Should I stay or should I go?

The impact of pay gap transparency on female workers *

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This paper examines the effects of the UK's 2017 gender pay transparency policy, which requires large firms to disclose gender pay gap data. Using a novel employer-employee dataset and a regression discontinuity design around the treatment threshold, we find that the policy led to increased job mobility among women, especially high-skilled, native-born workers in non-managerial roles. Treated firms also reduced female hiring, while promotion rates remained unchanged. Our findings suggest that transparency influences workforce composition, contributing to a narrowing of the gender pay gap—primarily through mobility of women.

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1 Introduction

After the UK introduced mandatory gender pay gap reporting in April 2017, the BBC published its own Gender Pay Report. In January 2018, Carrie Gracie resigned as the BBC's China Editor, after learning that the other two male international editors were paid 50% more than she was, and she denounced what she described as a "secretive and illegal" pay culture.[1] This episode shows how pay transparency can have strong effects on employees' career choices when newly revealed pay gaps are perceived as unfair.

Pay transparency policies have been adopted in many countries around the world, and over half (21 on 38) of the OECD countries now require private sector employers to make available gender pay gap information to the broad audience (OECD, 2023).[2] At the same time, gender pay gaps have been slowly declining. The ONS estimates that the UK's median gender pay gap decreased to 7% in April 2024, from 12.2% in 2009, while OECD data show a fall from 16% in 2005 to 11% in 2023, and from 14.1% to 9% during the same period within the EU-27.[3]

Despite this recent decline, gender pay disparities remain a concern for employers, policymakers, and workers, sustaining an active policy debate about how best to reduce it. Wage transparency policies - and the responses they trigger - are central to this conversation as a potential tool for reducing gender inequality. As in the Carrie Grace case, wage transparency rules generate an information shock in a domain that is typically hidden or opaque: coworkers' pay (Cullen and Perez-Truglia, 2023).

Women tend to have lower earning expectations than men and face high information frictions when entering the labour market (Leibbrandt and List, 2015; Kiessling et al., 2024). By revealing information about relative pay and, more broadly, about the firm's compensation culture, transparency changes women's beliefs about the returns to staying at the firm. In principle, increasing transparency in gender inequality should, by reducing information asymmetries, empower underpaid workers to bargain for higher salaries or adjust work effort. In practice, women typically have lower bargaining power and less aggressive negotiation skills than men (Manning and Saidi, 2010; Dittrich et al., 2014; Leibbrandt and List, 2015). If internal wage renegotiation is constrained by bargaining frictions, pay rigidity, or weak worker bargaining power, the most salient adjustment margin may be separation rather than wage revision. In this case, transparency affects not only wages, but also retention, by reducing job satisfaction and perceived fairness and by increasing the salience of outside options (Card et al., 2012).

From a policy perspective, identifying the first-order effect of pay transparency rules is important, but

[1] See https://www.ft.com/content/dff6f1c8-06b0-11e8-9650-9c0ad2d7c5b5 (accessed 22 February 2026).

[2] Cullen (2024) distinguishes between three forms of pay transparency: horizontal pay transparency, where coworkers within the same organization know each other's pay; vertical pay transparency, when workers are informed about the pay of more senior co-workers; cross-firm transparency, where employees get access to the pay of workers in other competing firms.

[3] https://www.oecd.org/en/data/indicators/genderwagegap.html

understanding second-order behavioral responses is arguably even more important. If firms comply with pay transparency mandates in ways that only minimize their costs, wage inequality may shrink without improving women's earnings or welfare. Ignoring these behavioral adjustments risks overstating the policy's effectiveness and lead policymakers to draw incorrect conclusions about its distributional and efficiency consequences.

How do firms respond to the introduction of gender pay reporting requirements? In a profit-maximizing setting with constant productivity, firms may seek to reduce the gender pay gap by restraining male wages rather than increasing female wages, thereby lowering average labor costs. Cullen and Perez-Truglia (2023) show that when salary differences are disclosed, firms are less inclined to offer high wages to underpaid employees, since doing so could trigger pressure on wage increases from other employees. As a result, overall wage levels may decline and wage dispersion may shrink. Consistent with this mechanism, after the introduction of the 2017 UK rules, the gender pay gap among affected UK firms declined primarily due to a slowdown in men's wage growth, while women's pay remained largely unchanged (Blundell et al., 2025).

Both theory and evidence suggest that revealing wage disparities can negatively affect effort and productivity, particularly among underpaid workers. For this reason, firms may preemptively adjust wages to maintain internal equity without triggering discontent (Akerlof and Yellen, 1990; Card et al., 2012; Breza et al., 2018). Card et al. (2012) show that random information on pay differences influence both job satisfaction and job market behaviour. Workers care more about their relative pay rank within the firm than about their absolute pay level. Employees whose pay falls below the median in their department report lower job satisfaction, and are more likely to search for a new job following the disclosure of salary information.

If *relative* pay affects satisfaction and job-search decisions, it may also influence negotiation incentives and the ability to negotiate effectively. Because women are more likely than men to be on the lower part of the wage distribution, they are more likely to experience a decrease in job satisfaction and increase in intentions to leave when pay disparities are disclosed. This is consistent with the vast literature on the gender pay gap (Bennedsen et al., 2023) that shows that although a substantial proportion of the gap can be explained by observable characteristics, a residual component remains unexplained, and is often attributed to factors like differences in bargaining power or risk preference.

On average, women have weaker bargaining power than men in the labour market. This does not reflect differences in productivity or ability, but rather differences in wage-setting institutions (Blau and Kahn, 2017, 2023), job structures (Goldin, 2014, 2021), negotiation outcomes, and outside options (Manning and Saidi, 2010). Outside options differ not only between men and women, tending to be weaker for women, but within gender they can also be affected by other characteristics, like years of education, experience, age, occupation and immigrant status, among others (Manning and Saidi, 2010).

This paper studies how the gender pay transparency policy introduced in the UK in 2017 affected female workers' labour market behaviour. The policy required companies with more than 250 employees to publish gender pay gap indicators with the aim of decreasing the pay gap in companies where it existed and, potentially, in the economy as a whole.

To address this question, we use a newly assembled employer-employee database that combines worker profiles from the web with firm-level financial information, described in Gray et al. (2025). As in Blundell et al. (2025), we exploit the firm-size cutoff to provide casual estimates of how the policy affects the gender composition of the workforce. Specifically, we analyze job mobility among individuals employed at firms just above or just below the 250-employee threshold, exploiting variation in policy exposure across firm size and over time. While Blundell et al. (2025) examine the effects of the reform on the gender pay gap, we focus on job mobility and leavers' characteristics. Compared to administrative data, our database allows us to observe complete career histories rather than only job flows across observed firms. This information let us identify where female leavers move and shed light on mechanisms underlying exit decisions.

Because firms may endogenously adjust headcounts in anticipation of the policy, we define the treatment status in 2015, two years before the policy implementation. We restrict the analysis to firms with +-50 employees from the 250-employee threshold and track workers on these firms before and after the policy, regardless of whether they remain employed at firms in the estimation sample. To control for unobserved heterogeneity, we include individual, firm, and gender-region-year fixed effects, which absorb time-invariant individual and firm unobserved heterogeneity, and allow for regional shocks that affect women and men differently.

We report three main findings. First, consistent with Blundell et al. (2025), women in treated firms are more likely to change employer after the policy compared to control firms both in absolute terms and relative to men. The policy led to a significant 1.4 percentage points (about the 10% of the pre-policy probability) increase in women's probability to leave the firm compared to men's probability. This effect is robust to alternative bandwidth selections and specifications. Second, we find that the policy decreased women's probability of being hired in treated firms compared to control firms by 2.4 percentage points (16% of the baseline) - however this is driven by an increase in men's probability of being hired, with no impact on the overall women's probability of being hired. On the other hand, we do not see any effect of the policy on the probability that women are promoted to higher paid position or alternatively to a managerial role either across or within firms. Third, we offer evidence on the mechanisms behind women's decision to leave. Specifically, we show that women leavers are more likely to be on the lower part of the wage distribution - measured by the occupation's average earnings- working in non-managerial occupations and more likely of being lower skilled than men in treated firms. These findings are consistent with the literature showing that revealing pay gaps affects mainly employees in the lower end of the wage distribution and experiencing the most discontent following the informational shock.

