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Pay Transparency and Cracks in the Glass Ceiling*

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Abstract

This paper studies firms' and employees' responses to pay transparency requirements. Each year since 2018, more than 10,000 UK firms have been required to disclose publicly their gender pay gap and gender composition along the wage distribution. Theoretically, pay transparency is meant to act as an information shock that alters the bargaining power of male and female employees vis-à-vis the firm in opposite ways. Coupled with the potential negative effects of unequal pay on firms' reputation, this shock could improve women's relative occupational and pay outcomes. We test these theoretical predictions using a difference-in-differences strategy that exploits variations in the UK mandate across firm size and time. This analysis delivers four main findings. First, pay transparency increases women's probability of working in above-median-wage occupations by 5 percent compared to the pre-policy mean. Second, while this effect has not yet translated into a significant rise in women's pay, the policy leads to a 2.8 percent decrease in men's real hourly pay, reducing the pre-policy gender pay gap by 15 percent. Third, combining the difference-in-differences strategy with a text analysis of job listings, we find suggestive evidence that treated firms adopt female-friendly hiring practices in ads for high-gender-pay-gap occupations. Fourth, a reputation motive seems to drive employers' reactions, as firms publishing worse gender equality indicators score lower in YouGov Women's Rankings. Moreover, publicly listed firms experience a 35-basis-point average fall in cumulative abnormal returns in the days following their publication of gender equality data.

JEL codes: J08, J16, J24.
Keywords: pay transparency; gender pay gap; glass ceiling.

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1 Introduction

The 5th of April 2018 was the first deadline for more than 10,000 UK firms to publish gender equality statistics. Before then, fewer than 3 percent of UK firms had ever publicly disclosed this information (Downing et al. 2015). The following day, newspapers widely covered these results. The second deadline fell in April 2019 and again drew significant media attention (*BBC* 2018, *The Guardian* 2018, *Financial Times* 2018, *Financial Times* 2019).[1]

While the UK is the only country in which some companies are required to disclose their gender pay gaps publicly, many governments are adopting pay transparency policies with the aim of improving gender equality.[2] The underlying hypothesis is that neither employers nor employees may fully realize the extent of gender disparities in their firm, or in competing ones. By making this apparent, pay transparency is meant to act as an information shock that alters the bargaining power of male and female employees vis-à-vis the firm in opposite ways (Cullen and Perez-Truglia 2018b, Cullen and Pakzad-Hurson 2019). Coupled with the potential negative effects of unequal pay on firms' reputation, pay transparency incentivizes targeted firms to hire more women in better paid positions, and discourages the promotion of male employees (Johnson 2020). In turn, this could translate into improved pay and occupational 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 must publish a series of indicators on a dedicated government website, including percentage mean and median gender hourly pay differentials, and the percentage of women in each quartile of the wage distribution.

We begin our analysis by studying the impact of this policy on occupational and pay out-

[1]In mid-March 2020, just two weeks before the publication deadline, the requirement to publish gender equality data was temporarily paused due to the Coronavirus outbreak. At that time, only half of firms had published their data (*Financial Times* 2020).

[2]Following the recommendations of the European Commission, Austria, Denmark, Italy, and Germany introduced transparency laws, for instance. See (Aumayr-Pintar, 2018) for a summary of European pay transparency policies. Though pay transparency requirements are less common in the United States, many states have prohibited employers from imposing pay secrecy clauses on their employees (Siniscalco et al. 2017).

comes of male and female workers using the UK matched-employer-employee data set (the Annual Survey of Hours and Earnings, or ASHE) from 2012 to 2019. To identify causal effects, we adopt a difference-in-differences 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 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.

This analysis delivers two main findings. First, in line with the theoretical predictions, the mandate increases the probability that women are employed in above-median-wage occupations by 3 percentage points (p.p. hereafter), or 5 percent relative to the pre-policy mean, closing half of the gender gap on this margin. The effect seems to be driven by newly hired women, and movements from the bottom to the middle of the wage distribution. Second, as the theory predicts, pay transparency reduces gender pay differentials, but, remarkably, this is achieved through pay compression from above: the mandate triggers a 2.8 percent decrease in male real hourly wages in treated firms relative to control firms, while the change in women's occupational composition has so far failed to translate into a visible increase in their salaries. Further results suggest that the reduction in men's pay comes from a combination of nominal cuts at the top of the wage distribution and pay freezes in lower paid occupations. In turn, this results in a 15 percent decrease in the gender pay gap, relative to an unconditional pre-policy level of 18 percent.

To complement these findings, we also study the impact of the policy on employees' retention and productivity. Recent evidence shows that disclosing information on peers' salaries may hurt job satisfaction and increase the job search intentions of low-paid employees (Card et al. 2012, Breza et al. 2018, Cullen and Perez-Truglia 2018a, Dube et al. 2019, Perez-Truglia 2020). At the same time, if firms respond to pay transparency requirements by promoting gender equality, a more egalitarian environment may increase employees' retention and productivity (Bennedsen et al. 2019). By combining ASHE with the Business Structure Database, a company survey covering 99 percent of UK firms, we find that, at least in the short run, neither of these two forces prevails.

Event-study exercises show that our results do not capture pre-policy differential trends in the outcomes of interest between treated and control groups. Additional robustness checks 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-differences regressions that account for within-treatment-group time shocks common to male and female employees. Second, difference-in-discontinuities specifications that control for firm-size specific time shocks deliver the same results as our difference-in-differences model. Third, we find no significant effect of placebo regressions, estimated by pretending that the policy binds at different firm size thresholds. 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 used to define treatment status.

To delve into the mechanisms driving the estimated effects, we follow two directions. First, we analyze firms' hiring practices to understand whether and how targeted firms have tried to attract more women. Second, we study the role played by reputation in influencing firms' responses to the mandate.

To study firms' recruitment practices, we conduct a text analysis of UK job listings, using Burning Glass Technologies (BGT) data from 2014/15 to 2018/19. We study three dimensions of hiring practices in BGT job ads: the wording of the job description, the offer of flexible working arrangements, and whether ads post a wage. First, we explore how these dimensions correlate with firms' gender equality indicators, and document three novel stylized facts. While gender-targeted job ads have been banned in many countries, psychology literature suggests that women are less likely to apply for jobs with male-oriented job postings, i.e., with a vocabulary that is usually associated with men in implicit association tests (Rudman and Kilianski 2000, Gaucher et al. 2011). Consistent with this evidence, we find that firms which, on average over the period observed, tend to use a female-oriented language, have also a larger percentage of women at the top of the wage distribution. The economic research has also shown that gender differences in preferences for temporal flexibility play a key role in explaining gender occupational segregation (Bertrand et al. 2010, Goldin 2014, Wiswall and Zafar 2018, Cortes and Pan 2019). Many studies also document a gender gap in bargaining skills in favor of men (Babcock et al. 2003, Bowles et al. 2007, Leibbrandt and List 2015). In accordance with these two strands of the literature, we find that firms that tend to offer flexible work arrangements and post upfront wage information, have also more women at the top of the wage distribution and a lower gender pay gap. Given these stylized facts, in a second step, we combine the text analysis with the difference-in-differences strategy, and find suggestive evidence that treated firms become more likely to use a more female-oriented wording, offer flexible work arrangements, and post wage information in ads for high-gender-pay-gap occupations.

To analyze the importance of the reputation motive, we first exploit the YouGov Women's Rankings, which collect information on women's impressions of around 1500 brands since 2017/18. Of these, around 1000 firms publish gender equality indicators. By tracking firms' rankings for the two years following the introduction of the pay transparency policy, we document that worse gender equality indicators are associated with a lower firm reputation score among women. This suggests that the reputation motive may partly explain why targeted firms have acted to reduce gender inequality, which is consistent with the recent literature on the disciplinary effects of information provision (Perez-Truglia and Troiano 2015, Luca 2018, Johnson 2020).

To further investigate this hypothesis, and fully understand the impact of this policy on targeted firms, we study how the stock market reacts to the publication of gender equality indicators by firms listed on the London Stock Exchange (around 10 percent of firms targeted by the mandate and one third of the listed firms). This analysis reveals that, in the first year of the mandate, firms' 3-day cumulative abnormal returns decrease by around 35 basis points following the publication of gender equality data. While this effect fades away after four days, it is consistent with investors expecting that pay transparency will inflict reputational damage on targeted firms.

Overall, this paper provides several contributions to different strands of literature. First, our study contributes to the analysis of policies aimed at tackling gender disparities in the labor market. As measures such as paternity leave and gender quotas have been proven to have a negligible impact so far, it is especially important to assess the role of other interventions, including pay transparency (Ekberg et al. 2013, Antecol et al. 2018, Bertrand et al. 2019, Wasserman 2019). Notably, relative to policies that prescribe a specific action, pay transparency puts on employers the burden (and the flexibility) of identifying the underlying causes of gender inequality. For this reason, analyzing a broad range of firms' and employees' responses is necessary to assess the implications of this policy, and this paper serves precisely this aim.

Second, our paper adds to the growing number of studies from the economic and management literature analyzing the impact of pay transparency policies on personnel management and the gender pay gap (Mas 2017, Baker et al. 2019, Bennedsen et al. 2019, Burn and Kettler 2019, Blundell 2020, Gulyas et al. 2020). The closest studies to ours are Baker et al. (2019), Bennedsen et al. (2019), Blundell (2020), and Gulyas et al. (2020). 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 Bennedsen et al. (2019) and Gulyas et al. (2020) analyze the effect on the gender pay gap of, respectively, a 2006 Danish law and a 2011 Austrian law, mandating that private firms provide employees with pay data 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 legislation has two unique features that could help improve our understanding of the effects of pay transparency. First, it mandates 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 not only on gender differentials, but also on their own gender average pay (Bennedsen et al. 2019). 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 its potential disciplinary effect on firms' behavior.

In parallel to our work, Blundell (2020) also analyzes the effects of the UK pay transparency using ASHE, but only focuses on wages of full-time workers aged 25 to 50 and uses the current number of employees to define treatment status. Similar to us, he finds a 2 p.p. narrowing of the gender pay gap in affected firms. Relative to his work, our paper looks at the impact of the transparency policy on wages of all workers, unpacks this effect by looking at different wage components, and investigates compositional effects. Second, it adopts a more conservative identification strategy that uses the pre-policy firm size to define treatment status. Third, while Blundell (2020) conducts an interesting survey with workers to investigate whether wage effects are explained by firms' recruitment and retention concerns, our paper directly looks at employees' retention in ASHE and firms' hiring practices using BGT data. Finally, our study offers a comprehensive analysis of the impact of the pay transparency policy that includes its effects on labor productivity and stock prices. These are economic outcomes that are important per sé, and also help explain the wage effects.[3]

Our third contribution is to the growing strand of economics papers that use job advertisement data to study the dynamics of the labor market, from the evolution of skill requirements to labor market concentration (Deming and Kahn 2018, Adams et al. 2020, Azar et al. 2020a, Azar et al. 2020b). To the best of our knowledge, this is the first paper to document a correlation between firms' hiring practices and the magnitude of firms' gender pay gaps, and to study how this relationship is affected by pay transparency requirements. As such, our paper complements contemporary work on the impact of pay history inquiry bans on recruitment practices (Sran et al. 2020). Our analysis of the relationship between gendered wording and gender equality specifically adds to the growing number of papers studying implicit biases in job postings (Gaucher et al. 2011, Tang et al. 2017, Burn et al. 2019), and complements studies analyzing discrimination in gender-targeted jobs (Kuhn and Shen 2013, Kuhn et al. 2018). More broadly, this part of the analysis relates to recent work on the relationship between linguistic traits and gender equality (Gay et al. 2018, Galor et al. 2020).

Finally, our analysis of the stock market reaction to the publication of gender equality indi-

[3] Another complementary study to ours is Gamage et al. (2020) who compare wage trajectories of male and female professors employed in UK Russell Group universities before and after the introduction of a pay transparency policy in the university sector in 2007. Interestingly, the study finds that the log of salaries of female academics increased by around 0.62 percentage points compared to male counterparts following the introduction of the policy, corresponding to a 4.37 percent reduction of the gender pay gap.

cators shows that investors consider gender equality to be a relevant aspect of personnel practices. Despite its importance, as far as we are aware, only Ahern and Dittmar (2012) and Greene et al. (2020) have considered this topic, by analyzing the response of the stock market to the introduction of gender quotas in, respectively, Norwegian and American firms' boards.

The paper proceeds as follows. Section 2 describes the institutional setting and the UK transparency policy. Section 3 introduces our conceptual framework for studying the effects of pay transparency. Section 4 discusses the identification strategy. Section 5 describes the data used in the empirical analysis. Section 6 presents the main results. Section 7 illustrates the robustness checks. Section 8 discusses the potential mechanisms behind the main results, focusing in particular on firms' hiring practices, and the importance of the reputation motive. Section 9 presents the analysis of the stock market reaction. Section 10 concludes by discussing the welfare implications of our results.

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 requires all firms registered in Great Britain that have at least 250 employees to publish gender equality indicators both on their own website and on a dedicated website managed by the Government Equalities Office (GEO hereafter).[4],[5]

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

[5]The mandate applies to both private and public sector; however, the public sector was already subject to some

The timing of publication works as follows: if a firm has at least 250 employees by the end of a financial year (April), it has to provide gender equality indicators by the end of the following financial year. Firms themselves must calculate their number of employees, using guidelines provided by the government. Importantly, they have to adopt an extended definition of employee that includes agency workers. Partners of firms are also included in the definition of employees, but should not be included 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; and 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 prior to 2020 in which firms had to publish them. 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.[6] The proportion 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 proportion of women is smaller in the upper part of the distribution. Yet, this proportion increases by around 1 percent over the two years. Finally, Figure 1 also shows that the

transparency measures. According to regulations introduced in 2011, public bodies in England with over 150 employees are 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 male and female employees broken down by pay level. Public authorities are also required to make arrangements for identifying and collecting (but not necessarily publishing) information about differences between the pay of people with protected characteristics such as gender or ethnicity. Where a difference can be linked to a protected characteristic, public authorities are required to set equality objectives to address the causes of such differences. Finally, Scottish public organizations with 20 or more employees have been required to publish information on the gender pay gap since 2012.

[6] 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.

mean gender hourly pay gap is larger in firms that have a lower percentage 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 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 the disclosure of pay gaps. As of 2020, all firms targeted by the law were deemed to have complied.[7] Figure 2 reports the distribution of submission dates for the two years the mandate has been in place. While some firms 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 conducted 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 when 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.

[7]Note that only 235 firms with less than 250 employees published gender equality indicators in 2018. These represent less than 0.1 percent of UK active firms in this size range in 2018, according to figures taken from the Business Structure Database. This tiny percentage is consistent with the hypothesis that firms are reluctant to disclose information on employees' pay if they are not forced to do so (Siniscalco et al. 2017).

3 Conceptual framework

Advocates of pay transparency policies argue that one of the reasons why gender inequality persists in the labor market is because it is hidden (Cullen and Perez-Truglia 2018b, Baker et al. 2019). While aggregate statistics on the gender pay gap are widely available, as the GEO survey mentioned above shows, neither employers nor employees may be fully aware of the extent of gender disparities in their own firm, or in competing ones (Downing et al. 2015).[8] In this case, pay transparency acts as an information shock both within and outside the firm,[9] with the public aspect of the shock being especially important in the UK in light of the salience of the policy (Cullen and Perez-Truglia 2018b).

From the point of view of female workers, this shock may strengthen their bargaining power vis-à-vis the firm for two reasons. First, the value of their outside options increases, as the relative expected profit of filling a job with a female worker rather than a male one is now higher for employers; second, they may feel entitled to ask for pay increases under pay equality legislation, though this channel may be less relevant in a context characterized by low trade-union coverage such as the UK.

As for men, transparency should have the opposite but potentially symmetric effects on their outside options. In addition, by forcing employers to reveal what they are willing to pay at most, the policy weakens men' chances to ask for wage increases (Cullen and Pakzad-Hurson 2019).[10]

Finally, pay transparency has several implications for firms. First, it has ambiguous effects on workers' productivity and retention. On the one hand, in accordance with the "fair wage-effort hypothesis" (Akerlof and Yellen 1990), the information revealed may decrease the job satisfaction of lower-paid employees, while the higher-paid may feel threatened by any attempt by the firm

[8] One can argue that employers may actively seek to hide the extent of the issue in their own firm if they fear the potential consequences of revealing it, including pressure for pay rises from low-paid employees, negative effects on job satisfaction and productivity, and reputational costs. This is consistent with employment contracts imposing pay secrecy, and the growing trend among governments to issue pay secrecy bans (Kim 2015, Burn and Kettler 2019).

[9] In principle, individuals' expectations may influence the magnitude and direction of the shock (Dube et al. 2019). In the absence of good data to measure these expectations, we do not analyze this source of heterogeneity in this paper.

[10] This channel is especially important under full transparency, while it may be less relevant in a context where only information on the gender pay gap is disclosed.

to mitigate inequality (Card et al. 2012, Breza et al. 2018). In addition, the public disclosure of gender equality indicators induces comparisons across firms.[11] Taken together, pay comparisons among employees within and outside the firm may lower overall labor productivity and cause more employees to quit. On the other hand, if firms respond to the policy by improving gender equality, this could boost the productivity and retention of those workers who care about working in an fair environment (Bennedsen et al. 2019).

The second implication for firms is that the public disclosure of gender equality indicators may create reputational costs, which could be especially high for publicly-listed firms (Anderson and Magruder 2012, Perez-Truglia and Troiano 2015, Luca 2018, Johnson 2020). Importantly, employers face these costs irrespective of the underlying reasons for the degree of gender inequality in their firm, whether it is due discrimination, the structure of the firm's activity, or an insufficient supply of skilled women. In other words, pay transparency puts the burden (and the flexibility) on employers of identifying the underlying causes of gender inequality. At the same time, imposing the publication of both gender pay gaps and the female share along the wage distribution helps to shape employers' incentives towards reducing pay differentials by hiring more women in better-paid positions. Yet, it cannot prevent alternative reactions, such as achieving pay compression from the top part of the wage distribution, rather than raising salaries of the lower-paid employees.

In what follows, we will test these theoretical predictions, by studying the impact of the UK policy on occupational and pay outcomes of male and female workers, labor productivity, and firms' hiring practices.[12] Moreover, we will study the role played by the reputation motive in shaping firms' responses, and the implications of the policy for the stock prices of the publicly-listed firms.

[11] In light of the literature analyzing behavioral responses to feedback on relative performance, a dimension that would be interesting to consider is how firms respond to the policy depending on their initial degree of gender equality (Allcott and Kessler 2019, Azmat et al. 2019). Unfortunately, this cannot be done with the data at hand, as we cannot construct a firm-level baseline measure of gender pay gaps.

[12] Note that firms' monopsony power can play an important role in influencing their response to the policy (Dube et al. 2019). However, in this paper, we are not going to consider this dimension for two reasons. First, recent evidence shows that sector-level labor market concentration is low in the UK (Abel et al. 2018). Second, the data at hand limit our ability to study heterogeneous effects along this dimension.

4 Identification strategy

To identify the impact of the UK transparency policy on occupational outcomes, wages, and firm-level outcomes, we exploit the variation across firm size and over time in its implementation. Specifically, we estimate a difference-in-differences 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.[13] To enhance comparability between treatment and control group, in the main specification we consider firms with $+/-50$ employees from the 250 threshold. As both choices can be considered arbitrary, 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. When studying employees' outcomes, our baseline regression model is as follows:

$$Y_{ijt} = \alpha_j + \theta_t + \beta \left(TreatedFirm_j * Post_t\right) + X_{it}'\pi + Z_{jt}'\delta + u_{ijt}, \tag{1}$$

where i is an employee working in firm j, having 200-300 employees, in year t, running between 2012 and 2019.[14] 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.[15] 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 2018 onward. The vector X_{it}

[13] Appendix Figure A1 shows the distribution of firms around the 250 cutoff in each year since the introduction of the mandate. Data are drawn from the Business Structure Database. While a McCrary test performed separately for each year does not reject the null that there is no jump at the cutoff, it seems cautious to define treatment status based on pre-policy firm size.

[14] As explained in section 5, we choose this time window because it is the maximum number of years over which we observe all outcomes of interest.

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

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 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. In all regressions, we use UK Labor Force Survey weights, though in the appendix we show that our results do not depend on this choice. Standard errors 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. Finally, as our hypothesis is that this policy will affect men and women differently, we will estimate each 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.

The validity of our identification strategy depends on three assumptions. First, it has to satisfy the parallel-trend assumption, that is the evolution of the outcomes of interest must be comparable in treated and control firms prior to the introduction of the policy. Second, our estimates should not capture the effect of other time shocks coinciding with the introduction of pay transparency and affecting firms on each side of the 250-employees cutoff differently. Third, the results should not depend on the size of the bandwidth considered around the policy cutoff, nor should they depend on the year chosen to define the treatment status.

To support the validity of the parallel-trend assumption and study the dynamic impact of pay transparency, we will open the discussion of our main findings by illustrating the results of the following event-study exercises:

$$Y_{ijt} \;=\; \alpha_j + \theta_t + \sum_{k=2012}^{2019} \beta_k (TreatedFirm_j * \mathbf{1}[t = k]) + X'_{it}\pi + Z'_{jt}\delta + u_{ijt}, \qquad (2)$$

where $\mathbf{1}[t = k]$ is an indicator variable that takes value 1 when t = k and 0 otherwise. In what follows, we take 2017, the year prior to the introduction of pay transparency, as reference year.

Next, section 7 will provide evidence in favor of the other two conditions and further support for the parallel-trend assumption.

5 Data

To study the overall effect of this government mandate on the outcomes of interest, we use of several sources of data, including individual-level data on pay and occupational outcomes, firm-level data on labor productivity, job vacancies and stock prices. Here we first introduce the data used to measure employees' outcomes. For this, we rely on 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.[16] 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 of which are measured in April. Other variables relating to age, occupation and industrial classification are also available. Once workers enter the survey, they are followed even when changing employer, though individuals are not observed when unemployed or out of the labor force. In practice, ASHE is an unbalanced panel data set at the employee level. Importantly, it also provides the number of employees in a firm and year, the crucial information to define the treatment status in our identification strategy.[17]

From ASHE, we create the following variables. First, to measure occupational outcomes and

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

[17]If none of the employees of a firm is interviewed in ASHE in the year used to define the treatment status, we cannot assign a treatment status to this firm and need to exclude it from the estimation sample. To recover the information on the number of employees for these firms, which represent 25 percent of our sample, we use the Business Structure Database. However, in section 7 we show that our results are not affected if these firms are excluded from the estimation sample.

worker flows, we proceed as follows. We construct 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, professional and managerial occupations. For brevity, we refer to this outcome as the probability of working in above-median-wage occupations. We then consider 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 use months of tenure in the firm, though this is missing for around 3 percent of the estimation sample. And, finally, we construct a 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; however, we also consider log basic real hourly wage, bonuses and allowances separately. To study the impact of the policy on bonuses and allowances, we use the inverse hyperbolic sine transformation to take into account the fact that many workers do not receive any bonus or allowance. Finally, we consider log real weekly pay, and weekly hours worked, distinguishing between contractual hours and overtime.

