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IZA DP No. 15817

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ISSN: 2365-9793

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ABSTRACT

The Ongoing Impact of Gender Pay Gap Transparency Legislation*

This paper examines the ongoing impact of gender pay gap transparency legislation using a sudden COVID-19-induced temporary suspension to legislation in the UK. Compared to organisations that did not report during the suspension year, reporting organisations have a 6% lower gender pay gap a year later. This is driven by a relative increase in females in the top pay quartile at the same time as rising female concentration in the workforce overall. Further analysis supports the hypothesis that ongoing reporting is most effective in organisations with weaker pre-existing pressures to narrow their gender pay gap through female representation and voice.

JEL Classification: J31, J38, J78

Keywords: gender pay gap, pay transparency, equality legislation, COVID-19

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* Gender Pay Gap Reporting Data is available under the Open Government Licence v3.0.

1. Introduction

Pay transparency policies and legislation have been among the most prominent and controversial contemporary tools aimed at addressing gender pay gaps (hereinafter, GPGs) (see, for example, Gulyas *et al*., forthcoming). Recent evidence has suggested the introduction of such legislation has served to narrow the GPG in countries including the UK (Blundell, 2021 and Duchini *et al*., 2020) and Denmark (Bennedsen *et al*., 2022). The mechanisms through which such impacts operate, and the ongoing effect of such reforms have, however, received far less attention. Yet, as Duchini *et al*. (2020) note, quantifying the longer-term impact of the legislation is important, particularly in the context of ongoing administrative costs to employers and the government. Moreover, understanding the mechanisms through which transparency impacts the GPG is essential to enhance future policy effectiveness, especially as it extends across countries and other protected characteristics including ethnicity.[1] This paper attempts to address this evidence gap by exploiting a COVID-19 induced unexpected temporary suspension of the UK's GPG reporting requirements in 2019/20 as a quasi-natural experiment from which to evaluate the ongoing annual impact of the reporting requirements on employers GPGs and the mechanisms through which such effects operate.

In 2017, the UK enacted legislation requiring all public and private sector employers with 250 or more employees to report a set of headline aggregate measures of gender equality, including their raw or unadjusted organisational GPG, on an annual basis. On the 24th of March 2020, three years after its introduction, the requirement to report for the 2019/20 financial year was suspended due to COVID-19, but was reinstated for the following year.[2] Since this unexpected announcement came relatively close to the reporting deadline, 2,934 employers (27% of expected reporters) had already reported their GPG for 2019/20. This paper analyses whether the absence of reporting in a single year affected future organisational GPGs by comparing post-suspension GPGs between organisations that did and did not report in 2019/20. In this respect, it measures the ongoing and more enduring annual impact of the legislation, beyond the initial high profile media attention and significant new information associated with its introduction.[3]

Moreover, since at the time of the COVID-19 induced suspension there was exceptionally little media and public attention on employer GPGs, and the partial information resulting from the suspension further made comparisons *between* employers more difficult, we argue our estimate

[1] See https://publications.parliament.uk/pa/cm5802/cmselect/cmwomeq/998/report.html.
[2] See https://www.gov.uk/government/news/employers-do-not-have-to-report-gender-pay-gaps.
[3] Evidence from analysis of Google search terms suggests that interest at the initial reporting was more than four times that at any point subsequently and more than 14 times that in March 2020 when the legislation was suspended (see Appendix Figure A.1).

predominately reflects other potential mechanisms through which the legislation operates. In particular, we explore the value of the legislation in providing new information for the employer and employees, to inform wage negotiations and generate an organisational or managerial response, including in terms of female recruitment and retention.

GPG reporting in 2019/20 is found to decrease the mean organisational hourly GPG in 2021/22 by around 0.8 percentage points (or 6%). In the context of the suspension, this suggests a sizeable year-on-year impact and therefore ongoing benefits of GPG transparency. Moreover, the results are robust to a variety of tests for potential selection determining which organisations reported prior to the suspension and the direct impact of COVID-19 between reporting and non-reporting organisations. We further find evidence that reporting increases the proportion of females in the workforce and the female concentration at the upper end of the wage distribution but without significantly affecting the concentration in the rest of the wage distribution. Finally, we provide suggestive evidence that reporting, at least when measured several years post-transparency, is most effective in organisations where pre-existing characteristics relating to female representation and voice, which might otherwise narrow their GPG, are weaker.

The remainder of the paper is structured as follows. Section 2 provides an overview of the UK legislation and the nature of the suspension, as well as evidence from existing international studies evaluating the impact of the introduction of GPG transparency legislation. Section 3 introduces the employer level GPG reporting data utilised in this analysis. Section 4 outlines the difference-in-differences approach adopted to evaluate the impact of suspension and presents the corresponding findings. Section 5 explores the potential mechanisms through which these effects operate, and Section 6 concludes.

2. Background

UK GPG Transparency Legislation

From April 2017 UK equality legislation required all large employers (defined as 250 or more employees) to report a series of organisational-level gender equality measures, including their mean and median GPG, annually. This information relates to the 'snapshot' date of the 5th of April for private and voluntary sector employers and 31st of March for employers in the public sector. The legislation requires that these figures are reported within a year to the Government Equalities Office (hereinafter, GEO), who make these data publicly available via a central website which facilitates comparisons *between* employers.[4] The policy was designed to provide an incentive for employers

[4] See https://gender-pay-gap.service.gov.uk/.

to narrow GPGs through the provision of new information. This information is available to employers and employees, as well as customers, investors and other external stakeholders, and the media, and facilitated comparisons both *within* and *between* employers. The legislation additionally encouraged employers to provide a narrative to explain, and an action plan to demonstrate how they intend to narrow, their GPG. Throughout for clarity we refer to each annual report in terms of both the year of the snapshot and reporting date (for example, 2017/18 refers to data collected in 2017 which was reported by the same point in 2018). Despite limited sanctions for non-compliance, virtually all employers deemed in the scope of the regulations reported in the first two years (GEO, 2019).

On the 24th of March 2020 the requirement to report for the 2019/20 financial year was suspended due to the COVID-19 crisis.[5] There was no suggestion that this was anything other than a temporary measure, with the requirement to report reinstated the following reporting year (2020/21), albeit with a 6-month extension to the original reporting deadline due to the ongoing impact of the pandemic.[6] The original requirements were reinstated in 2021/22. Since the 2019/20 announcement was unexpected and came relatively close to the reporting deadline, 27% of expected organisations (i.e. those that had reported in 2018/19) had already reported 2019/20 data by this point. Many (35% of expected organisations) also voluntarily opted to report subsequently (including after the usual deadlines) but a significant number (38% of expected organisations) chose not to report in the suspension year.