This paper studies the effects of gender pay transparency on women's labour market outcomes. However, despite its policy relevance, there exist only few empirical investigations on this topic. Most contributions focus on the effects of pay transparency policies on the gender pay gap. Bennedsen et al. (2022) assess a legislation change in Denmark that mandated firms with more than 35 employees to report salary information separately for men and women. Using employer-employee administrative data, Bennedsen et al. (2022) find a reduction of the gender pay gap, an increasing share of hired women and greater probability of promoting women in higher paid positions in treated firms relative to control firms. Baker et al. (2023) examines the effects of the introduction of public sector disclosure laws across Canadian provinces and find a reduction in the gender gap among full-time academic employees at Canadian universities in treated provinces. However, their findings also suggest that the reduction in the gender pay gap is driven by a slower relative growth of men's salaries. On the contrary, Gulyas et al. (2023) study the effects of the 2011 Austrian pay transparency law and do not find any economically significant effect on the gender pay gap or in separation rates after the reform. They conclude that revealing wages might have actually increased job satisfaction by alleviating previous concerns about unfair salary differences. Gamage et al. (2024) study the impact of a gender pay publication by the Times Higher Education guide in 2007, finding a reduction in the gender pay gap among academics after the policy. The estimated effect is entirely driven by an increase in female wages. By leveraging a German law that allowed employees in large firms to request information on coworkers' salaries, Brütt and Yuan (2022) do not find that the policy significantly affected the gender pay gap, nor it changed the workforce composition of treated firms. Evidence on employment composition is particularly scarce. Employing proprietary data from Revelio, Dambra et al. (2025) and Liang et al. (2025) study the Pay Ratio Disclosure (PRD) law in the US to show that disclosing firm pay increases job mobility, especially in the lower-end of the wage and rank distribution within firm.

This paper is structured as follows. Section 2 introduces the UK pay transparency policy and highlights its salience in the UK context. Section 3 describes data and empirical design. Section 4 presents the main results. We discuss extensions and robustness checks in Section 5. Finally, Section 6 summarizes the conclusions and discusses policy implications.

2 The UK Gender Pay Gap policy

In 2015, the UK government started a consultation with employers to design a policy reform aimed at enhancing pay transparency. As reported by the Government, the objective was to encourage employers to adopt workplace policies and practices that promote gender equality (Government Equalities Office, 2016). Following positive feedback from the consultation, in February 2017, the UK Government enacted the Equality Act 2010 (Gender Pay Gap Information) Regulations 2017 No. 172.[4] The regulations mandated all firms registered in Great Britain with at least 250 employees to publish gender equality indicators - both on their own website and on a dedicated portal managed by the Government Equalities Office - by 30 March 2018 for the first reporting year and annually thereafter by the end of the financial year (5 April).[5] The mandated indicators include mean and median gender hourly pay gaps, mean and median gender bonus gaps, the proportion of male and female employees who received bonuses, and the proportion of female employees in each quartile of a company pay distribution.[6] This policy applies to both public and private-sector organizations in England, Wales and Scotland; Northern Ireland is excluded.[7] All companies that are part of a group are required to report indicators individually if they meet thresholds. At the time of the law, 10,500 firms were in the targeted group. Treated firms account for only 0.4% of all UK firms, but 40% of employment and 48% of turnover in the UK economy.[8] Moreover, it is worth to highlight that no other policies targeted this slice of the firm population before the gender pay gap policy.

Importantly, there are no sanctions for firms failing to improve their gender equality indicators over time. However, the Equality and Human Right Commission, responsible for the policy implementation, could issue court orders and impose unlimited fines on firms that failed to report. As reported on the Governemnt's website, 94% of eligible firms complied with the policy by the deadline in the first year. Finally, fewer than 600 non-eligible firms (i.e., those below the 250-employee threshold) voluntarily published the gender equality indicator over the years, representing less than 0.1% of firm in that population, as per the Business Structure Database statistics.

The policy was designed to create an information shock both within and outside organizations. This is also justified by the fact that, according to a survey conducted by the Government Equalities Office before

[4]For the actual legislation, see https://www.legislation.gov.uk/uksi/2017/172/contents/made.

[5]Gender pay gap reports of the covered firms are available at: https://gender-pay-gap.service.gov.uk/search

[6]To compute their gender pay gap indicators, firms must first collect data separately for "relevant employees" and "full-pay relevant employees". Relevant employees are those with a regular employment contract, including workers who are part-time, job sharing, on leave and self-employed. Full-pay employees are those receiving their full basic pay or less than their full basic pay, but not because of leave. For a complete set of instructions for firms to prepare their gender pay gap reports, see: https://www.gov.uk/government/publications/gender-pay-gap-reporting-guidance-for-employers/preparing-your-data.

[7]Public sector entities include most government departments, the armed forces, local authorities, NHS bodies, schools and universities.

[8]See Blundell et al. (2025) for more information on the sample of firms that reported over the years and the evolution of the raw firm-level indicators.

the introduction of the policy, firms were not aware of their gender gaps, and employees were discouraged to talk about salaries with their colleagues.

Finally, the policy intended to shame firms into action and it was therefore characterized by substantial salience. Every year, the indicators have received substantial media attention, and reports are posted on firms' main webpages and the government website to ensure accessibility to employees (Blundell et al., 2025). Figure A1 in the Appendix shows two examples of how the gender pay gap indicator is reported in two different firms, one that always complied with the policy and submitted its report on time, while the other one that uploaded its report with delay. Everyone can thus access the website and identify those firms that submitted their reports inaccurately.

Moreover, the policy attracted significant public interest around the time of its implementation. Figure A2 shows Google searches for ”gender wage gap” between March 2013 and March 2022. Frequencies are normalized to the peak, which is on April 2018, the time of the first deadline for reporting the gender pay gap indicators. However, the figure also displays a peak of searches in March 2017, the month after the enforcement of the policy.

3 The effect of the policy

3.1 Data and sample description

To study the effects of the UK gender pay gap policy on individual-level outcomes, we leverage a newly assembled UK employer-employee dataset, which links proprietary individual-level data from Diffbot - a commercial knowledge graph provider - to administrative company-level data. Diffbot extracts individual and company-level profiles from multiple sources across the public web and organises them into a searchable knowledge graph. Individual records include socio-demographic characteristics (gender, year of birth, self-reported languages), education histories, detailed employment histories (employer identifiers, start and end dates, job titles, and brief job descriptions), and a taxonomy of self-reported skills. Diffbot uses supervised learning techniques to assign individual workers to companies based on their career histories. For UK firms, company-level profiles include company names and unique identifiers from Companies House, the UK's Open Companies Register.[9]

We use these company identifiers to merge individual-level data from Diffbot with company-level information from Bureau van Dijk's Orbis Historical, which gives us detailed financial data for companies over a

[9]The UK CRN coincides with the ORBIS BvD identifier, which is usually the country-specific registry identifier preceded by the country two-digit code.

long time frame.[10] Linking these sources yields an employer-employee database covering more than 800,000 individuals and 10,530 'medium' and 'large' companies active between 2007 and 2023.[11] While the Appendix summarises the elements most relevant to our research design, a comprehensive description of data construction and validation is provided in Gray et al. (2025).

To construct the firm sample for our design (see section 3.3), we select organizations around the 250-employee threshold and merge them to the individual-level dataset.[12] Then, we compare outcomes for individuals employed in firms whose size is just above the threshold of 250 employees (eligible to treatment) with those in firms just below the threshold. Because consultation on the policy began in 2015 and continued through 2017, we define treatment status using firm size in 2015 to avoid endogenous anticipated responses from those firms seeking to change headcounts to move above or below the threshold. Additionally, to enhance comparability between treatment and control firms, we restrict the estimation sample to firms in a bandwidth of +-50 employees around the 250-threshold.[13]

Our baseline estimation sample includes information for 35,613 employees and 210,450 individual-year observations for the period from 2013 to 2022. We hold the individual sample constant over time, retaining only those individuals observed in all years, thereby forming a balanced panel.

3.2 Outcome variables

We examine mobility across firms and within firms (via promotions) by assessing how the policy altered workforce composition. In the absence of wage data, we cannot estimate wage effects directly; yet, we can investigate how the policy has reshaped workforce composition and infer wage effects indirectly by examining shifts from lower-paid to better-paid occupations, using occupations' average earnings.

Our first outcome variable is the probability that an individual leaves the firm. Precisely, we build a dummy variable equal to one if an individual leaves the firm the following year and zero otherwise. By construction, this variable is missing for the last year of our sample. We begin with this outcome to align our analysis with prior research. This outcome is salient because pay transparency can affect job satisfaction through bargaining power. If individuals lack the power to renegotiate wages, job satisfaction may fall; conversely, if transparency revises downward workers' priors about their wage gap, job satisfaction may rise. Although our data do not include direct measures of job satisfaction, separation rates can serve as proxies

[10]Orbis is widely used for firm-level analysis and includes harmonised cross-country financial information on close to 462 million companies worldwide, making it one of the most extensive and reliable sources of corporate financial data.

[11]We classify companies as medium or large following Companies House, which distinguishes 'micro' and 'small' companies entities from medium and large based on thresholds for turnover, assets and employee counts. Further details are available at: https://www.gov.uk/government/publications/life-of-a-company-annual-requirements/life-of-a-company-part-1-accounts.

[12]As it is common in firm-level studies, we exclude firms in agriculture and mining sectors.

[13]We assess the robustness of our results to alternative bandwidth choices. Results are reported in figure A3 in the Appendix.

