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# Does Pay Transparency Affect the Gender Wage Gap? Evidence from Austria

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# Does Pay Transparency Affect the Gender Wage Gap? Evidence from Austria*

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**Abstract**

We study the 2011 Austrian Pay Transparency Law, which requires firms above a size threshold to publish reports on the gender pay gap. We exploit variation across firm size and time, to study the effects of transparency on the gender wage gap and individual wages, using the universe of Austrian social security records. Our results show that the policy had no discernible effects on male and female wages, and therefore no significant effects on the gender wage gap. The effects are precisely estimated and we can rule out that the policy narrowed the gender wage gap by more than half a percentage point. We find no evidence for wage compression at the establishment level. The policy led to an increase in the retention rate of workers, which points towards higher job satisfaction due to pay transparency.

Journal Classifications: J08, J31, J38, J78

Keywords: Gender Pay Gap, Pay Transparency

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*"[...] the effective enforcement of the right to equal pay [...] for women and men remains a major challenge, partly because of a lack of information on pay."*

– Factsheet on Pay Transparency 2019, European Commission[1]

# 1 Introduction

Gender disparity in earnings is a persistent feature of labor markets around the world. While the pay gap has decreased in most developed countries over the last few decades, women still earn about 23% less than men in the US, 20% in Austria, and 16% on average across the European Union.[2] There is an ongoing debate among academics, policy makers, as well as the general public about the reasons behind the gender wage disparity and about the best policy instruments to close the gap (see Blau and Kahn (2017) for a review).

One policy instrument that has recently received widespread attention is some form of pay transparency legislation, whereby firms are required to provide information on pay disparities between genders. As the introductory quote shows, proponents of transparency argue that the lack of information on pay helps to sustain the gender gap. Transparency is therefore prescribed as a mechanism to provide women with more information about their employers' willingness to pay, which could help them challenge discriminatory pay schedules. These policies have garnered widespread attention among policy makers and as a response, variants of it have been introduced in Finland, Sweden, Norway, Denmark, Austria, the UK, Germany, Iceland, and the United States.[3]

It is not clear ex-ante whether pay transparency policies are effective instruments to close the gender pay gap. Critics argue that besides the administrative costs imposed on employers, transparency will not help to close the pay gap as long as women cannot or do not use the available information more actively than men. In fact, if men use the information revealed by transparency more actively and aggressively than women, then the gender pay gap might actually widen. Despite its recent introduction in many countries, the causal evidence of transparency laws on the gender wage gap is scarce. This paper studies the introduction of the Austrian transparency law to provide evidence on its causal effects on individual wages and

[1] `https://ec.europa.eu/info/sites/info/files/factsheet-pay_transparency-2019.pdf`

[2] Eurostat, 2017

[3] In the United States, during President Obama's tenure, the Equal Employment Opportunity Commission (EEOC) proposed changes which would have required firms with more than 100 employees to provide annual reports on gender pay gap, to the Department of Labor. This move was subsequently rolled back by President Trump. See: Obama EEOC Action on Pay Data collection).

the gender pay gap.

The Austrian transparency law was rolled out in phases, starting off with the largest firms in 2011. Over the next three years smaller firms were brought under coverage, and by 2014 all firms with more than 150 employees were required to publish income reports biennially. These reports must contain annual gross income, separately by gender and occupation groups as defined in the respective collective bargaining agreement. Using the universe of Austrian social security records, we exploit the size-based cutoff rule and employ an event-study design to estimate the causal effects of pay transparency on wages and the gender wage gap.

In our baseline specification we focus on a narrow window around the cutoff to make the control group as comparable to treated firms as possible. We do not find evidence that transparency has any discernible effect on the gender wage gap. The point estimate is close to zero, precisely estimated, and we can rule out that the policy narrowed the gender wage gap by more than half a percentage points. When we study the effects on wages of men and women separately, we do not find any statistically nor economically significant effects. Therefore, transparency seems to have failed in its twin objectives of reducing the gender pay gap and achieving that by boosting female earnings. We show that this conclusion holds under a number of alternative specifications using different control variables, different treatments of top-coded individuals, using different firm size windows or considering only compliers, i.e. firms that did not change treatment status in the years after.

While pay transparency does not affect averages wages, it potentially leads to wage compression within firms. Yet again, we find no evidence for this. The variance of log-wages within treated establishments evolves in tandem with the control group with no discernible effect of the policy. Further we do not find heterogeneous effects for workers earning below or above the gender-specific establishment-level median wage.

We additionally investigate whether the policy affects firms with seemingly higher gender disparities. If anything, we find the opposite. The gender wage gap widens by a modest 1 p.p. after the pay transparency legislation in establishments with above average gender wage gaps and in male dominated establishments. In contrast, the gap somewhat narrows by 0.75 p.p. in response to the reform in establishments with above average female share.

Why does pay transparency not affect the gender pay gap and wage setting in general? One possible explanation is that the information revealed is already privately available to workers. A second candidate explanation could be that even if the reform reveals discriminatory pay schedules, workers lack the bargaining power to re-negotiate wages. Third, it is possible that pay reports alleviate workers' concerns about unfair compensation. One way to distinguish these three competing explanations is to study job satisfaction. If pay reports do not reveal any additional information, job satisfaction should remain unaffected. If the reports show evidence of pay discrimination, but workers lack the bargaining power to renegotiate wages, we would expect job satisfaction to decline. In contrast, we would expect job satisfaction to increase if pay transparency alleviates concerns about unfair compensation. The social security data does not contain a direct measure of job satisfaction, so we use worker turnover as a proxy. Past research (Card et al. (2012), Rege and Solli (2015), Dube et al. (2018)) has shown that workers who feel unfairly compensated have a higher quit rate. Moreover, since pay reports are internal, we would not expect workers' outside options to have changed and therefore affect quit rates. In our data, we find that pay transparency reduces the separation rate significantly by 1.1 p.p., a 9 percent decline relative to its pre-reform level. We interpret this as suggestive evidence for the third channel, whereby transparency alleviates concerns about unfair compensation among workers.

Our work contributes to a growing literature on the effects of pay transparency policies on pay inequality, and in particular the gender pay gap. Baker et al. (2019) study the 1996 public sector salary disclosure law for university faculty in Canada. They show that mandated disclosure reduces salaries on an average and also decreases the gender pay gap. These results are driven primarily by a slowdown in the growth of male salaries in treated groups in comparison to control groups. Bennedsen et al. (2019) exploit the 2006 legislation in Denmark which requires firms with more than 35 workers to report gender-disaggregated salary reports. They find that the gender wage gap decreases relative to the pre-treatment mean, as a result of a decrease in the wage growth of male workers in treated firms. Both Baker et al. (2019) and Bennedsen et al. (2019) fail to find any effects on female earnings. More recently, Duchini et al. (2020) find that the UK pay transparency reform led to an increased hiring of women in above-median wage occupations, and a decrease in male real hourly pay rate in treated firms.

There is a parallel literature on unintended effects of pay transparency. Mas (2016) shows that mandated disclosure of CEO compensation in the USA, led to 'ratcheting' effects, whereby CEOs who were paid below average got pay raises. Schmidt (2012) also find similar 'ratchet' effects for managerial salaries after a corporate governance reform in Germany. Using a field experiment in online labor markets, Cullen and Pakzad-Hurson (2019) show that when workers negotiate in a common chat room 100% of the time in comparison to 60% of time in private chat rooms, average wages decrease in common chat rooms.

More broadly, our work is related to the literature which studies the effects of information about relative earnings on different behavioral and labor market outcomes: municipal salary disclosure on quit rates and pay compression among city managers (Mas (2017)), tax records on earnings and job search (Rege and Solli (2015)), publicly available tax records on happiness and life satisfaction in Norway (Perez-Truglia (2019)), perceived peer and manager salaries on effort and output (Cullen and Perez-Truglia (2018)), average wage raise among peers on separation rates in the US (Dube et al. (2018)), pay inequality on attendance and output in India (Breza et al. (2017)), publicly available pay records on job satisfaction in California (Card et al. (2012)), relative earnings on worker effort (Cohn et al. (2014)) and on happiness and life-satisfaction (Clark and Oswald (1996); Luttmer (2005); Brown et al. (2008); Clark et al. (2009); Godechot and Senik (2015)).

The rest of this paper is structured as follows. We describe the pay transparency law in more detail in Section 2. Section 3 explains our data, sample selection and provides an overview of several relevant trends. In Section 4 we explain our empirical strategy. We show our results and discuss them in Section 5. Section 6 concludes.

# 2 Empirical Setting and the Pay Transparency Policy, 2011

In international comparisons, Austria has a relatively high gender pay gap. According to data from Eurostat, Austria had an unadjusted gender pay gap of 20 percent in 2017, which was the fifth highest in the European Union.[4] The sources of this gender wage gap and possible policy interventions to narrow it are regularly discussed in the public domain in Austria.

Pay discrimination towards women features center-stage in these discussions and pay in-transparency is seen as a major hurdle to achieve equal pay for women.[5] A prominent argument is that women, perhaps through smaller networks, have less information on the firm's willingness to pay, and hence are in a worse bargaining position. In addition, if the pay information is secret, women might not even know about pay discrimination, and hence cannot act upon it.

In light of these debates, the government in Austria decided in 2011 to introduce the Pay

[4]Source: Eurostat (online data code sdg_05_20)

[5]"However, the effective enforcement of the right to equal pay for the same work and work of equal value for women and men remains a major challenge, partly because of a lack of information on pay." (Quote from Factsheet on pay transparency 2019, European Commission, October 2019) `https://ec.europa.eu/info/sites/info/files/factsheet-pay_transparency-2019.pdf`

Transparency Law, as an addition to the existing anti-discrimination legislation of 2004. Since then a number of countries, among them Germany, Finland, Iceland, and the United Kingdom have followed suit and introduced some version of pay transparency laws themselves. Denmark had already enacted such a policy in 2006. The stated intention of the Austrian policy was to reduce gender pay gap by increasing transparency at the firm level. The reform mandates firms to produce internal gender pay gap reports informing employees about either the mean or the median earnings including all pay components by gender and occupational group, as well as the total number of employees within the respective groups. These occupational groups are not at the discretion of the employer, but have to follow the pre-defined classifications in collective bargaining agreements.[6] The collective bargaining agreements are quite detailed in their occupational categories. For example, the wholesale and retail sector, which is the collective bargaining agreement with the highest number of employees in Austria, has 8 predefined occupational categories, 9 firm tenure groups, in addition to 2 regional categories. Managerial positions are exempt from reporting requirements. In comparison to pay transparency legislation in other countries, the Austrian version is stricter and more detailed in various characteristics. First, to protect the anonymity of individuals, if less than 3 employees fall within a certain gender-occupation group, they are counted with the next larger occupational group. This is more comprehensive compared to Denmark and Germany, where firms have to aggregate cells with 10 and 7 employees respectively. The UK legislation is on an even more aggregated level, as it does not require a break down of income statistics by occupation. Furthermore, in Austria firms are obliged to provide the information, whereas in Germany the responsibility is on the worker to request information. In Austria, reports must be made available to all employees via the works council where they can be accessed by any employee. In the absence of a works council, the report must be put on public display in a 'common (break) room'. Failure to publish these reports can lead to monetary fines and being directed by the courts to publish them. Workers are permitted to internally (at the firm level) discuss the contents of the reports as well as with their union representatives or legal advocates. However, workers are prohibited from communicating the contents of these reports outside of the firm. Firms have no obligation to make these reports public. Nonetheless, income reports from many firms in the public sector are available online. A sample income report from the Austrian Federal Government is shown in Appendix Table $B1$.

The law was proposed for the first time on October 25, 2010, the legislation was passed in

[6] 99 percent of Austrian employees are under a bargaining agreement

January 2011, and the law came into effect in March 2011. The implementation was staggered over the next four years starting off with the largest firms. Firms with more than 1000 workers had to publish their first report in March 2011. Next, firms with more than 500 employees were brought under coverage in January, 2012. In 2013, firms with more than 250 workers were required to publish their first report and the policy was fully implemented for all firms with more than 150 workers in 2014. After their first report, firms are required to publish these reports biennially. Firms that grow and exceed the 150 employee threshold after 2014 have to produce a report in the first year they exceed the threshold. About 30% of the Austrian workforce was subject to the policy when it was first implemented in 2011 for the largest firms. By 2014, when the law was fully implemented, 50% of workers were brought under coverage (See Appendix Figure $A1$).

Exploratory non-representative surveys were conducted by the Austrian Chamber of Labor (*Arbeiterkammer*), the Austrian Trade Union Federation (OeGB), and the Austrian Federal Ministry for Education and Women's Affairs (AFMEW) in 2014 and 2015 to study the level of compliance with the policy among firms, the contents of the income reports, participation of works councils in the preparation of these reports, and the transmission of these reports to employees. Evidence from these surveys (Schrittwieser (2014); Aichinger et al. (2015)) show that there was near universal compliance with the policy. Most firms stuck to legal minimum requirements, and additional information was frequently provided upon request from council representatives. The reports were shared with works councils promptly and in more than half the cases, council representatives reported to having worked closely with employers in the preparation of these reports. Information in these reports were distributed most frequently via intranet, announcements, articles in employee newsletters and other information events. 80% of representatives reported that their employers were open to adopting measures that would address the gap.

## 3 Data and Sample Construction

We use administrative employment records from the Austrian social security administration from 1997-2018. This data comprises of day-to-day information on the universe of employment spells subject to social security (Lalive et al., 2009). The data contains information on the yearly income at the person-establishment level, broken down by regular wages and bonus payments. It further contains basic socio-demographic information of workers such as age,

Table 1: Sample Restriction and Composition

*The table below shows the composition of the sample under different sample restriction criteria. Columns (4) and (5) show the sample means respectively for the treated and control group of firms in pre-treatment (2014) years. Column (3) is our main sample used for all analyses in the rest of this paper. The adjusted gender wage gap was computed by including only a quartic age polynomial and firm fixed effects.*

| | | | | | |
|---|---|---|---|---|---|
| Fraction Female | 0.469 | 0.417 | 0.435 | 0.442 | 0.426 |
| Fraction Austrian | 0.758 | 0.744 | 0.735 | 0.761 | 0.750 |
| Fraction Manufacturing | 0.174 | 0.244 | 0.242 | 0.279 | 0.235 |
| Fraction Blue-Collar | 0.427 | 0.474 | 0.507 | 0.512 | 0.514 |
| Age (yrs) | 38.937 | 38.918 | 38.411 | 38.175 | 37.997 |
| Firm-Tenure (yrs) | 6.270 | 6.365 | 6.071 | 6.074 | 5.780 |
| ln(Daily Wage) | 4.389 | 4.459 | 4.411 | 4.407 | 4.401 |
| Gender Wage Gap | 0.363 | 0.369 | 0.339 | 0.358 | 0.329 |
| Adj. Gender Gap | 0.257 | 0.259 | 0.242 | 0.246 | 0.244 |
| Separation Rate | 0.128 | 0.117 | 0.121 | 0.122 | 0.128 |
| Fraction Topcoded | 0.057 | 0.067 | 0 | 0 | 0 |
| N | 41,429,703 | 5,269,153 | 4,914,038 | 1,039,328 | 1,651,146 |
| # Workers | 5,784,925 | 1,242,885 | 1,204,251 | 328,134 | 529,099 |
| # Establishments | 539,254 | 14,495 | 14,303 | 4,949 | 9,265 |
| Dominant Employers | ✓ | ✓ | ✓ | ✓ | ✓ |
| 75≤ Firm Size ≤225 | | ✓ | ✓ | ✓ | ✓ |
| Top-coded Removed | | | ✓ | ✓ | ✓ |
| Treated Firms (≥150) | | | | ✓ | |
| Control Firms (<150) | | | | | ✓ |
| Year <2014 | | | | ✓ | ✓ |

gender, and citizenship. Each establishment has a unique identifier, and we merge with this data, information on its geographic location, 4-digit NACE industry classification, as well as (from 2007 onward) the firm size of the establishment's parent company. The information about overall firm size is crucial, since the law applies to firm size, and not establishment size. That being said, other than for the firm size definition, we use firms and establishments interchangeably throughout the text.

