hypothesis that our estimates do not capture the effect of other time shocks coinciding with the introduction of pay transparency and affecting differently firms on the two sides of the 250-employees cutoff. Table 9 compares the results of the difference-in-difference model with that of a difference-in-discontinuity model. The main difference between the two is that the latter takes into account the possibility that firms with a different number of employees are on different trends (Grembi et al. 2016). Though our event studies seem to exclude that this is the case, this exercise should further support this assumption. Table 9 reads as follows. Panel A compares the estimates of the different models for men, while Panel B focuses on women. In each panel, the first three columns refer to the occupational outcome, while the last three refer to log real hourly wages. For each outcome and gender, the first column reports the estimates of the impact of the transparency policy from the double-difference model. Column 2 adds the interaction between post and normalized firm size in 2015, and the triple interaction term between post, normalized firm size in 2015, and the dummy for treated firms. The point estimates for both the occupational outcome and wages are barely affected. Though the impact on men real hourly wages becomes marginally insignificant in this specification, it gains significance when year fixed effects are replaced by a post dummy in column 6, as it is common in difference-in-discontinuity specifications.

Finally, we run a series of placebo tests pretending that the mandate binds at different firm size thresholds. Figures 8 and 9 present the estimates of these placebo policies, together with 95 percent confidence intervals. The placebo cutoff is indicated in correspondence of the estimates. The highlighted estimates represent the "true" estimates. In each regression, the estimation sample includes firms with $+/-50$ employees from the threshold considered. Reassuringly, the "150" placebo mandate does not appear to have an impact on either male or female outcomes. This should further exclude the possibility that we are capturing the impact of time shocks happening at the same time as the mandate and affecting larger firms differently from smaller ones. As for larger placebo cutoff values, it should be noted that these regressions include all treated firms. The fact that the magnitude of the effects are non-zero may simply point to heterogeneous effects of the policy across firm size, consistent with the idea that larger firms are more exposed to public scrutiny.

Specification. Our third and final set of robustness checks aims to verify that our results are robust to the choice of the bandwidth around the 250 cutoff, and do not depend on the fact that we defined the treatment status based on firms' number of employees in 2015. Figures 10 and 11 show how the estimates of β_0 from equation 1 change when restricting or enlarging the bandwidth around the 250 cutoff. As above, the top graph in each figure refers to men, while the bottom one refers to women. The x-axis reports the estimated coefficients with 95 percent confidence intervals, while the y-axis reports the bandwidth considered, from $+/-30$ to $+/-80$ employees around the policy cutoff. The estimates on the bandwidth of 50 correspond to the main specification. Figure 10 shows that the effects on women's probability of working in above-median-wage occupations is especially stable for bandwidths comprised between 30 and 60, while it vanishes for larger samples, possibly due to decreased comparability across treatment and control groups. Figure 11 shows instead that the estimated coefficients on men's real hourly pay are very similar across specifications, and only become marginally insignificant when estimating the model using the smallest sample. Conversely, estimates of the coefficient of interest on women's hourly pay are always close to zero and insignificant.

Finally, table 10 compares the results when we change the year used to define the treatment status. The table reads as follows. Panel A refers to men, and panel B to women. In each panel, columns 1-4 refers to the outcome "working on above-median-wage occupations", while columns 5-8 concern the outcome log real hourly pay. For each outcome, the first column reports the results from the main specification. The following columns present the estimates obtained when defining the treatment status based on firms' number of employees in the year indicated on top of the column, 2012, 2013, or 2014. While the estimates that are significant in the main specification become marginally insignificant for one year, they are significant and similar in magnitude for all the other years.[13]

To sum up, our estimates are remarkably stable across different specifications and sample sizes, which should strongly supports the validity of our identification strategy.[14]

7 Mechanisms

To delve into the mechanisms driving the estimated effects, we follow two main directions. First, to understand how treated firms may have been able to attract more women, we turn to analyze their hiring strategies. We are especially interested in studying three dimensions of response: the effect of the policy on wage posting decisions, the use of gendered wording and the offer of flexible work arrangements. So far, we are presenting our preliminary results on the first two dimensions. Second, to study the role played by reputation in triggering firms' response to the pay transparency policy, we focus on the reaction of the stock market following the publication of gender pay gap indicators by firms listed on the London Stock Exchange. If investors put firms under scrutiny, this may constitute an important incentive for businesses to address their gender pay gap.

[13] In the appendix, table A2, we further compare our identification strategy to one where the treatment status was defined based on actual firm size. While, if anything, we could have expected the coefficients to be larger when using the actual firm size due to potential positive selection, the point estimates are lower in magnitude and not statistically significant. Potentially the fact that treatment status changes over time using this definition induces noise in the estimates, on top of any selection issue.

[14] In tables A3 and A4, we further show that our results do not depend on the use of LFS weights, nor they are sensitive to age restrictions. Finally, in tables A5 and A6 in the appendix we show that the significance of our estimates is not affected by the clustering group considered, being this firm, firm-size or firm-size times industry.

7.1 Firms' hiring strategies

To study whether firms targeted by pay transparency change their hiring strategies in order to attract more women, we use Burning Glass Technologies (BGT) job-advertisement data. BGT offers UK job listing data for the period 2012-2019. The data are obtained by scraping firms' and official job advertisement platforms, and then removing duplicates. The resulting data set,, with around 40 million observations, offers key information. First, we have access to the text of the job advertisement. Second, more than 95 percent of vacancies have an occupation SOC identifier and a county identifier. Crucially, around one third of vacancies, or 13 million observations, give the name of the employer. This is the data set we are going to focus on, though first we investigated potential selection issues related to the presence of the firm name. To this aim, we compared the occupational distribution of the stock of vacancies in BGT and that of employment in the Labor Force Survey (LFS hereafter) for the same period. Reassuringly, figure C1 in the appendix shows that the two match well, mitigating potential concerns regarding the representativity of BGT.

We merge the BGT data with the GEO data, using a cosine similarity name-matching algorithm for the company names, and retain only firms that have an exact match, representing almost 90 percent of the entire sample - section C.1 of the appendix provides a detailed description of the matching algorithm.[15] In what follows, we present the key dimensions we explore in this matched data set.

Wage posting. Many studies document that there exists a gender gap in bargaining skills (Leibbrandt and List 2015, Bowles et al. 2007, Babcock et al. 2003.). In turn, the gender pay gap is larger in jobs that leave negotiation of wage ambiguous (Leibbrandt and List 2015). In BGT job vacancies, only around 30 percent of job listings contain information on wages, with large heterogeneity across industries, as shown in figure 12. Moreover, as shown in table 11, consistent with the studies cited above, on average GEO firms that are less likely to post wages also tend to have

[15] Note that the analysis done in this section is restricted to GEO companies that have a registration number in the GEO data, as this information is essential to further match them with FAME (see below). This implies loosing around 1000 companies per year.

a higher gender pay gap and a lower share of women at the top of the wage distribution. This descriptive evidence suggests that wage posting may be an important dimension of adjustment for firms willing to attract more women.

Gendered wording. A recent strand of psychology and management lab experiments study the importance of implicit biases in job postings (Tang et al. 2017, Gaucher et al. 2011). In particular, Gaucher et al. (2011) construct a list of job-listing-specific male and female-oriented words making use of implicit association tests. Table B1 in appendix, section B, shows the resulting list of words associated to each gender. Exploiting this list, the authors are able to classify job advertisements based on a gender score defined as follows:

$$GenderedScore_j = (N_{MaleW_j} - N_{FemW_j})/N_{TotalWords_j}$$

where j is a job listing, N_{MaleW_j} is the number of male-oriented terms, N_{FemW_j} is the number of female-oriented words, and $N_{TotalWords_j}$ is the total number of words in the job advertisement. Thus, a job listing with a positive score is considered to have a male-oriented wording, while one with a negative score presents a female-oriented wording, and a neutral job listing would have a score of 0. Importantly, both Tang et al. (2017) and Gaucher et al. (2011) present lab-based evidence that women are less willing to apply to a job if its posting uses male-oriented wording.

The top graph in figure 13 shows that in the matched data set there is variation in the gender score by occupation, with job listings for personal, administrative and elementary occupations using more feminine words, while advertisements for plant and machine operative occupations use more masculine words. Moreover, in the bottom graph, we can see that there is considerable variation in the wording of the job advertisements across industries as well. In particular, those in public administration and education use more feminine words, while at the other extreme are those in manufacturing and agriculture, forestry and fishing - industries that are also more likely to employ a higher proportion of men.

Next, in table 12, we analyze the raw correlations between job listings' gendered score and the published gender pay gap indicators. Column 1 shows that there is a positive correlation between firms' mean gendered score and the reported gender pay gap. In other words, companies that employ more male-orientated words for their job advertisements also present a large gender pay gap. In addition, column 2 shows that a higher gender score is also associated with a lower share of women in the top quartile of the firm wage distribution.

Regression analysis. Overall, this descriptive evidence suggests that firms' performance on gender pay gap indicators may be correlated with their hiring strategies, which pushes us to study the causal impact of the pay transparency policy on firms' wage posting decisions and choice of gendered wording.

In order to do this, we need two additional elements. First, we need a control group, and second we need to know the exact firm size to perform the difference-in-difference analysis. To construct the final sample, we then turn to FAME, the UK version of Amadeus, covering all UK-registered firms. For around 30 percent of them, we have information on the number of employees for at least one year in the pre-treatment period, crucial information to implement the difference-in-difference analysis. We then merge FAME with GEO firms using the Company House registration number.

Finally, we restrict the sample to FAME firms with 150 to 249 employees in the years 2014-2017, and merge the FAME firms that are not in GEO with BG directly using the same name-matching algorithm for the company name. The final data set contains 1,529,893 observations on 8046 firms. When we further restrict the sample to firms with 200 to 300 employees, which is going to be the main estimation sample, we end up with 91366 observations and 2109 firms.

To investigate the effect of the pay transparency policy on firms' wage posting decisions and choice of wording in job listings, we estimate the following difference-in-difference model at the vacancy level:

$$Y_{ijt} \quad = \quad \alpha_j + \theta_t + Z'_{ijt}\delta + \beta_0 TreatedFirm_j * Post_t + u_{ijt} \tag{3}$$

where Y_{jt} is either a dummy equal to one if vacancy i for job listing i of firm j in year t contains wage information, or it represents the gendered score, α_j and θ_t are, firm and year fixed effects, respectively, while Z_{ijt} contains 2-digit occupation fixed effects and occupation-specific time effects. Finally, standard errors are clustered at the firm level, and we also weight regressions by occupation-employment shares from the LFS.

Tables 13 and 14 present the preliminary results of this analysis. The first table refers to wage posting, while the second shows the results for the gendered score. In light of the variation we have seen in descriptive analysis, especially along the industry dimension, in both tables, we explore potential heterogeneous effects across different industries. In particular, in table 13, we present the results for the entire sample, column 1, and for industries with a low or high gender pay gap in the pre-treatment period.[16] While the coefficient is marginally insignificant, the point estimates in column one suggest that treated firms may have increased their tendency to post wages after the introduction of the policy. Interestingly, in the next two columns, we can see that there are indeed heterogeneous results across industries, and firms in industries with a high gender pay gap pre-treatment become indeed more likely to post wages after the pay transparency policy is introduced. As for the gendered score, table 14 also points to potential heterogeneous effects across industries. In particular, while in the entire sample it does not seem that the policy has influenced this margin of decision, in column 3 we can see that firms belonging to industries characterized by a high gendered score in the pre-treatment period may have decreased the use of male wording following the introduction of the pay transparency policy, though the coefficient is just marginally insignificant.

Overall, this preliminary analysis suggests that treated firms may have been able to attract more women in better-paid occupations by acting on their hiring strategies. Our next step will be to further investigate this channel, by digging into the composition of the gendered score, and studying the offer of flexible work arrangements.

[16] To define the two groups, we calculated the mean gender pay gap in each 1-digit industry from ASHE, and defined as high-gpg firms those with a gender pay gap above the median one. These include manufacturing, construction, banking and finance, and public administration, education and health sector.

