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Unequal hiring wages and their impact on the gender pay gap

Tho Pham[1] | **Daniel Schaefer**[2] | **Carl Singleton**[3]

[1]Department of Economics and Related Studies, University of York, York, UK

[2]Department of Economics, Johannes Kepler University Linz, Linz, Austria

[3]Economics Division, Stirling Business School, University of Stirling, Stirling, UK

Correspondence
Carl Singleton, Economics Division, Stirling Business School, University of Stirling, Stirling, UK.
Email: carl.singleton@stir.ac.uk

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Abstract

Payroll data from Great Britain reveal that men are paid more than women upon entry into firms. Although this hiring wage gap has narrowed over the past two decades, it still accounts for over two-thirds of the steady-state gender pay gap—the wage gap that would eventually prevail under constant employment levels. A significant part of this hiring wage gap is not explained by men and women working in different firms and occupations. Even when hiring men and women into the same job, there remains an unexplained hiring wage gap that favors men by 2.4 log points.

KEYWORDS
employer-employee data, gender segregation, occupation-specific wages

JEL CLASSIFICATION
J16, J31, J70

1 | INTRODUCTION

Across advanced economies, the headline gender pay gap has narrowed substantially over the last century. Yet, progress has stalled in recent decades, leaving sizable gaps (e.g., Blau & Kahn, 2017; Olivetti & Petrongolo, 2016). Strikingly, today's disparities already emerge at the very first interaction between a worker and a firm: women still enter new jobs at systematically lower wages than men hired for the same roles (e.g., Roussille, 2024; Säve-Söderbergh, 2019). Since modern labor markets are highly mobile, any hiring-wage shortfall may compound over successive moves and account for a large share of "lifetime" pay inequality (Kunze, 2018). In this paper, we use matched employer-employee data from Great Britain to examine how gender differences in hiring wages shape both the level and persistence of the gender pay gap. Further, after addressing the segregation of workers by gender across different firms and occupations, we analyze the ongoing extent of unequal hiring wages between men and women—pay differences between comparable workers when they are hired into the same firms and occupations, at approximately the same time.[1]

Abbreviations: AKM, Abowd, Kramarz and Margolis; ASHE, Annual Survey of Hours and Earnings; FEs, fixed effects; LFS, Labour Force Survey; NESPD, New Earnings Survey Panel Dataset; NUTS1, Nomenclature of Territorial Units for Statistics level 1; ONS, Office for National Statistics; SOC, Standard Occupational Classification; TTWA, travel-to-work areas.

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Most existing studies on the gender wage gap use Mincer-style earnings regressions to estimate how gender differences in observable characteristics, such as education and experience, contribute to pay disparities (Kunze, 2018). This literature often emphasizes human capital mechanisms, either due to women's career interruptions (e.g., Bertrand et al., 2010; Goldin, 2014; Light & Ureta, 1995; Mincer & Polacheck, 1974) or due to limited human capital accumulation while employed (e.g., Barron et al., 1993; Blundell et al., 2021; Kunze, 2005; Manning & Swaffield, 2008).[2] However, even after adjusting for these factors, a sizable and persistent "unexplained" gender wage gap remains (see the reviews by Blau & Kahn, 2017; Kunze, 2018). Yet, much of this literature abstracts from wage-setting at the point of hire, even though every employee's wage path begins with an initial hiring wage.

We complement this prior work with a decomposition that shows how job mobility and hiring wages contribute to overall gender pay inequality. Building on the insight that search frictions shape wage offers at the point of hiring, we partition employees in each year into two groups: (i) *stayers* who remained with the same employer and (ii) *new hires*, either from non-employment or via employer-to-employer switches.[3] The overall mean wage is the employment-weighted average of the hourly pay across these groups. For stayers, current pay equals last year's wage plus on-the-job growth; for movers, we measure gender differences in hiring wages, which, in a steady-state framework, can compound across successive job changes. Our framework extends Manning and Robinson (2004), who focused only on entrants from non-employment, thus allowing us to decompose the national wage gap into gender differences in within-employer wage growth, retention, and hiring wages at the start of each new match.

Using employer-employee data from Great Britain (2005–2020), we uncover three key patterns. First, the raw hiring wage gap declined from 14 log points to nine log points, largely reflecting reduced gender segregation across firms and occupations over time. This mirrors the evolution of the overall gender wage gap, which fell from 20 to 15 log points over the same period. Second, gender differences in retention and within-employer wage growth rates are minor. Third, and most importantly, the hiring wage disparities are sizable and account for around three-quarters of the steady-state gender gap in hourly earnings—the wage gap that would eventually prevail under constant employment levels. The mechanism is intuitive: since every career begins with a hire, and on-the-job wage growth differs little by gender, initial differences in hiring wages persist and compound across successive labor market moves. This, combined with the fact that women are more likely to switch employers (often after career breaks), helps explain why the gap widens over the lifecycle. These representative employer payroll-based findings confirm and update the similar household-survey-based results of Manning and Robinson (2004) from the 1990s in Great Britain, with richer administrative data. Additionally, these findings also speak to the literature on gender differences in the returns to job mobility: whereas Keith and McWilliams (1997, 1999) did not find evidence of such differentials, Barth et al. (2021), Del Bono and Vuri (2011), and Loprest (1992) documented sizable gender differences in the returns to job mobility.

Having established that hiring wages account for the majority of the steady-state gender pay gap, we ask whether this gap arises from where women and men are hired or from what they are paid for doing the same job. We examine each of these in turn. Our analysis of the former builds on an extensive literature documenting the importance of gender segregation for the overall gender wage gap. For example, several studies have shown that women are more likely to work in low-paying occupations (e.g., Groshen, 1991; Macpherson & Hirsch, 1995) or low-paying employers (e.g., Sorkin, 2017, for the United States, Heinze & Wolf, 2010, for Germany, and Jones & Kaya, 2023; Mumford & Smith, 2009, for Great Britain). More recently, Blau and Kahn (2017) found that the share of the United States gender wage gap accounted for by the gender segregation of occupations and industries increased to almost 50% in 2010. In addition, some studies provide evidence of the importance of gender segregation across employers and occupations (e.g., Card et al., 2016; Cardoso et al., 2016 for Portugal and Jewell et al., 2020 for Great Britain). Evidence on the contribution of within-employer, within-occupation segregation to the wage gap is scarce. The first evidence was provided by Bayard et al. (2003), who found that approximately half of the gender wage gap in the United States is explained by gender segregation across occupations within employers. Recently, Penner et al. (2023) confirmed these findings for a sample of 15 developed economies. We extend this previous research with the first evidence at the hiring margin. After accounting for within-employer and within-occupation gender segregation—which explains much of the observed decline in the aggregate hiring wage gap over time—more than 20% of the hiring wage gap in Great Britain remains persistently unexplained.

Next, we provide the first comprehensive analysis of differences in hourly hiring wages between co-hires: men and women hired into the same occupation, by the same employer, in the same year (hereafter referred to as *job*). These tightly matched cells allow us to estimate gender differences in starting wages while holding the firm, role, and timing constant. After controlling for working hours, contract type, commuting patterns, and proxies of labor market experience, there remains a statistically and economically significant residual gap of 2.4 log points in hiring wages within jobs, primarily reflecting wage-setting in larger employers where both male and female hires are observed in the same job cells. Notably, this unexplained gap is most pronounced in the highest quartile of the hiring wage distribution, where men are paid 4.5 log points more than women for the same job. Even after controlling for unobserved, time-invariant worker productivity using the method of Abowd et al. (1999), a statistically significant gender hiring wage gap remains within jobs.

Studies have shown that men's and women's average propensities to negotiate differ, with women being less likely to negotiate (Babcock & Lachever, 2003; Bertrand, 2011). Recently, Biasi and Sarsons (2022) found that a reform increasing pay flexibility among public school teachers in Wisconsin reduced women's salaries relative to men's, partly because women engaged in pay negotiations less frequently. Notably, women tend to enter negotiations that result in gains while avoiding negotiation opportunities that would result in losses (Exley et al., 2020). Consistent with these studies, we find that employees who earned more in their previous jobs tend to receive higher wages than their fellow new hires in their next roles.[4] Intuitively, the previous wage affects the threat point during the wage bargain between a potential new hire and the employer. However, the strength of this effect is significantly weaker for women, creating a path dependency in the hiring wage gap. These patterns are consistent with gender differences in bargaining contributing to the persistent within-job hiring wage gap. While we do not directly analyze the underlying reasons, our study provides novel evidence about hiring wage differentials that are embedded even in narrowly defined hiring interactions. Taken together with our segregation findings, this underscores that unequal pay arises not only from where women and men work, but also from differences that emerge within jobs at the point of hire and that contribute to persistent disparities.

Our analysis draws on the Annual Survey of Hours and Earnings (ASHE), a British payroll-based dataset with several unique features suited to this study. Crucially, ASHE records the year, employer, occupation (at the 4-digit level), and job start date for each new hire. This granularity allows us to match men and women who are hired into the same firm-occupation and year, thus abstracting from employer-specific wage premiums and gender sorting across roles and firms (Jewell et al., 2020). Second, employers are legally obliged to report employee earnings according to their payrolls, making the dataset more accurate than those obtained from household surveys (Nickell & Quintini, 2003). This enables us to construct hourly wage rates without relying on self-reported hours, addressing a common concern that the observed gender wage gap in some datasets might be partially explained by differences in how men and women report their hours relative to what is contracted or worked. ASHE also contains approximately 200,000 employee observations per year, of which approximately 21,000 are new hires. This provides a sufficient sample size to track hiring patterns across employers, occupations, and wage percentiles. Finally, earnings in ASHE are not top-coded, allowing us to capture the full wage distribution, including the upper tail, where gender disparities are often most pronounced.[5]

The rest of the paper is structured as follows: Section 2 describes the ASHE, our estimation sample selection, and discusses the representativeness of our sample of new hires. Section 3 introduces our decomposition method for the overall gender wage gap; Section 4 discusses the relative contribution of hiring wages to the overall gender wage gap; Section 5 presents estimates on the role of gender segregation across employers, occupations, and jobs in hiring wages; Section 6 presents estimates of the hiring wage within jobs, its determinants, and discussed robustness checks and possible explanations for our results; and Section 7 concludes.