We select all employment spells from 2007-2018. For each worker-year pair, we select the dominant employer based on yearly income. This yields over 46 million person-year observations. Table 1 presents descriptive statistics about the overall employment population as well as our sample. For each worker-year observation, we compute the daily wage as yearly earnings from the dominant employer divided by the number of days employed at that establishment. We further deflate wages by CPI to 2017 prices. One caveat of the administrative data is that it does not contain information on hours worked. Thus, we are only able to analyze the response of total daily wages, and not the hourly wage response.

To make our control group as similar as possible to treated firms, we focus our analysis on firms that became subject to the law in 2014, i.e. firms with over 150 employees and to firms which fall slightly below this threshold. In our main sample, we select all firms between 75-225, but we consider robustness checks with other firm size windows as well. Large firms are likely very different from the small firms in the control group, both along observed and unobserved dimensions of worker and firm characteristics. Selecting firms in a narrow window around the cutoff makes them as comparable as possible.

Wage information is top-coded, as the social security administration only records income up to the maximum contribution limit. The maximum contribution limits are relatively high in Austria and only about 6 percent of our cases are top coded.[7] There is no perfect way to make up for this missing information. Since we are interested in wage changes following the reform and wage changes cannot be measured for top coded individuals, we decided to drop these spells in our main sample. Table 1 shows, that this selection does not change the worker composition much. We consider whether our results are robust to either including top coded individuals, or excluding workers from the analysis that were ever top coded during our study period.

These sample restrictions leave us with close to 4.9 million worker-year observations, generated by 1,204,251 workers employed across 14,303 distinct establishments. The worker and firm characteristics of our baseline sample are overall quite similar to the whole population. The only significant difference is perhaps that manufacturing jobs are somewhat overrepresented in the baseline sample. They comprise 24 percent of all jobs, whereas the manufacturing share in the overall population is only 17 percent.

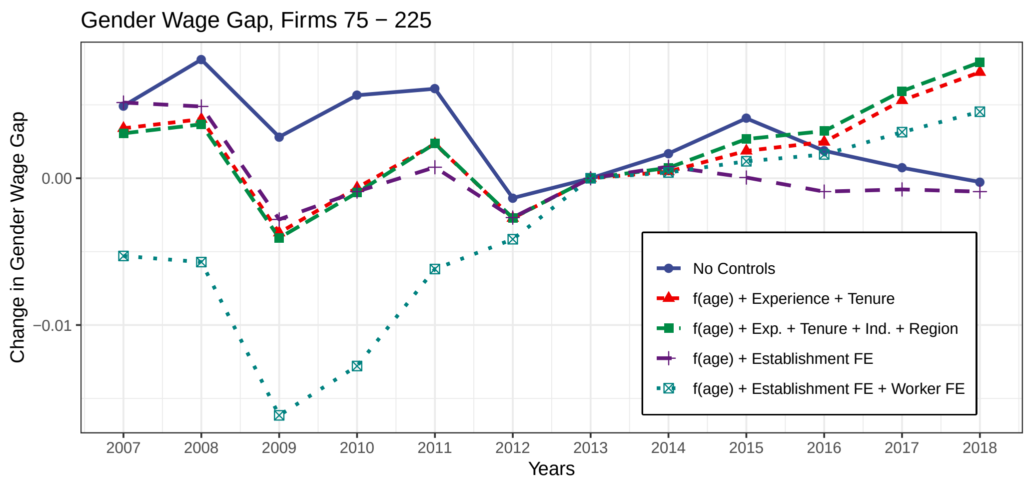
In our main sample, we specify treatment status based on the firm size in December 2013, just before firms with 150-250 employees became subject to the policy in 2014. The last two columns in Table 1 compare the sample characteristics of the treatment and control establishments in the years before the policy was rolled out. The two groups have very similar worker and firm characteristics. The treated establishments have a slightly higher share of jobs in manufacturing, a higher average tenure by about a quarter of a year, and have a slightly higher gender wage gap (36% versus 33%). Figure 1 shows that that while the unadjusted gender wage gap (GWG) in our final sample is stable over time, once we control for worker and firm fixed effects, the GWG is increasing over time. Also notable is the small dip in the GWG during the financial crisis of 2009.

The explicit goal of the Austrian Pay transparency legislation is to narrow the gender pay

[7] In 2016, the maximum monthly earnings used to calculate contributions was €4,860. There were no substantial changes in the maximum contribution threshold in Austria during our study period. It was essentially only valorized each year by the inflation rate.

Figure 1: Evolution of the unadjusted and adjusted gender wage gap in our baseline sample

*The figure below shows the normalized (in 2013) gender wage gap in Austria over our sample period with an expanding list of controls. The sample includes only firms which had between 75 and 225 employees in 2013.*




gap originating from different firm pay schedules across gender. Before we discuss our empirical strategy to evaluate the effects of pay transparency, we estimate firm pay differences between men and women in treated firms before the roll-out of the policy in 2014 to understand how much potential bite the reform might have had. Prior research has shown that differences in firm pay explain 30 percent of overall wage variation in Austria (Gulyas and Pytka, 2020). Average firm pay premia might be different between genders because of two reasons. First, men and women might work for firms with different firm pay premia. Second, differences might originate from different pay policies within firms. We follow Card et al. (2016) to decompose firm pay differences into a sorting effect capturing that men and women work for different firms, and a within firm component. For this we estimate the seminal two way fixed effect model following Abowd et al. (1999), but we allow firms to have different pay policies across gender:

$$w_{it} = \alpha_i + \psi_{J(it)}^{G(i)} + X_{it}^{G(i)} + \epsilon_{it}. \quad (1)$$

$w_{it}$ is the log-wage of individual $i$ in period $t$, $G(i)$ denotes gender, $\alpha_i$ captures a fixed worker component (due to ability or skills) and $X_{it}^{G(i)}$ is a gender specific quartic polynomial of age. $\psi_{J(it}^{G(i)}$ captures the gender specific pay policy of firm $J(i,t)$. The firm fixed effects obtained by equation (1) measure the firm pay premia relative to a reference firm for female and males. We follow Card et al. (2016) and normalize the firm effects by assuming that firms in the restaurant

Table 2: Decomposition of pre-treatment gender wage gap into *Within-Firm* and *Sorting Effects.*

*The table below shows the decomposition of pre-2014 gender wage gap into a within-firm effect and a sorting effect, following Card et al. (2016). The two rows show this decomposition using female and male job distributions respectively.*

| | | Within Firm | | Sorting | |
|---|---|---|---|---|---|
| | Gender Wage Gap | Level | Share | Level | Share |
| Decomposition (Female dist.: Eq. (2)) | 0.127 | 0.036 | 0.281 | 0.092 | 0.719 |
| Decomposition (Male dist.: Eq. (3)) | 0.127 | 0.049 | 0.387 | 0.078 | 0.613 |

industry have zero firm pay premia.[8] This assumption is motivated by an extensive literature showing that the restaurant industry has the smallest wage premia on average.[9] We then decompose the difference between the average pay premium received by men $E[\psi^M_{J(it)}|male]$ and women $E[\psi^F_{J(it)}|female]$ into a within firm component and a sorting effect:

$$E[\psi^M_{J(it)}|male] - E[\psi^F_{J(it)}|female] =$$

$$\underbrace{E[\psi^M_{J(it)} - \psi^F_{J(it)}|female]}_{\text{Within Firm}} + \underbrace{E[\psi^M_{J(it)}|male] - E[\psi^M_{J(it)}|female]}_{\text{Sorting}} = \tag{2}$$

$$\underbrace{E[\psi^M_{J(it)} - \psi^F_{J(it)}|male]}_{\text{Within Firm}} + \underbrace{E[\psi^F_{J(it)}|male] - E[\psi^F_{J(it)}|female]}_{\text{Sorting}} \tag{3}$$

The first term in equation (2) is the gender gap in firm pay policy within firms across the distribution of jobs held by women. The second term gives the difference originating from the fact that women and men work for different firms, which we label as the sorting effect. Equation (3) presents the same decomposition, but using the male job distribution instead.

Table 2 presents the results of this decomposition. The gender gap in firm pay policy is on average 12.7 percent. Independently whether we use the job distribution of men or women, the majority of the differences is due to men working for higher paying firms on average. In contrast, we find that the within-firm gender gap in firm pay policy in treated firms amounts to 28 to 38 percent of the overall gap. Thus, even though the raw gender wage gap is above 30 percent in treated firms, the policy can only addresses the 3.6 to 5 percent pay differences originating from unequal pay within firms. To estimate how much the transparency policy was successful to close this gap, we exploit the exogenous variation induced by the pay transparency legislation across time and the smallest firm size cutoff. The next section discusses our empirical

[8]Specifically, we use the restaurants and mobile food service activities (NACE 5610) category

[9]See for example Krueger and Summers (1988)

strategy in detail.

# 4 Empirical Strategy

To estimate the causal effect of pay transparency on the gender wage gap as well as on male and female wages we apply the following event-study model:

$$
\begin{aligned}
y_{ij(i,t)t} = \lambda_i + \lambda_j + \lambda_t + \sum_{k=2007}^{2018} \beta_1^k \mathbf{1}[t=k] * \mathbf{I}_i^m * Treat_{j(i,2013)} + \sum_{k=2007}^{2018} \beta_2^k \mathbf{1}[t=k] * Treat_{j(i,2013)} \\
+ \beta_3 \mathbb{I}_i^m * Treat_{j(i,2013)} + \sum_{k=2007}^{2018} \gamma_k \mathbf{1}[t=k] * \mathbb{I}_i^m + \varphi X_{it} + \epsilon_{ij(i,t)t}, \qquad (4)
\end{aligned}
$$

where $i$ denotes a worker employed in establishment $j(i,t)$ in calendar year $t$. $\mathbf{1}[t=k]$ is a year dummy that takes the value one if $k$ equals $t$ and zero otherwise. $\mathbb{I}_i^m$ denotes the gender dummy that takes the value one if individual $i$ is male. $Treat_{j(i,2013)}$ denotes the treatment indicator which equals one if an establishment belongs to a firm which has 150 to 225 employees in 2013 and zero otherwise[10]. $X_{it}$ is a vector of individual, time-varying controls: It contains a quartic polynomial in age and its interaction with gender. $\lambda_i$ denotes the individual worker fixed effect. $\lambda_j$ and $\lambda_t$ respectively denote the establishment and calendar year fixed effects. Our outcome variable of interest is the log of daily wages at the worker-establishment-year level. The event-study coefficients $\beta_1^k$ on the triple interaction term measure the percentage points change in the gender wage gap in treated establishments relative to the control group and a given base year. In addition, we are interested in the effects of pay transparency on male and female wages separately. The gender specific effects are measured with the coefficients $\beta_2^k$ for females and $\beta_1^k + \beta_2^k$ for males. Consistent with our treatment status assignment, we choose 2013 as our base year, the last pre-reform year for establishments in the smallest treatment group. We focus on the smallest cutoff to ensure that treated and control establishments are comparable.

[10] Assigning the treatment status based on the 2013 firm size is equivalent to estimating an intent-to-treat effect. To account for initial-treatment status violators in post-reform years, we consider a robustness exercise by estimating equation (4) for only those firms that comply with their initial treatment assignment, thus not exceeding (dropping below) the 150 employee cutoff post 2013. We refer to this sample as the "Complier Sample".

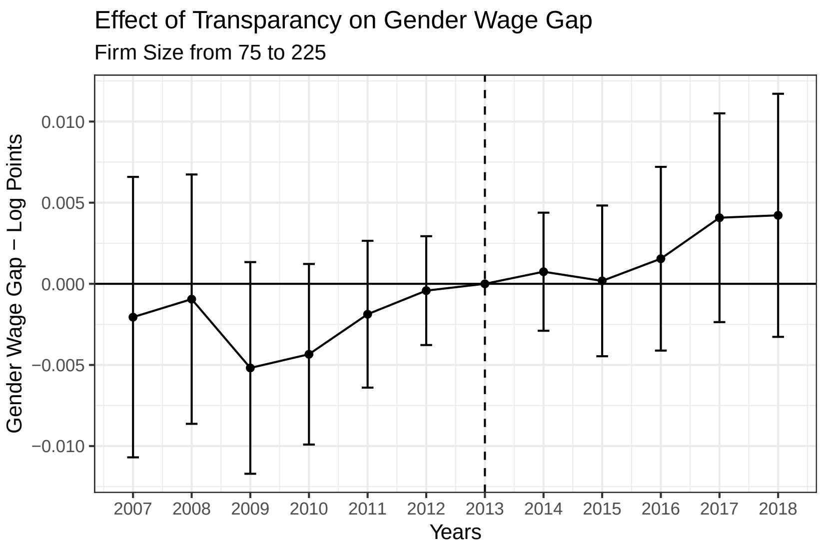
# 5 The Effects of Pay Transparency

## 5.1 Effects on Gender Wage Gap and Wages

The explicit goal of the Austrian pay transparency law was to reduce the gender wage gap. We therefore begin by examining its effect on the gender gap in daily wages. Figure 2 shows the estimated coefficients $\beta_1^k$ from equation (4), which measure the evolution of the gender wage gap (male-female) in treated establishments relative to those in the control group. We find little evidence for any significant and economically meaningful effects on the gender wage gap. At the 95% confidence level we can rule out that the policy narrowed the gender wage gap by more then 0.3 p.p. by the end of our study period. Although there is a small dip in the wage gap during the 2009 crisis, there is no clear pre-trend before 2014. Appendix Figure A2 shows that this dip is visible for both treated and untreated establishments and it is only somewhat more pronounced in treated establishments. By 2014, the difference in the gender wage gap had recovered to their pre-recession levels.

