How effective has mandatory pay gap reporting been at reducing the gender wage differential in the UK financial services sector?

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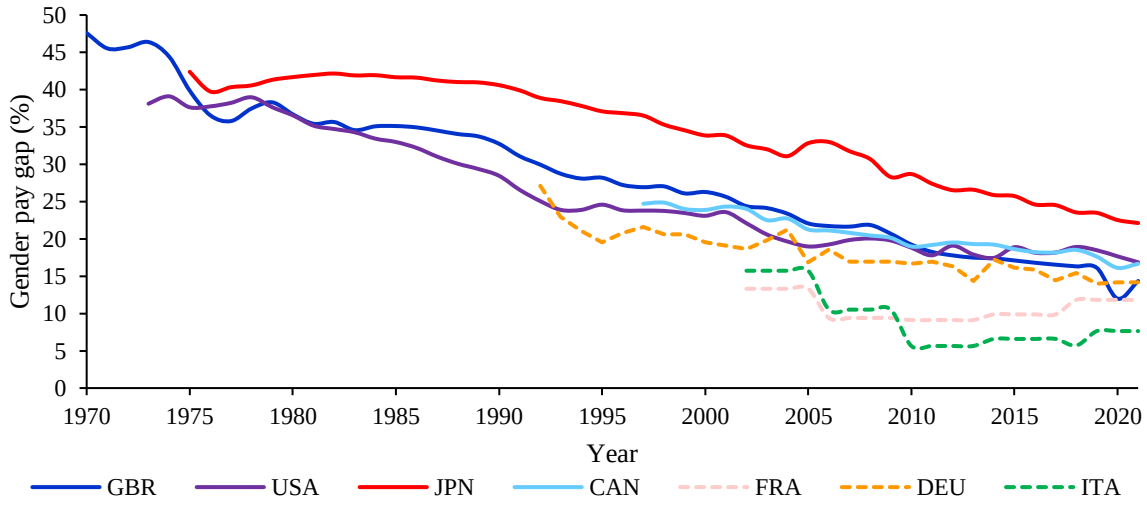
1. Introduction

Despite progress in a growing number of barometers of gender equality over recent decades, the gender pay gap remains a persistent and innate feature of labour markets. Proposals to address its existence have permeated academic and public domains alike, yet a consensus view on policy imposition has yet to form. This paper assesses wage transparency as a mechanism to reduce the gender wage gap by looking at the implementation of UK legislation.

FIGURE 1
Evolution of the gender pay gap (difference in average hourly earnings between males and females) across G7 countries



Source: OECD, 2022

In 2017, the UK government passed new wage transparency legislation mandating private and public firms with more than 250 employees to publicly report their gender pay gap (*The Equality Act 2010 (Gender Pay Gap Information) Regulations 2017)*. Following similar policy iterations elsewhere, and a new EU directive harmonising mandatory pay gap reporting across member states (European Commission, 2022), research into the UK policy's effectiveness is particularly timely.

Policy advocates sustain that transparency holds employers publicly accountable for their wage gaps (Burd et al., 2021) and promotes increased female bargaining power (International Labour Oganization, 2022). Opponents, however, refute the policy's ability to induce action and contest that imperatives to address unequal pay distributions are insufficient without accompanying targets (Cowper-Coles et al., 2021). This paper expects that the policy will be effective at reducing the wage differential.

The financial services industry's synonymity with inequality makes it a compelling setting to assess the legislation. The industry is characterised by longstanding discrimination (Metcalf and Rolfe, 2009; Mcdowell, 2010), poor female representation amongst upper management (Croxson et al., 2019), and disproportionate gender remuneration (Birindelli and Iannuzzi, 2022). In the context of multifaceted gender inequality, this paper adds to a relatively unexplored strand of literature documenting the effectiveness of mandatory pay gap reporting.

2. Literature Review

While myriad explanations for the wage gap exist[1], this section retains focus on determinants with particular relevance to this paper.

Preferences to risk and competition are widely acknowledged mechanisms underpinning the gender pay gap (Croson and Gneezy, 2009). Both field and laboratory settings have uncovered that women are more risk averse (Eckel and Grossman, 2008; Charness and Gneezy, 2012), and less willing to engage in competition than males (Niederle and Vesterlund, 2007; Ors et al., 2013). Bertrand (2011) acknowledges that higher wage occupations are often found in competitive environments, while Bonin et al. (2007) suggest

[1] See Goldin (2014) and Blau and Kahn (2017).

risk-averse individuals self-select into industries with stable and lower earnings. Taken together, these findings translate to financial services and its inherent association with risk and competition (Laeven and Levine, 2009).

