

# Do Consumers Care About Gender Pay Gap Disclosure? Evidence from Foot Traffic

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## Abstract

We investigate whether the disclosure of gender pay information leads consumers to change their purchasing decisions. Using the U.K.'s 2017 mandatory gender pay disclosure regulation, combined with high-frequency foot traffic data to individual retail outlets, we show that foot traffic increases to U.S. outlets of firms required to disclose gender pay practices. Consumers reduce visits to firms reporting the highest gaps and reward firms reporting the lowest gaps with more visits, even if the company reports a pay gap unfavorable to women. Local demographic or workforce characteristics like the relative number of women or women in the labor force do not strengthen this baseline effect. Instead, increases in foot traffic are larger in localities with social norms favoring gender equality. Localities with well-educated consumers are more likely to be skeptical of firms reporting low pay gaps and “see through” misreported data. These foot traffic patterns post-disclosure affect managerial decision-making. We document a feedback loop where changes to consumer behavior following disclosure impact firms’ future pay practices. Abnormally low foot traffic is predictive of smaller future pay gaps. Collectively, these findings indicate that transparency and equity in pay distributions can serve as a differentiating factor in a firm's brand image. Likewise, our findings demonstrate the usefulness of regulations mandating disclosure of gender pay gaps in providing customers with information relevant to their decision-making.

Keywords: Gender Pay; Pay Gap; Pay Inequality; Consumer Response; Purchasing Decisions; ESG; ESG Disclosure;

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## 1. Introduction

Brands and social values have become inseparable in the eyes of consumers. Two in three consumers now state that they would choose or switch between brands based on the brand's stance on societal issues (Edelman 2018). Among the values and beliefs important to a majority of consumers is a firm's commitment to equitable gender pay. A similar proportion of consumers report being more likely to support firms with equality and transparency in their pay practices (Barton et al. 2018). Experimental evidence supports these statements, demonstrating that consumers' preferences shift when information about gender pay gaps is easily accessible in the lab (Schlager et al. 2021).

Consumers, however, often do not “put their money where their mouths are” when it comes to purchasing decisions (Prothero et al. 2011).<sup>1</sup> Numerous factors — ranging from convenience (Seiders et al. 2005, Johnstone and Tan 2015), price (Eckhardt et al. 2010), availability of and trust in relevant information (Andrews et al. 1998, Erdem and Swait 1998), choice paralysis (Iyengar and Lepper 2000), and habitual purchasing patterns (Wood and Neal 2009) — contribute to this disconnect between beliefs and actions. Likewise, information from corporate disclosures may be costly to incorporate into decision-making. On the other hand, evidence suggests that at least some consumers are willing to pay for socially responsible products (Arora and Henderson 2007, Pigors and Rockenbach 2016).

Given the mixed evidence of consumers' ability to translate personal preferences into economic actions (Eckhardt et al. 2010, Servaes and Tamayo 2013), we study if gender pay disclosure leads consumers to alter real-world purchasing decisions. To shed light on this issue, we examine consumer behavior following the U.K.'s 2017 gender pay disclosure regulation. This regulation required all firms with over 250 employees that operate in the U.K. to disclose the difference in the mean hourly rate of pay for men and women employees, among other statistics. The U.K.'s Government Equalities Office published this information on a website tailored for access by the general public.

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<sup>1</sup> For a summary of the relationship between purchase intentions and future sales, see Morwitz (2014).

Consumers must be aware of a firm’s activities to make informed purchasing decisions (Mohr et al. 2001, Servaes and Tamayo 2013). The release of this data received significant public attention, not just in the U.K., but worldwide. Figure 1 shows that Google search volume for terms including “U.K. gender pay” and “U.K. gender pay gap” peaked in April 2018, the month the U.K. pay gap database was first published online, both in the United States (Panel A) and globally (Panel B). Thus, it seems likely that consumers, not just in the U.K., but worldwide, utilized this shock to gender pay information to gain insights into global corporations’ pay practices that were previously undisclosed or unverifiable.

We utilize foot traffic data to 278,324 outlets of 2,281 unique firms captured through mobile phone geolocation to analyze the changes in customer visits to individual outlets of firms required to disclose. Specifically, we focus on the U.S. outlets of U.S. firms required to disclose due to existing operations in the U.K. This allows us to use an extremely strict set of fixed effects in a difference-in-differences estimation to compare the traffic of disclosing outlets to the traffic of non-disclosing competitors in the same industry, in the same U.S. locality, in the same month, before and after disclosure. The granularity of this data allows us to compare, for example, the difference in foot traffic to a Starbucks in the West Village of Manhattan in the month after mandatory disclosure to that of another popular local coffee chain in the same neighborhood that was not required to disclose. Additionally, we can utilize this framework to compare how differences in local characteristics moderate outcomes post-disclosure across Starbucks outlets in different places across the U.S.

We begin by taking an agnostic view of the content of firms’ disclosures and test how disclosure, in general, affects traffic to disclosing firms. Consumers may reward firms with transparency around social issues, as transparency may build trust. Thus, verifiable and credible disclosure from the U.K. mandate might have a positive average effect across firms required to disclose relative to peers that do not disclose. However, the average firm in the sample reported a mean pay gap of over 11 percent between men and women in 2018 — a number that those incorporating pay gap information into purchasing decisions would likely perceive as unfair. Thus, how consumers might react when mandatory disclosure reveals information about a firm’s pay practices is not obvious.

We find that outlets of firms required to be more transparent experience higher volumes of both visits and visitors following disclosure.<sup>2</sup> The counts of unique visits and visitors increase by approximately one percent compared to local non-disclosing competitors. This positive effect implies that consumers might not be only “punishing with their wallets,” as approximately 90% of firms reported a pay gap unfavorable to women. Instead, the results suggest that consumers value the disclosure itself.

To investigate this notion further, we test if consumers’ purchasing decisions depend on the gap size reported. We show that consumers not only “punish with their wallets” by avoiding firms publishing the most significant pay gaps but also reward the firms with the smallest pay gaps with increased traffic. The negative effect of publishing a large gap is nearly twice the magnitude of the baseline positive effect of transparent disclosure. Thus, disclosure is associated with a negative impact overall for firms with the highest gaps. Conversely, the positive effect of disclosure on foot traffic for firms with the lowest gap is nearly double that of other disclosing firms. Although some firms in this category report null or even negative gaps, this positive effect of reporting a low gap even extends to firms that report a low gap that is still unfavorable to women. Thus, consumers value both the transparency and the content of these disclosures. These results from the field are novel, as they contrast with prior results from the lab that document a strictly negative reaction to pay gap disclosure (Schlager et al. 2021), demonstrating that disclosure can be valuable to consumers and firms.

An additional advantage of this empirical setting is that we can test for moderating effects over a wide range of local characteristics. Prior research has shown that women are more likely to perceive wage gaps as unfair, which lowers their willingness to pay (Schlager et al. 2021). This experimental evidence leads us to hypothesize that a higher local proportion of women, especially women in the workforce, should intensify the baseline results. We find that this is not the case. Instead, we show that these local

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<sup>2</sup> Outlet-level foot traffic from cell phone data has been previously shown to be an excellent predictor of firm-wide sales and thus is a valid proxy for purchasing decisions. Von Meyerinck et al. (2024) show that aggregate customer visits are strongly predictive of quarterly revenues within a similar sample of firms. Likewise, Gurun et al. (2023) document that the correlation between quarterly change in cellphone visitors and quarterly comparable store sales growth is 0.85 for U.S. Starbucks locations.

demographics do not moderate the effect but rather local attitudes toward women's equality. Foot traffic is higher for disclosing outlets across geographies. However, the effect is more significant where gender equality is higher, as measured by both the Gender Equality Index from Di Noia (2002) and the Georgetown Institute's Women's Peace and Safety Index (Klugman et al. 2020). These results are consistent with customers' social preferences for gender equality driving the changes in foot traffic.

Two final sets of analyses demonstrate that well-educated consumers can "see through" misreported data and that reduced traffic post-disclosure is associated with reporting smaller pay gaps in the future. Thus, we document that consumer reactions to disclosure affect firm behavior.

In the remainder of the paper, we first highlight the related literature about consumer purchasing decisions in business-to-consumer settings. Next, we summarize the U.K.'s 2017 Pay Disclosure Regulation and our data. We then describe our research design and present the results of our analysis. Finally, we provide a discussion of the managerial and public policy implications of the results.

## **2. Related Literature and Research Contribution**

Existing literature shows that consumers alter purchasing intentions based on social issues when doing so is convenient and information on such matters is available. Consumers alter their purchasing intentions after learning about firms' pay practices (Hill and Lloyd 2024). Consumer purchase intentions also decrease for firms with high pay ratios between CEOs and employees (Mohan et al. 2018) and, most relatedly, high gender pay gaps (Schlager et al. 2021).

However, there is often a disconnect between consumers' purchase intentions and their eventual purchase decisions (Prothero et al. 2011, Johnstone and Tan 2015, White et al. 2019). This tendency is also likely true in the case of gender pay disclosure. Highlighting this notion, Schlager et al. (2021) find that consumers do not factor preferences for gender pay equality into their purchase intentions when faced with a hypothetical inconvenience in the lab.

Outside the lab, consumers intending to align their values with purchasing decisions face an even more complex set of choices. They must not only process how one firm or product compares to all potential

other choices on a single dimension (e.g., gender pay) but also weigh other different characteristics they may deem important against each other both within and across firms (e.g., gender pay versus ethical supply chain sourcing). Fully processing and factoring in this information is costly and rarely convenient. Given the complexity of these choices, it is unclear whether information about gender pay gaps will factor into the real-world purchasing decisions of the average consumer. The primary contribution of this study is to use high-frequency foot traffic data to show how consumers translate information on gender pay disclosure that is available but not costless into real-world purchasing decisions.