Evidence on the Impact of GPG Transparency

The introduction of GPG transparency legislation internationally has attracted academic scrutiny, including in the UK, and is related to broader analysis of the implications of pay transparency, including for the GPG (see for example, Baker *et al*., forthcoming). Studies have typically applied a difference-in-differences approach to employee level data to provide an assessment of the impact of legislation by comparing organisations based on eligibility as defined by employment size. In the UK, Blundell (2021) finds that the introduction of reporting requirements led to a 1.6 percentage-point (or 19%) narrowing of the GPG among workers in-scope of the legislation. This stemmed from a decline in male rather than an increase in female wages and did not reflect changes in the workforce composition. He proposes that the information revelation element of the legislation facilitated by media attention, combined with worker preferences for low GPG employers acted to

[5] See footnote 2.
[6] See https://www.gov.uk/guidance/gender-pay-gap-reporting-changes-to-enforcement.

close organisational GPGs.[7] Importantly, however, the impact of the legislation is found to be similar in 2018 and 2019, leading to questions as to whether the policy has had additional benefits in subsequent years. Duchini *et al*. (2020) adopt a similar approach but focus on male and female wages separately and model the probability of females being hired in above-median-wage occupations. Consistent with Blundell (2021) they find evidence that the UK legislation reduced average male earnings in the first two years. They also find an increase in the probability of females working in above-median-wage occupations, which they attribute to changes in recruitment rather than retention. Focusing on UK higher education, Gamage *et al*. (2020) instead compare the wages of males and females before and after the introduction of University GPG reporting in 2007. Using data from 2004 to 2016, they find evidence that transparency narrowed the GPG, with a more pronounced effect over time. They attribute the narrowing to higher earning female academics negotiating wage increases and female academics moving to institutions with lower GPGs.

While Bennedsen *et al*. (2022) find similar narrowing of the GPG among private sector firms affected by legislation in Denmark, which required employers to disclose to employees detailed occupational wage information by gender, such impacts are not universal internationally. For example, Gulyas *et al*. (forthcoming) and Böheim and Gust (2021) find no evidence of an impact of transparency legislation on the GPG in Austria, where information is not made publicly available. The extent to which such differences in outcomes reflect differences in the nature of the legislation internationally and particularly the distinct UK requirement to make GPGs public rather than available only to employees within the organisation, facilitating external media attention and supporting customer and investor scrutiny, remains an open question but reinforces the importance of understanding the mechanisms through which transparency legislation operates.

In contrast to the existing evidence that focused entirely on the introduction of the legislation and its high profile and dramatic change in public information, we focus on the impact of the requirement to report annually and assess this three years post introduction. Our estimates thereby provide evidence of arguably the more sustainable annual changes resulting from reporting. Moreover, the timing of the 2019/20 reporting (March-April 2020) at the start of the UK's COVID-19-related lockdowns meant that media scrutiny was especially low (see Appendix Figure A.1), disproportionately restricting channels of reputation and consumer/investor preferences. Further, the partial coverage of the 2019/20 data itself limited inter-organisational comparisons. We therefore argue the impact we identify is more likely to reflect the channels of employee bargaining

[7] Consistent with dramatic information revelation on introduction, Duchini *et al*. (2020) provide evidence in relation to the low prevalence of voluntary GPG reporting prior to the legislation.

and institutional response to the annual updated information. Even then, the COVID-19 related nature of the suspension means the GPG is unlikely to have been the focus of employer and/or employee attention to the same extent as in other years, consistent with our estimates being a lower bound of the longer-term annual impact of reporting.

3. GPG Reporting Data

We use publicly available GPG Reporting Data covering the first five years since the introduction of the legislation (2017/18-2021/22).[8] As such, unlike the existing UK evidence based on employee data, our focus is on organisational GPGs as measured by the legislation. The timing of the data collection means that employers should have reported for both 2020/21 (deadline October 2021) and 2021/22 (deadline April 2022). We focus on the mean organisation hourly GPG each year, defined in terms of the percentage of the relevant male average hourly earnings, and based on the census of eligible employees.[9] These measures are calculated based on employer payroll records and are signed off as accurate by a senior manager in the organisation. All measures are standardised across organisations and, in a similar manner to the UK Office for National Statistics' (hereinafter, ONS) preferred measure, the mean GPG is calculated using employees' full basic pay during the reference period (i.e. excluding absences), excluding overtime payments but including performance-related pay. The measure is calculated on a headcount rather than full-time equivalent basis.

We restrict our analysis to organisations in scope of the legislation (as defined by employment size in a given year) and construct an unbalanced panel.[10,11] Mean organisational GPGs can take a positive (favouring men), negative (favouring women) or zero value. In terms of employer characteristics, the Reporting Data contain information on industry (5-digit Standard Industrial Classification (SIC) code), number of employees (banded) and public/private sector.[12] The data also contain administrative information, including a weblink to the employer's GPG figures and action plan (where available). Detailed information is also provided on the date and time of the last report

[8] Available from https://gender-pay-gap.service.gov.uk/viewing/download. The data can be continuously updated by employers and the analysis reflects reporting as of the date of download, 11th of April 2022.
[9] While the median is often thought as a more reliable measure statistically, it has been subject to greater debate about measurement error in terms of organisational GPGs (see Blundell, 2021). However, our findings are not sensitive to this choice (see Section 4).
[10] Specifically, we exclude 1,156 employers with fewer than 250 employees (comprising 1,860 observations), who are likely to be outside the scope of the legislation but use the service to voluntarily report their GPG.
[11] GPG Reports from organisations are matched across reporting years using a GEO provided employer identification variable.
[12] An organisation is defined as being in the public sector if their due date for reporting was listed as 30th of March in any year prior to the suspension.

for each organisation reporting year, as well as whether their initial report was submitted ahead of the relevant deadline.

As the reporting deadline approached in 2017/18 and 2018/19, the number of organisations reporting their GPG increased each day, as shown in Figure 1. A relatively small number of organisations reported after their deadline each year (12% in both 2017/18 and 2018/19). In 2019/20, prior to the 24th of March, the distribution of organisational GPG reports by day followed a similar pattern to the previous two years, consistent with the sudden and unanticipated nature of the suspension. When the reporting suspension was announced 2,934 organisations had reported in comparison to 2,952 by the equivalent date in 2017/18 and 3,343 in 2018/19. After the 24th of March 2020, organisations continued to report their GPG even though they were no longer required to, but in far fewer numbers than in 2017/18 and 2018/19 (see Figure 1).

[Figure 1 here]

We restrict our sample to organisations that reported in 2018/19, in order to exclude (as best as possible) organisations that did not report in 2019/20 because they did not exist or had fewer than 250 employees and hence were out of scope of the legislation.[13] On the basis of the 2019/20 data affected by the temporary suspension we then construct three exhaustive and mutually exclusive groups: those who reported prior to the suspension (reporters), those who reported (voluntarily) after the suspension, and those who did not report (non-reporters).[14] Reporters are defined as having provided GPG information for 2019/20 no later than the 23rd of March. Voluntary reporters are defined as having reported GPG information for 2019/20 on or after 24th of March.[15] Non-reporters are those that did not report their GPG for 2019/20. Our analysis focuses on the change in the difference in GPGs between reporters and non-reporters from before to after the suspension.[16] We exclude the GPG information provided for reporters in 2019/20 given the absence of comparable information for non-reporters.[17] Since the snapshot date for data from 2020/21 was within two weeks of the suspension and so unlikely to be affected by the 2019/20 reporting decision, we consider that to be part of the pre-suspension period.[18] After removing organisations with missing values on any of the variables of interest (see discussion below) we have information on 6,549

[13] Our results are robust to alternatively defining in scope on the basis of any report or a report in 2020/21 (see Section 4).
[14] The latter is defined by the date of data access (11th of April 2022) since it is still possible that organisations can provide information for this reporting year.
[15] In practice since the information available in the GPG Reporting Data relates to the final date of submission a small number of these organisations could be incorrectly classified because they have updated their GPG after the suspension.
[16] Our results are, however, not sensitive to including voluntary reporters (see Section 4).
[17] Our results are, however, not sensitive to including data from 2019/20 (see Section 4).
[18] Our results are, however, not sensitive to excluding data from 2020/21 (see Section 4).

employers (22,939 employer-year observations over the sample period).[19] Of these, 2,784 (43%) were reporters and 3,765 (57%) were non-reporters.