7.2 Stock market reaction

The public disclosure of firms' gender pay gap may induce businesses to tackle gender pay differentials to preserve their reputation. The spike in google searches for the topic "gender pay gap" in correspondence to the deadlines for the publication of gender pay gap indicators suggests that firms are under the scrutiny of the general audience. But what may matter more to them is what investors think. A negative reaction of the stock market to the publication of the gender pay gap figures may constitute a strong incentive for a firm to improve its performance on gender equality. This paragraph aims to measure this response of the stock market, using the traditional event-study methodology (Bell and Machin 2018, Lee and Mas 2012). We focus on the first year of publication as this is when gender pay gap indicators are more likely to represent an information shock for the market. We first combine the list of firms publishing gender pay gap figures in the financial year 2017/18 with FAME to identify both firms that are directly publicly listed on the London Stock Exchange (LSE), and those that have a parent company that is publicly listed. This leads us to identify 926 firms, or around 10 percent of firms publishing gender pay gap figures. Of this group, 101 are directly publicly listed, while the rest has a publicly listed parent company. Importantly, firms can have the same parent company. As a result, we follow 405 distinct publicly listed firms, or 35 percent of all firms listed on the main market of the London Stock Exchange in 2018. Also note that 80 percent of firms belonging to the same group publish on the same date. Hence, in what follows, we consider the publication date of the first that publishes. Extracting daily stock prices from Datastream, we then construct firms' abnormal returns, or AR, as the difference between a stock's actual return and the expected return, where this is estimated using a simple market model:

$$AR_{jt} \quad = \quad r_{jt} - (\alpha_j + \widehat{\beta}_j r_{mt}) \tag{4}$$

where r_{jt} is firm j stock market return on day t, and r_{mt} is the return of the LSE-all-shares index on day t. Figure 15 shows the cumulative abnormal returns from the day of publication to day Y relative to the publication date, or CARs(0, Y), with Y going from -10 to 10. While these are not statistically different to zero in the days prior to the publication date, they start to become negative from the publication date up to five days afterwards, where they decrease by up to 65 basis points - as also reported in table 15.[17] Table 16 further investigates whether this drop may be related to the performance on the gender pay indicators. Column one regresses the CARs at five days after the publication on a constant, the average gender pay gap reported by firms related to the same publicly listed firm, called "Group-avg GPG performance" in the table, a dummy equal to one if the gender pay gap is in favor of men, called "Group-avg GPG performance negative", and an interaction term between these two. Column 2 adds industry fixed effects, and column 3 also controls for the log of market capitalization at t-1, the book-to-market value at t-1 and the return on assets at t-1. In all columns, the point estimate on the coefficient for "Group-avg GPG performance negative" is negative, though marginally insignificant. On top of this, the positive and significant coefficient on "Group-avg GPG performance" indicates that abnormal returns are larger for firms with a gender pay gap closer to 0, while the negative and significant coefficient on the interaction term suggests that firms are penalized if they report a gender pay gap in favor of men. Overall, while this effect fades away after five days from the publication, it suggests that firms publishing gender pay gap indicators are under the scrutiny of investors. In turn, this indicates that the reputation motive may have played an important role in explaining the reaction of treated firms.

8 Conclusion

To tackle the persistence of the glass ceiling phenomenon, many governments are promoting pay transparency policies. Exploiting the variation across firm size and over time in the application of the UK transparency policy, this paper shows that this mandate has increased by 5 percent the probability that women work in above-median-wage occupations, with this effect being driven by newly hired women. While this compositional effect has not yet translated into a wage effect, this

[17] As a comparison, note that Bell and Machin (2018) find that the sudden increase in the minimum wage, announced by the UK government in May 2015, leads to a 70 basis points immediate decrease in abnormal returns of low-wage firms.

may materialize as women gain experience in the firm. In addition, the UK pay transparency law has led to a 2 percent decrease in male real hourly wages in treated firms relative to control ones. Finally, by combining the difference-in-difference strategy with a text analysis of job listings, we find suggestive evidence that treated firms belonging to industries with a high gender pay gap become more likely to post wage information after the policy is introduced.

Overall, our findings have two main implications. First, pay transparency leads to pay compression from above. Remarkably, this conclusion is in line with the findings of other studies on pay transparency. In particular (Mas, 2017) finds that pay transparency in the public sector in California leads to a 7 percent reduction in managers' compensations, while both (Baker et al., 2019) and (Bennedsen et al., 2019) find that disclosing employees' pay by gender leads to a reduction of the gender pay gap, through a negative effect on male real wages. Potentially, freezing wage increases of better-paid employees is the most viable option for firms in the short-run.

The second implication of our findings is that by making the glass ceiling visible, pay transparency creates cracks in it. The pre-policy 4 percentage-point gender gap in the probability of working in above-median-wage occupations is practically halved with the disclosure of gender pay gap indicators. On top of this, the 2.8 percent negative effect of transparency on men's real wages corresponds to approximately a 15 percent decrease in the in-sample pre-policy gender pay gap. As a comparison, Bertrand et al. (2019) find that female board quotas, another firm policy that has been largely discussed recently, has no impact on the gender pay gap. In other words, pay transparency seems to be more effective than other policies in cracking the glass ceiling. Importantly, this may be true only in the short run, when transparency raises strong attention from both the media, the stock market, and the public audience.

In sum, it is important to stress that our analysis identifies short-term effects, and we will need to keep monitoring the effects of this policy in the long run to fully understand its effect on the labor market.

Our next step is to further dig into the impact of this pay transparency policy on firms' hiring strategies. In particular, we will study how it affects the offer of flexible work arrangements. Analyzing this dimension of firms' decisions seems especially important in light of the compositional effect that we find on women's occupational distribution.

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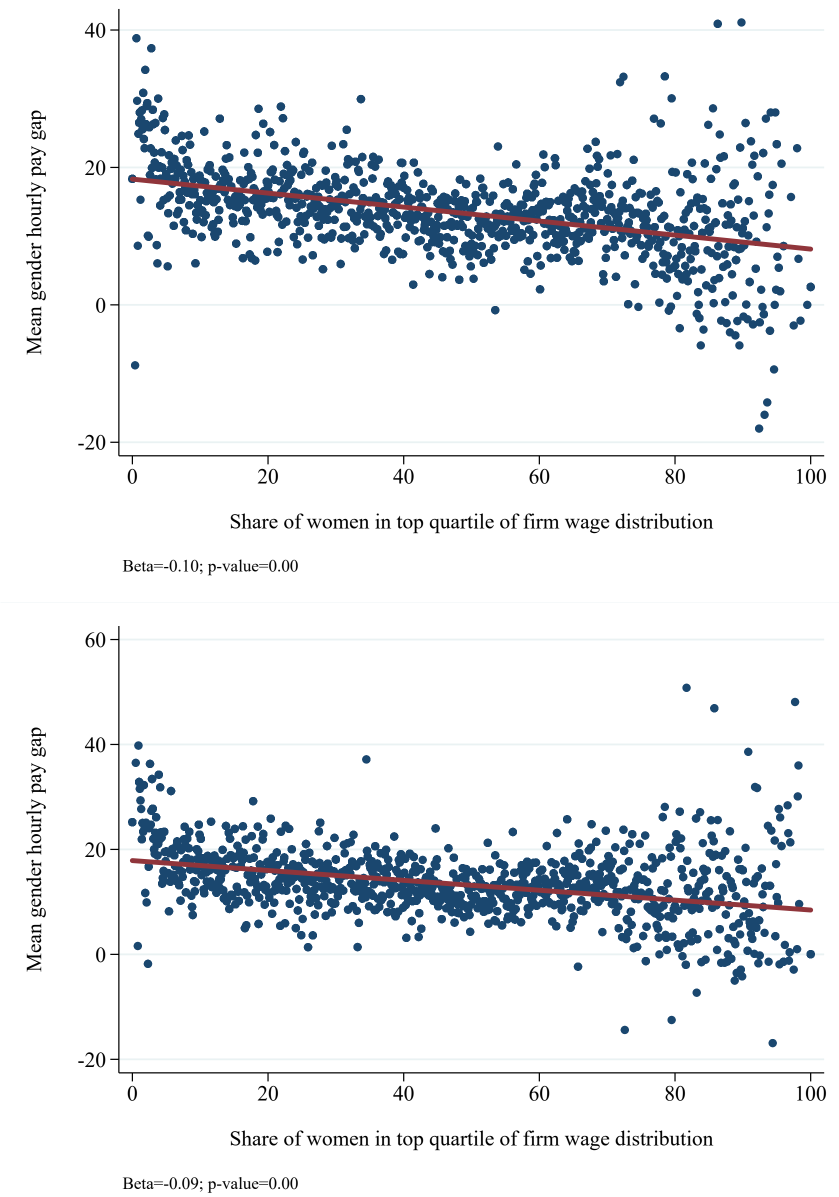
9 Figures and Tables

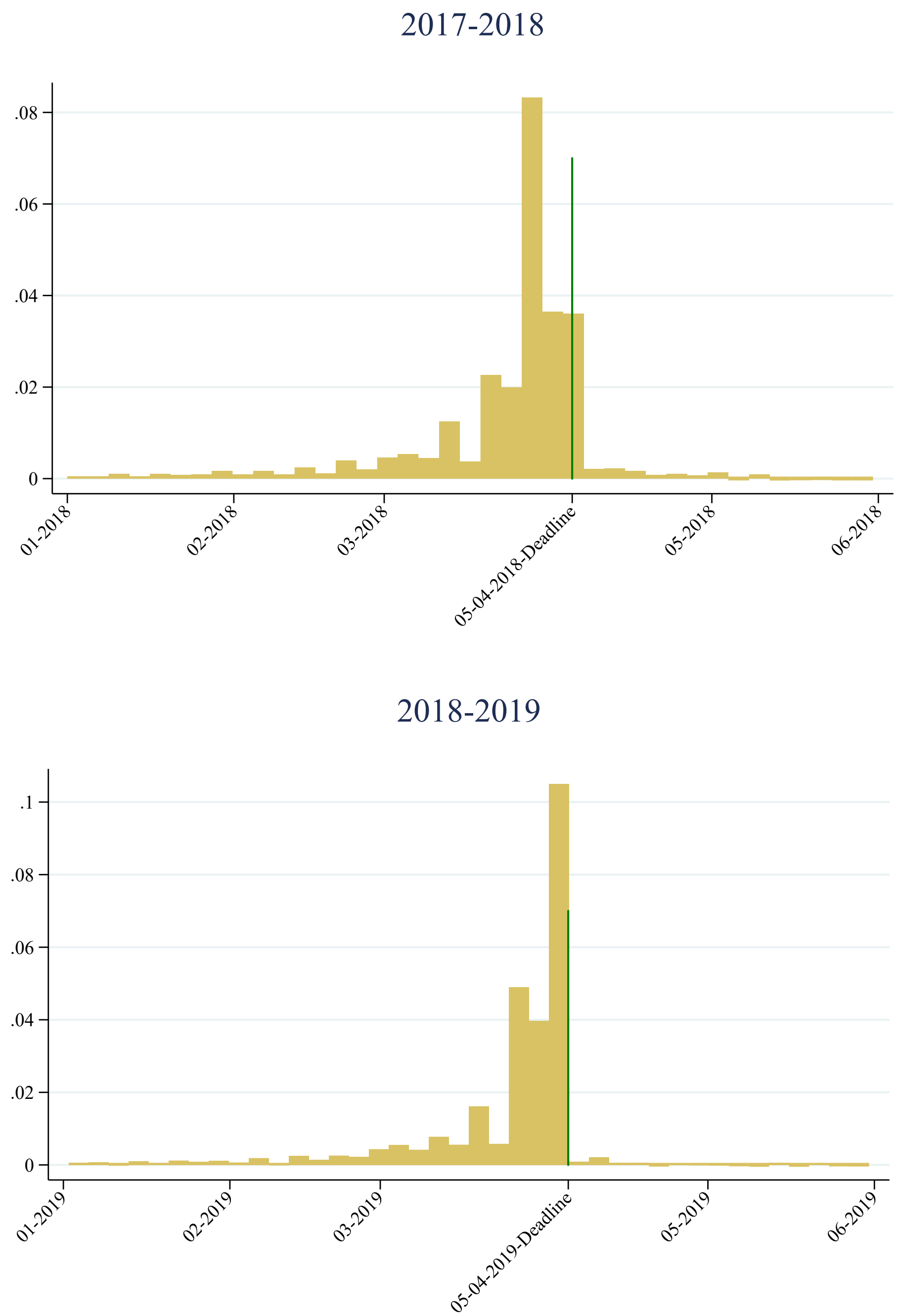
Figure 1: **Gender pay gap and women at the top**



Source: UK Government Equalities Office (GEO).

Note: This figure shows the correlation between firms' gender mean hourly pay gap and the share of women in the top-quartile of the firm wage distribution. The top graph refers to the 2017/18 data (10,558 observations), while the bottom one refers to 2018/19 (10,812 observations). The bottom and top 1 percent of the data are excluded from the sample.

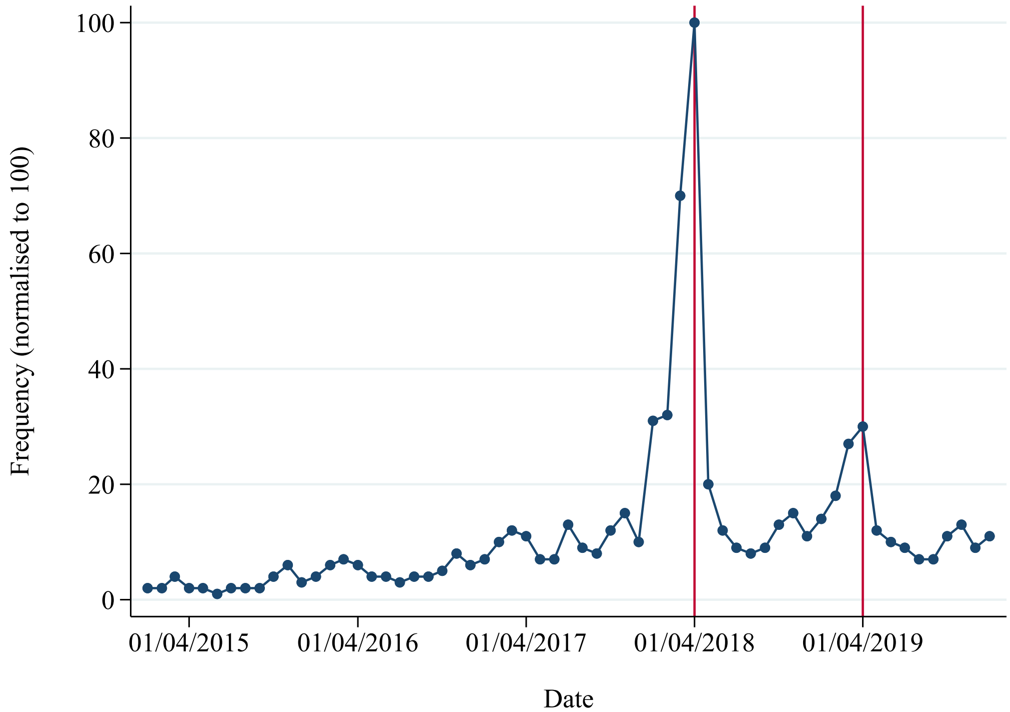
Figure 2: **Distribution submission date by year**



Source: UK Government Equalities Office (GEO).

