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 internal reports on the gender pay gap. Using an event-study design, we show that the policy had no discernible effects on male and female wages, thus leaving the gender wage gap unchanged. The effects are precisely estimated and we rule out that the policy narrowed the gender wage gap by more than 0.5 p.p.. Moreover, we do not find evidence for wage compression within firms. The Austrian transparency reform might have little 'bite' because wage reports are company secret and not public information.

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1 Introduction

Gender disparity in earnings is a persistent feature of labor markets around the world. Women earn about 23% less than men in the US, 20% in Austria, and 15% on average across the European Union.[1] 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.[2]

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. Proponents of transparency argue that the lack of information on pay sustains the gender gap and transparency helps women to challenge discriminatory pay schedules.[3] However, critics worry about administrative costs and that men might use the information revealed by transparency more actively than women, further widening the gender pay gap instead. Nevertheless, these policies have garnered widespread attention among policy makers and variants of it have been introduced in Finland, Sweden, Norway, Denmark, Austria, the UK, Germany, Iceland, and the United States.[4] Despite its recent introduction in many countries, the causal evidence of transparency laws on wages and the gender wage gap is scarce. This paper studies the Austrian transparency law to fill this 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 and update income reports every second year. These reports must contain annual gross income, separately by gender and occupation groups as defined in the respective collective bargaining agreements. However, wage reports are company secret and not public information. 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 lowest cutoff to make

[1] Eurostat, 2018

[2] see Blau and Kahn (2017) for a review.

[3] For example, the European Commission writes in the Factsheet on Pay Transparency (2019): "[...] 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."
`https://ec.europa.eu/info/sites/info/files/factsheet-pay_transparency-2019.pdf`

[4] 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 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 again do not find any statistically or economically significant effects. Therefore, transparency seems to have failed in its twin objectives of reducing the gender pay gap and boosting female earnings. We show that this conclusion holds under a number of alternative specifications using different control variables and alternative sample restrictions on top-coding, firm size windows, and firm compliance with treatment assignment. We further consider the full roll-out of the policy across all firm size groups and show that transparency did not affect the gender wage gap in large firms as well.

While pay transparency does not affect averages wages, it could potentially lead 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. Furthermore, we do not find heterogeneous effects for workers earning below or above the establishment-level gender-specific median wage.

Why does pay transparency not affect the gender pay gap and wage setting in general? Surveys of worker representatives and work councils reveal that compliance was universal and a majority of respondents found the reports informative and useful.[5] Therefore, imperfect implementation seems an unlikely explanation. One potential mechanism however could be that wage reports in Austria are company secret, and not public information. Consequently, this limited scope can only impact within-firm outcomes. To show this, we first decompose gender differences in firm pay into a within firm component originating from differences in firm pay policies across gender, and a sorting component capturing the fact that women and men work for firms with different firm pay levels. We show that the overwhelming majority of the gender differences in firm pay is because women work on average for lower paying firms. This is also in line with evidence from other countries (Card et al., 2016; Morchio and Moser, 2019), which find that the within-firm gender differences in pay policies are typically close to zero, whereas the sorting component explains the overwhelming majority. But the Austrian pay transparency policy cannot directly affect the sorting component, as it explicitly mandates wage reports to be company secret, and hence the information is not publicly available. Thus, the transparency reform likely targeted the wrong component of the gender wage gap. In contrast,

[5] Arbeiterkammer, 2014

transparency reforms that also target the sorting component, for example with making reports public information, might have a larger 'bite' to begin with.

Second, wage reports do not only reveal gender differences in firm pay, but also provide information for workers about their position on the wage ladder within firms. If the wage reports reveal large unjustified wage differences, 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 that transparency alleviates concerns about unfair compensation among workers and thus reduces worker turnover. This interpretation is also consistent with recent evidence that a vast majority of the inequality within firms can be accounted for by workers' characteristics (Lamadon et al., 2020), which we show also holds in Austria.

Our work contributes to a small literature studying the effects of transparency in very specific labor markets, which typically documents unintended consequences of such policies. Schmidt (2012) and Mas (2016) show that mandated disclosure of CEO compensation leads to 'ratcheting' effects, whereby CEOs who earned below the average, received a pay raise. Using a field experiment in an online labor market, Cullen and Pakzad-Hurson (2019) document that transparency led to overall wage reductions. Baker et al. (2019) show that a public sector salary disclosure law for university faculty in Canada reduced the gender wage gap, though partly by lowering male wages.

Our paper is one of the first to document the effects of a broad introduction of pay transparency. The most closely related studies are Bennedsen et al. (2019), Duchini et al. (2020) and Blundell (2020), which analyze similar policies in Denmark and the UK. These studies show, that similar to Austria, pay transparency in both countries failed to achieve its goal of increasing female wages. However, in contrast to our study, they find that transparency moderately depressed male earnings, and thus slightly narrowed the gender wage gap.

We complement these studies by arguing that transparency policies can potentially have a larger impact on the gender wage gap if the wage reports are public information. This can guide women in their job search towards more equitable and higher paying firms. This channel can be one of the reasons why the UK reform, which makes gender wage gaps public information, was more successful compared to Austria in closing the gender wage gap.

More broadly, our work is related to the literature which studies the effects of information about relative earnings on behavioral and labor market outcomes: municipal salary disclosure on pay compression among city managers (Mas, 2017), 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), pay inequality on attendance and output in India (Breza et al., 2017), relative earnings on worker effort (Cohn et al., 2014) and on happiness and life-satisfaction (Brown et al., 2008; Clark et al., 2009; Clark and Oswald, 1996; Godechot and Senik, 2015; Luttmer, 2005).

The rest of this paper is structured as follows. In Section 2 we describe the pay transparency law in detail. Section 3 lays out a conceptual framework for transparency policies, Section 4 explains our data, sample selection, and our empirical strategy. We present our results in Section 5, discuss the potential reasons behind the ineffectivness of the reform to affect the wage setting in Section 6, and the last section concludes.

2 Institutional Setting and the Pay Transparency Policy, 2011

In international comparisons, Austria has a relatively high gender pay gap. The unadjusted gender pay gap was 20 percent in 2017, being fifth highest in the European Union.[6] A commonly raised point in the public debate in Austria is that pay secrecy is a major obstacle to achieving equal pay because women might not know the degree of pay discrimination or have less precise information about pay schedules compared to their male colleagues.

In light of these debates, the Austrian government introduced a Pay Transparency law in 2011, serving two explicit goals: first, boosting female wages and second, thereby reducing the gender wage gap. To achieve these goals, firms have to produce and update internal gender pay gap reports every second year, disaggregated by occupation groups. These reports must include the number of employees within a gender-occupation cell and their average or median annual earnings, expressed in full-time equivalents. All components of pay must be included, but there is no obligation to separate them. It is important to note that employers

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

have no discretion about the occupational groups, but they have to follow the pre-defined classifications in collective bargaining agreements.[7] Managerial positions are exempt from reporting requirements.

In principle, workers are almost universally covered by collective bargaining agreements. These define minimum wages at the industry level for different occupations, but firms and workers are free to bilaterally agree on wages above this floor. We are not aware of any precise evidence on the fraction of workers paid above required levels, but evidence on the wage structure suggests that they are not very binding. As shown in Appendix 6.1, differences in firm pay explain almost the same fraction of wage inequality in Austria as in the United States, suggesting that firms have a lot of flexibility in setting their pay policies, and are not much constrained by the collective bargaining agreements.

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. Second, reports must be made available to all employees via work councils 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 can discuss the contents of the report with their colleagues, union representatives, and legal advocates. However, communication of the contents to the outside are prohibited. Firms have no obligation to make these reports public, yet many public sector firms make theirs available online (see Appendix Table $A1$).

The implementation of the legislation was staggered over the next four years. Firms with more than 1000 workers came under the legislation in March 2011. Then in January of each subsequent year, firms with more than 500, 250, and finally 150 employees became subject to the reporting requirements in 2012, 2013, and 2014 respectively. Firms that grow and exceed the 150 employee threshold after 2014 have to produce a report in the first year they exceed the threshold. In 2011, about 30% of the Austrian workforce became subject to the

[7]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.

legislation, which grew to 50% of workers by 2014 (see Appendix Figure *A*1). There are no other policy changes or legal requirements that specifically apply to these cutoffs and especially the 150-employee cutoff used in our baseline study.

Exploratory non-representative surveys 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 study the level of compliance among firms and the dissemination of reports to employees. Evidence from these surveys (Arbeiterkammer (2014); Deloitte (2015)) show near universal compliance with the policy. Reports were shared with works councils promptly and information was distributed most frequently via intranet, announcements, employee newsletters, etc. In more than half of the cases, council representatives reported close cooperation with their employers in preparing the reports and 80% reported that their employers were open to adopting measures addressing the gap.

We do not have precise information about what fraction of workers actively use the wage reports, but there is no reason to believe that pay reports are not widely known. The media regularly reports about the gender wage gap. In particular, this topic receives widespread attention on the so-called Equal Pay Days.[8] Around these dates, most newspapers and news stations discuss the existing gender pay gap in Austria, its roots and pathways to closing it. Pay reports are featured prominently in this debate, especially in the first four years after the reform.[9] We take this regular news coverage as evidence that the general public (and especially workers) are aware of the issue at hand and pay reports as way of addressing it. In addition, as mentioned above, the fact that many work councils are directly involved in the preparation of the wage reports suggests that this information should also percolate to workers.

3 Conceptual Framework

How should we expect pay transparency to affect the wage setting process? It has long been recognized that observationally similar workers are paid differently in the labor market. A recent literature emphasizes the role of firm pay in understanding wage differences across

[8]There are two Equal Pay Days in Austria: The first is in spring and marks the day until which women "work for free" in a given year based on the gender pay gap. The second is in fall and marks the date by which men would have earned the same annual income as women in full year (so to speak, from that day on, women work for free relative to men for the rest of the year).

[9]See for example https://www.tt.com/artikel/3502362/online-gehaltsrechner-soll-fuer-transparenz-sorgen (assessed Feb. 16th, 2021) or https://www.kleinezeitung.at/politik/innenpolitik/5298933/Equal-Pay-Day_Frauen-verdienen-in-ihrem-Leben-435000-Euro-weniger (assessed Feb. 15th, 2021)

workers, which has been shown to explain around a third of the overall wage variation.[10] In models with frictional labor markets, more productive firms are willing to pay higher wages, as their opportunity cost of a vacancy is higher (Cahuc et al., 2006; Postel-Vinay and Robin, 2002). Since search is a time and resource intensive process in such frameworks, both parties would be willing to accept a range of wages. These range from reservation wages holding the worker to their outside option, up to wages where the worker appropriates the firm's maximum willingness to pay. Typically, a particular bargaining protocol is assumed, where wages are pinned down by the bargaining power of workers. In such settings, wage differences within firms could arise due to differences between workers' bargaining power and outside options.

A less researched aspect in search framework is that asymmetric information between employers and workers and informational differences across workers about firm's willingness to pay can lead to differential wage outcomes. Therefore, pay transparency can alleviate these informational frictions and in turn affects wages and other labor market outcomes. If workers have different information about firms' output and willingness to pay, they would achieve different bargaining outcomes.[11] In particular, women might have less information than their male colleagues about the firm's willingness to pay, possibly because of smaller workplace networks.[12] These information gaps could generate pay disparities both within and across gender lines. Transparency by design reveals more information about the firm's willingness to pay and unequal pay schedules. If wage reports are company secret, this information empowers only current workers to challenge gender pay gaps and pay disparity in general.