Figure 2: Effects of Pay Transparency on Gender Wage Gap

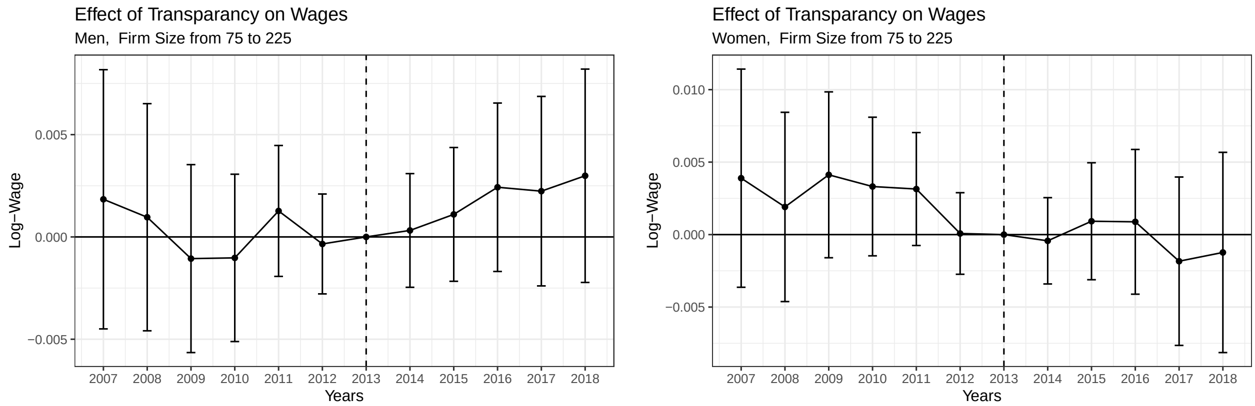
*The figure below plots the evolution of the gender gap in daily wage, in treated establishments relative to the control group in log points, for the smallest firms in 2014. We use the specification in (4). The sample is restricted to firms with 75-225 employees. Treatment is assigned to firms which had more than 150 employees in 2013. Standard errors are clustered at the establishment-year level. The standard error spikes represent 95% CI.*




Next, we investigate whether transparency has effects on female or male wages separately. For this we plot the respective coefficients in specification (4), i.e. $\beta_2^k$ for female and $\beta_1^k + \beta_2^k$ for male workers. The results are shown in Figure 3. Overall, we do not find any evidence

Figure 3: Heterogeneous Effects of Pay Transparency By Gender

*The figure below shows the evolution of male (left panel) and female (right panel) daily wages in treated firms relative to the control group in log points before and after the introduction of the pay transparency law for smallest firms in 2014. The sample is restricted to firms with 75-225 employees. Treatment is assigned to firms with more than 150 employees in 2013. Standard errors are clustered at the establishment-year level. The standard error spikes represent 95% CI.*




(a) Male $log$(Daily Wage) (b) Female $log$(Daily Wage)

that the pay transparency legislation has any effect on wages. Female wages are virtually unchanged after 2013, whereas male workers in treated firms have seen a modest increase of 0.25 p.p. compared to the control group. Both effects are statistically insignificant, although the effects are precisely estimated. At the 95% confidence level, we can rule out that the reform affected wages by more than 0.5 percent in the years immediately after the roll-out and by more than 0.8 towards the end of our study period. Overall, there is little evidence to suggest that transparency has any economically significant effects on female workers.

Pay reports are only available to current employees, thus the reports might have a limited impact on wages of newly hired employees. Even after joining a company with a pay report, it might take some time until the employee is able to act upon the information provided in the wage reports and renegotiate their contract. To investigate whether this group drives our zero results, we split the sample into workers with below and above median firm-tenure and re-estimate equation (4) on these two subgroups. Appendix Figure A4 shows we still find no discernible effects of pay transparency on wages for neither low, nor high tenure workers.

#### 5.1.1 Robustness Checks

In Appendix A, we conduct multiple robustness checks for the effects of transparency, by changing our sample and treatment definitions. First, Appendix Figure $A5$ shows the effects of transparency on the gender wage gap and male and female wages for firms in the Complier

Sample, where we condition on firms that did not violate their intended treatment assignment after 2013. These event studies mirror the results from the baseline specification. We do not find any statistical or economically significant effects either on the gender wage gap, or on wages in general. Appendix Figure $A6$ shows the effects for the set of firms which had between 100 and 200 employees in 2013, with treatment assigned to firms with more than 150 workers. In Appendix Figure $A7$ we include workers whose wages were top-coded, and in Appendix Figure $A8$ we drop all workers whose wages were ever top-coded during our sample period. These three event studies show the same results from our main sample: modest, yet insignificant increases in the gender wage gap, concentrated among male workers (if any effects at all). In Appendix Figure $A9$ we define treatment at the worker level. Any worker who was employed in a firm with more than 150 employees in 2013, is assigned to the treated group regardless of their employer identity after 2013. These event studies show that transparency led to modest increase of about 0.25 p.p. in wages for both men and women. Again these effects are not statistically significant.

Second, Appendix Table B2 shows that the results are robust to inclusion of different control variables. Our conclusions remain unchanged if we do not control for worker fixed effects or gender specific age profiles, or if we use match fixed effects instead of worker and firm fixed effects.

Finally, we investigate whether firms strategically changed firm size to avoid becoming subject to the law. Overall, we find little evidence of strategic bunching by firms around the 150 employee threshold. While about 10% of establishments below this cut-off upsize and become subject to the policy in 2014 and afterwards, this number is no different from the general size dynamics of firms in this group, even before the policy was implemented. Appendix Figures A10 and A11 report these results in detail. As a further robustness exercise, Appendix Figure A12 shows that the results are unchanged when we define treatment status based on firm size in 2010.

### 5.1.2 Pay Transparency and Wage Dispersion

What explains the lack of any discernible effects of transparency on male and female wages? Perhaps the policy only led to wage compression, leaving the average wage unaffected. Wage increases for workers earning below average might have been compensated by wage reductions for well paid individuals. To see whether this was indeed the case, we re-estimate our equation separately for workers earning below and above the gender-specific median establishment level wage.[11] The results from these regressions are shown in Figure 4.[12] All in all, we do not find compelling evidence for wage compression within establishments. The only sustained effects we find are for men who earn above the male median wage at the establishment-level. For this group of workers, transparency increased wages by a minuscule 0.4 p.p. (significant at 90% levels), while women who earn above the median wage for female workers, do not experience such changes. Transparency does not seem to have any effect on wages for highly paid women after the reform took effect. For both male and female workers who earn below their gender-specific mean, there is little evidence to suggest that they see any changes in their daily wages. In Appendix Figure $A13$ we show that these heterogeneous effects on men and women translate into a marginal increase in the gender wage gap for the sample of workers who earn above their gender-specific establishment-level median wage. There is no evidence for any effects on those who earn below this median.

In a final specification, we study the effect of the pay transparency on the establishment-level variance in male and female wages separately by estimating the following model:

$$wvar_{jt} = \lambda_j + \lambda_t + \sum_{k=2007}^{2018} \beta^k \mathbf{1}[t = k] * Treat_{j(2013)} + \epsilon_{jt}, \tag{5}$$

where $wvar_{jt}$ is the gender-specific variance in daily wages in establishment $j$ in year $t$, $Treat_{j(2013)}$ is a dummy which takes the value 1 for any establishment $j$ which belongs to a firm which had more than 150 workers in 2013, and the other variables have the same interpretation as in (4). The results are displayed in Figure 5. Again, we do not find any discernible effect of the policy on the variance of log-wages within treated establishments[13].

### 5.1.3 Why Was the Reform not Effective?

Our results show that the pay transparency law failed to narrow the gender wage gap and did not affect wage setting in general. Can we attribute this ineffectiveness to incomplete implementation or to the lack of discrimination in the first place? As we have already discussed in section 2, the Austrian policy is stricter in many aspects than comparable laws in Europe and there was near-universal compliance with the transparency policy. First, according to a

[11]The dataset only contains an establishment identifier, thus we cannot compute a firm level median wage. According to Schrittwieser (2014), 48 percent provide information not only at the firm level, but further itemize the wage reports by area of activity. The subgroups above and below median are defined on the basis of the establishment-level median wage in 2013.

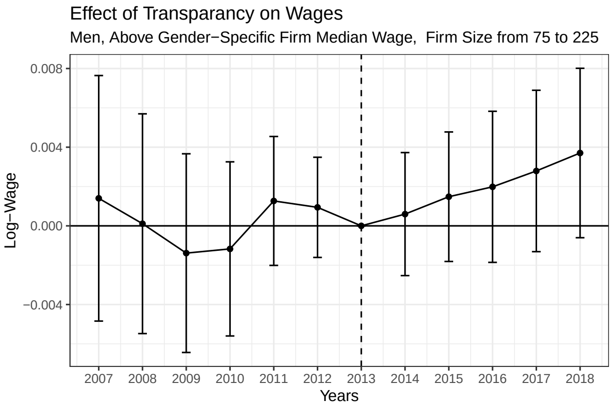
[12]The results for gender wage gap are shown in Appendix Figure $A13$

[13]Including establishment-year level aggregates in (5) does not change our results.

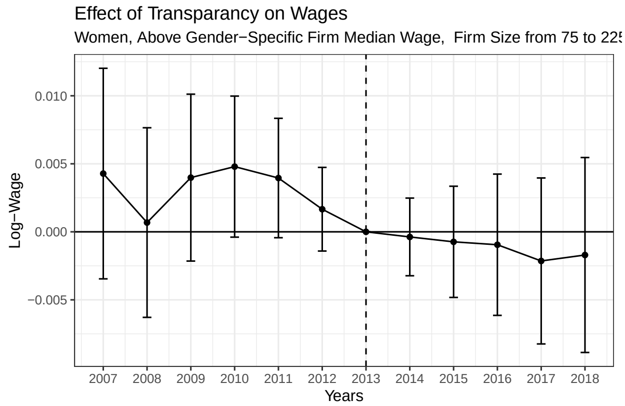
Figure 4: Gender-Specific Effects of Transparency on Daily Wages

**[Above/Below Firm-Level Gender-Specific Median Wage]**

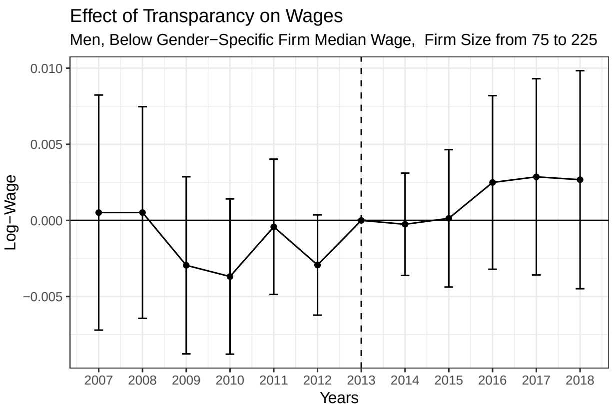
*The figure below plots the effects of transparency on male and female wages, for workers who earn above and below their gender-specific firm-level median wage in 2013, the year before treatment. Standard errors are clustered at establishment-year level. The standard error spikes represent 95% CI.*




(a) Above Median Male




(b) Above Median Female



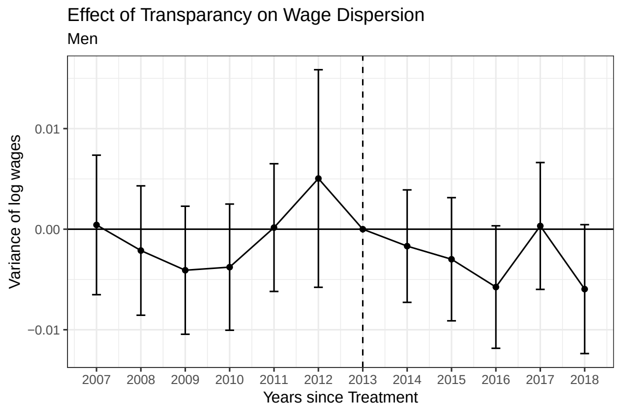

(c) Below Median Male


(d) Below Median Female

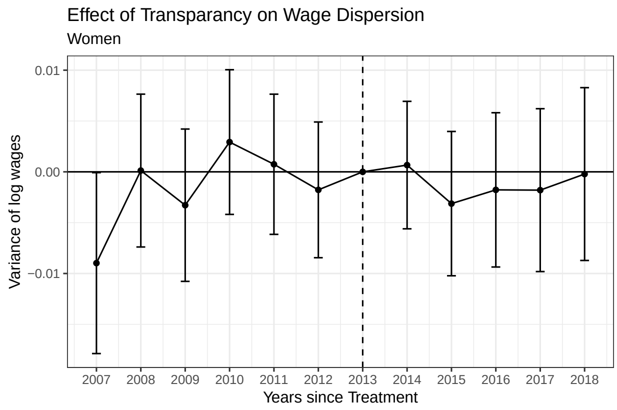
survey of work councils (Schrittwieser, 2014), in 54% of cases the company cooperated with the works council in generating these pay reports. 71% of workers reported that the reports are informative and 63% claimed that they are useful for work councils. Taken together, the reform clearly provided important information about pay structures within the firm. Nevertheless, one explanation for why the reform did not affect wages is that the revealed information was already privately available to workers. A second candidate explanation could be that although the reform revealed discriminatory pay schedules, workers lack the bargaining power to renegotiate wages. Third, it is also possible that the pay transparency legislation alleviated prior concerns about unfair worker compensation. In terms of wage implications, these three explanations are observationally equivalent, but call for very different policy responses. One possible way to distinguish these three channels is to study job satisfaction. Past research has

Figure 5: Effects of Pay Transparency on Establishment-level Wage Variance

*The figure below plots the effects of transparency on the establishment-level variance in daily wages for male and female workers separately. The standard error spikes represent 95% confidence intervals.*




(a) Male




(b) Female

shown that workers who feel unfairly compensated have lower job satisfaction and a higher quit rate (Card et al. (2012), Rege and Solli (2015), Dube et al. (2018)). Therefore, if the pay reports do not reveal any additional information, job satisfaction and quit rates should not be affected. If the reports show evidence of pay discrimination, but workers lack the bargaining power to renegotiate wages, we would expect job satisfaction to decline. In contrast, we would expect job satisfaction to increase if transparency leads workers to revise downwards their priors about unfair compensation. The social security data does not have a direct measure of job satisfaction, but we use turnover rates as a proxy. Since pay reports are internal, we would not expect workers' outside options to change and therefore to confound effects on quit rates. To study this channel, we estimate the effect of the policy on overall job separation rates by dropping the additional gender interaction from equation (4):

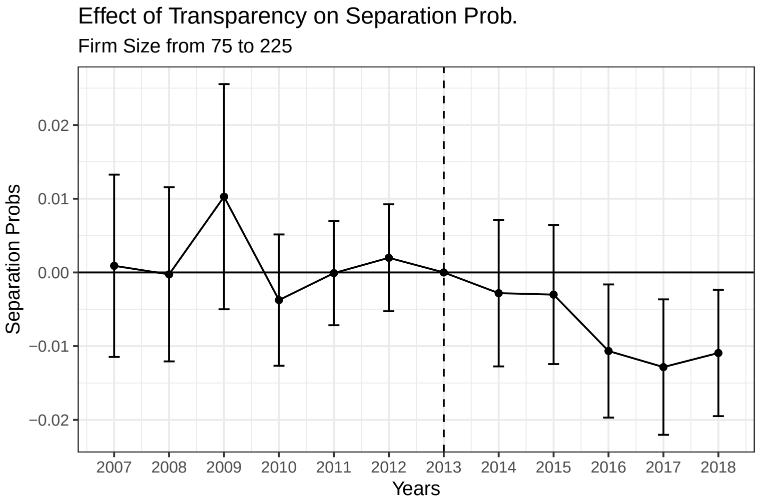
$$sepa_{ijt} = \lambda_j + \lambda_i + \lambda_t + \sum_{k=2007}^{2018} \beta^k \mathbf{1}[t = k] * Treat_{j(i,2013)} + \varphi X_{it} + \epsilon_{ij(i,t)t}, \tag{6}$$

where $sepa_{ij(i,t)t}$ is one if individual $i$ separated in period $t$ from establishment $j$ and the rest of the variables follow the same definitions as in the baseline equation (4). We also estimate the gender specific effects of transparency on job separation using the specification of equation (4). Figure 6 shows that the transparency policy reduced the separation rate significantly in treated firms relative to the control group by over 1.1 p.p., which is a 9 percent reduction compared to pre-treatment levels.[14] In Appendix Figure $A14$ we show the that these effects are uniform across genders. We interpret this as suggestive evidence that although transparency

[14]The separation rate is 0.122 in treated firms before the reform, see Table 1

Figure 6: Effects of Transparency on Job Separation Rate

*The figure below plots the effects of pay transparency on the year-on-year job separation rate. The sample is restricted to firms with 75-225 employees in 2013, and we pool male and female workers. Standard errors are clustered at the establishment level. The standard error spikes represent 95% confidence intervals.*




does not affect wage setting in Austria, it alleviates concerns about unfair compensation for both genders and thus leads to a signification reduction in turnover rates.