In the empirical analysis, we use data over the period 2012-2019. We chose this time window mainly because the SOC occupational classification changes in 2010, and the variables that follow the new classification are only available from 2012 onward in ASHE.

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 6 percent gender gap in the probability of working in above-median-wage occupations. Next, the unconditional hourly pay gap amounts to 18 percent. There is also a large gender gap in the probability of receiving allowances or bonuses (35 and 33 percent respectively), and a huge gap 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 which limits the possibility to study heterogeneous effects between public and private sector employees. Finally, it is worth noting that among both men and women, only one third of workers are covered by a collective agreement. This figure is important to consider when thinking about the mechanisms through which the policy may affect wages and occupational outcomes. In principle, pay transparency may induce women, 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' responses.

6 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. Finally, we discuss the impact of the policy on labor productivity.

6.1 Occupational outcomes and job mobility

Figure 4 introduces the analysis on occupational outcomes by reporting the estimates of the β_k from regression 2 on the probability of working in an above-median-wage occupation. The top graph reports the event study for men, while the bottom one refers to women. The year 2017 is taken as the reference year. Also note that these regressions include region-specific time effects to account for shocks to the local labor market where the firm operates and the individual works. We can observe two things from these figures. First, the evolution of this variable in the pre-policy period is comparable across treatment and control groups, both for male and female employees. Second, while the top graph shows that the male occupational distribution has not been affected by the policy, the bottom one indicates that treated firms have gradually changed the composition of their female workforce after the introduction of the policy, by increasing the share of women in above-median-wage occupations. Appendix Figure A2 also shows the raw trends of this outcome, confirming that the effect on women comes from the treated group, rather than the control group.

Table 3 presents the average effect of the policy, obtained from the estimation of regression

1. Panel A refers to 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 and in line with theoretical predictions, the mandate increases women's probability of working in above-median-wage occupations by 3 p.p. - 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 change little. Column 3 further includes year times region fixed effects to control for local labor market specific time shocks, and once again the results are little affected.[18] Columns 4 to 6 add different 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 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.

Importantly, in Appendix Table A1, we show that the effect on women's occupational distribution seems to be driven by a increase in women's probability of working in occupations in the middle tercile of the wage distribution - administrative and skilled trade occupations - and by a contemporaneous decrease in their likelihood of working in low-paid occupations - personal services, sales, elementary, and plant and machine-operative occupations. Note that breaking the wage distribution into smaller groups may lower the reliability of our estimates, as each group contains fewer individuals. However, these results suggest that the policy starts change women's occupational prospects, but so far it has failed to bring women in top-paid occupations.

Table 4 further 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

[18]We consider NUTS2 regions here, corresponding to 11 areas in the UK.

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 $t + 1$. According to the results in columns 2 and 3, pay transparency increases women's probability of having changed firm, and consequently decreases average tenure in the firm.[19] This strongly suggests that the positive impact on women's occupational outcomes comes from the newly hired women.[20] Column 4 shows instead that the policy has no effect on the probability of leaving the firm for either men or women.

Note that this last result is also consistent with the theoretical predictions introduced in section 3. In particular, it suggests that none of the contrasting effects that this policy can have on employees' retention prevails in this context. To complement these results, in section 6.3, we also describe the impact of this policy on labor productivity.

To conclude the discussion on this table, note that, as the policy does not affect men's occupational outcomes or job mobility, 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 precisely measure the share of women in treated and control firms both before and after the introduction of pay transparency legislation.

6.2 Wages

Figure 5 shows the event studies for the variable log real hourly pay. As above, 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 real hourly pay in the pre-policy period seems to be

[19]Figure A3 shows the corresponding event studies. As in the case of the probability of working in above-median wage occupations, almost all the leads of the reform are insignificant, supporting the parallel-trend assumption. As for the lags, the effect on the probability of having changed firm is is imprecisely estimated in the first year, while it is significant both in the first and second year for the case of tenure in the firm. Note that this may explain why the average effect on months of tenure seems larger than the impact on job mobility.

[20]Appendix Figure A4 investigates where movers come from, by plotting the size distribution of the previous firm for both men and women, in treated and control groups, before and after the introduction of the mandate. For all these groups, the distribution simply resembles the usual firm-size distribution. Focusing on treated women, this seems to rule out specific patterns of poaching across firms, but we will return to this point in section 7 when showing how the results evolve when enlarging the bandwidth to select the estimation sample.

comparable across treatment and control groups, both for male and female employees. Second, the top graph shows that male real hourly pay remarkably drops 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 corresponding average effects. As above, Panel A refers to 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 2.6 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's 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: as indicated by the p-value of the t-test on the equality of coefficients for men and women, this policy leads to a significant reduction of the gender pay gap, amounting to around 15 percent of the pre-policy mean.[21] However, what is especially striking is that the reduction of the gender pay gap is achieved through pay compression from above. While in section 3 we anticipated that the UK policy leaves room for this type of reaction, such an effect definitely deserves more explanations.

We begin with the effect on women's pay. In light of the results on occupational outcomes, we may have expected to see an increase in women's wages. Different factors may explain why this effect has not clearly materialized. First, note that if the changes in women's occupational prospects come from movements from the bottom to the middle of the wage distribution, this may

[21] 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 pence. 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.

result into little wage effects. Second, both treated and control firms may have decided to raise women's wages if they are competing for the same workers. Yet, in Appendix Figure A5, 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. Third, firms may have only increased wages of newly hired women. Appendix Table A2 explores this hypothesis by comparing the impact of the policy on wages of workers with at most two years of tenure and those with more than two years of tenure.[22] While we do not have enough power to detect significant effects, point estimates in column 2, Panel B suggest that firms have indeed started to increase wages of recently hired women.[23] Finally, an alternative explanation to account for the null effect on women's pay may have to do with compensating differentials. If firms have attracted more women by offering flexible work arrangements, these may have accepted such positions even without any wage increase. In section 8 we focus precisely on this mechanism. Note that this may further explain why firms have acted on men's wages to reduce the gender pay gap.

To better understand the impact on male workers' pay, Tables 6 and 7 further unpack the effects on hourly wages. First, Table 6 shows that weekly pay, rather than hours worked, is the margin of adjustment for men. Second, Table 7 shows that the changes brought by the policy are mainly due to contractual wages rather than allowances and bonuses.[24] Taken together, these results imply that the slowdown of male real hourly pay comes from either a cut or freeze in men's nominal wages. Newspapers reported cuts in CEOs' salaries following the introduction of pay transparency.[25] Consistent with this anecdotal evidence, Appendix Figure A7 shows that from 2018 nominal hourly contractual wages have decreased in treated firms compared to control ones.

[22]Note that, when analyzing the impact of the policy by subgroup, we exclude region-specific time shocks from the regression to avoid running out of degrees of freedom.

[23]Note also from Panel A that the effects on men's wages seem larger in magnitude for workers employed in the firm for more than two years, though the results are not statistically different across subgroups. This suggest that firms have in part spared newly hired men from wage compression, potentially to keep attracting talented men.

[24]Appendix Figure A6 reports the event studies for log weekly pay and log basic hourly pay, showing similar dynamics to those seen for log hourly pay.

[25]According to the New York Times, when pay transparency got introduced in the UK, Johan Lundgren, easyJet's chief executive, took a 4.6 percent pay cut to match the salary of his female predecessor (*New York Times* 2018). Similarly, in January 2018, The Guardian reported that "six high-profile male presenters have already agreed to pay cuts, including John Humphrys, Jeremy Vine and Nick Robinson" (*The Guardian* 2018).

Realistically, it is likely that firms opted for both cuts of high wages and a freeze of salaries further down in men's pay distribution. Appendix Table A3 supports this hypothesis by showing that the point estimates of pay effects are larger in magnitude for men working in above-median-wage occupations, though they are not statistically different from effects on workers in below-median-wage professions.

Overall, this second set of results confirms the theoretical prediction that pay transparency induces firms to address gender pay differentials, but also highlights the importance of considering the potential welfare implications of this policy. We will come back to this point in section 10, when presenting the concluding remarks of the paper.

6.3 Labor productivity

To measure labor productivity, we rely on the Business Structure Database (BSD).[26] The BSD provides information on firm output and employment for almost 99 percent of business organizations in the UK. The data are reported as of April of each year, and come from the Inter-Departmental Business Register (IDBR), a live register of data collected by the tax authorities (HM Revenue and Customs) via VAT and employee tax records.[27] From the BSD, we construct a measure of labor productivity as the ratio between firm output and employment, where this is set to firm size in 2015. As BSD is currently available until 2018, so far we only study the impact of the policy of this outcome in the first year of implementation. Moreover, we conduct this part of the analysis by running regression 1 at firm level.

Table 8 reports the results of different specifications, as we have done for employee-level outcomes. Consistent with the hypothesis that pay transparency negatively affects employees' job satisfaction, the point estimates suggest that the UK policy has decreased labor productivity (Oswald et al. 2015). However, the coefficients are insignificant in all columns. As in the case of

[26]Office for National Statistics. (2019). Business Structure Database, 1997-2018: Secure Access. [data collection]. 10th Edition. UK Data Service. SN: 6697, http://doi.org/10.5255/UKDA-SN-6697-10.

[27]If a business is liable for VAT (turnover exceeds the VAT threshold) and/or has at least one member of staff registered for the Pay-as-You-Earn tax collection system, then it will appear on the IDBR (and hence in the BSD). As a result, only very small businesses do not appear in the IDBR.

retention rates, we will continue monitoring this outcome as new data become available to study the potential ambiguous effects of this policy on this dimension.

7 Robustness checks

Parallel-trend assumption. Tables 9 and 10 show that our estimates change little when progressively restricting the pre-treatment period. In particular, while the impact on men's hourly pay becomes just marginally insignificant when we leave only 3 years of pre-treatment period, the magnitude of the coefficient is very similar across the different columns. As for women's probability of working in above-median-wage occupations, the effect remains similar and significant across the different specifications. This exercise further supports the hypothesis that we are not capturing the impact of differential pre-trends between treated and control group.

Contemporaneous shocks. To make sure that our estimates do not capture the effect of other phenomena occurring at the same time as the introduction of pay transparency requirements and affecting treated and control firms differently, we perform three robustness checks. First, Table 11 compares the estimates from the difference-in-differences model to those of the following triple-differences model with the gender dimension as the third difference:

$$\begin{aligned} Y_{ijt} \;=\;& \alpha_j + \theta_t + \beta\,(TreatedFirm_j * Post_t) \\ & + Fem_i[\gamma_0 + \gamma_1 TreatedFirm_j + \gamma_2 Post_t + \gamma_3 (TreatedFirm_j * Post_t)] \\ & + X'_{it}\pi + Z'_{jt}\delta + u_{ijt}, \end{aligned} \tag{3}$$

where Fem_i is a dummy variable that is equal to one if i is a woman, and all other variables are defined as in regression 1. As such, this alternative specification controls for within-group time shocks that are common to male and female employees. Table 11 reads as follows. The first three columns refer to the probability of 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-differences model for men, the second columns the effect on women, while the third one reports the estimates from the triple-differences 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. The estimates from the triple-differences model are practically indistinguishable from those of the difference-in-differences model, in the cases of both the occupational outcome 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. However, 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 each side of the 250-employees cutoff. Table 12 compares the results of the difference-in-differences model with that of the following difference-in-discontinuities model:

$$\begin{aligned} Y_{ijt} &= \alpha_j + X'_{it}\pi \\ &+ Post_t[\delta_0 + \delta_{reg} + \delta_1 FirmSize_{j2015} + TreatedFirm_j(\beta_0 + \beta_1 FirmSize_{j2015})] \\ &+ u_{ijt}, \end{aligned} \tag{4}$$

where δ_{reg} are region fixed effects and $FirmSize_{j2015}$ is a continuous variable measuring the number of employees in firm j in 2015. The main difference between our main specification and this one is that the difference-in-discontinuities model 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 12 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-differences model, while the second column presents those of the difference-in-discontinuities. While coefficients are only significant at 10 percent in this specification, the point estimates for both the occupational outcome and wages are very little affected.

Finally, we run a series of placebo tests pretending that the mandate binds at different firm size thresholds. Figures 6 and 7 present the estimates of these placebo policies, together with 95 percent confidence intervals.[28] The placebo cutoff is indicated on the y axis. The estimates corresponding to the 250 cutoff represent the coefficients estimated at the actual policy cutoff. 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 that affect larger firms differently to smaller firms. 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, do not depend on the fact that we defined the treatment status based on firms' number of employees in 2015, and are not sensitive to the information we use to define treatment status. Figures 8 and 9 show how the estimates of β from equation 1 change when restricting or enlarging the bandwidth around the 250 cutoff.[29] 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 8 shows that the effects on women's probability of working in above-median-wage occupations is especially stable

[28] Appendix Tables A4 and A5 display the corresponding detailed regression results.

[29] Appendix Tables A6 and A7 display the corresponding detailed regression results.

for bandwidths comprised between 30 and 60, while it vanishes for larger samples. On the one hand, this could be due to a decreased comparability across treatment and control groups. On the other hand, it could point to general equilibrium effects, capturing the impact of women moving from a treated firm to another. Figure 9 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, with the estimated zero effect becoming more precisely estimated as we enlarge the sample.

Table 13 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 probability of working in above-median-wage occupations, while columns 5-8 concern 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 at the top of the column, 2014, 2013, or 2012. 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.[30]

Finally, Appendix Table A9 shows that our results do not depend on the information used to define the treatment status. In particular, our main findings change little if we restrict the estimation sample to firms for which we can use only ASHE-based information on the number of employees to define the treatment status.

To sum up, our estimates are remarkably stable across different specifications and sample sizes, which should strongly support the validity of our identification strategy.[31]

[30]Note that to define the treatment status we only consider the firm size in years prior to the government's consultations with employers, when the 250 cutoff was decided. In Appendix Table A8, we further compare our identification strategy to one where the treatment status is defined based on actual firm size. On the one hand, effects may be larger when using the actual firm size if employers that self-select into the treatment are more willing to improve gender equality in their firm. On the other hand, effects could be smaller if these employers need to make fewer changes to improve gender equality in their firm. In Appendix Table A8, the point estimates are lower in magnitude and not statistically significant in this alternative specification. Importantly, the fact that treatment status changes over time using this definition could potentially induce noise in the estimates, in addition to any selection issue.

[31]In Tables A10 and A11, we further show that our results do not depend on the use of LFS weights, nor are they sensitive to age restrictions. Finally, in Appendix Tables A12 and A13 we show that the significance of our estimates

External validity. To conclude this section, we provide some insights regarding the external validity of our estimates. Figures 10 and 11 compare the occupational and industry distribution of men and women in the estimation sample to that of the entire ASHE population, over the period studied. Remarkably, the occupational distribution is very similar in the two samples both for men and women, with only some under-representation of sales occupations in the estimation sample of women. As for the industry distribution, Figure 11 shows that, with the exception of the manufacturing sector being over-represented in the estimation sample of men, the distribution also matches well across the two samples. Taken together, these figures suggest that, in the absence of large equilibrium effects, the estimated effects can hold across the firm size distribution.

8 Mechanisms

To delve into the mechanisms driving the estimated effects, we follow two directions. First, we investigate whether firms have changed their hiring practices to attract more women. We are particularly interested in three dimensions: the effect of the policy on the wording employed in job ads, the offer of flexible working arrangements, and wage posting decisions. Second, we study the role played by reputation in influencing firms' response, by investigating whether the publication of gender equality indicators correlates with firms' reputation among women.

8.1 Firms' hiring practices

To study the impact of pay transparency on firms' hiring practices, we use Burning Glass Technologies (BGT) online job advertisement data for the financial years 2014/2015 to 2018/2019.[32] The data are around 41 million (de-duplicated) individual job vacancies, collected from a wide range of online job listing sites. While the data set only includes online advertisements, and hence misses vacancies not posted online (e.g. those advertized informally and internal vacancies), it

is not affected by the clustering group considered, whether this be by firm, firm-size or firm-size times industry.

[32]BGT provided us with data from 2012 onward, but in the main analysis we exclude the first two years as BGT expressed concerned over the quality of data at the beginning of the sample.

includes a rich set of information that is especially useful for our analysis. First, each observation includes the text of the job advertisement. Second, more than 95 percent of vacancies have an occupational SOC identifier and 90 percent a county identifier. Finally, around one third of vacancies, or 13 million observations, include the name of the employer. As this is the only variable that can facilitate the merging of BGT data with other firm-level data, we focus on a restricted sample with non-missing employer names. To exclude potential selection issues related to the presence of the firm name, in Appendix Figure B1 we compare the industry distribution of the stock of vacancies in BGT to vacancies in the ONS Vacancy Survey for the same period. The two match well, mitigating concerns regarding the representativity of BGT. In what follows, we present the key dimensions we explore in this data set.

Gendered wording. A recent strand of psychology and management lab experiments study the importance of implicit biases in job postings (Gaucher et al. 2011, Tang et al. 2017). In particular, Gaucher et al. (2011) construct a list of job-listing-specific male and female-oriented terms derived from implicit association tests. Using this list of so-called gendered words, the authors present lab-based evidence that women are less willing to apply for a job if its posting uses male-oriented wording. From Gaucher et al. (2011), we borrow the dictionaries of terms w that are reported in Appendix Table B1.[33] The dictionaries are D^M and D^F for terms that are commonly associated with men or women respectively, and map terms according to $D : W \rightarrow \{0, 1\}$ depending on whether the term appears in the list in Table B1 or not. Using these dictionaries, we are able to classify each job advertisement based on a gender score defined as follows:

$$\text{Gender score} = \frac{1}{|w|}\left(\sum_{w} D_{\text{female}}(w) - \sum_{w} D_{\text{male}}(w)\right),$$

where w runs over all distinct terms in each job advertisement. A job description that gives a negative (positive) score is considered to have a male-oriented (female-oriented) wording, with

[33]Note that we have excluded words related to ”child” and ”analyst” that appear in the original list proposed by Gaucher et al. (2011), as we consider these terms to be mostly related to the work performed rather than a candidate’s trait. As shown in the appendix, all our results are unchanged if we include these words.

the magnitude of the score weighted by the total length of the job description. Figure 12 shows the resulting distribution of the score multiplied by 100 for clarity of presentation.[34] While the score is centered at 0 - which represents a gender-neutral vacancy - the graph shows that there is substantial variability in the gender orientation of job listings. Appendix Figure B2 complements the histogram by presenting the contribution of each "male" and "female" word to the score, and showing that some terms are much more prevalent than others, both in the male and female dictionaries.

Flexible working arrangements. Gender differences in preferences for temporal flexibility have been shown to play a key role in explaining gender segregation across occupations, which in turn contributes to the persistence of the gender pay gap (Bertrand et al. 2010, Goldin 2014, Wiswall and Zafar 2018, Cortes and Pan 2019). In order to attract more women, firms may have responded to the mandate by expanding the offer of flexible working arrangements (FWA hereafter). Importantly, this can have ambiguous effects on gender pay differentials. On the one hand, offering FWA in high-paid, male-dominated occupations may help reduce gender occupational segregation and the gender pay gap. On the other hand, wherever flexibility entails a wage penalty because it generates a productivity cost for the firm, the offer of FWA may increase gender pay differentials (Goldin 2014). To investigate this dimension of response, we constructed a vocabulary of flexible work terms using job listings from Timewise, a website specialized in flexible working, and the LFS definition of FWA. Appendix Table B2 presents the full list of variables included. Note that we do not consider FWA that give the employer discretion over scheduling, such as shift work or on-call work (Adams et al. 2020), but only those arrangements that can give the employee more control over their work-life balance. Also, in our main results, we exclude "job sharing" from our list of FWA, as we want to focus on full-time FWA. However, our results change little if this FWA is included. Based on this list, we created a categorical variable equal to 1 if a job vacancy includes at least one flexible working term. As shown in Figure 13, we find that, on average, 7 percent of

[34] The observations for the bottom and top 1 percent of the score have been removed to increase the readability of the graph.

job listings offer FWA, with flexi-time being the most frequent option, while remote working is very rarely offered.

Wage posting. Many studies document that there exists a gender gap in bargaining skills. In particular, women are less likely to ask for wage increases (Babcock et al. 2003, Bowles et al. 2007), and tend to avoid bargaining when they apply for jobs that leave wage negotiation ambiguous (Leibbrandt and List 2015). Having information on wages posted upfront in job adverts could therefore have a bearing on the gender of job applicants. To study this, we extract wages offered from the job ad text using natural language processing. To identify wages in the text, we use a series of targeted regular expressions, such as "30-35k per annum", or "20,000/year". The frequency of the wage offer (annual, weekly, hourly) is similarly inferred from the text. Finally, all values are transformed into annual wages. To validate this procedure, in Figure 14 we compare the resulting distribution of wages offered - censored at 100,000 pounds - with wages of employees who have at most 3 months of tenure from the LFS. While the distribution of offered wages is noisier, the two are very similar.[35] Using this procedure, we find that less than 30 percent of BGT job listings contain information on wages that can be automatically identified, leaving room for firms to respond to the policy along this margin.

Finally, Figure 15 investigates the 1-digit-occupation-level correlation between these hiring practices and the gender hourly pay gap computed from the Labor Force Survey. Remarkably, occupations with a higher average gender score across vacancies have a lower gender pay gap. Similarly occupations with a larger percentage of vacancies offering flexible work arrangements or posting wage information also have a lower gender pay gap. While these are only correlations, they suggest that hiring practices are a powerful tool to predict gender equality. In the next paragraph, we further investigate this hypothesis by looking at firm-level correlations.

[35]When a vacancy posts a wage interval, we consider the mid point of the interval. To remove outliers, the bottom and top 1 percent of wage posted are excluded from Figure 14.

Hiring practices and the glass ceiling. Before moving to the regression analysis, we explore the relationship between firms' hiring practices and gender equality indicators. To this aim, we merge BGT with GEO data, using a cosine similarity name-matching algorithm for the company names. We retain only firms that have an exact match, representing two thirds of the GEO sample - section B.3 of the appendix provides a detailed description of the matching algorithm.[36] Using this matched data set, we show three novel stylized facts in Figure 16.[37] Controlling for industry fixed effects, and the occupational composition of vacancies, we find that firms which, on average over the period observed, are more likely to offer FWA, tend to have a higher percentage of women at the top of the wage distribution and a lower gender pay gap. The same is true for firms that are more likely to post wage information. And finally, though a more female-oriented wording is associated with a larger gender pay gap,[38] firms using less male-oriented wording are more likely to have women at the top of the wage distribution. Importantly, these are just correlations. A larger percentage of women at the top of a firm wage distribution may influence firms' hiring practices, rather than vice versa. Also, hiring practices may reflect broader management strategies. Nevertheless, this novel descriptive evidence strongly motivated us to study the causal impact of pay transparency on firms' hiring practices.