(Card et al., 2012; Gulyas et al., 2023).

Our second outcome relates to the gender composition of new hires. The interest in this outcome comes from the fact that employers could have decided to close the gap by hiring more female workers. In addition, this outcome also allow us to understand how managers become more accountable for new hires and workforce composition, leading them to be more focused on observables (Castilla, 2015). On the other hand, they may hire more women in occupations where they can offer fairer compensations (Bennedsen et al., 2022). We measure this as the probability that an individual is hired by the firm in the current year.

Finally, we study occupational upgrading. In the Danish case, for example, female were less likely to leave treated firms but more likely to be promoted from the bottom of the hierarchy to more senior positions (Bennedsen et al., 2022). To implement a similar strategy, we link 4-digit SOC occupation codes to median occupational earnings by 4-digit SOC code extracted from the aggregated tables of the Annual Survey of Hours and Earnings (ASHE) provided by the UK Office of National Statistics (ONS). Occupational upgrading is then measured with a dummy variable equal to one if an individual moves from a lower-paid occupation to a better-paid occupation between $t-1$ and t. Similarly, we also examine promotions into managerial roles by constructing a dummy equal to one if an individual moves from a non-managerial to a managerial position between $t-1$ and t. We classify as managerial all occupations in the aggregated SOC 1-digit category "Managers, directors and senior officials".

3.3 Econometric design

Our aim is to estimate the impact of the UK pay transparency policy on individual labour market outcomes. We follow the identification strategy in Blundell et al. (2025), who estimate the impact of the UK's pay transparency policy on the gender gap in hourly pay and assess whether the effect operates through changes in women's or men's pay. We exploit variation in policy exposure across firm size and time, comparing the evolution of the outcome in firms just above the 250-employee threshold (treated) with firms just below it (controls) after the implementation of the policy. In our baseline analysis, we restrict the sample to firms within a bandwidth of $\pm$ 50 employees.

We employ a triple-difference regression model to estimate the impact of the policy on both men's and women's outcomes. Specifically, the policy effect is estimated for employee i, working in firm f, at time t, for the period from 2013 to 2022:

$$
\begin{aligned}
Y_{i,f,t} = {} & \beta_1 \left(Treated_f \times Post_t \right) \\
& + \beta_2 \left(Treated_f \times Post_t \times Female_i \right) \\
& + \beta_3 \left(Treated_f \times Female_i \right) \\
& + \alpha_i + \mu_f + \delta_{j,t} + \chi_{g,r,t} + \epsilon_{i,f,t}
\end{aligned} \tag{1}
$$

where $Y_{i,f,t}$ is an outcome variable at the individual level, $Post_t$ is a dummy equal to one from 2017 onward and zero before 2017, $Treated_f$ is a dummy equal to one if a firm's number of employees is between 250 and 300 employees in 2015 and 0 if it is between 200 and 249; $Female_i$ is a dummy equal to one if the individual is female (0 otherwise). We control for time-invariant individual characteristics (α_i), such as individual unobserved ability, and for time-invariant firm characteristics (μ_f). We control for time-varying shocks that are common to all firms in the same industry by including 1-digit-industry fixed effects interacted with year fixed effects ($\delta_{j,t}$), as well as for gender-specific regional shocks ($\chi_{g,r,t}$), which capture, for example, time-varying employment practices that are region-and gender-specific. In this baseline specification, we do not include any further controls.

Our parameter of interest is β_2, which identifies how the policy differently affects female outcomes relative to men in treated vis-à-vis control firms. Specifically, it captures the deviation from a parallel trend induced by the policy of the differences between females and males in the outcome variables in treated firms relative to control firms. The coefficient β_1 estimates the effect of the policy on male employees while the sum $\beta_1 + \beta_2$ gives us the effect on female employees. We formally test the effect on female employees by performing a t-test on the sum of the coefficients β_1 and β_2. Finally, the coefficient β_3 tells us the trend in the outcome variable for women in treated firms before the policy implementation. Standard errors are double-clustered at the individual and firm levels.

Our specification includes individual fixed effects to ensure that the estimated impact of the policy is not driven by selection on unobservables— for example, the most or least mobile individuals. By controlling for individual fixed effects, we account for such heterogeneity. Additionally, individual fixed effects also capture the differential sorting of men and women into high-wage firms, given that occupational choice is a major driver of the gender pay gap (Card et al., 2016; Bamieh and Ziegler, 2025). Similarly, we include firm fixed effects to rule out the possibility that our results are driven by differences across firms, such as only firms with highest/lowest employment turnover. This dual control strengthens our confidence that the observed policy effect is not confounded by selection at either the individual or firm-level. Therefore, we identify the effects of the policy within-firm and within-worker, i.e., the additional effect of the reform after controlling for unobserved but time-constant worker and company characteristics. Consequently, our results are not equally driven by individuals with some specific characteristics (i.e. greater preferences for mobility) moving to more flexible firms, which could be different across genders.

The assumption behind our research design is that, in absence of the policy, treated and control firms would have experienced parallel trends in the outcome variables between males and females. To formally test this assumption, we estimate the following dynamic specification:

$$\begin{aligned} Y_{i,f,t} = & \sum_{j=2013}^{2021} \beta_j^1 \left(Treated_f \times YEAR_{t=j} \right) \\ & + \sum_{j=2013}^{2021} \beta_j^2 \left(Treated_f \times Female_i \times YEAR_{t=j} \right) \\ & + \alpha_i + \mu_f + \delta_{j,t} + \chi_{g,r,t} + \epsilon_{i,f,t} \end{aligned} \tag{2}$$

where $YEAR_{t=j}$ is a binary variable that takes value equal to unity when $t = k$ and zero otherwise. The reference year is 2016, the year before the enforcement of the gender pay gap policy.

3.4 Descriptive statistics

This section provides summary statistics. Table 1 provides summary statistics for individual-level characteristics in our sample for the pre-policy period (2013-2017), separately for treated and control firms, as well as for the full sample. On average, employees in our sample are 44 years old and have 12 years of work experience. The share of female workers is 0.36, while the share of workers with a college degree is 0.69. Therefore, our sample under-represents female workers relative to UK statistics, while it over-represents high-skilled workers. The selection on these characteristics reflects both the paper's focus on large firms, which usually over-represent male high-skilled workers, on average, and the nature of our employer-employee database, as highlighted in Gray et al. (2025).

Several features are worth to notice. The profile of workers in treated and control firms is basically similar yet some slight differences emerge. Control firms have a slightly lower share of college-degree workers, younger workforce, more female and workers are, on average, less experienced. These small differences in firm-level variables are partly captured in our analysis by the inclusion of individual fixed effects. Results from t-tests on the differences in observable characteristics between control and treated firms indicate that college degree share, age, worker experience, promotion to manager and entering the company are the variables for which we observe statistically significant differences between the two groups.

Additionally, we run a linear probability model where the probability to be treated is regressed against the same observable characteristics in Table 1 averaged over the pre-policy period 2013-3017. Results from this test are presented in Table A2 in the Appendix. While age, being a new hire and being promoted to a managerial position are significantly related to the probability of being treated, the overall F-test on these observable characteristics is not statistically significant at standard confidence levels.

Table 1. Summary statistics, pre-policy period (2013-2017)

	Control		Treated		All (Treated + Control)			t-test
	Mean	SD	Mean	SD	Mean	SD	Obs.	p-value
College degree (share)	0.68	0.47	0.70	0.46	0.69	0.46	24,138	0.045
Migrant (share)	0.16	0.36	0.16	0.37	0.16	0.37	20,991	0.451
Age (years)	43.57	10.05	44.10	9.97	43.81	10.02	19,787	0.000
Female (share)	0.37	0.48	0.36	0.48	0.36	0.48	31,461	0.181
Worker experience (years)	12.29	7.91	12.56	7.93	12.41	7.92	31,461	0.002
Separation at $t+1$ (share)	0.13	0.21	0.13	0.20	0.13	0.21	31,461	0.989
Promotion (share)	0.14	0.24	0.14	0.24	0.14	0.24	31,461	0.174
Promotion to manager (share)	0.02	0.09	0.01	0.08	0.02	0.09	31,461	0.000
Enter the company (share)	0.22	0.29	0.21	0.28	0.22	0.28	31,461	0.000

Notes: This table reports mean and standard deviation of covariates, separately for individuals in treatment and control firms, before the implementation of the policy (2013-2017). Variables are averaged over the period 2013-2017, so that there is a single observation for each individual. Treatment status is defined based on firm size in 2015. The sample includes all firms with an employment level of +- 50 around the policy threshold, as defined in the baseline regression.

4 Results

This section presents the baseline results. Section 4.1 discusses the effects of pay gap disclosure on the leaving probability by gender. Section 4.2 reports the effects of the policy on the gender composition of new hires. Finally, Section 4.3 examines how the policy has affected promotions across and within firms by gender.