#### 5.1.4 Effects of Transparency on Firms with High Gender Disparity

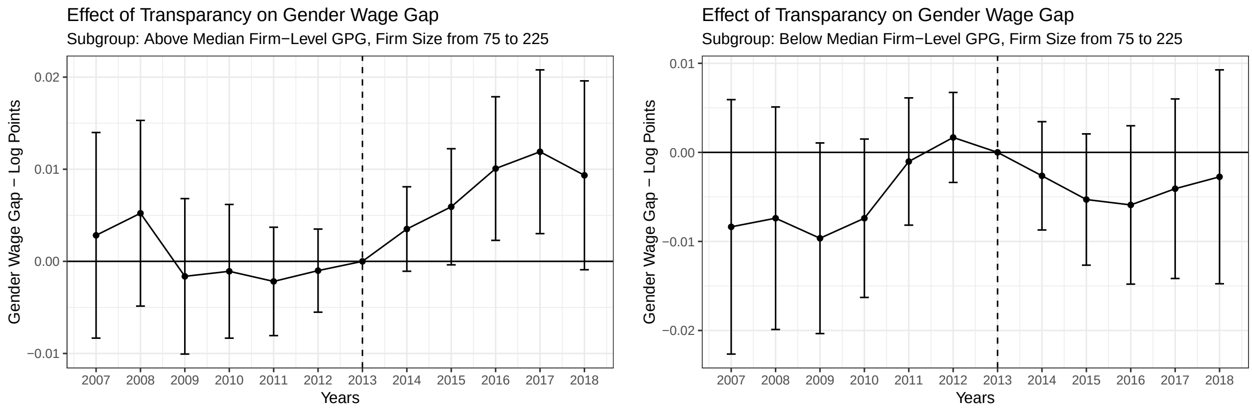
In previous sections we show that pay transparency does not have any effects on the overall gender wage gap in Austria. Perhaps, it helps to close the pay gap in establishments with higher gender disparity. We consider two proxies for gender disparity: (a) whether an establishment has above median establishment-level unadjusted gender wage gap (Figure 7) and (b) whether the establishment has below median female share in its workforce (Figure 8). To construct these subgroups we compute the relevant statistic (i.e. median establishment-level unadjusted GWG and median establishment-level share of female workers) in 2013, the year before treatment and then assign establishments to the above or below subgroups.

Figure 7 shows the effects on gender wage gap in establishments with above (left panel) and below (right panel) median establishment-level unadjusted gender wage gap (GWG) in 2013. Gender wage gap actually increases in establishments which had more than the average GWG, by about 1 p.p. (significant at 95% levels). Moreover, these effects are not driven by any pre-trends. In contrast, for establishments below the median GWG, there are strong upward trends in the wage gap before 2013, and transparency seems to have arrested this uptick. If at all, gender wage gap for these establishments actually decreased by 0.4 p.p. These results are

Figure 7: Effects of Transparency on Gender Wage Gap (GWG)

**[Above and Below Median Establishment-level GWG]**

*The figure below plots the effects of pay transparency on the gender wage gap for establishments with above (top panel) and below (bottom panel) median of establishment-level gender wage gap in 2013, the year before treatment. The sample is restricted to firms with 75-225 employees in 2013. Standard errors are clustered at the establishment-year level. The standard error spikes represent 95% CI.*




(a) **Above Median** (b) **Below Median**

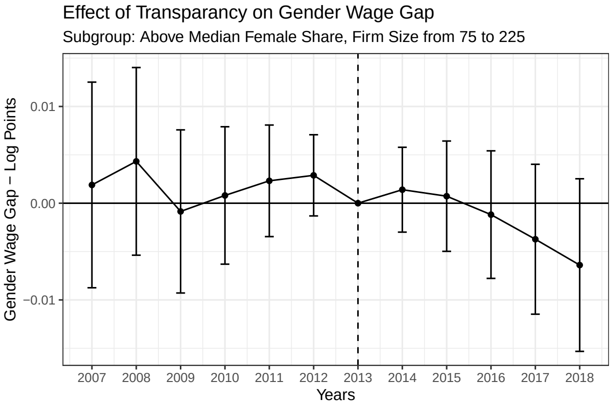
however not significant. In Appendix Figure $A15$ we show that the increase in gender wage gap in establishments with above median wage gap is driven both by an increase in male wages (*panel a*) and a decrease in female wages (*panel b*) by roughly in the same proportion. For firms with below median baseline gender wage gap, we do not find any evidence for effects on male wages (*panel c*). Although there appears to be some downward pre-trends in female wages (*panel d*), transparency seems to have induced trend reversal by actually increasing female wages by about 0.4 p.p.. Again, these results are not statistically significant. In summary, it appears that instead of reducing the overall gender wage gap, transparency seems to have widened the disparity between establishments with high and low baseline wage gaps. More importantly, women saw marginal (though not statistically significant) reduction in wages, especially in establishments which already had higher than average wage gaps.

Figure 8 shows the effects of transparency on establishments with higher (left panel) and lower (right panel) than median establishment-level share of women in the workforce in 2013. The gender wage gap declines by 0.75 p.p. (significant at 90% levels) by the end of our sample period in establishments which higher than median female share. In contrast, gender wage gap seems to have worsened in establishments which had below the median firm-level female share. By 2018, gender wage gap in this subgroup increased by more than 1 p.p. (significant at 90% levels). In Appendix Figure $A16$ we decompose the effects on gender wage gap, into effects on male and female wages separately. The reduction in gender wage gap in establishments with higher than median female share, seems to be driven by decrease in male wages (*panel a*), and

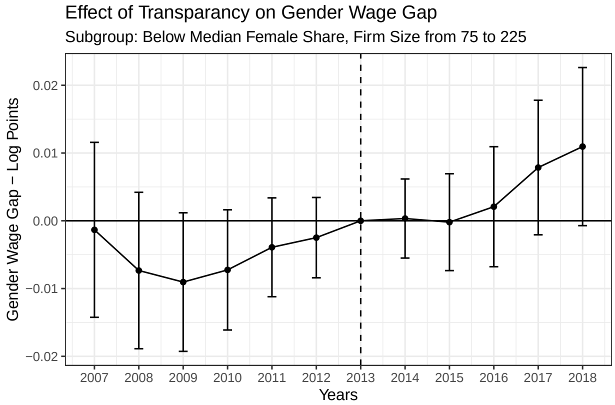
Figure 8: Effects of Transparency on Gender Wage Gap (GWG)

**[Above and Below Median Establishment-Level Female Share]**

*The figure below plots the effects of transparency on the gender wage gap, separately for firms which had above (top panel) and below (bottom panel) the median establishment-level share of female workers. The sample is restricted to firms which had between 75 and 225 employees in 2013, the year before treatment. Standard errors are clustered at the establishment-year level. The standard error spikes represent 95% CI*




(a) **Above Median**




(b) **Below Median**

not by an increase in female wages (*panel b*). In contrast, the increase in gender wage gap in establishments with less than median female share is driven both by a marginal increase in male wage (*panel c*) and a marginal decrease in female wages (*panel d*) at least in the last two years of our sample period. These effects are not statistically significant. Even if we rule out any adverse effects of transparency on individual wages, positive effects are concentrated only among male workers.

The two subgroup analyses in Figures 7 and Figures 8 indicate that establishment-level 'culture' is possibly instrumental in driving the aforementioned results. If an establishment already had a higher than average gender wage gap, then presumably men had more bargaining power in the workplace. Publication of gender and occupation-specific averages only increases men's bargaining power, but has little impact on how women bargain in these establishments. In contrast, if establishments had higher than the average female share, then women would benefit from transparency presumably because they have more bargaining power than their male colleagues. Without any data on what establishment-level 'culture' would entail, it is difficult to substantiate these channels.

# 6 Conclusion

Pay transparency is often prescribed as an instrument to close the gender pay gap. In this paper we study the causal effects of the 2011 Austrian pay transparency law which requires firms above a certain size threshold to publish reports on gender pay gap.

Using a difference-in-difference design and administrative data from social security records, we show that the transparency policy failed in its twin objectives of reducing the gender pay gap, and boosting female wages. We find that pay transparency neither narrowed the gender wage gap, nor affected male and female wages. Our estimates are precisely estimated, we can rule out at a 95% confidence level that the policy narrowed the gap by more than 0.3 p.p. by the end of our study period. We further show that this zero effect is not driven by wage compression, where wage increases below the median are compensated with wage cuts above the median. Past research has shown that workers who feel unfairly compensated have lower job satisfaction and a higher quit rate (Card et al. (2012), Rege and Solli (2015), Dube et al. (2018)). In Austria we find pay transparency leads to a reduction in separation rates in treated firms, which point towards higher job satisfaction. We interpret this as suggestive evidence that workers do not perceive the revealed pay schedules as unfair, which in turn leads to higher job satisfaction and lower quit rates.

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# Appendix A: Additional Figures

Figure A1: Proportion of Workers Employed in Treated Firms

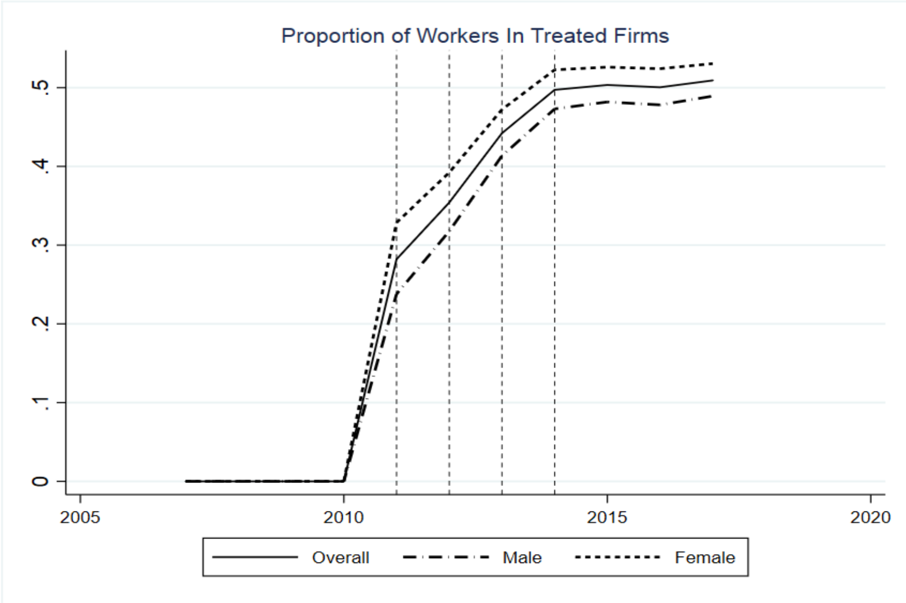



Figure A2: Effects of Pay Transparency on Adjusted Gender Wage Gap (By Treatment Status)

*The figure below shows the evolution of the gender wage gap, separately for the treated and control group of firms. The sample includes only firms which had between 75 and 225 employees in 2013, the year before treatment. Firms which had more than 150 employees in 2013 were assigned to treatment status, and others to the control group.*

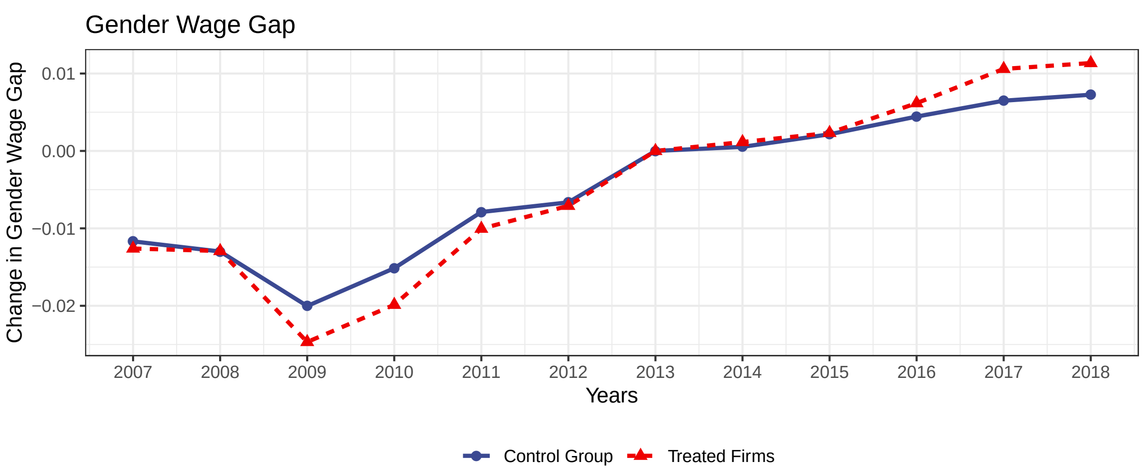


Figure A3: Evolution of Gender Wage Gap in Austria

*The figure below shows the evolution of gender wage gap (male − female) in log points over time for all firms.*