Further details on sample sizes, along with average organisational GPGs for reporters and non-reporters are provided pre- and post-suspension in Table 1. Prior to the suspension the average GPG among reporters is about 0.3 percentage points less than non-reporters but the differential widens to 0.9 percentage points in 2021/22, providing the first indication that reporting during the suspension narrowed organisational GPGs relative to non-reporters.

[Table 1 here]

Figure 2 provides a similar visual inspection by tracing the average organisational GPG for a balanced panel sample of reporters and non-reporters each year.[20] It confirms the similarity in average GPG pre-suspension, which is not significantly different between reporters and non-reporters. However, it points to substantial divergence by 2021/22, with the GPG among reporters narrowing faster than among non-reporters.

[Figure 2 here]

Further details of all explanatory variables and their means are included in Appendix Table A.1 by 2019/20 reporting status. As expected, given the earlier deadline, public sector organisations are over-represented among reporters. Consistent with this public administration, education and health organisations are more common among reporters than are other industry groups. Larger organisations and those who provide an organisation weblink are also more likely to have reported in 2019/20. In what follows, we account for compositional differences between reporters and non-reporters and test the validity of the parallel trends assumption more formally using a regression framework.

4. Impact of the GPG Transparency Suspension

To explore the impact of the temporary suspension, the GPG in organisation *i* at year *t* is regressed on the interaction between a dummy variable for whether organisation *i* reported its mean GPG in 2019/20 ($REPORTER_i$) and an indicator ($\mathbb{I}(\cdot)_t$) for post-suspension year 2021/22 as follows:

$$GPG_{it} = \beta REPORTER_i \times \mathbb{I}(YEAR = 2021/22)_t + \gamma_i + \lambda_t + \varepsilon_{it} \quad (1)$$

[19] Given our fixed effects model, we exclude 194 employers who are observed only once.
[20] The balanced panel includes only those organisations that appeared in the dataset in every year (except 2019/20) eliminating the effects of differences in composition of organisations across reporting years.

Our focus on 2021/22 allows sufficient time for reporting in 2019/20 to impact the organisational GPG.[21] The coefficient of interest, β gives the percentage point difference in the GPG in 2021/22 among organisations who reported their data in 2019/20, relative to organisations who did not report, when compared to reporting years before 2021/22 (excluding 2019/20). The controls for organisation fixed effects (γ_i) capture the influence of underlying organisational characteristics affecting their GPG. The inclusion of year fixed effects (λ_t) controls for trends in the GPG common across organisations.

The results of estimating equation (1) are reported in Table 2 columns (1) and (2), where we exclude and include organisation fixed effects, respectively. In both columns, the estimated value of β implies that the suspension led to a significant widening of the GPG between reporters and non-reporters in 2021/22, relative to earlier years. After accounting for organisational fixed effects, relative to the pre-suspension period, organisations which reported their data during the suspension year (in 2019/20) had a GPG that was 0.80 percentage points smaller post-suspension (in 2021/22) than organisations that did not report. This estimate is equivalent to 0.05 standard deviations of the mean organisational GPG or a 6% reduction. The evidence based on the suspension is thus consistent with the ongoing effectiveness of the legislation. Annual reporting leads to GPG narrowing in the years after transparency is introduced. The 0.8 percentage point effect suggests that a single year of non-reporting might "undo" around half of the reduction in the GPG on the introduction of transparency found by Blundell (2021), albeit based on different data and model specification.

[Table 2 here]

To provide a causal estimate of the impact of annual reporting, the difference-in-differences specification in equation (1) requires that in the absence of the suspension there would have been no systematic difference between reporters and non-reporters in the GPG in 2021/22 relative to previous years. A number of considerations are important here. First, it is important that 'early' reporters are not more pro-gender equality organisations, with a greater commitment to narrowing their GPG year-on-year. To explore the validity of our analysis, we augment equation (1) with leads of the treatment indicator where the impact of reporting during the suspension is allowed to vary each year as follows:

$$GPG_{it} = \sum_{t=2018/19}^{2021/22} \beta_t REPORTER_i \times \mathbb{I}(YEAR = t)_t + \gamma_i + \lambda_t + \varepsilon_{it} \quad (2)$$

[21] In contrast to the March/April 2020 snapshot date in 2020/21.

In equation (2), the coefficient β_t reflects the difference between reporters and non-reporters in reporting year *t*, relative to 2017/18 (the reference year). The parallel-trends assumption can be tested using differences in the GPG between reporters and non-reporters prior to and at the time of the suspension, that is, the coefficients $\beta_{2018/19}$ and $\beta_{2020/21}$ in equation (2). The latter of these refers to a snapshot date less than two weeks after the scheme's suspension, therefore it is effectively a measure of the difference in the GPG when the suspension was announced. Table 2 column (3) presents the full set of year-reporter interaction terms as described in equation (2). The coefficients on all the interaction terms prior to 2021/22 are insignificantly different from zero, consistent with reporters and non-reporters following a common trend up to the suspension of the legislation.[22] Hence, consistent with Figure 2, there is no evidence that prior to the suspension the within-firm GPG among reporters was narrowing faster than among those who chose not to report. In contrast, our estimate of $\beta_{2021/22}$, the difference in GPG between reporters and non-reporters in 2021/22 relative to 2017/18, is -0.86 percentage points.

Second, the GPG in 2020/21 and, to a lesser extent, 2021/22 are potentially directly affected by the impact of COVID-19 and its associated lockdowns. In particular, the UK government's job retention 'furlough' scheme, which guaranteed employees 80% of their normal salary (up to a £2,500 monthly cap), served to maintain employment but directly affected employee wages.[23,24] Should non-reporters disproportionately be those more adversely affected by COVID-19 this might lead to a changing GPG differential relative to reporters in 2021/22 regardless of reporting in 2019/20.[25] Whilst the direction of the bias is unclear, the insignificant estimate of $\beta_{2020/21}$ in Table 2 column (3) provides reassurance as this was not evident in the immediate post-pandemic national lockdown. However, we also re-estimate equation (1) adding interactions of industry (5-digit SIC code) and year dummies to account for the possibility that declines in the GPG have been more

[22] A recent paper by Roth (2022) notes the possibility that this approach might fail to detect a significant pre-trend due to low power which would produce biases in the measured treatment effect. Rambachan and Roth (2022) provide a method to test for this using information on the magnitude of period-to-period deviations observed during the pre-treatment period. Using their approach, our finding of a negative treatment effect is robust to a deviation from the common trend between 2020/21 and 2021/22 that is up to 1.7 times the deviation observed between 2017/18 and 2018/19 (the only two consecutive periods we observe in the pre-treatment period).