Regression analysis. To implement the difference-in-differences strategy, we need two additional elements: a control group, and firms' size by number of employees. To this aim, we use 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-differences analysis.[39]

[36]Appendix Table B3 further shows that most of the gender equality indicators of companies with a match score of one are not statistically different from those of firms with a lower score, mitigating selection concerns along this margin.

[37]Appendix Table B4 reports the corresponding regression table.

[38]Note that, rather than the gender score itself, here we use a dummy equal to one if the gender score is positive to measure the gender orientation of vacancies. We do this only to have the same scale across the different bars. The magnitudes of the correlations are larger but the conclusions are unchanged when using the gender score.

[39]To address selectivity concerns, we compare the industry distribution for firms with and without information on the number of employees in each year considered in Appendix Figure B3. While firms with missing information on employee numbers also tend to have missing information on industry, the rest of the distribution appears similar,

We first merge FAME with GEO firms using the company registration number. Then, we merge these with BGT using the same name-matching algorithm for the company name, and retain only firms with a match score equal to 1.[40] Finally, we restrict the sample to FAME firms with 200 to 300 employees. The final data set with non-missing information on occupation and counties contains 97,831 observations on 3,114 firms.

To investigate the effect of the pay transparency policy on firms' hiring practices, we estimate the following difference-in-differences model at the vacancy level:

$$Y_{ijt} \;\; = \;\; \alpha_j + \theta_t + \beta(TreatedFirm_j * Post_t) + Z'_{jt}\delta + u_{ijt}, \tag{5}$$

where Y_{jt} is either a dummy equal to one if vacancy i of firm j in month t offer FWA, or contains wage information. Alternatively, it represents the gender score associated with the vacancy; α_j and θ_t are firm and month fixed effects respectively, and Z_{jt} includes region-specific time shocks. Finally, standard errors are clustered at the firm level.

Table 14 presents the results of this analysis. Each panel refers to a different outcome, namely the gender score of the vacancy, the probability that the vacancy offers FWA, and the probability that it contains wage information. The first column presents the results for the entire sample. In light of the correlations shown above, in columns 2 and 3 we compare the effect of the policy in vacancies for occupations with a low and high gender pay gap. This table offers two insights. On the one hand, the policy does not significantly affect these practices in the entire sample. If anything point estimates in the last panel point to a negative effect on wage posting. On the other hand, when comparing the estimates in columns 2 and 3, the results on the entire sample seem to mask heterogeneous effects across occupations. In particular, point estimates suggest that the policy increases the probability that firms use a more female-oriented language, offer FWA and post wage information in high-gender-pay-gap occupations, though the effects are not statistically

especially from 2016.

[40] Appendix Table B5 shows that the average number of employees is not statistically different in firms with a match score below or equal to 1.

different across subgroups.[41]

Overall, this analysis suggests that treated firms may have started to change their hiring practices to attract more women in high-gender-pay-gap occupations. Table 15 further investigates the dynamics of compensating differentials in this sample, restricting the analysis to the pre-policy period. In particular, it analyzes how the offer of FWA correlates with wage posting, and whether, conditional on wage posting, vacancies with FWA post lower wages. Both regressions control for the gender score, occupation, month, and firm fixed effects. Column 1 shows that vacancies offering FWA, as well as those with a larger gender score, are more likely to have wage information. Conditional on wage posting, vacancies offering FWA post a wage that is 2 percent lower than other vacancies. Though the coefficient is not significant, this correlation suggests that FWA are a costly amenity. Whenever firms have increased their offers of FWA to attract women to better-paid positions, compensating differentials may help explain why this has not translated into a wage increase. Remarkably, vacancies with a larger gender score are also associated with lower wages.

Now, we want to investigate why firms have chosen to respond to the policy at all, and in particular, whether this response has been influenced by a reputation motive.

8.2 Firms' reputation

To study the role played by firms' reputation, we use the YouGov Women's Ranking for 2018 and 2019. This index ranks 1590 brands every year by surveying a representative sample of women between February 201y and January 201y+1. In particular, it constructs impression scores based on answers to the question: "Overall, of which of the following brands do you have a positive/negative impression?", as follows:

$$Score = \frac{PositiveAns - NegativeAns}{AllAns} \times 100 \tag{6}$$

[41] Appendix Table B6 shows that these results change little when estimating regression 5 over the period 2013-2019, closer to that considered in the analysis of occupational and pay effects. Appendix Table B7 shows instead that the results are not affected when we use all terms in the original list of Gaucher et al. (2011) to construct the gender score, and include vacancies offering job sharing in the definition of FWA.

Our objective is to investigate whether firms' rank is associated to their performance in gender equality indicators. Using the name-matching algorithm described above, combined with manual matching, to link YouGov data with GEO firms, we were able to match 996 companies in 2018 and 1018 in 2019.[42] Note that firms voluntarily ask YouGov to be included in their surveys. While we do not find any statistically significant difference between gender equality indicators of firms that are and are not included in the YouGOV list, the former are potentially the ones that care the most for their reputation.

Despite this, Panel A of Table 16 shows that, over the two years of data, a larger median gender pay gap is negatively correlated with women's impression score, while a higher percentage of women at the top of the wage distribution is positively associated with the YouGov score. Panels B and C further break down the analysis by year, to show that these correlations tend to become larger in magnitude and more significant from the first to the second year of data, when potentially more women become aware of gender equality indicators. To us, this suggests that firms are under the scrutiny of women, and this could help explain why and how firms have responded to the pay transparency mandate.[43]

9 Stock market reaction

The last paragraph supports the hypothesis that the public disclosure of firms' gender equality indicators may induce businesses to tackle gender pay differentials to preserve their reputation. But firms may also be concerned about what investors think. A negative reaction of the stock market to the publication of the gender equality indicators may constitute a strong incentive for a firm to improve its performance on gender equality. Importantly, a priori it is not clear how

[42] Most of the YouGov companies which we cannot link with the GEO data are not registered in the UK.

[43] In Appendix Table C8, we also explore the correlation between firms' gender equality indicators and their score in the YouGov Workforce Ranking. This is obtained by asking to both men and women the following questions about a sample of 1466 firms: "Imagine you (or your friend) were applying for the same sort of role at the following brand that you currently have or would apply for?" coupled with "Which of the following brands would you be proud to work for?" and "Which of the following brands would you be embarassed to work for?". The point estimates in Table C8 suggest that a worse performance on gender equality indicators is associated with a lower rank in this index too, though the correlations are not significant.

the stock market may react. On the one hand, investors could punish firms, and especially those with a high gender pay gap, assuming that these will have to increase women's wages, with a resulting increase in the wage bill and lower profits. Also, the stock market may fear potential negative repercussions of the policy on labor productivity. On the other hand, investors may reward firms that publish gender equality indicators, if this may stimulate improvements in management practices, with positive knock-on effects on workers' productivity and firms' profits.

To investigate these dynamics, we adopt the traditional event-study methodology (Lee and Mas 2012, Bell and Machin 2018). In particular, we focus on the first year of publication as this is when gender equality indicators are more likely to represent an information shock for the market.[44] We first combine the list of firms publishing gender equality indicators 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 equality indicators. Of this group, 101 are directly publicly listed, while the others have a publicly listed parent company. Importantly, different 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 gender equality indicators on the same date. Hence, in what follows, we consider the publication date of the first one to publish. 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 over the previous year of data:[45]

$$AR_{jt} \quad = \quad r_{jt} - (\widehat{\alpha}_j + \widehat{\beta}_j r_{mt}), \tag{7}$$

[44] One element that is important for this analysis is the extent to which the publication date predicts firms' performance on gender equality. Appendix Figure D4 shows that if anything, there is a weak negative correlation between the publication date and the median gender pay gap, meaning that firms with a worst performance on gender equality are actually publishing before others. The small magnitude of this correlation does not seem to threaten our research design.

[45] The 15 days before t are excluded from the estimation of predicted returns to avoid capturing any potential anticipation effect of events happening at time t.

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.[46] As it is standard when employing this methodology (Lee and Mas 2012, Bell and Machin 2018), we then look at the evolution of 3-day cumulative abnormal returns, or $CARS(-1,1) = \sum_{k=-1}^{+1} AR_{jk}$. This allows us to take into account both potential leaks of information in the day prior to the event of interest, as well as lagged responses in the day following this event. Figure 17 plots the $CARS(-1,1)$, in the five days before and after the publication date. While these are not statistically different from zero in the days prior to the publication date, they start to become negative from the publication date up to four days afterwards, with an average loss per day of around 35 basis points.[47] Appendix Figure D5 shows similar but noisier dynamics when considering 5-day cumulative abnormal returns, or $CARS(-2,2)$. Table 17 further investigates how this drop relates to the performance on the gender equality indicators. Column one regresses the 3-day CARs on the day of the publication on a constant, the average median gender pay gap reported by firms related to the same publicly listed firm, called "Group-avg GPG" in the table, a dummy equal to one if the gender pay gap is in favor of men, called "Group-avg GPG positive", and an interaction term between these two. Column 2 adds the following controls: a categorical variable for whether the listed firm directly publishes the gender equality indicators, or has a subsidiary publishing them; and the number of firms in the group publishing the gender equality indicators. Column 3 adds industry fixed effects, and column 4 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. While it does not seem that firms publishing a gender pay gap in favor of men are penalized more than others, the main message of this analysis is that firms publishing gender equality indicators are under the scrutiny of investors. In turn, this supports the hypothesis that the reputation motive may have played an important role in explaining the reaction of treated firms.

[46]Alternatively, one could use a CAPM model or Four-Factor model to predict firms' returns, but this is beyond the scope of this paper.

[47]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.

10 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's transparency policy, this paper shows that making the glass ceiling visible is one way to create cracks in it. First, the policy increases the probability that women work in above-median-wage occupations by 5 percent, more than halving the pre-policy gender gap in this dimension. Second, it leads to a 2.8 percent decrease in male real hourly pay in treated firms relative to control firms, corresponding to approximately a 15 percent decrease in the in-sample pre-policy gender pay gap. By combining the difference-in-differences strategy with a text analysis of job listings, we also find suggestive evidence that firms may have started to modify their hiring practices to attract more women in high-gender-pay-gap occupations. Moreover, by linking the gender equality indicators with YouGov Women's Rankings, we find that a worse performance on gender equality is associated with a lower impression score among women, suggesting that reputation may help explain firms' response to the policy. Further supporting this hypothesis, we find that the stock market has an immediate, though short-lived, negative reaction to the publication of gender equality indicators.

In light of these results, pay transparency seems to be more effective than other firm-targeting policies in cracking the glass ceiling. In particular, as a comparison, Bertrand et al. (2019) find that female corporate board quotas, another policy that has been widely discussed in the media, has no substantial impact on gender equality outside of firms' boards.

However, to fully assess our findings, we need to take a welfare perspective as well. In this respect, the first important point to consider is that the effect of pay transparency on women's occupational outcomes seems to come from movements from the bottom to the middle of the wage distribution, and we do not have enough evidence to say that the policy increases women's probability of working in the highest-paid occupations, where they are especially under-represented. In other words, the road to break through the glass ceiling is still long. Second, pay transparency leads to pay compression from above. While this result is in line with the conclusions of other studies on pay transparency,[48] taken together, our findings point to limited benefits of this policy. At the same time, while we find suggestive evidence that firms may have paid a reputational cost with consumers and investors, the policy also has limited costs in terms of labor productivity and employee retention. In other words, even if we assume that firms were acting as profit-maximizing before the introduction of the policy, the changes they have made in response to the policy in terms of hiring and wage setting do not seem to have hurt their profit components. Overall, while a detailed cost-benefit analysis of this policy is outside the current scope of this paper, a comparison of the costs and benefits mentioned here points to small net benefits of this policy.

To conclude, it is important to stress that our analysis identifies short-term effects. Potentially, transparency stimulates firms' response only in the short run, when it acts as an information shock that attracts strong attention from the media, the stock market, and the general public, generating reputation concerns among firms. However, if the policy does not produce an actual change in firms' culture, its effect may fade away over time as the strength of the information shock weakens (Giuliano 2020). Therefore, it is necessary to keep monitoring the effects of this policy to fully understand its effect on the labor market in the long run.

[48] In particular, Mas (2017) finds that pay transparency in the public sector in California leads to a 7 percent reduction in managers' compensation, 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.

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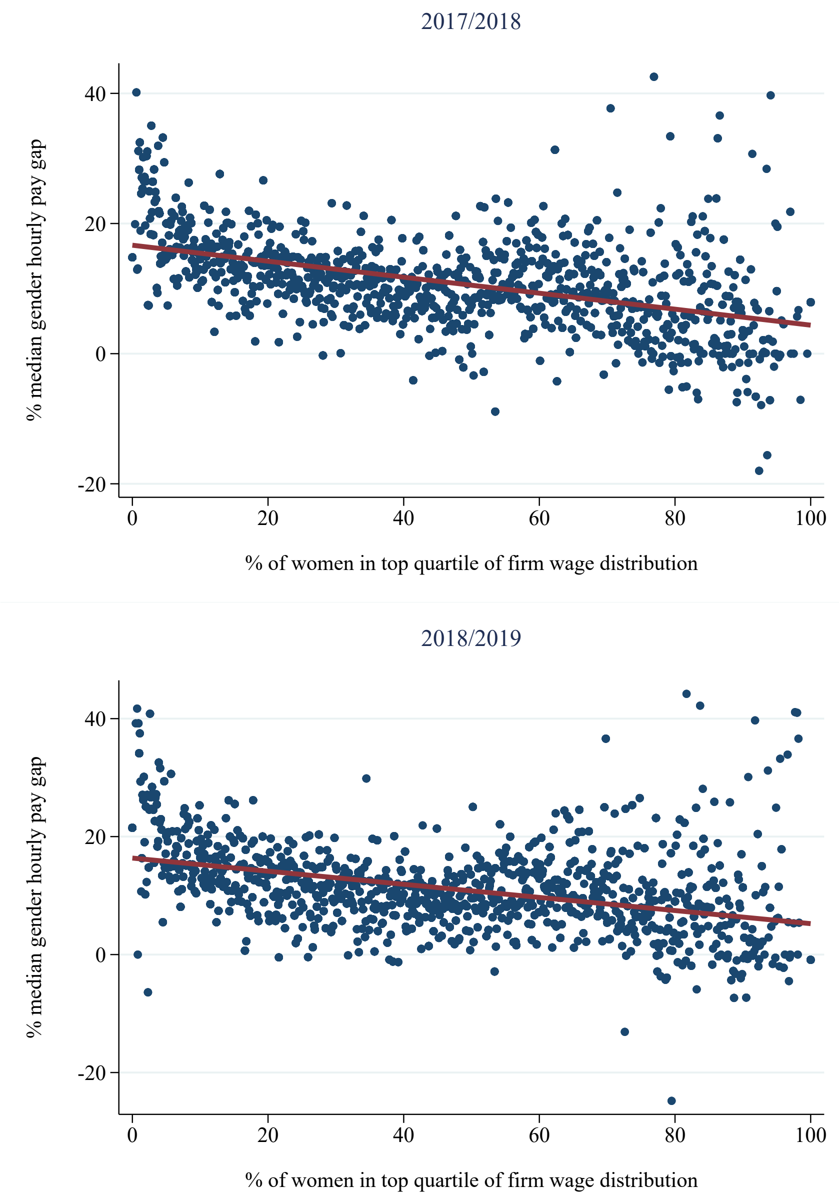
11 Figures and Tables

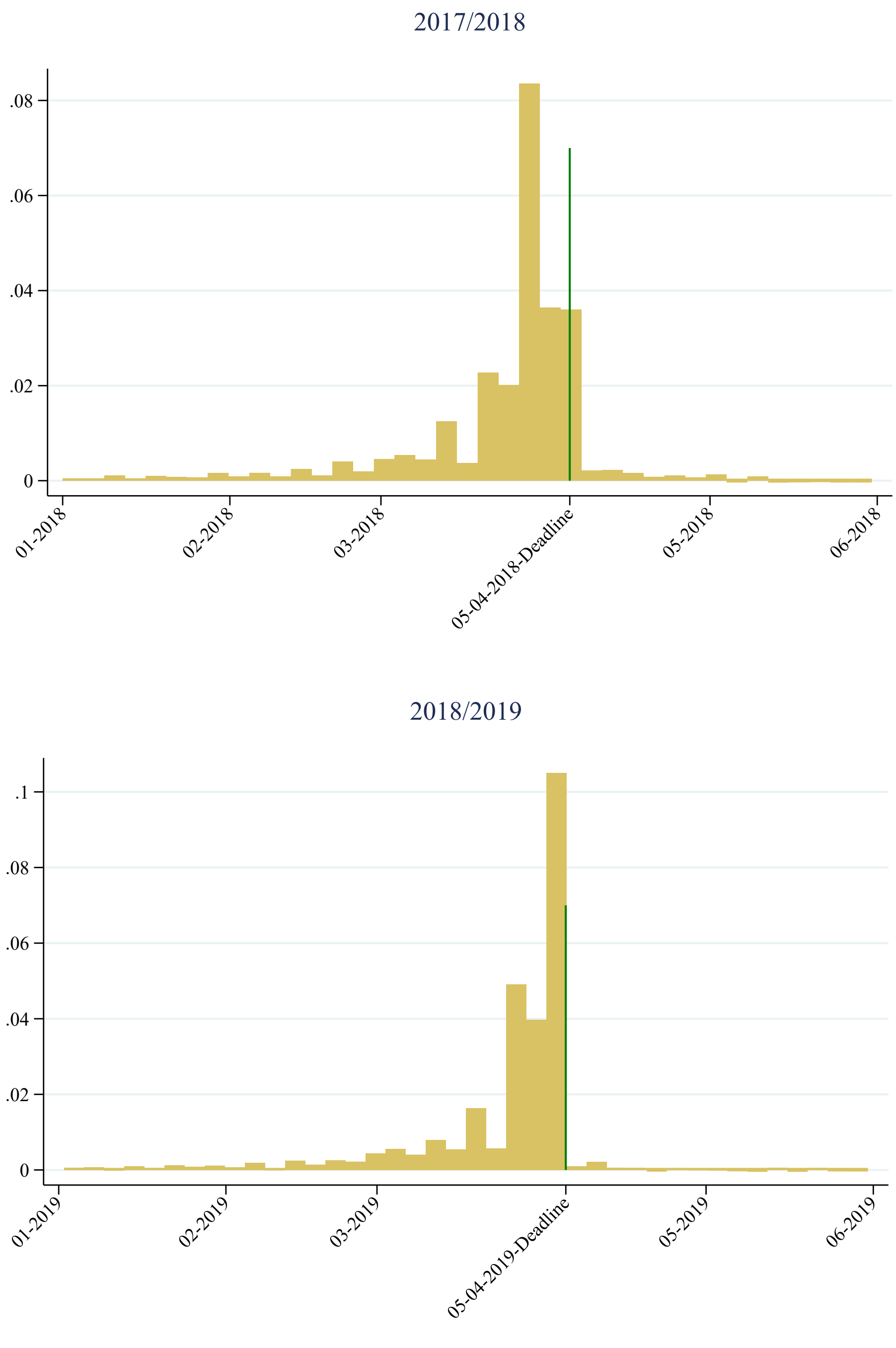
Figure 1: **Gender pay gap and women at the top**



Source: UK Government Equalities Office (GEO).

Note: This figure shows the correlation between firms' gender median hourly pay gap and the proportion of women in the highest quartile of the firm wage distribution. The top graph refers to the 2017/18 data (10,557 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.

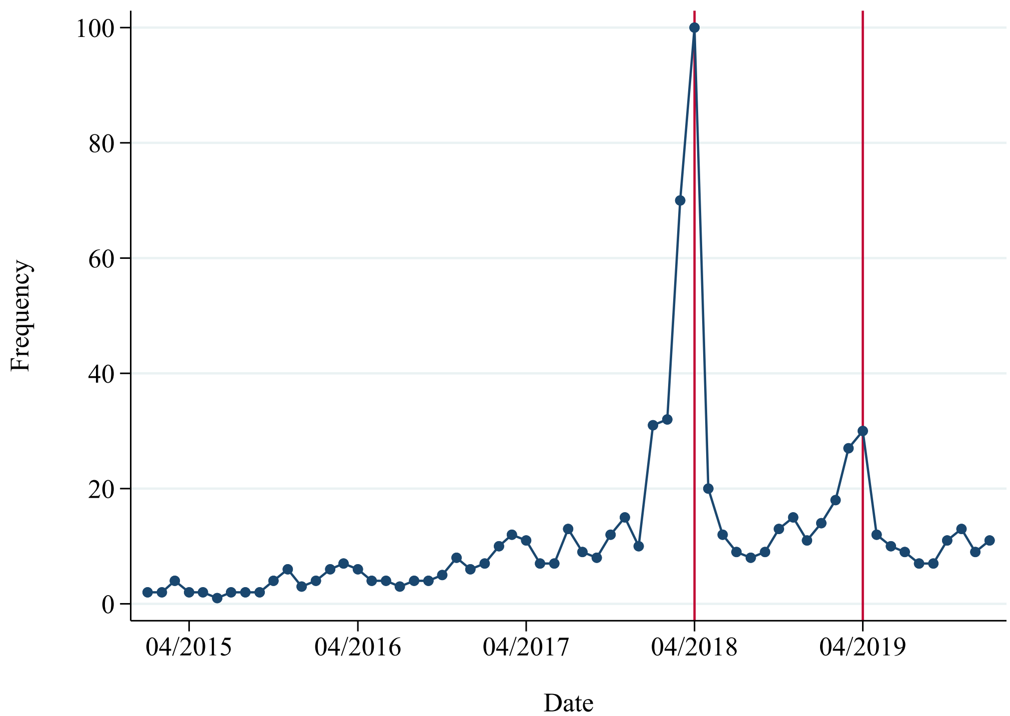
Figure 2: **Distribution submission date by year**



Source: UK Government Equalities Office (GEO).

Note: This figure shows the distribution of days when firms published their gender equality indicators. The top graph refers to the 2017/18 data (10,557 observations), while the bottom one refers to 2018/19 (10,812 observations). Around 5 percent of firms publish before January of the deadline year.