Our findings provide insights into consumers' response to gender pay gap disclosure and highlight the importance of this issue in shaping consumer behavior. In this regard, we contribute to a nascent body of literature that uses large empirical datasets to show how consumers respond to information about the environmental, social, or governance policies of the firm (Houston et al. 2022, Meier et al. 2022, Duan et al. 2023). While these contemporaneous studies look at consumers' reactions to "ESG Incidents" or ESG ratings, broadly defined and measured, we focus specifically on responses to information revealed about this specific social issue — one deemed as important by consumers and the subject of a set of international disclosure rules.

Moreover, this paper adds to the literature on how gender pay disclosure affects the firm's future pay practices. Mandatory disclosure can significantly affect employee pay and pay inequality (Blundell et al. 2023), employee job satisfaction (Sharkey et al. 2022), and hiring policies (Bennedsen et al. 2022, Blundell et al. 2023). We demonstrate that consumers alter purchasing decisions in response to mandatory disclosures, which has real effects on future pay practices. Thus, our study uncovers an additional mechanism through which pay disclosure can affect employee welfare.

### **3. Data and Methodology**

#### **3.1. Background on the U.K.'s 2017 Pay Disclosure Regulation**

The passage of the U.K. Equality Act 2010 strengthened and modernized several existing anti-discrimination laws in the U.K. This broad legislative overhaul led to the adoption of the Gender Pay Gap

Information Regulations 2017. This regulation came into force in April 2017. Since April 2018, employers in the U.K. with more than 250 employees in the private, non-profit, and government sectors have reported comparative gender pay information annually on both the employer’s website and a public website hosted by the U.K. government.<sup>3</sup> Any business operating in the U.K. with an employee count above the threshold, not just domestic U.K. firms, must report data for their employees in England, Scotland, and Wales.

The data include the difference in the mean hourly rate of pay for men and women employees, as well as the median difference in hourly pay, the difference in mean and median bonus pay, the proportion of men and women employees receiving bonuses, and the proportion of male and female employees in each quartile of pay bands.<sup>4</sup> The first round of reporting in April 2018 was a snapshot of the April 5th, 2017 pay period and the cumulative bonus pay over the year.

All firms that meet the inclusion criteria must comply — there is no “comply or explain” clause in the regulation. The Equality and Human Rights Commission can penalize non-compliance with potentially “unlimited fines.” Initial compliance with the regulation was extremely high. 93% of private sector organizations reported data by the deadline, and 100% of required firms reported by August 2018 (Government Equalities Office 2018) .

An important caveat is that the gender pay gap, as measured by the regulation and in this study, is not, by definition, the same as wage discrimination or unequal pay — paying women employees less than men employees for equivalent work. The U.K. disclosure law only captures disparities in overall averages in salaries and bonus pay across the entire firm and within income quartiles. Thus, firms may present a pay gap in the U.K. database if, for example, women have a more significant proportion of lower-paying jobs in a firm than men, even if within-job pay is equitable across gender groups.

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<sup>3</sup> <https://gender-pay-gap.service.gov.uk/>

<sup>4</sup> Details of how each metric should be calculated can be found in the Government Equalities Office’s “Summary of reported data for 2017/18” at [https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment\\_data/file/751209/GPG-Reporting-Portal-Report.pdf](https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment_data/file/751209/GPG-Reporting-Portal-Report.pdf)

The U.S. Equal Pay Act of 1963 and the U.K. Equal Pay Act of 1970 prohibit unequal pay based on gender. Despite this fact, however, the reality of unequal pay for women persists in both the U.K., where women file over 29,000 complaints of violations of the Equal Pay Act per year (Goodley 2020), and the U.S., where the Court of Appeals for the Ninth Circuit in *Rizo v. Yovino* affirmed that “salaries speak louder than words... the financial exploitation of working women embodied by the gender pay gap continues to be an embarrassing reality of our economy” (*Rizo v. Yovino* 2017).

The first round of reporting in 2018 highlighted stark gender pay gaps in the U.K. 88% of firms reported an hourly gender pay gap unfavorable to women. Likewise, more than half reported a gap in average bonus pay. Although the U.K. database does not provide details of wage discrimination, consumers concerned with this issue likely use mandatory gender pay gap reporting as a proxy for a corporation’s commitment to gender pay equality.

### **3.2. Data Sources**

We obtained two data sets to complete our primary analysis. The first source of data is the U.K.’s Gender Pay Gap Service. As previously mentioned, the U.K. government hosts an easily accessible website targeted at the general public that contains all gender pay disclosure data for firms that meet the inclusion criteria. We utilize this database and hand-collect the names of U.K. employers that also have operations in the U.S. by investigating each registered firm with internet searches. We limit our search to the firms with the following 2-digit UK SIC codes: 10-33, 45-47, 55-56, 69-75, and 77-82. These codes cover sectors including accommodation and food service, administrative and support service activities, manufacturing, and wholesale and retail trade. Firms in these sectors tend to have large numbers of outlets and are the most likely ones to have foot traffic data available for individual retail outlets in the U.S.

Second, we use cell phone location data from SafeGraph to measure consumer visits to retail outlets. SafeGraph gathers location data from its users on nearly 19 million devices, representing approximately 6% of the U.S. population and 10% of U.S. cell phones. Based on SafeGraph’s analysis of its user base, it claims that its customers are representative of the income, age, and other demographics of

the U.S. population. The data are anonymized and aggregated to the total number of unique monthly visits and visitors at the outlet level. SafeGraph’s data begins in January 2018, covering the same period as the rollout of gender pay gap disclosure in the U.K. — allowing us to examine changes in consumer behavior in response to gender pay gap disclosure. These data have also been used to measure consumer responses to events affecting firms in other contexts, including private equity takeovers (Pursiainen and Tykvova 2023), restriction of gun sales (Painter 2023), class action lawsuits (Von Meyerinck et al. 2024), access to stores for non-customers (Gurun et al. 2023). These data have been used to study other social outcomes beyond foot traffic to outlets, including stay-at-home behavior during the COVID-19 pandemic (Simonov et al. 2021, Dastidar et al. 2023, Zhao et al. 2023).

We match outlets in the SafeGraph data to U.K. gender pay disclosure data based on firm names and industries. This results in a sample of 133 matched firms, with 89,364 unique outlets in the U.S. We construct the control sample by selecting all monthly observations of outlets in the same ZIP code and six-digit NAICS industry as the treated outlets. This procedure yields a matched control sample comprising 188,960 distinct outlets. Figure 2 shows the spatial distribution of treated outlets across the continental U.S. As stated, control outlets are limited to the same zip codes as the treated outlets.

We also utilize demographic data from several different sources. Data on local gender composition, education, employment and labor force participation, and wages are from the U.S. Census’s 2017 American Community Survey. Data on local voting are from the MIT Election and Data Science Lab. Data on local religious participation is from the ARDA 2010 U.S. Religion Census. Finally, data on local gender equality come from the Georgetown Institute for Women, Peace and Security (Di Noia 2002, Klugman et al. 2020).

### **3.3. Research Design**

Our research question requires us to measure the causal effect of gender pay disclosure on foot traffic. However, estimating the causal effect of gender pay gap disclosure on foot traffic is a challenging empirical exercise for several reasons. First, numerous factors might confound the relationship between disclosure quality and foot traffic, including economic conditions, industry-specific trends, or seasonality.

Additionally, it is not possible to estimate the causal effect of disclosure when firms voluntarily produce gender pay disclosures due to endogeneity concerns. For example, it might only be the firms with the most equitable gender pay practices or with customers that explicitly demand disclosure that self-select to disclose.

To help overcome these endogeneity problems, we use the U.K.'s national mandatory disclosure regulation as an exogenous shock to gender pay disclosure. However, identification concerns remain. Namely, it is difficult to estimate a causal effect among firms in the U.K. because the transparency and accessibility of gender pay data increased simultaneously for almost all firms. Thus, the options for control firms within the U.K. are limited. Using firms under the boundary of 250 employees as controls would be possible. But, due to the small size of these firms, it is not clear that the results would be generalizable to other contexts or larger U.K. firms.

So, to improve on the validity of the design, we utilize U.S. firms that operate in the U.K. as our treated group and compare trends in purchasing patterns to other U.S. firms that do not operate in the U.K. as our control group in difference-in-differences regressions. The design of this difference-in-differences approach is crucial to the causal validity of our empirical findings. Other studies that measure the relationship between voluntary disclosure of CEO-to-employee pay ratios and sales, for instance, can only measure associations rather than the causal effects of disclosure and customer behavior (Mohan et al. 2018).

We use the following regression specification for monthly observations of foot traffic to U.S. outlets:

$$\text{Log}(\text{Visits}/\text{Visitors})_{i,t} = \alpha + \beta_1 \text{Post Disclosure}_{i,t} + \mu_i + \pi_{j,z,t} + \varepsilon_z \quad (1)$$

In this model,  $i$  indexes the outlet,  $t$  indexes the month,  $j$  indexes the industry, and  $z$  indexes the zip code.  $\text{Log}(\text{Visits}/\text{Visitors})_{i,t}$  captures the outlet-level number of monthly unique visits or visitors.  $\text{Post Disclosure}$  is the variable of interest and  $\beta_1$  captures the average percentage change in foot traffic in the months following disclosure.  $\text{Post Disclosure}$  is a dummy variable equal to one beginning in April 2018, the first month of publicly available gender pay disclosure through the U.K. database. Before that, it

equals zero. We control for all time-invariant differences across firms with a firm-level fixed effect,  $\mu_i$ . We also include a NAICS 5-digit industry-by-zip code-by-month fixed effect,  $\pi_{j,z,t}$ . The fixed effects absorb the two components of the interaction term that indicate whether an outlet is treated and if the month is in the post-disclosure period. We cluster standard errors,  $\varepsilon_j$ , by zip code to account for factors like local policies, economic conditions, and social norms that might create spatial correlation in the error terms.<sup>5</sup>

This specification estimates the effect of gender pay disclosure regulations on treated outlets by comparing traffic at these outlets against control outlets in the same industry located in the same immediate geographic region during the same monthly period.<sup>6</sup> There are several advantages to this empirical setup. The granular outlet-level and zip code by industry by month-level fixed effects mitigate potential confounding factors like local economic conditions, industry-specific trends, and sales seasonality. Likewise, the rule change is plausibly exogenous to U.S. firms operating in the U.K. Although the firms required to disclose in our sample are large multinational corporations, it is unlikely that these firms had significant influence over the timing or scope of the regulation. Finally, although the rollout of the regulation was lengthy, attention to the topic online spiked in the month the data was publicly available (see Figure 1). The monthly average foot traffic plots in Figure 3 demonstrate that the parallel trends condition is satisfied. The trends for treated and control outlets appear similar in the three months before disclosure. Then, a gap emerges in the month of disclosure. The change in foot traffic patterns follows the same immediate shift as the Google search volumes, and the gap size remains relatively consistent until the following year's reporting period. Thus, it is unlikely that consumers in the U.S. adjusted their purchasing decisions in anticipation of the disclosure.