[23] The furlough scheme was announced on the 20th of March 2020 and covered the period from 1st of March 2020 to 30th of September 2021, albeit it was subject to a series of changes (including the extent of government subsidy) over this period.

[24] Equally, however, COVID-19 resulted in some (key) workers being in high demand. Importantly our measure of pay excludes overtime pay, which may have increased for these workers.

[25] Organisations which voluntarily 'topped up' workers' wages to 100% would only be affected through changes in workforce composition directly resulting from COVID-19. Those organisations that paid employees 80% should have excluded these workers from their GPG calculations which are based on 'full-pay relevant employees' (for full details of the data requirements see: The gender pay gap data you must gather - GOV.UK (www.gov.uk)). This in itself could have an impact on their GPG through changes in the composition of eligible employees.

rapid in some sectors of the economy than others, including as the direct effect of COVID-19.[26] As seen in Table 2 column (4) our estimate is robust to this, suggesting COVID-19 is not driving the results. We nevertheless further consider whether the findings are driven by specific industries, by performing separate analysis for nine broad industry groupings based on the SIC code in Appendix Table A.2 but find no evidence of a stronger effect for industries (such as distribution, hotels and restaurants, and other services) that are more adversely affected by COVID-19.[27]

A final concern might be spillovers and specifically the possibility that the reported GPGs affect the subsequent behaviour of non-reporters (that is, a violation of Rubin's (1974) stable unit treatment value assumption), for instance, by serving as comparators. Two points seem relevant here. First, since non-reporters have the necessary data, and might have already calculated their GPG prior to the suspension, it is possible that there is an organisational response among non-reporters, despite their GPG never being made public. Such reaction should downward bias our estimate, which is measured as the difference between reporters and non-reporters, given the potential partial response of the latter. Second, should non-reporters experience a negative public or employee reaction due to not reporting *per se* they might feel under greater pressure to subsequently narrow their GPG. Albeit there has been limited media attention on non-reporting, such concerns would also downward bias our estimate, consistent with our estimates having a lower bound interpretation.

We explore the robustness of our initial findings in Table 3. First, we enhance the comparability between organisations in the treatment and control groups. In column (1) we restrict the sample to a narrow reporting window in 2018/19 to include organisations that reported within 10 days of the 24th of March 2019, that is, exactly one year prior to the suspension. We argue these organisations by reporting at a 'similar' time, are more likely to be 'similar' in terms of unobserved characteristics. In columns (2) and (3) we include a control for the number of days and cubic in days reported before the relevant due date each year to capture potentially different GPGs based on the timing of reporting.[28]

In column (4) we retain organisations that reported voluntarily during the suspension (i.e. on or after 24th of March 2020) in the sample. The same pattern emerges with a robust estimate of a

[26] Following GEO (2019), industry is defined using the first SIC code reported within these data. Where a SIC code is not reported by an organisation in a given year, the value reported in the previous year is used. 162 employers had a public sector SIC code '1' and are treated as missing.
[27] See https://www.ons.gov.uk/economy/grossvalueaddedgva/articles/effectsofthecoronaviruscovid19pandemiconhighcontactindustries/2022-05-06 for differences in the impact of COVID-19 by industry.
[28] Appendix Figure A.2 provides the distribution of reporting dates in 2018/19 by reporting status in 2019/20.

0.8 percentage point smaller GPG among reporters relative to non-reporters in 2020/21. The choice to report after the 24th of March 2020 is likely to be endogenous one. Therefore, in column (5) we again include the voluntary reporters in the sample, but this time instrument the interaction between *REPORTER* and 2021/22 dummies using the interaction between a dummy variable for whether a firm reported before 24th of March 2020 (that is, it was not a voluntary reporter) and a 2021/22 year dummy. This approach exploits the fact that the exact timing of the suspension provided an exogenous source of variation in the likelihood of reporting, despite the presence of endogeneity in the decision to report. Our core result is, however, unaffected.

We further find similar evidence of narrowing when using the median rather than the mean organisation GPG (column (6)). The findings are also robust to weighting the estimates by employer size (column (7)), to retaining the partial 2019/20 data reported during the suspension (column (8)) or to excluding the 2020/21 data, which had the snapshot date within two weeks of suspension (column (9)) in our sample.[29] Restricting our sample to a balanced panel to remove organisations with periods of non-reporting and organisations that move in and out of scope (column (10)), or eliminating GPG outliers (column (11)), or focusing exclusively on the private sector (column (12)) further leaves our key result unchanged.[30] We also explore the robustness of our findings to the definition used to determine organisations that are deemed to be in-scope of the legislation in 2019/20 by using GPG reports in 2020/21 and GPG reports during any point in the panel (instead of 2018/19) but our results are not sensitive to this choice (columns (13) and (14), respectively). This extensive set of sensitivity analysis confirms the robustness of our core result both in terms of magnitude as well as sign and statistical significance. Annual reporting narrows organisational GPGs.

[Table 3 here]

5. Mechanisms for the Impact of Suspension

To explore the mechanisms through which the effects of the suspension operate we first consider the extent to which reporting affected additional organisational outcomes, including workforce composition in terms of the percentage of female employees (Bennedsen *et al*., 2022) and female workforce composition at different quartiles of the wage distribution. Unfortunately, our data do not contain information on recruitment or retention from which to distinguish promotions or pay rises among the existing workforce from changes in the GPG through changes in workforce

[29] Since employer size is provided in bands, to weight the data we use the midpoint of each band and 30,000 for the "20,000 or more" band.
[30] Outliers are defined as observations with a GPG smaller than -50% (that is, where men earn 50% less than women) or greater than 50%.

composition directly. Instead, we estimate alternative versions of equation (1), where the dependent variable is the percentage of the workforce that is female within the entire workforce and at given quartiles of the organisational pay distribution.[31]

All else constant, if reporters tend to narrow their GPG by hiring females at above the organisational average female wage (or not retaining above average wage males), then we would expect the overall proportion of women in the workforce to rise at the same time as an increasing female concentration at above median pay quartiles. In contrast, reducing the GPG by increasing the wages of existing female workers would be consistent with no change in overall workforce composition, but a shift in female concentration higher up the wage distribution. Finally, reducing the prevalence of female low paid workers through lower retention (or recruiting more male low wage workers) would instead be consistent with a reduction in female workforce composition, especially at the lower end of the wage distribution.

In Table 4, we consider female workforce composition (column (1)) and female concentration across pay quartiles conditional on overall female workforce composition (columns (2)-(5)). Reporting has a significant positive impact on the concentration of females in the workforce, suggesting a net aggregate impact on recruitment and retention in favour of females. Conditional on female workforce composition, reporting increases the fraction of females in the top pay quartile, without significantly affecting the female percentage further down the wage distribution.[32,33] While it is not possible to identify exactly how such a change operates given the nature of our data, it is consistent with greater pay rises, promotion and/or enhanced seniority of females via recruitment/retention as drivers of the narrowing GPG.