Note: This figure shows the distribution of days when firms published their gender pay gap indicators. The top graph refers to the 2017/18 data (10,558 observations), while the bottom one refers to 2018/19 (10,812 observations). Around 5 percent of firms publish before January of the deadline year.

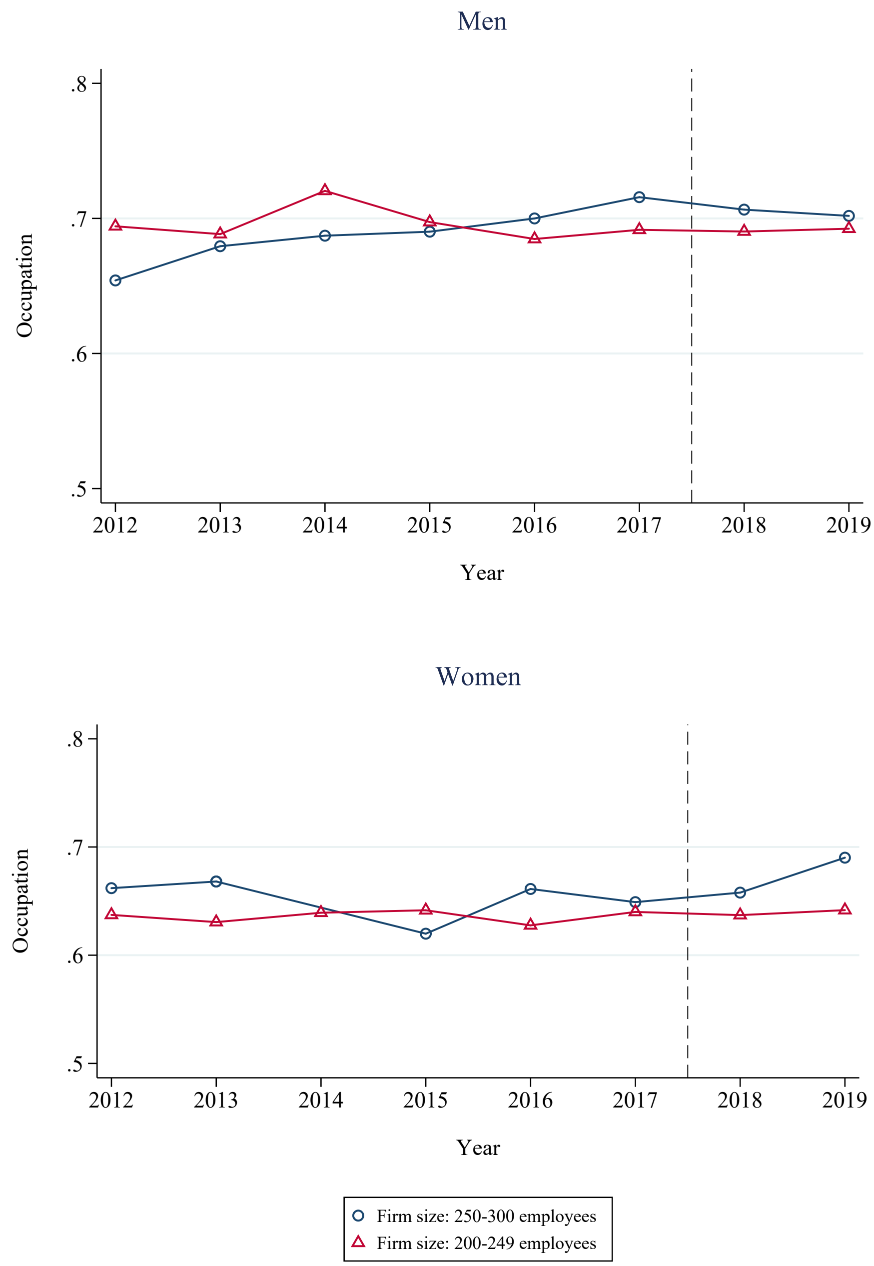
Figure 3: **GPG searches on Google**



Source: Google Searches.

Note: This figure reports google searches for the term "gender pay gap" between April 2015 and June 2019. The data are normalized at 100 on April 5 2018.

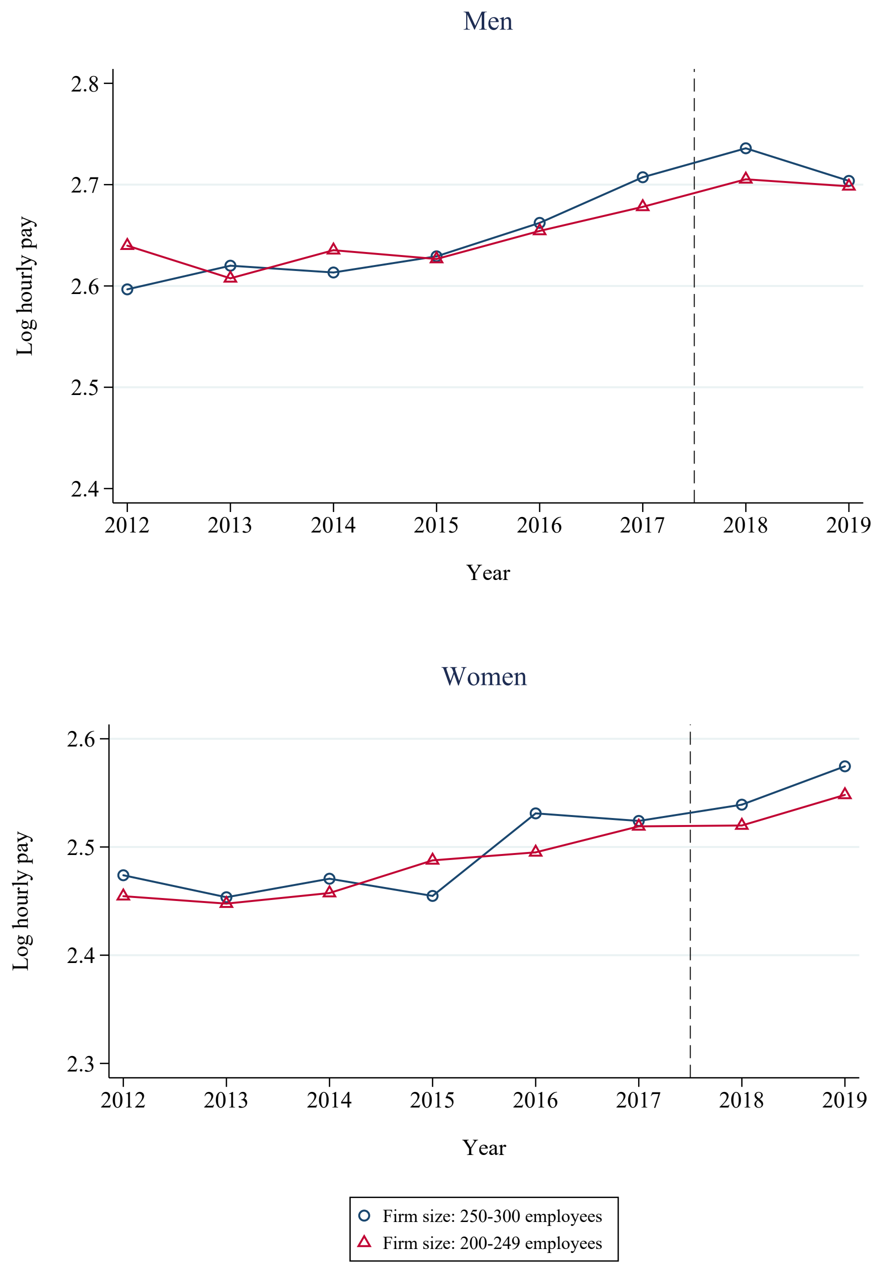
Figure 4: **Raw trends: working in an above-median-wage occupation**



Source: ASHE, 2012-2019.

Note: This figure reports the trends in the share of employees working in occupations paid above the median wage. The top graph refers to men, the bottom one to women. The blue line represents the treatment group, individuals working in firms with 200-249 employees, and the red line the control group, individuals working in firms with 250-300 employees.

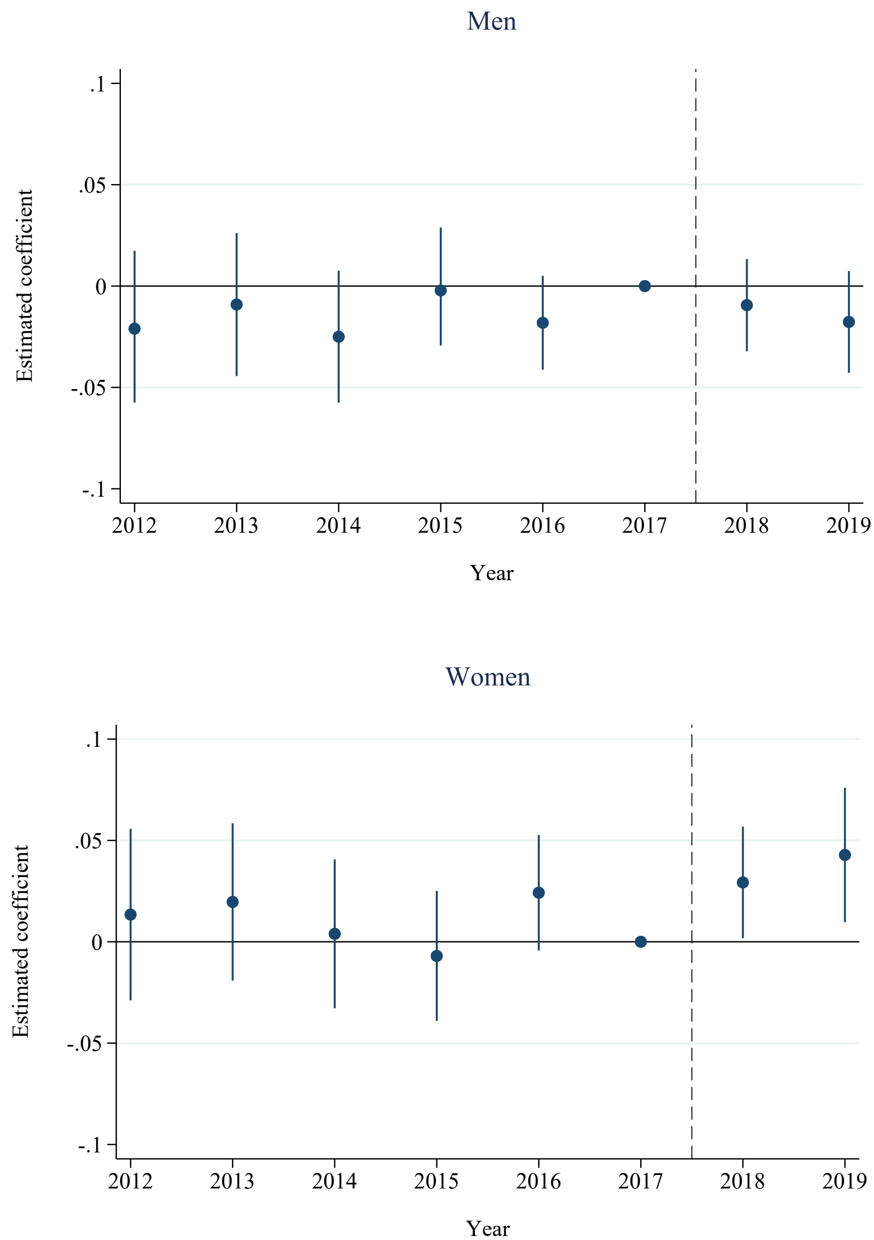
Figure 5: **Raw trends: log real hourly pay**



Source: ASHE, 2012-2019.

Note: This figure reports the trends in log real hourly wages. The top graph refers to men, the bottom one to women. The blue line represents the treatment group, individuals working in firms with 200-249 employees, and the red line the control group, individuals working in firms with 250-300 employees.

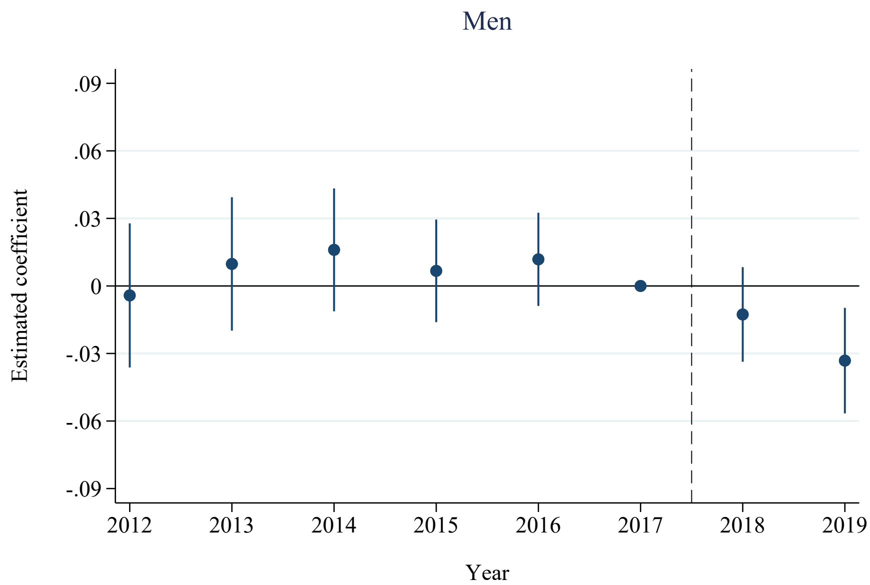
Figure 6: **Event studies - working in above-median wage occupation**



Source: ASHE, 2012-2019.