4.1 Baseline results: Impact of the policy on separation rates

This section presents the results from estimating the effect of the policy on the probability of leaving the firm at $t+1$. Precisely, Table 2 shows the results from estimating alternative specifications of equation (1). At the bottom of the table, we report the p-value of the t-test on the sum of the two reported coefficients β_1 and

Table 2. Impact of the policy on the likelihood of leaving the firm

	(1) Leaving firm at t+1	(2) Leaving firm at t+1	(3) Leaving firm at t+1	(4) Leaving firm at t+1
Treated firm × post	-0.00534*	-0.00622**	-0.00408	-0.00411
	(0.00276)	(0.00304)	(0.00309)	(0.00312)
Treated firm × post × female	0.0109***	0.0134***	0.0133**	0.0138***
	(0.00389)	(0.00515)	(0.00521)	(0.00523)
Treated firm × female	-0.00395	-0.00441	0.0807	0.0766
	(0.0753)	(0.0752)	(0.0631)	(0.0634)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Absorbed	Absorbed	Absorbed
Individual FE	Yes	Yes	Yes	Yes
Gender-Year FE	No	Yes	Absorbed	Absorbed
Gender-Year-Region FE	No	No	Yes	Yes
Industry-Year FE	No	No	No	Yes
Observations	210,450	210,450	210,450	210,450
Adj. R-squared	0.230	0.230	0.232	0.233
Diff. in women coeff	0.00560	0.00717	0.00924	0.00973
p-value women coeff	0.119	0.0844	0.0275	0.0216
Men's pre-policy mean	0.14	0.14	0.14	0.14
Women's pre-policy mean	0.16	0.16	0.16	0.16

Notes: This table reports different specifications of the model presented in equation (1). The dependent variable is a dummy equal to one if the individual leaves the firm at $t + 1$. The estimation sample includes individuals working in firms between 200 and 300 employees. A treated firm is defined as having at least 250 employees in 2015. Post is a dummy equal to one from 2017 onward. The p-value reported at the bottom of the table refers to the t-test of the two reported coefficients in the Table, corresponding to the impact of the policy on female employees. The pre-policy mean represents the mean of the dependent variable for the treated group between 2013 and 2017 separately for women and men. Standard errors double-clustered at the individual and firm levels are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

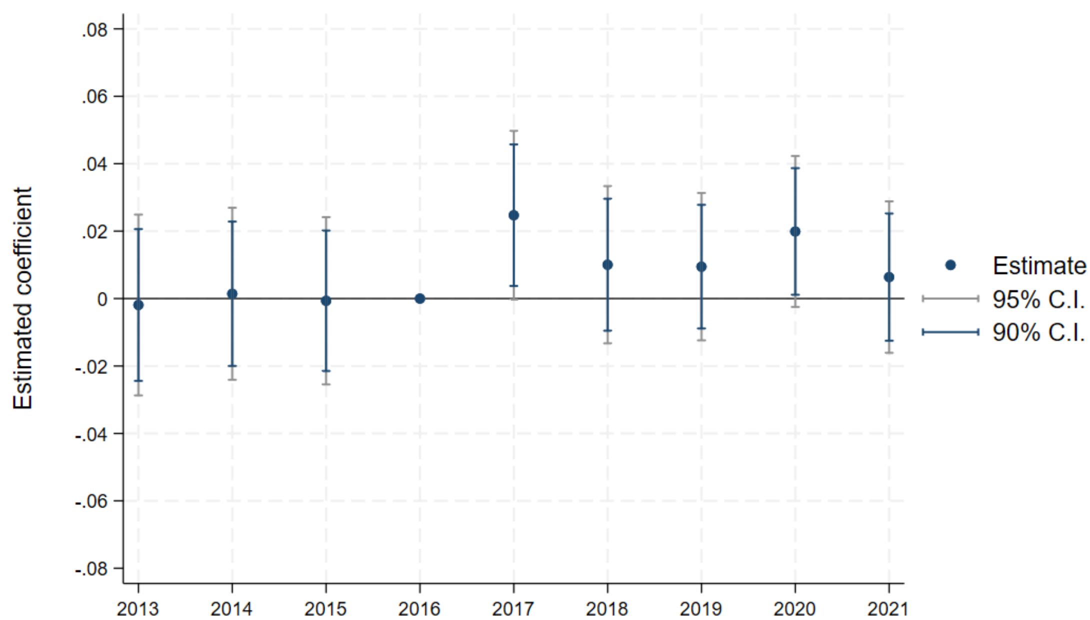
β_2, which represents the effect of the policy on female employees. The baseline specification in column (1) include individual, firm and year fixed effects; additional sets of fixed effects are added sequentially. Column (2) adds gender-by-year fixed effects, which capture time-varying gender-specific factors. We allow for gender-region-year fixed effects in column (3). Finally, we further control for time-varying industry-specific shocks in column (4).

Across all the specifications of Table 2, the differential effect of the policy on females (the coefficient of ”Treated firm × post × female”) is positive and statistically significant. The coefficient of the full specification reported in column (4) suggests that the policy leads to a nearly 1.4% increase in women's probability to leave relative to men's probability. Relative the pre-policy value for male of 0.14, the 0.014 coefficient estimated in column (4) corresponds to a 10% increase in women's relative probability of leaving. The coefficient on

”Treated firm x post” suggests that the policy had not effect on men’s probability to leave. On the contrary, the p-value reported at the bottom of Table 2 indicates that effect of the policy was driven by a higher probability of women of leaving treated firms after the policy implementation. Albeit the underlying sample is different and the underlying estimation strategy are not perfectly overlapping, this result is in line with Blundell et al. (2025), who find that the policy increases women’s probability to leave by 16% relative to men.[14] Finally, the coefficient of the interaction ”Treated firm × female” does not yield a statistically significant coefficient, telling us that there is no pre-policy trend in female separation rates in treated firms relative to control firms.

To verify the parallel trends assumption, we run the event-study analysis illustrated in equation 3. Figure 1 shows the event studies for the gender gap in leaving rates, with the 2016 as the benchmark year. To minimize differences across firms, the analysis is carried out for firms with +-20 employees around the 250-treshold. Figure 1 shows that the evolution of the outcome variable is comparable across treatment and control groups for both female and male employees. After 2016, there is an immediate increase in women’s leaving rates that persists until 2020.

Figure 1. Event study for the probability of leaving the firm



Notes: The graph reports the estimates of the coefficients β_k^1 of equation 3. These coefficients leads and lags of the differential effect of the policy on women’s probability of leaving relative to men’s probability. The sample is restricted to firms around a bandwidth of +-20 relative to the cutoff. The graph also reports confidence intervals at the 90% and 95% level.

[14]See column (2), Table 4, in Blundell et al. (2025). It is worth to highlight, however, that their result does not pass all the robustness checks they perform. Moreover, in their specification, they do not include worker fixed effects. Therefore, their effects also capture individual sorting.

We report a series of robustness checks on the effect of the policy on women's leaving rates. First, we show that our results vary little when we change the bandwidth around the 250-employee cutoff. Figure A3 reports the estimates of β_2 from equation 1 for different bandwidths ranging from 100 to 20 around the 250-employee cutoffs. Precisely, the estimated coefficient of "Treated x post x female" is only marginally insignificant when we consider bandwidths of +-100 (p-value 0.141) and +-70 (p-value 0.109), in which, however, treated and control firms might become less comparable.

Second, we run a set of placebo regressions where we use different cutoffs from the 250-employee level of the policy. A positive and statistically significant effect from this set of regressions would caution us against the presence of contemporaneous shocks that affect our dependent variable other than the pay transparency law. Specifically, we pretend that the cutoff was at 300, 200 or 150 employees, and include all the firms with +-50 employees around the cutoff, as in our main specification. However, as shown in figure A5, the estimates are not statistically significant for these different cutoffs.

Third, we check the robustness of our results to additional specifications. Figure A6 shows that the estimated effect of the policy on the female probability to leave is robust to restricting the sample to individuals who are older than 25 years, considering only workers with more than 8 years of experience (corresponding to the 25^{th} percentile of the distribution), excluding sectors for which our sample is less representative than the BPE (sectors G, I and Q), or the over-represented Information and Technology sector J. Finally, our results are robust to the inclusion of time-varying gender-industry specific shocks. However, our results are not robust to considering the year 2018 as the year of the reform, albeit the sign of the coefficient is still positive.