[Table 4 here]

As a further means of exploring the likely drivers of the influence of the suspension we examine heterogeneity in the impact of reporting on the organisational GPG by adding interaction terms in equation (1) which allow the difference-in-differences parameter to vary by key organisational characteristics, as measured prior to the suspension.[34] These include female presence, seniority in the workforce and levels of unionisation as measures of female workers' influence and voice (see Gamage *et al*., 2020; Blundell, 2021 and Baker *et al*., forthcoming), as well as sector and organisational size, and prior dissemination of GPG reports as potential indicators of a pro-equality

[31] The latter are among the organisational measures of gender equality required as part of the legislation.
[32] This is also true if we do not condition on the percentage of the workforce female (results available upon request).
[33] The point estimate is positive for the second quartile (column (3)) and negative for the bottom two quartiles (columns (4) and (5)), but none of these estimates are statistically significant.
[34] Hence, the main effects are absorbed by firm fixed effects.

management and/or reputational pressure. On the one hand, these characteristics might be anticipated to magnify the impact of transparency via enhancing employee bargaining or the managerial response. On the other hand, especially when measured several years post-transparency and in the context of the unexpected temporary nature of the suspension, these factors might also be expected to lead to greater GPG narrowing even in the absence of reporting given such organisations are more likely to have developed longer-term plans. In this respect, these organisations might be less responsive to reporting in a single year or, to put it another way, reporting might be most beneficial for organisations that would otherwise have less focus on their GPG. These estimates are presented in Table 5.

In column (1) we explore the average percentage of the workforce who are female and in column (2) the percentage of females in the highest pay quartile is used as a proxy for female influence (Theodoropoulos *et al*., 2022). We find that the narrowing impact of reporting on the GPG is reduced in organisations with a larger proportion of females (column (1)) and greater female representation in the upper quartile prior to the suspension of the scheme (column (2)). In both cases, the results suggest that in female-dominated organisations, the suspension of the scheme had no effect on GPGs. In contrast, male-dominated organisations appear to behave in a more myopic fashion, paying attention to their GPG if they report in a given year. In column (3) the effect of the suspension is allowed to vary according to the percentage of employees who are members of a union.[35] In a similar manner, the impact of reporting is smallest in industries with the highest union membership rates, potentially reflecting greater female bargaining power and scrutiny of the GPG in unionised workplaces even in the absence of reporting.

In the remaining columns we allow the impact of reporting to vary according to characteristics which proxy organisational equality. In column (4) we use the presence of a weblink to organisational GPG information in a pre-suspension year as a proxy for prior recognition by management, in column (5) we consider differences between the public and private sector given the additional more pro-active requirements of the Public Sector Equality Duty and in column (6) large organisations (defined as ever reporting more than 5,000 employees prior to 2019/20) which might have greater reputational concerns, are distinguished from smaller firms. None of these organisational characteristics significantly influence the effectiveness of reporting. In this respect, our results support the hypothesis that reporting might be most effective in organisations where existing pressures, particularly relating to female representation and voice, are more limited.

[35] These data are available from the Labour Force Survey see: https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment_data/file/988897/Trade_Union_Membership_Statistics_2020_Final.ods. The average between 2016-2020 is used for each 2-digit SIC classification.

[Table 5 here]

6. Conclusion

Studies including Blundell (2021) and Duchini *et al*. (2020) find that the introduction of the GPG Reporting Legislation in the UK had a significant narrowing impact on the GPG. The extent to which this reflects a one-off shock based on new information and/or high-profile initial media, employee, consumer and investor attention is, however, unclear. In this paper, we use a sudden and unanticipated COVID-19-related suspension of the UK GPG reporting as a quasi-natural experiment with which to evaluate the ongoing, year-on-year impact of the GPG reporting legislation and better understand the mechanisms through which this might operate.

Comparing organisations that reported prior to the suspension with those who did not report their GPG in the same year we find evidence that reporting narrows the mean organisational GPG by 0.8 percentage points (or 6%) one year later. We further demonstrate comparability between reporting and non-reporting organisations in terms of trends in their GPG pre-suspension, and the robustness of our estimates to accounting for the direct impact of COVID-19 on their GPGs. Due to the COVID-19 related nature of the suspension we argue our findings can be considered as relatively conservative estimates of the longer-term annual impact of reporting. Our evidence thus suggests a significant additional and ongoing impact of transparency legislation over and above its impact upon introduction. In other words, despite potentially modest new information being provided each year, annual GPG reporting is effective even when future reporting is anticipated. This is clearly critical to inform the ongoing review of GPG legislation, current proposals to extend reporting to smaller organisations and other protected characteristics in the UK and the future design and reform of GPG transparency legislation internationally.

We find that narrowing organisational GPGs arising from reporting is associated with an increasing female presence in the workforce and occurs through increasing the proportion of females in the top pay quartile but without reducing females at the lower end of the pay distribution. Our further analysis suggests that reporting is most beneficial for organisations with lower female presence, influence and bargaining power, consistent with reporting being particularly important in organisations where the GPG is perhaps less well-established on the organisational agenda.