Note: This figure reports the estimates of the leads and lags of the policy obtained from regression 2 on the outcome "working in an above-median-wage occupation". The top graph refers to men, while the bottom one refers to women. In the top (bottom) graph, the estimation sample includes men (women) employed in firms with 200-300 employees, and present in ASHE between the financial years 2011/2012 and 2018/2019. All regressions are estimated using LFS weights. 95 percent confidence intervals are also reported. The dash vertical line indicates the month when the mandate is approved, i.e. February 2017.

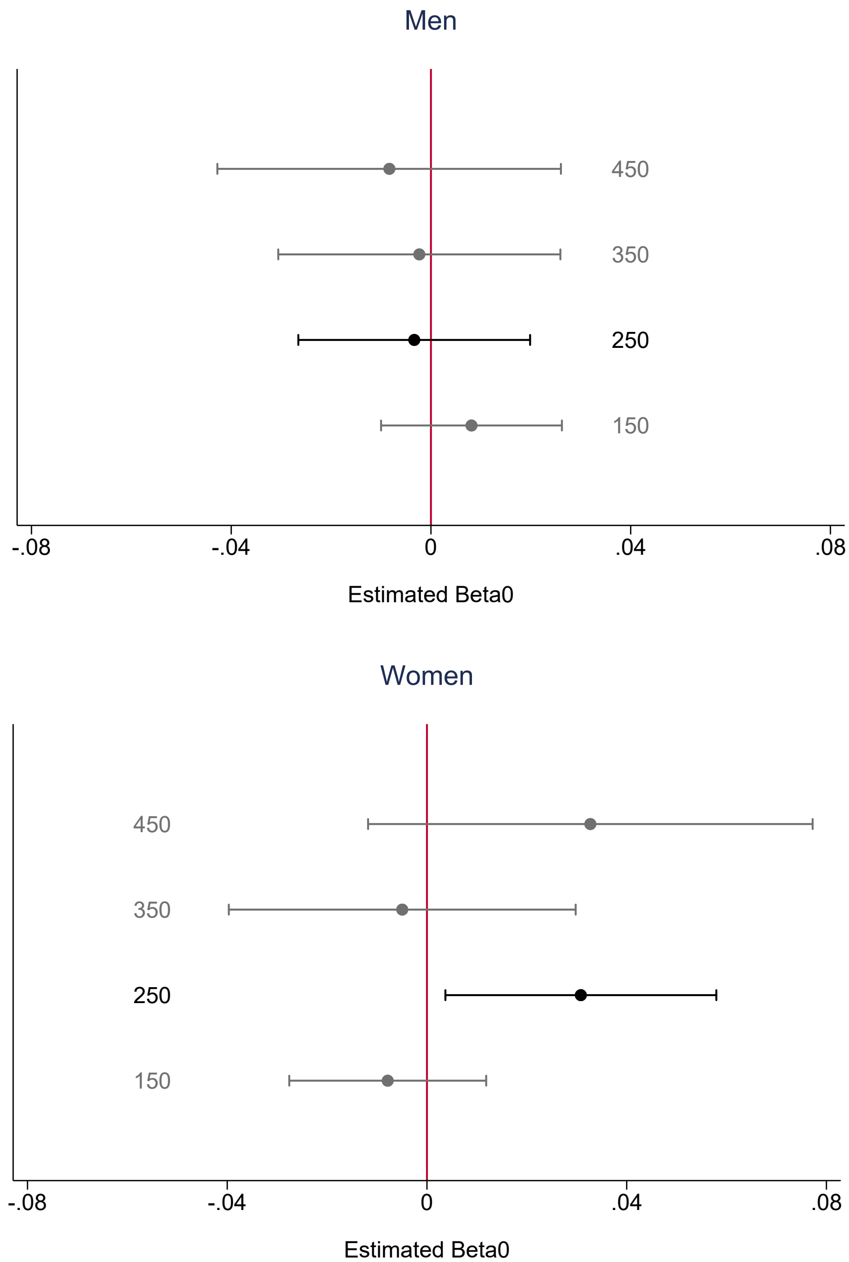
Figure 7: **Event studies - log real hourly pay**



Source: ASHE, 2012-2019.

Note: This figure reports the estimates of the leads and lags of the policy obtained from regression 2 on the outcome log real hourly wage. The top graph refers to men, while the bottom one refers to women. In the top (bottom) graph, the estimation sample includes men (women) employed in firms with 200-300 employees, and present in ASHE between the financial years 2011/2012 and 2018/2019. All regressions are estimated using LFS weights. 95 percent confidence intervals are also reported. The dash vertical line indicates the month when the mandate is approved, i.e. February 2017.

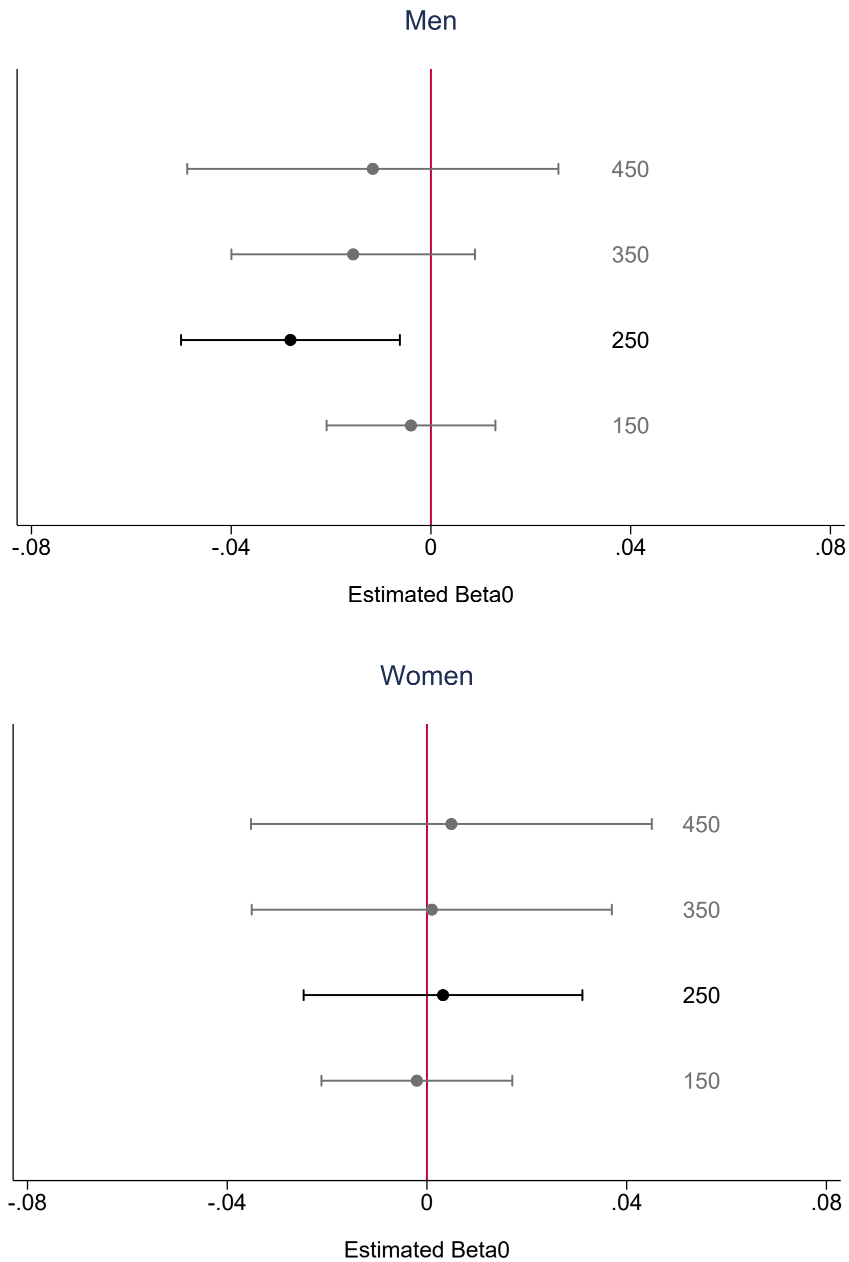
Figure 8: **Placebo cutoffs - above-median-wage occupation**



Source: ASHE, 2012-2019.

Note: This figure presents the estimated effects of placebo policies on the probability of working in occupations paid above the median wage. The placebo cutoff is indicated in correspondence of the estimates. The highlighted estimates represent the actual estimated effect of the policy from regression 1. In each regression, the estimation sample includes firms with $+/-50$ employees from the threshold considered. The top graph refers to men, while the bottom one refers to women. All regressions are estimated using LFS weights. 95 percent confidence intervals are also reported.

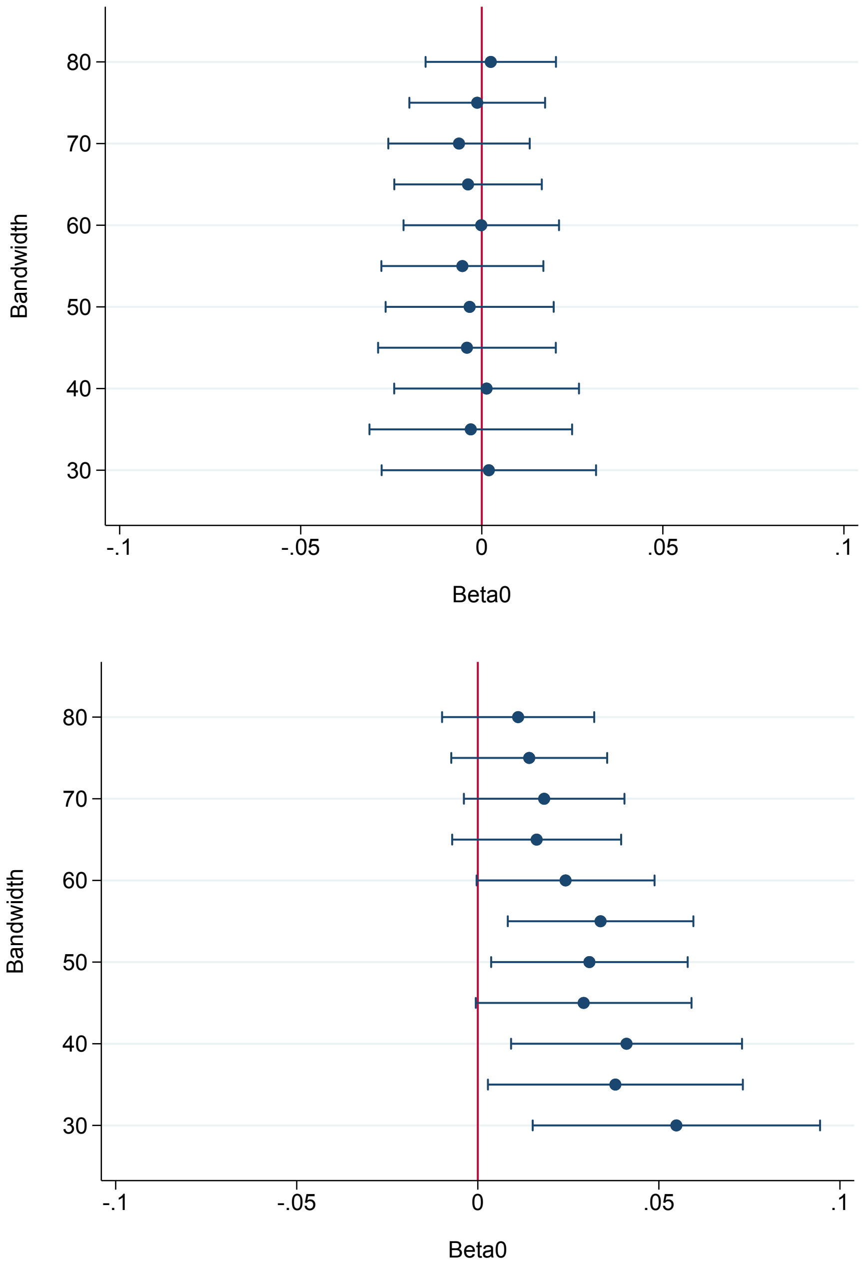
Figure 9: **Placebo cutoffs - log real hourly pay**



Source: ASHE, 2012-2019.

Note: This figure presents the estimated effects of placebo policies on log real hourly pay. The placebo cutoff is indicated in correspondence of the estimates. The highlighted estimates represent the actual estimated effect of the policy from regression 1. In each regression, the estimation sample includes firms with $+/-50$ employees from the threshold considered. The top graph refers to men, while the bottom one refers to women. All regressions are estimated using LFS weights. 95 percent confidence intervals are also reported.