4.2 Impact of the policy on hiring rates

We now turn to the effects of the policy on hiring and the gender composition of new hires. Precisely, as discussed in section 3.2, we want to assess whether the policy has increased hiring of new employees or altered gender composition of new hires. Our dependent variable equals one if the individual is hired in the current year and zero otherwise. We show the estimates of the effect of the policy on new hires in Table 3. We replicate the same specifications of Table 2. Column (1) uses individual, firm and year fixed effects. Column (2) adds gender-year fixed effects. We also introduce a full set of gender-region-year fixed effects in column (3). Finally, column (4) includes industry-year fixed effects. The coefficient of Treated firm × post × female is negative in almost all specifications but statistically significant only for column (1). On the contrary, the coefficient "Treated firm × post", which yields the effect of the policy on male hiring, is positive and statistically significant across all columns. Column (4) indicates that men are more likely to be hired by about 0.9 percentage points in treated firms relative to control firms after the policy. As indicated by the p-value of the t-test on the sum of the reported coefficients, the effect on female only is not statistically significant at standard confidence levels. Therefore, we conclude that the pay transparency policy has changed the gender composition of new hires towards a greater share of men employees, but it did not have any effect on female employees. These findings seem to be consistent with a standard firm microeconomic reaction. As female labour becomes more costly relative to male labour, firms respond by reducing the demand for female labour, the costlier factor input, towards a greater share of male labour.

Table 3. Impact of the policy on the likelihood of entering the firm

	(1) New hire	(2) New hire	(3) New hire	(4) New hire
Treated firm × post	0.0171***	0.0101**	0.00808*	0.00891**
	(0.00392)	(0.00426)	(0.00432)	(0.00436)
Treated firm × post × female	-0.0247***	-0.00541	-0.00603	-0.00840
	(0.00553)	(0.00744)	(0.00754)	(0.00756)
Treated firm × female	0.0547	0.0484	0.0405	0.0408
	(0.0671)	(0.0672)	(0.0660)	(0.0660)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Absorbed	Absorbed	Absorbed
Individual FE	Yes	Yes	Yes	Yes
Gender-Year FE	No	Yes	Absorbed	Absorbed
Gender-Year-Region FE	No	No	Yes	Yes
Industry-Year FE	No	No	No	Yes
Observations	210,450	210,450	210,450	210,450
Adj. R-squared	0.150	0.151	0.152	0.154
diff women coeff	-0.00762	0.00466	0.00206	0.000508
p-value women coeff	0.136	0.445	0.739	0.935
Men's pre-policy mean	0.16	0.16	0.16	0.16
Women's pre-policy mean	0.19	0.19	0.19	0.19

Notes: The dependent variable is a dummy equal to one if the individual is a new hire. The estimation sample includes individuals working in firms between 200 and 300 employees. A treated firm is defined as having at least 250 employees in 2015. Post is a dummy equal to one from 2017 onward. The p-value reported at the bottom of the table refers to the t-test of the two reported coefficients in the Table, corresponding to the differential impact of the policy on female employees. The pre-policy mean represents the mean of the dependent variable for the treated group between 2013 and 2017. Standard errors double-clustered at the individual and firm levels are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

4.3 Impact of the policy on occupational upgrading

So far, we have shown that, following the policy implementation, women in treated firms are more likely to leave than those in control firms. We also documented a shift in the composition of new hires, as treated firms hired relatively more men than control firms after the policy.

We now examine the policy's effects on the probability of being promoted to a higher-paid occupation or to a managerial role. As discussed in Section 3.2, our dependent variables are: (i) a binary variable equal to one if an individual moves to an occupation whose average earnings exceed those of their previous occupation, and zero otherwise; and, (ii) a binary variable equal to one if an individual moves from a non-managerial to a managerial occupation, and zero otherwise. The results are reported in Table 4. Columns (1) and (3) use the full specification with individual, firm, year, and gender–region–year fixed effects, capturing the policy's impact on the probability of promotion both within and across firms. By contrast, Columns (2) and (4) include interacted individual–firm fixed effects, isolating the policy's impact on within-firm promotions. As shown in Table 4, we find no statistically significant effects for either men or women. None of the coefficients reaches statistical significance at standard confidence levels. We thus conclude that the policy has no distinguishable effects on promotions—either within firms or across firms—for men or women. These findings are consistent with Blundell et al. (2025) for the UK policy.

Table 4. Alternative outcome variables

	(1) Promotion	(2) Promotion within	(3) Promotion to manager	(4) Promotion to manager within
Treated firm $\times$ post	0.000365	0.000226	-0.00218	-0.00220
	(0.00724)	(0.00667)	(0.00178)	(0.00164)
Treated firm $\times$ post $\times$ female	-0.0120	-0.0118	0.00414	0.00417
	(0.00874)	(0.00803)	(0.00330)	(0.00303)
Firm FE	Yes	Yes	Yes	Yes
Individual FE	Yes	Yes	Yes	Yes
Individual-Firm FE	No	Yes	No	Yes
Gender-Year-Region FE	Yes	Yes	Yes	Yes
Industry-Year FE	Yes	Yes	Yes	Yes
Observations	120,454	120,393	120,454	120,393
Adj. R-squared	0.124	0.124	0.0727	0.0731
diff women coeff	-0.0116	-0.0116	0.00196	0.00197
p-value women coeff	0.178	0.145	0.517	0.479

Notes: The dependent variables are: 1) a dummy equal to one if the individual is promoted from a lower-paid to a better-paid occupation; 2) a dummy equal to one if the individual is promoted from a lower-paid to a better-paid occupation within the firm; 3) a dummy equal to one if the individual is promoted from a non-managerial to a managerial occupation; 4) a dummy equal to one if the individual is promoted from a non-managerial to a managerial occupation within the firm. The estimation sample includes individuals working in firms between 220 and 280 employees. A treated firm is defined as having at least 250 employees in 2015. Post is a dummy equal to one from 2017 onward. The p-value reported at the bottom of the table refers to the t-test of the two reported coefficients in the Table, corresponding to the differential impact of the policy on female employees. Standard errors double-clustered at the individual and firm levels are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

5 Mechanisms

5.1 Heterogeneous effects

This section shows selected heterogeneous effects of the policy. Precisely, we want to understand whether the policy had differential gender effects on selected categories of workers: managers vs non-managers, migrant vs non-migrant, high-skilled vs low-skilled and workers with 'complex' vs non-'complex'-skills. For example, to examine the differential effects by gender for managers relative to non-managers, we estimate the following quadruple-differences-in-differences model:

$$
\begin{aligned}
Y_{i,f,t} = {} & \beta_1 \left(Treated_f \times Post_t\right) \\
& + \beta_2 \left(Treated_f \times Post_t \times Female_i\right) \\
& + \beta_3 \left(Treated_f \times Post_t \times Manager_i\right) \\
& + \beta_4 \left(Treated_f \times Post_t \times Female_i \times Manager_i\right) \\
& + \alpha_i + \mu_f + \delta_{j,t} + \chi_{g,r,t} + \epsilon_{i,f,t}
\end{aligned} \tag{3}
$$

In this analysis, β_1 indicates the baseline effect of the policy on male non-managers; β_2 yields the differential effect of the policy on female non-managers relative to male non-managers, while the sum $\beta_1 + \beta_2$ indicates the effect of the policy on female non-managers; β_3 indicates the differential effect of the policy on managers vs non-managers within males; the sum $\beta_2 + \beta_4$ yields the differential effect of the policy on female managers relative to male managers, while the sum of all the coefficients $\beta_1 + \beta_2 + \beta_3 + \beta_4$ yields the effect of the policy on all female employees. Therefore, this specification allows us to examine whether the policy had an impact on the gender gap in job separation within managers and non-managers, as well as to understand whether these effects are driven by male or female employees. Additionally, one can also test whether the effect of the policy has a differential effect within female employees, namely between female non-managers and female managers. This last effect is given by the sum of the two coefficients β_3 and β_4. The same specification used in equation 3 is then used to assess the gender gap in job separation within migrant and non-migrant, and within high-skilled and low-skilled workers.

Table 5 shows the results of these different specifications. Column (1) shows the differential effects of the policy by gender on managers and non-managers. The coefficient of ”Treated firm x post x female” is positive and statistically significant, indicating that the the policy has a positive differential effect on non-manager females relative to non-manager males. The p-value of the t-test on the sum of ”Treated firm x post” and ”Treated firm x post x female”, reported at the bottom of the table (”Female baseline”), indicate that this effect is driven by non-manager female employees. The p-value of the t-test on the sum of the coefficients ”Treated firm × post × female” and ”Treated firm × post × female x manager” yields the differential effect of the policy on female managers relative to male manager. However, as indicated by the p-value of ”Female-Male non-baseline”, this difference is not statistically significant, even though the policy has an an effect on the probability of leaving by manager females (as shown by the ”Female non-baseline” p-value).

Column (2) shows the differential effects of the policy by gender on migrant and non-migrant workers. The coefficient of ”Treated firm x post x female” is positive and statistically significant, indicating that the the policy has a positive differential effect on native females relative to native males. The p-value of the t-test on the sum of ”Treated firm x post” and ”Treated firm x post x female”, reported at the bottom of the table (”Female baseline”), indicate that this effect is driven by native females. The p-value of the t-test on the sum of the coefficients ”Treated firm × post × female” and ”Treated firm × post × female x migrant” yields the differential effect of the policy on migrant females relative to migrant males. However, as indicated by the p-value of ”Female-Male non-baseline”, this difference is not statistically significant. Additionally, the policy does dot display statistically significant effects on migrant females(as shown by the ”Female non-baseline” p-value reported at the bottom of the table).