2 | DESCRIPTION OF THE DATA

2.1 | Dataset overview and sampling

The ASHE (Office for National Statistics, 2024) is a longitudinal, matched employer-employee dataset that began in 2005 and is administered by the UK Office for National Statistics (ONS). The data are based on a one per cent random sample of British employees who pay income tax or make National Insurance contributions. People who are eligible to appear in the ASHE sample remain so for their entire working lives. Eligibility is determined by the last two digits of a person's National Insurance number. As such, approximately one per cent of new entrants to the labor market each year, either 16-year-olds or immigrants, enter the ASHE eligible sample, replacing those who leave due to death, retirement, or emigration. Employers are legally obliged to respond to the survey questionnaire sent to them if they are employing a member of the population eligible to appear in ASHE. Nevertheless, the ASHE suffers from employer-level non-response, which may not be random and can introduce bias. As such, the ONS provides official sample weights derived from other sources, which are intended to make the ASHE representative of the employee population with

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respect to age, gender, occupation, and region. We therefore use the ONS sample weights in our descriptive statistics and subsequent segregation analysis.

2.2 | Employer linkage

The ASHE provides information concerning the pay period that includes a specific survey reference date in April, for individuals employed on the survey reference date. The longitudinal aspect of the ASHE allows us to track these employees over time and link them to their respective employers using the provided employer identifiers.[6] Hereafter, we frequently use the more common term "firm" instead of "employer" or "enterprise," which refers to the UK-specific administrative definition of an employer that may encompass several local units or plants.

2.3 | New hires

We define a *new hire* as an employee who joined their employer, either from non-employment or a different firm, less than a year before the April reference period. The employer reports the date on which an employee started working for the organization, meaning that individuals who return to the same firm after a break, for example, maternity leave, are not classified as new hires. We study employees aged 16–64 who did not incur any loss of pay in the April reference period (e.g., unpaid sick leave) and were not paid at an apprenticeship or a trainee rate. We exclude a small number of person-year observations in which a worker held multiple jobs, was reported as having worked on average less than one or more than 100 h per week in April, was reported as being paid <80% of the age-relevant statutory National Minimum Wage, or had missing or imputed values for any of the pay variables that we are interested in.

2.4 | Wage measures

Our preferred wage measure is gross earnings per hour, excluding overtime, hereafter referred to as the *hourly wage* or simply *wage*. The ONS uses the ASHE data and gross earnings per hour, excluding overtime, to derive the official national statistics on the gender pay gap.[7] Earnings include all basic pay, premium payments for shift, night, and weekend work, incentive pay for work carried out in the pay period, and any pay received through payroll for other reasons, for example, meal allowances. We also analyze basic earnings per basic hour worked, hereafter the *basic wage*, to assess the impact of the extra pay components on gender differences in pay. We convert all nominal wage variables in the ASHE to 2002 pound sterling prices using the UK Consumer Price Index in April, also published by the ONS.

2.5 | Occupational classification

Employers provide information to ASHE that indicates an employee's occupation up to the detailed 4-digit level according to the UK Standard Occupational Classification (SOC) 2000 between 2005 and 2010 and its slightly updated version, SOC 2010, from 2011 onward.[8] Our analysis below uses occupational categories at the 3-digit (minor groups) and 4-digit (unit groups) levels. SOC 2000 uses 81 and 353 categories at the 3-digit and 4-digit levels, respectively. The updated version, SOC 2010, uses 90 and 369 categories at the 3-digit and 4-digit levels, respectively. According to SOC 2000, examples of 3-digit occupations and their associated 4-digit occupations are SOC 322 "Therapists"; SOC 3221 "Physiotherapists"; SOC 3222 "Occupational therapists"; SOC 3223 "Speech and language therapists"; and SOC 3229 "Therapists not elsewhere classified."

2.6 | Shortcomings and solutions

A shortcoming of the ASHE is that it does not report measures of actual labor market experience, which is important for explaining gender wage gaps (e.g., Light & Ureta, 1995). As a proxy for labor market experience, we observe the number of years a worker has been in employment each April, from 1975 onward, by linking individuals in ASHE to its

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predecessor, the New Earnings Survey Panel Dataset (NESPD) (Office for National Statistics, 2017).[9] This variable will be a good proxy for actual labor market experience, provided that the relationship between employment disruptions and the calendar month of April does not vary significantly by gender. The ASHE also lacks information on other important determinants of the gender wage gap, such as education and family responsibilities (e.g., Kleven, Landais, Posch, et al., 2019). We partially address this shortcoming by comparing the wages of new hires within the same firms and detailed occupational categories, which may reasonably proxy for educational attainment and skill levels. For instance, using household survey data, Vecchie et al. (2023) found that almost 70% of UK graduates' education matches their one-digit occupation, which does not vary notably by gender. Additionally, as a robustness check, we use the employer-employee panel aspect of ASHE and a standard Abowd, Kramarz and Margolis (AKM) model (Abowd et al., 1999) to estimate worker effects, accounting for observable and unobservable fixed worker characteristics, such as time-invariant components of human capital and productivity.

2.7 | Descriptive statistics and representativeness of the new hires sample

Table 1, columns (III) and (IV), display descriptive statistics for all the new hires in ASHE from 2005 to 2020, broken down by gender. On average, men are hired with higher wages and more years of labor market experience. Women are notably more likely to be hired by the public sector and to have their pay affected by a collective agreement than men. We also observe that women work, on average, fewer hours than men within the first year of being hired, resulting in a lower full-time (≤30 h) new hire employment share than among men. This highlights one advantage of our data: we can control for hours worked by deriving hourly wage measures.

While the ASHE is representative of the stock of British employees at a given time, it is less clear whether it is representative of the flow of new hires. To assess this issue, we compare the new hires in the ASHE to the new hires in the quarterly UK Labour Force Survey (LFS) (Office for National Statistics, 2025) for the same period, whereby new hires in the LFS also include new matches formed and quickly dissolved between the ASHE survey reference date (when employers are looked up) and the pay reference period in each April—these matches are not observable in the ASHE. The first two columns of Table 1 show that the new hires in the LFS broadly resemble those in the ASHE regarding their average weekly hours worked, excluding overtime, and age. However, new hires in the ASHE are less likely to work in the public sector than in the LFS, though the former is determined administratively, whereas the latter is self-reported by workers.[10] To derive a wage measure that excludes overtime in the LFS, we take the gross weekly pay variable from the LFS and assume that the overtime pay rate is 1.5 times an employee's regular hourly wage.[11] Average hiring wages tend to be lower in the LFS than in the ASHE. However, the latter contains payments reported by employers, such as allowances, incentive pay, and shift premiums, that people may be less likely to report in a household survey. Notably, the average basic hourly wage rate recorded in ASHE is much closer to the average hourly pay that workers report in the LFS. Importantly for the interpretation of our results later, the average hiring hourly wage gaps in the LFS and the ASHE are comparable, 16.4% versus 15.4%, respectively.[12]

In addition, Panel B displays the industry distribution, showing that new hires in the ASHE are less likely to work in the production sector (industry codes A–F), Education, and the Health and Social Work industries, compared to what employees report in the LFS. The ASHE records relatively more new hires in service sector industries (Transport and Telecommunication; Financial Intermediation; Real Estate and Business Services; Public Administration and Defence, Social Security). The distribution of new hires across 3-digit occupation categories is similar between the ASHE records from employers and the LFS reports by workers, as are the gender shares of new hires and starting wages within each of those occupation categories (see Supporting Information S2: Tables A1 and A2 which display statistics for the 20 occupations with the most observed new hires).

3 | DECOMPOSING THE GENDER WAGE GAP INTO HIRES AND FIRM STAYERS

To inform our subsequent discussion and analysis of the gender hiring wage gap, and its importance, we first decompose the overall gender wage gap into its principal dynamic components, extending the work of Manning and Robinson (2004) by including job-to-job transitions in the sample of new hires.

Suppose that we observe N_0 workers indexed by $i = 1, \ldots, N_0$ in year 0. Their log wages are w_{0i}, with an average wage over all employees of $\overline{w}_0$. For each worker, let θ_{1i} equal one if worker i is still employed 1 year later in the same

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TABLE 1 Descriptive statistics for new hires (<12 months tenure).

	LFS hires		All ASHE hires		Dual-gender sample	
	Mean (I)	St. dev. (II)	Mean (III)	St. dev. (IV)	Mean (V)	St. dev. (VI)
A. Worker characteristics						
Hourly wage (2002 GBP)						
Men	8.40	7.75	9.86	9.43	7.41	5.91
Women	7.02	4.66	8.34	5.34	6.92	4.34
Basic wage (2002 GBP)						
Men			9.44	8.43	7.06	5.60
Women			8.09	5.08	6.64	4.04
Age (years)						
Men	32.28	12.28	32.61	11.89	28.53	11.18
Women	31.77	12.03	31.94	11.86	29.19	11.75
Weekly hours						
Men	35.08	11.68	33.76	11.44	27.49	13.03
Women	27.72	12.40	27.66	12.44	24.02	12.69
Years employed						
Men			5.81	7.13	3.94	5.74
Women			5.35	6.34	3.97	5.32
Public sector (%)						
Men	12.2		9.7		12.3	
Women	23.7		19.5		13.1	
Collective agreement (%)						
Men			29.0		48.5	
Women			35.6		48.5	
Full-time (%)						
Men	79.5		76.2		51.4	
Women	54.5		53.7		38.6	
Permanent contract (%)						
Men	80.2		80.7		74.6	
Women	79.6		78.9		78.3	
Commuter (%)						
Men			60.7		65.5	
Women			70.3		70.0	
B. Industry distribution (%)						
A–F	23.0		12.0		2.0	
G–H	30.7		27.3		50.7	
M–N	31.6		23.6		13.2	
I–L, O	14.7		37.0		34.1	
N men (unweighted)	30,241		162,535		39,198	

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TABLE 1 (Continued)

	LFS hires		All ASHE hires		Dual-gender sample	
	Mean (I)	St. dev. (II)	Mean (III)	St. dev. (IV)	Mean (V)	St. dev. (VI)
N women (unweighted)	34,259		174,481		44,379	

Note: Author calculations using the LFS (2005–2020), the ASHE (2005–2020), and the NESPD (1975–2016). "LFS hires" gives statistics for new hires in the LFS, defined as those self-reporting working with their current employer for <12 months. "All ASHE hires" gives statistics for all observed new hires in the ASHE, applying the official sample weights described in the main text. "Dual-gender sample," as defined in the Section 6, gives statistics over the unweighted sample of new hires used for the main results in Table 3. Monetary values deflated to 2002 pound sterling using the UK Consumer Price Index for April. For the LFS, we follow the ONS and assume that overtime is paid at 1.5 times the standard rate. Panel B follows the Office for National Statistics Standard Industrial Classification 2003: A, Agriculture; hunting and forestry; B, Fishing; C, Mining and quarrying; D, Manufacturing; E, Utilities; F, Construction; G, Wholesale and retail; H, Hotels and restaurants; I, Transport and telecommunication; J, Financial intermediation; K, Real estate, business services; L, Public admin. and defence, social security; M, Education; N, Health and social work; O, Other community and social services.