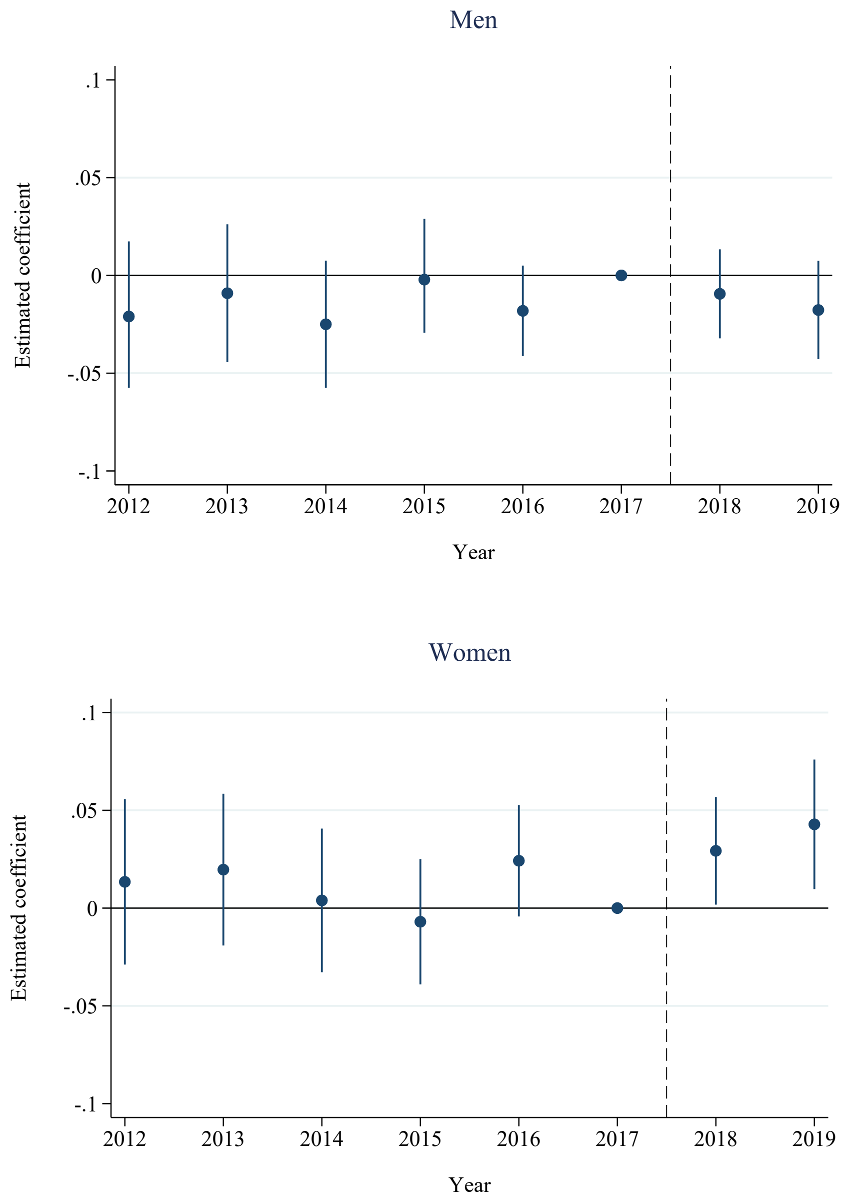
Figure 3: **Google searches for "gender pay gap"**



Source: Google, 2015-2019.

Note: This graph reports the UK relative search volume for the term "gender pay gap" between April 2015 and June 2019 using Google's search services. The frequency is indexed to the peak, which occurred in the week commencing 1st April 2018.

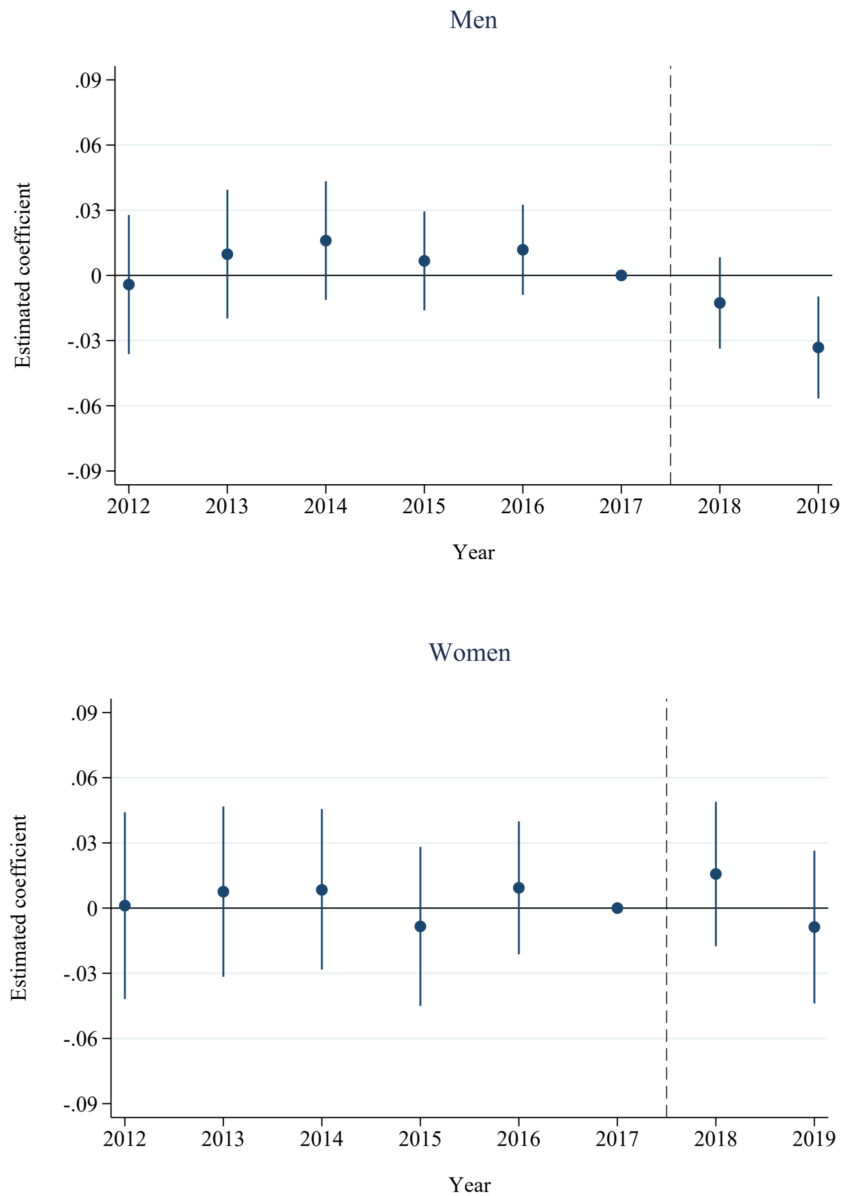
Figure 4: **Event studies - above-median wage occupations**



Source: ASHE, 2012-2019.

Note: This figure reports the estimates of the leads and lags of the policy on the probability of working in above-median-wage occupations. These results are obtained from the estimation of regression 2. The top graph refers to men, and the bottom 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 associated with firm-level clustered s.e. are also reported. The dash vertical line indicates the month when the mandate is approved, i.e., February 2017.

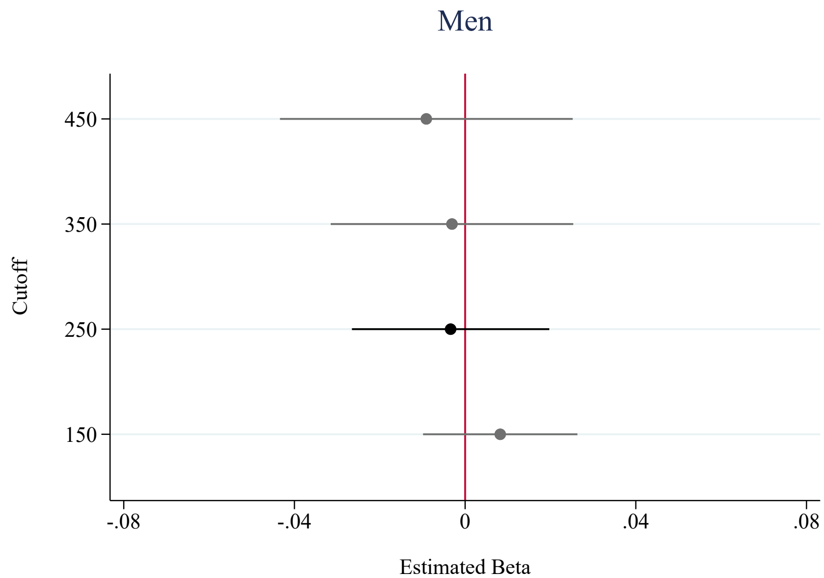
Figure 5: **Event studies - log real hourly pay**

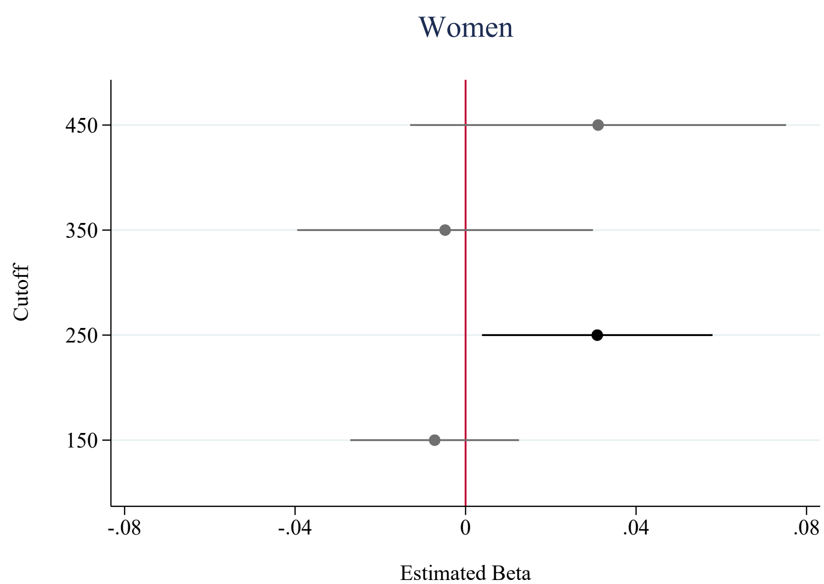


Source: ASHE, 2012-2019.

Note: This figure reports the estimates of the leads and lags of the policy on the outcome log real hourly pay. These results are obtained from the estimation of regression 2. The top graph refers to men, and the bottom one 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 associated with firm-level clustered s.e. are also reported. The dash vertical line indicates the month when the mandate is approved, i.e., February 2017.

Figure 6: **Placebo cutoffs - above-median-wage occupations**

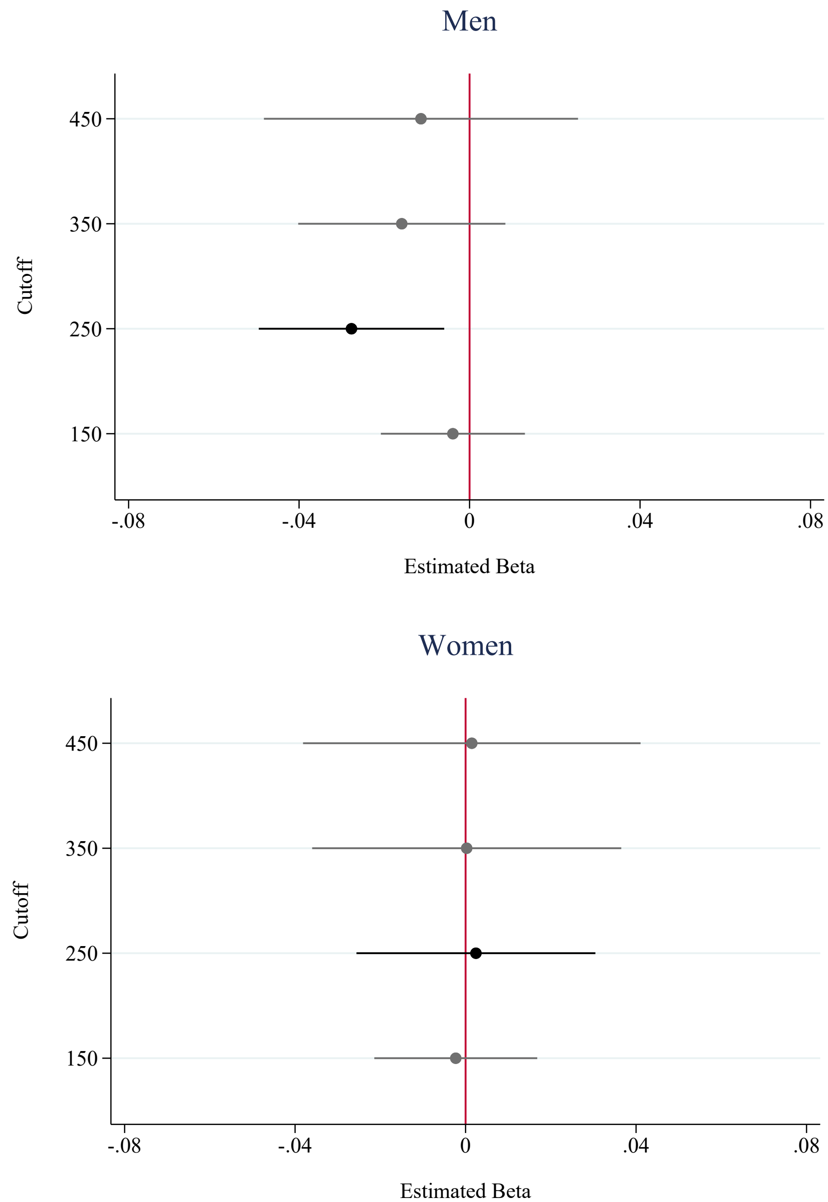




Source: ASHE, 2012-2019.

Note: This figure presents the estimated effects of placebo policies on the probability of working in above-median-wage occupations. The top graph refers to men, and the bottom one to women. In each graph, the x-axis reports the estimated coefficients, while placebo cutoffs are reported on the y-axis. The coefficient corresponding to the 250 cutoff represent the estimated effect of the actual policy. All coefficients are obtained from the estimation of regression 1. In each regression, the estimation sample includes firms with $+/-50$ employees from the threshold considered. All regressions are estimated using LFS weights. 95 percent confidence intervals associated with firm-level clustered s.e. are also reported. The corresponding regression results are displayed in Appendix Table A4.

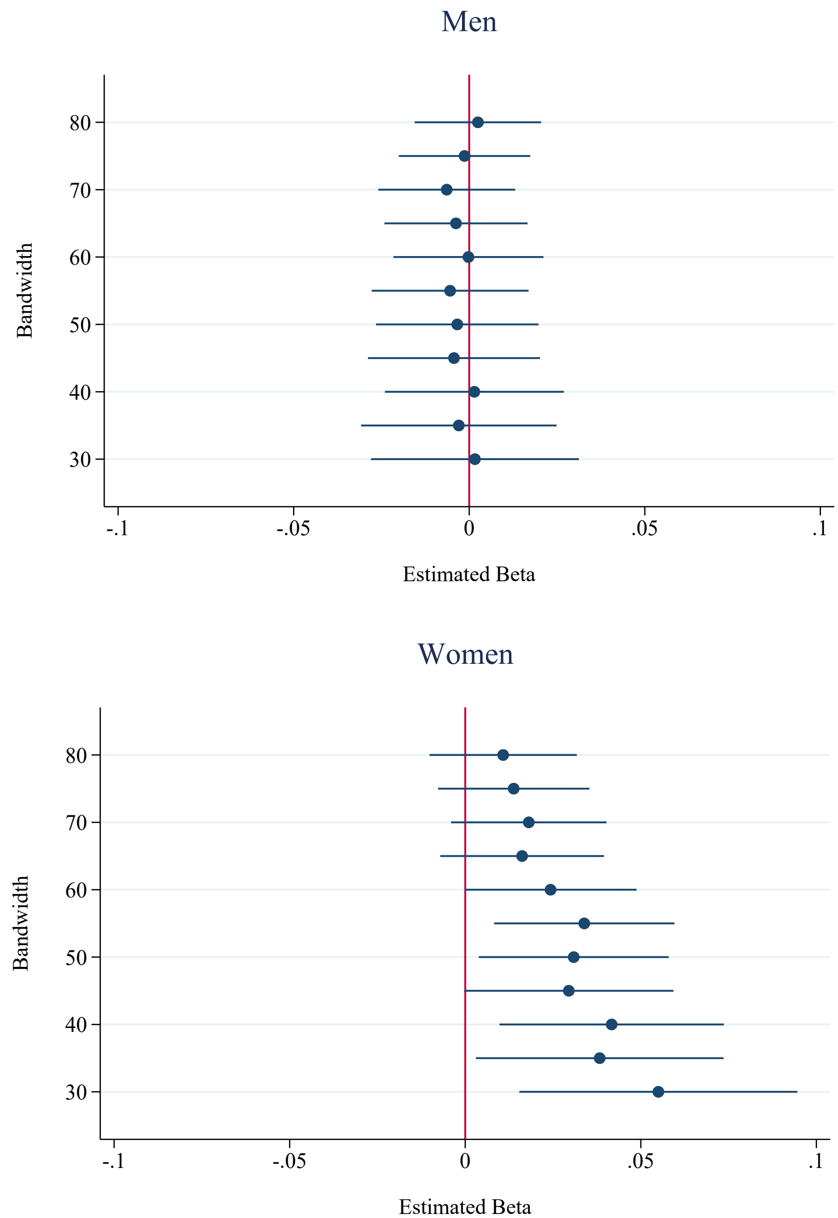
Figure 7: **Placebo cutoffs - log real hourly pay**



Source: ASHE, 2012-2019.

Note: This figure presents the estimated effects of placebo policies on log real hourly pay. The top graph refers to men, and the bottom one to women. In each graph, the x-axis reports the estimated coefficients, while placebo cutoffs are reported on the y-axis. The coefficient corresponding to the 250 cutoff represent the estimated effect of the actual policy. All coefficients are obtained from the estimation of regression 1. In each regression, the estimation sample includes firms with $+/-50$ employees from the threshold considered. All regressions are estimated using LFS weights. 95 percent confidence intervals associated with firm-level clustered s.e. are also reported. The corresponding regression results are displayed in Appendix Table A5.

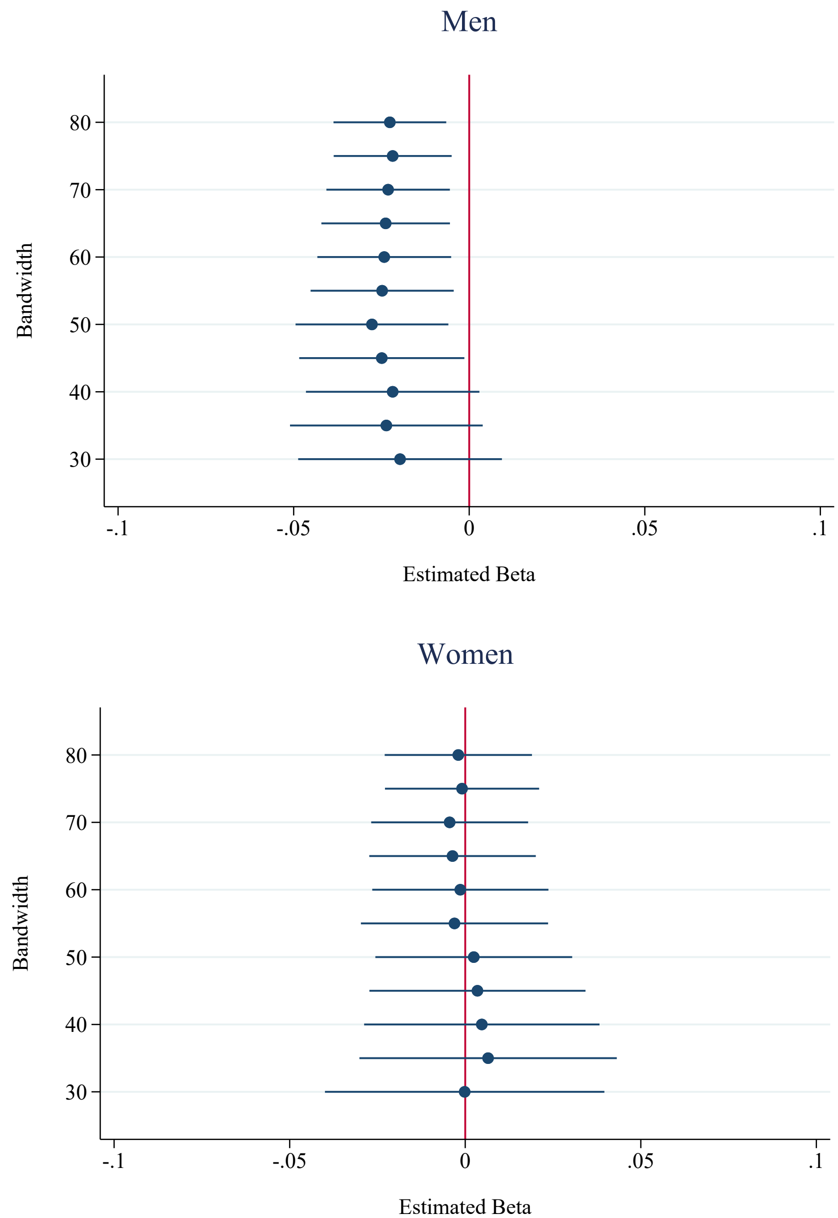
Figure 8: **Varying bandwidth - above-median wage occupations**



Source: ASHE, 2012-2019.

Note: This figure shows how the estimates of β from regression 1 change when restricting or enlarging the bandwidth around the 250 cutoff. The outcome considered is the probability of working in above-median-wage occupations. The top graph refers to men, and the bottom one to women. In each graph, the x-axis reports the estimated coefficients, 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 associated with firm-level clustered s.e. are also reported. The corresponding regression results are displayed in Appendix Table A6.