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<sup>5</sup> The results are robust to clustering on alternate dimensions including clustering by outlet and two-way clustering on zip code-by-industry or zip code-by-month. These results are in the Internet Appendix Table IA1. However, there are low numbers of clusters or observations within clusters for these alternate clustering choices due to the length of the time series and the limited number of industries with foot traffic data. Thus, these clusters may not meet the asymptotic conditions required and have less precision than clustering by zip code alone.

<sup>6</sup> Internet Appendix Table IA2 uses nearest-neighbor matching to select control outlets. The results are unchanged.

## **4. Results**

### **4.1. Descriptive Statistics**

Table 1 provides summary statistics for the sample. During this period, we observe nearly 1.9 billion unique visits at 278,324 outlets across 12,257 zip codes. These outlets represent 133 treated and 2,148 untreated firms in the United States. Although there are far fewer treated firms than untreated firms, 32% of observations are “treated” at the outlet-month level — the unit of observation for this study. Due to the nature of treatment (i.e., multinational operations in the U.K.), treated firms are likely to be larger with more outlets. The average outlet with geolocation data available in SafeGraph receives 422 monthly unique visits from 272 unique visitors.

At the firm level, the gap in mean reported pay between men and women is 11.1% for firms in this sample, and the gap in median reported pay is 4.8%. These gaps are slightly lower than the average across the entire U.K. database. There, the mean and median reported pay gaps are 14.4% and 12.0%, respectively. Figure 4 gives the full distribution of mean reported pay gaps for firms in this sample required to disclose by U.K. law. Approximately 90% of these firms reported a mean pay gap unfavorable to women.

### **4.2. Gender Pay Gap Disclosure and Consumer Foot Traffic**

We begin by estimating the overall effect of gender pay gap disclosure on foot traffic to quantify how consumers value the disclosure of this information. It is unclear, ex-ante, how consumers will react. On the one hand, consumers may reward firms for transparency. In this sample, very few firms are disclosing pay gap information. Consumers may value this transparency favorably, even if the disclosure is legally mandated. For example, LaViers and Sandvik (2023) show that potential employees are more likely to accept offers from employers that disclose workplace gender diversity statistics, regardless of what the statistics say.

On the other hand, consumers may react negatively and reduce foot traffic to disclosing firms. As mentioned, nearly all firms in the sample report a pay gap unfavorable to women. Consumers trying to incorporate this issue into their purchasing decisions may view these disclosures negatively. After learning

that a firm has a pay gap unfavorable to women in a lab setting, consumers reduced purchase intentions (Schlager et al. 2021). Thus, how consumers respond to this type of transparency is an open empirical question.

We present the results of a regression of foot traffic on disclosure mandates in Table 2. Regardless of the choice of fixed effects, the estimated coefficient for *Post Disclosure* is positive and statistically significant across all specifications for both the number of visits and the number of visitors. Relative to other outlets in the same sector and zip code and average traffic to the outlet itself, disclosing firms' outlets experience an increase in visits and visitors of approximately one percent following the disclosure. At the sample mean, these effects amount to an increase of 4.2 unique visits (Column 3) and 2.4 unique visitors (Column 6) for the average outlet. These effects are unchanged when we cluster standard errors on alternate dimensions (Table IA1) or match treated firms with other local competitors on firm size (Table IA2).

These results are also persistent when, in untabulated regressions, we drop firms that report a pay gap that is favorable to women. Taken together, these results suggest that consumers value transparency around gender pay gap disclosure and are willing to favor disclosing firms relative to non-disclosing firms, even if the reported pay gap is unfavorable to women.

### 4.3. Pay Gap Size and Consumer Foot Traffic

Although the overall effect in Table 2 is positive, the severity of the pay gap may moderate this baseline effect. We test this conjecture in Table 3 by adding an interaction term to equation (1). This results in the following specification:

$$\begin{aligned} \text{Log}(\text{Visits}/\text{Visitors})_{i,t} = & \alpha + \beta_1 \text{Post Disclosure}_{i,t} \times \theta_i + \\ & \beta_2 \text{Post Disclosure}_{i,t} + \mu_i + \pi_{j,z,t} + \varepsilon_z \end{aligned} \quad (2)$$

This model is equivalent to equation (1) with an additional interaction term between the conditional variable,  $\theta_i$ , and *Post Disclosure*. In this table, we interact the treatment effect with the indicator variables *High (Low) Gap*. These variables denote whether the firm's gender pay gap in its 2018 report was in the highest (lowest) quartile of disclosing firms in this sample.

Columns 1 and 2 of Table 3 include an interaction term for *High Gap* and compare the effect of reporting a high gap to the average effect for all other disclosers. The negative coefficient on this interaction term  $\beta_1 Post Disclosure_{i,t} \times High Gap_i$  is highly significant ( $p < 0.01$ ) and nearly double the magnitude of the positive effect for  $\beta_2 Post Disclosure_{i,t}$ . Thus, compared to firms with lower pay gaps, traffic at *High Gap* firms decreases by 1.1% ( $\beta_1 + \beta_2 = -0.024 + 0.013 = -0.011$ ), on average.

Columns 3 and 4 substitute the *Low Gap* term for *High Gap*. Conversely, firms with the lowest reported pay gaps experience substantially more significant increases in foot traffic than those with higher pay gaps. Whereas other firms' disclosure results in an increase in traffic of 0.9%, firms with the lowest gap experience a 1.5% increase ( $\beta_1 + \beta_2 = 0.006 + 0.009 = 0.015$ ) in customer visits.

These findings both confirm prior experimental findings and also contribute new insights. Experimental studies have found that consumers negatively react to gender pay gaps using the published pay gap for Adidas (18% lower average pay) and Uber (33% lower average pay) (Schlager et al. 2021). The size of these two gaps puts both firms into the *High Gap* quartile, with Uber above the 95<sup>th</sup> percentile of gap size in the sample. Thus, our findings in columns 1 and 2 show that consumers' lowered purchase intentions for firms with markedly high pay gaps do translate into purchase decisions for a significant portion of consumers.

The conclusions from the remainder of the table are more aligned with the findings from Table 2. These results show that firms that reported pay gaps below 15% (i.e., 75<sup>th</sup> percentile) experienced increased traffic despite unequal pay. This finding suggests that consumers value both the content of the disclosure as well as the transparency provided by the disclosure itself.

#### **4.4. Determinants of Changes in Traffic**

Next, we study how several local norms moderate the effect of gender pay disclosure on foot traffic. Again, we use the empirical setup in equation (2) with an interaction term if the local norm is above the sample average value. These local norms include indices for local gender norms, the size of the local wage and employment gaps between men and women, education, political leanings, and religiosity. Several of

these characteristics have been shown to moderate how the environmental and social policies of the firm affect consumers' purchasing decisions (Duan et al. 2023). Additionally, we examine how the percentage of women in the local population and the local labor force participation rate for women may affect the findings. (Schlager et al. 2021) find that information about gender pay gaps has strong and persistent effects on women's purchasing intentions.

The choice of these potential moderators is also motivated by results from a survey within the U.S. Women, Peace, and Security Index 2020 report (Klugman et al. 2020). When asked to determine the cause of their state's high or low values on the WPS Index, respondents in states high on the index cited politics, education, and culture as top reasons. Similarly, when the question was posed to respondents from states at the bottom of the index, respondents cited tradition/gender roles, politics, religion, economic conditions, and attitudes among the primary causes.

The results are presented in Figure 5. Panel A of this figure gives the results across all disclosing firms. This panel shows a positive baseline effect of disclosure on foot traffic, regardless of the local norms tested. We show that neither the share of women in the local zip code nor the women's labor force participation rate strongly significantly moderates the effect of gender pay disclosure. Likewise, other local norms, including political leanings, do not affect the baseline result. Instead, we find that local attitudes towards gender equality and gender roles, as proxied by the Gender Equality Index and the WPS Index, strongly amplify the positive effects on foot traffic. Thus, transparent pay disclosure seems to be valued commonly across distinct locales but is valued most in places where local laws and norms already favor women's equality. We also present a tabulated version of these results in Table 4.

Panels B and C split the sample into a subsample of *High Gap* disclosers and controls (Panel B) and *Low Gap* disclosers and controls, respectively, to further investigate how local norms influence purchasing dynamics. Again, the effects of disclosure are relatively homogenous across zip codes in both panels. The baseline effect is negative and highly significant across all specifications for *High Gap* firms and positive and highly significant for *Low Gap* firms. The only significant moderators of the negative effect of disclosing a high pay gap are the *M-W Wage Gap* and the *% Religious*. Consumers in areas where

their local pay gaps are high and in some other cultural contexts may not avoid purchasing firms with high gaps as strongly, though the summed coefficients are still directionally negative.