Abbreviations: ASHE, Annual Survey of Hours and Earnings; LFS, Labour Force Survey; NESPD, New Earnings Survey Panel Dataset.

firm and zero otherwise. We refer to workers with $\theta_{1i} = 1$ as *firm stayers* and denote a firm stayer's year-to-year log wage change by g_{1i}. *Hires* are workers with <1 year of tenure with their current employer who were hired either from non-employment or directly from another employer. There are H_1 such hires in year 1 (the following year), indexed by $j = 1, \ldots, H_1$, and each hire has a log wage w_{1j}^h. Total employment in year 1 is thus $\sum_{i=1}^{N_0} \theta_i + H_1$, and the average employee wage in that year can be written as:

$$\overline{w}_1 = \frac{\sum_{i=1}^{N_0} \theta_{1i}(w_{0i} + g_{1i}) + \sum_{j=1}^{H_1} w_{1j}^h}{\sum_{i=1}^{N_0} \theta_{1i} + H_1}. \tag{1}$$

This equation splits overall average wages between those of job stayers and hires. Denoting the share of new hires in total employment by:

$$\alpha \equiv \frac{H_1}{\sum_{i=1}^{N_0} \theta_{1i} + H_1},$$

we can rewrite the year 1 average wage as:

$$\overline{w}_1 = \alpha \overline{w_1^h} + (1-\alpha)\left(\overline{w}_0 + \overline{g}_1 + \frac{\text{Cov}(w_{0i}, \theta_{1i})}{\overline{\theta}_1}\right), \tag{2}$$

where the average log wage change $\overline{g}_1$ is computed only over firm stayers. Equation (2) shows that average pay is a weighted average of the new hires' and firm stayers' average wages. The latter is equal to the sum of the firm-stayers' average wages from the previous year, their average wage growth, and a covariance term that captures the association between last year's wages and the probability of an employee staying in the same firm until the following year. If this covariance term is positive, it implies that workers with higher wages are more likely to remain with their employer from one year to the next, such that the average wage among firm stayers was higher than the overall average wage in the previous year.

However, Equation (2) alone does not explain the origin of the gap, because current wages reflect the entire history of hiring wages and subsequent wage growth components, with geometrically declining weights. To further examine the fundamental determinants of wage differences, we consider a steady state with a constant employment level, such that employment inflows equal outflows:

$$\alpha = 1 - \overline{\theta}.$$

Assuming constant wages in steady state (which is not as restrictive as it might first appear because we deflate wages in our analysis), we can derive the *steady-state* average hourly wage:

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$$\overline{w} = \overline{w^h} + \frac{1-\alpha}{\alpha}\left(\overline{g} + \frac{\mathrm{Cov}(w_i, \theta_i)}{1-\alpha}\right). \tag{3}$$

This expression shows that to understand the determinants of the steady-state average wage, we must examine the average hiring wage and, after adjusting for selection, the average change in the log wages of firm stayers. The level of hiring wages plays a central role in determining the overall average steady-state wage because, intuitively, every firm stayer was a new hire at some point. The role of hiring wages diminishes the longer the average job tenure, the higher the average on-the-job wage growth, and the higher the positive wage selection of firm stayers.

We compute each term in Equations (2) and (3) separately by year and gender. By subtracting women's values from men's in each year, we can characterize how the overall current gender wage gap and its implied steady-state level over time depend on average gender differences in hiring wages and wage growth among firm stayers. When interpreting results from Equation (3), we rely on an assumption that the steady-state provides a good approximation to the UK labor market. This seems reasonable when the entire sample period (2005–2020) is considered, as the average employment rate was 72.9%, with a relatively small standard deviation of 1.8%.[13] However, for some of the studied subperiods, specifically the period after the financial crisis (2009–2012), the steady-state assumption is unlikely to be a good approximation, and this should be kept in mind when interpreting results for these subperiods.

4 | THE AGGREGATE IMPORTANCE OF THE GENDER HIRING WAGE GAP

In this section, we apply the analytical decompositions described in Section 3 to each year from 2005 to 2020, deriving both the current and the implied steady-state gender gaps in average hourly wages. We use the full ASHE dataset and apply the official ONS sample weights to ensure the results are representative.

Panel A of Table 2 displays the decomposition results based on the out-of-steady-state Equation (2), separately for men and women.[14] For ease of exposition, we present the overall averages for the entire period 2005–2020 and for 4-year sub-periods: 2005–2008, 2009–2012, 2013–2016, and 2017–2020, while complete annual estimates are reported in Supporting Information S2: Tables B1 and B2. As row 6 shows, the average gender wage gap is 17.2 log points across the sample, decreasing from 20.1 log points in 2005–2008 to 14.7 log points in 2017–2020. The differences between male and female firm stayers in the employment-share-weighted mean log stayer wages (row 7) account for around 95% of the gender wage gap across all sub-periods. Once a wage gap emerges, it is highly persistent.

Turning to hiring wages (row 2), we find that the gap peaked at 14 log points in 2005–2008, then declined steadily to nine log points in 2017–2020. To provide some context for this finding, Loprest (1992) analyzed a sample of labor market entrants from 1979 to 1983, who were "very attached" to the labor market. She found hiring wage gaps ranging from 11% to 15% in the United States. Manning and Robinson (2004) documented larger wage gaps among entrants to employment from unemployment in Great Britain. Starting in 1992–1994, the gap between men's and women's entrant wages was 20 log points, which then declined to 16 log points during 1995–1997, and finally stood at 14 log points at the end of their sample period during 1998–2000. This suggests that a long-lasting downward trend in hiring wages may have existed in Great Britain for at least the past three decades.

To further examine the sources of differences in firm stayers' past wages, Panel B of Table 2 displays the implied steady-state gender wage gap, as per Equation (3). The steady-state gap is, on average, 14.8 log points, falling from 16.7 log points in 2005–2008 to 13.6 log points in 2009–2012 (the financial crisis period), and remaining near that level thereafter. As can be shown analytically, the speed of convergence of the gender wage gap to its steady-state value is approximately proportional to its distance from $\overline{w}$. This may explain why the decline in the gender wage gap has slowed, from an initial 3.1 log points between 2005–2008 and 2009–2012 to just 1.8 log points between 2013–2016 and 2017–2020. That is, given the relative constancy of the steady state since 2009–2012, the gender wage gap has consistently approached this level, and so the speed of convergence has decreased.[15]

What drives the steady-state gender wage gap? Although the differences between last year's wages of male and female firm stayers are sizable, they are also the outcome of gender differences in the principal components of the steady-state wage gap (Equation 3). Of these components, gender differences in hiring rates, wage growth rates within the firm, and employee retention rates can account for only around one-quarter of the steady-state gender wage gap over 2005–2020. By contrast, the average hiring wage gap is around 10.9 log points, implying that around three-quarters of the steady-state wage gap originates from hiring wages (row 9 of Table 2).[16] Since the financial crisis, gender differences in the covariance between retention rates and wages have become more relevant in accounting for the steady-

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TABLE 2 Decomposition of the gender wage gap.

	2005–2020			2005–2008			2009–2012			2013–2016			2017–2020		
	Men	**Women**	**Gap**	**Men**	**Women**	**Gap**	**Men**	**Women**	**Gap**	**Men**	**Women**	**Gap**	**Men**	**Women**	**Gap**
A. Decomposition, Equation (2)															
1. Share of new hires, α	0.173	0.181	−0.007	0.175	0.184	−0.009	0.149	0.154	−0.004	0.177	0.183	−0.006	0.193	0.203	−0.010
2. Mean log hiring wage, $\overline{w_1^h}$	2.134	2.025	0.109	2.147	2.007	0.140	2.116	2.005	0.111	2.083	1.987	0.097	2.191	2.101	0.090
3. Mean log stayer wage, $\overline{w}_0$	2.386	2.208	0.178	2.404	2.195	0.209	2.409	2.225	0.184	2.343	2.176	0.168	2.388	2.237	0.151
4. Mean log wage change, $\overline{g}_1$	0.021	0.021	0.000	0.032	0.031	0.001	−0.001	0.002	−0.002	0.026	0.023	0.003	0.027	0.027	−0.001
5. Mean covar. term, $\mathrm{Cov}/\overline{\theta}_1$	0.037	0.032	0.005	0.031	0.030	0.001	0.031	0.025	0.006	0.039	0.032	0.007	0.049	0.042	0.007
6. Mean log wage, $\overline{w}_1$	2.391	2.218	0.172	2.411	2.210	0.201	2.391	2.214	0.178	2.350	2.186	0.164	2.410	2.264	0.147
7. Employment-share-weighted mean log stayer wage, $(1-\alpha)\,\overline{w}_0$	1.972	1.809	0.163	1.984	1.792	0.191	2.050	1.883	0.166	1.929	1.778	0.151	1.927	1.783	0.143
B. Steady-state, Equation (3)															
8. Gender wage gap			0.148			0.167			0.136			0.153			0.135
9. Hiring wage gap share (2./8.)			0.739			0.838			0.817			0.634			0.665
Sample sizes (unweighted)															
Firm-stayer, N_0	752,378	788,327		181,201	180,346		196,375	205,019		204,429	216,292		170,374	186,670	
New hires, H_1	157,750	173,705		38,418	40,626		34,418	36,696		43,929	48,507		41,231	47,876	

Note: Summary of decomposition results for the average log hourly wage in the current year following Equation (2) and steady-state values following Equation (3). The column "Gap" shows the difference between men's and women's values. Row 1 shows the share of new hires in total employment. Row 2 shows the current average log hourly wage of new hires. Row 3 shows last year's average log hourly wage of firm stayers. Row 4 shows the average year-to-year change in the log hourly wages of firm stayers. Row 5 shows the average covariance term divided by the average proportion of firm stayers. Row 6 shows the current average log hourly wage. Row 7 shows the average log hourly wage of firm stayers in the last year, weighted by the share of firm stayers. Row 8 shows the implied steady-state log hourly wage gap. Row 9 shows the share of the log hiring wage gap in the steady-state log hourly wage gap, computed using the values in rows 2 and 8. Based on the ASHE (2005–2020) and NESPD (2004). All statistics are weighted sample averages. See Supporting Information S2: Tables B1 and B2 for the annual time series of men and women, respectively.