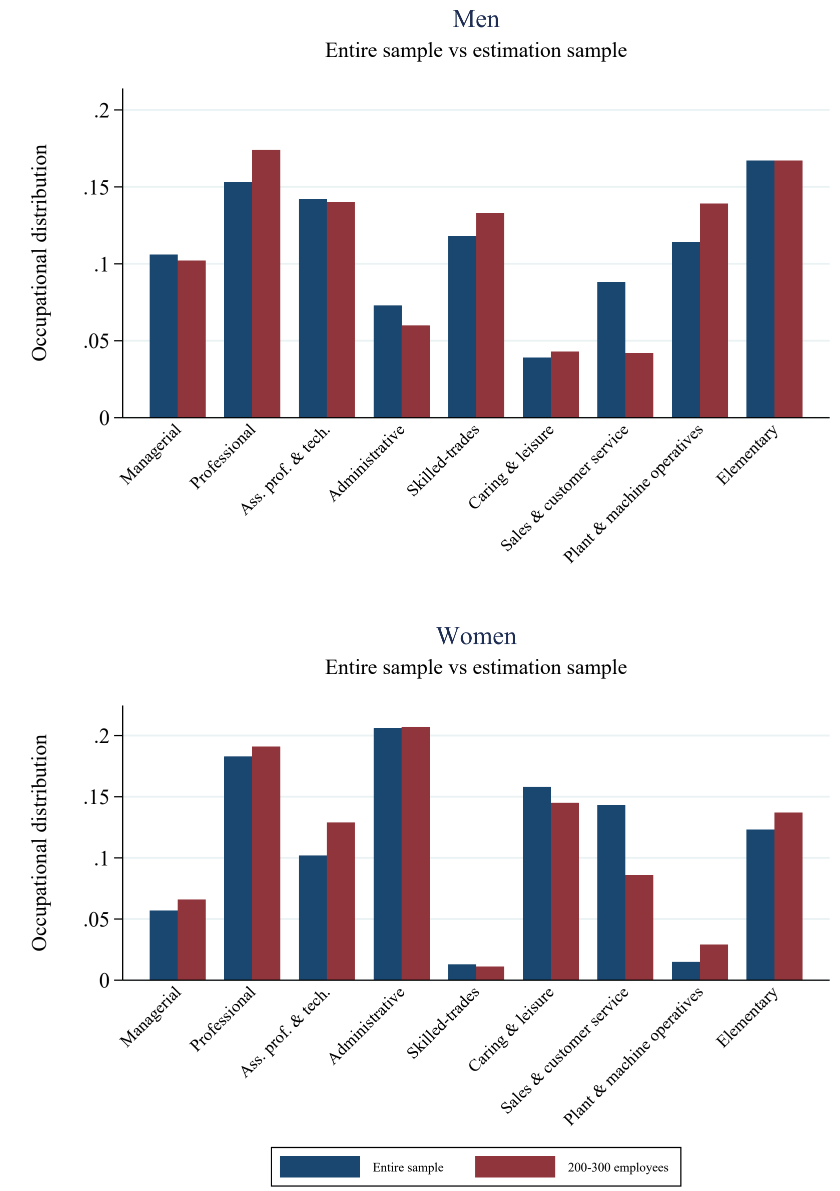
Figure 9: **Varying bandwidth - log real hourly pay**



Source: ASHE, 2012-2019.

Note: This figure shows how the estimates of β from regression 1 change when restricting or enlarging the bandwidth around the 250 cutoff. The outcome considered is log real hourly pay. The top graph refers to men, and the bottom one to women. In each graph, he x-axis reports the estimated coefficients, 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 associated with firm-level clustered s.e. are also reported. The corresponding regression results are displayed in Appendix Table A7.

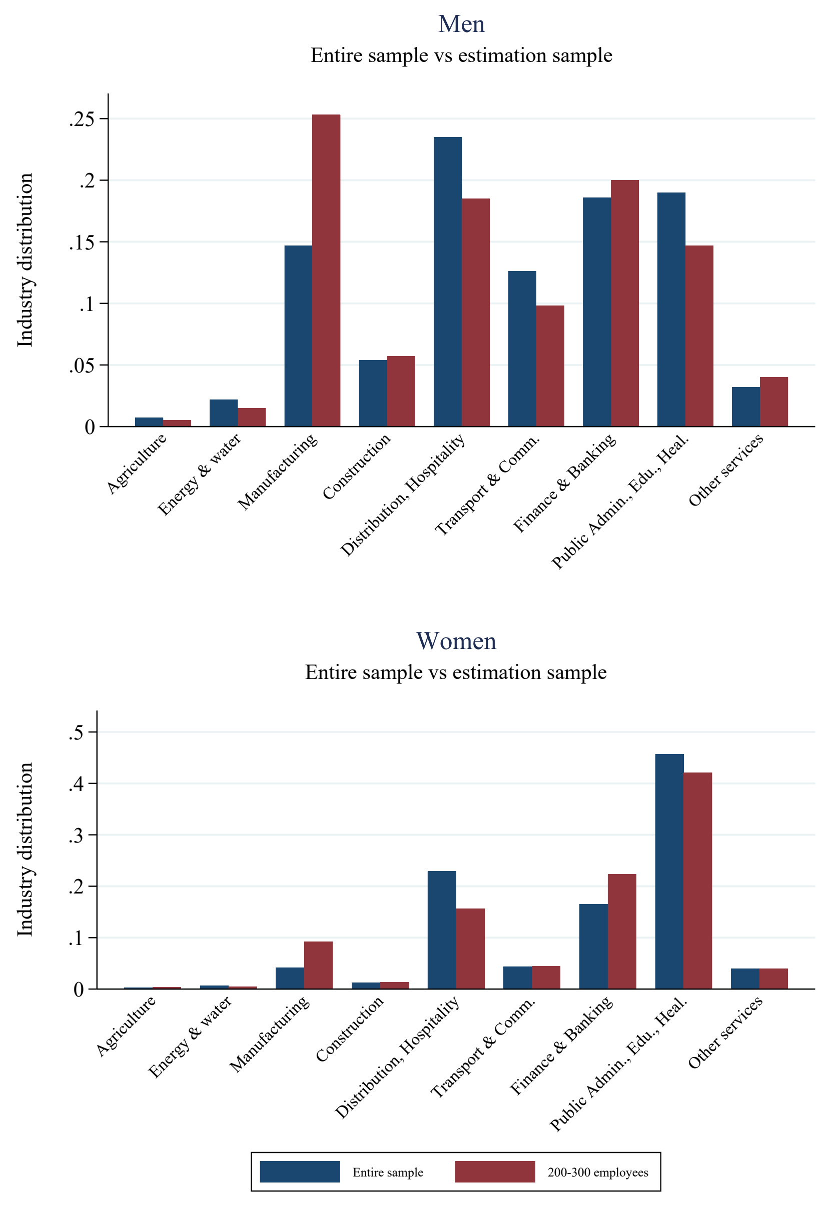
Figure 10: **Occupational distribution**



Source: ASHE, 2012-2019.

Note: The two graphs compare the occupational distribution of men and women in the estimation sample and in the entire population of ASHE, over the period considered.

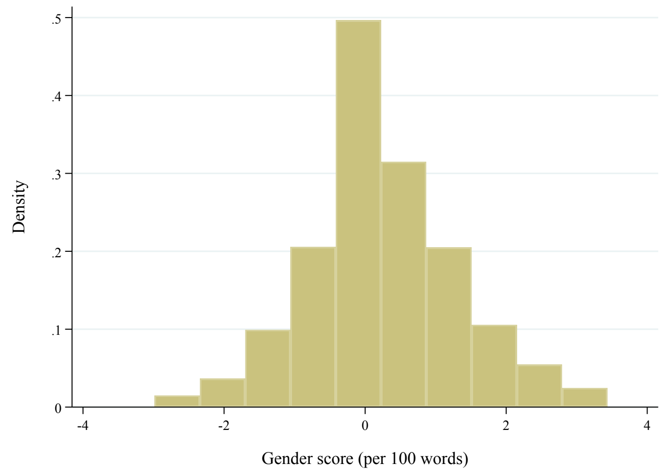
Figure 11: **Industry distribution**



Source: ASHE, 2012-2019.

Note: The two graphs compare the industry distribution of men and women in the estimation sample and in the entire population of ASHE, over the period considered.

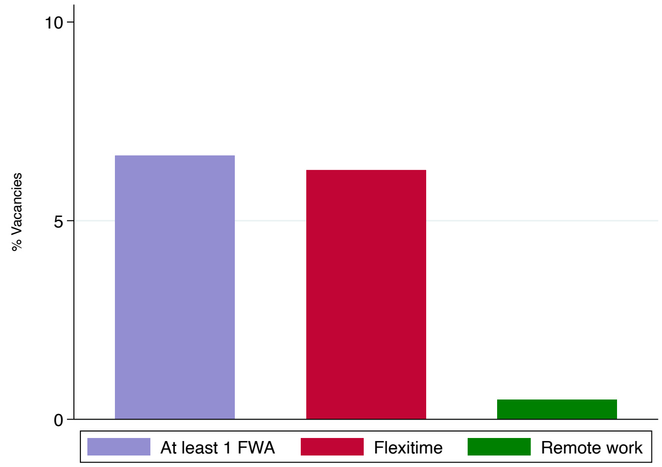
Figure 12: **Gender score**



Source: BGT, 2015-2019.

Note: This figure presents the sample distribution of the gender score assigned to job listings. The words used to construct the gender score are reported in Appendix Table B1. The bottom and top 1 percent values have been excluded from the histogram. N. observations = 13,090,103.

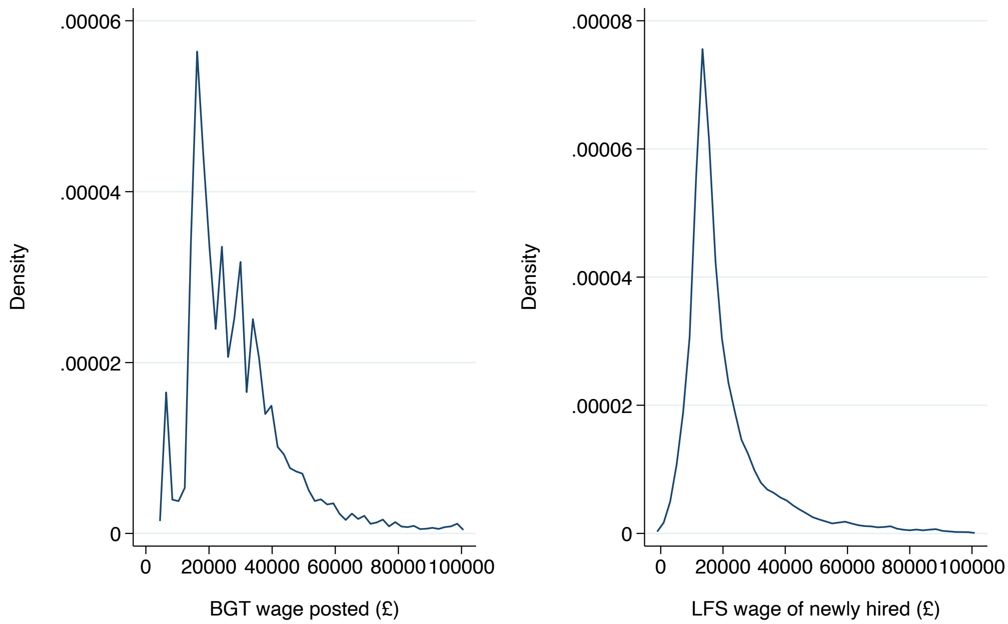
Figure 13: **Flexible work arrangements**



Source: BGT, 2015-2019.

Note: This figure presents the frequency of flexible work arrangements (FWA) in BGT data. The words used to define FWA are reported in Appendix Table B2. N. observations = 13,357,248.

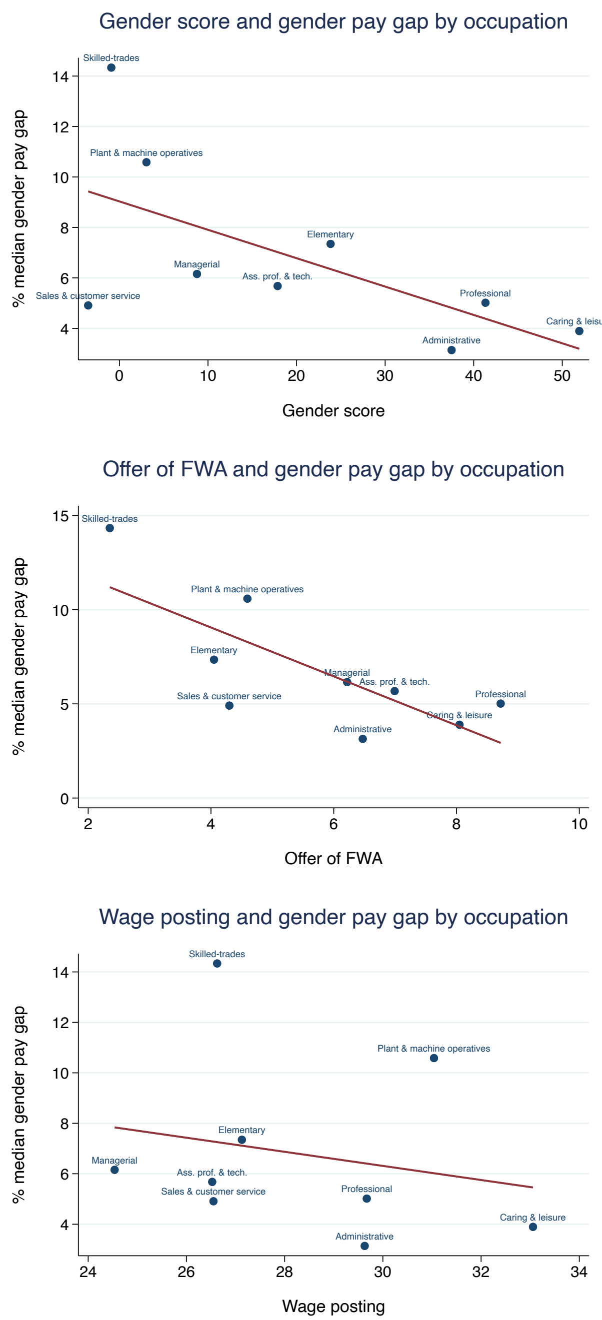
Figure 14: **Real annual wages in BGT and LFS**



Source: BGT, LFS, 2015-2019.

Note: This figure compares the distribution of posted annual wages extracted from BGT, with that of annual wages of newly hired workers (at most 3 months of tenure) computed from the LFS. Both measures are expressed in pounds and in real terms. The bottom and top 1 percent of posted wages have been removed from the left graph.

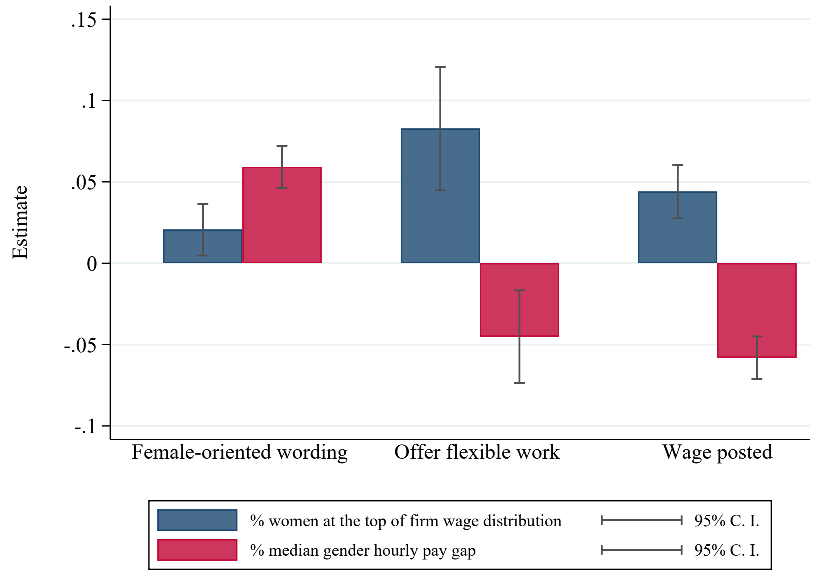
Figure 15: **Hiring practices and gender pay gap - occupational level**



Source: BGT, LFS, 2015-2019.

Note: This figure shows the correlation between firms' hiring practices and the median gender pay gap in 1-digit occupations. The x-axis measures, respectively, the average gender score across vacancies for each occupation (top graph), the percentage of vacancies in each occupation offering flexible work arrangements (middle graph), and the percentage of vacancies in each occupation reporting wage information (bottom graph). The median hourly gender pay gap is calculated using the LFS over the period 2015-2019.

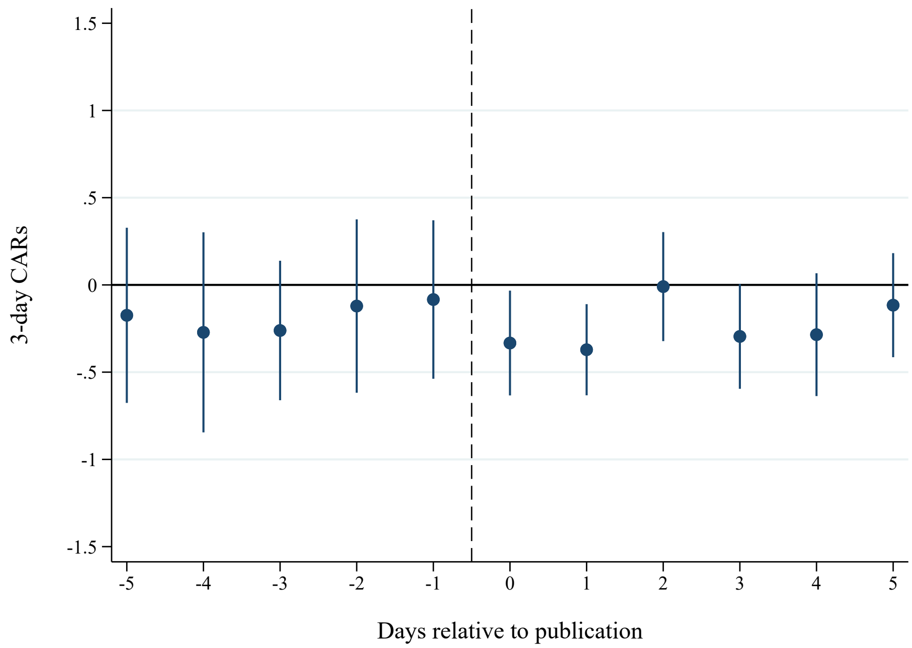
Figure 16: **Hiring practices and the glass geiling - Conditional correlations**



Source: BGT 2015-2019, GEO 2018-2019.

Note: The bar graph reports estimated coefficients from regressions of gender equality indicators (averaged across 2017/18 and 2018/19) on hiring practices (averaged over the period 2014/15-2018/19), the occupational composition of firms' vacancies and their sector. The detailed regression results are reported in Appendix Table B4. The graph also displays 95 percent confidence intervals associated with heteroskedasticity-robust standard errors. The sample includes firms publishing gender equality indicators both in 2017/18 and in 2018/19, with non-missing registration numbers, and perfectly matched with BGT. N. observations = 4,722.

Figure 17: **3-day cumulative abnormal returns around 2017/18 publication date**



Source: Datastream, FAME, GEO, 2017-2018.

Note: This figure plots 3-day cumulative abnormal returns around the publication date of gender equality indicators in 2017/2018. In particular, it shows CARS(-1, 1) around the day reported on the graph. 95 percent confidence intervals associated with standard errors clustered at the level of publication date are also displayed. The sample includes firms that had to publish gender equality indicators by April 5th 2018, or that have a subsidiary that had to publish these data.

Table 1: **Public indicators of gender equality**

	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)	
% men receiving bonus	35.39	35.72	0.01
	(36.33)	(36.68)	
% 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	10,557	10,812	

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

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

	Treated men (1)	Control men (2)	Treated women (3)	Control women (4)
Above-median-wage 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)	(27.42)
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 agreements	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, 2012-2017.

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 deviations are 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			✓	✓	✓	✓
Product Market Concentration				✓		
Industry Trends					✓	
2011 Firm Output*Year FE						✓
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 above-median-wage occupations, obtained from the estimation of regression 1. 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. The estimation sample comprises men (women) working in firms that have between 200 and 300 employees. 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 estimated with LFS weights. 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. 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 panels 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	0.69	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	72.62	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. Panel A presents results for men, Panel B for women. Each column refers to a different outcome, as specified at the top of it. The estimation sample comprises men (women) working in firms that have between 200 and 300 employees. All regressions include firm and year fixed effects, region-specific time shocks, 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 estimated with LFS weights. 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. 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 panels 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			✓	✓	✓	✓
Product Market Concentration				✓		
Industry Trends					✓	
2011 Firm Output*Year FE						✓
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. 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. The estimation sample comprises men (women) working in firms that have between 200 and 300 employees. 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 estimated with LFS weights. 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. 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 panels 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	16.92	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 hourly pay, weekly pay and weekly hours worked, obtained from the estimation of regression 1. Panel A presents results for men, Panel B for women. Each column refers to a different outcome, as specified at the top of it. The estimation sample comprises men (women) working in firms that have between 200 and 300 employees. All regressions include firm and year fixed effects, region-specific time shocks, 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 estimated with LFS weights. 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. 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 panels 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	16.60	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 components, obtained from the estimation of regression 1. Panel A presents results for men, Panel B for women. Each column refers to a different outcome, as specified at the top of it. The estimation sample comprises men (women) working in firms that have between 200 and 300 employees. All regressions include firm and year fixed effects, region-specific time shocks, 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 estimated with LFS weights. 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. 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 panels A and B).

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

Table 8: **Impact on labor productivity**

	(1)	(2)	(3)	(4)
Treated Firm*Post	-0.0292	-0.0290	-0.0285	-0.0296
	(0.0350)	(0.0353)	(0.0353)	(0.0353)
Observations	22800	22800	22800	22800
Pre-Treatment Mean	3.980	3.980	3.980	3.980
Year*Region FE		✓	✓	✓
Product Market Concentration			✓	
Industry Trends				✓

Source: BSD, 2012-2018.

Notes: This table reports the impact of pay transparency on labor productivity, obtained from the estimation of regression 1 at firm level. Each column refers to a different specification as indicated at the bottom of the table. All regressions include firm and year fixed effects. 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 9: **Impact on above-median-wage occupations - restricting pre-period**

	Main specification (1)	From 2013 (2)	From 2014 (3)	From 2015 (4)	From 2016 (5)
Panel A: Men					
Treated firm*post	-0.00340	-0.00634	-0.00770	-0.0125	-0.00273
	(0.0118)	(0.0116)	(0.0113)	(0.0107)	(0.0103)
Observations	24658	21954	19018	15811	12693
Pre-Treatment Mean	0.69	0.69	0.70	0.70	0.71
Panel B: Women					
Treated firm*post	0.0309**	0.0339**	0.0362***	0.0371**	0.0315***
	(0.0138)	(0.0137)	(0.0137)	(0.0132)	(0.0131)
Observations	21484	19248	16896	14103	11321
Pre-Treatment Mean	0.65	0.65	0.65	0.64	0.66

Source: ASHE, 2012-2019.

Notes: This table reports the impact of pay transparency on the probability of working in above-median-wage occupations, obtained from the estimation of regression 1. Panel A presents results for men, Panel B for women. The first column reports the main estimates, while in the following ones, the pre-treatment period is progressively restricted. The estimation sample comprises men (women) working in firms that have between 200 and 300 employees. All regressions include firm and year fixed effects, region-specific time shocks, 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 estimated with LFS weights. 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 (or the year specified at the top of the column) and 2017.