Beyond wages, transparency can affect job turnover through changes in job satisfaction. If workers perceive that they are underpaid and have little bargaining power to demand higher wages, they will have lower job satisfaction and likely quit with a higher propensity. In contrast, job satisfaction and retention might increase if within-firm transparency alleviates previously held concerns about unfair compensation. If wage reports are not only available within firms, but are also public information, workers and especially women can direct their search towards more equitable and higher paying firms.

On the firm side, transparency can induce firms to reduce wage dispersion out of equity concerns when large differences within the company become salient and information in wage

[10] see e.g. Abowd et al., 1999, Card et al., 2016, Song et al., 2018, among many others

[11] See for example the framework in Cullen and Pakzad-Hurson, 2019

[12] Previous research shows that women are less informed about their market value than men (Babcock and Laschever, 2003), more private about their pay than men (Goldfarb and Tucker, 2011), and communicate about pay with their peers less often than men (Cullen and Pakzad-Hurson, 2019). According to a (Glassdoor, 2016) survey, globally 59% of men versus 51% of women believe they have a good understanding of how pay is determined at their company.

reports begin to serve as reference points in negotiations. In addition, if wage reports are public information, wage and gender pay gap differences across firms would invite public scrutiny and criticisms that might pressure firms to correct their wage policies.

To summarize, internal wage reports can in theory be an effective policy tool to address wage differences within companies. But the above discussion makes it clear that transparency will only affect wage setting under certain conditions. The Austrian transparency legislation only requires firms to compile wage reports, but does not mandate them to act upon pay gaps. Therefore, it becomes the workers' responsibility to challenge pay disparities. First, assuming that wage re-negotiations entail some costs on part of the worker, the revealed wage differences must be perceived as unjustified and large enough to warrant acting upon them. Second, workers must have the bargaining power to even act upon this new information and demand higher wages. And finally, transparency as enacted in Austria only addresses information frictions in the wage setting due to differences in knowledge about firms' willingness to pay. If workers already had good information about how much their coworkers earn on average and therefore their employer's willingness to pay, it is likely that within-firm transparency would have no effects on the wage setting process.

In conclusion, it is a priori not clear whether internal wage reports will affect the gender wage gap and wage setting in general. Therefore, the empirical evaluation of the Austrian pay transparency policy not only estimates the efficacy of transparency legislation, but also the importance of informational differences about firms' willingness to pay for the wage setting. Before we delve into these results, we describe our data and empirical strategy in the next section.

4 Data and Empirical Strategy

We use administrative employment records from the Austrian social security administration from 1997-2018 in our analysis. 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, gender, and citizenship. Except a flag for blue collar jobs, the dataset does not contain information on workers' occupation. Each establishment has a unique identifier, and we merge with this data information on its geographic location, 4-digit NACE industry clas-

Table 1: Sample Restriction and Composition

The table below shows the composition of the sample under different sample restriction criteria. Column (3) is our main sample used for all analyses in the rest of this paper. Columns (4) and (5) show the sample means respectively for the treated and control group of firms in pre-treatment years (2007-2013). The adjusted gender wage gap was computed by controlling for Austrian citizenship, a quartic age polynomial, work experience, and firm fixed effects.

	(1)	(2)	(3)	(4)	(5)
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	0.237	0.238	0.222	0.222	0.222
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				✓	✓

sification, 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 the terms firm and establishment 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. The adjusted gender wage gap is above 20 percent in our dataset, although the true gender pay gap conditional on observables is likely much smaller. The social security dataset contains only few worker characteristics, studies with a larger set of controls find the adjusted gender wage gap to be below 8 percent (Böheim et al., 2020). 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 deflated 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. 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, and so we drop them for our baseline estimation. In our main sample, we select all firms between 75-225, but we consider robustness checks with other firm size windows as well as estimating the effect of the reform including all the larger firms.

Since the social security administration only records income up to the maximum contribution limit, wage information is top-coded, which applies to 6 percent of our sample.[13] As we cannot observe any change in wages for this group, we drop top-coded spells in our baseline sample. Table 1 shows that this selection does not change the worker composition much. In additional checks we explore the robustness of our results to either including top coded individuals, or excluding workers 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 2013, just before firms with 150-250 employees became subject to the policy in 2014. The last two columns in Table 1 show that the treatment and control establishments had similar worker and firm characteristics in the years before the policy was rolled out.

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:

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

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

[13]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.

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.[14] X_{it} is a vector of individual, time-varying controls: It contains a quartic polynomial in age and its interaction with gender. λ_i denotes the individual worker fixed effect. λ_j and λ_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 β_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. If the pay transparency reform is effective in reducing the gender pay gap, the coefficient β_1^k will be negative for $k > 2013$, i.e. the post-treatment years. Conversely, a positive coefficient implies that the gender pay gap has opened up. 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 β_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. Standard errors in all our analyses are clustered at the firm level.

Our two-way fixed effects strategy implies that our effects are identified within-firm and within-worker, i.e. the additional effect of this pay transparency reform after controlling for unobserved but time-constant worker and firm characteristics. Workers who stay with their employers before and after the policy contribute to these effects only if their wages change as a result of the policy. This is also true for workers who move across firms. Consequently, our results are not driven by sorting of higher individual fixed effect workers to higher paying firms, which could be different across genders.[15]

We estimate equation (1) for our baseline sample, i.e. firms that are located around the lowest firm-size cutoff of the reform to ensure their comparability with respect to (un)observables. Under the assumption that larger firms exhibit the same parallel trends, we can analyze the full staggered role-out of the reform. To this end we are applying a staggered difference-in-difference design for all treated firms, again accounting for response heterogeneity over time.

[14]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 (1) 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".

[15]In an alternative specification we include firm-worker match fixed effects directly controlling for potential "assortative" matching. Both point estimates and confidence intervals are not sensitive to this alternative specification.

We modify equation (1) as follows:

$$y_{ij(i,t)t} = \sum_{k=-4}^{4} \beta_1^k \mathbf{1}[YST = k] * \mathbf{I}_i^m * Treat_{j(i,2010)} + \sum_{k=-4}^{4} \beta_2^k \mathbf{1}[YST = k] * Treat_{j(i,2010)}$$
$$+ \beta_3 \mathbb{I}_i^m * Treat_{j(i,2010)} + \sum_{k=2007}^{2018} \gamma_k \mathbf{1}[t = k] * \mathbb{I}_i^m + \lambda_i + \lambda_j + \lambda_t + \varphi X_{it} + \epsilon_{ij(i,t)t}, \quad (2)$$

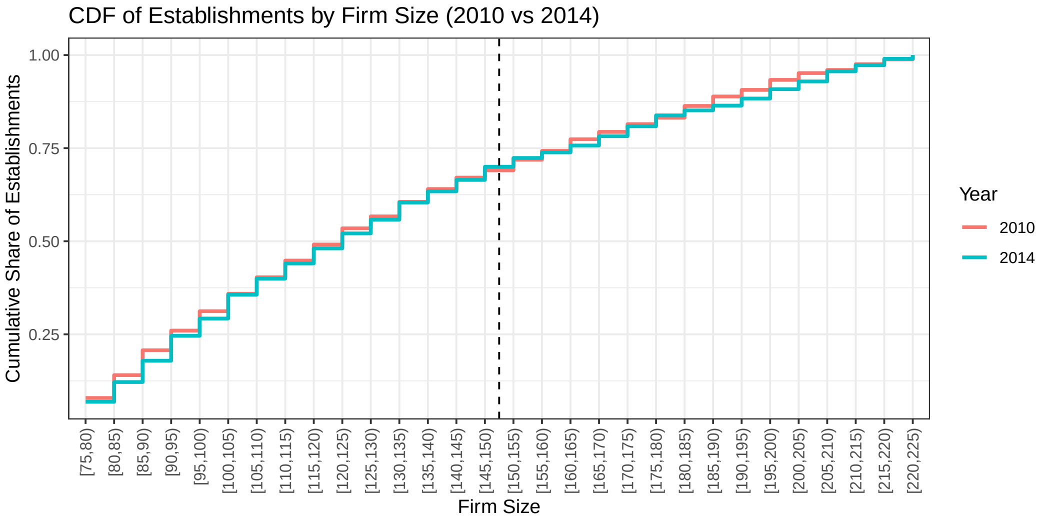
where all variables have the same definition as above except that we now define the treatment status based on the firm size in 2010 and replace the year dummy $\mathbf{1}[t = k]$ with a "years-since-treatment" (YST) dummy $\mathbf{1}[YST = k]$. We choose 2010 as the base year for defining the treatment status as this is the last pre-treatment year for the largest firm size group (more than 1000 employees). Moreover, we replace the year dummy by the years-since-treatment dummy because the different firm size groups are treated at different points in time. Hence, we recenter the actual treatment for each firm at YST equal to 0, which corresponds to different calendar years for each treatment group, e.g. 2011 for the largest firm size group (more than 1000 employees) and 2014 for the smallest firm size group (150 - 249 employees). We include four pre- and post-treatment years in our analysis, which corresponds to the number of pre- /post-treatment years we can observe for all treated firm-size groups. Observations outside this window are binned and are included in the first/last window respectively. The β_1^k coefficients inform us about the evolution of the gender pay gap in treated firms relative to their specific treatment date and relative to never-treated firms after controlling for year, worker, and firm specific heterogeneity (fixed effects), again omitting period $k = -1$.

Before presenting our results, we briefly discuss two key identifying assumptions for unbiased estimates in our context. First, we have to impose the parallel trend assumption: The gap between male and female wages in the control (75-149 employees) and treatment group (150-225 employees) exhibits the same trends absent any policy change. If this holds, we can attribute any post-transparency deviations between the groups to the policy. While not directly testable, the estimated coefficients β_1^k for pre-treatment years show that the difference in the gender wage gap between treated and control groups is not significantly different from zero (see Figure 2). Note that this also precludes anticipation effects: If treated firms respond to the reform prior to the actual reform date, for example by eliminating unfair pay practices, then this would also show up as a deviation from the parallel trend assumption.

A second concern is that firms use the time between the implementation of the reform in 2011 and its effective date in 2014 to downsize and locate themselves right below the 150 employee cutoff, this avoiding treatment in 2014. If the worst offenders (largest gender pay gap) from among this sample, move below the cutoff, then our estimates will be biased towards zero. To show that this does not pose a threat to identification, we show in Figure 1 that the firm size distributions are almost identical in 2010 and 2014, and there is no evidence of bunching around the threshold.

Figure 1: Cumulative Firm Size Distribution in Baseline Sample

The figure below shows the cdf of firm size distribution for the years 2010 (before the policy was announced) and for 2014 (one year after the policy was fully implemented for all firms with more than 150 employees). The figure shows that there is virtually no change in the size distribution between these two years.



In Appendix Figure A2 we check for violations of intended treatment rule by establishments after the policy was implemented and in Appendix Figure A3 we plot the year-on-year transitions of establishments in treated and control groups across the size cutoff. These figures additionally show that even though there were some violations of the intended treatment rule, the proportions are in line with pre-policy firm size dynamics, thus further ruling out strategic bunching.

5 The Effects of Pay Transparency

5.1 Effects on Gender Wage Gap and Wages

In line with the primary goal of the Austrian Pay Transparency law, we begin by examining its effect on the gender gap in daily wages. Panel (a) in Figure 2 shows the estimated coefficients β_1^k from equation (1), which measure the evolution of the gender wage gap (male wage premium) in treated establishments relative to those in the control group. First, we check that the parallel trend assumption is satisfied. Studying the coefficients in pre-treatment years, we find little evidence for any statistically and economically distinct evolution of the gender wage gap in treated versus control firms. There is a noticeable, but statistically insignificant dip in the gender wage gap around the great recession. In Appendix Figure A4 we show that this dip occurs in both treated and untreated firms and is only somewhat (by about 0.5 percentage points) more pronounced in treated firms. By the time the policy is implemented in 2014, gender wage gap in both groups had recovered to their pre-recession levels.