Willingness to negotiate is another constituent of the earnings gap with pertinence to the UK legislation. Leibbrandt and List (2015) depict bargaining through the lens of job advertisements and found women were 2.4 percentage points less likely to negotiate salaries, while Hall and Krueger (2012) also showcased that women negotiated 50% less often. These studies align with earlier findings where 57% of males bargained for higher compensation compared to 7% of females (Babcock et al., 2003). The UK policy's ability to overcome this gender ask gap (Roussille, 2020), perhaps by raising female salary expectations (Kiessling et al., 2021), will underscore its effectiveness.

This paper also relates to literature on the implications of pay transparency. Leveraging a California wage transparency mandate (explored by Card et al. (2012)), Mas (2017) found that public aversion to higher earnings drove a significant reduction in wages. With public accountability embedded into the UK law's framework (Government Equalities Office, 2017), this finding is explicitly reassuring for policymakers. A small body of research explores the unmediated impact of transparency on the wage gap. Using legislation in Canada that made salaries of university faculty publicly accessible, Baker et al. (2019) documented a 20-40% reduction in wage differentials.

A handful of papers assess policies centred on combatting wage gaps. Gulyas et al. (2021) and Böheim and Gust (2022) analysed a policy iteration in Austria, concluding that mandatory pay gap reporting failed to generate a discernible impact on the wage gap. Subtly, however, reputational risk is less apparent with the Austrian policy manifestation compared to the UK, since publication of statistics was limited to internal employees. This distinction may explain the contrasting findings in this paper.

Blundell (2020) analysed firms' responsiveness to the UK legislation and found a statistically significant reduction in the wage gap post-implementation. While Blundell's paper assesses the aggregate impact, this paper uncovers the policy's effectiveness where gender barriers are particularly pronounced (Adams and Ragunathan, 2017).

3. Data Analysis

A. Data

This paper utilises data from the Annual Population Survey (APS) to obtain a representation of British employment, wages, education and other social variables. The dataset comprises yearly iterations spanning April to March between 2012-2021 which, given the legislation was enacted on 6th April 2017, affords the ability to bisect the data into pre- and post-policy periods.

Upholding consistency with the UK law's reporting requirements, hourly wages constitute the dependent variable and are deflated to 2015 prices (ONS, 2022). Analysis is limited to full-time financial services professionals according to SOC2020 classifications (ONS, 2021). Summary statistics and raw gender pay gaps are presented in Tables 1 and 2 respectively.

Importantly, the APS includes a variable that identifies the number of employees in the firm where each individual works, allowing us to adopt the triple difference-in-difference (DDD) specification outlined below.

The APS, however, cannot be consolidated alongside richer firm-level controls, meaning results may be biased by firm size-wage premiums (Brown and Medoff, 1989; Idson and Oi, 1999). Equally, controlling for individual firm (and firm X worker) fixed effects in the framework would have provided a depiction of how the law affected a single worker

within the same employment tenure at a specific firm. Subsequently, the potential that positive female outcomes may be driven by treated firms altering the composition of their workforce cannot be eliminated; for example, if they fired lower-paid or hired higher-paid females.

B. Empirical Strategy

Leveraging the requirement that pay gap reporting only applies to firms with over 250 employees, individuals are naturally allocated into treatment (control) groups based on whether they were employed in firms with more (fewer) than 250 employees and were exposed to mandatory pay gap reporting. A DDD regression framework uncovering the differential impact of the legislation on female wages (δ) is therefore adopted:

$$\begin{aligned}\log(wage_{it}) = \beta_0 + \beta_1 Over250_i + \beta_2 Female_i + \beta_3 Post2017_t + \beta_4 Over250_i * Female_i \\ + \beta_5 Over250_i * Post2017_t + \beta_6 Female_i * Post2017_t \\ + \delta Over250_i * Female_i * Post2017_t + X_{it} + \alpha_t + \mu_{it} \quad (1)\end{aligned}$$

where i and t reflect individuals and years respectively; $wage_{it}$ is hourly wage as the dependent variable; $Over250_i$ equals 1 for individuals working in firms comprising over 250 employees; $Female_i$ is a gender dummy; $Post2017_t$ equals 1 for post-policy years 2017-2021; X_{it} are worker controls (age, experience, hours worked, % holding a degree, location); and α_t represents year fixed effects.