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

Table 10: **Impact on log real hourly pay - restricting pre-period**

	Main specification (1)	From 2013 (2)	From 2014 (3)	From 2015 (4)	From 2016 (5)
Panel A: Men					
Treated firm*post	-0.0277**	-0.0277**	-0.0265**	-0.0165	-0.0158
	(0.0111)	(0.0109)	(0.0107)	(0.0104)	(0.0104)
Observations	24658	21954	19018	15811	12693
Pre-Treatment Mean	16.92	17.06	17.12	17.36	17.61
Panel B: Women					
Treated firm*post	0.00243	0.00382	0.00452	0.00794	0.00412
	(0.0143)	(0.0143)	(0.0145)	(0.0147)	(0.0154)
Observations	21484	19248	16896	14103	11131
Pre-Treatment Mean	13.89	13.91	14.03	14.13	14.49

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. Panel A presents results for men, Panel B for women. The first column reports the main estimates, while in the following ones, the pre-treatment period is progressively restricted. The estimation sample comprises men (women) working in firms that have between 200 and 300 employees. All regressions include firm and year fixed effects, region-specific time shocks, 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 estimated with LFS weights. 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 (or the year specified at the top of the column) and 2017.

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

Table 11: **Diff-in-Diff vs Triple Diff-in-Diff**

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

Source: ASHE, 2012-2019.
Notes: This table compares the results of the difference-in-differences model to those obtained from the estimation of a triple-differences model as specified in equation 3. In columns 1-3 the outcome is the probability of working in above-median-wage occupations, while in columns 4-6 it is the log real hourly pay. The estimation sample comprises men (women) working in firms that have between 200 and 300 employees. All regressions control for firm and year fixed effects, and region-specific time shocks. Columns 1 to 3 also include age and age squared. Columns 4 to 6 include individual fixed effects. A treated firm is defined as having at least 250 employees in 2015. All regressions are estimated with LFS weights. Heteroskedasticity-robust standard errors clustered at firm level in parentheses. The p-value at the bottom of the table refers to the t-test on the effect for women in the triple-differences model (Treated Firm*Post+Treated Firm*Post*Female) .
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 12: **Diff-in-Diff vs Diff-in-Disc**

	Above-median-wage occupation		Log real hourly pay	
	Diff-in-Diff (1)	Diff-in-Disc (2)	Diff-in-Diff (3)	Diff-in-Disc (4)
Panel A: Men				
Treated Firm*Post	-0.00340	-0.00299	-0.0277**	-0.0278*
	(0.0118)	(0.0147)	(0.0111)	(0.0148)
Observations	24658	24658	24658	24658
Pre-Treatment Mean	0.69	0.69	16.92	16.92
Panel B: Women				
Treated Firm*Post	0.0309**	0.0303*	0.00243	-0.00915
	(0.0138)	(0.0167)	(0.0143)	(0.0176)
Observations	21484	21484	21484	21484
Pre-Treatment Mean	0.65	0.65	13.89	13.89
Year FE	✓		✓	
Year*Region FE	✓		✓	
Post		✓		✓
Post*Region FE		✓		✓
Norm. Firm Size*Post		✓		✓
Norm. Firm Size*Treated Firm*Post		✓		✓
Individual controls	✓	✓	✓	✓
P-value Men Vs Women	0.056	0.127	0.0823	0.399

Source: ASHE, 2012-2019.

Notes: This table compares the results of the difference-in-differences model to those obtained from the estimation of a difference-in-discontinuities model as specified in equation 4. In columns 1-2 the outcome is the probability of working in above-median-wage occupations, while in columns 3-4 it is the log real hourly pay. Panel A presents results for men, Panel B for women. The estimation sample comprises men (women) working in firms that have between 200 and 300 employees. All regressions include firm fixed effects. In columns 1 and 2, the individual controls comprise age and age squared. Columns 3 and 4 include individual fixed effects. A treated firm is defined as having at least 250 employees in 2015. All regressions are estimated with LFS weights. 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. 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 panels A and B).

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

Table 13: **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
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. In columns 1-4, the outcome is the probability of working in above-median wage occupations, while in columns 5-8 it is 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 men (women) working in firms that have between 200 and 300 employees. All regressions include firm and year fixed effects, and region-specific time shocks. Individual controls include age and age squared in columns 1-4, and individual fixed effects in columns 5-8. A treated firm is defined as having at least 250 employees in the year indicated at the top of the column. The post dummy is equal to one from 2018 onward. All regressions are estimated with LFS weights. 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. 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 panels A and B).

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

Table 14: **Impact on hiring practices**

	Entire sample (1)	Low GPG (2)	High GPG (3)
Panel A: Gender score			
Treated Firm*Post	0.0371	0.0332	0.108
	(0.0575)	(0.0602)	(0.0824)
Pre-Treatment Mean	0.13	0.17	0.04
P-value difference		0.42	
Panel B: Flexible work			
Treated Firm*Post	0.00999	0.00541	0.0221
	(0.0133)	(0.00994)	(0.0259)
Pre-Treatment Mean	0.03	0.03	0.03
P-value difference		0.49	
Panel C: Wage posting			
Treated Firm*Post	-0.0233	-0.0214	0.0199
	(0.0234)	(0.0232)	(0.0253)
Pre-Treatment Mean	0.25	0.24	0.28
P-value difference		0.14	
Observations	97831	71354	26477

Source: BGT, FAME, GEO, 2015-2019.

Notes: This table reports the impact of pay transparency on firms' hiring practices, obtained from the estimation of regression 5. The estimation sample comprises firms that have between 200 and 300 employees. All regressions include firm and month fixed effects, and region-specific time-shocks. The post dummy is equal to one from 2018 onward. A treated firm is defined as having at least 250 employees in 2015. High-gender-pay-gap occupations include managerial, skilled trades, machine operatives and elementary occupations. 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 2015 and 2017. The p-value reported at the bottom of each panel refers to the test of equality of coefficients on low- and high-gender-pay-gap occupations.

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

Table 15: **Investigating compensating differentials**

	(1) Wage posted	(2) Log real wage
Offer flexible work	0.0457	-0.0249
	(0.0309)	(0.0345)
Gender score	0.00676***	-0.0139*
	(0.00234)	(0.00794)
Observations	76196	19268

Source: BGT, FAME, GEO, 2015-2017.

Notes: This table investigates the correlation between the offer of flexible work arrangements and wage posting. In column 1, the sample includes firms with 200 to 300 employees between 2014/15 and 2016/17, while in column 2, it is restricted to those that post wage information in BGT. All regressions control for occupation, month, and firm fixed effects.

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

Table 16: **Gender equality indicators and firms' reputation**

	Women's impression score	
	(1)	(2)
Panel A: Two years pooled		
Median gender pay gap	-0.0393**	
	(0.0190)	
% women at the top		0.0351***
		(0.0129)
Observations	2014	2014
Panel B: 2017/2018		
Median gender pay gap	-0.0320	
	(0.0250)	
% women at the top		0.0320*
		(0.0181)
Observations	996	996
Panel C: 2018/2019		
Median gender pay gap	-0.0489*	
	(0.0291)	
% women at the top		0.0378**
		(0.0183)
Observations	1018	1018

Source: GEO, YouGov, 2018-2019.

Notes: This table shows the raw correlation between firms' gender equality indicators and their score in YouGov Women's Rankings. Panel A refers to both years, panel B refers to 2017/2018, while Panel C refers to 2018/2019. In each panel, the sample includes the GEO firms that have been perfectly matched with YouGov entries.

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

Table 17: **CARs(-1,1) around publication**

	(1)	(2)	(3)	(4)
Group-avg GPG performance negative	0.618	0.669	0.651	0.392
	(0.868)	(0.932)	(0.956)	(1.020)
Group-avg GPG performance	-0.0265	-0.0266	-0.0277	-0.0426
	(0.0556)	(0.0551)	(0.0562)	(0.0578)
Group-avg perf.*group-avg perf. negative	0.0365	0.0359	0.0363	0.0477
	(0.0564)	(0.0559)	(0.0573)	(0.0586)
Constant	-1.057	2.163**	2.823***	0.560
	(0.742)	(0.893)	(0.952)	(1.552)
Observations	405	405	405	383
Ownership structure		✓	✓	✓
N. Firms in the group		✓	✓	✓
Industry FE			✓	✓
Other controls				✓

Source: Datastream, FAME, GEO, 2017-2018.

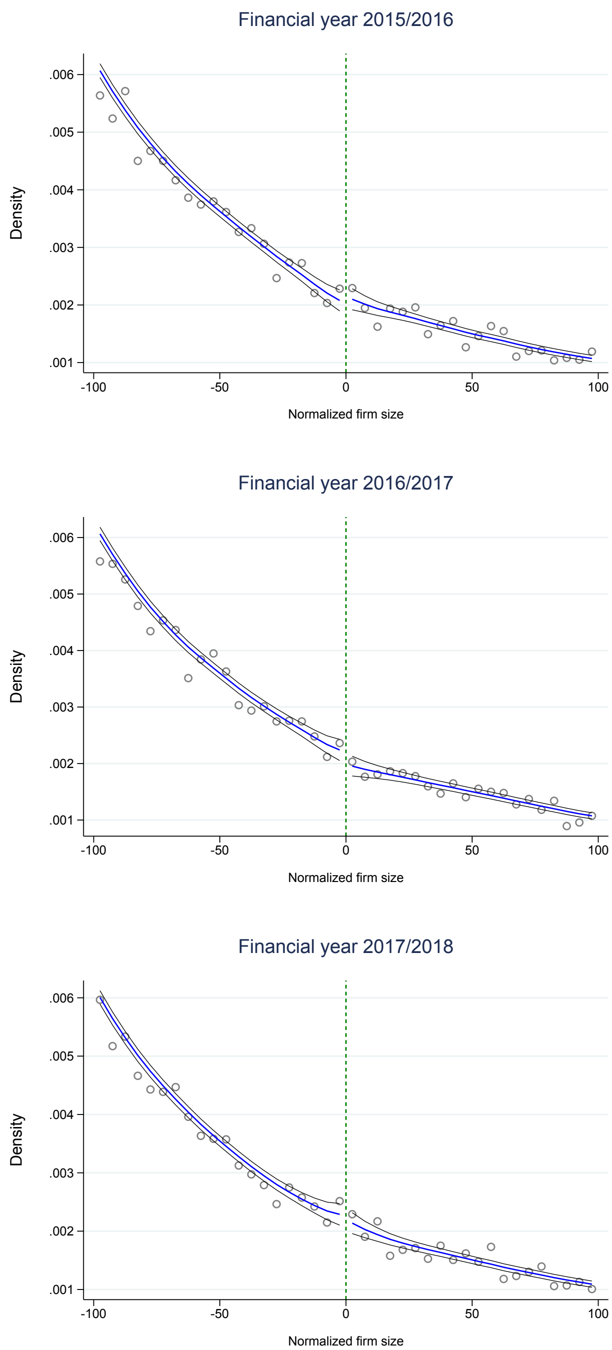
Notes: This table shows the estimates of the cumulative abnormal returns around the publication of gender equality indicators. The dependent variable is the sum of abnormal returns in the 3-day window around the publication date. The sample includes firms which had to publish gender equality indicators by April 5th 2018, or which have a subsidiary that had to publish these data. From column 2 onward we include a variable measuring the number of firms in the group publishing the gender equality indicators, and dummies for whether it is the listed firm or the immediate, domestic or global owner of a firm that has to publish the gender equality indicators. Other controls in column 4 include the lagged values of log of market capitalization, price to book value ratio, and the return on assets.

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

Appendix

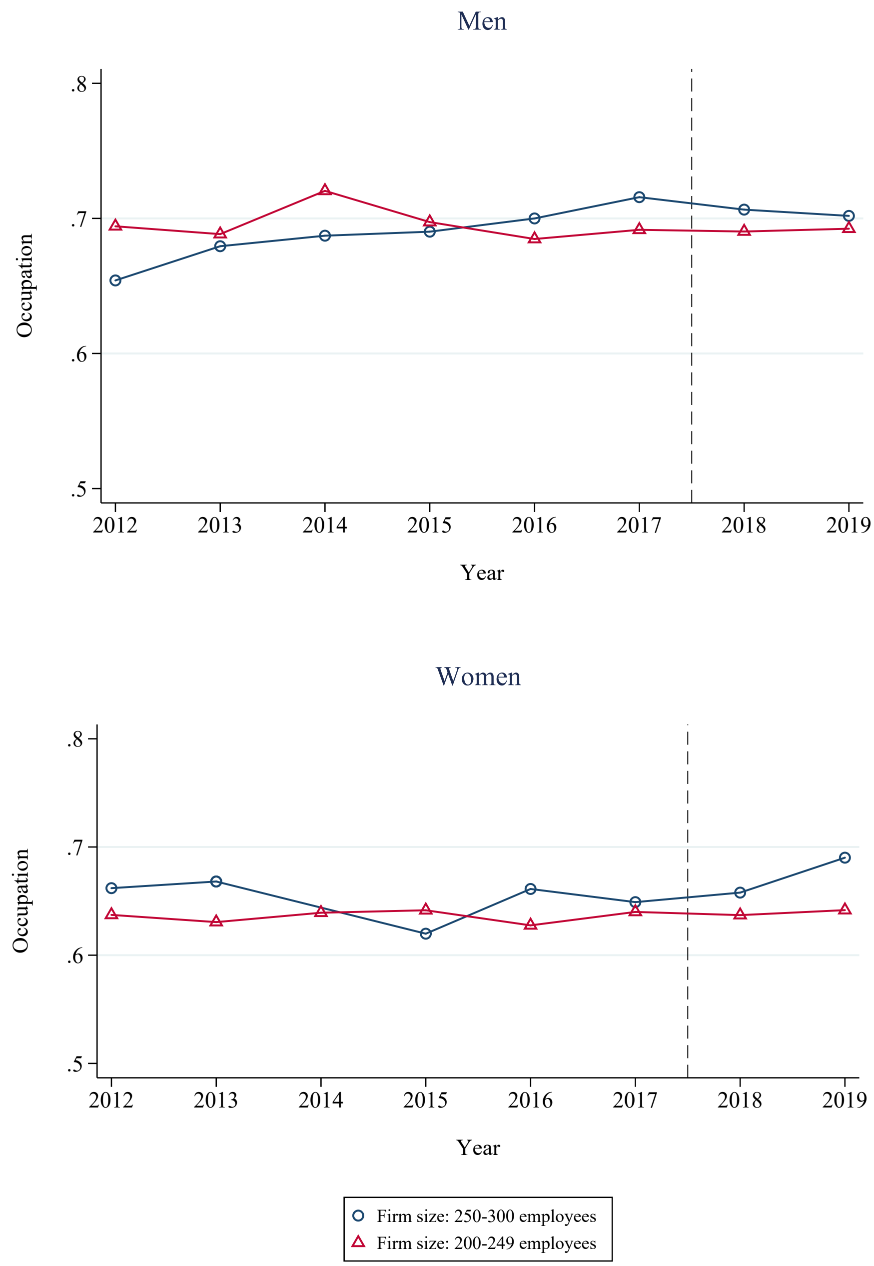
A Further results and robustness checks - ASHE and BSD

Figure A1: **Firm distribution**



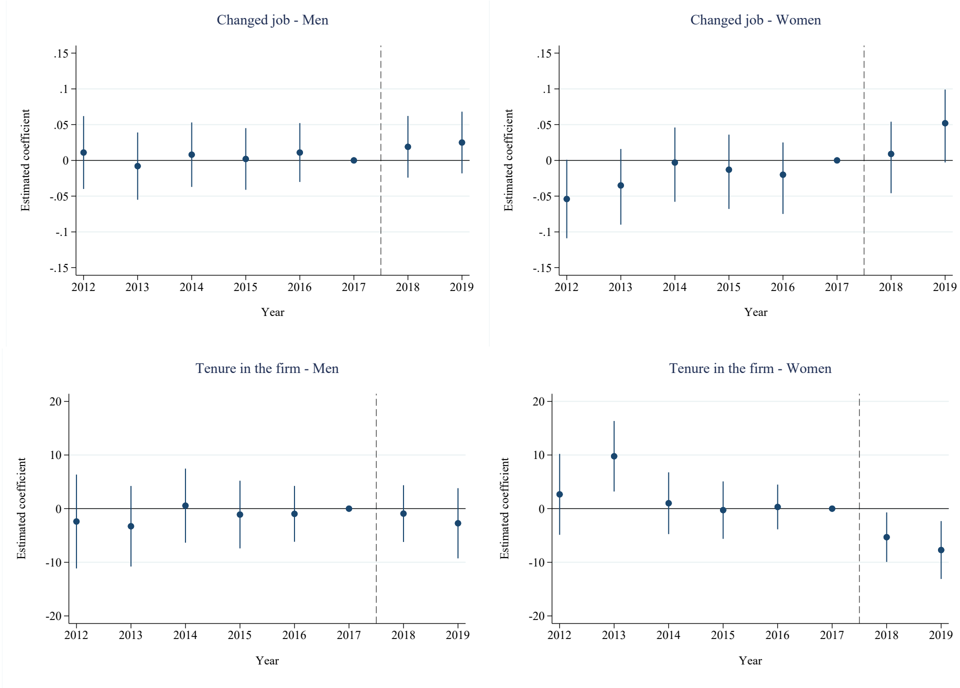
Source: BSD, 2016-2018.
Note: These graphs show the distribution of firms around the 250 cutoff in each year since the announcement of the policy. In each figure, the sample includes firms with +/100 employees from the threshold, grouped in 20 bins. Each dot represents the share of firms with a number of employees comprised in the corresponding bin.

Figure A2: **Raw trends: above-median-wage occupations**



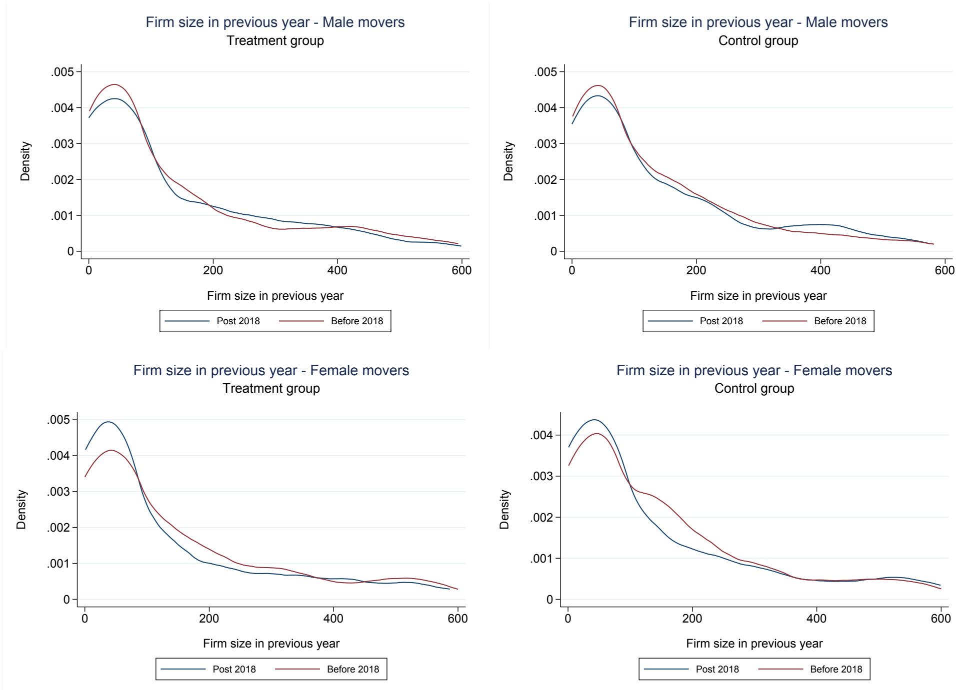
Source: ASHE, 2012-2019.
Note: This figure reports the trends in the probability of working in above-median-wage occupations. The top graph refers to men, the bottom one to women. The blue line represents the treatment group, individuals working in firms with 250-300 employees, and the red line the control group, individuals working in firms with 200-249 employees.

Figure A3: **Event studies: job mobility**



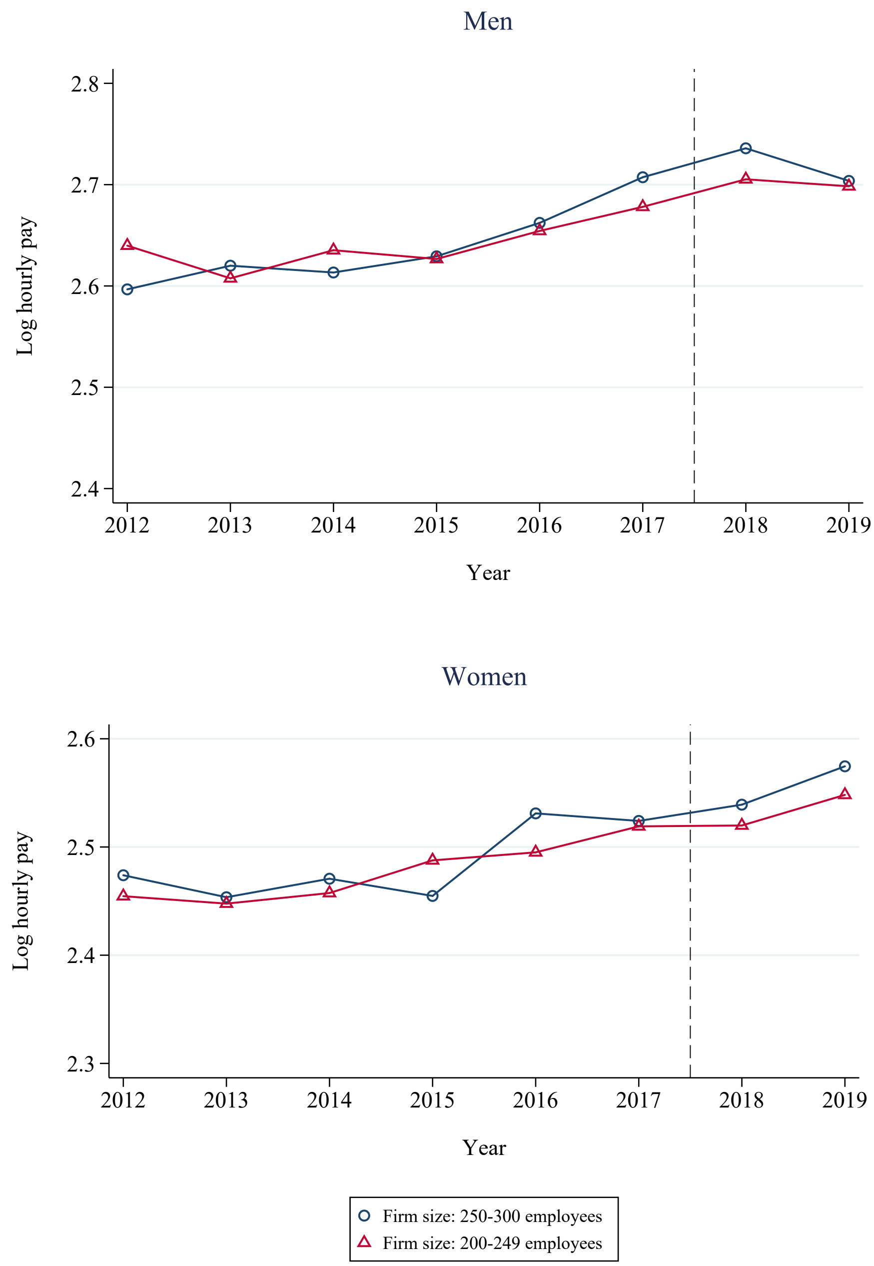
Source: ASHE, 2012-2019.
Note: This figure reports the estimates of the leads and lags of the policy on the probability of having changed job since last year and tenure in the firm. These results are obtained from the estimation of regression 2. The graphs on the left refer to men, while the ones on the right refer to women. In the left (right) graphs, 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 associated with firm-level clustered s.e. are also reported. The dash vertical line indicates the month when the mandate is approved, i.e., February 2017.