Post-treatment, we also find little evidence for any significant and economically meaningful effects of the reform on the gender wage gap. The gender wage gap between treated and control group started opening up only in 2015, and we can rule out at the 95% confidence level that by the end of our study period the policy narrowed the gender wage gap by more than 0.3 p.p..

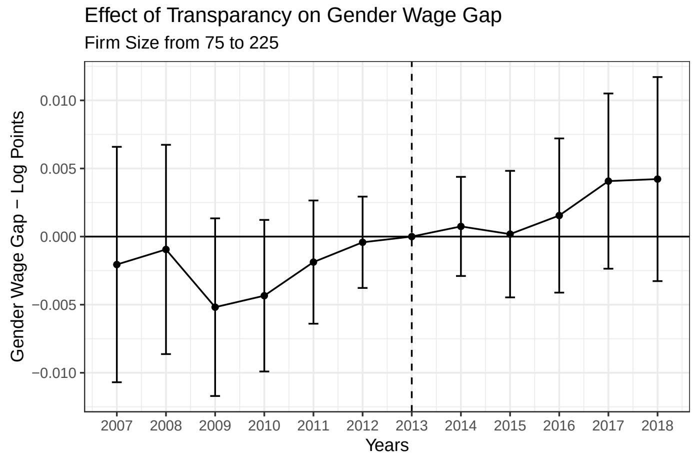
In Panel (b) and (c) we plot the effects on male ($\beta_1^k + \beta_2^k$) and female (β_2^k) wages respectively. Female wages are virtually unchanged after 2013, whereas male workers in treated firms have seen a modest increase of 0.25 p.p. compared to the control group. Both effects are statistically insignificant, although they 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.

While our baseline sample focuses on firms around the threshold to make our control and treatment group as comparable as possible, we next investigate whether transparency had an effect in larger firms by studying the full roll-out over all firm size groups. Figure 3 presents the estimation results for β_1^k from the staggered difference-in-difference model detailed in equation (2). Again, these coefficients inform us about the evolution of the gender pay gap (male wage premium) in treated establishments relative to those in the control group. Including all firm size groups eventually treated does not change the results found in the baseline sample. There are no discernible pre-trends and post treatment there is little evidence for any significant and economically meaningful effects on the gender wage gap. As above, these effects are precisely estimated and we can rule out any effect greater than 0.5 p.p. except for the last year, where the effect is somewhat less precisely estimated.

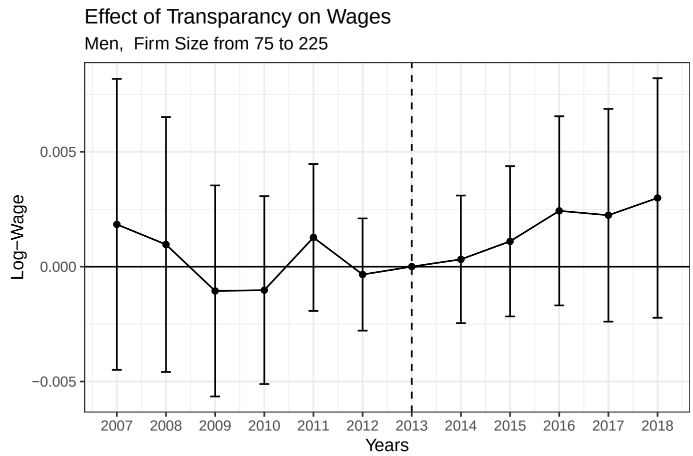
Since pay reports are only available to current employees, the reports might have a limited impact on wages of newly hired employees. Even after joining a company with a pay report, it might take some time until the employee is able to act upon the information provided in the wage reports and renegotiate their contract. Therefore, it is possible that transparency

Figure 2: Effects of Pay Transparency on Gender Wage Gap and Daily Wages

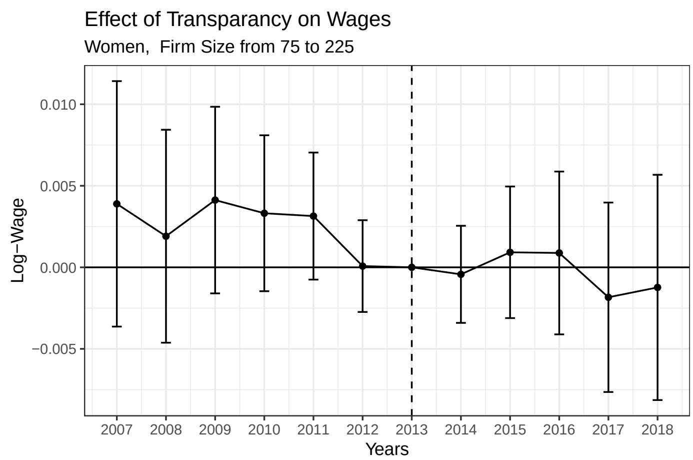
The figure below plots the evolution of the gender gap in daily wage (panel a), male (panel b) and female wages (panel c), in treated establishments relative to the control group in log points (Eq. (1)). The sample is restricted to firms with 75-225 employees. Treatment is assigned to firms which had more than 150 employees in 2013. Standard errors are clustered at the establishment level. The standard error spikes represent 95% CI. All regression results can be found in Appendix Table A2.



(a) Gender Wage Gap (Male-Female)



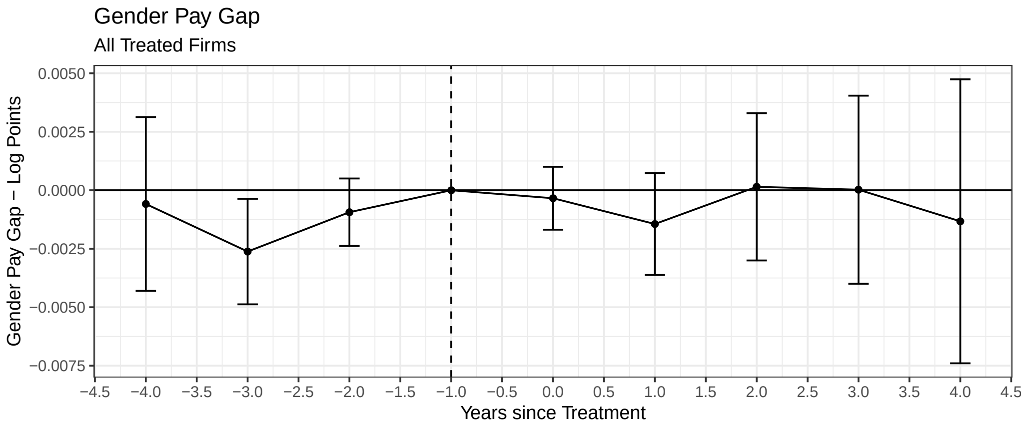
(b) Male log(Daily Wage)



(c) Female log(Daily Wage)

Figure 3: Effects of Pay Transparency on Gender Wage Gap

The figure below plots the evolution of the gender gap in daily wages in treated establishments relative to the control group in log points based on the staggered difference-in-difference model in equation (2). The sample is restricted to firms above 75 employees. Treatment is assigned based on the 2010 firm-size and the treatment time is re-centered around 0, which is the first treatment year. We bin years outside our event windows adding them to the initial (final) bin respectively. Standard errors are clustered at the establishment level. The standard error spikes represent 95% CI.

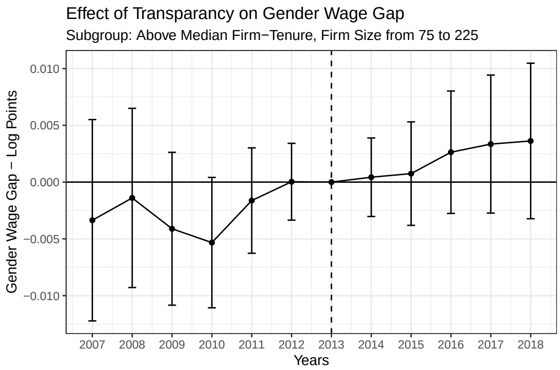


has significant effects only for those who have been with their current employer for a while. To investigate whether this group drives our zero results, we re-estimate equation (1) on the sample of workers with above 3.5 years of firm tenure, which is the median value in our baseline sample. The results displayed in panel (a) of Figure 4 below show that there is no discernible effects of transparency for high tenure workers.

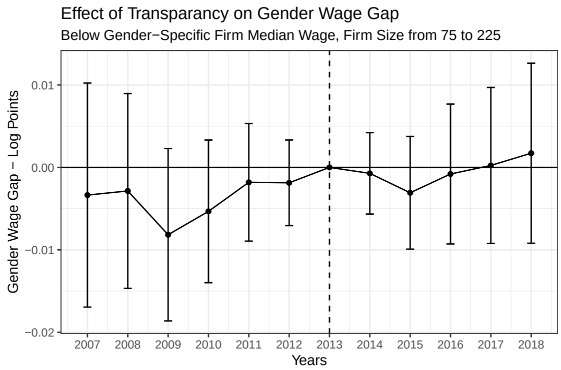
Furthermore, since the pay reports reveal average wages by gender and occupation, it is more likely that workers with below-average earnings are the ones who benefit from this act. Therefore, the most relevant group to look at would be workers who earn less than their occupation and gender-specific average within the firm. However, in the absence of detailed occupational information in the data, the closest we can investigate are workers who earn below their firm and gender-specific median wage. We estimate the gender wage gap for this group and plot the coefficient estimates in panel (b) of Figure 4 below. Again as before, pay transparency had hardly any effects even for this subgroup of workers.

Figure 4: Effects of Transparency on Gender Wage Gap (GWG)

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



(a) **Above Median Tenure**



(b) **Below Median Firm-Gender-Specific Wage**

5.2 Robustness Checks

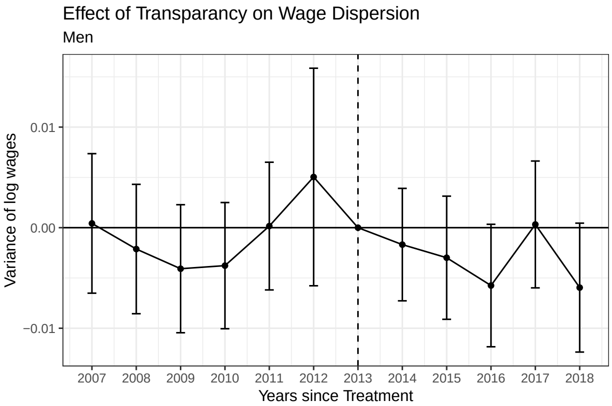
In the Appendix we show that the results of our baseline specification hold under multiple robustness checks with different sample and treatment definitions. In Appendix Figure A5 we restrict our sample to firms with 100-200 employees in 2013. In contrast to our main analysis sample, we include all topcoded workers in Appendix Figure A6 and drop all ever-topcoded workers in Appendix Figure A7. For Appendix Figure A8 we include only those firms which do not change their intended treatment assignment based on their size in 2013. We also change the definition of treatment in the following two ways. In Appendix Figure A9 we define firm treatment status based on their size in 2010, instead of 2013. For Appendix Figure A10 we assign treatment status to workers (instead of firms) depending on whether they worked in a firm with more than 150 employees in 2013. In Appendix Table A2, we re-estimate the gender wage gap results for our main sample with match fixed effects instead of worker and firm fixed effects. Finally, in Appendix Figure A11 we re-estimate the effects of transparency at the firm-year level and thus on the firm level gender wage gap.[16]

All these studies confirm our main results: pay transparency had no economic or statistically significant effects on the gender wage gap and individual wages.