Abbreviations: ASHE, Annual Survey of Hours and Earnings; NESPD, New Earnings Survey Panel Dataset.

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state gender wage gap, implying that high-wage men are more likely to continue working for their employer than high-wage women.[17]

5 | THE UNEXPLAINED HIRING WAGE GAP

In this section, we analyze how much of the gender hiring wage gap *cannot be explained* by the differences in where men and women work, across both firms and occupations. Our analysis captures the effects of both selection and discrimination on who is hired by which firm and into which occupation. Differences in the firm-stayers' covariance terms are also important for the gender wage gap. However, our payroll-based dataset lacks information on these terms, notably on childbirth and complete labor market experience (e.g., Light & Ureta, 1995). Although our proxy variables for experience and detailed occupation categories partially control for the effects of actual experience and education, this limitation is inherent to our data compared to household surveys.

We estimate the following regressions using least squares (with notational convenience):

$$\log\left(w^h_{ifot}\right) = \text{constant}_t + \gamma_t \text{ Female}_i + \mathbf{Firm}'_{ft}\boldsymbol{\phi}_t + \mathbf{Occ}'_{ot}\boldsymbol{\omega}_t + \mathbf{x}'_{it}\boldsymbol{\beta}_t + \varepsilon_{ifot}, \tag{4}$$

and

$$\log\left(w^h_{ifot}\right) = \text{constant}_t + \gamma_t \text{ Female}_i + \left(\mathbf{Firm}_{ft} \times \mathbf{Occ}_{ot}\right)'\boldsymbol{\eta}_t + \mathbf{x}'_{it}\boldsymbol{\beta}_t + \varepsilon_{ifot}, \tag{5}$$

where w^h_{ifot} is the hourly hiring wage of worker i who is hired by firm f in occupation o in year t. "Female$_i$" equals one if worker i is female and zero otherwise. "$\mathbf{Firm}_{ft}$" is a firm-specific wage effect for the worker's hiring employer f in year t. "$\mathbf{Occ}_{ot}$" likewise gives a wage effect each year for occupation o (3-digit or 4-digit levels), and $\mathbf{x}$ is a vector of worker characteristics that includes age and its square, with time-varying coefficient vector $\boldsymbol{\beta}_t$. The estimates of γ_t give the average amount of the subsample-specific gender hiring wage gap between coworkers, within the same firm-year (Equation 4) or within the same firm-occupation-year (Equation 5), conditional on the other explanatory variables in the models. These are the estimated hiring wage gaps after accounting for gender segregation across employers and jobs within the subsamples, the "unexplained" hiring wage gap. We estimate Equations (4) and (5) over all newly hired employees in the ASHE from 2005 to 2020, allowing all coefficients to change from one year to the next.

There are two important caveats to keep in mind when interpreting the estimates of γ_t: First, Supporting Information S2: Table A4 documents that many firm-year and firm-occupation-year cells contain either a single hire or hires of only one gender. Such cells offer no within-cell gender variation and therefore do not contribute to estimating γ_t. As a consequence, identification relies disproportionately on firms with sufficiently large hiring volumes to employ both men and women in the same year, or in the same year and the same occupation. Given that the ASHE is based on a random one per cent sample of all British employees, the resulting subsample for estimating γ_t mechanically over-represents large firms, particularly those with 1000 or more employees. Specifically, whereas such firms account for around half of all hires in the ASHE, they account for 90%–95% of hires in the subsamples used to estimate Equations (4) and (5) (see Supporting Information S2: Table A5). If firm size correlates with the gender hiring wage gap, the resulting estimates will not recover the contribution of firm-level segregation to the economy-wide gender hiring wage gap, but rather the contribution within the subset of firms where within-firm gender comparisons are feasible.

Second, if there is non-random selection into the ASHE survey according to response rates at the firm level, and if this selection is correlated with the true gender hiring wage gap, then the estimated values of γ_t may not be representative of the corresponding population parameters. Importantly, however, this concern pertains to external validity rather than internal identification. Conditional on the observed sample and on the set of controls included in Equations (4) and (5), the estimates of γ_t consistently capture the average within-cell gender hiring wage gap for the firms, occupations, and years that contribute to identification. Consequently, our results should be interpreted as describing the structure of gender hiring wage gaps among firms with sufficient hiring intensity to permit within-firm and within-occupation gender comparisons, rather than as a full decomposition of the economy-wide gender hiring wage gap.

To assess the quantitative relevance of these limitations, we recompute the raw gender wage gap within each of the two subsamples *of firms* that are used to estimate γ_t in Equations (4) and (5), and compare these gaps to the economy-wide raw gender hiring wage gap. This comparison isolates the extent to which differences in sample

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composition—rather than differential segregation patterns—drive discrepancies between the estimates of the unexplained gap and the aggregate hiring wage gap. Figure 1 plots the economy-wide raw hiring wage gap, the raw hiring wage gaps and the estimates of γ_t from Equations (4) and (5) within each estimation, subsample using 3-digit occupation categories (Supporting Information S2: Table B3 shows the underlying values). We see that the overall raw hiring wage gap from the ASHE, as documented in the previous section, declined by one-third from above 14 log points in 2006–2008 to around 9 log points in 2014 and has remained at that level.

Panel A shows that the gender hiring wage gap in the subsample of firm-years that permits identification of γ_t in Equation (4) closely tracks the overall hiring wage gap, albeit at a lower level (10.9 log points vs. 8.8 log points, on average), and exhibits a similar decline by one-third. Hence, the downward trend is not driven solely by single-gender hiring firms or cells excluded from the estimation sample. Once we control for segregation across firms and occupations within this subsample, the time-series pattern flattens markedly. The unexplained gender hiring wage gap is relatively stable over time. The estimated unexplained gender hiring wage gap, $\widehat{\gamma}_t$, after accounting for occupations, firms, and other covariates, has been relatively stable over time at around 4.4 log points.

The vertical distance between the raw gap and the unexplained gap in Figure 1a gives the contribution of gender segregation across firms and minor occupation groups to the gender hiring wage gap. This gender segregation accounted for over six percentage points in the years up to the Great Financial Crisis; since then, its contribution has declined to below four percentage points. Over the same period, Schaefer and Singleton (2020) have shown that income inequality between employers and occupations has risen in Great Britain. Combined, these two empirical facts suggest that the sorting of women into occupations and employers with particularly low wages must have weakened over the last two decades. The timing of this decline in gender segregation coincides with the public discussion and enactment of the Equality Act 2010 by the UK government. This legislation consolidated and strengthened protections against both direct and indirect discrimination, making it legally harder for employers to prevent women from entering stereotypically high-paying male roles, stimulating female employment from the demand side (Leoncini et al., 2024). In addition, there were substantial and sustained declines in relative pay and employment in the public sector compared with the private sector following the Great Financial Crisis, especially for women (Singleton, 2019), thus likely reducing the role of segregation in the gender pay gap. Further, legislation stemming from the Equality Act 2010 also mandated gender pay gap reporting by both public and private sector employers, and there is evidence that this has lowered gender inequality in the British labor market by dampening male wage growth in firms subject to greater public scrutiny (Blundell et al., 2025).

Panel B of Figure 1 displays similar results from estimating Equation (5), where we address the maximum level of detail and potential segregation over work types by using 4-digit occupation categories within and across firms. The raw gender hiring wage gap in the subsample of firm-occupation-years that permits identification of γ_t in Equation (5) is, on average, 5.7 log points and seems to follow a *different* time path than the economy-wide hiring wage gap: there is no pronounced downward trend to begin with. The unexplained gap between coworkers in these narrow firm-occupation cells accounts, on average, for about 2.4 log points and shows no clear trend. According to these latter estimates, the unexplained share of the gender hiring wage gap in this subsample of jobs has been approximately 40%.

Taken together, these patterns suggest that the decline in the aggregate hiring wage gap is largely attributable to reduced gender segregation across firms and occupations, rather than to changes within narrowly defined jobs. Conditional on being hired into the same firm and detailed occupation in the same year, the gender hiring wage gap is substantially smaller and shows no clear downward trend. A crucial caveat concerns the composition of the estimation samples. As shown in Supporting Information S2: Table A5, firms with 1000 or more employees account for roughly half of hires in the representative ASHE but around 90%–95% of hires in the subsamples used to estimate Equations (4) and (5). Consequently, the within-firm and within-job estimates predominantly reflect wage-setting behavior in large employers. These firms represent a substantial share of employment and hiring, but they are not representative of the entire firm-size distribution in the British labor market. Small firms and highly gender-segregated hiring environments are underrepresented in the estimation samples. This selection does not affect internal validity within the estimation sample: conditional on being in the sample, the coefficients identify the average within-cell gender hiring wage gap. However, it limits the scope for extrapolating the within-job estimates to the broader labor market, particularly to smaller firms where wage-setting institutions and hiring practices may differ.