In individual regressions for Low Gap firms, the positive baseline effect is attenuated by the labor force participation rate of women, higher education levels, higher proportions of Democratic voters, and higher religiosity, all at high levels of statistical significance ( $p < 0.01$ ). However, when these interactions for the negative moderators are included together in a single regression, we find that *% Democrat* and *% LF Participation Women* lose their statistical significance, and *% College* and *% Religious* are only weakly significant at the 10% level. Religiosity attenuating the baseline effect is consistent with Panel B. Higher education attenuating the effect is not. We surmise that college-educated consumers may be more skeptical of firms that report a smaller pay gap than peers. We investigate this hypothesis in the next section.

The results across these panels indicate that the effect of disclosure on consumers is fairly homogeneous across demographics. This finding is novel and contrasts with research showing how several different demographic characteristics can moderate consumers' reactions to ESG policies.

#### **4.5. Can Consumers See Through Misreported Data?**

When firms disclose information, they reduce information asymmetry between the firm and consumers (Spence 1973, Connelly et al. 2011) and build trust and credibility for their brand (Cambier and Poncin 2020). However, the quality and trustworthiness of disclosed information matters. Violations of trust lead to negative reactions from consumers (Wagner et al. 2009, Skarmeas and Leonidou 2013).

Prior research finds that some firms misreport their pay gaps to the U.K. database. Some firms report statistically dubious numbers (e.g., perfectly balanced gender statistics), and around 4% of firms report mathematically impossible information in a given year (Andrews et al. 1998, Bailey et al. 2022). If consumers can recognize misinformation in disclosures or if they devote attention to learning about misreporting through the media or academic sources, firms that misreport information could suffer negative consequences. Customers' attention, however, may be too constrained, or they may not have the statistical

sophistication to detect inaccurate disclosures themselves. We use the measure of *Impossible* reporting statistics from Bailey et al. (2022) to examine if consumers can see through misreported data in Table 5.

Columns 1 and 2 present the results for the entire sample. The baseline effect of *Post Disclosure* remains approximately a 1% increase for disclosing firms that do not report impossible statistics. However, consumers punish misreporters with decreased traffic. Column 1 shows that within-outlet visits fall by 4.6% for misreporters ( $p < 0.01$ ). The size of this effect narrows and is no longer statistically significant in Column 2 when industry by zip code by month fixed effects are added. However, the negative coefficient here is still larger than the baseline effect. The loss of statistical significance in Column 2 may be due to limited power, as there are a limited number of firms misreporting in this sample.

In the prior section, we conjectured that college-educated consumers may not trust the disclosure. We test this conjecture in the context of misreporting in Columns 3 through 6. Columns 3 and 4 (5 and 6) use the subsample of observations from areas with above-median (below-median) college education levels, respectively. Misreporters consistently experience nearly 4% lower traffic in areas with high college education levels after disclosing impossible numbers, regardless of the specification. We do not find a similar effect for regions with low college education levels.

These findings confirm that some consumers do see through misreported data. Given that the benefits of transparency on foot traffic extend to many firms that report unfavorable pay gaps, managers' attempts to misreport more "ideal" statistics appear misguided and harmful to sales. These findings also provide support for why college education moderates the effect of reporting a low gap. These consumers are more likely to understand that some firms are reporting inaccurate information and may place less trust in reports claiming higher pay equity levels.

#### **4.6. Feedback Effects on Disclosing Firms**

Our results suggest that gender pay practices have significant effects on consumer behavior. Customers' reactions to social issues can impact how firms approach those issues in the future (McDonnell and King 2013). Thus, these firms may learn from consumer reactions to pay gap disclosure and adjust their

compensation practices accordingly. This would mean that firms experiencing more negative reactions might work to reduce their gender pay gaps. To test this conjecture, test how lower-than-expected visits post-disclosure relate to lower pay gaps in the future.

To do so, we collapse the foot traffic data to create the variable *Missing Visits<sub>2018</sub>*. This variable captures the mean monthly drop in visit counts for the outlet in the 2018 post-period compared to its predicted foot traffic across all months post-disclosure. The values for *Missing Visits<sub>2018</sub>* are the residuals from a regression of outlet-level raw visit counts on outlet and industry-by-zip code-by-month-level fixed effects (inverted such that higher values indicate less traffic than predicted).

This analysis is in Table 6. Panel A shows that missing visits in 2018 positively predict lower gender wage gaps in future years. The economic magnitudes of these values are significant. A one standard deviation increase in *Missing Visits<sub>2018</sub>* is associated with a 1.7 to 4.9% increase in the probability that the firm reports a lower pay gap in future years.

Panel B measures the relationship between traffic and future pay practices with an interaction term for *High Gap* firms. The coefficients for *Missing Visits<sub>2018</sub>* have a similar direction, magnitude, and significance as those in Panel A. Surprisingly, customer visits do not play the same disciplining role for *High Gap* firms. The interaction term for *Missing Visits<sub>2018</sub> x High Gap* is the opposite sign and magnitude as the main effect and the coefficients, taken together, are not statistically significant from zero. Additionally, we find that, although *High Gap* firms are less likely to report a lower pay gap in 2019 and 2020 than other firms, they do lower pay gaps in 2021 and 2022. Some other mechanism (e.g., internal employee pushback, updated practices for new hiring) is likely responsible for this change.

## 5. Discussion

By leveraging a unique dataset that captures a substantial portion of traffic to consumer-facing businesses in the U.S., we provide large-scale empirical evidence that consumers' stated intent to integrate information about firms' gender pay practices into their purchases translates into real-world purchasing decisions. Interestingly, we find that consumers respond both to the content of the disclosures — with

higher foot traffic for firms with smaller pay gaps and lower traffic for those with larger gaps — and the disclosure itself. Our results show that, on average, outlets of firms required to disclose gender pay gaps experienced a significant increase in foot traffic compared to non-disclosing competitors, even though most firms reported a pay gap that was unfavorable to women. These results suggest that consumers value transparent, readily available, and comparable disclosure on this topic. This pattern is shared across different geographies and demographic groups. Although some local characteristics slightly amplified or attenuated the results, the overall conclusions remained unchanged. These findings add important nuance to the discussion on the value of gender pay disclosures to consumers.

Although this data and empirical setting help to identify the causal effects of disclosure, it is accompanied by some limitations. First, we cannot observe whether customers knew about the disclosure during their visit. However, the strength of the empirical setting, including the use of a strict set of fixed effects across time and the cross-section of outlets, helps to rule out concerns that another omitted variable may be driving the results. Second, the data reported on the U.K. website does not cover U.S. employees and is an imperfect proxy for the company's actual gender pay gap. We consider this mismeasurement (from the point of view of a U.S. consumer) as an issue that should weaken the response to gender pay disclosure in the U.S. and bias us away from finding significant results. The fact that U.S. consumers respond strongly to disclosures about workers in the U.K. adds credence to the view that this issue is crucial to consumers. Finally, these results are only generalizable to public-facing business-to-consumer businesses. It is not clear that the effects of disclosure would result in similar outcomes in other settings like business-to-business sales.

### **5.1. Managerial Implications**

Our findings highlight the relevance of how gender pay disclosure can build or erode customers' trust in managers. More specifically, by establishing that consumers value both equal pay practices and transparency around gender pay, we believe managers may better understand the value of these types of disclosures to consumers. It may be that managers of large multinational corporations are already aware of

the value — or the risk, in some cases — of such disclosures. Indeed, our results show that firms learn from consumer reactions and take action to adjust their policies to reduce gender pay gaps. However, smaller firms might not be aware of the potential reputational and operational risks of mandatory disclosure. It is possible that small to medium-sized firms in other jurisdictions will be required to report such information in the future, which will result in consequences for various stakeholder groups (e.g., customers, employees, investors). Managers of firms of all sizes should understand these consequences. The U.K. law requires firms with as few as 250 employees to make a report.

Self-disclosure of gender pay gaps might be a valuable marketing strategy to help attract a large segment of consumers who value pay equality. Since pay data is well-documented within firms for financial reporting and tax purposes, marketing managers can collaborate with H.R. managers to aggregate this information and analyze the firm's gender pay statistics. Marketing managers of firms with smaller pay gaps could then communicate this information to consumers with the expectation that transparency around such policies may strengthen brand identity and positively impact sales and customer loyalty.

However, whether a single firm voluntarily disclosing this information would meaningfully impact customers' perceptions is unclear. Mandatory disclosure may produce effects that firms cannot replicate with voluntary disclosure. Consumers may not value it similarly if the information is not verifiable, comparable with peers, or audited. However, voluntary disclosure may be a low-cost strategy to implement, even though the findings of this study do not guarantee its success. Perhaps a more successful strategy for firms with low pay gaps would be to lobby for more disclosure to expose industry competitors with differing pay and hiring standards.

## **5.2. Regulatory Implications**

Mandatory reporting of corporate policies and data related to environmental, social, or governance issues — topics typically not covered by standard financial accounting reports — has recently been a key focus of many regulatory bodies worldwide. Among these topics, the reporting of gender pay arguably has received less attention than other important issues. As of 2023, 18 countries have mandated gender pay

reporting requirements. There is wide variation, however, in the scope of these regulations, and only four countries require the data to be made available to the public.<sup>7</sup> The U.K.’s mandate is among the most comprehensive and available to the general public. Corporations in many countries with large workforces, including those in the United States, are not mandated to report gender-disaggregated pay information for employees within the country. Thus, discussion of the appropriate regulatory prescriptions will remain prevalent as employers, employees, citizens, and governments worldwide continue to reckon with the issue of fairness and equality for women in the workforce.

An important caveat when discussing fairness and equality is that the data used in this study does not directly measure the frequency or severity of unlawful pay discrimination. In the future, regulators may consider mandating companies to publish this information rather than overall averages in pay by gender. In the U.S., for example, pay discrimination complaints to the Equal Employment Opportunity Commission may never become available to the public, and publicly available information, including lawsuits and settlements with regulators, is often difficult to aggregate. Easily accessible, mandatory disclosure on discrimination lawsuits or complaints to regulatory authorities may elicit even stronger reactions from consumers concerned with pay inequality.