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Figures and Tables

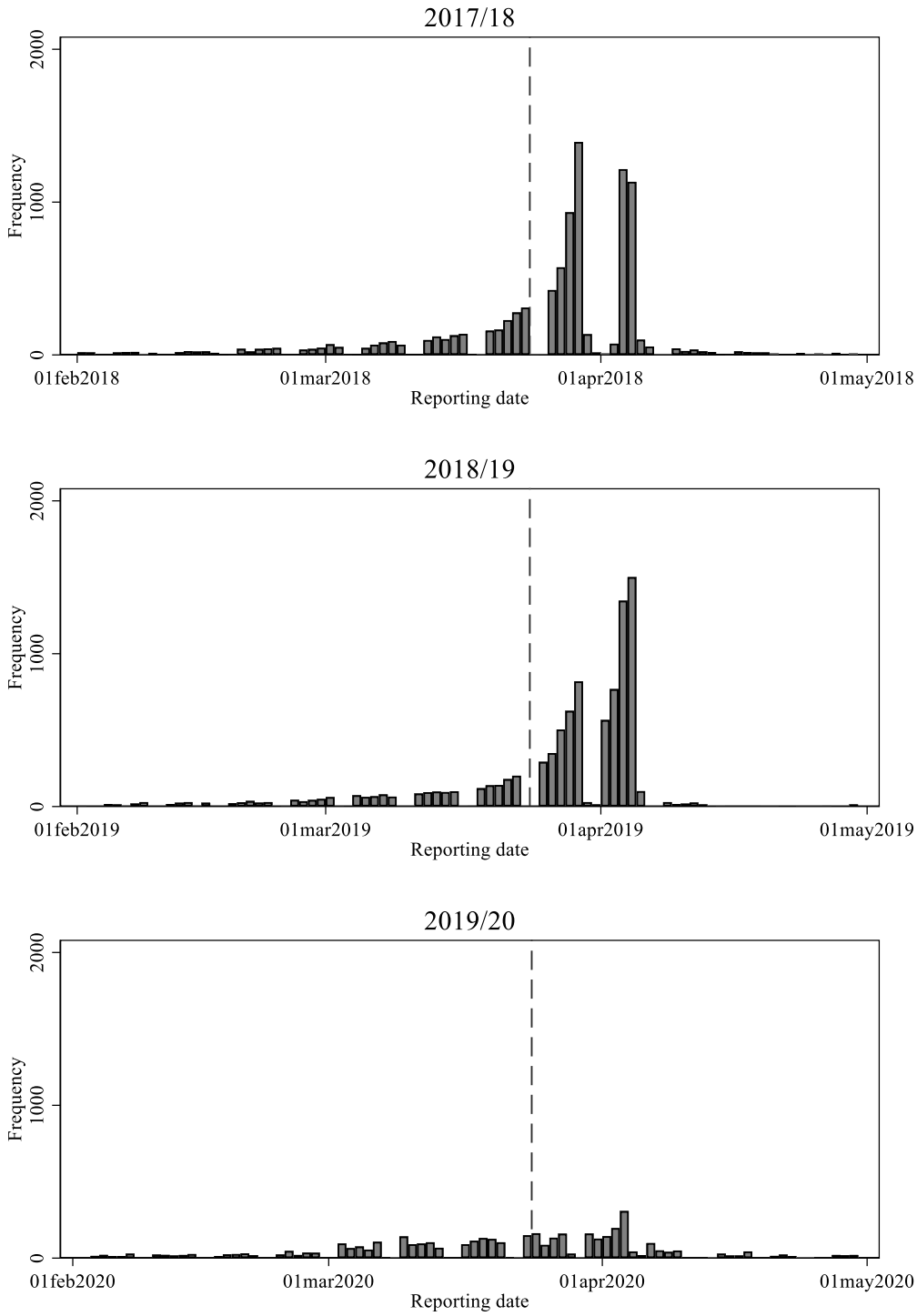


Figure 1: Distribution of reporting dates in 2017/18, 2018/19 and 2019/20

Notes: Authors' calculations based on the UK GPG Reporting Data (downloaded on the 11th of April 2022). The Reporting Data contain final (not necessarily first) reports. The number of organisations is 9,115, 9,295 and 4,203 in 2017/18, 2018/19 and 2019/20, respectively. The dotted lines indicate the 24th of March in each year.

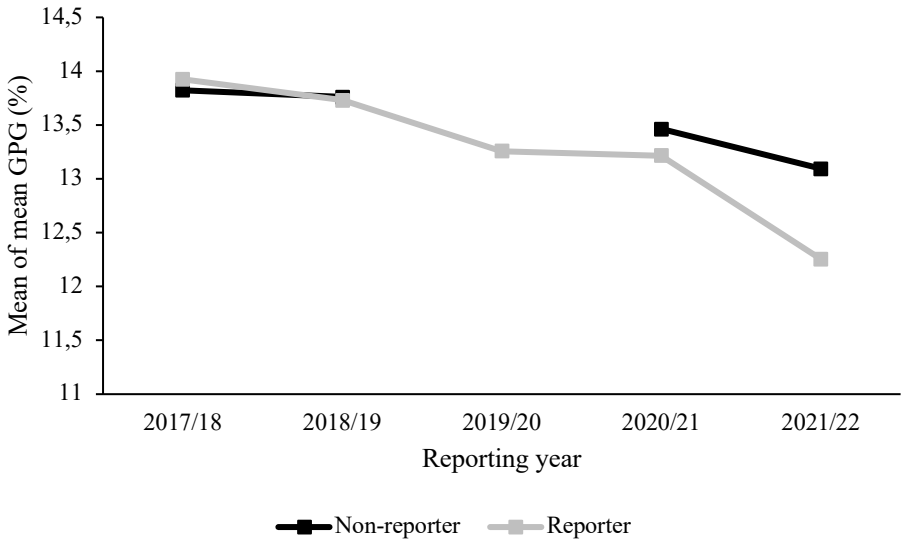


Figure 2: Organisational GPGs by reporting status in 2019/20

Notes: Authors' calculations based on the UK GPG Reporting Data (downloaded on the 11th of April 2022), restricted to organisations that did not report voluntarily in 2019/20, but reported in every year (reporters) or every year except 2019/20 (non-reporters). The number of organisations that did and did not report is 2,136 and 2,212, respectively.

Table 1: Descriptive statistics

	Before suspension (2017/18, 2018/19, 2020/21)			After suspension (2021/22)		
	$REPORTER$	$NON - REPORTER$	Total	$REPORTER$	$NON - REPORTER$	Total
Mean GPG	13.249	13.591	13.440	12.284	13.195	12.763
Number of organisations	2,784	3,765	6,549	2,378	2,641	5,019
Number of observations	7,917	10,003	17,920	2,378	2,641	5,019

Notes: Authors' calculations based on the UK GPG Reporting Data (downloaded on the 11th of April 2022). Sample is restricted to organisations which were observed more than once, with non-missing values on any of the variables of interest (see Appendix Table A.1) and excludes the partial GPG information provided for reporters in 2019/20 and data for all years for voluntary reporters in 2019/20.

Table 2: Difference-in-difference estimates of the impact of reporting in 2019/20 on the organisational GPG

	(1)	(2)	(3)	(4)
$REPORTER$ × 2018/19	-	-	0.177	-
			(0.207)	
$REPORTER$ × 2020/21	-	-	-0.376	-
			(0.312)	
$REPORTER$ × 2021/22	-0.575*	-0.800***	-0.859***	-0.842***
	(0.299)	(0.269)	(0.331)	(0.312)
Organisation fixed effects	No	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	No
Industry × Year fixed effects	No	No	No	Yes
Adjusted R^2	0.000	0.737	0.737	0.729
Number of observations	22,939	22,939	22,939	20,986

Notes: Standard errors are clustered by employer and are presented in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. In column (1) there is a control for $REPORTER$. In column (3) the omitted group is $REPORTER$ × 2017/18. In column (4) the sample is smaller due to missing information on detailed industry codes. Partial data from 2019/20 and data for all years for voluntary reporters in 2019/20 are excluded from the sample.

Table 3: Difference-in-difference estimates of the impact of reporting in 2019/20 on the organisational GPG, sensitivity analysis

	(1) Restricted reporting window	(2) Linear trend in days to deadline	(3) Cubic in days to deadline	(4) Include voluntary reporters	(5) IV	(6) Median GPG	(7) Weight by employer size
$REPORTER \times$ 2021/22	-0.726*	-0.848***	-0.870***	-0.811***	-0.797*	-0.571**	-0.853***
	(0.371)	(0.269)	(0.270)	(0.243)	(0.450)	(0.288)	(0.328)
Adjusted R^2	0.765	0.737	0.737	0.758	0.758[a]	0.726	0.797
Number of observations	10,273	22,939	22,939	36,435	36,435	22,939	22,497
	(8) Include 2019/20 data	(9) Exclude 2020/21 data	(10) Balanced panel	(11) Remove GPG outliers	(12) Private sector	(13) Non-reporter based on 2020/21	(14) Non-reporter based on any year
$REPORTER \times$ 2021/22	-0.753***	-0.915***	-0.783***	-0.669***	-0.952***	-0.787***	-0.801***
	(0.270)	(0.294)	(0.292)	(0.208)	(0.323)	(0.254)	(0.249)
Adjusted R^2	0.723	0.756	0.725	0.784	0.725	0.807	0.740
Number of observations	25,745	17,606	17,392	22,584	18,752	21,551	24,433

Notes: All columns also include employer and year fixed effects. Standard errors are clustered by employer and are presented in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. In column (4) organisations that reported voluntarily during the suspension (i.e. after 24th of March 2020) are retained in the sample and the specification includes an interaction between a dummy for voluntary reporters and a 2021/22 year dummy. In column (5) we instrument $REPORTER \times$ 2021/22 (see text for details). [a]Reported R^2 is from the first stage. In column (10) only organisations with four annual observations (excluding 2019/20) are included. In column (11) observations with a GPG smaller than -50% (that is, where men earn 50% less than women) or greater than 50% are excluded.