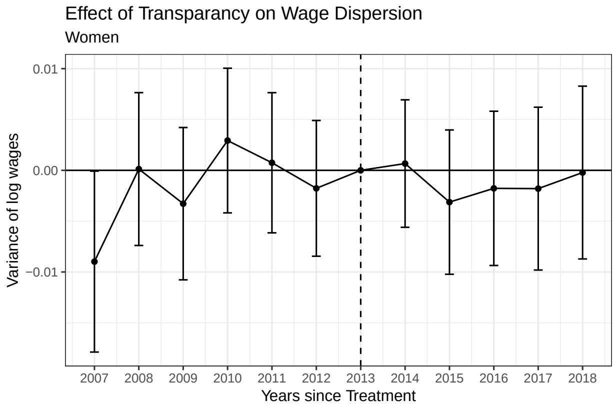
[16]The appendix section A.5 describes the regression specification in detail.

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 (Eq. (3)). The sample is restricted to firms with 75-225 employees in 2013. Standard errors are clustered at the establishment level. The standard error spikes represent 95% confidence intervals.



(a) Male



(b) Female

5.3 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 highly paid individuals. To check whether this was indeed the case, we estimate the effects of the pay transparency on the establishment-level variance in male and female wages separately by estimating the following model in our baseline sample:

$$wvar_{jt} = \sum_{k=2007}^{2018} \beta^k \mathbf{1}[t = k] * Treat_{j(2013)} + \lambda_j + \lambda_t + \epsilon_{jt}, \tag{3}$$

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 (1). A negative β_k coefficient implies that the variance narrowed in post-treatment years relative to the control group, which implies wage compression. The results are displayed in Figure 5. Transparency has no statistically significant effects on the establishment-level variance in wages for either men or women. Moreover, we do not find evidence for any discernible pre-trends in wage variances either.

An alternative way to study the effects of the policy on wage compression is to estimate the impact separately on workers earning below and above their respective gender-specific average firm wage. In the appendix, Figure $A12$ show that the policy had little effect on the wages of any subgroup. All in all, we do not find any compelling evidence for wage compression within establishments.[17]

6 Why Was the Reform not Effective?

Our results show that the pay transparency law failed to narrow the gender wage gap and has not affected wage setting in general. Can we attribute this ineffectiveness to incomplete implementation or to the lack of unfair compensation practices? 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 survey of work councils (Arbeiterkammer, 2014), in 54% of cases the company cooperated with the works council in generating these pay reports. 71% of respondents reported that the reports are informative and 63% claimed that they are useful for work councils. Taken together, incomplete implementation is unlikely to explain the zero effect.[18]

As argued in the the conceptual framework, pay transparency can only affect the gender wage gap if firms have meaningfully different pay policies towards men and women. In addition, these wage differences have to be perceived as unjustified, and workers need to have the bargaining power to renegotiate wages. To understand why the policy did not affect the wage setting in Austria, we proceed as follows. We estimate the wage differences not accounted for by worker characteristics, as well as the gender gap in firm pay policy, to quantify the extent of within-firm wage variations across workers. Furthermore, we provide additional evidence on how workers perceive these wage differences by studying turnover as a proxy for job satisfaction.

6.1 Unexplained Wage Differences Within Firms

Wage reports try to address wage inequality originating from different information on firm's willingness to pay. Hence, it address the within firm wage variation not accounted for by worker characteristics. To understand how large these wage differences are in treated firms before the wage transparency reform, and thus how much 'bite' transparency potentially has, we follow Song et al. (2018) and Lamadon et al. (2020) by decomposing the overall wage variation into

[17]Including establishment-year level aggregates in (3) does not change our results.

[18]As mentioned in section 2, the gender pay gap is also prominently discussed in the media twice a year at the "Equal Pay Days", once in spring and once in fall. In this context, pay reports are also mentioned and discussed. This suggests that society as a whole is aware of both the gender pay gap and the pay transparency legislation meant to tackle it.

a between and within firm component, which we further split up into the part explained by worker characteristics and a residual component. Following Abowd et al. (1999) (AKM), the log wage w_{it} of a worker i at time t is assumed to reflect permanent worker productivity α_i, plus enumeration for time-varying observable characteristics X'_{it}, a firm pay premium common to all workers $\psi_{J(it)}$, in addition to a residual:

$$w_{it} = \alpha_i + X'_{it}b + \psi_{J(it)} + \epsilon_{it}. \tag{4}$$

The variation in the wage residual is of particular interest, as these include differential bargaining positions of workers or different firm pay policies across workers. Consequently, pay transparency policies have the most direct effect on this component. To understand whether these components are important factors in wage setting in Austria, we first estimate equation (4) on the universe of social security observations from 2007-2013. The social security data lacks detailed worker characteristics, thus X'_{it} only contains a cubic polynomial in age. Therefore, any unobserved time-varying worker characteristic will be captured in the variation of the wage residual. We then decompose log-wage variation in treated firms from our baseline sample into a within-firm and between firm component in equation (5). Consequently, we use the AKM model of equation (4) to further decompose the within firm variation into a part that is explained by permanent and time-varying worker characteristics and the residual component. Equation (6) presents this step.

$$\begin{aligned} Var(w_{it}) &= \underbrace{Var(w_{it} - \mathbb{E}[w_{it}|J(i,t) = J])}_{\text{Within-firm}} + \underbrace{Var(\mathbb{E}[w_{it}|J(i,t) = J])}_{\text{Between-firm}} && (5) \\ &= \underbrace{Var(\alpha_i + X'_{it}b - \mathbb{E}[\alpha_i + X'_{it}b|J(i,t) = J])}_{\text{Worker Heterogeneity within firms}} + \underbrace{Var(\epsilon_{it})}_{\text{Residual}} \\ &\quad + \underbrace{Var(\mathbb{E}[w_{it}|J(i,t) = J])}_{\text{Between-firm}} && (6) \end{aligned}$$

Table 2 presents the results from this decomposition. The pay reports are only available to current employees of the firm. Thus the Austrian pay transparency reform can only directly affect the within firm wage differences, which explains 60 percent of log-wage variation in Austria. The overwhelming majority of this 60 percent is explained by the variation in the fixed worker component in addition to age. Despite lacking detailed worker characteristics, only 17 percent of the within firm wage variation, or 10% of the total is left unexplained as the residual component. Thus, differential bargaining outcomes, which are most directly targeted

Table 2: Decomposition of log daily wage variation into within and between firms

Using equation (5) we decompose the total variation in log daily wages into a between firm component and a within firm component. The second part can be further divided into variation explained by worker heterogeneity and a residual. We do this for workers in treated firms (150-225 in 2013) before the roll out of pay transparency for this group (2007-2013).

	Expression	Share	Level
Total log-wage variation	$\text{Var}(\text{w}_{it})$	1.00	0.203
Between Firm	$Var(\mathbb{E}[w_{it}\|J(i,t)=J])$	0.41	0.083
Within Firm	$Var(w_{it}-\mathbb{E}[w_{it}\|J(i,t)=J])$	0.59	0.120
Decomposition Within Firm			
Worker heterogeneity	$\text{Var}(\alpha_i + X_{it}'b - \mathbb{E}[\alpha_i + X_{it}'b\|J(i,t)=J])$	0.837	0.101
Residual	$\text{Var}(\epsilon_{it})$	0.174	0.021

by transparency, do not play an out-sized role explaining wage differences.

Even if the residual wage variation is small, pay transparency might still be effective if workers perceive these differences as unjustified and have the bargaining power to renegotiate them. Past research has shown that workers who feel unfairly compensated have lower job satisfaction and a higher quit rates (Card et al., 2012, Rege and Solli, 2015, Dube et al., 2018). If the reports show evidence of pay discrimination or unfair wage differences, 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 (1):

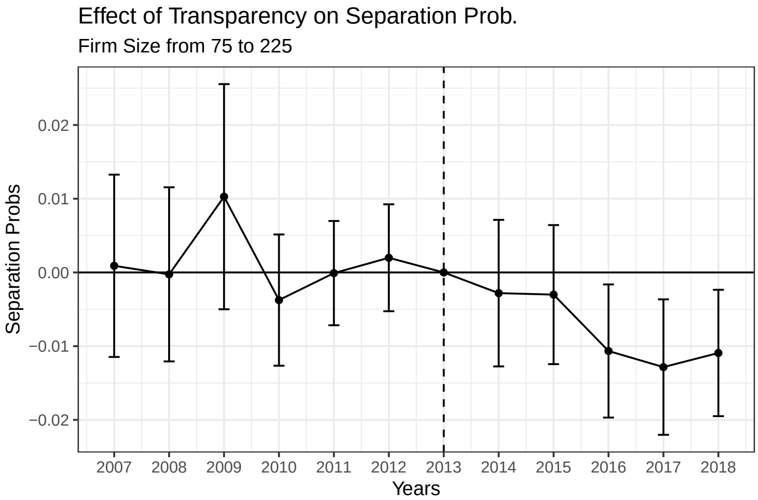
$$sepa_{ijt} = \sum_{k=2007}^{2018} \beta^k \mathbf{1}[t=k] * Treat_{j(i,2013)} + \sum_{k=2007}^{2018} \gamma_k \mathbf{1}[t=k] * \mathbb{I}_i^m + \lambda_j + \lambda_i + \lambda_t + \varphi X_{it} + \epsilon_{ij(i,t)t}, \quad (7)$$

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 (1).

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

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 (Eq. 7). 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.



compared to pre-treatment levels.[19] 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.

We also estimate the gender specific effects of transparency on job separation using the specification of equation (1). In Appendix Figure $A13$ we show that these effects apply to women as well, which might suggest that women also did not perceive unfair firm pay practices. In light of the large unadjusted gender wage gaps, this conclusion might be surprising. However, it is important to note that men and women might receive different firm pay premia because of two reasons. First, firms might discriminate between men and women in their firm pay policies. Second, men might work on average for better paying firms compared to women. Wage reports are company secret in Austria, which can only directly affect gender pay disparities at the firm level. But the information is not available to outside job seekers, and therefore cannot directly affect the sorting of workers across firm pay levels. Thus, in the next section we decompose gender differences in firm pay into these two channels.

6.2 Gender Differences in Firm Pay Policy

To understand the potential 'bite' of the reform, we estimate the difference in firm pay across gender following Card et al. (2016). We use the wage equation (4), but we allow firms to have

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

different pay policies across gender:

$$w_{it} = \alpha_i + \psi_{J(it)}^{G(i)} + X_{it}^{G(i)} b + \epsilon_{it}. \tag{8}$$

w_{it} is the log-wage of individual i in period t, $G(i)$ denotes gender, α_i captures a fixed worker component (due to ability or skills) and $X_{it}^{G(i)}$ represents a gender specific time fixed effect and a cubic 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 (8) measure the firm pay premia relative to a reference firm for female and males. To be able to compare firm wage premia across gender, we need to normalize the firm effects. We follow Card et al. (2016) and assume that the lowest paying firms do not offer any pay premia. Motivated by an extensive literature documenting that the restaurant industry has the smallest wage premia on average (see e.g. Krueger and Summers (1988)), we normalize the effects such that the restaurant industry have zero firm effects for both genders.[20] We then decompose the difference between the average pay premium received by men $E[\psi_{J(it)}^{M}|male]$ and women $E[\psi_{J(it)}^{F}|female]$ into a within firm component and a sorting effect:

$$E[\psi_{J(it)}^{M}|male] - E[\psi_{J(it)}^{F}|female] =$$

$$\underbrace{E[\psi_{J(it)}^{M} - \psi_{J(it)}^{F}|female]}_{\text{Within Firm}} + \underbrace{E[\psi_{J(it)}^{M}|male] - E[\psi_{J(it)}^{M}|female]}_{\text{Sorting}} = \tag{9}$$

$$\underbrace{E[\psi_{J(it)}^{M} - \psi_{J(it)}^{F}|male]}_{\text{Within Firm}} + \underbrace{E[\psi_{J(it)}^{F}|male] - E[\psi_{J(it)}^{F}|female]}_{\text{Sorting}} \tag{10}$$

The first term in equation (9) is the gender gap in firm pay policy within firms across the distribution of jobs held by women. The second term measures the difference originating from the fact that women and men work for different firms with different pay levels, which we label as the sorting effect. Equation (10) presents the same decomposition, but using the male job distribution instead.