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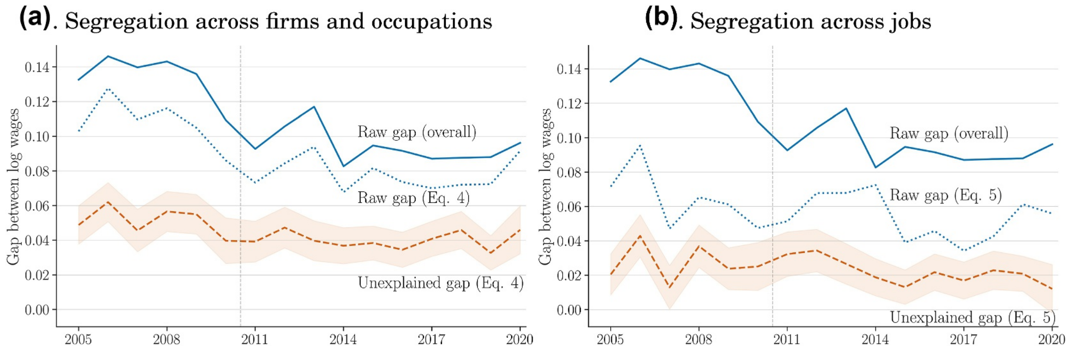

FIGURE 1 Gender hiring wage gap. Estimation results based on Equations (4) and (5), the shaded area indicates the 95% confidence bands. "Raw gap (overall)" shows the difference between the average log hiring wages for men and women in a given year in the ASHE. "Raw gap (Equation 4)" shows the difference between the average log hiring wages for men and women in a given year in the subsample of firm-years that permits identification of γ_t in Equation (4). "Raw gap (Equation 5)" shows the difference between the average log hiring wages for men and women in a given year in the subsample of firm-occupation-years that permits identification of γ_t in estimate Equation (5). "Unexplained gap (Equation 4)" shows the estimates of $\widehat{\gamma}_t$ from Equation (4). "Unexplained gap (Equation 5)" shows the estimates of $\widehat{\gamma}_t$ from Equation (5). Weighted estimates based on the ASHE (2005–2020) and the New Earnings Survey Panel Dataset (2004). The vertical line indicates the change from SOC 2000 to SOC 2010—since all coefficients are allowed to vary year-by-year, both occupational classifications are used without cross-walking. See Supporting Information S2: Table B3 for the underlying estimates. ASHE, Annual Survey of Hours and Earnings; SOC, Standard Occupational Classification.

6 | THE HIRING WAGE GAP WITHIN JOBS

In this section, we focus on the gender hiring wage gap in firms that hire both men and women into the same 4-digit occupation and the same year (hereafter a *job*) within the previous 12 months. This *dual-gender sample* of men and women (Table 1, columns V and VI) comprises just over one-quarter of all hires in the entire ASHE. Because the dual-gender sample is *not* representative of the ASHE or the British labor market as a whole, we report unweighted descriptive statistics and regression estimates throughout this section.

Compared to all new hires, workers in the dual-gender sample are, on average, younger, less experienced, and earn lower gross and basic hiring wages; and women are less likely to be hired by the public sector. The gender hiring wage gap is also smaller in this subsample than in the entire ASHE (6.6% vs. 15.4%), reflecting the omission of single-gender jobs, which, as discussed above, tend to exhibit larger gender differences in pay between them. The distribution across the 20 most common occupations is similar to the previous sample of all ASHE hires (Supporting Information S2: Table A3). However, the industry distribution differs notably (Table 1, Panel B), with the Education, Health and Social Work, and production sectors less frequent than in the entire ASHE sample. Jobs with a relatively high turnover of employees, as well as in larger firms (Supporting Information S2: Table A5, Panel A), are naturally overrepresented in this sample because we are more likely to observe a worker of each gender being hired in such jobs. The distribution of new hires over British regions in the dual-gender sample is not much different to that in the entire ASHE, with the West Midlands being relatively the most over-represented region and London the most under-represented (Supporting Information S2: Table A5, Panel B).

Next, we compute the difference between men's and women's average log wages within each job in the dual-gender sample. The light bars in Figure 2 show substantial dispersion in the gender wage gaps among new hires. Only eight per cent of employees start jobs where the women's hiring wages are, on average, within half a log point of the men's hiring wages in the same year; the average employee in the sample is hired into a job with a hiring wage gap of 1.9 log points between men and women.

When instead looking at all the employees (not only new hires) in the same set of firms and years as the jobs in our dual-gender sample, without requiring that men and women work in the same occupations, the dark bars in Figure 2 show that the overall average gender wage gaps at the firm-level are generally much larger than hiring gender wage gaps within jobs at the same firms. This could be explained by factors that have been widely studied elsewhere but are beyond the scope of our focus on the hiring wage gap here (e.g., constraints that prevent women from working long hours to compete with men and thus progress in their careers; Cortés & Pan, 2019; Goldin, 2014). Nonetheless, in an even more heavily selected sample of employees who remain in the same job for up to 2 years after we observe them as

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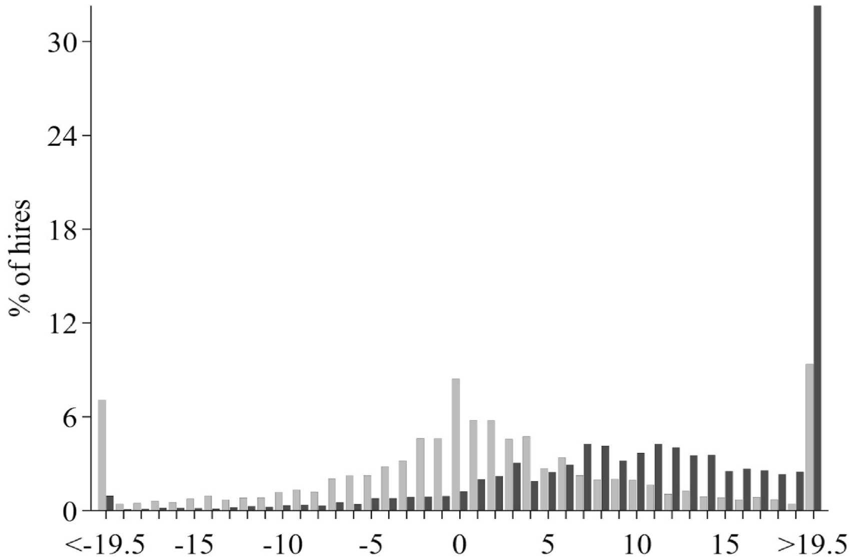

FIGURE 2 Distributions of the log hiring wage gap within jobs over new hires and the log wage gap across all other employees within the same firms. Based on the Annual Survey of Hours and Earnings (2005–2020). The zero bar shows the range $[-0.5, 0.5]$. Bars to the right and left, then go up and down in one log point increments. Light bars: Mean difference in log wages between men and women (male-female) who were hired in the same year and the same firm-occupation (mean = 1.9 and st. dev. = 19.0). Dark bars: Difference in log wages between all men and women in a firm-year that features in the hiring sample, not necessarily in the same occupation (mean = 15.2 and st. dev. = 14.5).

new hires alongside someone of the opposite gender, we find no significant gender differences in year-to-year wage growth within jobs.

To more formally analyze the hiring wage gap, we estimate the following wage regression using least squares (again, somewhat abusing notation):

$$\log\left(w^h_{ifot}\right) = \text{constant} + \gamma \text{ Female}_i + \left(\mathbf{Firm}_{ft} \times \mathbf{Occ}_{ot}\right)'\boldsymbol{\eta} + \mathbf{x}'_{it}\boldsymbol{\beta} + \mathbf{Region}'_{it}\boldsymbol{\rho} + \varepsilon_{ifot}, \tag{6}$$

where as before, w_{ifot} is the hourly wage of worker i hired into firm f and occupation o in year t. Female$_i$ is an indicator that equals 1 for women and is 0 for men, γ estimates the unexplained or residual within-job female wage penalty. The vector $\left(\mathbf{Firm}_{ft} \times \mathbf{Occ}_{ot}\right)$ consists of firm-year-occupation-fixed effects or *job*-fixed effects, subsuming year-fixed effects that control for the state of the aggregate labor market at the time of hiring. The term $\mathbf{x}_{it}$ is a vector of covariates. The *baseline covariates* include an employee's age and the cumulative number of years a worker can be observed as an employee in the ASHE or its predecessor (NESPD), dating back to 1975, as well as the squared values of these variables. We include age not only as a more general proxy for labor market experience but also to capture any potential effects of age discrimination. Additionally, the covariates include the log of weekly hours worked, a full-time indicator variable that equals one if the new hire's basic weekly hours worked (excluding overtime) are at least 30 h per week, and a permanent job indicator variable that equals one if the position is permanent rather than temporary or fixed-term. Finally, we include an indicator variable for commuting, which equals one if the workplace is outside the travel-to-work area of the employee's home address and zero otherwise.[18] We also include fixed effects for the 11 NUTS1 British regions of an employee's workplace, for each year (e.g., London vs. Scotland), $\mathbf{Region}_{it}$, to account for any regional differences in local labor market wage trends—these are identified because a firm in ASHE can be observed with new hires in the same occupation and year but at different workplace locations accross the country. The remaining unobserved wage variation is in the residual ε_{ifot}. We begin by pooling the years in our estimation sample to obtain a single estimate of γ, which we subsequently relax.