TABLE 1
Summary statistics

Panel A: By treatment group (2012-2016, pre-policy)

	Treatment		Control		Total	
	Male (1)	Female (2)	Male (3)	Female (4)	Male (5)	Female (6)
Hourly wage (£)	25.01 (12.88)	20.03 (9.67)	21.40 (11.76)	17.43 (8.85)	23.14 (12.44)	18.68 (9.34)
Log(hourly wage)	3.10 (0.49)	2.89 (0.46)	2.92 (0.55)	2.75 (0.48)	3.01 (0.53)	2.82 (0.48)
Yearly wage (£)	36,307 (7,506)	32,996 (8,624)	34,008 (9,006)	30,051 (9,126)	35,113 (8,466)	31,463 (9,009)
Age (years)	40.83 (10.09)	39.72 (10.09)	42.61 (11.31)	41.01 (10.90)	41.75 (10.78)	40.34 (10.54)
Work experience (years)	10.03 (9.53)	9.82 (8.86)	8.35 (8.73)	8.34 (8.51)	9.16 (9.16)	9.05 (8.71)
Hours worked	38.43 (14.39)	34.05 (15.35)	38.47 (14.43)	34.50 (14.47)	38.45 (14.41)	34.28 (14.90)
Degree obtained (%)	0.64 (0.48)	0.58 (0.49)	0.54 (0.49)	0.49 (0.50)	0.59 (0.49)	0.53 (0.50)
N	*4,727*	*3,164*	*5,106*	*3,434*	*9,833*	*6,598*

Panel B: By treatment group (2017-2021, post-policy)

	Treatment		Control		Total	
	Male (1)	Female (2)	Male (3)	Female (4)	Male (5)	Female (6)
Hourly wage (£)	24.44 (12.04)	20.55 (10.19)	21.93 (11.98)	17.50 (8.61)	23.14 (12.07)	19.02 (9.55)
Log(hourly wage)	3.08 (0.51)	2.91 (0.48)	2.95 (0.54)	2.76 (0.47)	3.01 (0.53)	2.83 (0.48)
Yearly wage (£)	34,406 (6,536)	31,896 (7,425)	32,709 (7,869)	29,506 (8,253)	33,530 (7,304)	30,697 (7.941)
Age (years)	41.03 (10.43)	40.64 (10.52)	42.82 (11.45)	41.36 (11.32)	41.95 (11.00)	41.00 (10.93)
Work experience (years)	9.34 (9.15)	9.91 (9.41)	8.49 (8.98)	8.66 (8.92)	8.90 (9.07)	9.28 (9.19)
Hours worked	37.51 (14.10)	33.78 (15.10)	37.65 (14.28)	34.38 (14.26)	37.59 (14.20)	34.08 (14.69)
Degree obtained (%)	0.64 (0.48)	0.59 (0.49)	0.56 (0.50)	0.50 (0.50)	0.60 (0.49)	0.55 (0.50)
N	*3,673*	*2,873*	*3,919*	*2,894*	*7,592*	*5,767*

Source: Annual Population Survey, 2012-2021.
Notes: Statistics include mean and standard deviation (in brackets). Sample is restricted to those working full-time in the financial services industry, denoted by SOC codes 242 ('Finance Professionals') and 353 ('Finance Associate Professionals'). Wages in GBP and deflated according to ONS' 2015 price level. Panel A reflects the pre-policy period of 2012-2016, Panel B reflects 2017-2021. Treatment individuals (columns (1) and (2)) incorporate those working in firms with more than 250 employees. Control individuals (columns (3) and (4)) refer to those working in firms with fewer than 250 employees. Columns (5) and (6) refer to the entire sample of full-time financial services professionals.