Table 4: Difference-in-difference estimates of the impact of reporting in 2019/20, mechanisms

	% Female				
	(1) Overall	(2) Top quartile	(3) Second quartile	(4) Third quartile	(5) Bottom quartile
$REPORTER \times 2021/22$	0.286*	0.307*	0.041	-0.107	-0.241
	(0.148)	(0.175)	(0.172)	(0.189)	(0.209)
Adjusted R^2	0.970	0.955	0.963	0.956	0.939
Number of observations	22,725	22,725	22,725	22,725	22,725

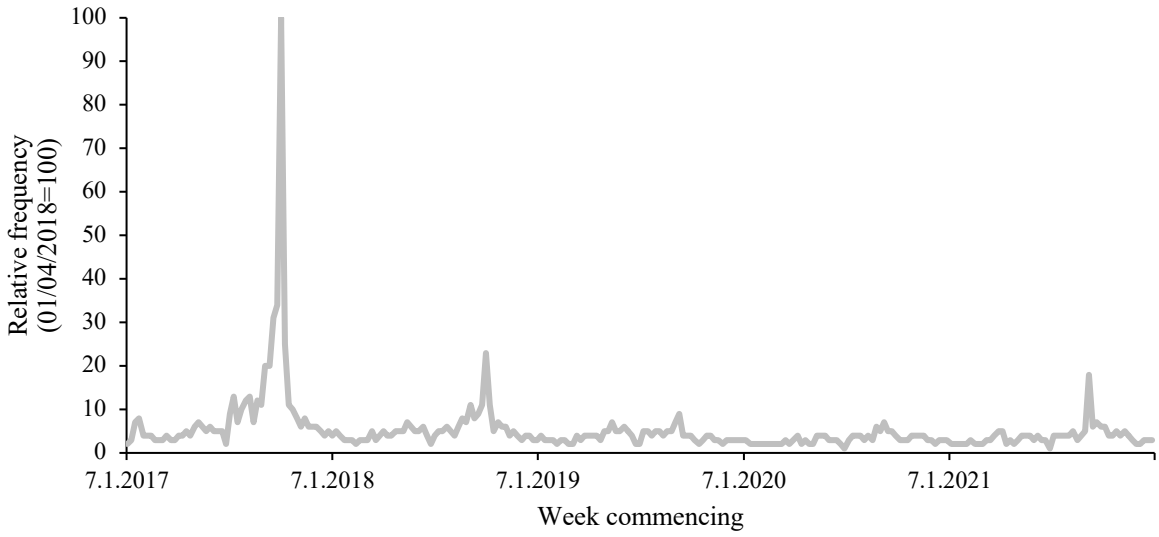
Notes: All columns also include employer and year fixed effects. In the quartile specifications the overall percentage female is also included. Standard errors are clustered by employer and are presented in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. The sample is smaller than Table 2 due to missing information on female workforce composition at different quartiles of the wage distribution (123 observations in 2020/21 and 127 observations in 2021/22).

Table 5: Difference-in-difference estimates of the impact of reporting in 2019/20, heterogeneity analysis

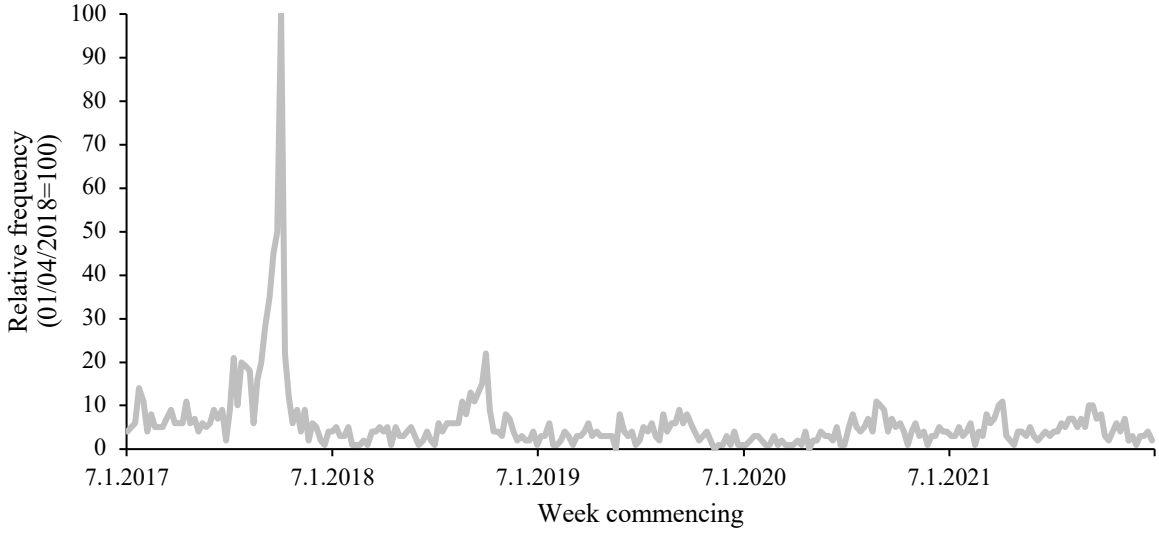
	(1) % Female	(2) % Female top quartile	(3) % Union member	(4) Organisation weblink	(5) Public sector	(6) Large organisations
$REPORTER$ × 2021/22	-1.885***	-2.187***	-1.268***	-1.519***	-0.921***	-0.826***
	(0.474)	(0.412)	(0.442)	(0.593)	(0.306)	(0.277)
$REPORTER$ × 2021/22 × % female	0.021***					
	(0.007)					
$REPORTER$ × 2021/22 × % female top quartile		0.032***				
		(0.006)				
$REPORTER$ × 2021/22 × % union member			0.016*			
			(0.009)			
$REPORTER$ × 2021/22 × organisation weblink				0.810		
				(0.575)		
$REPORTER$ × 2021/22 × public sector					0.402	
					(0.267)	
$REPORTER$ × 2021/22 × large organisation						0.272
						(0.379)
Adjusted R^2	0.737	0.737	0.734	0.737	0.737	0.737
Number of observations	22,939	22,939	20,244	22,939	22,939	22,935

Notes: All columns also include employer and year fixed effects. Standard errors are clustered by employer and are presented in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. In columns (3) and (6) the samples are smaller due to missing information on the percentage of employees who are members of a union (or industry which is used to link the percentage of employees who are members of a union) and firm size, respectively.