Column (3) shows the differential effects of the policy by gender on high-skilled and low-skilled workers. The coefficient of ”Treated firm x post x female” is positive but does reach statistical significance, suggesting that the the policy has not any distinguishable differential effects on low-skilled females relative to low-skilled males. The p-value of the t-test on the sum of ”Treated firm x post” and ”Treated firm x post x female”, reported at the bottom of the table (”Female baseline”), indicate that the policy has an effect on low-skilled female only that is weakly statistically significant (at the 90% level). The p-value of the t-test on the sum of the coefficients ”Treated firm × post × female” and ”Treated firm × post × female x High-skilled” yields the differential effect of the policy on high-skilled females relative to high-skilled males. This difference, as shown by the p-value of ”Female-Male non-baseline”, is statistically significant. Additionally, this effect is driven by an effect of the policy on high-skilled females (as shown by the ”Female non-baseline” p-value reported at the bottom of the table).

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6 Conclusion

Table 5. Heterogeneous effects on the probability of leaving the firm

	(1) Managers vs non-managers	(2) Migrants vs non-migrants	(3) High-skilled vs low-skilled	(4) More complex vs less-complex
Treated firm × post	-0.000391	0.00393	0.00368	-0.000137
	(0.00464)	(0.00655)	(0.00801)	(0.00479)
Treated firm × post × female	0.0205***	0.0211**	0.0133	0.0191**
	(0.00742)	(0.0107)	(0.0126)	(0.00786)
Treated firm × post × manager	0.0119			
	(0.00892)			
Treated firm × post × female × manager	-0.00644			
	(0.0156)			
Treated firm × post × migrant		0.00218		
		(0.0118)		
Treated firm × post × female × migrant		-0.00481		
		(0.0199)		
Treated firm × post × High-skilled			0.00268	
			(0.00804)	
Treated firm × post × female × High-skilled			0.0123	
			(0.0132)	
Treated firm × post × More complex				0.00393
				(0.00698)
Treated firm × post × female × More complex				0.000296
				(0.0110)
Observations	118602	76284	88815	108444
Adj. R-squared	0.667	0.636	0.641	0.228
Baseline	Non-manager	Non-migrant	Low-skilled	Low-complex
Female baseline	0.00104	0.00521	0.0907	0.00396
Female vs. Male non-baseline	0.370	0.406	0.0131	0.0733
Female non-baseline	0.0471	0.158	0.000215	0.00577
Female baseline vs non-baseline	0.671	0.870	0.161	0.638

Notes: This table reports different specifications of the model presented in equation 3. The dependent variable is a dummy equal to one if the individual leaves the firm at $t+1$. The estimation sample includes individuals working in firms between 220 and 280 employees. A treated firm is defined as having at least 250 employees in 2015. Post is a dummy equal to one from 2017 onward. The p-value reported at the bottom of the table refers to the t-test of the two reported coefficients in the Table, corresponding to the differential impact of the policy on female employees. Standard errors double-clustered at the individual and firm levels are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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Appendices

A Data description

This section describes the data used in this paper, along with sources and variables.

A.1 Diffbot

The empirical analysis of this work leverages a novel employer-employee database for UK companies. This section provides a more detailed description of these data. We invite the reader to refer to Gray et al. (2025) for full details.

Individual-level data are sourced from Diffbot, the world's leading commercial knowledge graph database of the public web. At the start of 2025, the graph included 3.6m active UK companies with CRN and 10.4m workers. This compares to totals of 5.5m active firms and 33.9m workers aged 16+ from the 2025 Business Population Estimates.

Diffbot constructs a knowledge graph by scraping the web continually, using a combination of tools ranging from image recognition, natural language processing and supervised learning. Then, it organizes such data into a graph of entities, including individuals, organizations, places, which are linked to each other. This database has several desirable properties that we exploit to build an employer-employee database. First, the graph includes data on UK companies, including the Company Registration Number (CRN). This information allows us to link companies in Diffbot to other company-level data like Orbis. Second, Diffbot includes extensive information on individuals, including gender, age, detailed education and employment histories, skills and job titles, among others. All these information allows us to assemble an employer-employee database for a set of UK companies.

We construct the employer-employee database in three steps. First, we assembled a dataset with fixed individual characteristics, including the year of birth, languages and gender. Second, we built a panel dataset with education histories, including information on highest qualification, field and school/university. We use the country of the earliest available educational qualification achieved to proxy for the migrant status of an individual. For example, if someone's lowest level of qualification reported on the web is high-school, and this qualification has been obtained in the UK, we assume that the individual is British. Additionally, we also use information on education histories to construct an individual's highest level of education attained based on a set of keywords. Further details on these procedures and validation checks are reported in Gray et al. (2025).

Third, we compile full employment histories, including the start and end dates of each employment spell, employers, job titles, and brief job descriptions. Finally, we merge these three components (individual fixed characteristics, education and employment histories) into an employer-employee panel dataset, converting education variables into time-invariant characteristics while preserving the time dimension of employment histories. Diffbot assigns to each entity a unique identifier, allowing us to study job flows across organizations.

Diffbot also provides brief job-title description but does not link them to standard occupational classifications. Therefore, we use LinkTransformer (Arora and Dell, 2023), an open-source LLM-based text classifier, to map the universe of Diffbot job-title descriptions in our sample to the UK 2020 Standard Occupational Classification (SOC2020). We experiment with multiple levels of SOC granularity and obtain the best performance at the 4-digit level.

A.2 Company-level data

As explained in Gray et al. (2025), the employer-employee database is build on a tractable sample of firms. Relying on extensive checks, we find that web information on entities is available especially for large organizations. Additionally, as it is well-known, financial and employment information in databases such as Orbis is more complete and higher quality for medium and large firms (Kalemli-Özcan et al., 2024). Therefore, we first preselect a sample of 'medium' and 'large' firms active at some point between 2007 and 2023. Companies House distinguishes 'micro' and 'small' companies entities from medium and large companies based on turnover, assets and employee thresholds. We apply these post-2016 thresholds backwards to ensure a time-consistent sampling frame. Companies above this threshold need to provide complete, audited annual accounts. This means that company-level information is most complete and highest quality for this sample. Applying these definitions to Orbis Historical, using data from unconsolidated balance sheets only, we obtain a sample of 55,775 companies. We then match this sample of companies to Companies House (through OpenCorporates), which leaves us with a sample of 55,187 companies.

We follow the cleaning procedures for Orbis data documented in Kalemli-Özcan et al. (2024) and De Loecker et al. (2024). We keep only firm-year observations for which financial variables are expressed in GBP pounds. We use the account closing date to determine the calendar year. If the closing date is after or on June 1st, we assign it to the current year. If it is before June 1st, we assign it to the previous year. At this stage, Orbis may contain multiple annual observations for some firms. We design a routine of sequential steps to remove firm-year observations duplicates, similar to De Loecker et al. (2024):

- We keep the annual report values when both the annual report and local registry filing are present, and the annual report values are non-missing.
- When annual report and local registry filing values are not the same (and both are non-missing), we

prefer annual report values.

- When annual report values are missing, and local registry filings are non-missing, we keep local registry filings.
- After the selection above, we prefer consolidated accounts to unconsolidated accounts.
- We remove remaining duplicates by taking the observations with fewer missing values for the number of employees, EBITDA and costs of employees.

A.3 Tables

Table A1. SIC07 distribution in 2015

1-digit industry code	% Our sample	% BPE
C(10-33)	17.1	10
D(35) - E(36-39)	1	1
F(41-43)	8.2	8
G(45-47)	9.9	19
H(49-53)	2.5	5
I(55-56)	1.4	8
J(58-63)	15.2	5
K(64-66)	8.0	4
L(68)	1.3	2
M(69-75)	14.4	10
N(77-82)	9.8	11
P(85)	5.2	2
Q(86-88)	2.1	7
R(90-93)	2.0	3
S(94-96)	1.7	3

Notes: This table reports the 2015 distribution of the individuals in our sample based on their employer's industry in column 2 and the corresponding employee distribution across sectors from the Business Population Estimates in 2015.