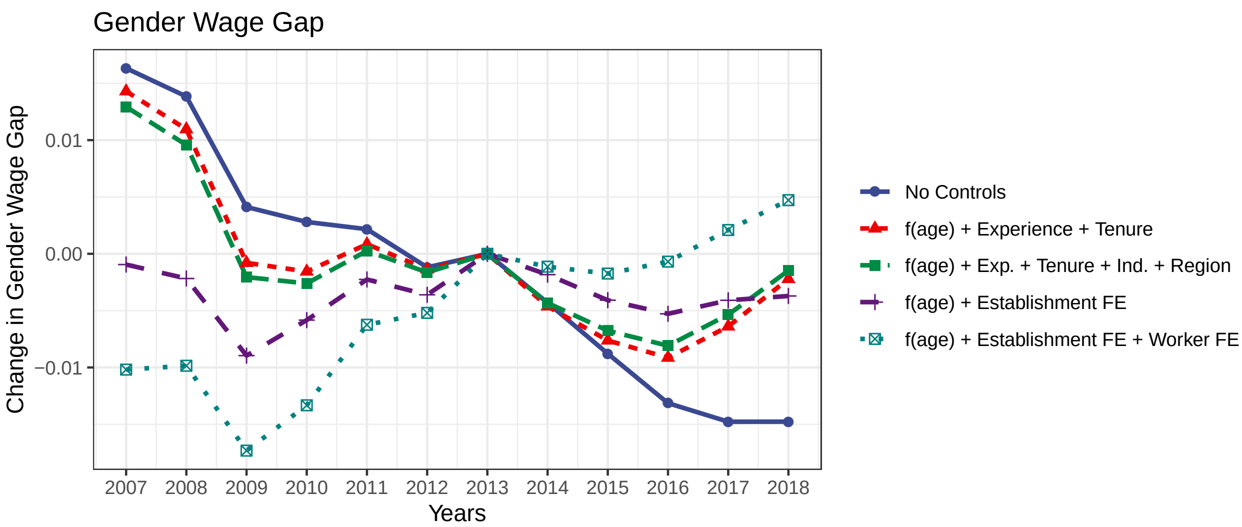
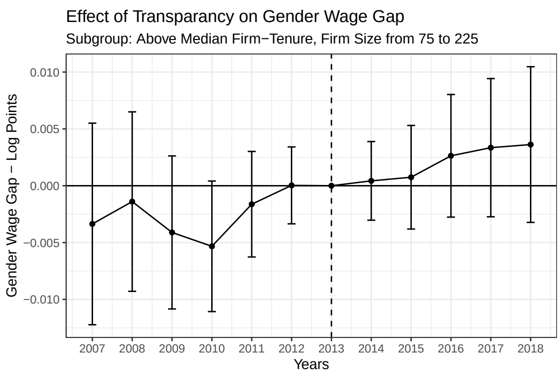


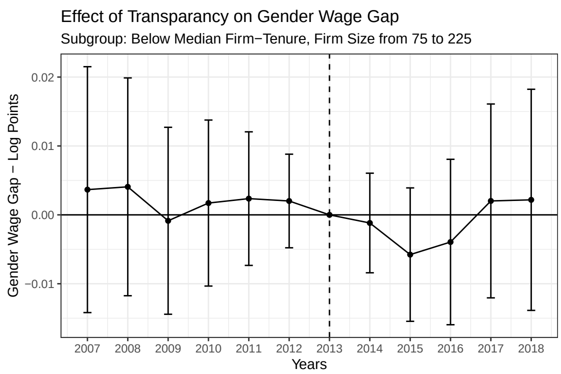

Figure A4: Effects of Transparency on Gender Wage Gap (GWG)

**[Above and Below Median Firm Tenure]**

*The figure below plots the effects of pay transparency on the gender wage gap for workers with above (left panel) and below (right panel) median firm tenure. The sample is restricted to firms with 75-225 employees in 2013. Standard errors are clustered at the establishment-year level. The standard error spikes represent 95% CI.*



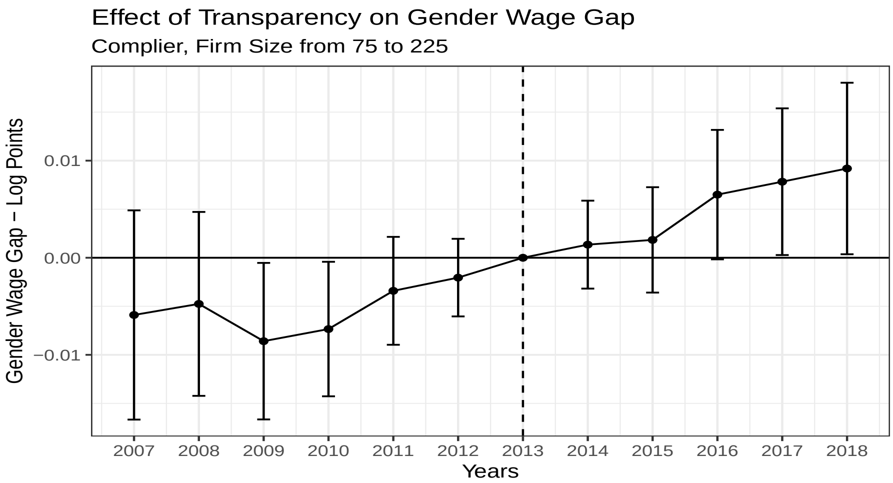

(a) **Above Median**



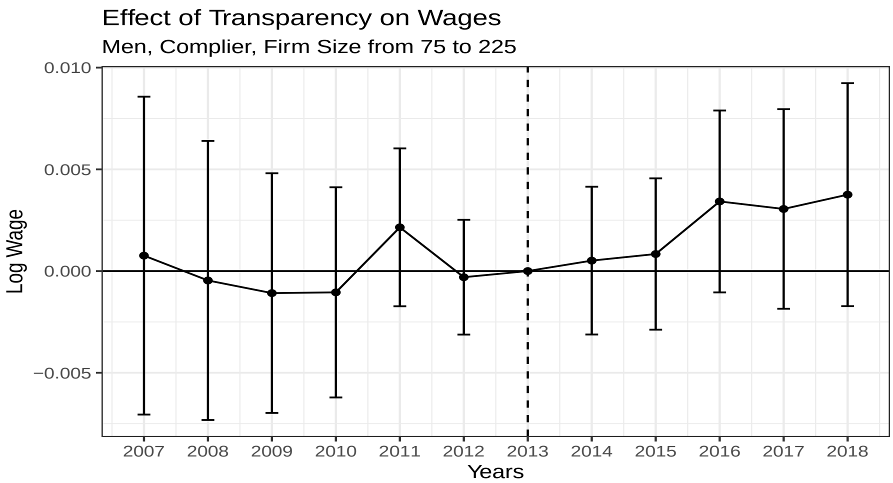

(b) **Below Median**

Figure A5: Effects of Transparency on Daily Wage and GWG (Complier Sample)

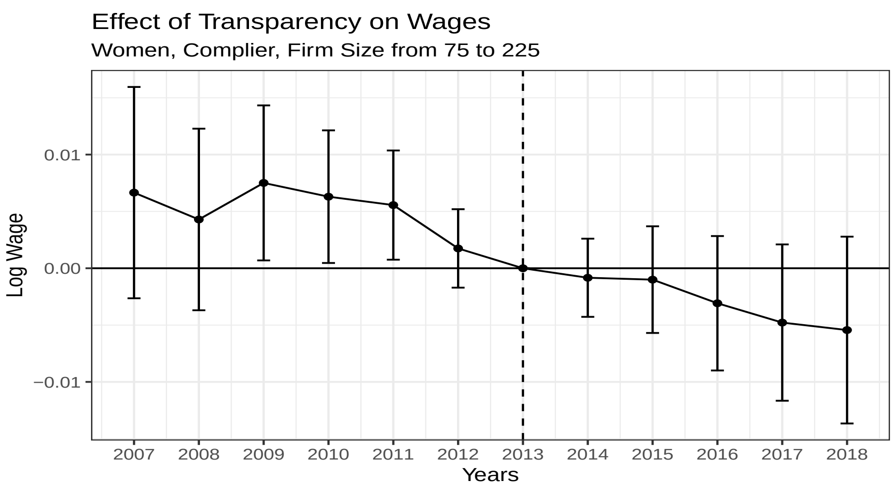
*The figure below plots the effects of transparency on gender wage gap (panel (a)), and male (panel (b)) and female (panel (c)) separately, for those firms which do not change their treatment assignment after 2013. The sample includes only firms with 75-225 employees in 2013. Standard errors are clustered at the firm-year level. The standard error spikes represent 95% CI.*




(a) Gender Wage Gap



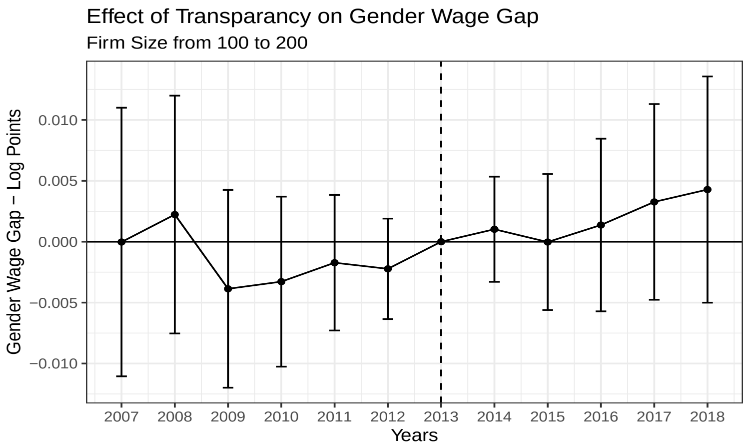

(b) Male Daily Wage



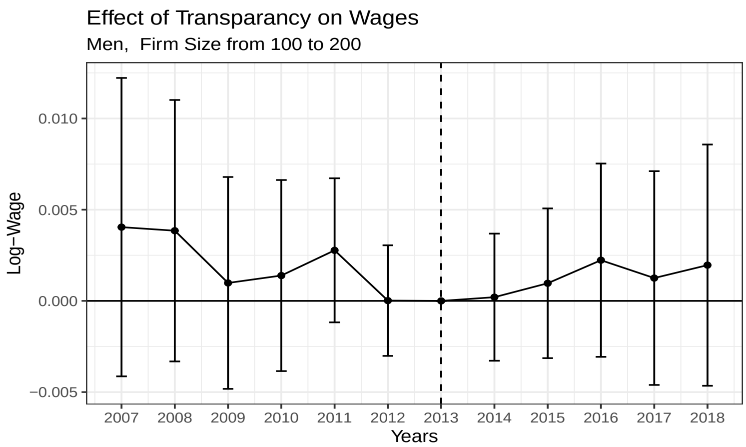

(c) Female Daily Wage

Figure A6: Effects of Transparency on GWG and Daily Wage (100<=Firm Size<=200)

*The figure below plots the effects of transparency on gender wage gap (Panel (a)), and daily wages for male (Panel (b)) and female (Panel (c)) workers separately, in firms which had between 100-200 employees in 2013. Treatment is assigned to firms which had more than 150 workers in 2013. Standard errors are clustered at firm-year level. The standard error spikes represent 95% CI.*




(a) Gender Wage Gap

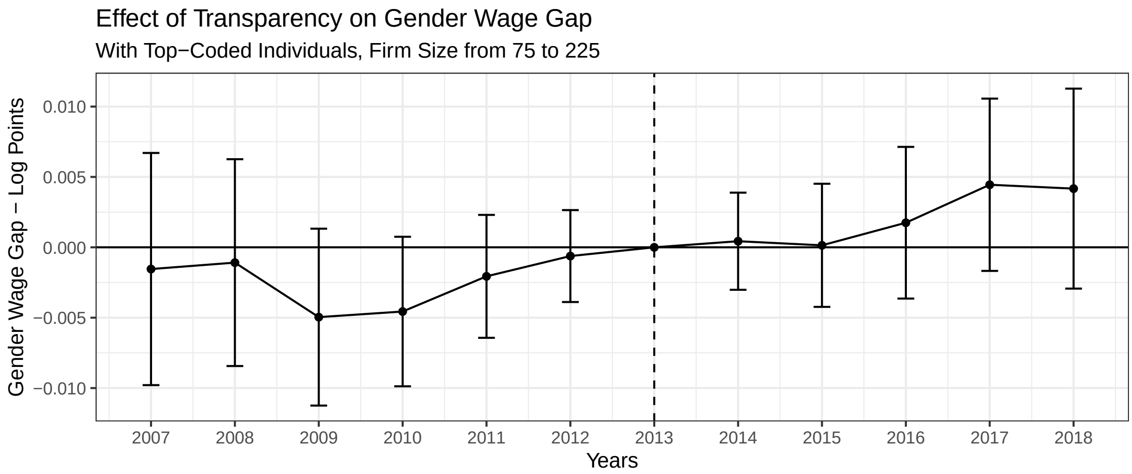



(b) Male Daily Wage

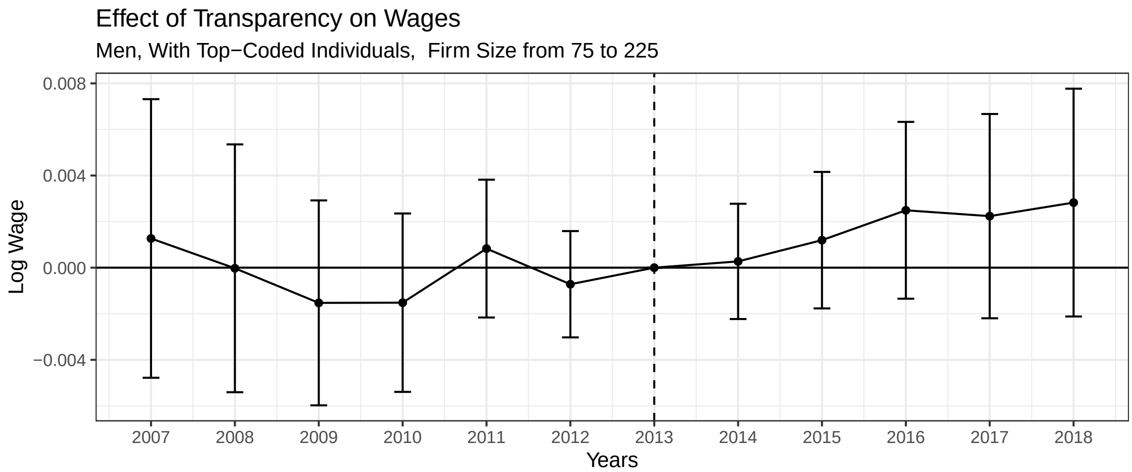

(c) Female Daily Wage

Figure A7: Effects of Transparency on GWG and Daily Wage (With Top-Coded)

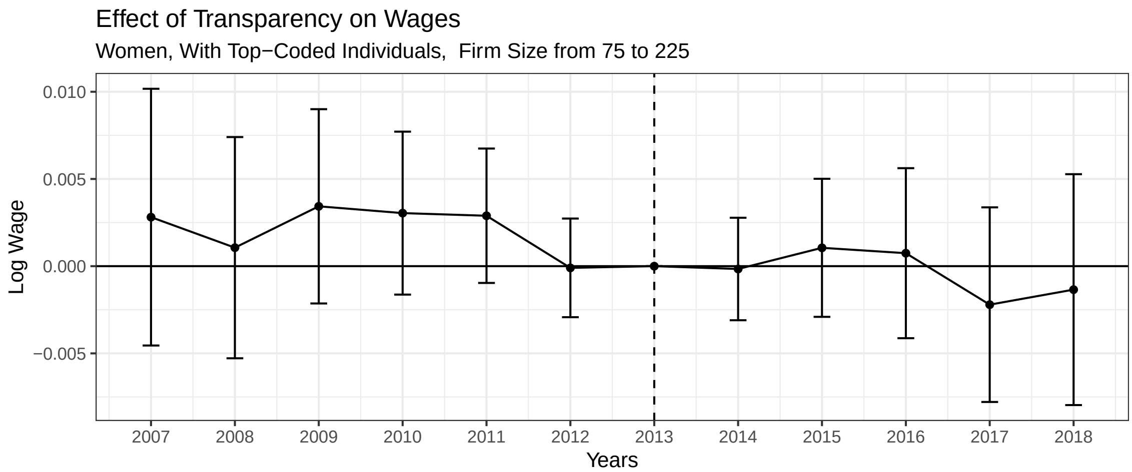
*The figure below plots the effects of transparency on gender wage gap (Panel (a)), and daily wages for male (Panel (b)) and female (Panel (c)) workers separately. The sample is restricted to firms with 75-225 employees in 2013. All workers with top-coded daily wage are included in the sample, with their daily wage assigned to the year-specific top-coding. Standard errors are clustered at firm-year level. The standard error spikes represent 95% CI.*