Note that Equation (6) is different from Equation (5) in three dimensions. First, it is estimated on the *dual-gender* subsample. Second, it enriches the control set by adding worker covariates, $\mathbf{x}$, and region-by-year fixed effects. Because the unexplained gap γ is identified solely from wage differences **within** a firm-occupation-year cell, any cell that contains hires of only one gender contributes no information. Hence, when the covariate vector $\mathbf{x}$ is omitted, both the full-hire sample and the dual-gender sample use the same cells to estimate γ and yield equivalent unweighted point estimates (and very similar clustered standard errors). When covariates are included, the estimates can differ between the two samples because the full-hire sample also exploits single-gender cells to pin down $\boldsymbol{\beta}$, which in turn feeds back into $\widehat{\gamma}$. Third, in Equation (6), we do not estimate the coefficients separately for each year.

We prefer our approach, restricting the estimation to the dual-gender sample, because it offers two main advantages. First, it is more transparent about the number of unweighted observations used to estimate $\widehat{\gamma}$. Second, the coefficients for $\mathbf{x}$ are estimated from the same dual-gender jobs, avoiding contamination from single-gender cells. This matters because very segregated jobs that do not hire both genders may remunerate observable characteristics differently from jobs in which men and women are commonly hired together.

Table 3 displays the results from estimating Equation (6) using different sets of fixed effects. Column (I) shows that the gender hiring wage gap in the dual-gender sample is 4.6 log points. That is, women earn, on average, around 4.5% less per hour than men hired into the same job. In the previous section, we found that, over the same sample period, the raw gender hiring wage gap among all new hires is, on average, 10.9 log points (Table 2). Supporting Information S2: Table B4 displays the estimate of $\widehat{\gamma}$ for each year.

When we control for the average hiring wage within workplace region, firm-years, and 3-digit occupation-years (column II), the estimated unexplained wage gap is 1.9 log points, indicating that women were more likely than men to be hired by low-wage firms or occupations, or in years or regions with lower average wages. Once we additionally control for 4-digit occupations within the same firm and year, $\left(\mathbf{Firm}_{ft} \times \mathbf{Occ}_{ot}\right)$, the average hiring wage gap within jobs is 1.8 log points (column III) and stable since 2014, consistent with our findings in the previous section (Figure 1 and Supporting Information S2: Table B3).

In column (IV) of Table 3, we add the baseline controls in $\mathbf{x}_{it}$. We center variables around their sample averages, so that $\widehat{\gamma}$ can be interpreted as the average partial effect of Female in all columns. Hiring wages are increasing and concave in age and in the number of years of previous employment. If a job is a full-time or permanent position, hiring wages tend to be significantly higher. In addition, log basic weekly hours worked have a significantly negative partial effect on the hiring wage within jobs, generating an elasticity estimate of −0.024. Interestingly, when an employee commutes from their home travel-to-work-area to their workplace, their hiring wage is significantly lower, though only by around one per cent. Adding these control variables yields an estimated hiring wage gap within jobs of 2.4 log points; over half of the gender hiring wage gap in the dual-gender sample remains unexplained (column I vs. column IV).

Finally, column (V) of Table 3 reports estimates obtained by additionally interacting all observable characteristics with the Female indicator variable, allowing these characteristics to have differential effects on men's and women's hiring wages. We find significant differences between men and women in the coefficient estimates for age and age squared: age has a significantly more negative effect on women's hiring wages until age 40. Additionally, women receive a lower hiring wage premium for permanent positions than men. We do not find a differential effect of commuting patterns on men's and women's hiring wages. This is interesting, since Le Barbanchon et al. (2021) have shown that women prefer shorter commuting times more strongly than men. Similarly, Caldwell and Danieli (2024) find that the implicit costs of commuting and moving can explain around 20% of the gender earnings gap in Germany. Further, using the ASHE for the period 2012–2019, Petrongolo and Ronchi (2020) find evidence not only of a sizable age-increasing commuting gap between British men and women, and smaller changes in commuting distances when women move jobs, but also that changes in commuting distances are compensated slightly more for women than men. However, the lack of information on worker characteristics, particularly education and actual cumulative labor market experience, rather than our proxy measures for the latter, means that our results must be interpreted with caution. As explained in Section 2, our estimates are unbiased under the following assumptions: (1) the relationship between employment disruptions and the calendar month of April does not significantly depend on an individual's gender; and (2) the sorting of new hires with different education levels across detailed occupation categories and firms does not depend on gender.

The main estimate of an unexplained hiring wage gap of 2.4 log points is economically significant, translating into an annual earnings loss of £337 (2002 pound sterling) for the average female new hire in our sample in the year of hiring.[19] Moreover, as we demonstrated in the previous section, almost three-quarters of the steady-state wage gap can be attributed to gender differences in hiring wages over our whole sample period, and two-thirds in 2017–2020 (Table 2). Our finding that around two-thirds of the hiring wage gap remains unexplained translates roughly into half of the steady-state overall wage gap, or 7.4 log points, being left unexplained after controlling for observable characteristics and detailed job-fixed effects.

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TABLE 3 Estimation results for the gender hiring wage gap within jobs.

Coefficient	(I)	(II)	(III)	(IV)	(V)
1. Female ($\hat{\gamma}$)	−0.0458***	−0.0191***	−0.0182***	−0.0235***	−0.0316***
	(0.0032)	(0.0017)	(0.0016)	(0.0016)	(0.0025)
2. Age				0.0055***	0.0066***
				(0.0002)	(0.0002)
3. Age × female					−0.0018**
					(0.0002)
4. Age^2 (00s)				−0.0229***	−0.0254***
				(0.0009)	(0.0012)
5. Age^2 × female (00s)					0.0045**
					(0.0014)
6. Years employed				0.0043***	0.0051***
				(0.0004)	(0.0006)
7. Years employed × female					−0.0012*
					(0.0007)
8. (Years employed)^2 (00s)				−0.0116***	−0.0151***
				(0.0019)	(0.0026)
9. (Years employed)^2 × female (00s)					0.0049
					(0.0038)
10. log(weekly hours)				−0.0242***	−0.0276***
				(0.0029)	(0.0038)
11. log(weekly hours) × female					0.0067
					(0.0042)
12. Full-time				0.0234***	0.0269***
				(0.0029)	(0.0040)
13. Full-time × female					−0.0095*
					(0.0048)
14. Permanent				0.0237***	0.0292***
				(0.0039)	(0.0046)
15. Permanent × female					−0.0102**
					(0.0041)
16. Commmuter				−0.0116***	−0.0105***
				(0.0023)	(0.0029)
17. Commuter × female					−0.0013
					(0.0035)
Constant	1.8864***	1.8722***	1.8717***	1.8674***	1.8590***
Workplace region × year FEs		✓	✓	✓	✓
Firm × year FEs		✓			
Occupation (3d) × year FEs		✓			
Firm × occupation (4d) × year FEs			✓	✓	✓

(Continues)

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TABLE 3 (Continued)

Coefficient	(I)	(II)	(III)	(IV)	(V)
R^2	.003	.763	.787	.797	.798
N firm-years/jobs		9714	13,001	13,001	13,001
N hires	83,577	83,577	83,577	83,577	83,577

Note: Estimation results based on Equation (6), the ASHE (2005–2020), and the NESPD (1975–2016; used only to construct cumulative years of worker experience). The sample contains jobs (firm-4-digit-occupation-year) in which at least one man and one woman joined the firm in the past year. Wages are deflated to 2002 pound sterling using the UK Consumer Price Index for April. "Age" measures the age of a new hire in years. "Years employed" measures the number of years we observe a new hire in any previous jobs in the ASHE and the NESPD since 1975. "Weekly hours" are basic, that is, excluding any overtime. "Full-time" is an indicator variable that equals one if the previous job was full-time (≥30 h) and zero otherwise. "Permanent" indicates an open-ended employment contract. "Commuter" is an indicator variable that equals one if the workplace is outside the travel-to-work area of the employee's home address and zero otherwise. We center variables on their sample-average before squaring or interacting.

Abbreviations: ASHE, Annual Survey of Hours and Earnings; FEs, fixed effects; NESPD, New Earnings Survey Panel Dataset.

***, **, * indicate significance from zero at 1%, 5%, and 10% levels, respectively, two-sided tests. Standard errors in parentheses are robust to clustering by firm-occupation-year.

6.1 | Robustness

6.1.1 | Basic wages

Even when workers are hired into the same job, individual pay packages may still differ by gender due to additional payments. For example, women may be less likely to work weekend or night shifts because of family responsibilities. We repeat all estimations in Table 3 using the *basic* hourly wage as the dependent variable, which excludes extra pay components such as incentive pay or shift premium pay (Supporting Information S2: Table B5). The gender hiring wage gap in basic wages within jobs is slightly smaller at 1.9 log points (column IV). This result suggests that the non-basic components of pay account for only a small share of the overall hiring wage gap. We also find that basic hours worked have no significant effect on basic hourly hiring wages. Furthermore, their small negative impact on hourly earnings is attributable to components of pay that do not vary with hours worked, for example, meal and travel allowances.

6.1.2 | Plants

If a firm has multiple local units, each is assigned an individual identifier in the ASHE, linked to a specific geographic location. Unfortunately, in the ASHE dataset, the local unit identifier variable is created each year but only as a marker, and hence, it cannot be linked longitudinally (Whittard et al., 2022). For a subset of observations, we also have these "plant identifiers." Using this subset, we can compare the hiring wages of men and women hired into the same occupation by the same firm in the same year, *and into the same plant*. Our coefficient estimates remain quantitatively robust to this refinement, although the much smaller sample size would yield an overall underpowered study.

6.1.3 | Full-time and permanent positions

Women may have a higher propensity for working in temporary and part-time positions, despite holding the same jobs as men. Since these contract types pay systematically lower wages, this may explain the observed gender hiring wage gap. To analyze this possibility, we have repeated our estimation of Equation (6) using a subset of the data that includes only hires into full-time, permanent positions (Supporting Information S2: Table B6). We find that the raw average hiring gap is somewhat smaller than in the full sample (3.3 log points vs. 4.6 log points), and the unexplained hiring wage gap is larger than in the full sample once we add control variables. Specifically, adding baseline controls in addition to workplace region and firm-occupation-year fixed effects yields an estimate of the hiring wage gap of 2.7 log points in the subsample of full-time and permanent positions, compared with 2.4 log points in the full sample. This suggests that systematic differences in the propensity of women to be hired in temporary and/or part-time roles, within jobs, cannot explain the documented hiring wage gap between coworkers.