Table A2. Probability to be treated on observable characteristics

	(1)
College or higher degree	-0.00493
	(0.0348)
Migrant	0.0317
	(0.0401)
Age	0.00212*
	(0.00122)
Female	0.00396
	(0.0319)
Worker experience	0.00101
	(0.00145)
Leaving company	0.0251
	(0.0413)
New hire	-0.0925**
	(0.0409)
Promotion	0.0226
	(0.0348)
Managerial promotion	-0.117*
	(0.0696)
Observations	9,011
Adj. R-squared	0.00612
F-test p-value	0.114

Notes: This table reports a linear probability model of the probability to be in a treated firm against a set of observable characteristics. Variables are averaged over the pre-policy period 2013-2017. Standard errors double-clustered at the individual and firm levels are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A3. Heterogeneous effects on the probability of entering the firm

	(1) Managers vs non-managers	(2) Migrants vs non-migrants	(3) High-skilled vs low-skilled	(4) More complex vs less-complex
Treated firm × post	0.00766	0.0203**	0.0175	0.0241***
	(0.00645)	(0.00911)	(0.0111)	(0.00646)
Treated firm × post × female	-0.0167	-0.0234	-0.0226	-0.0162
	(0.0107)	(0.0154)	(0.0184)	(0.0111)
Treated firm × post × manager	0.0291***			
	(0.0104)			
Treated firm × post × female × manager	-0.0420**			
	(0.0203)			
Treated firm × post × migrant		-0.0186		
		(0.0161)		
Treated firm × post × female × migrant		0.0226		
		(0.0271)		
Treated firm × post × High-skilled			-0.00669	
			(0.0111)	
Treated firm × post × female × High-skilled			-0.00121	
			(0.0187)	
Treated firm × post × More complex				-0.0491***
				(0.0101)
Treated firm × post × female × More complex				-0.00356
				(0.0158)
Observations	106805	68643	79948	108444
Adj. R-squared	0.150	0.149	0.147	0.151
Baseline	Non-manager	Non-migrant	Low-skilled	Low-complex
Female baseline	0.312	0.806	0.733	0.411
Female vs. Male non-baseline	0.00447	0.975	0.101	0.184
Female non-baseline	0.216	0.972	0.287	0.000244
Female baseline vs non-baseline	0.456	0.856	0.603	0.0000232

Notes: This table reports different specifications of the model presented in equation 3. The dependent variable is a dummy equal to one if the individual enters the firm at the current year. The estimation sample includes individuals working in firms between 220 and 280 employees. A treated firm is defined as having at least 250 employees in 2015. Post is a dummy equal to one from 2017 onward. The p-value reported at the bottom of the table refers to the t-test of the two reported coefficients in the Table, corresponding to the differential impact of the policy on female employees. Standard errors double-clustered at the individual and firm levels are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

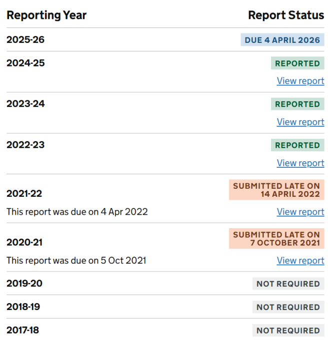
Table A4. Impact of the policy on the probability to leave the firm - Good vs bad firms

	(1) Leaving firm at t+1	(2) Leaving firm at t+1	(3) Leaving firm at t+1	(4) Leaving firm at t+1
Treated firm $\times$ post	0.00167	0.00170	0.00254	0.00424*
	(0.00197)	(0.00207)	(0.00222)	(0.00232)
Treated firm $\times$ post $\times$ female	-0.000984	-0.00113	0.0000870	0.00134
	(0.00296)	(0.00340)	(0.00355)	(0.00357)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	-	-	-
Individual FE	Yes	Yes	Yes	Yes
Gender-Year FE	No	Yes	-	-
Gender-Year-Region FE	No	No	Yes	Yes
Industry-Year FE	No	No	No	Yes
Observations	108,444	108,444	108,444	108,444
Adj. R-squared	0.226	0.226	0.228	0.228
Diff. in women coeff	0.000689	0.000568	0.00263	0.00559
p-value women coeff	0.782	0.833	0.343	0.0532
Men's pre-policy mean	0.12	0.12	0.12	0.12
Women's pre-policy mean	0.13	0.13	0.13	0.13

Notes: This table reports different specifications of the model presented in equation 1. The dependent variable is a dummy equal to one if the individual leaves the firm at $t + 1$. The estimation sample includes individuals working in firms between 220 and 280 employees. The variable assumes values: 1) equal to zero if the firm is not treated; 2) 1 if the firm has a gender pay gap below the median; 3) 2 if the firm has a gender pay gap above the median. Post is a dummy equal to one from 2017 onward. The p-value reported at the bottom of the table refers to the t-test of the two reported coefficients in the Table, corresponding to the impact of the policy on female employees. The pre-policy mean represents the mean of the dependent variable for the treated group between 2013 and 2017 separately for women and men. Standard errors double-clustered at the individual and firm levels are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.4 Figures

Figure A1. Gender policy reports uploaded to the Government website



Reporting Year	Report Status
2025-26	DUE 4 APRIL 2026
2024-25	REPORTED View report
2023-24	REPORTED View report
2022-23	REPORTED View report
2021-22 This report was due on 4 Apr 2022	SUBMITTED LATE ON 14 APRIL 2022 View report
2020-21 This report was due on 5 Oct 2021	SUBMITTED LATE ON 7 OCTOBER 2021 View report
2019-20	NOT REQUIRED
2018-19	NOT REQUIRED
2017-18	NOT REQUIRED

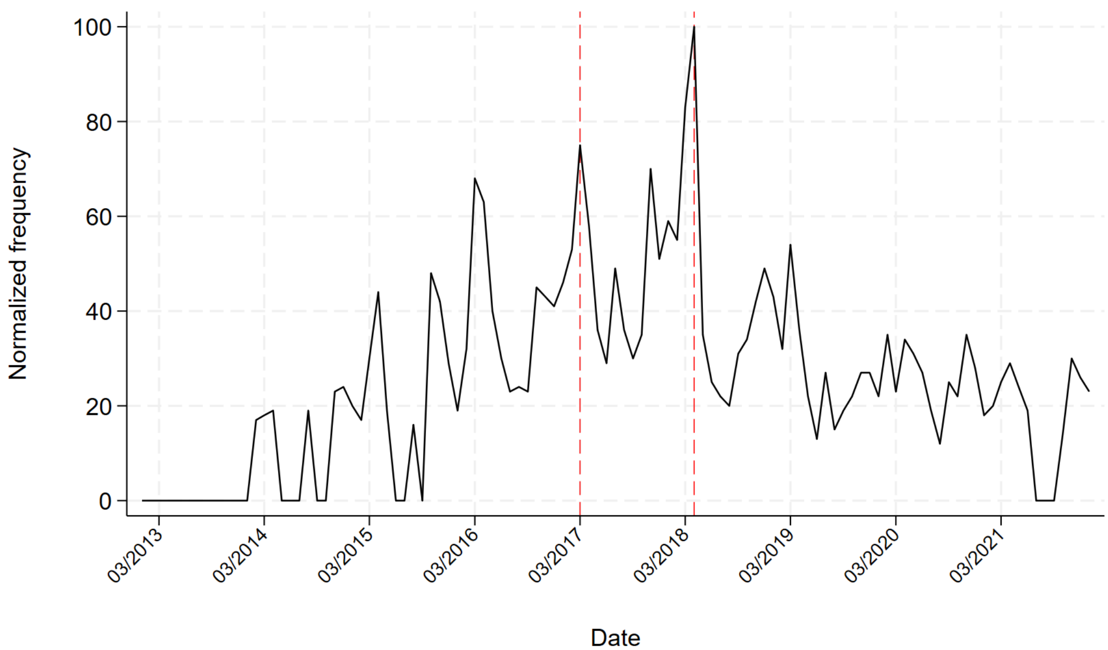
(a) Firm uploading its gender report with delays

Reporting Year	Report Status
2025-26	DUE 4 APRIL 2026
2024-25	REPORTED View report
2023-24	REPORTED View report
2022-23	REPORTED View report
2021-22	REPORTED View report
2020-21	REPORTED View report
2019-20	REPORTED View report
2018-19	REPORTED View report
2017-18	REPORTED View report

(b) Firm uploading its gender report on time

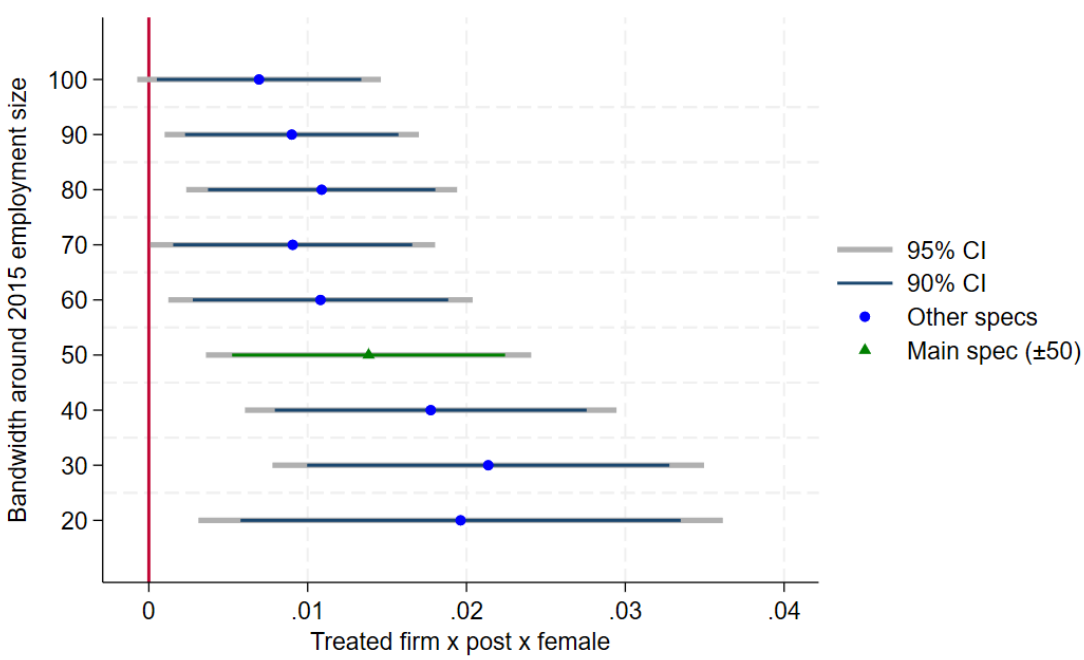
Notes: The figure shows how gender policy reports are reported on the Government website.