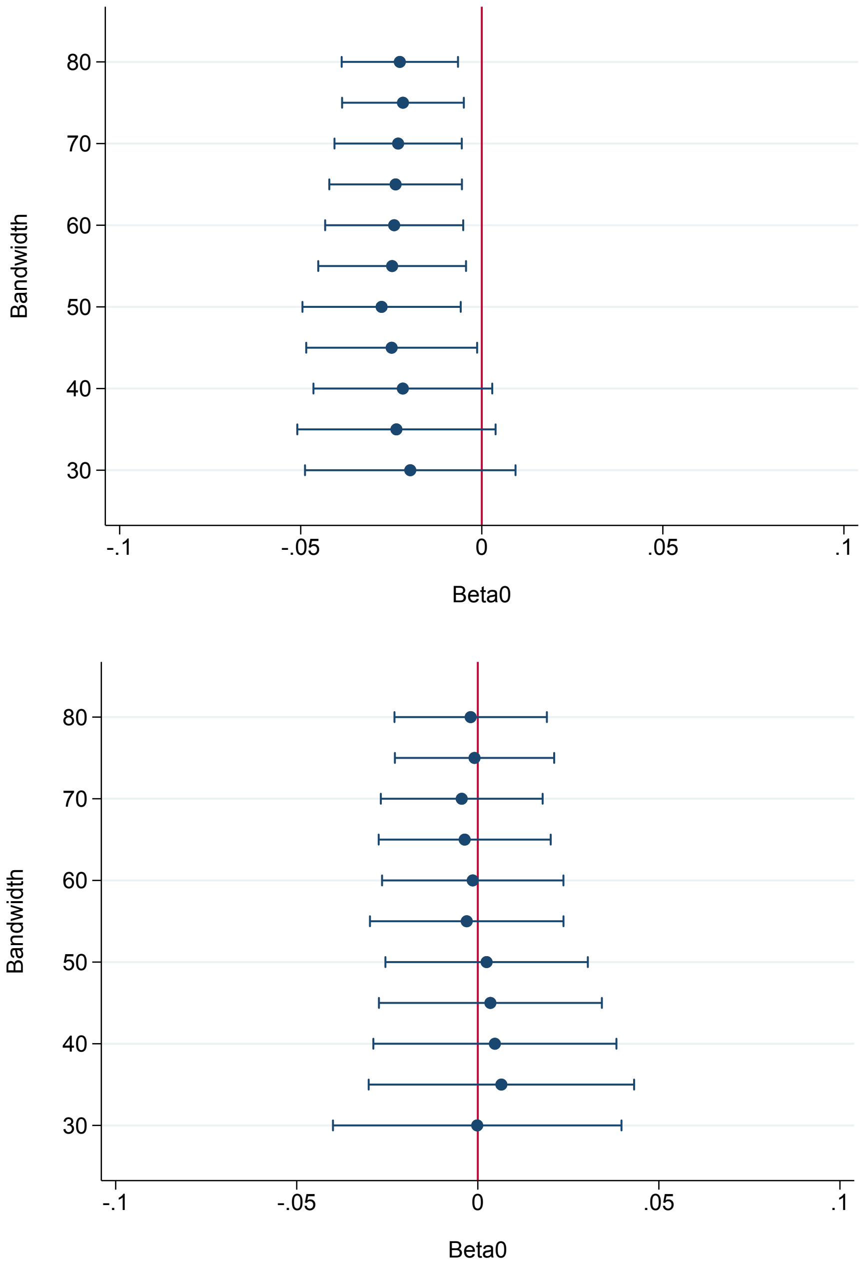
Figure 10: **Varying bandwidth - working in above-median wage occupation**



Source: ASHE, 2012-2019.

Note: This figure shows how the estimates of β_0 from regression 1 change when restricting or enlarging the bandwidth around the 250 cutoff. The outcome considered is the probability of working in occupations paid above the median wage. The top graph refers to men, while the bottom one refers to women. The x-axis reports the estimated coefficients with 95 percent confidence intervals, while the y-axis reports the bandwidth considered, from $+/-30$ to $+/-80$ employees around the policy cutoff. The estimates on the bandwidth of 50 correspond to the main specification. All regressions are estimated using LFS weights. 95 percent confidence intervals are also reported.

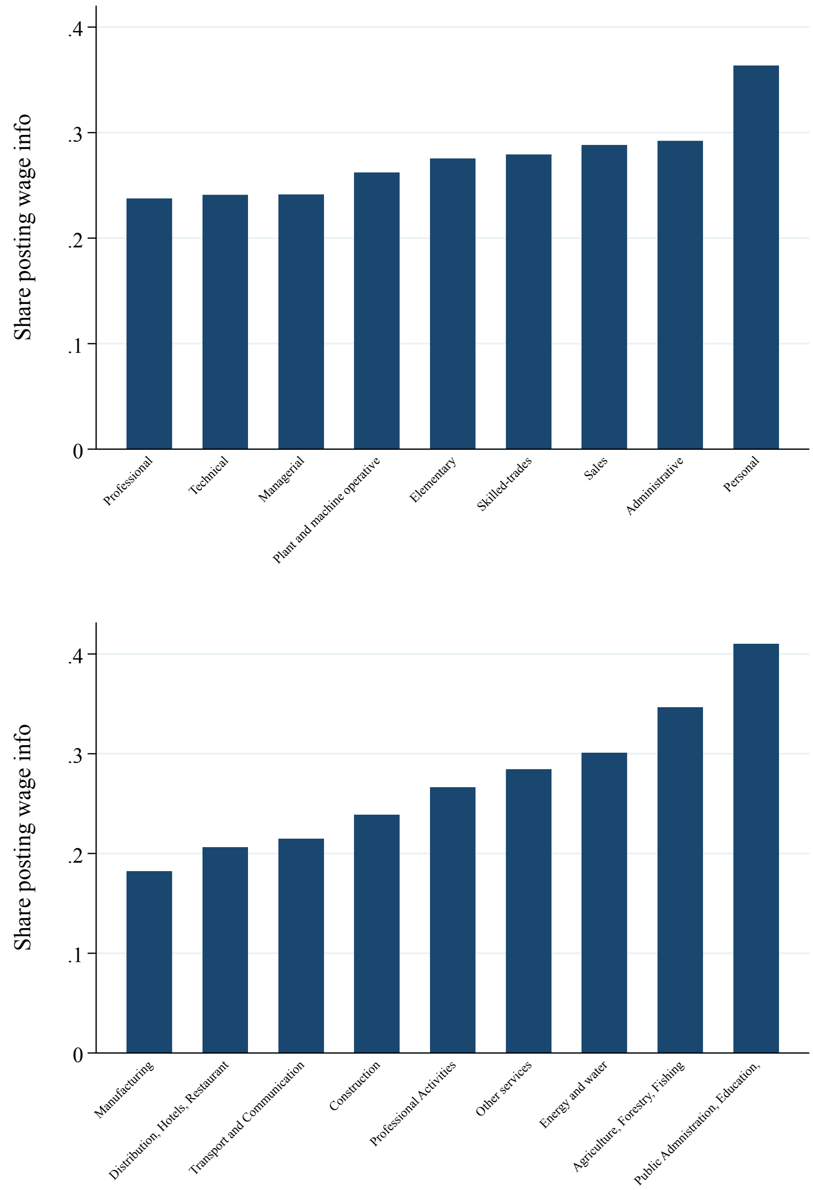
Figure 11: **Varying bandwidth - log real hourly pay**



Source: ASHE, 2012-2019.

Note: This figure shows how the estimates of β_0 from regression 1 change when restricting or enlarging the bandwidth around the 250 cutoff. The outcome considered is log real hourly wage. The top graph refers to men, while the bottom one refers to women. The x-axis reports the estimated coefficients with 95 percent confidence intervals, while the y-axis reports the bandwidth considered, from $+/-30$ to $+/-80$ employees around the policy cutoff. The estimates on the bandwidth of 50 correspond to the main specification. All regressions are estimated using LFS weights. 95 percent confidence intervals are also reported.

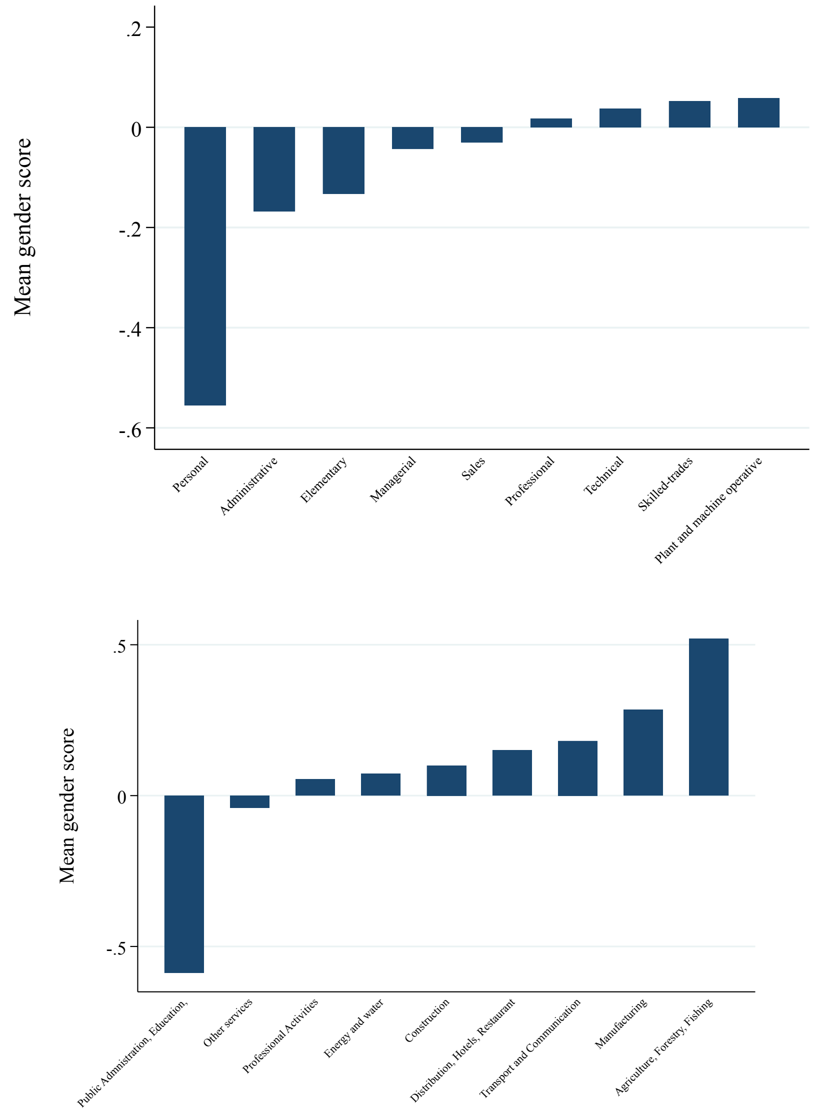
Figure 12: **Wage posting by occupation and industry**



Source: BGT 2012-1019.

Note: These graphs present the occupational and industry distribution of job listing wage posting.

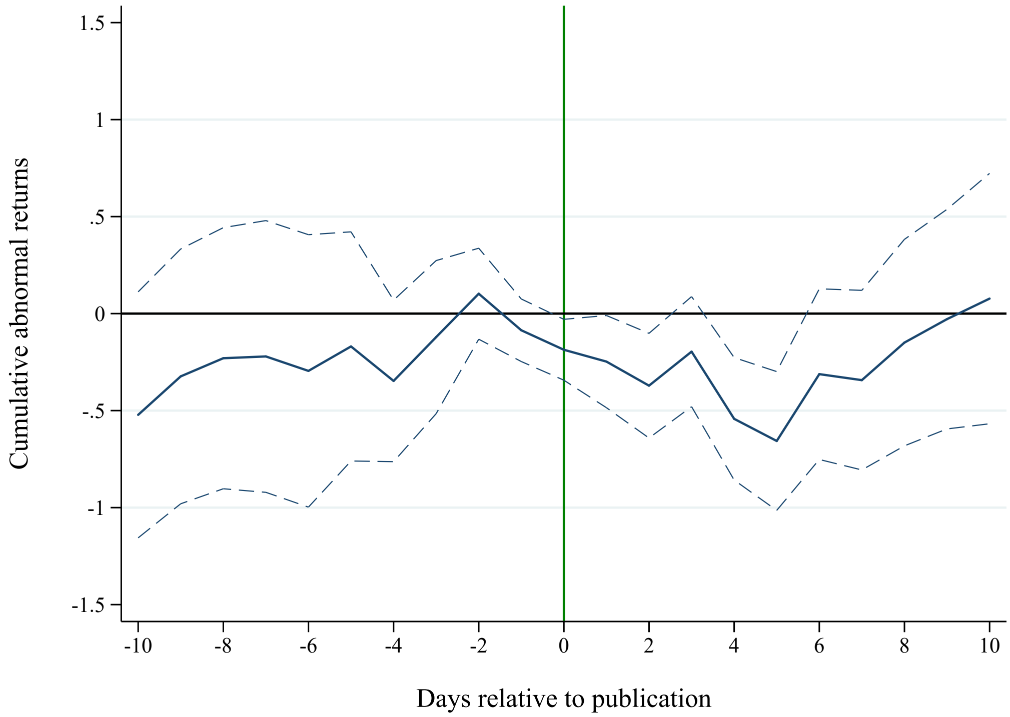
Figure 13: **Gendered score by occupation and industry**



Source: BGT 2012-1019.

Note: These figures present the occupational and industry distribution of job listings' gendered score.

Figure 14: **Cumulative abnormal returns around publication date - 2017 -2018**



Source: Datastream, FAME, GEO.