We are now in a position to estimate the differences in firm-specific pay premia across gender in treated firms before the policy roll-out. We estimate equation (8) on all observations before 2013 and apply the decomposition in equation (9) and (10) to treated firms in our baseline sample (firm size 75-225). Table 3 presents the results of this decomposition. The first column shows that men in treated firms before the transparency policy obtained on average

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

Table 3: Decomposition of pre-treatment gender wage gap into *Within-Firm* and *Sorting Effects*.

The table below shows the decomposition of the gender wage gap during 2007-2013 into a within-firm effect and a sorting effect for treated firms (size 150-225 in 2013), following equation (9) *and* (10). *The two rows show this decomposition using female and male job distributions respectively.*

	(1)	(2)	(3)	(4)	(5)
		Within Firm		Sorting	
	Gender Wage Gap	Level	Share	Level	Share
Decomposition (Female dist.: Eq. (9))	0.127	0.036	0.281	0.092	0.719
Decomposition (Male dist.: Eq. (10))	0.127	0.049	0.387	0.078	0.613

a 12.7 percent higher firm pay than women. The dominant factor behind this difference is, that women tend to work for lower paying firms. As the last column shows, this sorting effect explains between 61 and 72 percent of the overall gap, depending whether we use the male or female job distribution for the decomposition. This accounts for 8 to 9 percentage points of the gender wage gap. Columns (2) and (3) in contrast show that a much smaller share of the overall gap is explained by gender differences in firm pay policies within companies. This gap amounts to between 3.6 and 4.9 percentage points, and thus explains only 28 to 39 percent of the overall gender gap in firm wage premia. This decomposition is similar to what was found in other settings. Card et al. (2016) finds that the firm pay policy gender gap in Portugal is only between 0 and 1.5 percentage point, whereas Morchio and Moser (2019) finds 0 to 2 percentage point differences in within firm pay policy gender gaps in Brazil. As argued before, because the wage reports are only available to current employees, the Austrian transparency policy can only directly address the gender pay gap arising from firms following differential pay policies for men and women. As the above results show, these difference tend to be rather small, perhaps too small to overcome any cost of wage renegotiations. Thus, any transparency reform that by design only targets differential firm pay policies within companies, will likely only have a limited effect on the gender wage gap.

6.3 Firm-level Pay Transparency versus Market Level Transparency

Our decomposition reveals that pay transparency reforms where the wage reports are public information could have a larger 'bite', as women can use this information to direct their search towards more equitable firms. The UK reform of 2017 provides an example for such a transparency policy. Every company above a size threshold has to publicly report income statistics by gender. Although Duchini et al. (2020) does not study how the reform affected sorting across firms, it provides evidence that it lead to more women being employed in above-median-wage occupations. An additional advantage of the public nature is that the policy is more salient, as the gender wage gaps reported are regularly discussed in the media.[21] If consumers have a preference for gender equality in pay, the public scrutiny can put additional pressure on firm to equalize earnings (Blundell, 2020). Another example from Canada is Baker et al., 2019, where public access to information about the salaries of university faculties led to a reduction in the gender wage gap. The most prominent design difference of these reforms compared to our setting is, that the wage reports in Austria are company secret and not available to the public. Perhaps this public nature is therefore an important element for effective transparency reforms.

7 Conclusion

Pay transparency is often prescribed as an instrument to close the gender pay gap, and reduce wage inequality. 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 an event-study design and administrative data from social security records, we show that the transparency policy neither affected male and female wages nor did it narrow the gender wage gap. 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.

To understand why the reform did not affect the wage setting, we proceed in two steps. First, we show that 83% wage variation within firms is accounted for by the worker characteristics. Even if the residual wage variation is small, wage transparency might still be effective if workers perceive these differences as unjustified and have the bargaining power to renegotiate them. If however, workers have no recourse to renegotiation then their job satisfaction might decrease. Since past research has shown that workers who feel unfairly compensated have lower job satisfaction and higher quit rates (Card et al., 2012; Dube et al., 2018; Rege and Solli, 2015), we estimate how the separation rate in treated firms changes after the policy reform to provide some evidence on how workers have perceived the revealed information from pay reports. We

[21] The Independent, a newspaper in the UK, regularly publishes the worst offenders in terms of gender pay gap based on the UK transparency reform. `https://www.independent.co.uk/news/business/news/gender-pay-gap-worst-offenders-each-sector-revealed-reporting-deadline-passes-a8290566.html`

find that it reduces by 1.1 p.p. (9% relative to pre-treatment mean), which we interpret as suggestive evidence that workers do not perceive the revealed pay schedules as unfair.

Second, we show that gender differences in firm pay policies are small in treated firms and that most of the differences originate from the fact that women work for lower paying firms on average. The Austrian pay transparency reform by design only targets differential firm pay policies within companies and we find that it has limited effects on the gender wage gap. Therefore, a more promising policy approach would be to make pay reports public, thereby allowing workers and especially women to direct their search towards better paying and more equitable firms.

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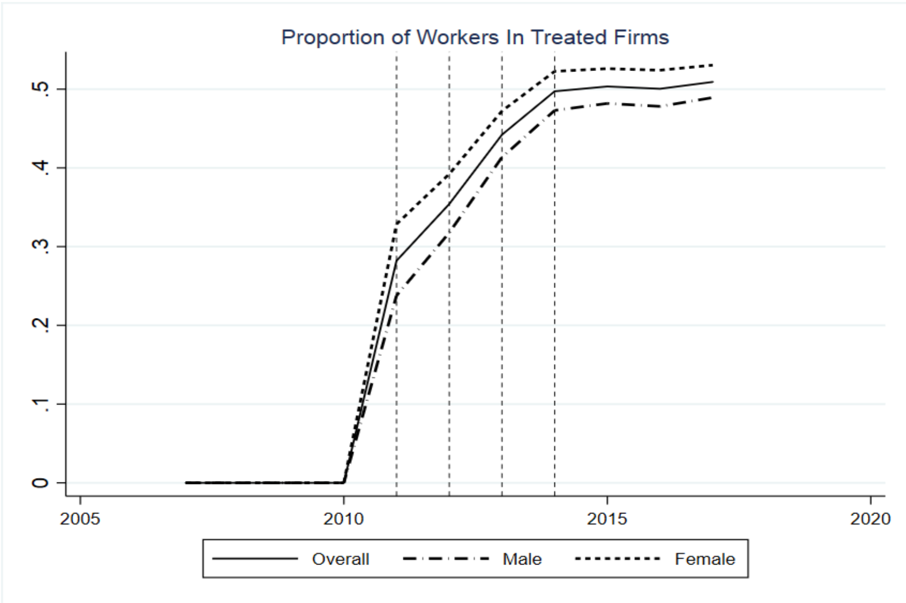
A Online Appendix

A.1 Other Summary Figures

Figure A1: Proportion of Workers Employed in Treated Firms



A.2 Sample Income Report from the Public Sector

Table A1: 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
Federal 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

A.3 Bunching of Establishments

Figure A2: Establishments Violating Intended Treatment Status based on 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.

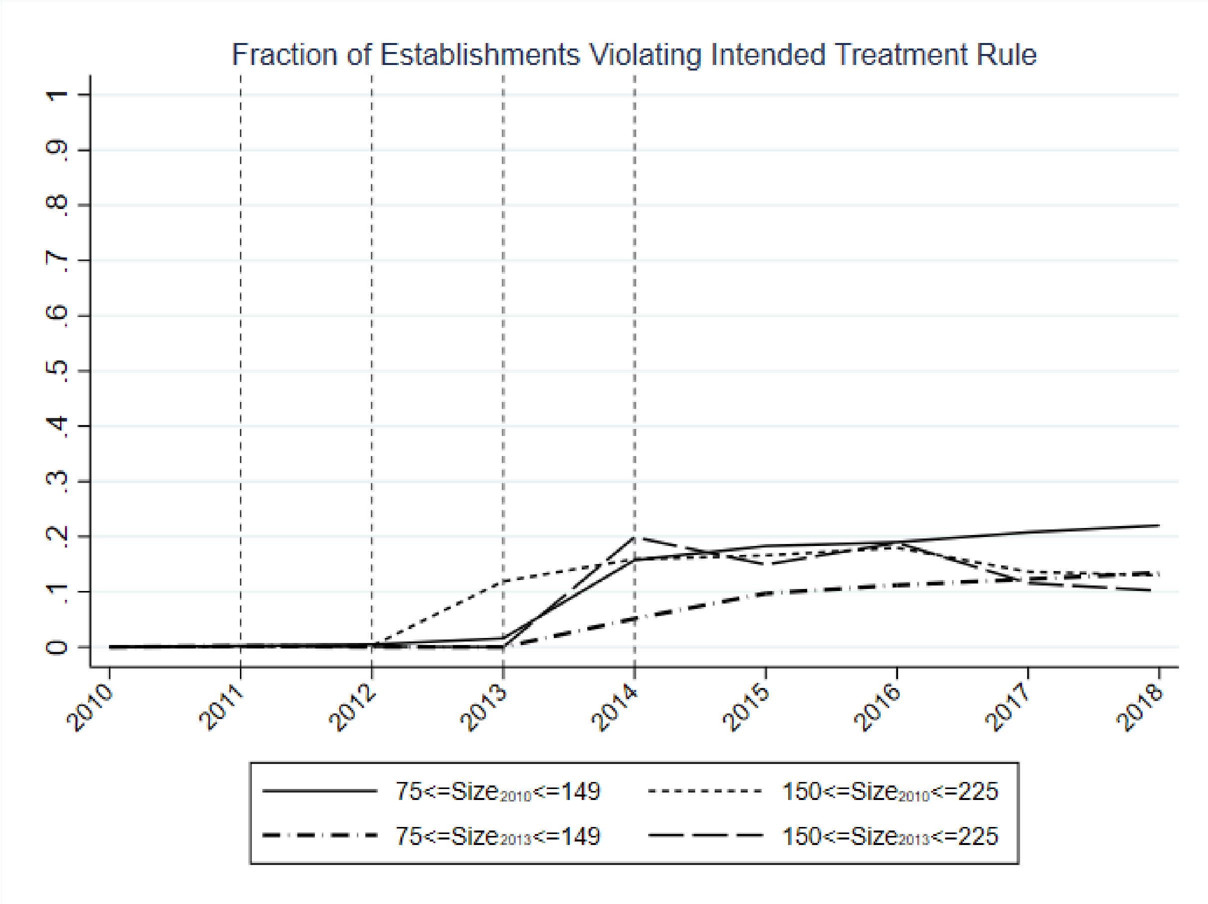
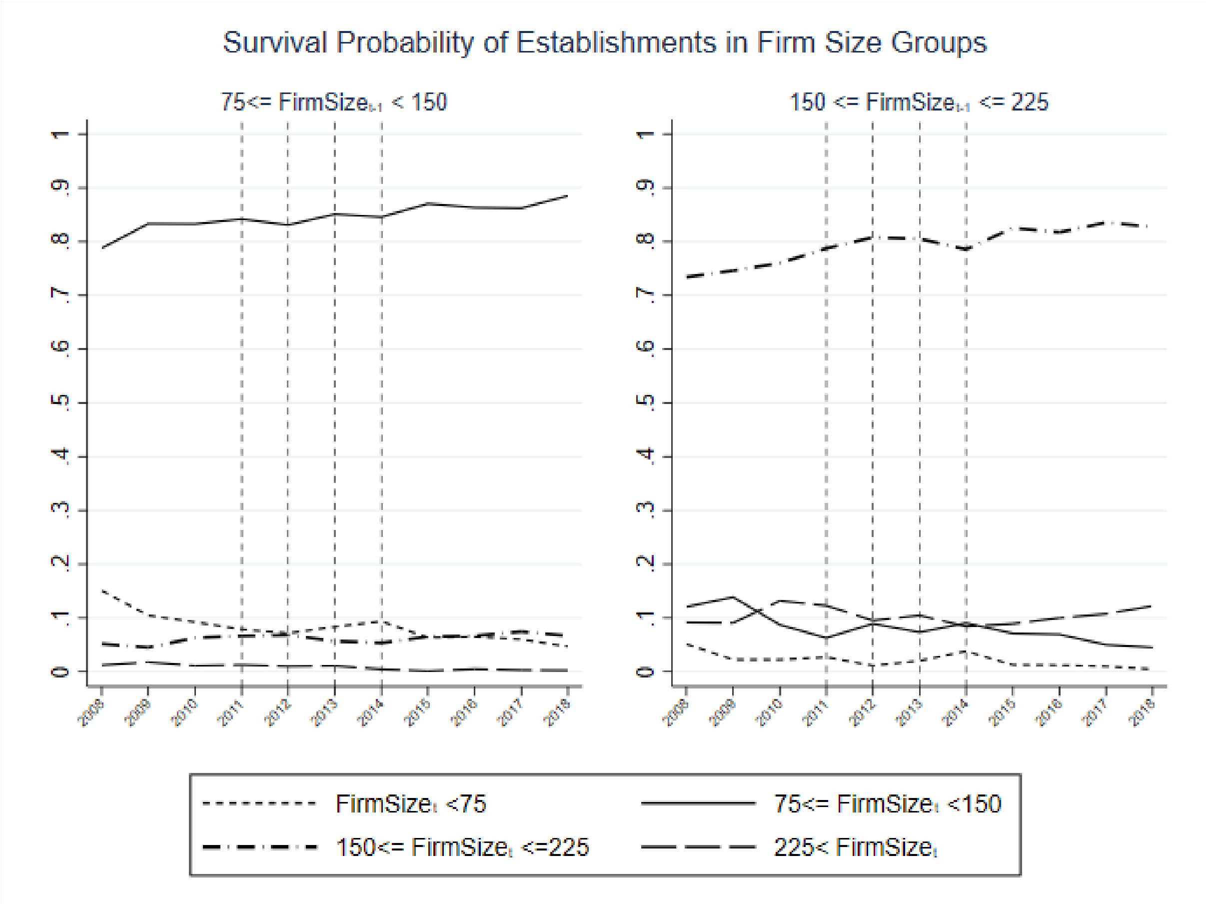