(a) Gender Wage Gap



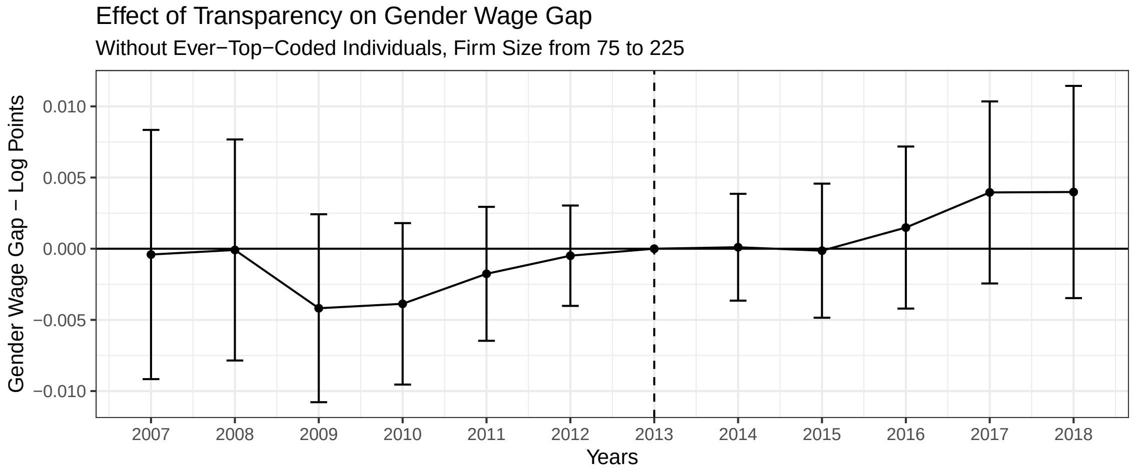

(b) Male Daily Wage



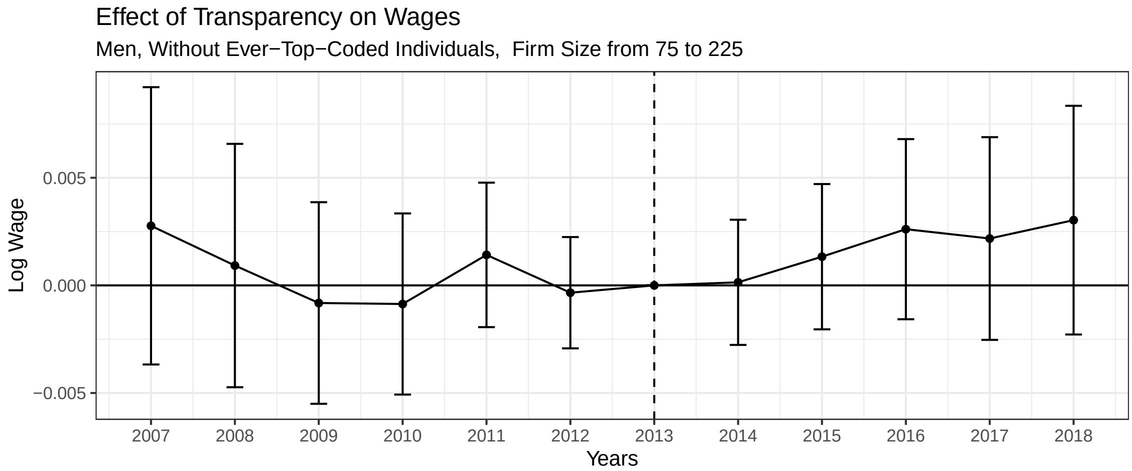

(c) Female Daily Wage

Figure A8: Effects of Transparency on GWG and Daily Wage (Without Ever-Top-Coded)

*The figure below plots the effects of transparency on gender wage gap (Panel (a)), and daily wages for male (Panel (b)) and female (Panel (c)) workers separately. The sample is restricted to firms with 75-225 employees in 2013. All workers who were ever top-coded in the sample period are dropped. Standard errors are clustered at firm-year level. The standard error spikes represent 95% CI.*




(a) Gender Wage Gap

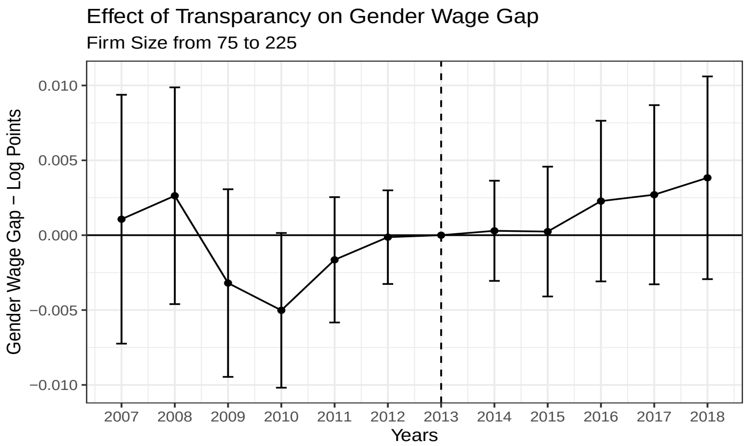



(b) Male Daily Wage


(c) Female Daily Wage

Figure A9: Effects of Transparency on GWG and Daily Wage (Worker-level Treatment)

*The figure below plots the effects of transparency on gender wage gap (Panel (a)), and daily wages for male (Panel (b)) and female (Panel (c)) workers separately. Individuals are assigned to treatment status if they worked in a firm which had more than 150 employees in 2013, and to the control group otherwise. Standard errors are clustered at firm-year level. The standard error spikes represent 95% CI.*




(a) Gender Wage Gap


(b) Male Daily Wage


(c) Female Daily Wage

## Bunching of Firms

In Appendix Figure $A10$ we plot the size-weighted fraction of establishments which violate their intended treatment rule, based on their firm size in 2010, and separately based on firm size in 2013. In Appendix Figure $A11$ we plot the fraction of establishments which survive in the four size groups: <75, 75-149, 150-225, >225, depending on their size group in the previous year. In line with previous analyses, we focus on the two firm size groups: (75-149), (150-225) for both these figures.

Establishments in the size group (75-149) would never be subject to transparency unless they increase their size beyond the 150 employee threshold. Defining treatment based on 2013 size shows that about 10% of these establishments (Appendix Figure $A10$) were subject to the policy after 2013 onwards, which implies that they increased their size above the 150 employee threshold. Furthermore, defining treatment based on 2010 size shows that these violations start only in 2014. Therefore there is little evidence for strategic resizing for firms in the 75-149 group, between 2010 when the policy was first announced, and 2013. In the left panel of Appendix Figure $A11$ we show that the year-on-year transition rate for firms in the size group 75-149 to size groups 150 and beyond, was very close to 10% even before the policy. Therefore, the 10% violation of intended treatment rule for firms in the size group 75-149 is no different from the pre-reform size dynamics for these firms.

We replicate this exercise for firms which had between 150-225 employees in 2013 and 2010 separately. When defining treatment by 2013 size, we find that roughly 20% of establishments in the size group 150-225 violate their intended treatment rule in 2014, and this fraction drops to about 10% by the end of our sample period in 2018 (Appendix Figure $A10$). The only way for these firms to avoid transparency is to downsize to below the 150 employee threshold. Again we compare this violation to general firm size dynamics in the right panel of Appendix Figure $A11$. Before the policy was implemented, roughly 15% of establishments in the size group 150-225 downsized to below the 150-worker cut-off from one year to the next. While we do see a small dip in the fraction of establishments which survive in the size group 150-225 from 2013 to 2014, this trend reverses in the very next year in 2015 and is stable thereafter. Moreover, there is little evidence to suggest that firms in the size group 150-225 strategically downsize in the time period between 2010 and 2013. Therefore, it does not appear that there is any strategic bunching of firms around the 2014 implementation threshold.

Figure A10: Establishments Violating Intended Treatment Status based on 2010 Size Rule

*The figure below shows the establishment-size weighted fraction of establishments that violate intended treatment rule based on their firm sizes in 2010 and 2013, separately. Establishments would violate their intended treatment rule if they enter treatment either before the intended start year because of an increase in firm size, or they manage to delay treatment beyond their intended year by reducing firm size.*

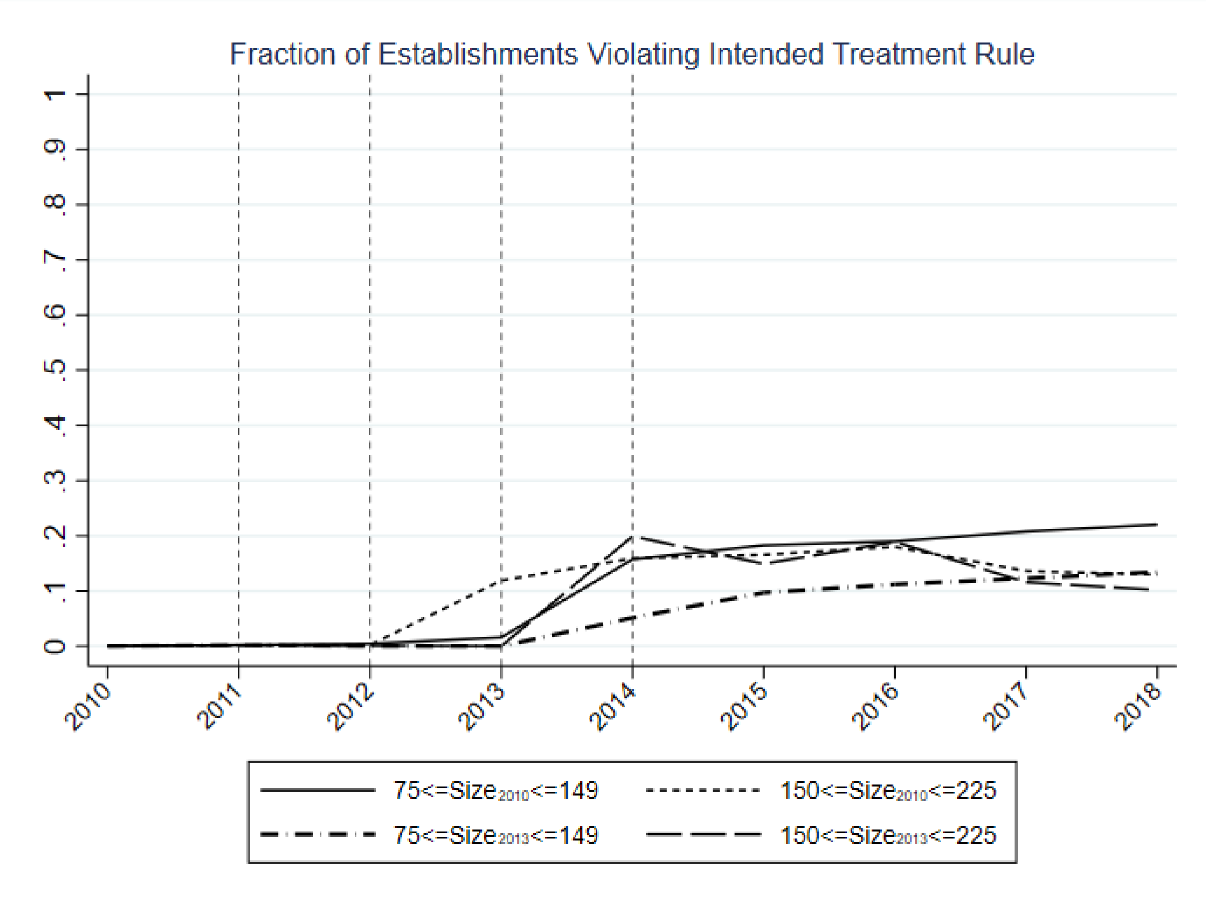


Figure A11: Transitions of Establishments Across Firm Size Groups

*The figure below plots the fraction of establishments, weighted by establishment size, that survive in the same firm size group or transition to other firm size groups, relative to the number of establishments in each size group for the previous year. Each of the five panels below represents the five firm size groups that an establishment can belong to in the previous year. The graphs show that the establishment-size weighted probability of transitioning into a size group different from the previous year is small, with the cumulative across the other four groups being less than 20%. Even in cases where the transition probabilities to other groups are close to 10% (for example in the size group: 150-250) there is no systematic evidence for bunching down, as the transition probabilities to both immediately higher and lower size groups, are similar.*

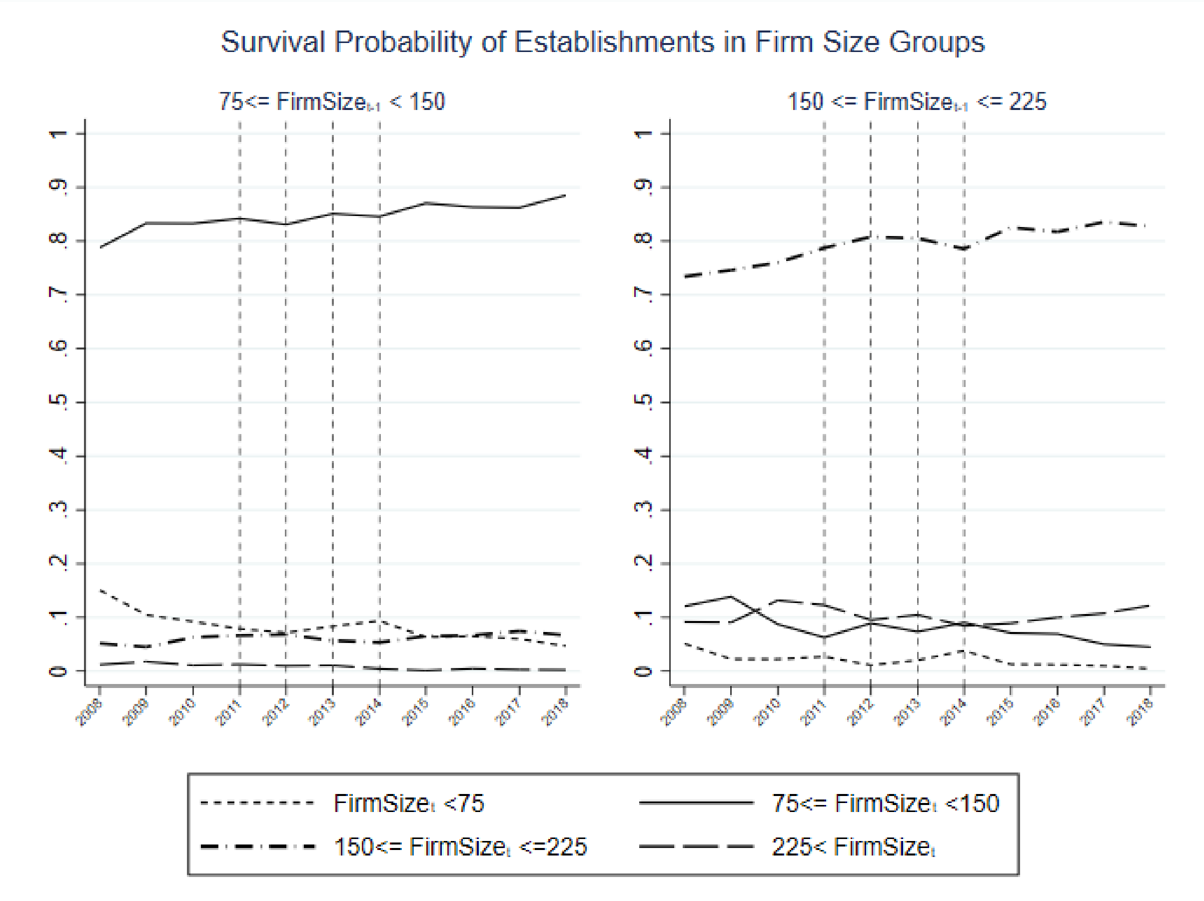
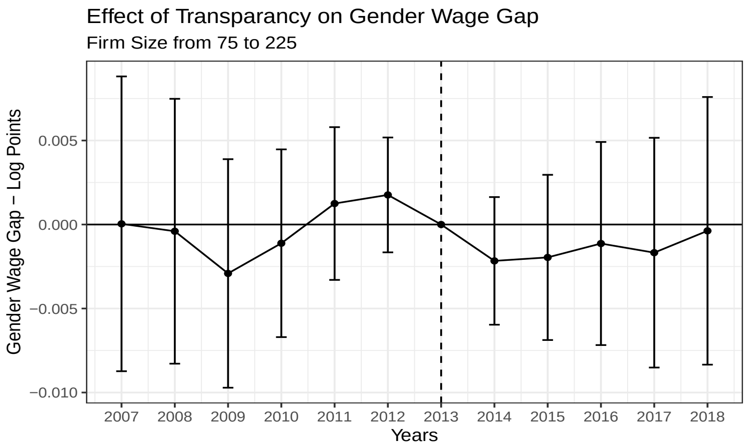