TABLE 2
Raw gender gaps in earnings before and after implementation of the 2017 UK legislation

Dependent variable: Log(hourly wage)	2012-2016 (1)	2017-2021 (2)	Overall (3)
	Panel A: Treatment group		
Gender wage gap	-0.206*** (0.011)	-0.166*** (0.012)	-0.188*** (0.008)
Male mean wage	*3.099*	*3.079*	*3.091*
Female mean wage	*2.894*	*2.914*	*2.903*
N	*7,891*	*6,546*	*14,437*
	Panel B: Control group		
Gender wage gap	-0.175*** (0.012)	-0.192*** (0.013)	-0.182*** (0.009)
Male mean wage	*2.923*	*2.949*	*2.934*
Female mean wage	*2.748*	*2.757*	*2.752*
N	*8,540*	*6,813*	*15,353*
	Panel C: Entire sample of financial services professionals		
Gender wage gap	-0.190*** (0.008)	-0.177*** (0.009)	-0.184*** (0.006)
Male mean wage	*3.008*	*3.012*	*3.100*
Female mean wage	*2.818*	*2.835*	*2.826*
N	*16,431*	*13,359*	*29,790*

Source: Annual Population Survey, 2012-2021

Notes: Standard errors in brackets. Sample is restricted to those working full-time in the financial services industry. Wage data reflects the log of hourly wages. Treatment individuals (Panel A) refer to those working in firms with more than 250 employees. Control individuals (Panel B) refer to those working in firms with fewer than 250 employees. Panel C reflects the entire restricted sample of those working full-time in the financial services industry.

C. Results

TABLE 3
Univariate test
Change in average wages in the periods before and after implementation of the 2017 UK legislation

Log(hourly wage) difference (2017-21 avg - 2012-16 avg)	Treatment (1)	Control (2)	DD/DD/DDD (3)
Female	0.0198	0.0092	0.0106
			(0.0171)
Male	-0.0198	0.0259	-0.0457***
			(0.0161)
DD/DD/DDD	0.0397**	-0.0167	0.0564**
	(0.0165)	(0.0172)	(0.0238)

Source: Annual Population Survey, 2012-2021
Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors in brackets.
This table presents the results of a univariate test with no controls. Coefficients in the top 2 rows of columns (1) and (2) are calculated by subtracting average wages in the 5-year period preceding the law's imposition (2012-2016) from the average wage in the successive 5-year period (2017-2021) (eq(2)). Treatment individuals refer to those working in firms with more than 250 employees. Control individuals refer to those working in firms with fewer than 250 employees. Column (3) is the difference between columns (1) and (2). The bottom right value of the table is the DDD coefficient and uncovers the differential effect of the law on female wages. Sample is restricted to those working full-time in the financial services industry.

Table 3 documents the univariate test uncovering the main impact of the law. Results represent the difference in average wages for males and females between pre- and post-policy periods, according to whether they worked in firms with more than 250 employees (treatment) or not (control):

$$\hat{\theta}_{g,T} = \overline{\log(\text{wage})}_{2017\text{-}21} - \overline{\log(\text{wage})}_{2012\text{-}16} \quad (2)$$

where g and T index male/female and treatment/control respectively

Column (3) uncovers the difference-in-differences. Between the two timeframes, male wages fell by 4.57pp in treatment relative to control groups at a statistically significant rate, while female wages grew marginally, albeit insignificantly. In the DDD framework, the law

induced a 5.64pp increase in female wages relative to males, which, against the sample's 19% pre-policy wage differential[2], constitutes a 30% reduction in the gap.

TABLE 4
Triple difference-in-difference specifications

	All (1)	Male (2)	Female (3)	All (4)	Male (5)	Female (6)	All (7)
Female	-0.2055*** (0.0116)	-	-	-0.1694*** (0.0110)	-	-	-0.1689*** (0.0110)
Over250	0.1765*** (0.0102)	0.1699*** (0.0101)	0.1333*** (0.0112)	0.1655*** (0.0097)	0.1691*** (0.0101)	0.1322*** (0.0114)	0.1646*** (0.0097)
Over250 × Post	-0.0457*** (0.0154)	-0.0412*** (0.0152)	0.0093 (0.0162)	-0.0407*** (0.0146)	-0.0394*** (0.0152)	0.0121 (0.0162)	-0.0382*** (0.0146)
Female × Post	-0.0167 (0.0166)	-	-	-0.0203 (0.0158)	-	-	-0.0208 (0.0158)
Over250 × Female × Post	0.0564** (0.0238)	-	-	0.0482** (0.0226)	-	-	0.0482** (0.0226)
Controls	×	✓	✓	✓	✓	✓	✓
Year FE	×	×	×	×	✓	✓	✓
N	29,790	17,425	12,365	29,790	17,425	12,365	29,790

Source: Annual Population Survey, 2012-2021.
Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors in brackets.
The table presents the results of the main triple difference-in-difference regression outlined in equation (1). Estimates in the row corresponding to *Over250×Female×Post* reflect the coefficient of interest (δ). Column (1) provides the uncontrolled, univariate DDD specification. Columns (2)-(7) incorporate worker controls comprising age, experience, hours worked, % holding a degree and employment location. Columns (5)-(7) also include year fixed effects to control for economic shocks that may distort our results. Columns (2) and (5) ((3) and (6)) are the results of regressions using only males (females) in the sample to showcase individual impacts on wages in treatment vs control firms over the timeframe. Sample is restricted to those working full-time in the financial services industry.