Our study demonstrates that regulators considering the potential economic implications of gender pay disclosure laws can look to the U.K. mandate itself and the academic research about its effects for guidance. The results of this study show that gender pay disclosure information isn’t just important for employees, as previously demonstrated in the literature. Customers also care about the information and factor it into their decision-making. In this regard, we add to the growing academic evidence available to regulators that demonstrates the efficacy of such regulations in providing valuable information for different stakeholder groups — especially when the data is broadly accessible to the public.

It is likely that the specific features of this regulation, including its universality, high levels of compliance, and accessibility, have made it useful to consumers in their decision-making. However, as our

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<sup>7</sup>The OECD’s “Pay Transparency Tools to Close Gender Wage Gap” report provides a detailed overview of global gender pay reporting requirements (OECD 2021).

results on misreporting show, the disclosures must face regulatory scrutiny with penalties for misreporting. Otherwise, firms may be incentivized to misreport, causing consumers to be misinformed or mistrust the data — a counterproductive outcome.

### **5.3. Directions for Future Research**

The marketing literature has underutilized foot traffic data to measure real-world purchasing decisions, both in general and in the context of consumer's reactions to firms' disclosure of environmental or social practices. Research on customer reactions to corporate disclosures of environmental or social information is a relatively new field. Academic research has overlooked the impact and relevance of many recent disclosure regulations on this vital stakeholder group. Potential avenues for future research would include investigating customers' responses to several recent disclosure settings, including sustainability reporting standards, the U.K.'s modern slavery reporting, and conflict mineral reporting from the Dodd-Frank Act.

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## Appendix A. Variable Definitions

<i>Variable</i>	<i>Definition</i>
<b><i>Disclosure Variables</i></b>	
Disclosure	An indicator variable equal to one if the outlet was required to disclose by the U.K.'s Gender Pay Gap Information Regulations 2017, otherwise zero. [Source: U.K. Gender Pay Gap Service]
Post Disclosure	An indicator variable equal to one if the outlet was required to disclose and the observation is on or after the initial disclosure month (April 2018), otherwise zero. [Source: U.K. Gender Pay Gap Service]
Gap	The difference reported in the firm's 2018 filing between the mean hourly pay for men and the mean hourly pay for women in 2017 [Source: U.K. Gender Pay Gap Service]
High (Low) Gap	An indicator variable equal to one if <i>Gap</i> is in the highest quartile (lowest quartile) across the distribution of <i>Gap</i> for all firms. In this context, a higher value for <i>Gap</i> is a larger gender pay gap favoring men. [Source: U.K. Gender Pay Gap Service]
Impossible	Bailey, Glaeser, Omartian, Raghunandan (2022) show that firms report median <i>Gap</i> data that is statistically impossible. If women comprise a greater proportion of a firm's employees in the lower (upper) two pay quartiles, then the firm cannot have a negative (positive) median pay gap. Violations of this distributional property are classified as <i>Impossible</i> . [Source: U.K. Gender Pay Gap Service]
<b><i>Outcome Variables</i></b>	
Visits	Total number of unique visits to an outlet in a month [Source: SafeGraph]
Visitors	Total number of unique visitors to an outlet in a month [Source: SafeGraph]
Missing Visits <sub>2018</sub>	The measure captures abnormal traffic post-disclosure. Abnormal traffic is measured by the residuals from a regression of outlet-level raw visit counts on outlet and industry-by-zip code-by-month-level fixed effects over the sample period. The residuals are inverted so that higher values of the residuals represent less traffic than predicted.
Lower Gap <sub>t</sub>	An indicator variable equal to one if a firm reports a lower <i>Gap</i> in year <i>t</i> than in 2018 [Source: U.K. Gender Pay Gap Service]
<b><i>Local Norms</i></b>	
Gender Equality	The state-level Gender Equality Index from Di Noia (2002). This measure captures the attainment of women relative to men in a state on economic, political, and legal dimensions. [Source: Di Noia (2002)]
WPS	The state-level U.S. Women, Peace, and Security Index 2020. This measure captures three dimensions of justice, security, and inclusion and uses 12 indicators. [Source: GIWPS "The Best and Worst States to Be a Woman" Report 2020]
M-W Wage Gap	Median earnings in last 12 months for women age 25 and over / Median earnings in last 12 months for men age 25 and over in a zip code (S2001_C05_015E /S2001_C03_015E) [Source: 2017 5-year American Community Survey Earnings Table S2001]
% Pop. Women	Total population of women / Total population in a zip code (S0101_C05_001E/S0101_C01_001E) [Source: 2017 5-year American Community Survey Age and Sex Table S0101]
% L.F. Participation Women	Labor force participation rate of women aged 20-64 in a zip code (S2301_C02_023E) [Source: 2017 5-year American Community Survey Employment Status Table S2301]
M-W Employment Gap	% L.F. Participation Men – % L.F. Participation Women [Source: 2017 5-year American Community Survey Employment Status Table S230]

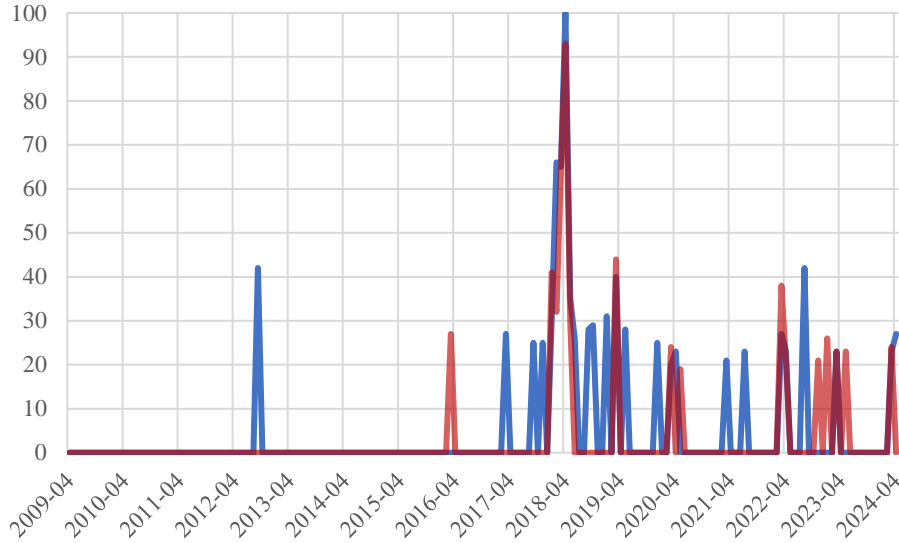
% College	Percent of population 25 and over with a bachelor's degree or higher in a zip code. (S1501_C06_015E) [Source: 2017 5-year American Community Survey Educational Attainment Table S1501]
% Democrat	Percent of votes for the Democratic Party in the 2016 Presidential Election in a zip code. [Source: MIT Election Data and Science Lab]
% Religious	Percent of population that are adherents to a religious body or group in a zip code (TOTRATE/1000). [Source: ARDA 2010 U.S. Religion Census]

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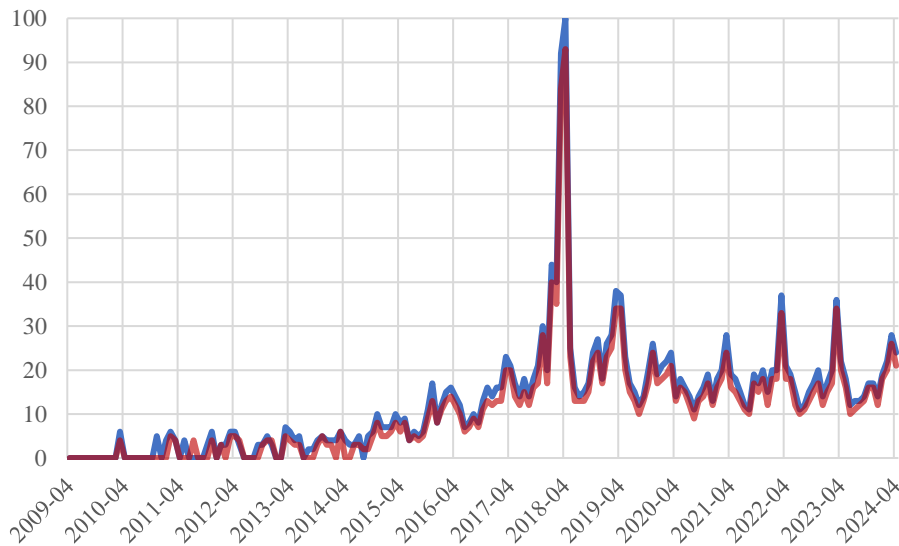
### Figure 1. “U.K. Gender Pay” Google Searches

Google search trends results for the terms “U.K. gender pay” (blue line) and “U.K. gender pay gap” (red line) from the period 4/1/2009-4/1/2024 in the United States (Panel A) and globally (Panel B). 100 represents the peak of search popularity for the term over the period. The search volumes below 100 are relative to this peak.