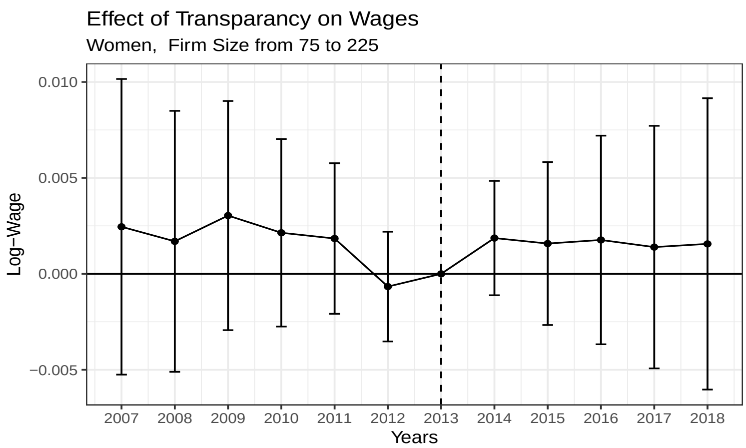
Figure A12: Effects of Transparency on GWG and Daily Wage (Treatment Defined as of 2010)

*The figure below plots the effects of transparency on gender wage gap (Panel (a)), and daily wages for male (Panel (b)) and female (Panel (c)) workers separately. Treatment is assigned based on firm size in 2010, one year before the reform was announced. Standard errors are clustered at firm-year level. The standard error spikes represent 95% CI.*




(a) Gender Wage Gap


(b) Male Daily Wage

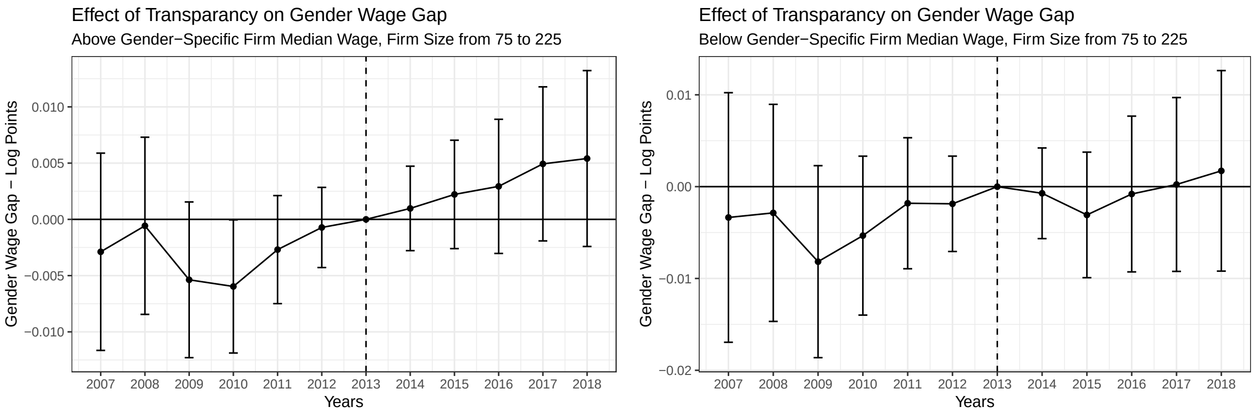



(c) Female Daily Wage

Figure A13: Effects of Transparency on Gender Wage Gap (GWG)

**[Above and Below Firm-Gender-Specific Median Daily Wage]**

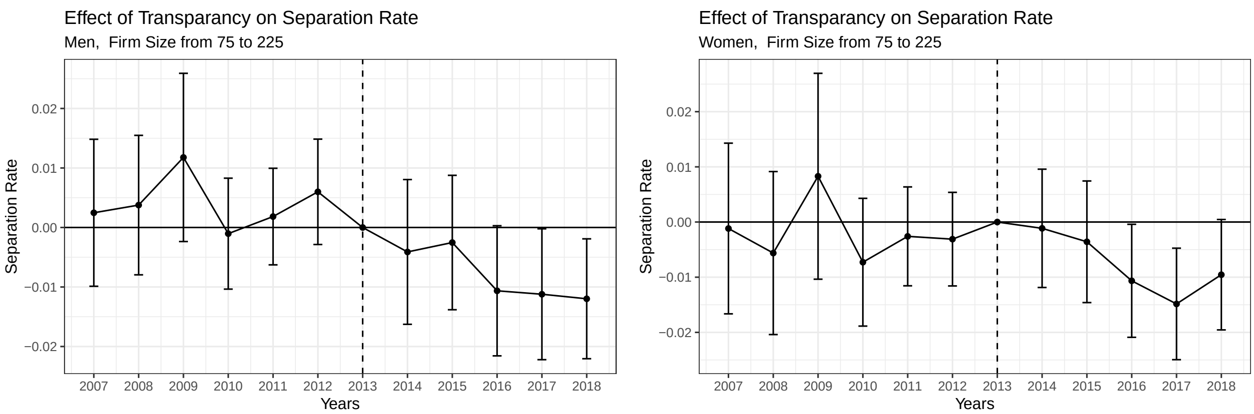
*The figure below plots the effects of pay transparency on the gender wage gap for workers who earned above (top panel) and below (bottom panel) their firm and gender-specific median daily wage in 2013, the year before treatment. The sample is restricted to firms with 75-225 employees in 2013. Standard errors are clustered at the establishment-year level. The standard error spikes represent 95% CI.*




(a) **Above Median** (b) **Below Median**

Figure A14: Effects of Transparency on Job Separation Rate

*The figure below plots the effects of pay transparency on the year-on-year job separation rate for male and female workers. The sample is restricted to firms with 75-225 employees in 2013. The standard error spikes represent 95% confidence intervals.*

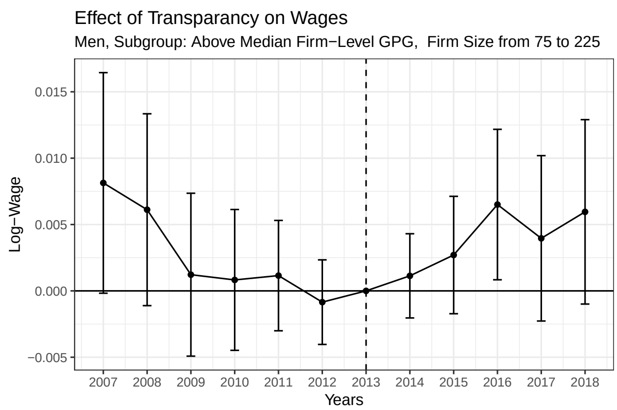



(a) Male (b) Female

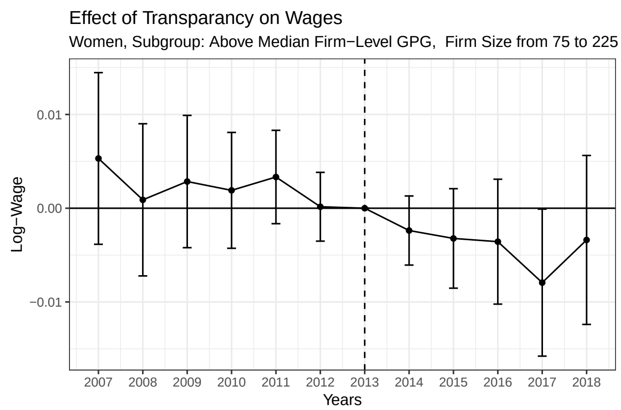
Figure A15: Gender-Specific Effects of Transparency on Daily Wages

**[Above/Below Median Establishment-level GWG]**

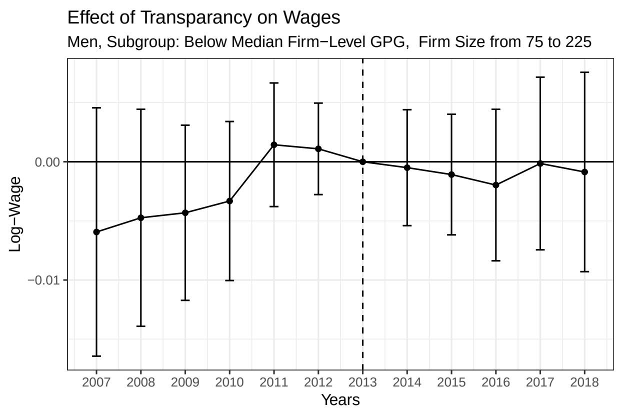
*The figure below plots the effects of transparency on male and female wages, in establishments with above and below median establishment-level gender wage gap in 2013. The sample includes only firms with 75-225 employees. Standard errors are clustered at the firm-year level. The standard error spikes represent 95% CI.*



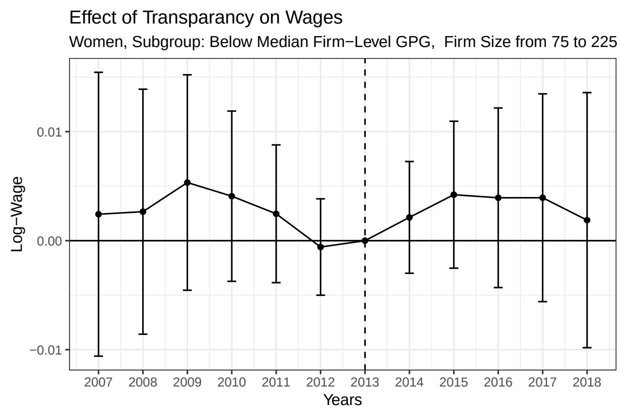

(a) Above Median Male




(b) Above Median Female




(c) Below Median Male

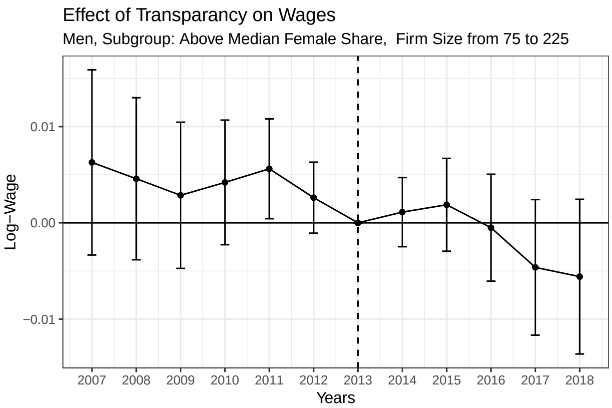



(d) Below Median Female

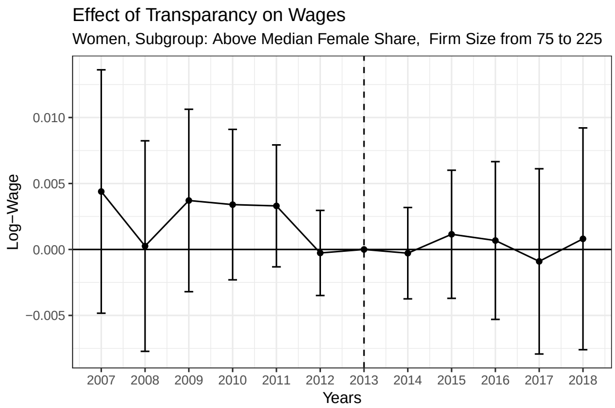
Figure A16: Gender-Specific Effects of Transparency on Daily Wages

**[Above/Below Median Establishment-level Female Share]**

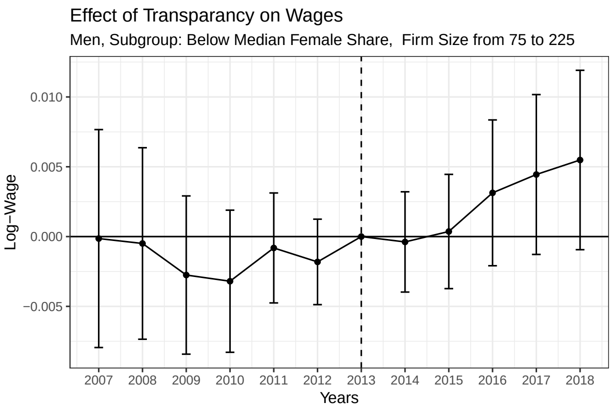
*The figure below plots the effects of transparency on male and female wages, in establishments with above and below median establishment-level share of female workers, in 2013. The sample includes only firms with 75-225 employees. Standard errors are clustered at the firm-year level. The standard error spikes represent 95% CI.*