Figure A2. Google searches for 'gender wage gap'



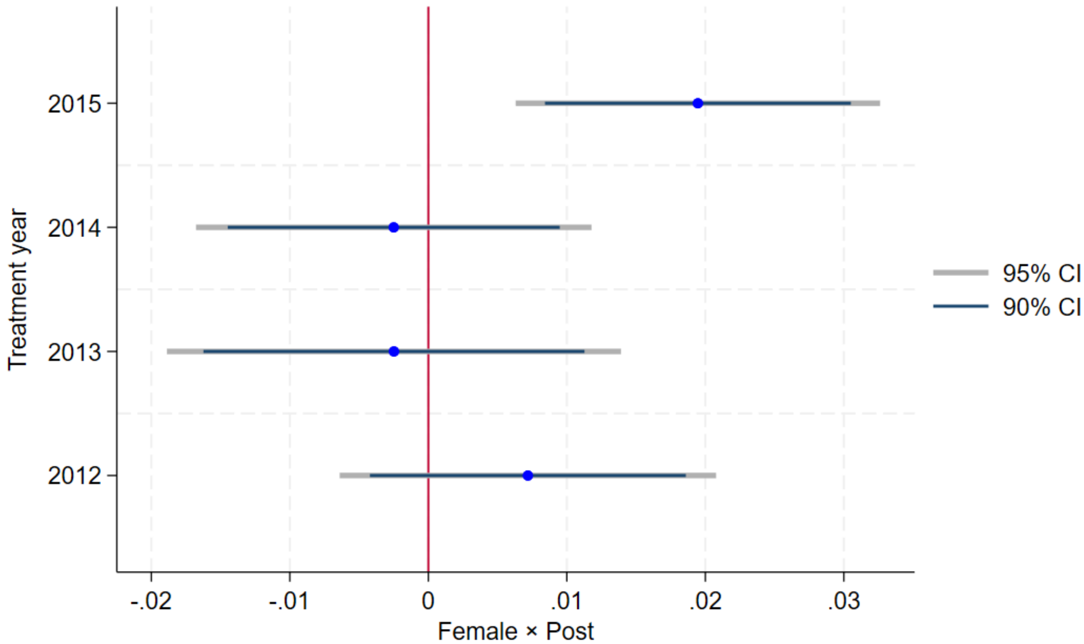
Notes: The figure shows the relative Google searches for "gender wage gap" in the UK between March 2013 and March 2022. Numbers are expressed as relative to the peak in April 2018.

Figure A3. Differential effect of the policy on females relative to males for alternative bandwidths



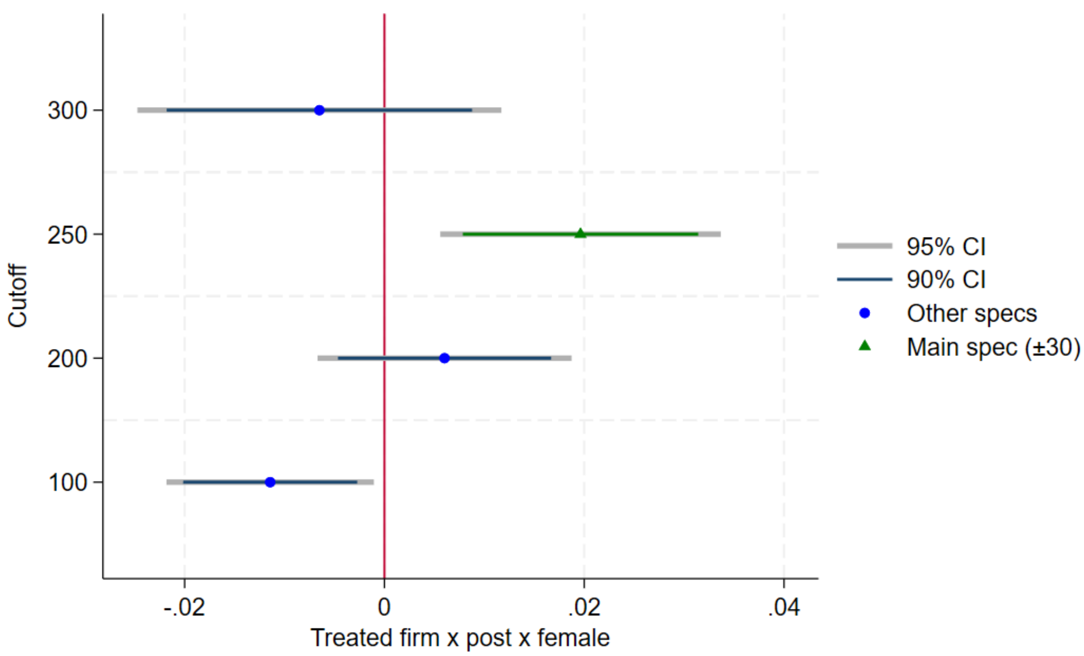
Notes: The figure shows the estimated coefficient of Treated firm × post × female for alternative bandwidths.

Figure A4. Robustness to alternative treatment years



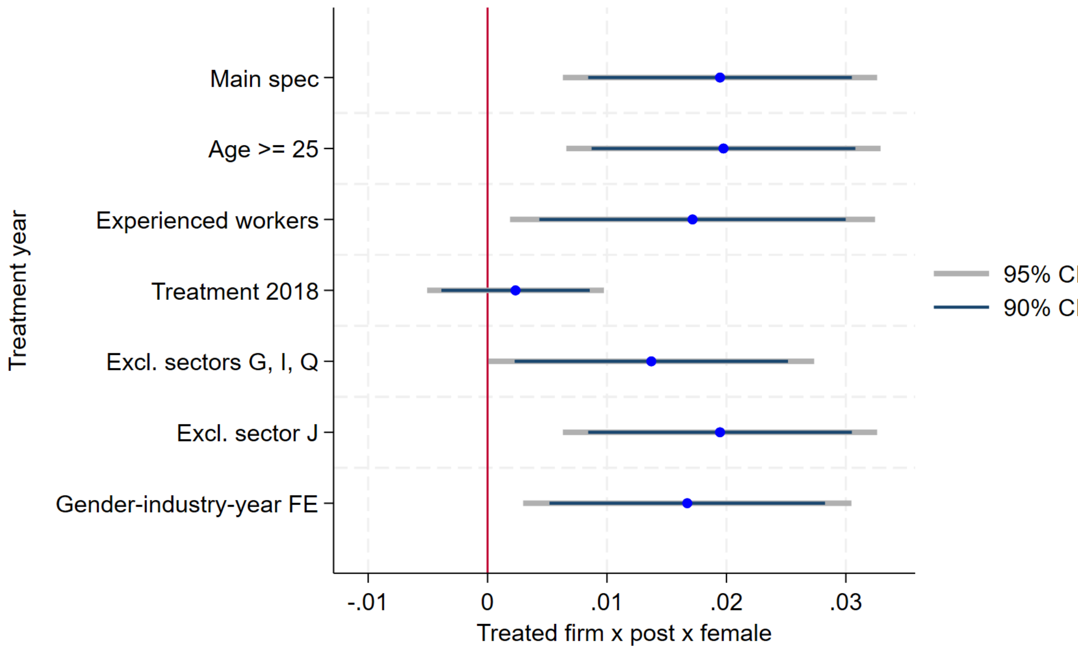
Notes: The figure shows the estimated coefficient of Treated firm × post × female for alternative treatment years.

Figure A5. Placebo regressions



Notes: The figure shows the estimated coefficient of Treated firm × post × female for alternative placebo cutoffs.

Figure A6. Additional robustness checks



Notes: The figure shows the estimated coefficient of Treated firm × post × female for alternative treatment years.