Note: This figure plots cumulative abnormal returns relative to the publication date of gender pay gap indicators in 2017-2018. In particular, it shows CARS(0, Y), where 0 is the publication date and Y goes from -10 to 10. 95 percent confidence intervals are also included. The sample includes firms that had to publish gender pay gap indicators by April 5th 2018, or that have a subsidiary that have to publish these figures.

Table 1: **GPG public indicators**

	2017-18 (1)	2018-19 (2)	Change (%) (3)
Mean gender hourly pay gap	14.34	14.19	-0.01
	(14.91)	(14.21)	
Median gender hourly pay gap	11.79	11.88	0.01
	(15.84)	(15.51)	
Mean gender bonus gap	7.67	15.44	1.01
	(833.02)	(200.70)	
Median gender bonus gap	-21.71	-0.86	-0.96
	(1,398.97)	(270.51)	
Share men receiving bonus	35.39	35.72	0.01
	(36.33)	(36.68)	
Share women receiving bonus	33.93	34.40	0.01
	(36.02)	(36.38)	
% women lower quartile	53.67	53.88	0.00
	(24.13)	(24.11)	
% women lower-middle quartile	49.49	49.82	0.01
	(26.09)	(26.19)	
% women upper-middle quartile	45.14	45.62	0.01
	(26.22)	(26.32)	
% women top quartile	39.20	39.75	0.01
	(24.41)	(24.48)	
Observations	10,558	10,812	

Source: UK Government Equality Office (GEO).
Notes: This table reports mean values of the indicators published by the firms targeted by the mandate, separately by year. Standard errors reported in parentheses.

Table 2: **ASHE Summary statistics - pre-mandate period**

	Treated men (1)	Control men (2)	Treated women (3)	Control women (4)
Highly-paid occupation	0.69	0.70	0.65	0.64
	(0.46)	(0.46)	(0.48	(0.48
Bottom tercile	0.19	0.18	0.32	0.33
	(0.39)	(0.39)	(0.47)	(0.47)
Middle tercile	0.32	0.32	0.21	0.24
	(0.47)	(0.47)	(0.41)	(0.42)
Top tercile	0.49	0.50	0.46	0.43
	(0.50)	(0.50)	(0.50)	(0.50)
Changed job since last year	0.18	0.20	0.21	0.22
	(0.39)	(0.40)	(0.41)	(0.41)
Tenure in months	86.83	88.01	72.62	72.10
	(96.70)	(98.11)	(78.86)	(80.72)
Leaving the firm in t+1	0.11	0.11	0.09	0.10
	(0.31)	(0.31)	(0.29)	(0.30)
Hourly pay	16.92	16.71	13.89	13.88
	(14.83)	(12.38)	(9.15)	(10.64)
Weekly pay	618.00	610.86	432.55	430.39
	(551.95)	(456.36)	(318.64)	(329.67)
Receiving allowances	0.23	0.22	0.15	0.14
	(0.42)	(0.42)	(0.36)	(0.34)
Allowance amount	18.26	17.06	6.96	7.29
	(70.23)	(57.28)	(26.55)	(37.01)
Allowance amount (per hour)	0.49	0.46	0.23	0.23
	(1.88)	(1.59)	(0.88)	(1.12)
Receiving bonus pay	0.08	0.10	0.05	0.05
	(0.28)	(0.30)	(0.23)	(0.23)
Bonus amount	9.10	11.27	3.48	3.16
	(78.46)	(104.69)	(29.55)	(27.4)2
Bonus amount (per hour)	0.23	0.30	0.12	0.09
	(1.91)	(2.87)	(1.81)	(0.77)
Weekly hours	36.51	36.71	30.86	30.76
	(8.14)	(7.95)	(10.35)	(10.52)
Overtime hours	1.51	1.50	0.55	0.50
	(4.23)	(4.03)	(2.24)	(2.09)
Full-time	0.90	0.91	0.67	0.67
	(0.29)	(0.29)	(0.47)	(0.47)
Private sector	0.90	0.91	0.79	0.77
	(0.30)	(0.29)	(0.41)	(0.42)
Covered by collective agreement	0.28	0.28	0.32	0.34
	(0.45)	(0.45)	(0.47)	(0.47)
Observations	8,350	9,859	6,988	8,706

Source: ASHE, 2012-2016

Notes: This table reports the mean of the main variables used in the analysis separately for men and women, and treatment and control group, before the implementation of the mandate. Standard errors reported in parentheses.

Table 3: **Impact on above-median-wage occupations**

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Men						
Treated Firm*Post	-0.00404	-0.00374	-0.00340	-0.00335	-0.00328	-0.00449
	(0.0120)	(0.0118)	(0.0118)	(0.0118)	(0.0118)	(0.0124)
Observations	24658	24658	24658	24658	24658	22722
Pre-Treatment Mean	0.69	0.69	0.69	0.69	0.69	0.69
Panel B: Women						
Treated Firm*Post	0.0289**	0.0313**	0.0309**	0.0308**	0.0311**	0.0320**
	(0.0142)	(0.0140)	(0.0138)	(0.0138)	(0.0139)	(0.0149)
Observations	21484	21484	21484	21484	21484	18610
Pre-Treatment Mean	0.65	0.65	0.65	0.65	0.65	0.65
Individual controls		✓	✓	✓	✓	✓
Year*Region FE			✓	✓	✓	✓
Region FE			✓	✓	✓	✓
Product Market Concentration				✓		
Industry Trends					✓	
Firm Output						✓
P-value Men Vs Women	0.072	0.053	0.056	0.057	0.057	0.057

Source: ASHE, 2012-2019.

Notes: This table reports the impact of pay transparency on the probability of working in an occupation above the median wage, obtained from the estimation of regression 1. The estimation sample comprises individuals working in firms that have between 200 and 300 employees. Panel A presents results for men, Panel B for women. Each column refers to a different specification, as specified at the bottom of the table. All regressions include firm and year fixed effects. The post dummy is equal to one from 2018 onward. A treated firm is defined as having at least 250 employees in 2015. All regressions are weighted with LFS weights. Heteroskedasticity-robust standard errors clustered at firm level in parentheses. The pre-treatment mean represents the mean of the outcome variables for the treated group between 2012 and 2017. The p-value at the bottom of the table refers to the t-test on the equality of coefficients for men and women (reported in panel A and B).

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 4: **Impact on job mobility**

	Above-median-wage occupation (1)	Changed job since last year (2)	Tenure in months (3)	Leaving the firm in t+1 (4)
Panel A: Men				
Treated Firm*Post	-0.00340	0.0181	-0.990	0.0216
	(0.0118)	(0.0149)	(2.698)	(0.0240)
Observations	24658	24658	23986	21539
Pre-Treatment Mean	0.690	0.18	86.83	0.11
Panel B: Women				
Treated Firm*Post	0.0309**	0.0438***	-7.786***	0.0162
	(0.0138)	(0.0157)	(2.393)	(0.0230)
Observations	21484	21484	20847	18652
Pre-Treatment Mean	0.65	0.21	76.620	0.09
P-value Men Vs Women	0.056	0.224	0.056	0.865

Source: ASHE, 2012-2019.

Notes: This table reports the impact of pay transparency on various occupational outcomes, obtained from the estimation of regression 1. The estimation sample comprises individuals working in firms that have between 200 and 300 employees. Panel A presents results for men, Panel B for women. Each column refers to a different outcomes, as specified in the title of the columns. All regressions include firm, year, region, year-region specific fixed effects and individual controls for age and age squared. A treated firm is defined as having at least 250 employees in 2015. The post dummy is equal to one from 2018 onward. All regressions are weighted with LFS weights. Heteroskedasticity-robust standard errors clustered at firm level in parentheses. The pre-treatment mean represents the mean of the outcome variables for the treated group between 2012 and 2017. The p-value at the bottom of the table refers to the t-test on the equality of coefficients for men and women (reported in panel A and B).

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 5: **Impact on log real hourly pay**

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Men						
Treated Firm*Post	-0.0260**	-0.0259***	-0.0277**	-0.0281**	-0.0274**	-0.0281**
	(0.0114)	(0.00944)	(0.0111)	(0.0112)	(0.0111)	(0.0118)
Observations	24658	24658	24658	24658	24658	22722
Pre-Treatment Mean	16.92	16.92	16.92	16.92	16.92	16.92
Panel B: Women						
Treated Firm*Post	0.00139	0.00138	0.00243	0.00322	0.00261	0.000440
	(0.0143)	(0.0118)	(0.0143)	(0.0143)	(0.0143)	(0.0147)
Observations	21484	21484	21484	21484	21484	18610
Pre-Treatment Mean	13.89	13.89	13.89	13.89	13.89	13.89
Individual FE		✓	✓	✓	✓	✓
Firm* Individual FE		✓				
Year*Region FE			✓	✓	✓	✓
Region FE			✓	✓	✓	✓
Product Market Concentration				✓		
Industry Trends					✓	
Firm Output						✓
P-value Men Vs Women	0.116	0.006	0.082	0.070	0.083	0.113

Source: ASHE, 2012-2019.

Notes: This table reports the impact of pay transparency on log real hourly pay, obtained from the estimation of regression 1. The estimation sample comprises individuals working in firms that have between 200 and 300 employees. Panel A presents results for men, Panel B for women. Each column refers to a different specification, as specified at the bottom of the table. All regressions include firm and year fixed effects. The post dummy is equal to one from 2018 onward. A treated firm is defined as having at least 250 employees in 2015. All regressions are weighted with LFS weights. Heteroskedasticity-robust standard errors clustered at firm level in parentheses.The pre-treatment mean represents the mean of the outcome variables for the treated group between 2012 and 2017. The p-value at the bottom of the table refers to the t-test on the equality of coefficients for men and women (reported in panel A and B).

*** p<0.01, ** p<0.05, * p<0.1.

Table 6: **Impact on different pay measures**

	Log real hourly pay (1)	Log real weekly pay (2)	Weekly hours worked (3)
Panel A: Men			
Treated Firm*Post	-0.0277**	-0.0217*	0.128
	(0.0111)	(0.0127)	(0.226)
Observations	24658	24658	24658
Pre-Treatment Mean	17.92	618.00	36.51
Panel B: Women			
Treated Firm*Post	0.00243	-0.00571	-0.311
	(0.0143)	(0.0188)	(0.398)
Observations	21484	21484	21484
Pre-Treatment Mean	13.89	432.55	30.86
P-value Men Vs Women	0.078	0.480	0.321

Source: ASHE, 2012-2019.

Notes: This table reports the impact of pay transparency on various wage measures, obtained from the estimation of regression 1. The estimation sample comprises individuals working in firms that have between 200 and 300 employees. Panel A presents results for men, Panel B for women. Each column refers to a different outcomes, as specified in the title of the columns. All regressions include year, firm, region, year-region specific and individual fixed effects. The post dummy is equal to one from 2018 onward. A treated firm is defined as having at least 250 employees in 2015. All regressions are weighted with LFS weights. Heteroskedasticity-robust standard errors clustered at firm level in parentheses.The pre-treatment mean represents the mean of the real hourly pay, the real weekly pay and the weekly hours for the treated group between 2012 and 2017. The p-value at the bottom of the table refers to the t-test on the equality of coefficients for men and women (reported in panel A and B).

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 7: **Impact on log real hourly pay - different pay components**

	Log real hourly pay (1)	Log real hourly basic pay (2)	Allowances (per hour) (3)	Incentive pay (per hour) (4)
Panel A: Men				
Treated Firm*Post	-0.0277**	-0.0257**	-0.0329	-0.00306
	(0.0111)	(0.0113)	(0.0249)	(0.0196)
Observations	24658	24658	24658	24658
Pre-Treatment Mean	16.92	16.6	0.49	0.23
Panel B: Women				
Treated Firm*Post	0.00243	0.00709	-0.0140	-0.0286
	(0.0143)	(0.0137)	(0.0256)	(0.0196)
Observations	21484	21484	21484	21484
Pre-Treatment Mean	13.89	13.29	0.23	0.12
P-value Men Vs Women	0.082	0.052	0.588	0.359

Source: ASHE, 2012-2019.

Notes: This table reports the impact of pay transparency on various wage measures, obtained from the estimation of regression 1. The estimation sample comprises individuals working in firms that have between 200 and 300 employees. Panel A presents results for men, Panel B for women. Each column refers to a different outcomes, as specified in the title of the columns. All regressions include year, firm, region, year-region specific and individual fixed effects. The post dummy is equal to one from 2018 onward. A treated firm is defined as having at least 250 employees in 2015. All regressions are weighted with LFS weights. Heteroskedasticity-robust standard errors clustered at firm level in parentheses. The pre-treatment mean represents the mean of the real hourly wages, the real hourly basic pay, the real hourly allowances and real hourly incentives for the treated group between 2012 and 2017. The p-value at the bottom of the table refers to the t-test on the equality of coefficients for men and women (reported in panel A and B).

*** p<0.01, ** p<0.05, * p<0.1.

Table 8: **Diff-in-Diff vs Triple Diff-in-Diff**

	Above-median-wage occupation			Log real hourly pay		
	Men	Women	Triple Diff	Men	Women	Triple Diff
	(1)	(2)	(3)	(4)	(5)	(6)
Treated Firm*Post	-0.00340	0.0309**	-0.00814	-0.0277**	0.00243	-0.0255**
	(0.0118)	(0.0138)	(0.0125)	(0.0111)	(0.0143)	(0.0114)
Treated Firm*Post*Female			0.0399**			0.0264
			(0.0194)			(0.0174)
Post*Female			-0.0285**			-0.0191
			(0.0120)			(0.0117)
Treated Firm*Female			-0.0951			-0.131
			(0.0136)			(0.119)
Observations	24658	21484	46142	24658	21484	46142
P-value Women			0.031			0.953

Source: ASHE, 2012-2019.