Appendix: The ongoing impact of gender pay gap transparency legislation



a. Searches for "gender gap"



b. Searches for "gender gap reporting"

Figure A.1: Frequency of Google search terms over time

Notes: Weekly search data were downloaded from Google Trends (on the 5th of July 2022). The data are provided in rescaled form so that the largest value in the sample period has the value 100. The frequency of searches for "gender pay gap" and "gender pay gap reporting" is almost identical to the values shown here (available upon request).

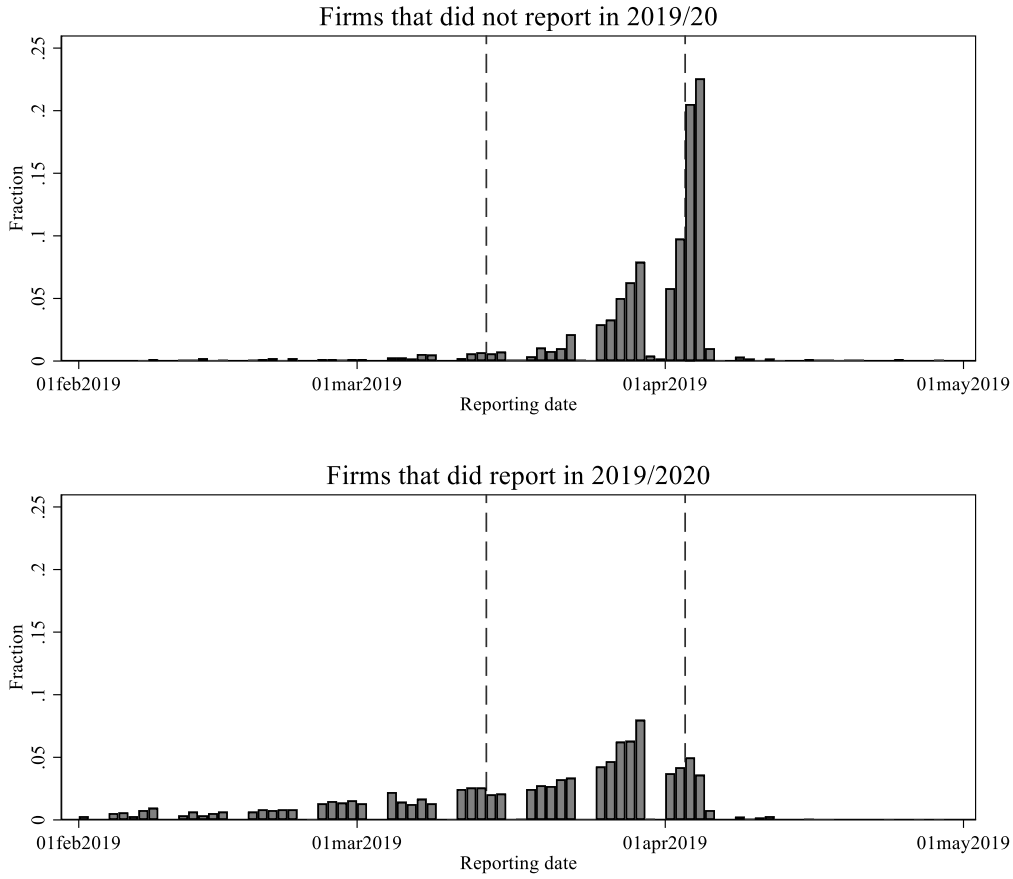


Figure A.2: Distribution of reporting dates in 2018/19 by reporting status in 2019/20

Notes: Authors' calculations based on the UK GPG Reporting Data (downloaded on the 11th of April 2022), restricted to organisations that did not report voluntarily in 2019/20, but reported in every year (reporters) or every year except 2019/20 (non-reporters). The Reporting Data contain final (not necessarily first) reports. The number of organisations that did and did not report is 1,664 and 2,148, respectively. The dotted lines indicate the 14th of March and 3rd of April 2019, that is, 10 days either side of 24th of March 2019.

Table A.1: Sample means

	Before suspension (2017/18, 2018/19, 2020/21)		After suspension (2021/22)	
	$REPORTER$	$NON - REPORTER$	$REPORTER$	$NON - REPORTER$
Mean GPG (%)	13.25	13.59	12.28	13.19
Median GPG (%)	12.06	10.57	12.00	10.87
% Female	50.21	46.64	51.28	46.83
1st quartile	56.62	52.50	57.66	52.88
2nd quartile	52.86	49.08	53.73	49.03
3rd quartile	48.50	45.23	49.48	45.20
4th quartile	42.84	39.74	44.25	40.25
Organisation weblink (%)	73.32	61.54	68.67	56.53
Days to deadline	61.94	8.01	49.44	20.24
Industry (SIC 2007) (%)				
Agriculture, forestry and fishing	0.65	0.43	0.62	0.51
Energy and water	1.88	1.64	1.88	1.53
Manufacturing	13.09	14.69	12.59	14.22
Construction	3.21	3.09	2.82	2.86
Distribution, hotels and restaurants	12.65	21.40	12.06	20.49
Transport and communication	6.02	8.78	5.78	9.05
Banking and finance	21.27	24.04	20.08	22.76
Public administration, education and health	36.87	20.13	40.97	23.46
Other services	4.36	5.79	3.18	5.13
Union member (%)	25.99	19.35	27.53	20.44
Public sector (%)	28.00	9.63	30.19	10.94
Employment size (number of employees) (%)				
250-499	43.83	50.26	39.78	45.13
500-999	24.24	26.68	24.98	28.74
1,000-4,999	22.46	18.57	24.22	20.22
5,000-19,999	6.15	2.91	7.06	3.07
20,000+	1.19	0.34	1.35	0.45
Number of organisations	2,784	3,765	2,378	2,641
Number of observations	7,917	10,003	2,378	2,641

Notes: Authors' calculations based on the UK GPG Reporting Data (downloaded on the 11th of April 2022). Sample is restricted to organisations which were observed more than once, with non-missing values on any of the variables of interest and excludes the partial GPG information provided for reporters in 2019/20 and data for all years for voluntary reporters in 2019/20. Descriptive statistics are presented for aggregate industry (regrouped SIC sections) for conciseness, but more detailed classifications are available upon request.

Table A.2: Difference-in-difference estimates of the impact of reporting in 2019/20 on the organisational GPG, by industry

	Industry								
	Agriculture, forestry and fishing	Energy and water	Manufacturing	Construction	Distribution, hotels and restaurants	Transport and communication	Banking and finance	Public administration, education and health	Other services
REPORTER × 2021/22	-0.432 (1.883)	-1.171 (1.527)	-1.282* (0.706)	-2.404 (1.538)	-0.317 (0.770)	0.511 (0.896)	-1.643** (0.688)	-0.781 (0.487)	-1.046 (1.578)
Organisation fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × year fixed effects	No	No	No	No	No	No	No	No	No
Adjusted R^2	0.801	0.874	0.667	0.753	0.575	0.792	0.764	0.708	0.876
Number of observations	115	374	2,985	658	3,738	1,630	4,837	6,072	1,060

Notes: Standard errors are clustered by employer and are presented in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. Industry is measured by the (regrouped) SIC sections.