Figure A4: **Number of employees in previous firm - movers**



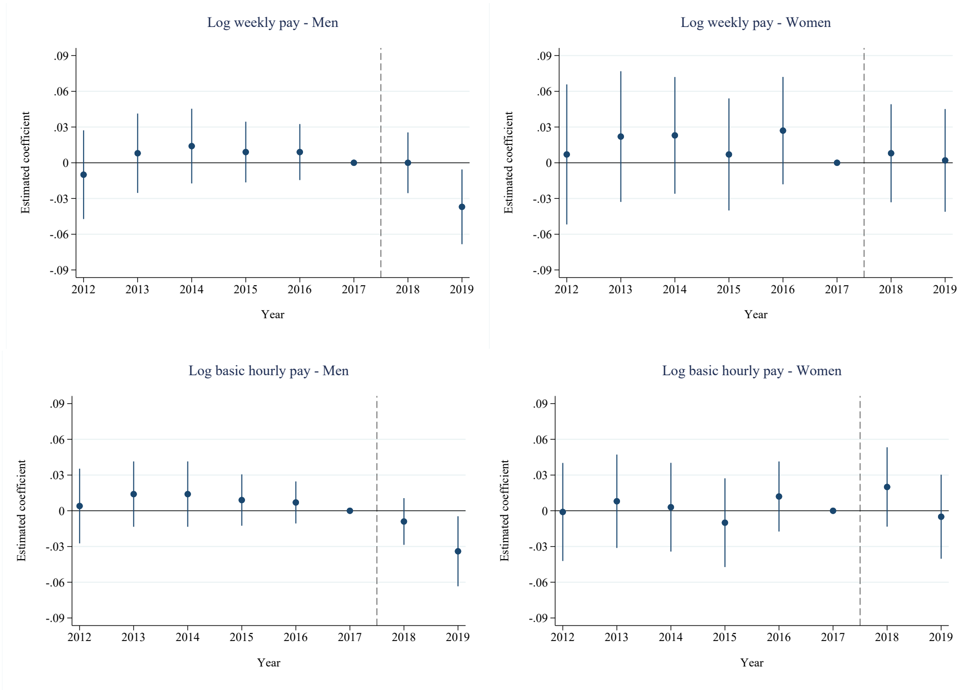
Source: ASHE, 2012-2019.
Note: These graph report the size distribution of the previous firm for workers that have changed job since last year. N. observations: top-left graph 184 (blue line) and 280 (red line); top-right graph 226 (blue line) and 391 (red line); bottom-left graph 169 (blue line) and 293 (red line); bottom-right graph 243 (blue line) and 364 (red line).

Figure A5: **Raw trends: log real hourly pay**



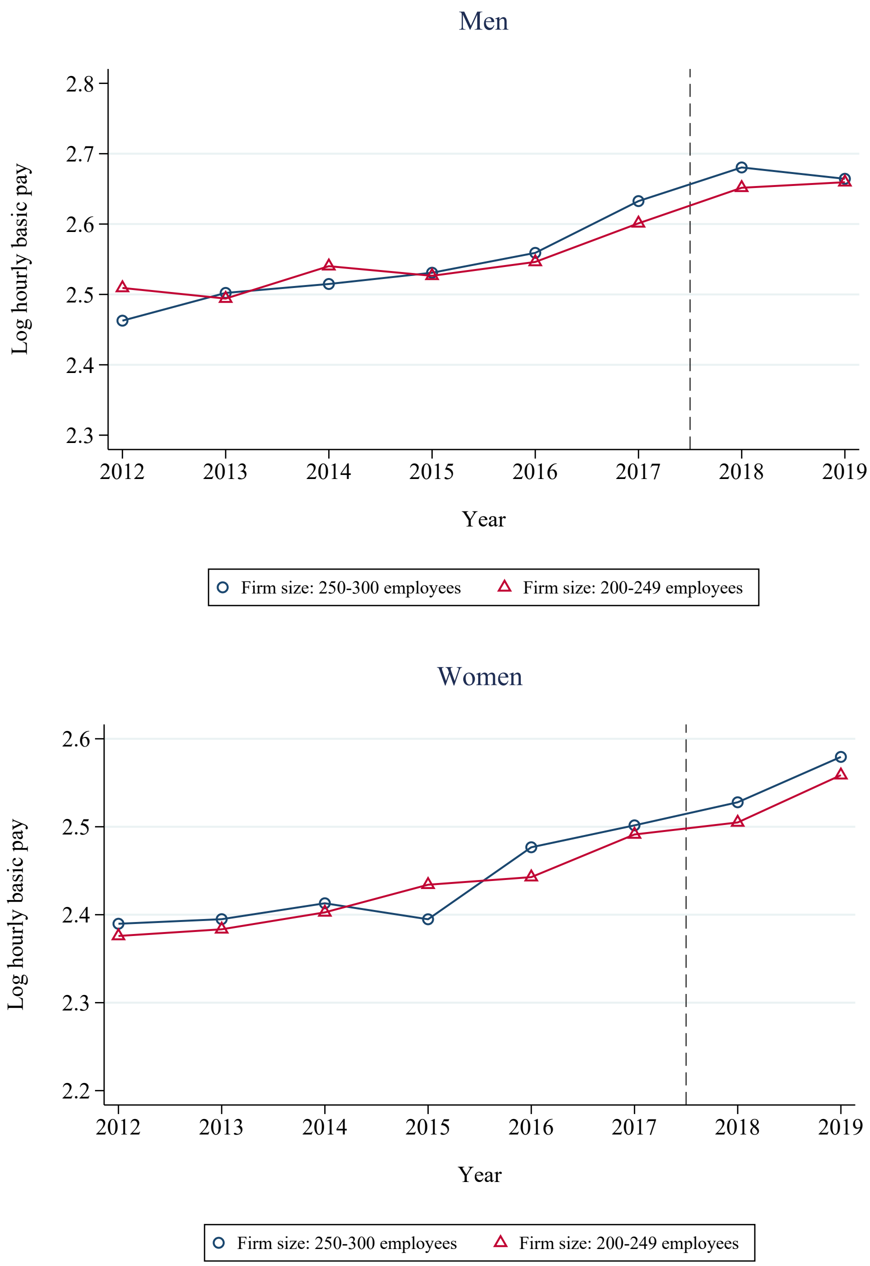
Source: ASHE, 2012-2019.
Note: This figure reports the trends in log real hourly pay. The top graph refers to men, the bottom one to women. The blue line represents the treatment group, individuals working in firms with 250-300 employees, and the red line the control group, individuals working in firms with 200-249 employees.

Figure A6: **Event studies: alternative pay measures**



Source: ASHE, 2012-2019.
Note: This figure reports the estimates of the leads and lags of the policy on log weekly pay and log basic hourly pay. These results are obtained from the estimation of regression 2. The graphs on the left refer to men, while the ones on the right refer to women. In the left (right) graphs, 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 associated with firm-level clustered s.e. are also reported. The dash vertical line indicates the month when the mandate is approved, i.e., February 2017.

Figure A7: **Raw trends: log nominal hourly basic pay**



Source: ASHE, 2012-2019.
Note: This figure reports the trends in log nominal hourly basic wages. The top graph refers to men, the bottom one to women. The blue line represents the treatment group, individuals working in firms with 250-300 employees, and the red line the control group, individuals working in firms with 200-249 employees.

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 the probability of working in occupations in each wage tercile, obtained from the estimation of regression 1. Panel A presents results for men, Panel B for women. Each column refers to a different outcomes, as specified at the top of each column. The estimation sample comprises men (women) working in firms that have between 200 and 300 employees. All regressions include firm and year fixed effects, region-specific time shocks, 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 estimated with LFS weights. 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 A2: **Impact on log real hourly pay by tenure**

	Entire (1)	Tenure $\leq$ 2 years (2)	Tenure $>$ 2 years (3)
Panel A: Men			
Treated Firm*Post	-0.0277**	-0.0182	-0.0215*
	(0.0111)	(0.0327)	(0.0113)
Observations	24658	7973	16685
Pre-Treatment Mean	16.92	13.51	19.20
P-value Low vs High		0.928	
Panel B: Women			
Treated Firm*Post	0.00243	0.0449	0.000524
	(0.0143)	(0.0480)	(0.0160)
Observations	21484	13867	7617
Pre-Treatment Mean	13.89	11.65	14.71
P-value Low vs High		0.306	

Source: ASHE, 2012-2019.

Notes: This table reports the impact of pay transparency on log real hourly pay by tenure in the firm, obtained from the estimation of regression 1 on each subgroup. Panel A presents results for men, Panel B for women. The first column reports results for the entire sample, the second and third columns refer to the subgroup indicated on top of them. The estimation sample comprises men (women) working in firms that have between 200 and 300 employees. In column 2 (3), it is restricted to individuals with at most (more than) two years of tenure. All regressions include year, firm, 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 estimated with LFS weights. 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. The p-value at the bottom of each panel corresponds to the test of equality of coefficients displayed in columns 2 and 3.

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

Table A3: **Impact on log real hourly pay by occupation**

	Entire sample (1)	Below-median wage (2)	Above-median wage (3)
Panel A: Men			
Treated Firm*Post	-0.0277**	-0.0142	-0.0222
	(0.0111)	(0.0150)	(0.0144)
Observations	24658	9625	15033
Pre-Treatment Mean	16.92	10.12	21.47
P-value Low vs High		0.70	
Panel B: Women			
Treated Firm*Post	0.00243	0.00587	-00853
	(0.0143)	(0.0211)	(0.0188)
Observations	21484	8547	12917
Pre-Treatment Mean	13.89	8.83	17.09
P-value Low vs High		0.61	

Source: ASHE, 2012-2019.

Notes: This table compares the impact of pay transparency on log real hourly pay for workers employed in below- and above-median-wage occupations. The coefficients are obtained from the estimation of regression 1 on each subgroup. Panel A presents results for men, Panel B for women. The first column reports results for the entire sample, the second and third columns refer to the subgroup indicated on top of them. The estimation sample comprises men (women) working in firms that have between 200 and 300 employees. In column 2 (3), it is restricted to individuals employed in below- (above-) median-wage occupations. All regressions include year, firm, 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 estimated with LFS weights. 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. The p-value at the bottom of each panel corresponds to the test of equality of coefficients displayed in columns 2 and 3.

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

Table A4: **Impact on above-median-wage occupations - placebo regressions**

	150 (1)	**250** (2)	350 (3)	450 (4)
Panel A: Men				
Treated firm*post	0.00823	-0.00340	-0.00308	-0.00909
	(0.00923)	(0.0118)	(0.0145)	(0.0175)
Observations	41323	24658	17414	13233
Panel B: Women				
Treated firm*post	-0.00725	0.0309**	-0.00480	0.0311
	(0.0101)	(0.0138)	(0.0177)	(0.0225)
Observations	36619	21484	14668	11214

Source: ASHE, 2012-2019.

Notes: This table reports the impact of placebo policies on the probability of working in above-median-wage occupations, obtained from the estimation of regression 1. Panel A presents results for men, Panel B for women. In each regression, the estimation sample comprises men (women) working in firms that have +/- 50 employees from the threshold c specified at the top of each column. All regressions include firm and year fixed effects, region-specific time shocks, 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 c employees in 2015, where c is the threshold specified at the top of each column. Heteroskedasticity-robust standard errors clustered at firm level in parentheses.

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

Table A5: **Impact on log hourly pay - placebo regressions**

	150	**250**	350	450
	(1)	(2)	(3)	(4)
Panel A: Men				
Treated firm*post	-0.00391	-0.0277**	-0.0159	-0.0114
	(0.00862)	(0.0111)	(0.0124)	(0.0188)
Observations	41323	24658	17414	13233
Panel B: Women				
Treated firm*post	-0.00230	0.00243	0.000264	0.00146
	(0.00975)	(0.0143)	(0.0185)	(0.0202)
Observations	36619	21484	14668	11214

Source: ASHE, 2012-2019.

Notes: This table reports the impact of placebo policies on log real hourly pay, obtained from the estimation of regression 1. Panel A presents results for men, Panel B for women. In each regression, the estimation sample comprises men (women) working in firms that have +/- 50 employees from the threshold c specified at the top of each column. All regressions include firm and year fixed effects, region-specific time shocks, and individual fixed effects. The post dummy is equal to one from 2018 onward. A treated firm is defined as having at least c employees in 2015, where c is the threshold specified at the top of each column. Heteroskedasticity-robust standard errors clustered at firm level in parentheses.

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

Table A6: **Impact on above-median-wage occupations - different bandwidths**

	30	35	40	45	**50**	55	60	65	70	75	80
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Men											
Treated firm*post	0.00163	-0.00296	0.00147	-0.00434	-0.00340	-0.00544	-0.000226	-0.00377	-0.00640	-0.00133	0.00248
	(0.0151)	(0.0142)	(0.0130)	(0.0125)	(0.0118)	(0.0114)	(0.0109)	(0.0104)	(0.00995)	(0.00955)	(0.00918)
Observations	14847	17158	19722	21992	24658	27286	29814	32443	35116	37933	40520
Panel B: Women											
Treated firm*post	0.0550***	0.0383**	0.0417**	0.0295*	0.0309**	0.0339***	0.0243*	0.0162	0.0181	0.0138	0.0108
	(0.0202)	(0.0180)	(0.0163)	(0.0152)	(0.0138)	(0.0131)	(0.0125)	(0.0119)	(0.0113)	(0.0110)	(0.0107)
Observations	12573	14785	16995	19131	21484	23757	25953	28225	30568	32796	35014

Source: ASHE, 2012-2019.

Notes: This table reports the impact of pay transparency on the probability of working in above-median-wage occupations, obtained from the estimation of regression 1. Panel A presents results for men, Panel B for women. In each regression, the estimation sample comprises men (women) working in firms that have +/- h employees from the 250 threshold, where h is indicated at the top of each column. All regressions include firm and year fixed effects, region-specific time shocks, 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 estimated with LFS weights. Heteroskedasticity-robust standard errors clustered at firm level in parentheses.

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

Table A7: **Impact on log real hourly pay - different bandwidths**

	30	35	40	45	**50**	55	60	65	70	75	80
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Men											
Treated firm*post	-0.0197	-0.0236*	-0.0218*	-0.0249**	-0.0277**	-0.0248**	-0.0242**	-0.0238**	-0.0231**	-0.0218**	-0.0226***
	(0.0148)	(0.0140)	(0.0126)	(0.0120)	(0.0111)	(0.0104)	(0.00972)	(0.00934)	(0.00897)	(0.00857)	(0.00819)
Observations	14847	17158	19722	21992	24658	27286	29814	32443	35116	37933	40520
Panel B: Women											
Treated firm*post	-0.000162	0.00651	0.00472	0.00348	0.00243	-0.00306	-0.00140	-0.00362	-0.00444	-0.000907	-0.00197
	(0.0203)	(0.0187)	(0.0171)	(0.0157)	(0.0143)	(0.0136)	(0.0128)	(0.0121)	(0.0114)	(0.0112)	(0.0107)
Observations	12573	14785	16995	19131	21484	23757	25953	28225	30568	32796	35014

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. Panel A presents results for men, Panel B for women. In each regression, the estimation sample comprises men (women) working in firms that have +/- h employees from the 250 threshold, where h is indicated at the top of each column. All regressions include firm and year fixed effects, region-specific time shocks 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 estimated with LFS weights. Heteroskedasticity-robust standard errors clustered at firm level in parentheses.

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

Table A8: **Treatment status based on past vs actual firm size**

	Above-median-wage occupation		Log real hourly pay	
	2015	Current	2015	Current
	(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

Source: ASHE, 2012-2019.

Notes: This table compares the results of our specification with those obtained by defining treatment status based on actual firm size. In columns 1-2, the outcome is the probability of working in above-median-wage occupations, while columns 3-4 it is the 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 men (women) working in firms that have between 200 and 300 employees. All regressions include firm and year fixed effects, and region-specific time shocks. 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 in 2015, while in the second column a firm is treated whenever it has at least 250 employees. All regressions are estimated with LFS weights. 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. 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 panels A and B).
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A9: **Impact on main outcomes - ASHE only**

	Above-median-wage occupation (1)	Changed job since last year (2)	Tenure in months (3)	Log real hourly pay (4)
Panel A: Men				
Treated Firm*Post	-0.00451	0.0166	-0.245	-0.0239**
	(0.0125)	(0.0153)	(2.895)	(0.0115)
Observations	20649	20649	20623	20649
Pre-Treatment Mean	0.69	0.17	90.20	16.96
Panel B: Women				
Treated Firm*Post	0.0318**	0.0497***	-9.774***	0.00248
	(0.0150)	(0.0165)	(2.604)	(0.0149)
Observations	17886	17886	17858	17886
Pre-Treatment Mean	0.65	0.20	74.64	13.90

Source: ASHE, 2012-2019.

Notes: This table reports the impact of pay transparency on the main outcomes, obtained from the estimation of regression 1 on the sample of firms that have non-missing information on the number of employees in ASHE. Panel A presents results for men, Panel B for women. Each column refers to a different outcome, as specified at the top of each column. The estimation sample comprises men (women) working in firms that have between 200 and 300 employees. All regressions include firm and year fixed effects, and region-specific time shocks. In columns 1-3 individual controls include age and age squared, while in column 4 they include 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 estimated with LFS weights. 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 A10: **Impact on above-median-wage occupations - changing the estimation sample**

	Main specification		Age 25+		Age 16-65		Full-time	
	With	Without	With	Without	With	Without	With	Without
	LFS weights		LFS weights		LFS weights		LFS weights	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Men								
Treated Firm*Post	-0.00340	-0.00202	-0.00393	-0.00271	-0.00170	0.00161	-0.00669	-0.00455
	(0.0118)	(0.0129)	(0.0121)	(0.0133)	(0.0121)	(0.0132)	(0.0123)	(0.0136)
Observations	24658	24658	21895	21895	24146	24146	22088	22088
Pre-Treatment Mean	0.69	0.60	0.71	0.63	0.69	0.61	0.72	0.64
Panel B: Women								
Treated Firm*Post	0.0309**	0.0313**	0.0347**	0.0337**	0.0266*	0.0273*	0.0322**	0.0340**
	(0.0138)	(0.0143)	(0.0138)	(0.0143)	(0.0139)	(0.0144)	(0.0156)	(0.0166)
Observations	21484	21484	18922	18922	21116	21116	14161	14161
Pre-Treatment Mean	0.65	0.61	0.69	0.65	0.65	0.61	0.73	0.69

Source: ASHE, 2012-2019.

Notes: This table reports the impact of pay transparency on the probability of working in above-median-wage occupations, obtained from the estimation of regression 1. 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. The estimation sample comprises men (women) working in firms that have between 200 and 300 employees. All regressions include firm and year fixed effects, region-specific time shocks, 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 variable for the treated group between 2012 and 2017.

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

Table A11: **Impact on log hourly pay - changing the estimation sample**

	Main specification		Age 25+		Age 16-65		Full-time	
	With	Without	With	Without	With	Without	With	Without
	LFS weights		LFS weights		LFS weights		LFS weights	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Men								
Treated Firm*Post	-0.0277**	-0.0268**	-0.0202*	-0.0193**	-0.0267**	-0.0258**	-0.0221**	-0.0204**
	(0.0111)	(0.0105)	(0.0104)	(0.00984)	(0.0112)	(0.0105)	(0.0104)	(0.00980)
Observations	24658	24658	21895	21895	24146	24146	22088	22088
Pre-Treatment Mean	16.92	15.82	17.96	16.74	16.95	15.88	17.44	16.37
Panel B: Women								
Treated Firm*Post	0.00243	0.00243	0.00569	0.00537	-0.000606	-0.000985	0.0109	0.0107
	(0.0143)	(0.0140)	(0.0147)	(0.0143)	(0.0145)	(0.0142)	(0.0167)	(0.0160)
Observations	21484	21484	18922	18922	21116	21116	14161	14161
Pre-Treatment Mean	13.89	13.40	14.70	14.10	13.91	13.43	14.56	14.09

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. 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. The estimation sample comprises men (women) working in firms that have between 200 and 300 employees. All regressions include firm and year fixed effects, region-specific time shocks, 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 variable for the treated group between 2012 and 2017.

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

Table A12: **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	0.69	0.69	0.69
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	0.65	0.65	0.65
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 above-median-wage occupations, obtained from the estimation of regression 1. Panel A presents results for men, Panel B for women. Each regression uses different clustering groups for the standard errors as specified at the top of each column. The estimation sample comprises men (women) working in firms that have between 200 and 300 employees. All regressions include firm and year fixed effects, region-specific time shocks, 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 estimated with LFS weights. The pre-treatment mean represents the mean of the outcome variable 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 panels A and B).

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

Table A13: **Impact on log real hourly pay - 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	16.92	16.92	16.92
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	13.89	13.89	13.89
Number of clusters	4639	101	655
P-value Men Vs Women	0.082	0.041	0.028

Source: ASHE, 2012-2019.

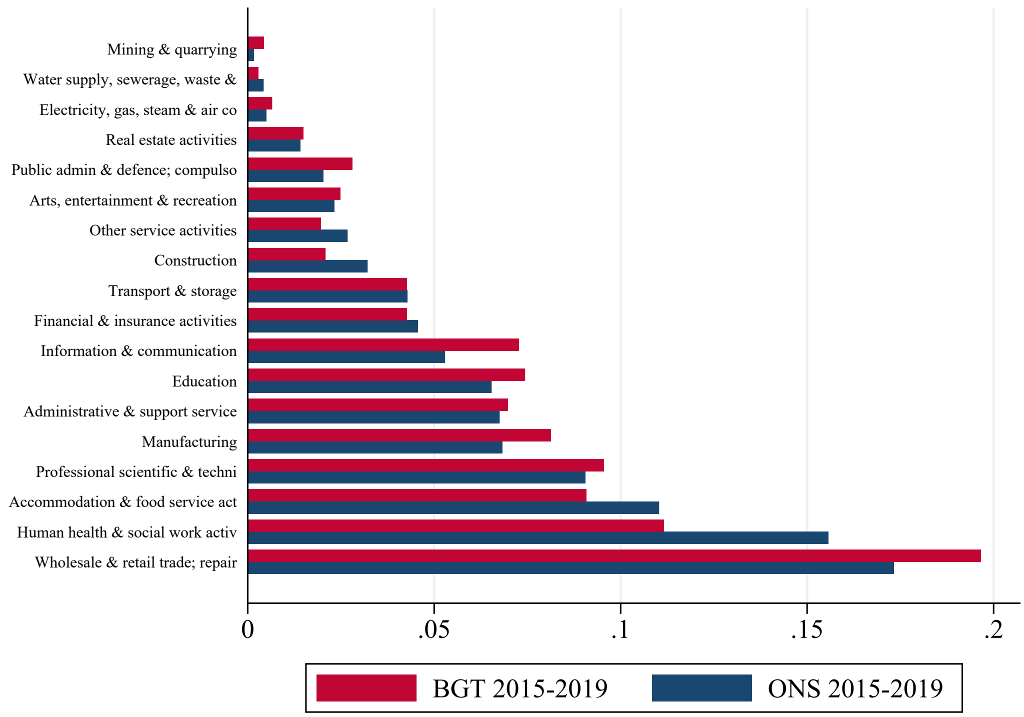
Notes: This table reports the impact of pay transparency on log real hourly pay, obtained from the estimation of regression 1. Panel A presents results for men, Panel B for women. Each regressions uses different clustering groups for the standard errors as specified at the top of each column. The estimation sample comprises men (women) working in firms that have between 200 and 300 employees. All regressions include firm and year fixed effects, region-specific time shocks, 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 estimated with LFS weights. The pre-treatment mean represents the mean of the outcome variable 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 panels A and B).