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6.1.4 | Time-invariant person effects

Since our individual-level control variables are limited to age, gender, and the number of years employed, it is possible that (for the econometrician) unobserved employee characteristics may account for the estimated gender hiring wage gap within jobs. We investigate this by estimating worker-specific earnings fixed effects, following the method of Abowd et al. (1999), hereafter referred to as AKM. First, we use all worker and firm observations in the ASHE from 2005 to 2020 in the largest mobility group (connected set) to estimate the AKM wages model with worker and firm fixed effects (see Jewell et al., 2020, for details on this AKM method and its implementation in the context of Great Britain using the ASHE). The estimated AKM worker effects reflect the average contribution of an individual worker's time-invariant factors (e.g., some skills and abilities) to their hourly wages, holding other factors constant in the model, including the time-invariant wage levels (policies) of the worker's employers. Thus, these AKM worker effects account for all observed and unobserved fixed worker characteristics that are relevant to earnings. Second, we allocate the AKM worker effects into deciles over all employee-years in the model estimation sample. Third, we compute the most common AKM-worker-effect decile among the new hires within each job (i.e., firm-occupation-year). Fourth, we only retain new hires in our wage gap analysis whose own AKM worker effect decile matches the most common decile in their job. This should ensure that new hires in a job are more comparable than in our previous analysis, given the time-invariant unobserved worker characteristics that matter for earnings. Finally, we repeat our main regression analysis using only the described subsample of workers.

Despite the somewhat restrictive sample selection, we retain approximately half of the observations from the original estimation sample. Since the decomposition exercise in Section 4 has shown that the hiring wage gap is persistent throughout a worker's career, the AKM worker effects reflect, to a large extent, the hiring wage. This means that, by keeping only new hires with similar AKM worker effects, we effectively condition on workers having similar hiring wages, stacking the estimation against finding any significant unexplained gender hiring wage gap. The results of the estimation are displayed in Supporting Information S2: Table B7. As expected, the raw hiring wage gap is lower in the restricted analysis than in the full sample, at 3.6 log points versus 4.6 log points. When adding the baseline controls (column IV), we find that the unexplained portion of the hiring wage gap decreases from 2.4 log points in the baseline analysis to approximately one log point in the AKM subsample, which is only around 30% of the original gap but remains statistically significant. These estimates suggest that, even among a sample of new hires that accounts for any unobservable time-invariant worker characteristics affecting earnings, we still find evidence of unequal hiring wages.

6.1.5 | Omitted variable bias

We also evaluate the robustness of our results to potential omitted variable bias, providing bias-adjusted bounds of the gender hiring wage gap estimates following Oster (2019). Applying her method, we find bounds for $\widehat{\gamma}$ of $[-0.0252, -0.0235]$, suggesting that selection on unobservable characteristics is leading us to marginally *underestimate* the hiring wage gap (see Supporting Information S2: Appendix C for computations). Note that the lower bound is more negative than the original coefficient estimate, because selection on unobservables is assumed to be proportional to selection on observables according to the Oster (2019) method, and the hiring wage gap increases in Table 3 from column (III) to column (IV). Indeed, for the set of bounds to include zero, the selection on unobservables would need to be both in the opposite direction as the selection on observables and more than 14 times as strong in magnitude. Similarly, when we repeat the computation for the comparable estimates of $\widehat{\gamma}$ using basic wages and AKM worker effects, we obtain bounds of $[-0.0208, -0.0194]$ and $[-0.0101, -0.0096]$, respectively.

6.2 | Potential explanations of the within-job hiring wage gap

The evidence in Sections 5 and 6 indicates that, conditional on being hired into the same firm and detailed occupation in the same year, a gender hiring wage gap of around 2–3 log points remains. This within-job gap is substantially smaller than the aggregate hiring wage gap and, importantly, shows no clear downward trend over the sample period in our large-firm-dominated estimation sample. In this section, we examine potential mechanisms that could account for this persistent within-job hiring wage gap.

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The existing literature has proposed three broad classes of explanations for gender pay differences arising at the point of hiring. First, gender differences in wage bargaining, documented in laboratory and field settings (Bertrand, 2011; Roussille, 2024; Säve-Söderbergh, 2019), may translate directly into lower starting pay for women. Second, women may accept lower wages than men in exchange for non-wage amenities, and employers may exploit lower firm-specific labor-supply elasticity among women (Flabbi & More, 2012; Le Barbanchon et al., 2021; Rosen, 1986). Third, taste-based or statistical discrimination may depress women's wage offers even when jobs and tasks are identical (Becker, 1971; Black, 1995; Bowlus & Eckstein, 2003; Phelps, 1972). In what follows, we examine each mechanism where the data allow.

6.2.1 | Gender differences in bargaining

If bargaining matters at the hiring stage, we should expect the unexplained within-job gap to widen in contexts where individual negotiation is most prevalent, and to narrow where bargaining discretion is limited. We evaluate this prediction in three ways.

Hiring wage gaps across the wage distribution: We first re-estimate column (IV) of Table 3 separately for each quartile of the job-level hiring wage distribution using the dual-gender sample. The unexplained gender wage gap within jobs is statistically significant in each quartile of the hiring wage distribution, but the largest gap (4.5 log points) occurs in the top quartile (Supporting Information S2: Table B8). Because wage posting is more common in low-paid jobs, whereas high-paid jobs are more likely to involve individual negotiation (e.g., Lachowska et al., 2022), the sharp widening of the upper tail is consistent with the bargaining channel operating within large firms.

Elasticity of the hiring wage to the previous wage: We next examine whether a new hire's previous wage affects their hiring wage and whether this relationship differs by gender. Since the previous wage proxies for the threat point in wage bargaining, the hiring wage elasticity with respect to the previous wage is informative about bargaining dynamics. For this analysis, we restrict the dual-gender sample to individuals with payroll records from a previous job within the past 2 years.[20] Compared to the broader dual-gender sample, employees in this subsample are, on average, older, more likely to be hired into the public sector, have more prior work experience, and earn higher wages (Supporting Information S2: Table B9, columns VII and VIII). The hiring wage gap is also larger in this subsample (8.1% vs. 6.5%).

We augment the baseline covariate vector with additional controls for the previous log wage and a commuter-status change indicator, interacting both with Female_i to test for gender-specific effects. Supporting Information S2: Table B10 reports the resulting estimates. A new hire's previous wage has a statistically significant positive effect on their hiring wage (row 18), but the effect is significantly smaller for women (row 19): the estimated elasticity is 0.205 for men and 0.181 for women. The commuter-status change indicator and its interaction with Female are not statistically significant, suggesting that commuting is not of first-order importance for understanding the within-job gender hiring wage gap.[21] These findings are consistent with gender differences in bargaining contributing to the persistent hiring wage gap.[22]

Professional jobs and collective agreements: To further probe the bargaining mechanism, we split the sample by job type and pay-setting regime. First, we approximate "professional" jobs as those *not* in the retail, hospitality or catering industry and those classified as managerial, professional, or technical occupations.[23] Following Hall and Krueger (2012), who showed that wage bargaining is more prevalent among professional "knowledge" workers, we expect greater scope for negotiation in these jobs. Among professional jobs, the elasticity of a man's hiring wage to his previous wage is relatively large, while women exhibit a significantly lower elasticity (Supporting Information S2: Table B11, columns I and II). This gender difference is not observed in non-professional jobs Second, we split jobs into those covered by a collective pay agreement.[24] In both covered and uncovered jobs, the previous wage significantly affects the hiring wage (Supporting Information S2: Table B11, columns III and IV). However, the gender difference in elasticity is concentrated in jobs not covered by collective agreements, where individual wage-setting discretion is greater.

Taken together with our evidence on declining segregation across firms and occupations, the above findings suggest a two-part interpretation. While changes in sorting patterns account for much of the aggregate convergence in hiring wages over time, unequal pay also arises because women may negotiate less frequently or less effectively at the point of hire (Bertrand, 2011; Roussille, 2024; Säve-Söderbergh, 2019). Because we observe realized wages but not wage requests, we cannot disentangle bargaining behavior from differences in wage expectations. Nevertheless, the evidence is consistent with the notion that gender differences in bargaining contribute to the persistent within-job hiring wage gap, particularly in jobs with greater wage-setting discretion. These patterns are identified among firms with sufficiently large hiring flows to observe both male and female hires in the same job.

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6.2.2 | Non-wage amenities and discrimination

Non-wage amenities and discrimination. Another explanation is that women place a higher value on non-wage amenities, leading to lower wage bargains (e.g., Morchio & Moser, 2026). Relatedly, Manning (2003) argues that women face greater mobility constraints and may be less responsive to wage differentials, implying greater employer monopsony power and the possibility of Robinsonian discrimination (Robinson, 1933). We control for key non-wage amenities—occupation, hours worked (Card et al., 2016), and commuting patterns (Le Barbanchon et al., 2021)—and still find a residual gender hiring wage gap within jobs. Additionally, gender differences in non-wage amenity valuations cannot account for the systematic gender differences in the elasticity of the hiring wage to the previous wage.

Standard models of taste-based or statistical discrimination could also explain a residual gender hiring wage gap. However, such models do not naturally explain why employers increase hiring wages in response to higher previous wages, but do so less strongly for women. That said, in environments where bargaining discretion is limited—such as collectively covered jobs or non-professional occupations—our findings remain consistent with a potential role for discrimination.

Finally, we note that we do not exploit random or experimental variation in bargaining power or discrimination, which limits our ability to causally identify these mechanisms. Nonetheless, the patterns observed are consistent with gender differences in bargaining and, to a lesser extent, discrimination contributing to the gender hiring wage gap. We emphasize that these mechanisms are intended to explain the persistent within-job hiring wage gap observed in our large-firm-dominated estimation sample. They do not, by themselves, account for the decline in the aggregate hiring wage gap over time, which Section 5 shows is closely linked to reduced gender segregation across firms and occupations.