Notes: Columns 1 to 3 report the impact of pay transparency on the probability of working in an occupation above the wage median. Columns 4 to 6 report the impact of pay transparency on log real hourly pay. The estimation sample comprises individuals working in firms that have between 200 and 300 employees. All regressions control for firm and year fixed effects. Columns 1 to 3 also include age and age squared. Columns 4 to 6 also include individual fixed effects. A treated firm is defined as having at least 250 employees in 2015. All regressions are weighted with LFS weights. Heteroskedasticity-robust standard errors clustered at firm level in parentheses. The pre-treatment mean represents the mean of the real hourly pay for the treated group between 2012 and 2017. The p-value at the bottom of the table refers to the t-test on the effect for women in the triple difference-in-difference regression (Treated Firm*Post+Treated Firm*Post*Female) .

*** p<0.01, ** p<0.05, * p<0.1.

Table 9: **Diff-in-Diff vs Diff-in-Disc**

	Above-median-wage occupation			Log real hourly pay		
	Diff-in-Diff	Diff-in-Disc		Diff-in-Diff	Diff-in-Disc	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Men						
Treated Firm*Post	-0.00340	-0.00244	-0.00339	-0.0277**	-0.0201	-0.0259*
	(0.0118)	(0.0147)	(0.0147)	(0.0111)	(0.0148)	(0.0147)
Observations	24658	24658	24658	24658	24658	24658
Pre-Treatment Mean	0.69	0.69	0.69	16.92	16.92	16.92
Panel B: Women						
Treated Firm*Post	0.0309**	0.0322*	0.0322*	0.00243	-0.00221	-0.00978
	(0.0138)	(0.0169)	(0.0169)	(0.0143)	(0.0178)	(0.0178)
Observations	21484	21484	21484	21484	21484	21484
Pre-Treatment Mean	0.65	0.65	0.65	13.89	13.89	13.89
Year FE	✓	✓		✓	✓	
Region	✓			✓		
Year*Region FE	✓			✓		
Post			✓			✓
Norm. Firm Size*Post		✓	✓		✓	✓
Norm. Firm Size*Treated Firm*Post		✓	✓		✓	✓
Individual controls	✓	✓	✓	✓	✓	✓
P-value Men Vs Women	0.056	0.013	0.104	0.082	0.425	0.470

Source: ASHE, 2012-2019.

Notes: Columns 1 to 3 report the impact of pay transparency on the probability of working in an occupation above the wage median. Columns 4 to 6 report the impact of pay transparency on log real hourly pay. The estimation sample comprises individuals working in firms that have between 200 and 300 employees. Panel A presents results for men, Panel B for women. All regressions include firm fixed effects. In columns 1 to 3, the individual controls comprise age and age squared. In columns 4 to 6, the individual controls include individual fixed effects. A treated firm is defined as having at least 250 employees in 2015. All regressions are weighted with LFS weights. Heteroskedasticity-robust standard errors clustered at firm level in parentheses. The pre-treatment mean represents the mean of the real hourly pay for the treated group between 2012 and 2017. The p-value at the bottom of the table refers to the t-test on the equality of coefficients for men and women (reported in panel A and B).

*** p<0.01, ** p<0.05, * p<0.1.

Table 10: **Changing year to define treatment status**

	Above-median-wage occupation				Log real hourly pay			
	2015	2014	2013	2012	2015	2014	2013	2012
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Men								
Treated Firm*Post	-0.00340	-0.00973	-0.00273	-0.00273	-0.0277**	-0.0163	-0.0199*	-0.0369***
	(0.0118)	(0.0128)	(0.0128)	(0.0135)	(0.0111)	(0.0112)	(0.0114)	(0.0120)
Observations	24658	24586	24476	24239	24658	24586	24476	24239
Pre-Treatment Mean	0.69	0.70	0.69	0.69	16.92	16.80	17.01	17.09
Panel B: Women								
Treated Firm*Post	0.0309**	0.0501***	0.0394***	0.0235	0.00243	-0.00770	-0.00518	-0.00777
	(0.0138)	(0.0149)	(0.0148)	(0.0152)	(0.0143)	(0.0154)	(0.0150)	(0.0149)
Observations	21484	21310	21097	20746	21484	21310	21097	20746
Pre-Treatment Mean	0.65	0.65	0.65	0.65	13.89	13.90	13.92	13.77
Individual controls	✓	✓	✓	✓	✓	✓	✓	✓
Year*Region FE	✓	✓	✓	✓	✓	✓	✓	✓
P-value Men Vs Women	0.06	0.19	0.03	0.00	0.08	0.10	0.16	0.63

Source: ASHE, 2012-2019.

Notes: This table compares the impact of pay transparency on the main outcomes, when the treatment status is defined using different pre-policy years. The first four columns refer to the outcome "Working in above-median wage occupations", while the last four columns present the results for the outcome log real hourly pay. For each outcome, the column name indicates the year used to define treatment status. Panel A presents results for men, Panel B for women. In all regressions, the estimation sample comprises individuals working in firms that have between 200 and 300 employees. All regressions include firm and year times region fixed effects. Individual controls include age and age squared in columns 1-4, and individual fixed effects in columns 5-8. The post dummy is equal to one from 2018 onward. A treated firm is defined as having at least 250 employees in the year indicated on top of the column. All regressions are weighted with LFS weights. Heteroskedasticity-robust standard errors clustered at firm level in parentheses.The pre-treatment mean represents the mean of the outcome variables for the treated group between 2012 and 2017. The p-value at the bottom of the table refers to the t-test on the equality of coefficients for men and women (reported in panel A and B).

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 11: **Raw correlation wage posting and gender pay gap indicators**

	Mean Gender Pay Gap (1)	Share of women at the top (2)
Panel A: 2017-2018		
Wage Posted	-0.0824***	0.164***
	(0.00767)	(0.0123)
Observations	4671	4671
Panel B: 2018-2019		
Wage Posted	-0.0789***	0.157***
	(0.00709)	(0.0124)
Observations	4660	4660

Source: GEO, BGT 2012-2019.

Notes: This table shows the raw correlation between GEO firms wage posting decisions and their gender pay gap indicators. Panel A refers to the publication year 2017-2018, while Panel B refers to the publication year 2018-2019. In each panel, the sample includes the GEO firms that have been matched with BGT entries with a name-matching score of at least 0.8. Wage posted represents the share of a firm's vacancies that contain wage information over the period 2012-2019.

*** p<0.01, ** p<0.05, * p<0.1.

Table 12: **Raw correlation gendered score and gender pay gap indicators**

	Mean Gender pay gap (1)	Share of women at the top (2)
Panel A: 2017-2018		
Gendered Score	0.884***	-12.81***
	(0.301)	(0.449)
Observations	4671	4671
Panel B: 2018-2019		
Gendered Score	0.528*	-12.86***
	(0.275)	(0.444)
Observations	4660	4660

Source: GEO, BGT 2012-2019.
Notes: This table shows the raw correlation between GEO firms gendered score and their gender pay gap indicators. Panel A refers to the publication year 2017-2018, while Panel B refers to the publication year 2018-2019. In each panel, the sample includes the GEO firms that have been matched with BGT entries with a name-matching score of at least 0.8. The gendered score represents the average gendered score per company between 2012-2019.
*** p<0.01, ** p<0.05, * p<0.1.

Table 13: **Impact on wage posting**

	Entire sample	Low-gpg	High-gpg
	(1)	(2)	(3)
Treated Firm*Post	0.0410	-0.0222	0.0968**
	(0.0357)	(0.0429)	(0.0439)
Observations	91366	36401	54943
Pre-Treatment Mean	0.330	0.290	0.350

Source: BGT, GEO, FAME 2012-2019.

Notes: This table reports the impact of pay transparency on firms' wage posting decisions, obtained from the estimation of regression 3. The estimation sample comprises firms that have between 200 and 300 employees. All regressions include year, firm, occupation, and occupation times year fixed effects. The post dummy is equal to one from 2018 onward. A treated firm is defined as having at least 250 employees in the pre-treatment period. High-gpg industries are those with a gender pay gap above the across-industry median in the pre-treatment period. These include manufacturing, construction, banking and finance, and public administration, education and health sectors. Heteroskedasticity-robust standard errors clustered at firm level in parentheses.The pre-treatment mean represents the mean of the outcome variable for the treated group between 2012 and 2017.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 14: **Impact on gendered score**

	Entire sample (1)	Low (2)	High (3)
Treated Firm*Post	-0.000141	0.0864	-0.168
	(0.0736)	(0.0957)	(0.116)
Observations	91366	54591	36758
Pre-Treatment Mean	-0.0600	-0.230	0.170

Source: BGT, GEO, FAME 2012-2019.
Notes: This table reports the impact of pay transparency on firms' wording in job listings, obtained from the estimation of regression 3. The estimation sample comprises firms that have between 200 and 300 employees. All regressions include year, firm, occupation, and occupation times year fixed effects. The post dummy is equal to one from 2018 onward. A treated firm is defined as having at least 250 employees in the pre-treatment period. High-score industries include agriculture, forestry and fishing, manufacturing, transport and communication, and distributive sectors. Heteroskedasticity-robust standard errors clustered at firm level in parentheses.The pretreatment mean represents the mean of the outcome variable for the treated group between 2012 and 2017.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 15: **Cumulative abnormal returns relative to publication date**

	CAR(0, 5) (1)	CAR(-5, -1) (2)
Constant	-0.657***	-0.169
	(0.217)	(0.358)
Observations	405	405

Source: Datastream, FAME, GEO.
Notes: This table shows the estimates of the cumulative abnormal returns around the publication of gender pay gap indicators. The sample includes firms that have to publish gender pay gap indicators by April 5th 2018, or that have a subsidiary that have to publish these figures.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 16: **CAR(0,5) around submission**

	(1)	(2)	(3)
Group-avg GPG performance negative	-1.724	-1.496	-1.581
	(1.059)	(1.106)	(1.118)
Group-avg GPG performance	0.315**	0.300**	0.290**
	(0.128)	(0.138)	(0.138)
Group-avg perf.*group-avg perf. negative	-0.298**	-0.283**	-0.273*
	(0.130)	(0.139)	(0.140)
Constant	0.754	0.630	-0.507
	(1.058)	(1.133)	(1.817)
Observations	405	405	383
Industry FE		✓	✓
Other controls			✓

Source: Datastream, FAME, GEO.

Notes: This table shows the estimates of the cumulative abnormal returns around the publication of gender pay gap indicators. The sample includes firms that have to publish gender pay gap indicators by April 5th 2018, or that have a subsidiary that have to publish these figures. Other controls in column 3 include the log of market capitalization, price to book value ratios, and the return on assets.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Appendix

A Robustness checks on ASHE analysis

Table A1: **Impact on occupations in each wage tercile**

	Top (1)	Middle (2)	Bottom (3)
Panel A: Men			
Treated Firm*Post	0.00787	-0.00979	0.00192
	(0.0137)	(0.0131)	(0.0104)
Observations	24658	24658	24658
Pre-Treatment Mean	0.49	0.32	0.19
Panel B: Women			
Treated Firm*Post	-0.0206	0.0456***	-0.0251*
	(0.0155)	(0.0139)	(0.0138)
Observations	21484	21484	21484
Pre-Treatment Mean	0.46	0.21	0.32

Source: ASHE, 2012-2019.

Notes: This table reports the impact of pay transparency on occupations in each wage tercile, obtained from the estimation of regression 1. The estimation sample comprises individuals working in firms that have between 200 and 300 employees. Panel A presents results for men, Panel B for women. Each column refers to a different outcomes, as specified in the title of the columns. All regressions include year, firm, region, year-region specific and individual controls for age and age squared. A treated firm is defined as having at least 250 employees in 2015. The post dummy is equal to one from 2018 onward. All regressions are weighted with LFS weights. Heteroskedasticity-robust standard errors clustered at firm level in parentheses. The pre-treatment mean represents the mean of the outcome variables for the treated group between 2012 and 2017.

*** p<0.01, ** p<0.05, * p<0.1.

Table A2: **Treatment status based on past vs actual firm-size**

	Above-median-wage occupation		Log real hourly pay	
	Main specification	Actual	Main specification	Actual
	(1)	(2)	(3)	(4)
Panel A: Men				
Treated Firm*Post	-0.00340	-0.0159	-0.0277**	-0.00235
	(0.0118)	(0.0118)	(0.0111)	(0.0114)
Observations	24658	25256	24658	25256
Pre-Treatment Mean	0.69	0.68	16.92	16.84
Panel B: Women				
Treated Firm*Post	0.0309**	0.0149	0.0126	-0.00617
	(0.0138)	(0.0139)	(0.0143)	(0.0151)
Observations	21484	22111	21484	22111
Pre-Treatment Mean	0.65	0.64	13.89	13.93
Individual controls	✓	✓	✓	✓
Year*Region FE	✓	✓	✓	✓

Source: ASHE, 2012-2019.