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

B Burning Glass Technologies

B.1 Representativity

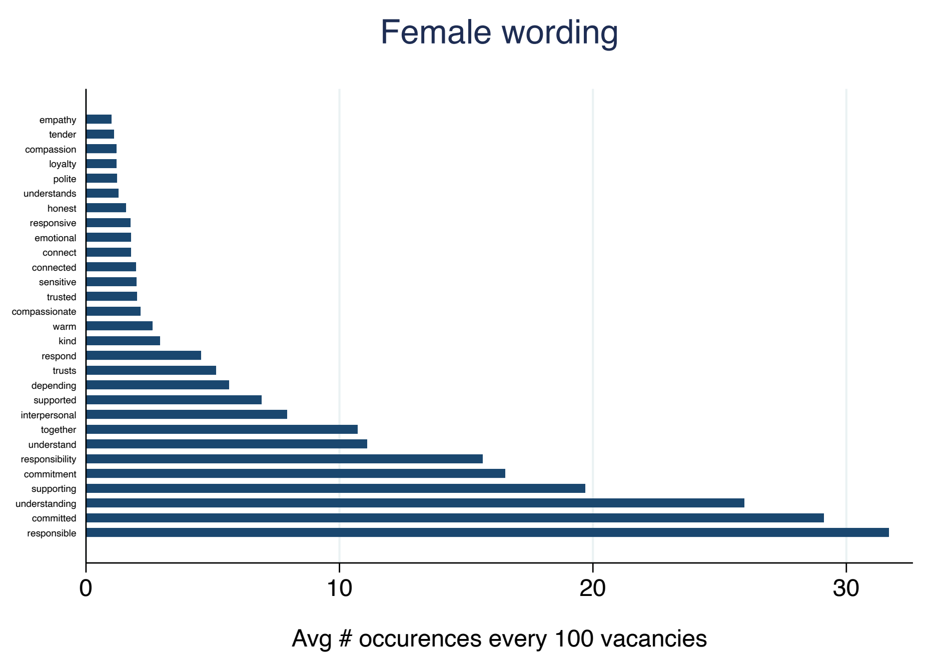
Figure B1: **Industry distribution in BGT and ONS Vacancy Survey**

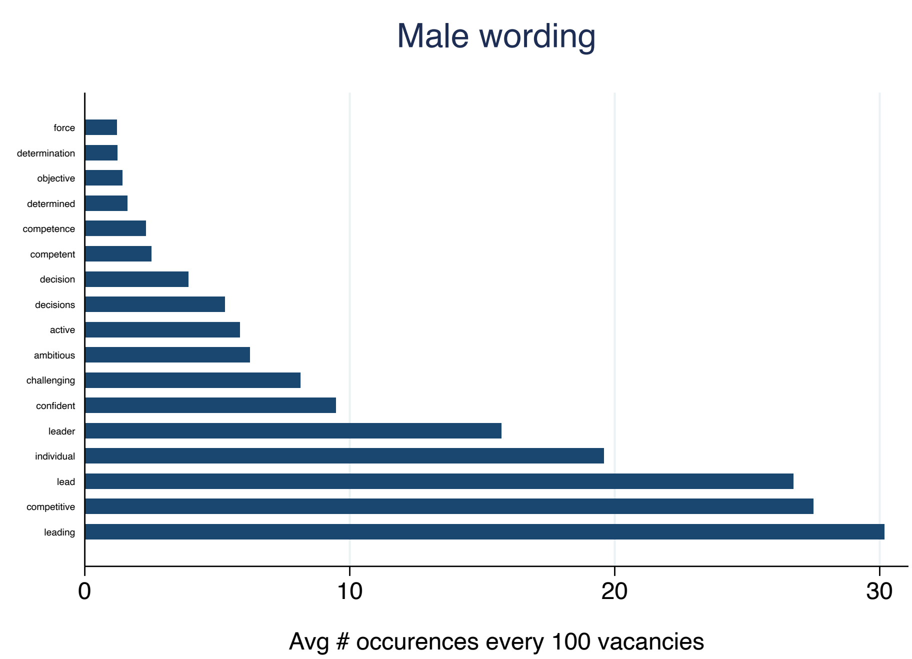


Source: BGT, ONS Vacancy Survey, 2015-2019.
Note: This figure compares the industry distribution in the stock of BGT vacancies with non-missing employer name and in the ONS Vacancy Survey.

B.2 Outcomes analyzed

Figure B2: **Gendered wording**





Source: BGT, 2015-2019.
Note: The two graphs report the frequencies of female-oriented (top) and male-oriented (bottom) words every 100 vacancies. Only words that appear at least once every 100 vacancies are included in the graphs.

Table B1: **List of gendered words**

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

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

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

Table B2: **Vocabulary for flexible work arrangements**

(1)	(2)
annualised hours	mobile work
compressed hours	mobile working
flextime	nine day fortnight
flexible working	remote working
ffour and a half day week	telework
home work	teleworking
home working	returner

Source: Based on LFS and Timewise.

Notes: This table presents the words used to measure full-time flexible work arrangements.

B.3 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 into, 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).

Table B3: **Gender equality indicators and name-matching algorithm**

	Entire sample	Match score below 1	Match score above 1	P-value difference
	(1)	(2)	(3)	(4)
Panel A: 2017/2018				
Mean gender hourly pay gap	14.60	14.29	14.77	0.15
	(15.46)	(15.52)	(15.43)	
Median gender hourly pay gap	11.86	11.99	11.78	0.55
	(16.21)	(16.80)	(15.85)	
Mean gender bonus gap	24.71	24.54	24.81	0.77
	(43.67)	(44.17)	(43.38)	
Median gender bonus gap	13.34	13.97	12.98	0.36
	(50.67)	(52.07)	(49.84)	
% women top quartile	37.19	35.96	37.90	0.00
	(24.74)	(24.33)	(24.94)	
Observations	9,410	3,464	5,946	
Panel B: 2018/2019				
Mean gender hourly pay gap	14.45	14.14	14.63	0.11
	(14.69)	(14.96)	(14.52)	
Median gender hourly pay gap	12.02	12.17	11.93	0.46
	(15.83)	(15.98)	(15.74)	
Mean gender bonus gap	25.25	25.46	25.12	0.69
	(40.60)	(41.38)	(40.13)	
Median gender bonus gap	12.84	14.83	11.65	0.00
	(50.12)	(48.42)	(51.07)	
% women top quartile	37.80	36.73	38.43	0.00
	(24.84)	(24.64)	(24.94)	
Observations	9,684	3,603	6,081	

Source: BGT, GEO, 2015-2019.

Notes: This table explores potential selection patterns of GEO firms due to the name-matching algorithm. The sample includes GEO firms with non-missing registration numbers. Panel A refers to the year 2017/18, while Panel B concerns the second year of publication of gender equality indicators. For each year, the first column reports gender equality indicators for the entire sample, the second column refers to firms with a match-score with BGT data lower than 1, the third column to those with a match-score of 1, and the last column reports the p-value of the difference in the two sample means.

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

B.4 Descriptive analysis

Table B4: **Hiring practices and gender equality indicators**

	% women at the top				Median gender pay gap			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Female-oriented wording	0.0217**			0.0206**	0.0582***			0.0591***
	(0.00966)			(0.00963)	(0.00796)			(0.00789)
Offer flexible work		0.0870***		0.0827***		-0.0461***		-0.0451***
		(0.0231)		(0.0230)		(0.0175)		(0.0173)
Wage posted			0.0452***	0.0440***			-0.0580***	-0.0580***
			(0.00998)	(0.00997)			(0.00801)	(0.00794)
Observations	4722	4722	4722	4722	4722	4722	4722	4722
Occupation shares	✓	✓	✓	✓	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓	✓	✓	✓	✓

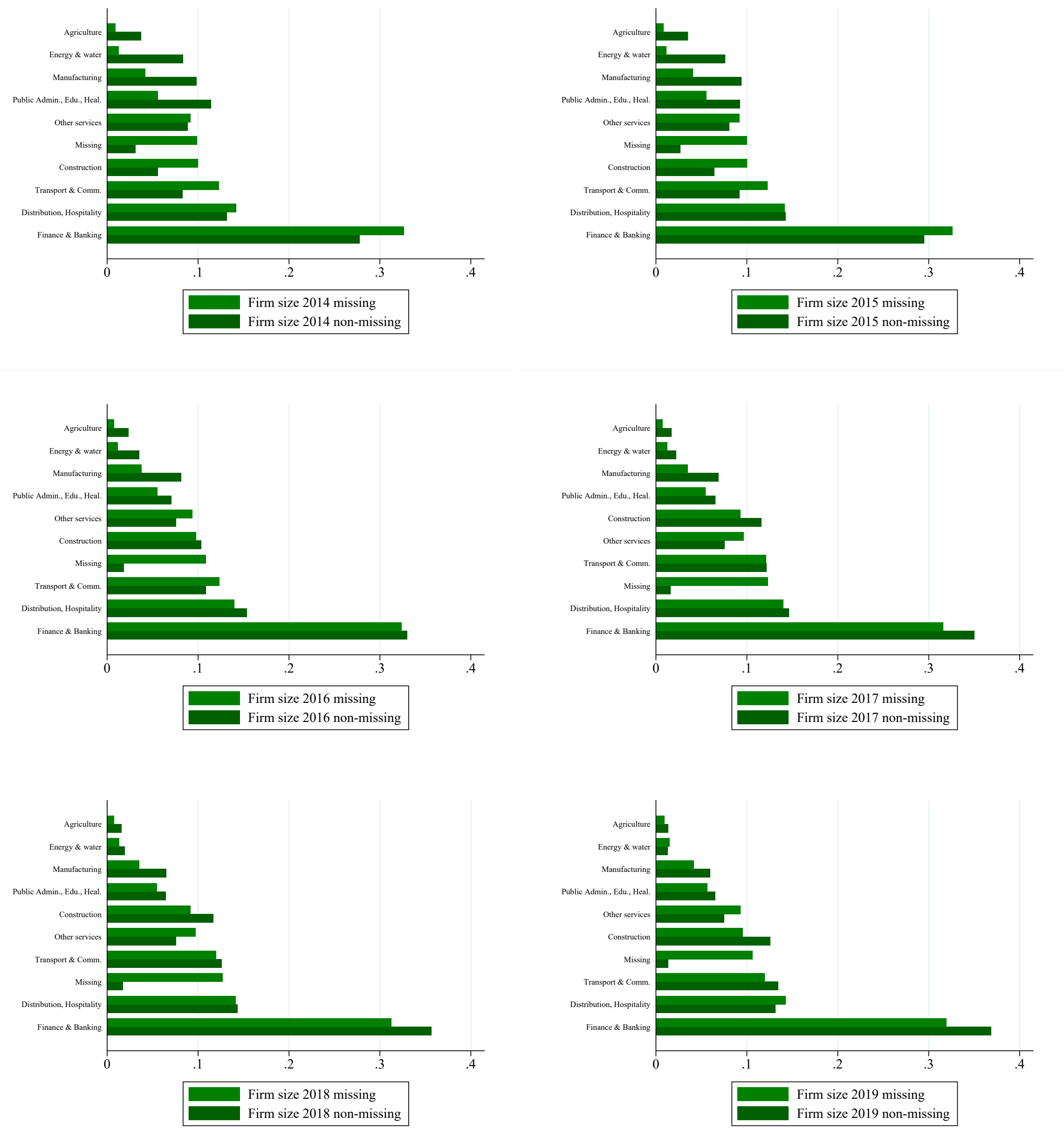
Source: BGT 2015-2019, GEO 2018-2019.

Notes: This table presents conditional correlations between firms' hiring practices and gender equality indicators. The sample includes firms that have published gender equality indicators both in 2018 and 2019, and have been perfectly matched with BGT via the name-matching algorithm. The dependent variables are averages of gender equality indicators across 2017/18 and 2018/19. The variables female-oriented wording, offer of flexible work, and wage posted are averaged over firms' vacancies for the period 2015-2019. Controls include the occupational composition of vacancies over the period considered, and industry fixed effects.

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

B.5 Regression analysis

Figure B3: **Representativity of FAME sample**



Source: FAME, 2014-2019.
Note: The graphs compare the industry distribution of FAME firms with missing and non-missing size information in each year considered.

Table B5: **Firm size and name-matching algorithm**

	Entire sample	Match score below 1	Match score above 1	P-value difference
	(1)	(2)	(3)	(4)
Number of employees	452.97	433.94	471.83	0.1591
	(13.45)	(23.14)	(13.82)	
Observations	41,649	20,738	20,911	

Source: BGT, FAME, 2015-2019.
Notes: This table explores potential selection patterns of FAME firms due to the name-matching algorithm. The first column reports the average firm size for the entire sample, the second column refers to firms with a match-score with BGT data lower than 1, the third column to those with a match-score of 1, and the last column reports the p-value of the difference in the two sample means. The entire sample refers to FAME firms with non-missing firm size comprised between 100 and 500 employees.
*** p<0.01, ** p<0.05, * p<0.1.

Table B6: **Impact on hiring practices - 2013-2019**

	Entire sample (1)	Low GPG (2)	High GPG (3)
Panel A: Gender score			
Treated Firm*Post	0.0342	0.0205	0.120
	(0.0575)	(0.0617)	(0.0806)
Pre-Treatment Mean	0.12	0.15	0.05
P-value difference		0.29	
Panel B: Flexible work			
Treated Firm*Post	0.0110	0.00643	0.0250
	(0.0130)	(0.00984)	(0.0249)
Pre-Treatment Mean	0.03	0.03	0.03
P-value difference		0.42	
Panel C: Wage posting			
Treated Firm*Post	-0.0193	-0.0181	0.0191
	(0.0239)	(0.0236)	(0.0247)
Pre-Treatment Mean	0.29	0.28	0.32
P-value difference		0.29	
Observations	129877	94980	34897

Source: BGT, FAME, GEO, 2013-2019.

Notes: This table reports the impact of pay transparency on firms' hiring practices, obtained from the estimation of regression 5. The estimation sample comprises firms that have between 200 and 300 employees. All regressions include firm and month fixed effects, and region-specific time-shocks. The post dummy is equal to one from 2018 onward. A treated firm is defined as having at least 250 employees in 2015. High-gender-pay-gap occupations include managerial, skilled trades, machine operatives and elementary occupations. 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 2013 and 2017. The p-value reported at the bottom of each panel refers to the test of equality of coefficients on low- and high-gender-pay-gap occupations.

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

Table B7: **Impact on hiring practices - original outcomes**

	Entire sample (1)	Low GPG (2)	High GPG (3)
Panel A: Gender score			
Treated Firm*Post	0.0463	0.0644	0.115
	(0.0703)	(0.0716)	(0.0876)
Pre-Treatment Mean	0.62	0 .73	0.33
P-value difference		0.60	
Panel B: Flexible work			
Treated Firm*Post	0.00645	0.00166	0.0228
	(0.0147)	(0.0119)	(0.0278)
Pre-Treatment Mean	0.06	0.06	0.06
P-value difference		0.43	
Panel C: Wage posting			
Treated Firm*Post	-0.0233	-0.0214	0.0199
	(0.0234)	(0.0232)	(0.0253)
Pre-Treatment Mean	0.25	0.24	0.28
P-value difference		0.14	
Observations	97831	71354	26477

Source: BGT, FAME, GEO 2015-2019.

Notes: This table reports the impact of pay transparency on firms' hiring practices, obtained from the estimation of regression 5. Compared to table 14, here the gender score is constructed using all terms from the list of Gaucher et al. (2011), and job sharing is included in the definition of FWA. The estimation sample comprises firms that have between 200 and 300 employees. All regressions include firm and month fixed effects, and region-specific time-shocks. The post dummy is equal to one from 2018 onward. A treated firm is defined as having at least 250 employees in 2015. High-gender-pay-gap occupations include managerial, skilled trades, machine operatives and elementary occupations. 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 2014 and 2017.

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

C YouGov Rankings

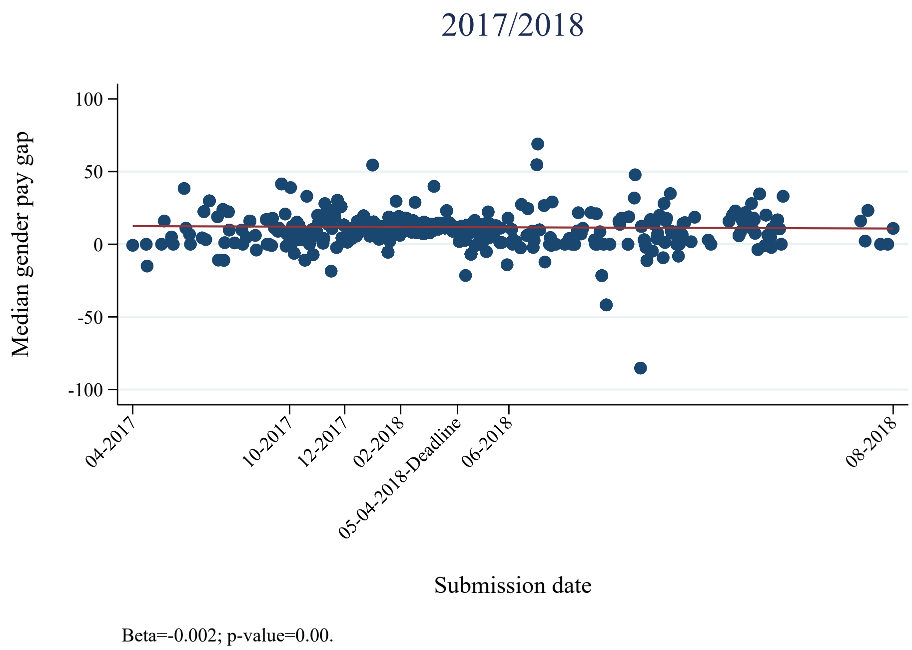
Table C8: **Gender equality indicators and firms' reputation in the workforce**

	Workforce's reputation score	
	(1)	(2)
Panel A: Two years pooled		
Median gender pay gap	-0.00612	
	(0.0137)	
% women at the top		0.0106
		(0.00923)
Observations	2018	2018
Panel B: 2017/2018		
Median gender pay gap	-0.00393	
	(0.0183)	
% women at the top		0.0107
		(0.0131)
Observations	998	998
Panel C: 2018/2019		
Median gender pay gap	-0.00902	
	(0.0208)	
% women at the top		0.0105
		(0.0130)
Observations	1020	1020

Source: GEO, YouGov, 2018-2019.
Notes: This table shows the raw correlation between firms' gender equality indicators and their score in YouGov Workforce Rankings. Panel A refers to both years, panel B refers to 2017/2018, while Panel C refers to 2018/2019. In each panel, the sample includes the GEO firms that have been perfectly matched with YouGov entries.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

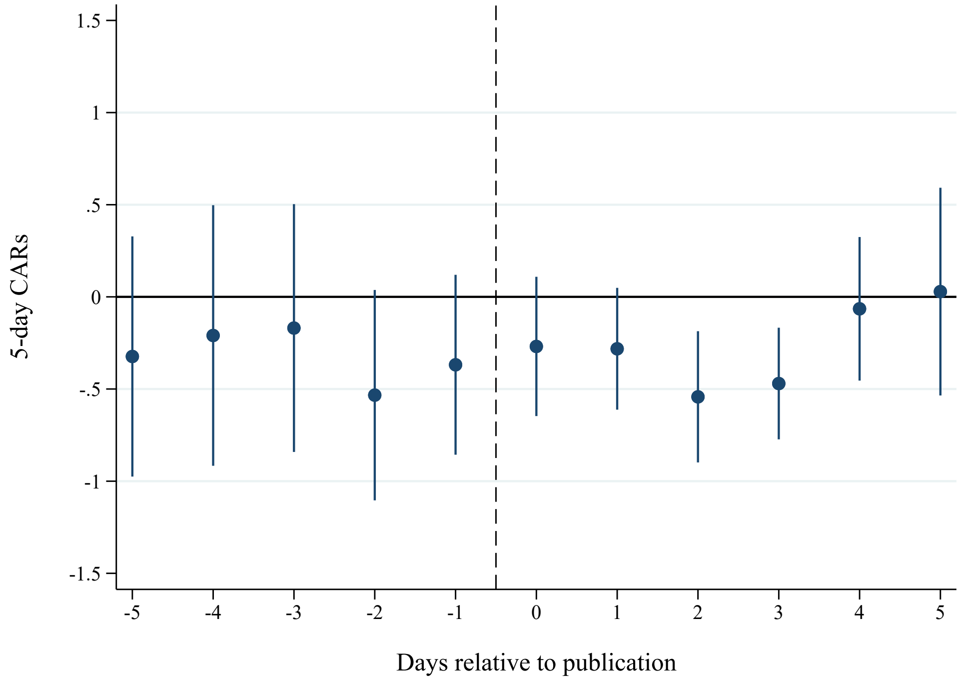
D Stock market

Figure D4: **Firms' publication date and median gender pay gap**



Source: GEO, 2018.
Note: The graph shows the relationship between firms' publication date and the median gender pay gap published. The sample includes firms publishing in 2017/18 (10,557 observations).

Figure D5: **5-day cumulative abnormal returns around 2017/18 publication date**



Source: Datastream, FAME, GEO, 2017-2018.
Note: This figure plots 5-day cumulative abnormal returns around the publication date of gender equality indicators in 2017/2018. In particular, it shows CARS(-2, 2) around the day reported on the graph. 95 percent confidence intervals associated with standard errors clustered at the level of publication date are also displayed. The sample includes firms that had to publish gender equality indicators by April 5th 2018, or that have a subsidiary that had to publish these data.