**Panel A:** U.S Search Volumes

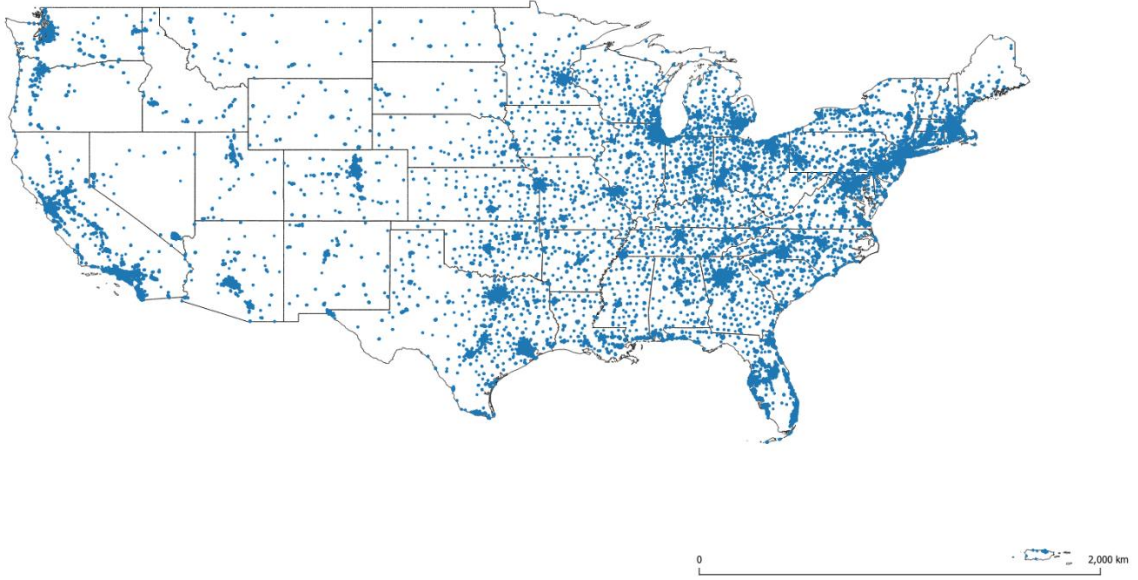


**Panel B:** Worldwide Search Volumes



### Figure 2. Treated Outlet Locations

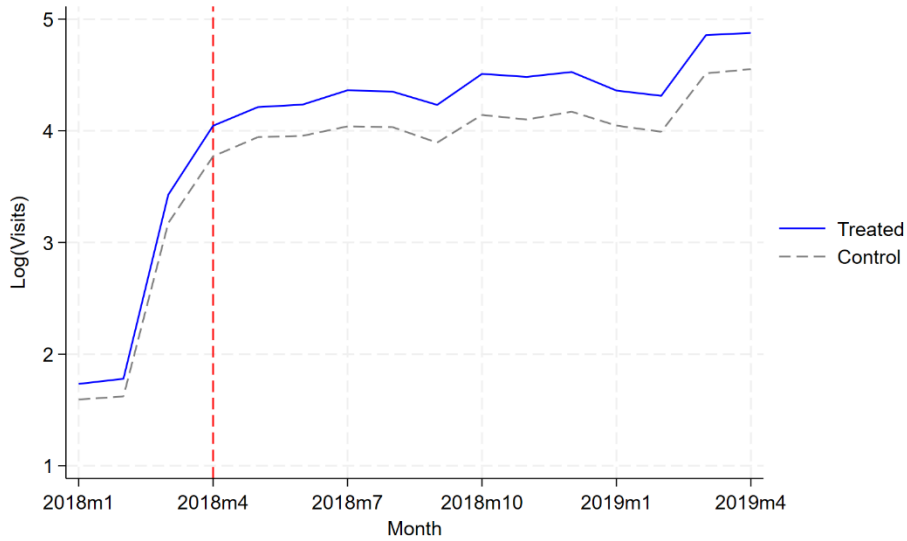
This figure shows the spatial distribution of the affected outlets in our sample (excluding Alaska & Hawaii).



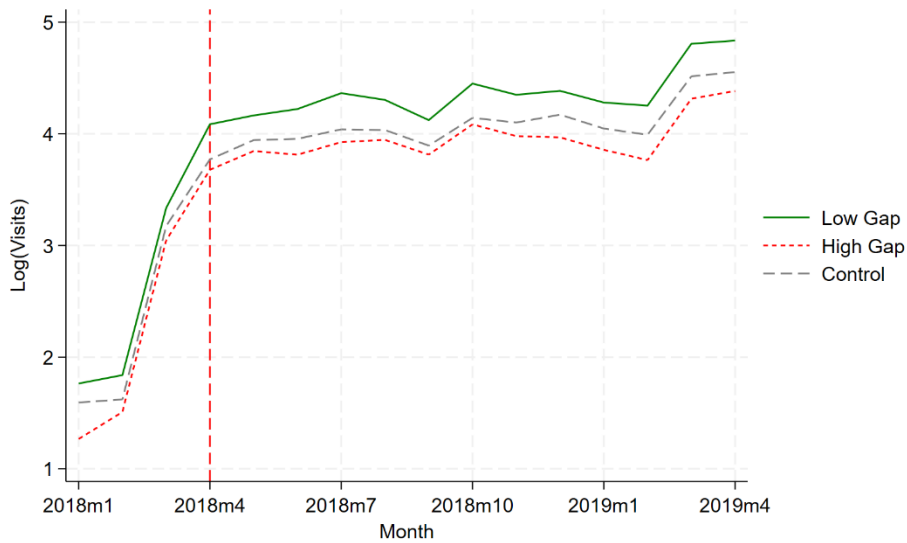
**Figure 3. Parallel Trends**

This figure shows the logarithm of the mean monthly visit counts for treated and control outlets. Raw visit counts by outlet are demeaned by the pre-treatment average for each outlet. Panel A presents visit trends for the entire sample. Panel B presents visit trends for outlets in the lowest quartile (*Low Gap* = smallest pay gap) and highest quartile (*High Gap*) for the 2018 report. The vertical line denotes the month when gender pay disclosures were made publicly available on the U.K. government website.

**Panel A: Total Visits**

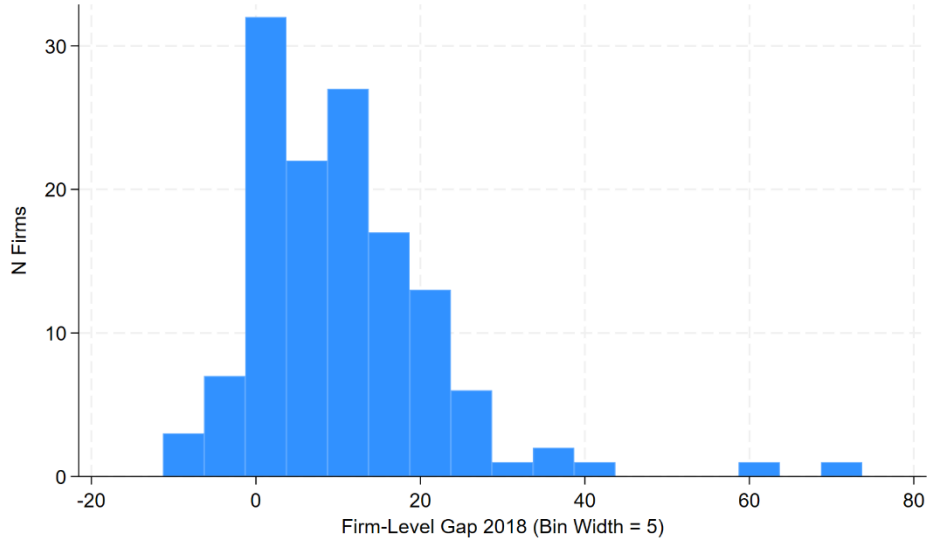


**Panel B: Visits Low vs. High Pay Gap**



### Figure 4. Distribution of Reported Pay Gap

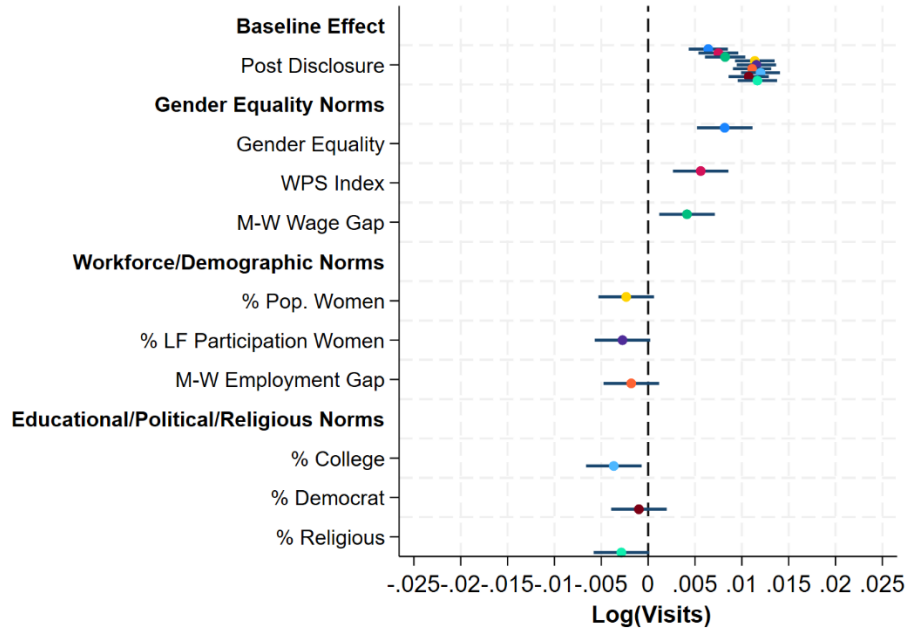
This figure is a histogram of the distribution of the mean pay gap reported by the 133 firms required to disclose in this sample. A positive gap indicates that men are paid more on average than women at the firm. The width of each bin represents 5 cents to the dollar. The bins give the count of the total number of firms (*N Firms*) that fall within each range.