Columns (1), (4) and (7) in Table 4 present the estimates of the main DDD specifications (eq(1)). Across regressions, results in the bottom row (*Over250×Female×Post*) indicate $\hat{\delta}$ and show the differential impact of the policy on female wages was ubiquitously positive. Coefficients in the row *Over250×Post* indicate this was driven by statistically significant wage reductions of male employees in treatment firms over the timeframe.

[2] Refer to Table 2: Column (1), Row (1), Panel C

Analysed together, the law prompted a significant 4.8-5.6pp increase in female wages compared to males, driven by real wage reductions for treated males. Contextualised within the pre-policy gap of 19%[3], the law invoked a considerable 25-30% reduction in the gender wage differential. Although larger, these findings align with prior literature (Baker et al., 2019; Bennedsen et al., 2020; Blundell, 2020) and correspond with the hypothesis that the law would reduce the wage differential.

Correspondingly, Oaxaca-Blinder decomposition (Table 5) unearths a 5pp reduction in the unexplained portion of the gender differential (Blinder, 1973; Oaxaca, 1973; Altonji and Blank, 1999). While causality cannot be interpreted, this provides another pillar of progression in the presence of mandatory reporting.

Consistent reductions in male wages between treatment and control groups suggest firms respond to wage transparency by holding constant, or reducing, nominal male wages. However, compounding data limitations, wage reductions may be driven by males responding to transparency requirements by lowering working hours amid unchanged hourly pay rates. Equally, firms may offset headline wage reductions by increasing male bonuses. Both outcomes could mitigate the policy's effectiveness.

[3] Refer to Table 2, Panel C: Column (1), Row (1)

TABLE 5
Oaxaca-Blinder decomposition (Blinder, 1973; Oaxaca, 1973) of the gender wage differential

	Females vs males	
	2012	2021
(1) Male average log(hourly wage)	3.018	3.037
(2) Female average log(hourly wage)	2.826	2.916
(3) Log(hourly wage) difference	0.192	0.121
Amount due to		
(4) Explained	0.045	0.025
(5) Unexplained	0.147	0.097
Differences to characteristics		
(6) Age	0.014	0.000
(7) Education	0.021	0.000
(8) Experience	0.000	-0.002
(9) Hours worked	0.007	0.018
(10) Location	0.003	0.008
Differences to parameters		
(11) Age	0.185	0.283
(12) Education	-0.033	-0.004
(13) Experience	-0.005	-0.029
(14) Hours worked	-0.062	-0.033
(15) Location	0.002	-0.014
(16) Intercept	0.060	-0.107

Source: Annual Population Survey, 2012 & 2021.
Notes: Based on regressions using 2012 and 2021 iterations of the data. Sample is restricted to those working full-time in the financial services industry.

4. Conclusion

Leveraging an idiosyncrasy in reporting requirements of UK legislation, the DDD specification adopted in this paper suggests that mandatory pay gap reporting reduced the wage differential by between 4.8-5.6 percentage points, primarily by reducing wages of males working in firms where reporting became mandatory. This represents a 25-30% reduction in the gender wage gap based on the pre-policy gender wage differential. Findings suggest that firms respond to mandatory pay reporting by reducing male wages.

Caveating the analysis is the potential that firms responded to the law by reducing hourly wages while increasing male bonuses, or that lower male wages merely reflected lower working hours. Data limitations also prevent an understanding of the law's effect within a given worker's employment tenure at the same firm. Moreover, excluding part-time workers neglects a key determinant of the wage gap (Bardasi and Gornick, 2008; Fernandez-Kranz and Rodríguez-Planas, 2011), and limits to those with strong labour market attachment.

In sum, however, the implementation of mandatory pay gap reporting has been effective at substantially reducing the gender wage differential in the UK financial services sector. The results hold considerable implications regarding potential policy extensions. Given its effectiveness amid pronounced inequality, the findings suggest installation of similar mechanisms in other countries or settings of inequality (notably racial (Brynin and Güveli, 2012)) may be equally viable.

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