Figure A3: Transitions of Establishments Across Firm Size Groups

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. We do this exercise for the treated and control groups of firms which represent those just above and below the 150 size-cutoff respectively.



A.4 Robustness Checks

Figure A4: 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.

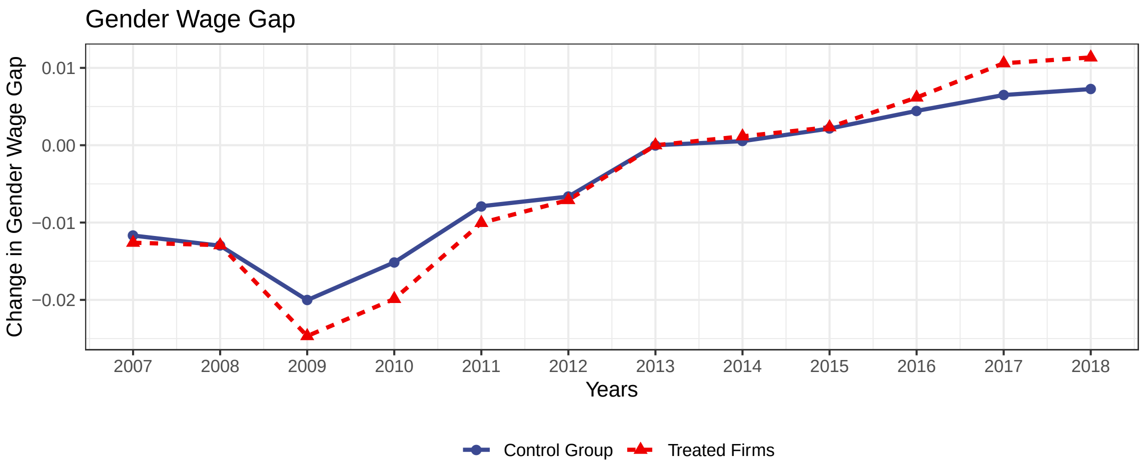
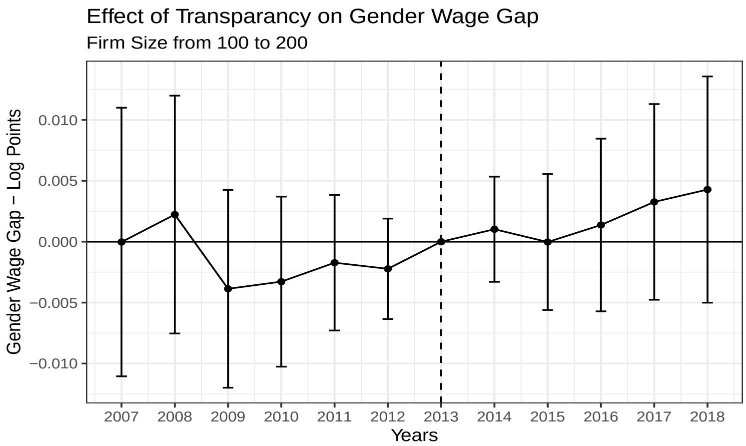
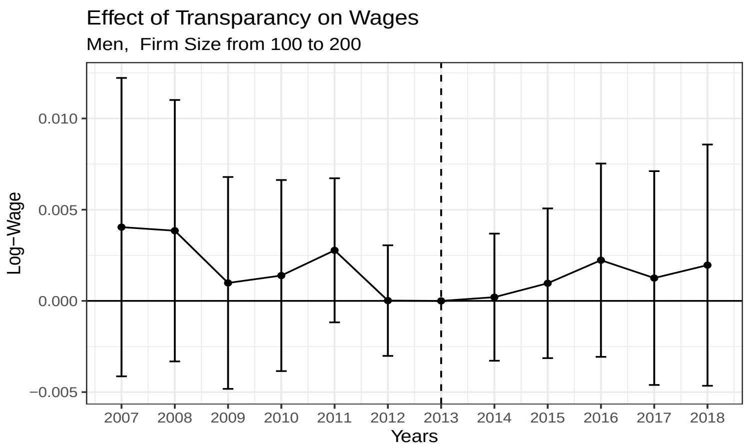


Figure A5: Effects of Transparency on GWG and Daily Wage (100<=Firm Size<=200)

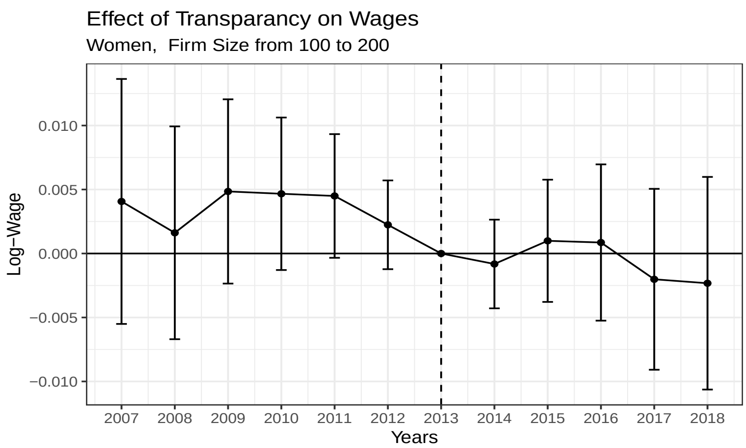
The figure below plots the effects of transparency on gender wage gap (Panel (a)), and daily wages for male (Panel (b)) and female (Panel (c)) workers separately, in firms which had between 100-200 employees in 2013 (Eq. 1). Treatment is assigned to firms which had more than 150 workers in 2013. Standard errors are clustered at firm level. The standard error spikes represent 95% CI.



(a) Gender Wage Gap



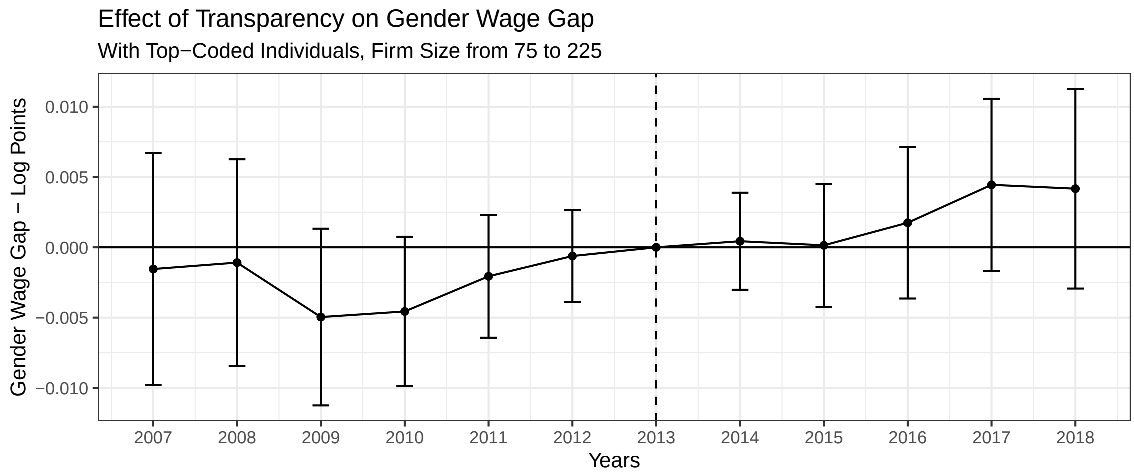
(b) Male Daily Wage



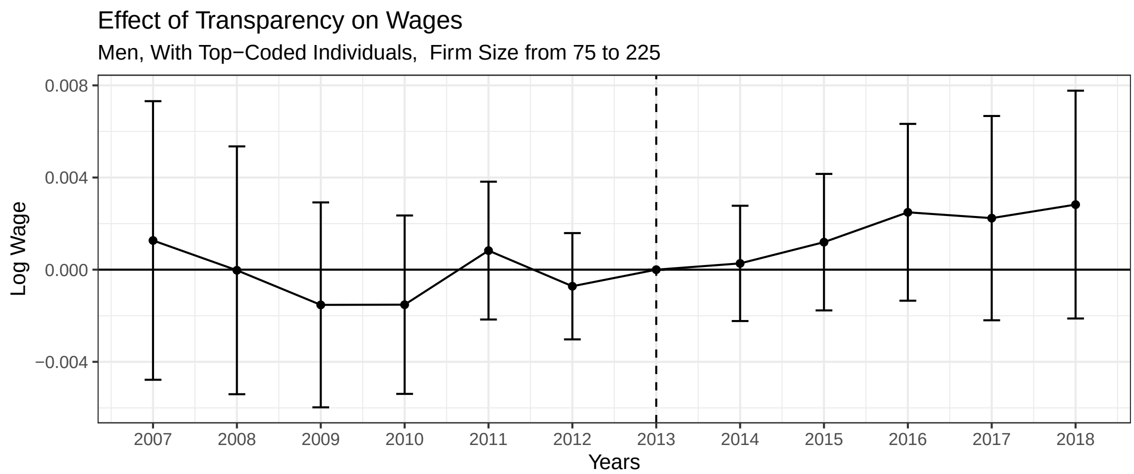
(c) Female Daily Wage

Figure A6: Effects of Transparency on GWG and Daily Wage (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 (Eq. 1). 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 level. The standard error spikes represent 95% CI.