(a) Above Median Male




(b) Above Median Female




(c) Below Median Male


(d) Below Median Female

# Appendix B: Additional Tables

Table B1: Income Report for 2016: All Federal Services

*The following table is from "Einkommensbericht 2017" of the Austrian Federal Government, Public Administration. It is publicly available at Einkommensbericht, 2017. The table illustrates how an income report can be written. The first column depicts the occupational groups/task groups as defined by collective bargaining agreements. The rows printed in bold summarize the statistics averaged for each occupation.task group. The same is repeated for employees in training and those who previously worked for the government, but are now employed in a (semi-) private company, e.g. postal services or telecommunications. All these tables are accompanied by brief discussion on why there are wage differences and measures taken to reduce differences that stem from factors not related to the seniority structure or composition within task groups (for example: office clerks and technicians are in the same group but technicians are paid more. The former group is mostly female, while the latter is mostly male, which explains some of the differences in remuneration schedules by group.*

| | Number of Workers | | Median Gross Income/Yr | | Mean Age | | Gender Pay Gap | Age Diff |
|---|---|---|---|---|---|---|---|---|
| Occupation Clusters | Men | Women | Men | Women | Men | Women | % | (Men-Women) |
| **Central Administration** | 23872 | 27002 | 45637 | 35799 | 49.2 | 46.1 | 21.6% | 3.1 |
| A1, v1 | 4157 | 3211 | 75141 | 61482 | 48.6 | 44.0 | 18.2% | 4.6 |
| A2, v2 | 7598 | 6454 | 57201 | 47898 | 49.7 | 45.9 | 16.3% | 3.8 |
| A3, v3, h1 | 6401 | 10721 | 38151 | 34285 | 49.8 | 46.7 | 10.1% | 3.1 |
| A4-7, v4-5, h2-5 | 4421 | 5962 | 28336 | 25749 | 46.5 | 45.1 | 9.1% | 1.5 |
| Service Rank: Central Administration | 756 | 553 | 78994 | 65742 | 57.3 | 56.0 | 16.8% | 1.4 |
| Data Services and Management | 539 | 101 | 60305 | 56189 | 46.7 | 48.5 | 6.8% | -1.8 |
| **Police and Law Enforcement (Executive)** | 27484 | 5230 | 51504 | 40776 | 44.8 | 34.2 | 20.8% | 10.5 |
| E1 | 649 | 42 | 81756 | 64668 | 52.3 | 44.4 | 20.9% | 7.9 |
| E2a | 9742 | 975 | 58561 | 46584 | 50.3 | 39.7 | 20.5% | 10.6 |
| E2b, Lowest Rank Officer | 15344 | 3519 | 48284 | 40797 | 43.0 | 34.5 | 15.5% | 8.5 |
| E2c, Aspirant | 1705 | 694 | 17442 | 17442 | 26.3 | 24.5 | 0.0% | 1.8 |
| Service Rank, Executive Office | 44 | 0 | 54334 | - | 54.8 | - | - | - |
| **Judges, District Attorneys (Judiciary)** | 1491 | 1746 | 91417 | 80341 | 48.4 | 43.9 | 12.1% | 4.5 |
| R3, III | 96 | 37 | 144402 | 123945 | 55.9 | 51.5 | 14.2% | 4.4 |
| R2, II | 106 | 85 | 111366 | 106649 | 54.0 | 52.3 | 4.2% | 1.7 |
| R1a, R1b, I | 739 | 1011 | 88651 | 80341 | 48.4 | 44.7 | 9.4% | 3.7 |
| Supreme Court Judges | 225 | 195 | 96489 | 99331 | 52.4 | 50.9 | -3.0% | 1.4 |
| Judge Aspirants | 71 | 136 | 34192 | 34192 | 29.8 | 28.6 | 0.0% | 1.2 |
| Procurator General's Office | 12 | 6 | 128815 | 125434 | 52.7 | 49.5 | 2.6% | 3.2 |
| St2, STII | 55 | 30 | 90827 | 84100 | 46.3 | 45.1 | 7.4% | 1.2 |
| St1, STI | 187 | 246 | 81175 | 70271 | 43.9 | 39.3 | 13.4% | 4.6 |
| **Military Service** | 15661 | 421 | 41589 | 28777 | 41.6 | 31.1 | 30.8% | 10.4 |
| MBO1, MZO1 | 735 | 45 | 91956 | 78806 | 48.7 | 45.2 | 14.3% | 3.4 |
| MBO2, MZO2 | 2160 | 23 | 56766 | 43759 | 45.3 | 33.5 | 22.9% | 11.8 |
| MBUO1, MZUO1 | 6673 | 63 | 44411 | 34442 | 49.6 | 37.3 | 22.5% | 12.3 |
| MBUO2, MZUO2, MZO3 | 2477 | 92 | 34108 | 29580 | 33.1 | 31.6 | 13.3% | 1.5 |
| MZ Charge | 1684 | 171 | 27910 | 22792 | 24.1 | 25.3 | 18.3% | -1.3 |
| Service Rank: Military Service | 557 | 0 | 42654 | - | 55.1 | - | - | - |
| International Strike Force | 1375 | 27 | 29231 | 27493 | 24.1 | 26.2 | 5.9% | -2.1 |
| **Teachers** | 19339 | 30109 | 60584 | 52635 | 48.2 | 45.4 | 13.1% | 2.8 |
| L1, I1 | 14837 | 23628 | 64858 | 55453 | 49.0 | 46.1 | 14.5% | 3.0 |
| L2, I2 | 4156 | 5750 | 48396 | 43609 | 46.7 | 44.9 | 9.9% | 1.8 |
| L3, I3 | 123 | 118 | 24360 | 24599 | 45.9 | 47.0 | -1.0% | -1.2 |
| Foreign Exchange Teachers | 223 | 523 | 17154 | 17293 | 25.5 | 24.7 | -0.8% | 0.8 |
| **Lecturers (University)** | 679 | 852 | 69591 | 65002 | 52.4 | 50.9 | 6.6% | 1.5 |
| **Educational Board** | 171 | 143 | 85325 | 83103 | 56.6 | 56.0 | 2.6% | 0.6 |
| **Nursing and Health Services** | 91 | 175 | 44317 | 39369 | 48.1 | 47.8 | 11.2% | 0.4 |
| K2, k2 | 25 | 28 | 49982 | 43525 | 48.7 | 44.7 | 12.9% | 4.0 |
| K3, k3 | 7 | 11 | 56430 | 55410 | 55.2 | 55.8 | 1.8% | -0.7 |
| K4, k4 | 43 | 95 | 42875 | 40192 | 47.6 | 46.4 | 6.3% | 1.2 |
| K5, k5 | 8 | - | 40734 | - | 49.1 | - | - | - |
| K6, k6 | 15 | 34 | 32272 | 33825 | 46.6 | 50.7 | -4.8% | -4.1 |
| **Others** | 184 | 452 | 106960 | 106960 | 53.5 | 51.3 | 0.0% | 2.2 |
| Medical professionals | 168 | 449 | 106960 | 106960 | 55.4 | 51.4 | 0.0% | 4.0 |
| Others | 16 | 3 | 25269 | 27723 | 33.7 | 34.0 | -9.7% | -0.3 |

Table B2: Effects of Pay Transparency on Gender Wage Gap

| | *Dependent variable: ln(Daily Wage)* | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Male | 0.24*** | 0.32*** | | |
| | (0.003) | (0.004) | | |
| Male*Treat | 0.01 | 0.003 | −0.01 | |
| | (0.01) | (0.01) | (0.004) | |
| Male*Treat*$\mathbf{1}$[t=2007] | −0.01 | −0.01 | −0.002 | −0.001 |
| | (0.01) | (0.01) | (0.004) | (0.005) |
| Male*Treat*$\mathbf{1}$[t=2008] | −0.01 | −0.01 | −0.001 | 0.001 |
| | (0.01) | (0.01) | (0.004) | (0.004) |
| Male*Treat*$\mathbf{1}$[t=2009] | −0.01** | −0.01** | −0.01 | −0.01 |
| | (0.005) | (0.005) | (0.003) | (0.003) |
| Male*Treat*$\mathbf{1}$[t=2010] | −0.005 | −0.01 | −0.004 | −0.01* |
| | (0.004) | (0.004) | (0.003) | (0.003) |
| Male*Treat*$\mathbf{1}$[t=2011] | −0.004 | −0.005 | −0.002 | −0.003 |
| | (0.003) | (0.003) | (0.002) | (0.002) |
| Male*Treat*$\mathbf{1}$[t=2012] | −0.002 | −0.002 | −0.0004 | −0.001 |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| Male*Treat*$\mathbf{1}$[t=2013] | 0.00 | 0.00 | 0.00 | 0.00 |
| | − | − | − | − |
| Male*Treat*$\mathbf{1}$[t=2014] | −0.01** | −0.01** | 0.001 | 0.001 |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| Male*Treat*$\mathbf{1}$[t=2015] | −0.01** | −0.01*** | 0.0002 | 0.001 |
| | (0.003) | (0.003) | (0.002) | (0.002) |
| Male*Treat*$\mathbf{1}$[t=2016] | −0.01 | −0.01* | 0.002 | 0.002 |
| | (0.004) | (0.004) | (0.003) | (0.003) |
| Male*Treat*$\mathbf{1}$[t=2017] | −0.001 | −0.002 | 0.004 | 0.003 |
| | (0.004) | (0.004) | (0.003) | (0.003) |
| Male*Treat*$\mathbf{1}$[t=2018] | 0.002 | 0.001 | 0.004 | 0.003 |
| | (0.004) | (0.004) | (0.004) | (0.004) |
| Treat∗$\mathbf{1}$[t=2007] | 0.005 | 0.01 | 0.004 | 0.003 |
| | (0.004) | (0.004) | (0.004) | (0.004) |
| Treat∗$\mathbf{1}$[t=2008] | 0.003 | 0.004 | 0.002 | 0.001 |
| | (0.004) | (0.004) | (0.003) | (0.004) |
| Treat∗$\mathbf{1}$[t=2009] | 0.004 | 0.005 | 0.004 | 0.004 |
| | (0.003) | (0.003) | (0.003) | (0.003) |
| Treat∗$\mathbf{1}$[t=2010] | 0.003 | 0.003 | 0.003 | 0.004 |
| | (0.003) | (0.003) | (0.002) | (0.003) |
| $\lambda_j$ | ✓ | ✓ | ✓ | |
| f(Age)∗$\mathbb{I}^m$ | | ✓ | ✓ | ✓ |
| $\lambda_i$ | | | ✓ | |
| $\lambda_{ij}$ | | | | ✓ |

**Continued on next page**

Table B2 – continued from previous page

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Treat∗**1**[t=2011] | 0.01** | 0.01** | 0.003 | 0.003 |
| | (0.003) | (0.002) | (0.002) | (0.002) |
| Treat∗**1**[t=2012] | 0.002 | 0.002 | 0.0001 | 0.0002 |
| | (0.002) | (0.002) | (0.001) | (0.001) |
| Treat∗**1**[t=2014] | 0.004** | 0.005** | −0.0004 | −0.0003 |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| Treat∗**1**[t=2015] | 0.01*** | 0.01*** | 0.001 | 0.001 |
| | (0.003) | (0.003) | (0.002) | (0.002) |
| Treat∗**1**[t=2016] | 0.01** | 0.01** | 0.001 | 0.0002 |
| | (0.003) | (0.003) | (0.003) | (0.003) |
| Treat∗**1**[t=2017] | 0.002 | 0.003 | −0.002 | −0.001 |
| | (0.004) | (0.004) | (0.003) | (0.003) |
| Treat∗**1**[t=2018] | −0.0001 | 0.001 | −0.001 | −0.001 |
| | (0.004) | (0.004) | (0.004) | (0.004) |
| Male∗**1**[t=2007] | 0.01*** | 0.01** | −0.04*** | −0.04*** |
| | (0.003) | (0.003) | (0.003) | (0.003) |
| Male∗**1**[t=2008] | 0.01*** | 0.01** | −0.03*** | −0.04*** |
| | (0.003) | (0.003) | (0.003) | (0.003) |
| Male∗**1**[t=2009] | 0.001 | −0.001 | −0.04*** | −0.04*** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| Male∗**1**[t=2010] | 0.001 | −0.0001 | −0.03*** | −0.03*** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| Male∗**1**[t=2011] | 0.003 | 0.002 | −0.02*** | −0.02*** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| Male∗**1**[t=2012] | −0.002 | −0.002 | −0.01*** | −0.01*** |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| Male∗**1**[t=2014] | 0.003* | 0.003* | 0.01*** | 0.01*** |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| Male∗**1**[t=2015] | 0.003 | 0.002 | 0.01*** | 0.01*** |
| | (0.002) | (0.002) | (0.001) | (0.002) |
| Male∗**1**[t=2016] | 0.001 | 0.0001 | 0.02*** | 0.02*** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| Male∗**1**[t=2017] | −0.002 | −0.003 | 0.02*** | 0.02*** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| Male∗**1**[t=2018] | −0.003 | −0.01** | 0.03*** | 0.03*** |
| | (0.003) | (0.003) | (0.003) | (0.003) |
| **1**[t=2007] | −0.04*** | −0.03*** | −0.05*** | −0.06*** |
| | (0.003) | (0.002) | (0.003) | (0.003) |
| **1**[t=2008] | −0.02*** | −0.01*** | −0.03*** | −0.03*** |
| | (0.002) | (0.002) | (0.002) | (0.003) |
| $\lambda_j$ | ✓ | ✓ | ✓ | |
| f(Age)∗$\mathbb{I}^m$ | | ✓ | ✓ | ✓ |
| $\lambda_i$ | | | ✓ | |
| $\lambda_{ij}$ | | | | ✓ |

Continued on next page

**Table B2 – continued from previous page**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **1**[t=2009] | −0.001 | 0.004** | −0.01*** | −0.01*** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| **1**[t=2010] | −0.003** | 0.001 | −0.01*** | −0.01*** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| **1**[t=2011] | −0.01*** | −0.01*** | −0.02*** | −0.02*** |
| | (0.002) | (0.001) | (0.001) | (0.001) |
| **1**[t=2012] | −0.01*** | −0.01*** | −0.01*** | −0.01*** |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| **1**[t=2014] | 0.01*** | 0.01*** | 0.02*** | 0.0.02*** |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| **1**[t=2015] | 0.02*** | 0.02*** | 0.03*** | 0.0.03*** |
| | (0.002) | (0.002) | (0.001) | (0.001) |
| **1**[t=2016] | 0.03*** | 0.02*** | 0.04*** | 0.0.04*** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| **1**[t=2017] | 0.03*** | 0.03*** | 0.05*** | 0.05*** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| **1**[t=2018] | 0.04*** | 0.03*** | 0.07*** | 0.07*** |
| | (0.002) | (0.002) | (0.002) | (0.003) |
| Age | | −0.04*** | | |
| | | (0.01) | | |
| AgeSq | | 0.73*** | 0.92*** | 1.05*** |
| | | (0.03) | (0.03) | (0.03) |
| AgeCu | | 1.62*** | 1.35*** | 1.18*** |
| | | (0.05) | (0.05) | (0.06) |
| AgeQuart | | −4.37*** | −3.99*** | −3.95*** |
| | | (0.10) | (0.10) | (0.11) |
| Male*Age | | 0.29*** | | |
| | | (0.01) | | |
| Male*AgeSq | | −1.58*** | −1.65*** | −1.74*** |
| | | (0.03) | (0.03) | (0.04) |
| Male*AgeCu | | −0.76*** | −0.60*** | −0.42*** |
| | | (0.05) | (0.05) | (0.06) |
| Male*AgeQuart | | 4.39*** | 3.69*** | 3.55*** |
| | | (0.11) | (0.10) | (0.11) |
| Observations | 4914038 | 4914038 | 4914038 | 4914038 |
| $R^2$ | 0.46 | 0.49 | 0.92 | 0.94 |
| Adjusted $R^2$ | 0.46 | 0.49 | 0.90 | 0.91 |

*p<0.1; **p<0.05; ***p<0.01