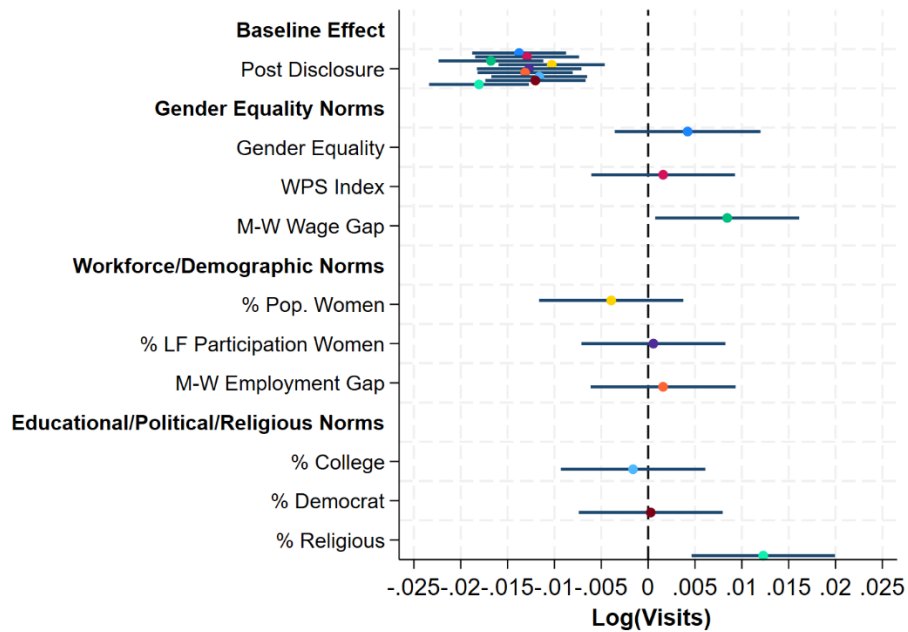
### Figure 5. Moderating Effects of Local Norms

This figure shows how various local norms moderate the baseline effects from Tables 2 and 3. The empirical specification follows Equation 2, where the interaction term is a local norm. The baseline effect and interaction term for each local norm are estimated in individual regressions but are plotted in the same graph for comparability. Panel A gives the results for the entire sample. Panels B and C condition the sample on *High Gap* and *Low Gap* firms, respectively, with untreated controls. The point estimates are plotted with dots, and the lines represent 95% confidence intervals.

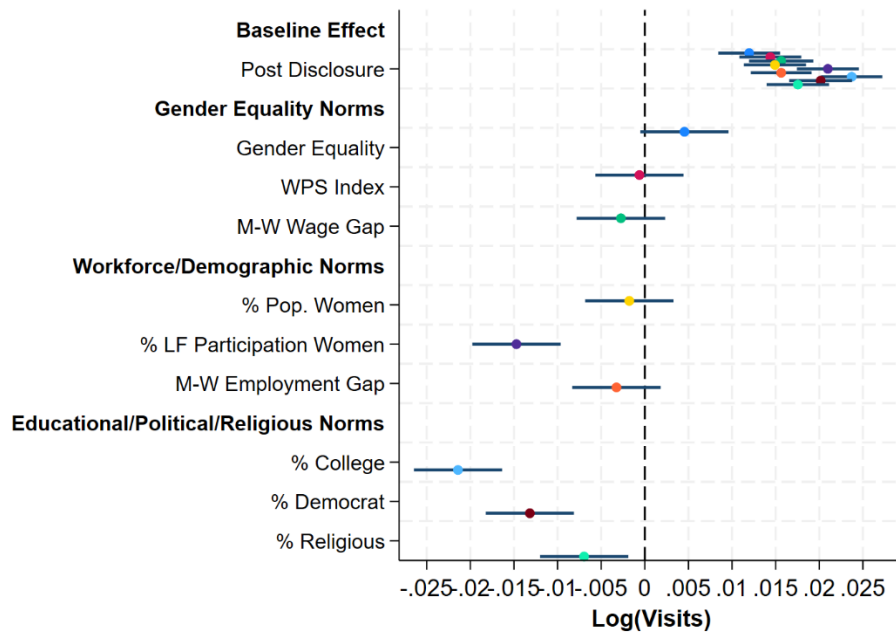
#### Panel A. Full Sample



#### Panel B. Conditional on High Gap



**Panel C. Conditional on Low Gap**



**Table 1. Summary Statistics**

This table presents summary statistics for the sample. The unit of observation is an outlet-month. There are 4,390,798 outlet-month observations for 278,324 unique outlets of 2,281 unique firms. Appendix A provides definitions for all variables.

	Mean	Median	SD	N
<b><i>Treatment</i></b>				
Post Disclosure	0.24	0.00	0.43	4,390,798
Disclosure	0.32	0.00	0.47	4,390,798
<b><i>Outcome</i></b>				
Visits	422.55	270.00	640.72	4,390,798
Visitors	272.06	178.00	367.82	4,390,798
<b><i>Local Norms</i></b>				
Gender Equality	59.77	61.90	6.86	4,367,967
WPS	47.35	48.50	12.15	4,379,718
M-W Wage Gap	72.66	72.32	12.52	4,370,896
% Pop. Women	50.86	51.08	2.78	4,380,393
% L.F. Participation Women	72.99	73.70	6.74	4,379,605
M-W Employment Gap	9.97	10.40	7.79	4,379,605
% College	33.35	30.00	16.91	4,380,265
% Democrat	48.23	49.30	16.84	4,365,738
% Religious	48.51	47.60	12.04	4,379,718
<b><i>Firm Controls</i></b>				
Log (Outlets)	7.20	7.63	1.89	4,390,798
<b><i>Firm-Level 2018 Pay Gap</i></b>				
Gap (Mean)	11.13	10.20	11.47	133
Gap (Median)	4.75	3.00	9.18	133

**Table 2. Gender Pay Disclosure and Foot Traffic**

This table presents OLS regressions of the outlet-level customer visits on mandatory pay gap disclosure. The dependent variable is shown above each column. *Visits* measure the number of total unique visits to the outlet in a month. *Visitors* measure the number of total unique visitors in a month. The independent variables of interest are *Disclosure*, which is equal to one if the firm is required to disclose gender pay gaps by U.K. law and zero otherwise, and *Post Disclosure*, which is equal to one for *Disclosure* firms from April 2018 (the first U.K. gender pay report date) onwards. Industry-by-zip code-by-month-level fixed effects are included in Columns 1 and 4. *Outlets*, a firm-level count of total national outlets, is added as a control in these columns. Columns 2 and 5 have outlet and month-level fixed effects. Columns 3 and 6 have the same fixed effect as Column 1 but also include an outlet-level fixed effect. Standard errors clustered at the zip code-level are in parentheses. Statistical significance is denoted by \*, \*\*, and \*\*\* at the 10%, 5%, and 1% level.

	(1)	(2)	(3)	(4)	(5)	(6)
	Log(Visits)	Log(Visits)	Log(Visits)	Log(Visitors)	Log(Visitors)	Log(Visitors)
<b>Post Disclosure</b>	<b>0.011***</b>	<b>0.008***</b>	<b>0.010***</b>	<b>0.010***</b>	<b>0.004***</b>	<b>0.009***</b>
	<b>(0.002)</b>	<b>(0.001)</b>	<b>(0.001)</b>	<b>(0.001)</b>	<b>(0.001)</b>	<b>(0.001)</b>
Disclosure	0.419***			0.417***		
	(0.005)			(0.005)		
Log(Outlets)	0.012***			0.007***		
	(0.002)			(0.002)		
Outlet F.E.	N	Y	Y	N	Y	Y
Month F.E.	N	Y	N	N	Y	N
Industry X Zip X Month F.E.	Y	N	Y	Y	N	Y
Adj. R-squared	0.275	0.947	0.954	0.277	0.952	0.960
Observations	4,390,798	4,390,798	4,390,798	4,390,798	4,390,798	4,390,798

**Table 3. Gender Pay Gaps and Foot Traffic**

This table presents OLS regressions of the outlet-level customer visits on mandatory pay gap disclosure, conditional on the firm's 2018 reported mean pay gap. The dependent variable is shown above each column. *Visits* measure the number of total unique visits to the outlet in a month. *Visitors* measure the number of total unique visitors in a month. The independent variables of interest are *High (Low) Gap*, which is equal to one if a firm was required to disclose and had a mean pay gap in the highest (lowest) quartile compared to other disclosing firms, and *Post Disclosure*, which is equal to one for disclosing firms from April 2018 onward. Industry-by-zip code-by-month and outlet-level fixed effects are included in all columns. Standard errors clustered at the zip code-level are in parentheses. Statistical significance is denoted by \*, \*\*, and \*\*\* at the 10%, 5%, and 1% level.

	(1) Log(Visits)	(2) Log(Visitors)	(3) Log(Visits)	(4) Log(Visitors)
<b>Post Disclosure x High Gap</b>	<b>-0.024***</b> (0.004)	<b>-0.020***</b> (0.003)		
<b>Post Disclosure x Low Gap</b>			<b>0.006**</b> (0.003)	<b>0.007***</b> (0.002)
Post Disclosure	0.013*** (0.001)	0.012*** (0.001)	0.009*** (0.002)	0.007*** (0.001)
Outlet F.E.	Y	Y	Y	Y
Industry X Zip X Month F.E.	Y	Y	Y	Y
Adj. R-squared	0.954	0.960	0.954	0.960
Observations	4,390,798	4,390,798	4,390,798	4,390,798

**Table 4. Moderating Effects of Local Norms**

This table presents OLS regressions of the outlet-level customer visits on mandatory pay gap disclosure, conditional on local norms. *Visits* measure the number of total unique visits to the outlet in a month. Each local norm is an indicator variable indicating that the local area has a value above the sample median. *Gender Equality* is the Gender Equality Index from Di Noia (2002), *WPS* is the Women’s Peace and Security Index from the Georgetown Institute, *M-W Wage Gap* is the gap between men and women’s local median annual income, *% Pop. Women* is the proportion of the local population that is a woman, *% L.F. Participation Women* is women’s local labor force participation rate, *M-W Employment Gap* is the gap between men and women’s labor force participation rate, *% College* is the proportion of the local adult population that holds a bachelor’s degree or higher, *% Democrat* is the proportion of local votes for the Democrat federal presidential candidate in 2016, *% Religious* is the proportion of the local population that is part of any religious body. More detailed descriptions of these local characteristics are in Appendix A. All columns include outlet-level and industry-by-zip code-by-month-level fixed effects. Standard errors clustered at the zip code-level are in parentheses. Statistical significance is denoted by \*, \*\*, and \*\*\* at the 10%, 5%, and 1% level.