(a) Gender Wage Gap

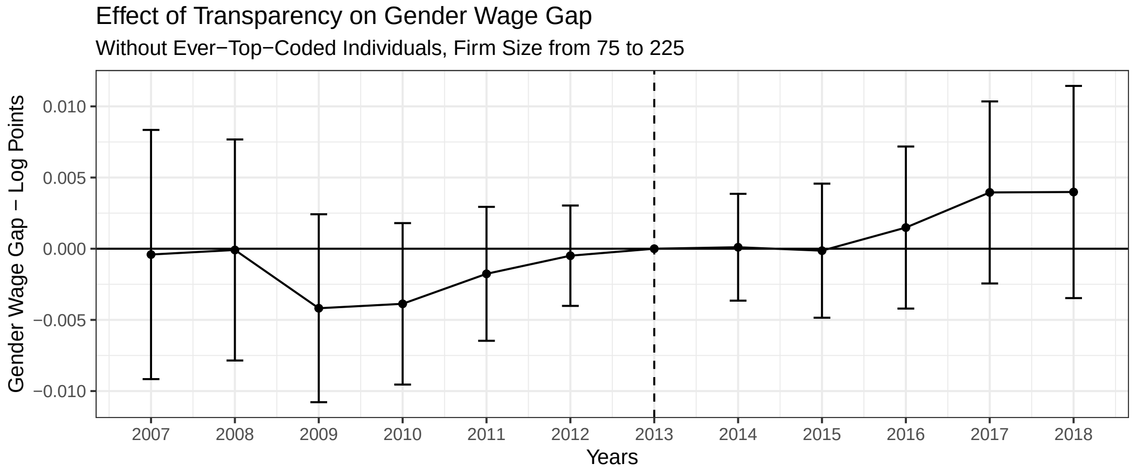


(b) Male Daily Wage

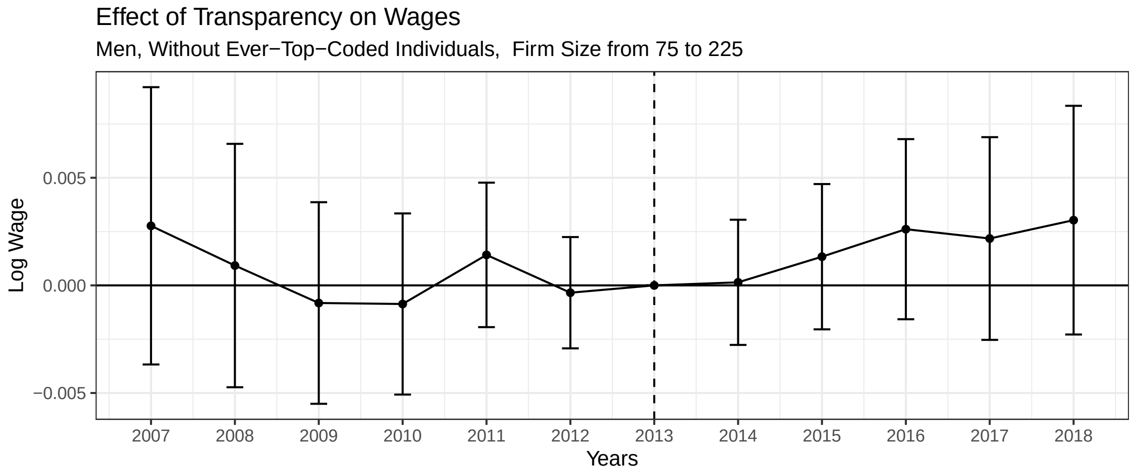
(c) Female Daily Wage

Figure A7: Effects of Transparency on GWG and Daily Wage (Without Ever-Top-Coded)

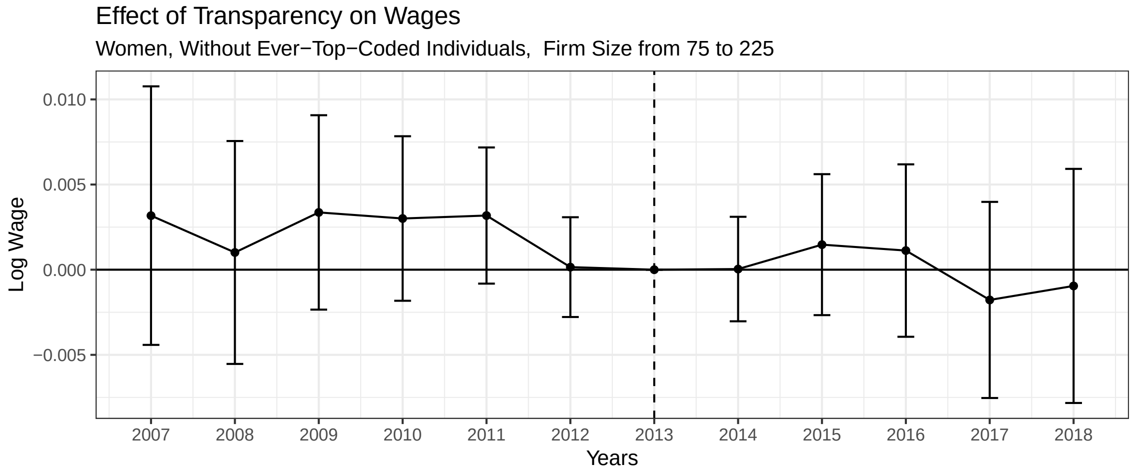
The figure below plots the effects of transparency on gender wage gap (Panel (a)), and daily wages for male (Panel (b)) and female (Panel (c)) workers separately (Eq. 1). The sample is restricted to firms with 75-225 employees in 2013. All workers who were ever top-coded in the sample period are dropped. Standard errors are clustered at firm level. The standard error spikes represent 95% CI.



(a) Gender Wage Gap



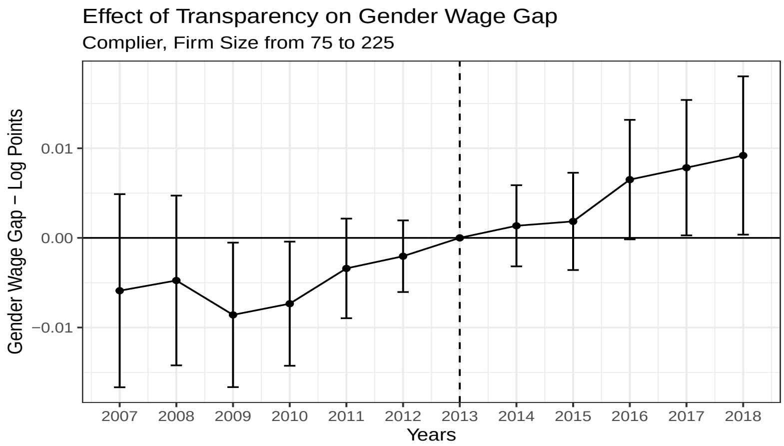
(b) Male Daily Wage



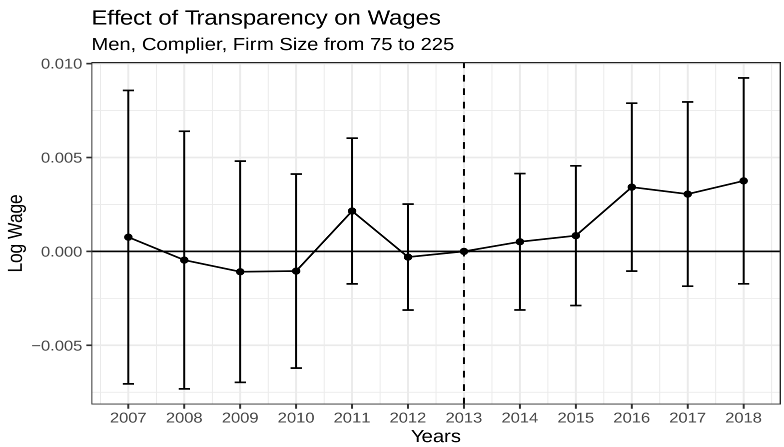
(c) Female Daily Wage

Figure A8: Effects of Transparency on GWG and Daily Wage (Complier Sample)

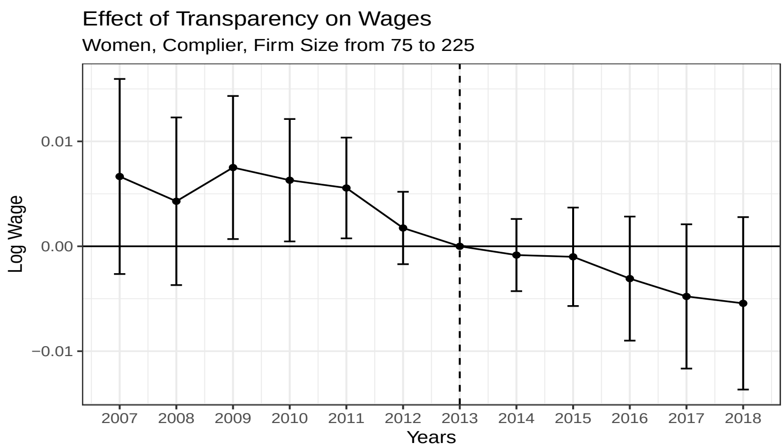
The figure below plots the effects of transparency on gender wage gap (panel (a)), and male (panel (b)) and female (panel (c)) separately, for those firms which do not change their treatment assignment after 2013. The sample includes only firms with 75-225 employees in 2013. Standard errors are clustered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



(a) Gender Wage Gap



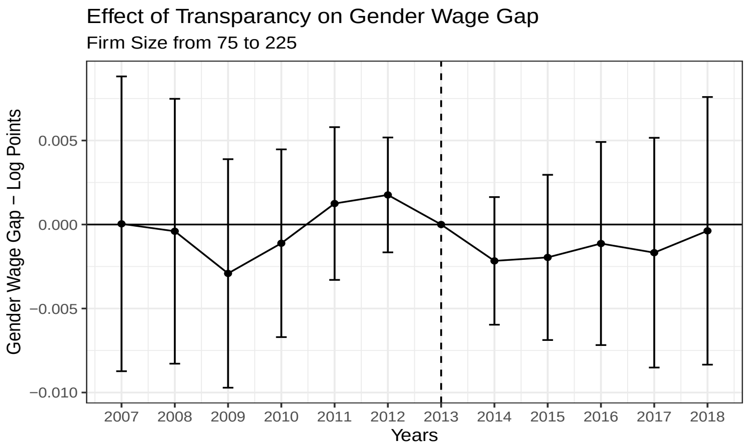
(b) Male Daily Wage



(c) Female Daily Wage

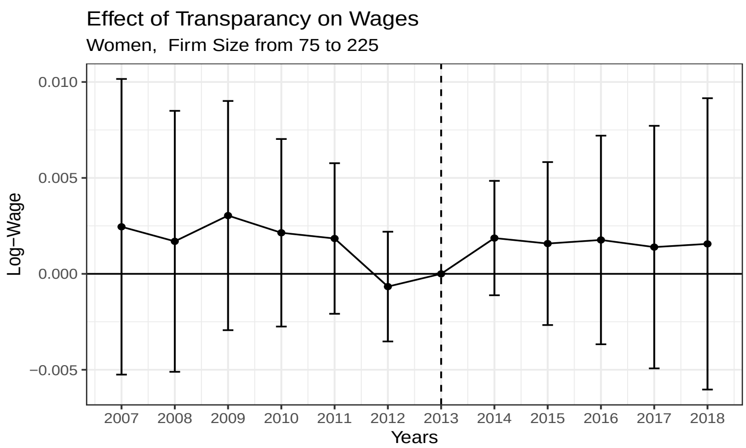
Figure A9: Effects of Transparency on GWG and Daily Wage (Treatment Defined as of 2010)

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. The rest is as specified in equation (1). Standard errors are clustered at firm level. The standard error spikes represent 95% CI.