Notes: This table compares the results of our specifications with those we would obtained if treatment status was based on actual firm size. The first two columns refer to the outcome "Working in above-median wage occupations", while the last two columns present the results for the outcome log real hourly pay. For each outcome, the column name indicates the year used to define treatment status. Panel A presents results for men, Panel B for women. In all regressions, the estimation sample comprises individuals working in firms that have between 200 and 300 employees. All regressions include firm and year times region fixed effects. Individual controls include age and age squared in columns 1-2, and individual fixed effects in columns 3-4. The post dummy is equal to one from 2018 onward. In column 1, a treated firm is defined as having at least 250 employees if it is above this threshold in 2015, while in the second column a firm is treated whenever it has at least 250 employees. All regressions are weighted with LFS weights. Heteroskedasticity-robust standard errors clustered at firm level in parentheses. The pre-treatment mean represents the mean of the outcome variables for the treated group between 2012 and 2017. The p-value at the bottom of the table refers to the t-test on the equality of coefficients for men and women (reported in panel A and B).

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A3: **Impact on above-median-wage occupations - changing the estimation sample**

	Main specification		Age 25+		Age 16-65	
	With	Without	With	Without	With	Without
	LFS weights		LFS weights		LFS weights	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Men						
Treated Firm*Post	-0.00340	-0.00202	-0.00393	-0.00271	-0.00170	0.00161
	(0.0118)	(0.0129)	(0.0121)	(0.0133)	(0.0121)	(0.0132)
Observations	24658	24658	21895	21895	24146	24146
PreTreatmentMean	0.69	0.60	0.71	0.63	0.69	0.61
Panel B: Women						
Treated Firm*Post	0.0309**	0.0313**	0.0347**	0.0337**	0.0266*	0.0273*
	(0.0138)	(0.0143)	(0.0138)	(0.0143)	(0.0139)	(0.0144)
Observations	21484	21484	18922	18922	21116	21116
Pre-Treatment Mean	0.65	0.61	0.69	0.65	0.65	0.61

Source: ASHE, 2012-2019.

Notes: This table reports the impact of pay transparency on the probability of working in an occupation above the median wage, obtained from the estimation of regression 1. The estimation sample comprises individuals working in firms that have between 200 and 300 employees. Panel A presents results for men, Panel B for women. Each column refers to a different specification, as specified at the top of each column. All regressions include firm, year, region, year-region specific fixed effects and individual controls for age and age squared. The post dummy is equal to one from 2018 onward. A treated firm is defined as having at least 250 employees in 2015. Heteroskedasticity-robust standard errors clustered at firm level in parentheses. The pre-treatment mean represents the mean of the outcome variables for the treated group between 2012 and 2017.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A4: **Impact on log hourly wages - changing the estimation sample**

	Main specification		Age 25+		Age 16-65	
	With	Without	With	Without	With	Without
	LFS weights		LFS weights		LFS weights	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Men						
Treated Firm*Post	-0.0277**	-0.0268**	-0.0202*	-0.0193**	-0.0267**	-0.0258**
	(0.0111)	(0.0105)	(0.0104)	(0.00984)	(0.0112)	(0.0105)
Observations	24658	24658	21895	21895	24146	24146
Pre-Treatment Mean	16.92	15.82	17.96	16.74	16.95	15.88
Panel B: Women						
Treated Firm*Post	0.00243	0.00243	0.00569	0.00537	-0.000606	-0.000985
	(0.0143)	(0.0140)	(0.0147)	(0.0143)	(0.0145)	(0.0142)
Observations	21484	21484	18922	18922	21116	21116
Pre-Treatment Mean	13.89	13.40	14.70	14.10	13.91	13.43

Source: ASHE, 2012-2019.

Notes: This table reports the impact of pay transparency on log real hourly wage, obtained from the estimation of regression 1. The estimation sample comprises individuals working in firms that have between 200 and 300 employees. Panel A presents results for men, Panel B for women. Each column refers to a different specification, as specified at the top of each column. All regressions include firm, year, region, year-region specific fixed effects and individual fixed effects. The post dummy is equal to one from 2018 onward. A treated firm is defined as having at least 250 employees in 2015. Heteroskedasticity-robust standard errors clustered at firm level in parentheses. The pre-treatment mean represents the mean of the outcome variables for the treated group between 2012 and 2017.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A5: **Impact on above-median-wage occupations - different clustering**

	S.E. clustered at the level of:		
	firm	firm-size	firm-size* industry
	(1)	(2)	(3)
Panel A: Men			
Treated Firm*Post	-0.00340	-0.00340	-0.00340
	(0.0118)	(0.0106)	(0.0105)
Observations	24658	24658	24658
Pre-Treatment Mean	0.660	0.660	0.660
Panel B: Women			
Treated Firm*Post	0.0309**	0.0309***	0.0309**
	(0.0138)	(0.0115)	(0.0124)
Observations	21484	21484	21484
Pre-Treatment Mean	0.610	0.610	0.610
Number of clusters	4639	101	655
P-value Men Vs Women	0.0578	0.026	0.035

Source: ASHE, 2012-2019.

Notes: This table reports the impact of pay transparency on the probability of working in an above-median-wage occupation, obtained from the estimation of regression 1. The estimation sample comprises individuals working in firms that have between 200 and 300 employees. Panel A presents results for men, Panel B for women. Each regressions uses different clustering groups for the standard errors as specified at the top of each column. All regressions include firm, year, region, year-region specific fixed effects and individual controls for age and age squared. A treated firm is defined as having at least 250 employees in 2015. The post dummy is equal to one from 2018 onward. All regressions are weighted with LFS weights.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A6: **Impact on log real hourly wages - different clustering**

	S.E. clustered at the level of:		
	firm	firm-size	firm-size* industry
	(1)	(2)	(3)
Panel A: Men			
Treated Firm*Post	-0.0277**	-0.0277***	-0.0277***
	(0.0111)	(0.00891)	(0.00870)
Observations	24658	24658	24658
Pre-Treatment Mean	17.630	17.630	17.630
Panel B: Women			
Treated Firm*Post	0.00243	0.00243	0.00243
	(0.0143)	(0.0114)	(0.0107)
Observations	21484	21484	21484
Pre-Treatment Mean	13.850	13.850	13.850
Number of clusters	4639	101	655
P-value Men Vs Women	0.082	0.041	0.028

Source: ASHE, 2012-2019.

Notes: This table reports the impact of pay transparency on log real hourly wages, obtained from the estimation of regression 1. The estimation sample comprises individuals working in firms that have between 200 and 300 employees. Panel A presents results for men, Panel B for women. Each regressions uses different clustering groups for the standard errors as specified at the top of each column. All regressions include year, firm, region, year-region specific and individual fixed effects. A treated firm is defined as having at least 250 employees in 2015. The post dummy is equal to one from 2018 onward. All regressions are weighted with LFS weights.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

B Gendered score

Table B1: **Words used for the gendered score**

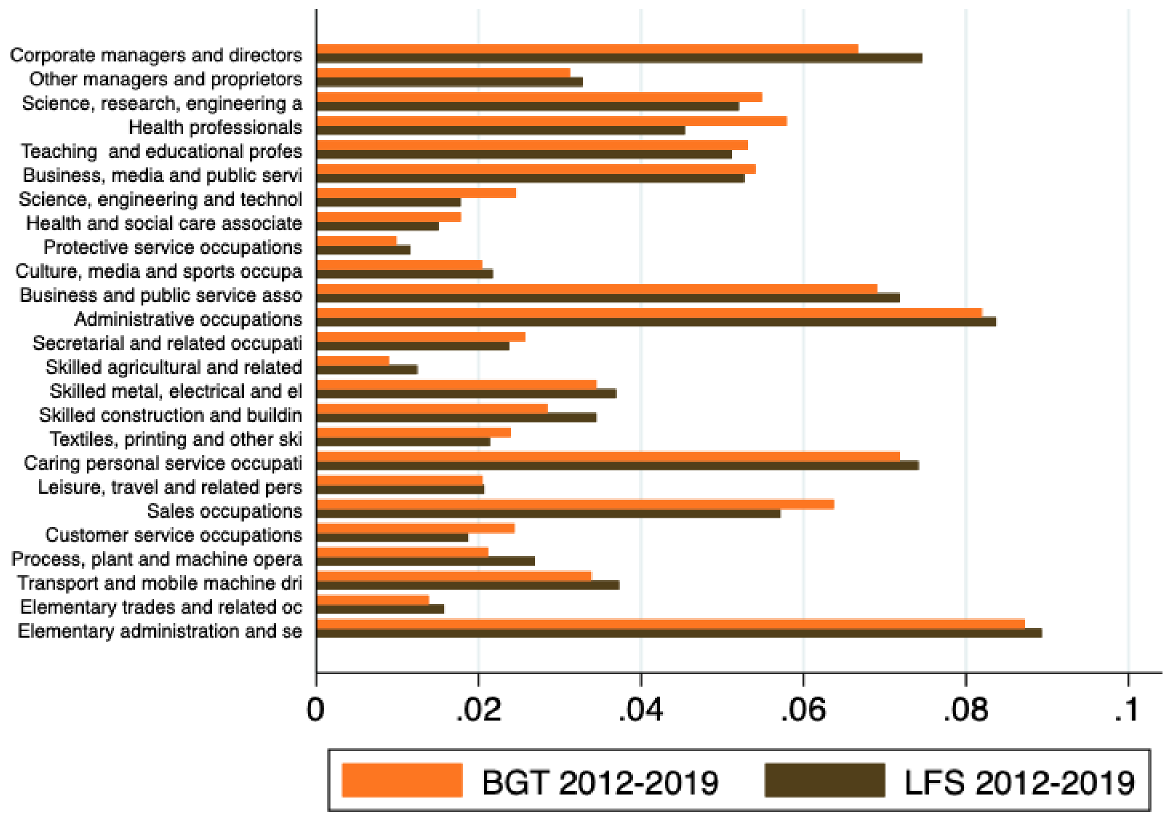
Masculine words		Feminine words	
active	decisive	affectionate	modesty
adventurous	determination	cheered	nag
aggression	determined	cheerful	nurture
aggressive	dominant	cheers	nurtured
aggressiveness	dominate	cheery	nurtures
aggressor	domination	childlike	nurturing
ambitious	domineering	children	pleasantly
ambitiousness	forced	childrens	polite
analysing	forceful	commitment	quietly
analysing	greedy	committed	respond
analysis	headstrong	committing	responsibility
analyst	hierarchical	communal	responsible
analytical	hierarchy	compassionate	responsive
asserting	hostile	connected	responsively
assertive	hostility	connecting	sensitive
asserts	impulsive	connections	sensitivity
athlete	individualistic	considerate	submissive
athletic	intellectual	cooperating	supported
athleticism	leader	cooperative	supporting
autonomous	leading	dependable	supports
autonomy	logic	depending	sympathetic
boasted	masculine	emotional	sympathy
boaster	objective	empathetic	tenderly
boasting	opinion	empathic	togetherness
challenged	outspoken	empathy	trusted
challenger	persist	feminine	trusting
challenging	principled	flatterable	trusts
compete	reckless	gentle	understanding
competence	self-reliance	honest	understands
competent	self-reliant	interdependence	warming
competing	self-sufficiency	interdependent	warmly
competitive	self-sufficient	interpersonal	warms
confident	stubborn	interpersonal	whine
courage	superior	interpersonally	whining
courageous		kind	yielded
decide		kinship	yielding
decision		loyally	yields
decisions		loyalty	

Source: Based on Gaucher et al. (2011).

Notes: This table presents the words used to construct the gendered score.

C Burning Glass Techonologies

Figure C1: **Occupational distribution in BGT and LFS**



Source: BGT and LFS, 2012-2019.
Note: This figure compares the occupational distribution in the stock of vacancies in BGT and across employed individuals in the LFS.

C.1 Name matching algorithm

Due to the large number of job vacancy postings, we used a combination of techniques to match individual job vacancy postings to firm-level data from FAME or the GEO list directly. We first collapsed all firm names in each data set down to a unique set of firm names using standard text cleaning procedures. We identified any exact matches between firm names in postings and our firm-level data set, giving these a match score of unity. We matched the remaining N firm names from the vacancy postings with the universe of official firm names, with M unique entries, using a combination of techniques provided in the scitkit-learn software package (**?**). First, the vacancy firm names are expressed as character-level 2- and 3-grams with a maximum of 8,000 features, creating a matrix T with dimensions (number of postings) X (number of features). The 8,000 features define a vector space that we used to express the official firm names in too, with a matrix G. Matching directly with these matrices would require NXM inner products of 8,000 dimensional vectors. Instead, we created a reduced vector space of just 10 dimensions using truncated singular value decomposition on T, creating a reduced dimension matrix $\hat{T}$ and expressing G as $\hat{G}$ in the reduced space. The vectors representing $\hat{G}$ and $\hat{T}$ were then sorted into 500 clusters using k-means, providing an associated cluster for each firm name on both sides of the matching problem. For each cluster c_i with $i \in \{1, 500\}$ the problem was reduced to finding matches between $c_i(N) \leq N$ and $c_i(M) \leq M$ entries - where the equality holds for at most one of the clusters respectively (and rarely holds in practice). Within each cluster, we computed all of the pair-wise cosine similarities between $c_i(T)$ and $c_i(G)$; i.e. within a cluster, and with features indexed by f, the matches for T are found by solving

$$\arg\max_{m} \{T_{nf} \cdot G_{fm}\}$$

The score is the cosine similarity of the matched vectors scaled by 0.99 (to distinguish exact matches from exact-in-the-vector-space matches). Exact matches were found for 39% of the unique firm names in vacancy postings, and over 50% of matches had a score of 0.8 or greater.