7 | CONCLUSION

In a large, representative sample of payroll-based data for Great Britain, we decompose the overall gender wage gap into two components: pay progression within firms and hiring wages. Hiring wage differences are the primary driver, accounting on average for almost three-quarters of the long-run gender wage gap if wages converge to their steady-state levels. While the aggregate hiring wage gap has declined over time, largely reflecting reduced gender segregation across firms and occupations, hiring wage differences remain central to the steady-state level of pay inequality. Even after accounting for the segregation of men and women across different firms and occupations, roughly 20% of the recent hiring wage gap in Great Britain remains unexplained.

When men and women are hired into the same firm and detailed occupation in the same year (the dual-gender sample)—a sample dominated by larger employers with sufficient hiring flows to observe both male and female co-hires—the raw gap is 4.6 log points. Controlling for job-year fixed effects reduces the gap to 1.8 log points, and adding controls for age, hours worked, commuting patterns, and proxies of previous labor market experience raises it to 2.4 log points. A worker's previous wage positively affects their hiring wage, but the elasticity is significantly smaller for women—except in non-professional jobs or those covered by collective pay agreements. These patterns are consistent with gender differences in wage bargaining contributing to the persistent within-job hiring wage gap, particularly in jobs with greater wage-setting discretion. We caution, however, that simply urging women to negotiate more may backfire, as Exley et al. (2020) provide evidence that women already enter negotiations that result in gains, avoiding negotiation opportunities that would result in losses.

Accordingly, our within-job estimates should be interpreted as describing wage-setting behavior in large firms rather than the entire firm-size distribution of the British labor market. Yet it reflects the furthest extent to which hiring-stage inequality can be studied with existing payroll data, since such datasets rarely contain records on both hours worked and detailed occupations—both are advantages of the ASHE. Further work with richer administrative sources could allow us to track successive cohorts of new hires and test whether cohort-specific hiring accounts for the cohort-driven decline in the gender pay gap documented by Arellano-Bover et al. (2024).

The 2018 UK mandate requiring all firms with at least 250 employees to publicly and annually report their gender pay gaps has already reduced within-firm gender pay inequality (Blundell et al., 2025).[25] Because the ASHE covers only one per cent of employees, we cannot exploit this reform to analyze the effect of the reporting law on hiring wage gaps within jobs, but doing so remains an important area for future research. Nevertheless, our findings suggest that disclosure requirements focusing solely on the realized wage gap might miss a critical margin: the hiring wage gap.

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Asking firms to report gender differences in *starting* pay would isolate (Un)Equal Pay at the point of hire, before promotions, tenure, and other career dynamics obscure disparities rooted in initial hiring decisions. Our results underscore that hiring wages, not just realized ones, deserve greater scrutiny in efforts to close the gender pay gap. Future work is needed to assess whether transparency reforms affect wages within narrowly defined jobs.

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DATA AVAILABILITY STATEMENT

The data that support the findings of this study are available from the UK Data Service. Restrictions apply to the availability of these data, which were used under license for this study. The code that supports the findings of this study is openly available in ICPSR at https://doi.org/10.3886/ICPSR304158.V2, see Pham et al. (2026).

ORCID

Daniel Schaefer https://orcid.org/0000-0001-5413-7924
Carl Singleton https://orcid.org/0000-0001-8247-8830

ENDNOTES

[1] Thus, we draw a parallel with the essence of what is generally known as "Equal Pay." For example, since 1970 in the UK, women can claim for discrimination in their pay and work conditions by pointing toward a male comparator in the same firm whose work is the same or broadly similar, or whose work has equal value, in terms of factors like skill, effort, responsibility, or decision-making.

[2] Recent studies that apply event-study methods also found a lasting impact of (fertility-related) career breaks on the gender wage gap, for example, Angelov et al. (2016), Kleven, Landais and Søgaard (2019), and Lundberg and Rose (2000).

[3] The two categories of hires account for approximately equal proportions of all new hires in the United Kingdom (Gomes, 2012).

[4] This is also consistent with the results of a recent field experiment by Barach and Horton (2021).

[5] See, for example, Fortin et al. (2017) for an insightful discussion of how top-coding incomes affects gender wage gap estimates.

[6] The information in the dataset on employer characteristics comes from the UK's administrative Inter-Departmental Business Register, the official list of UK enterprises. Employers in the ASHE are typically enterprises, as defined for UK government administrative purposes: the smallest combination of legal units that is an organizational unit and benefits from a certain degree of autonomy in decision-making, especially in allocating its current resources. Employers include all private, public, and voluntary sector employers.

[7] For the official figures on the UK gender pay gap see https://www.ons.gov.uk/employmentandlabourmarket/peopleinwork/earningsandworkinghours/bulletins/genderpaygapintheuk/2023.

[8] For information on the UK SOC 2000 and SOC 2010, see https://www.ons.gov.uk/methodology/classificationsandstandards/standardoccupationalclassificationsoc/socarchive and https://www.ons.gov.uk/methodology/classificationsandstandards/standardoccupationalclassificationsoc/soc2010, respectively.

[9] The NESPD had the same eligible population as ASHE, based on the same last two digits of National Insurance numbers. However, the NESPD does not include firm identifiers, so we can only conduct our analysis within jobs and between co-workers from 2005 onward.

[10] For instance, staff in UK higher education institutions are technically in the private sector, but many tend to self-report being in the public sector.

[11] We compute the hourly wage in the LFS as GRSSWK/[BUSHR + 1.5 × (TTUSHR − BUSHR)], where GRSSWK, BUSHR, and TTUSHR denote gross weekly pay in the main job, total usual hours worked in the main job, excluding overtime, and total usual hours worked in the main job, including overtime, in the LFS, respectively. Assuming a uniform 1.5 multiplier in the LFS for overtime pay may mask important gender differences from this data source.

[12] We calculate the hiring wage gap as the difference between the average (mean) pay of men and women divided by the average pay of men, as per ONS official statistics. However, in most of what follows, we analyze the average differences between men's and women's log wages.

[13] Source: ONS Labour Market Statistics Time Series https://www.ons.gov.uk/employmentandlabourmarket/peopleinwork/employmentandemployeetypes/datasets/labourmarketstatistics, accessed August 12, 2025.

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[14] For job-stayer wages in 2004–2005, we use NESPD data.

[15] We identify new hires by the starting date at the company in the ASHE as provided by the survey respondent. If company mergers or spin-offs result in incorrect employee starting date entries, we would incorrectly identify some firm stayers as new hires. To assess whether this would have the potential to affect our results substantially, we recalculated the decompositions, dropping firms and all their employees from the dataset in the year that their identifier first appeared after the year 2002 (before which there is substantially incomplete coverage of employer identifiers in the ASHE). This would remove any truly new firms, as well as any firms that received a new administrative identifier due to mergers and spin-offs, which might also have led to new employment start dates for workers who, in effect, actually stayed in the same job around the administrative change in firm identifier. Our findings are unaffected by these sample adjustments, and the results are available on request from the authors.

[16] The hiring wage gap expressed here, as the difference in average log wages, appears smaller than the values described in Table 1, column (III). This is because the hiring wage distribution is right-skewed, particularly for men, and we use the natural logarithm to transform hiring wages in the decomposition and following regression models.

[17] For some evidence on the importance of these covariance terms for the gender wage gap in Great Britain, we refer the reader to Manning and Swaffield (2008).

[18] Travel-to-work areas (TTWA) are defined by the ONS using census data for commuting between local districts, based on the different locations of employees' home and workplace addresses. A TTWA is a collection of local districts for which at least 75% of the resident economically active population work in the area, and also, for which at least 75% of everyone working in the area also lives in the area. According to this measure, there were 228 TTWAs within the United Kingdom based on the 2011 census.

[19] 2002 pound sterling and using men's average hourly hiring wage £9.23 (Table 1), assuming the average annual hours worked in the UK of 1538 (OECD, 2020): $£9.23 \times 1538 \times (1-\exp(-0.024))$.

[20] We do not consider using information from workers' past jobs that are further in the past to exclude individuals with long breaks from work, which would likely correlate with both gender and wages.

[21] These results are not necessarily inconsistent with those in Petrongolo and Ronchi (2020), who, using ASHE 2012–2019, find that women are compensated more than men for changes in commuting distance when moving jobs. There are three main plausible reasons why we may not replicate this pattern. First, our commuting change variable, which is based on travel-to-work areas, is substantially coarser than the geometric distance measure based on Census areas used by Petrongolo and Ronchi (2020). Second, our estimates focus on hiring wages, which are not limited to job-to-job switchers. Third, we include substantially more granular controls for job characteristics (Firm × Occupation (4-digit) × Year fixed effects), whereas Petrongolo and Ronchi (2020) use broader occupation and industry controls. As a result, gender differences associated with commuting adjustments may operate through between-job mobility across firms and detailed occupations—variation that remains in their specification but is absorbed by our finer fixed-effects structure.

[22] If human capital is not fully transferable across jobs and women are more likely to switch occupations, lower estimated elasticities could arise without gender differences in bargaining. To assess this possibility, we include an indicator for switching 3-digit (or 2-digit) occupations between jobs. Since human capital is largely occupation-specific (Kambourov & Manovskii, 2009; Postel-Vinay & Sepahsalari, 2023), occupational changes are the relevant margin for human capital loss. Including this control leaves the coefficients on gender, previous wages, and their interaction essentially unchanged, suggesting that differential human capital loss does not explain women's lower wage elasticity.

[23] We use the ONS Standard Industrial Classification 2003 (G–H: Wholesale and retail, Hotels and restaurants) and the SOC 2000/2010 (1–3: Managers, directors and senior officials, Professional occupations, and Associate professional and technical occupations).

[24] The ASHE records whether an employee's pay is set with reference to an agreement affecting more than one worker, for example, pay agreed by a trade union or a workers' committee. We classify this as a worker's pay being subject to a collective agreement.

[25] Examples of other countries include Austria and Denmark, where firms must report gender pay gaps to their employees instead of publicly. For evaluations of how these policies have affected pay gaps within firms, see Böheim and Gust (2021) for Austria and Bennedsen et al. (2022) for Denmark.

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