<i>Local Norm =</i>	<i>Gender Equality</i>	<i>WPS</i>	<i>M-W Wage Gap</i>	<i>% Pop. Women</i>	<i>% L.F. Part. Women</i>	<i>M-W Employ. Gap</i>	<i>% College</i>	<i>% Democrat</i>	<i>% Religious</i>
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Log(Visits)	Log(Visits)	Log(Visits)	Log(Visits)	Log(Visits)	Log(Visits)	Log(Visits)	Log(Visits)	Log(Visits)
<b>Post Disclosure x Local Norm</b>	<b>0.008***</b>	<b>0.006***</b>	<b>0.004***</b>	<b>-0.002</b>	<b>-0.003*</b>	<b>-0.002</b>	<b>-0.004**</b>	<b>-0.001</b>	<b>-0.003*</b>
	<b>(0.002)</b>	<b>(0.002)</b>	<b>(0.002)</b>	<b>(0.002)</b>	<b>(0.002)</b>	<b>(0.002)</b>	<b>(0.002)</b>	<b>(0.002)</b>	<b>(0.002)</b>
Post Disclosure	0.006***	0.007***	0.008***	0.011***	0.012***	0.011***	0.012***	0.011***	0.012***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Outlet F.E.	Y	Y	Y	Y	Y	Y	Y	Y	Y
Industry X Zip X Month F.E.	Y	Y	Y	Y	Y	Y	Y	Y	Y
Adj. R-squared	0.954	0.954	0.954	0.954	0.954	0.954	0.954	0.954	0.954
Observations	4,367,967	4,379,718	4,370,896	4,380,393	4,379,605	4,379,605	4,380,265	4,365,738	4,379,718

**Table 5. Misreporting and Foot Traffic**

This table presents OLS regressions of the outlet-level customer visits on mandatory pay gap disclosure, conditional on whether the firm reports statistically impossible information. *Visits* measure the number of total unique visits to the outlet in a month. The independent variables of interest are *Impossible*, which captures whether the firm reported statistically impossible gender pay gap information in 2018 using the method from Bailey, Glaeser, Omartian, and Rahnunanda (2022), and *Post Disclosure*, which is equal to one for disclosing firms from April 2018 onward. Outlet and month-level fixed effects are included in Columns 1, 3, and 5. Columns 2, 4, and 6 include outlet-level and industry-by-zip code-by-month-level fixed effects. Columns 3 and 4 (5 and 6) condition the sample on above (below) median levels of local college education. Standard errors clustered at the zip code level are in parentheses. Statistical significance is denoted by \*, \*\*, and \*\*\* at the 10%, 5%, and 1% level.

	<i>Full Sample</i>		<i>% College - High</i>		<i>% College - Low</i>	
	(1) Log(Visits)	(2) Log(Visits)	(3) Log(Visits)	(4) Log(Visits)	(5) Log(Visits)	(6) Log(Visits)
<b>Post Disclosure x Impossible</b>	<b>-0.046***</b> (0.015)	<b>-0.018</b> (0.018)	<b>-0.044**</b> (0.019)	<b>-0.042*</b> (0.022)	<b>-0.028</b> (0.025)	<b>0.023</b> (0.032)
Post Disclosure	0.008*** (0.001)	0.010*** (0.001)	0.005** (0.002)	0.009*** (0.002)	0.009*** (0.002)	0.012*** (0.002)
Outlet F.E.	Y	Y	Y	Y	Y	Y
Month F.E.	Y	N	Y	N	Y	N
Industry X	N	Y	N	Y	N	Y
Zip X Month F.E.						
Adj. R-squared	0.947	0.954	0.949	0.957	0.944	0.951
Observations	4,390,798	4,390,798	2,184,411	2,184,411	2,195,854	2,195,854

**Table 6. Feedback Effects on Gender Pay**

This table presents OLS regressions of abnormally low foot traffic at the outlet and the probability of a firm reporting a lower mean pay gap in the future. The foot traffic data is collapsed to create the independent variables of interest *Missing Visits<sub>2018</sub>*. This variable captures the mean monthly drop in actual visit counts for the outlet in the post-period compared to its predicted foot traffic, such that higher values indicate less traffic than predicted. The values for *Missing Visits<sub>2018</sub>* are the residuals from a regression of outlet-level raw visit counts on outlet and industry-by-zip code-by-month-level fixed effects. They are standardized to zero with unit variance. The unit of observation is an outlet. *Lower Gap<sub>t</sub>* is an indicator variable that captures whether a firm reports a lower mean pay gap than 2018 in year *t*. Panel A gives the baseline result and Panel B is conditional on the firm's 2018 reported mean pay gap. *High Gap* in Panel B is equal to one if a firm was required to disclose and had a mean pay gap in the highest quartile, compared to other disclosing firms. Since the unit of observation is at the outlet-level, the fixed effect structure from prior tables is no longer possible. Instead, industry-level fixed effects are included in all columns. *Log(Outlets)* is added as a control in all columns. Standard errors clustered at the zip code-level are in parentheses. Statistical significance is denoted by \*, \*\*, and \*\*\* at the 10%, 5%, and 1% level.

**Panel A. Outlet Level**

	(1)	(2)	(3)	(4)
	Lower Gap <sub>2019</sub>	Lower Gap <sub>2020</sub>	Lower Gap <sub>2021</sub>	Lower Gap <sub>2022</sub>
<b>Missing Visits<sub>2018</sub></b>	<b>0.040***</b>	<b>0.017***</b>	<b>0.049***</b>	<b>0.038***</b>
	<b>(0.008)</b>	<b>(0.005)</b>	<b>(0.011)</b>	<b>(0.009)</b>
Controls	Y	Y	Y	Y
Industry F.E.	Y	Y	Y	Y
Adj. R-squared	0.530	0.607	0.407	0.506
Observations	89,346	89,346	89,346	89,346

**Panel B. Outlet Level – Conditional on Pay Gap Size**

	(1)	(2)	(3)	(4)
	Lower Gap <sub>2019</sub>	Lower Gap <sub>2020</sub>	Lower Gap <sub>2021</sub>	Lower Gap <sub>2022</sub>
<b>Missing Visits<sub>2018</sub> x</b>	<b>-0.049***</b>	<b>-0.023***</b>	<b>-0.059***</b>	<b>-0.047***</b>
<b>High Gap</b>	<b>(0.011)</b>	<b>(0.007)</b>	<b>(0.014)</b>	<b>(0.012)</b>
<b>Missing Visits<sub>2018</sub></b>	<b>0.048***</b>	<b>0.022***</b>	<b>0.058***</b>	<b>0.045***</b>
	<b>(0.011)</b>	<b>(0.007)</b>	<b>(0.014)</b>	<b>(0.012)</b>
High Gap	-0.038***	-0.128***	0.174***	0.119***
	(0.008)	(0.010)	(0.008)	(0.005)
Controls	Y	Y	Y	Y
Industry F.E.	Y	Y	Y	Y
Adj. R-squared	0.531	0.642	0.479	0.578
Observations	89,346	89,346	89,346	89,346

**Appendix B: Internet Appendix**  
**Table IA1. Alternative Clustering**

This table re-estimates the baseline OLS regressions of the outlet-level customer visits on mandatory pay gap disclosure from Columns 3 and 6 of Table 2 but varies the level at which standard errors are clustered across columns. Statistical significance is denoted by \*, \*\*, and \*\*\* at the 10%, 5%, and 1% level.

	(1)	(2)	(3)	(4)	(5)	(6)
	Log(Visits)	Log(Visitors)	Log(Visits)	Log(Visitors)	Log(Visits)	Log(Visitors)
<b>Post Disclosure</b>	<b>0.010***</b>	<b>0.009***</b>	<b>0.010***</b>	<b>0.009***</b>	<b>0.010***</b>	<b>0.009***</b>
	<b>(0.001)</b>	<b>(0.001)</b>	<b>(0.001)</b>	<b>(0.001)</b>	<b>(0.001)</b>	<b>(0.001)</b>
Outlet F.E.	Y	Y	Y	Y	Y	Y
Industry X Zip X	Y	Y	Y	Y	Y	Y
Month F.E.						
Clustering	Zip X	Zip X	Zip X	Zip X	Outlet	Outlet
	Industry	Industry	Month	Month		
Adj. R-squared	0.954	0.960	0.954	0.960	0.954	0.960
Observations	4,390,798	4,390,798	4,390,798	4,390,798	4,390,798	4,390,798

**Table IA2. Nearest Neighbor Matching**

This table re-estimates the baseline OLS regressions of the outlet-level customer visits on mandatory pay gap disclosure from Table 2 but uses a narrower set of control firms through nearest-neighbor matching. Treated outlets are matched to control outlets in the same industry and zip code by closest neighbor in size, measured by each firm's total number of national outlets at the beginning of the sample period. Nearest neighbor pair-by-month-level fixed effects are included in Columns 1 and 3.  $\text{Log}(\text{Outlets})$  is added as a control in these columns. Columns 2 and 4 add outlet-level fixed effects. Standard errors are clustered at the zip code level and are reported in parentheses. Statistical significance is denoted by \*, \*\*, and \*\*\* at the 10%, 5%, and 1% level.

	(1)	(2)	(3)	(4)
	Log(Visits)	Log(Visits)	Log(Visitors)	Log(Visitors)
<b>Post Disclosure</b>	<b>0.025***</b>	<b>0.015***</b>	<b>0.027***</b>	<b>0.017***</b>
	<b>(0.002)</b>	<b>(0.002)</b>	<b>(0.002)</b>	<b>(0.002)</b>
Disclosure	0.671***		0.683***	
	(0.008)		(0.008)	
Controls	Y	N	Y	N
Pair X Month F.E.	Y	Y	Y	Y
Outlet F.E.	N	Y	N	Y
Adj. R-squared	0.270	0.952	0.278	0.959
Observations	2,470,000	2,469,990	2,470,000	2,469,990