(a) Gender Wage Gap

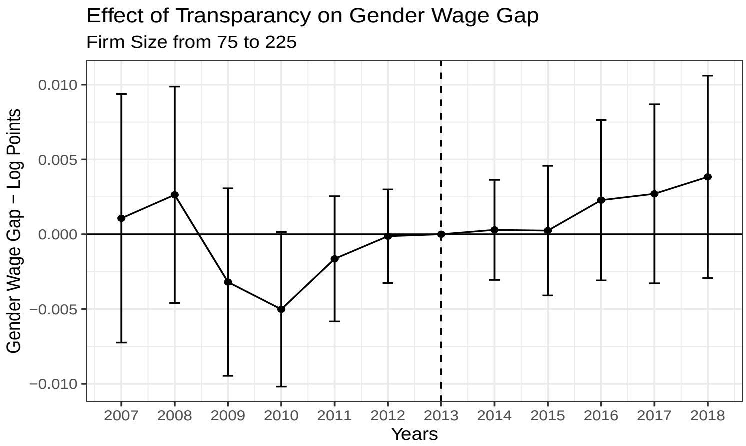
(b) Male Daily Wage



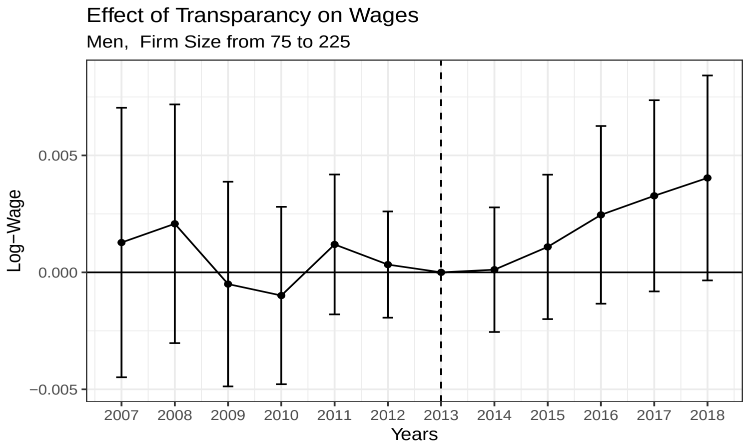
(c) Female Daily Wage

Figure A10: 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. The rest is as specified in equation (1). Standard errors are clustered at firm level. The standard error spikes represent 95% CI.



(a) Gender Wage Gap



(b) Male Daily Wage

(c) Female Daily Wage

Table A2: Effects of Pay Transparency on Gender Wage Gap

	Dependent variable: ln(Daily Wage)			
[-1.8ex]	(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***1**[t=2007]	−0.01	−0.01	−0.002	−0.001
	(0.01)	(0.01)	(0.004)	(0.005)
Male*Treat***1**[t=2008]	−0.01	−0.01	−0.001	0.001
	(0.01)	(0.01)	(0.004)	(0.004)
Male*Treat***1**[t=2009]	−0.01	−0.01	−0.01	−0.01
	(0.005)	(0.005)	(0.003)	(0.003)
Male*Treat***1**[t=2010]	−0.005	−0.01	−0.004	−0.01
	(0.004)	(0.004)	(0.003)	(0.003)
Male*Treat***1**[t=2011]	−0.004	−0.005	−0.002	−0.003
	(0.003)	(0.003)	(0.002)	(0.002)
Male*Treat***1**[t=2012]	−0.002	−0.002	−0.0004	−0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Male*Treat***1**[t=2013]	0.00	0.00	0.00	0.00
	–	–	–	–
Male*Treat***1**[t=2014]	−0.01	−0.01	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Male*Treat***1**[t=2015]	−0.01	−0.01	0.0002	0.001
	(0.003)	(0.003)	(0.002)	(0.002)
Male*Treat***1**[t=2016]	−0.01	−0.01	0.002	0.002
	(0.004)	(0.004)	(0.003)	(0.003)
Male*Treat***1**[t=2017]	−0.001	−0.002	0.004	0.003
	(0.004)	(0.004)	(0.003)	(0.003)
Male*Treat***1**[t=2018]	0.002	0.001	0.004	0.003
	(0.004)	(0.004)	(0.004)	(0.004)
Treat∗**1**[t=2007]	0.005	0.01	0.004	0.003
	(0.004)	(0.004)	(0.004)	(0.004)
Treat∗**1**[t=2008]	0.003	0.004	0.002	0.001
	(0.004)	(0.004)	(0.003)	(0.004)
Treat∗**1**[t=2009]	0.004	0.005	0.004	0.004
	(0.003)	(0.003)	(0.003)	(0.003)
Treat∗**1**[t=2010]	0.003	0.003	0.003	0.004
	(0.003)	(0.003)	(0.002)	(0.003)
Treat∗**1**[t=2011]	0.01	0.01	0.003	0.003
	(0.003)	(0.002)	(0.002)	(0.002)
λ_j	✓	✓	✓	
f(Age)$*\mathbb{I}^m$		✓	✓	✓
λ_i			✓	
λ_{ij}				✓

Continued on next page

Table A2 – continued from previous page

	(1)	(2)	(3)	(4)
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)
1[t=2009]	−0.001	0.004	−0.01	−0.01
	(0.002)	(0.002)	(0.002)	(0.002)
λ_j	✓	✓	✓	
f(Age)∗$\mathbb{I}^m$		✓	✓	✓
λ_i			✓	
λ_{ij}				✓

Continued on next page

Table A2 – continued from previous page

	(1)	(2)	(3)	(4)
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

A.5 Analysis at the Firm Level

In our main specification we estimate the effect of the Austrian pay transparency reform on individual (daily) wages. Here we present an alternative specification of our baseline model, in which we regress the gender pay gap of firm j in year t (GPG_{jt}) on the interaction of the year indicator $\mathbf{1}[t = k]$ and the treatment indicator $Treat_{j(2013)}$. Thereby, we focus again on firms with 75-225 employees in 2013 and assign firms with 150 and more employees in 2013 to the treatment group:

$$GPG_{jt} = \sum_{k=2007}^{2018} \beta^k \mathbf{1}[t = k] * Treat_{j(2013)} + \lambda_j + \lambda_t + \epsilon_{jt}, \tag{11}$$

As in the baseline specification in equation (1), λ_j and λ_t denote the firm and year fixed effects respectively. ϵ_{jt} denotes the idiosyncratic error term.

Figure A11 plots the β^k coefficients from estimating equation (11) for the firms in our baseline sample. Overall, this analysis corroborates our baseline results: The Austrian pay transparency legislation had no discernible economic or statistically significant effect on the gender pay gap in treated firms. Only in 2011 and 2012 we observe a small significant pre-trend in the gender pay gap. However, the gender pay gap is actually increasing rather than decreasing, such that we can rule out anticipation effects.

Figure A11: Effect of Transparency on Firm Level Gender Wage Gap

The figure below plots the effects of pay transparency on the firm-level gender wage gap using equation (11)*. The sample is restricted to firms with 75-225 employees in 2013. Standard errors are clustered at the establishment level. The standard error spikes represent 95% confidence intervals.*

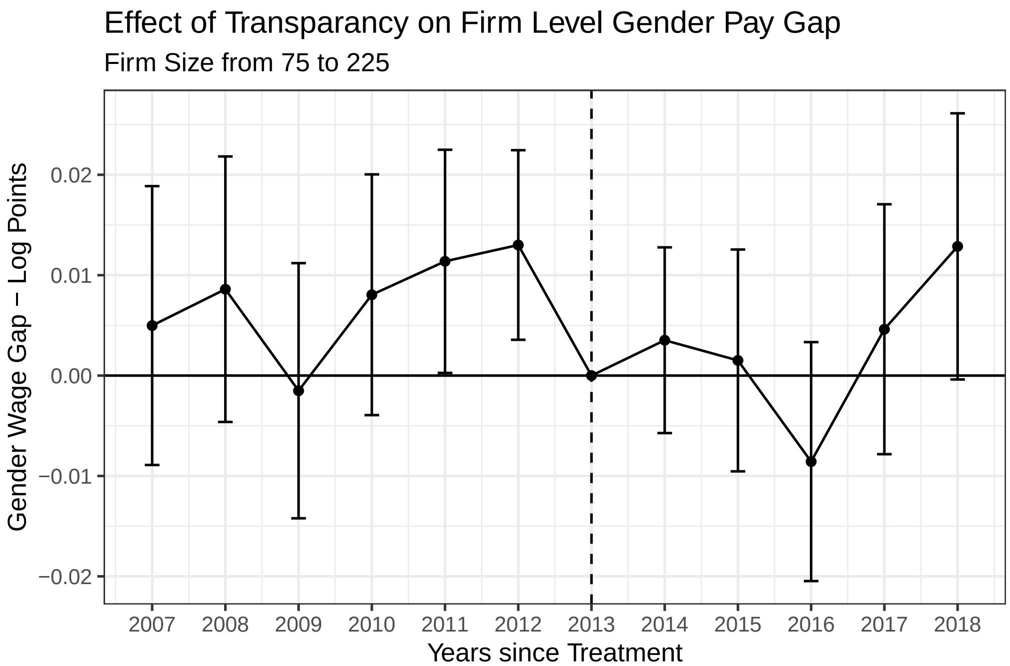
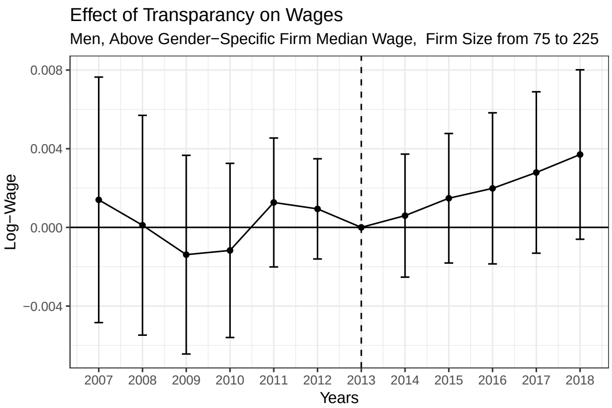


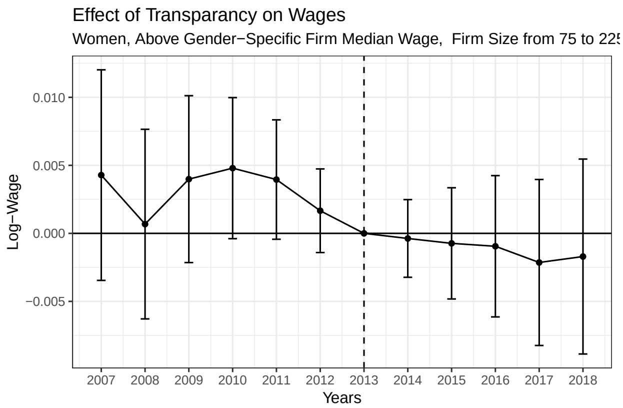
Figure A12: Gender-Specific Effects of Transparency on Daily Wages

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

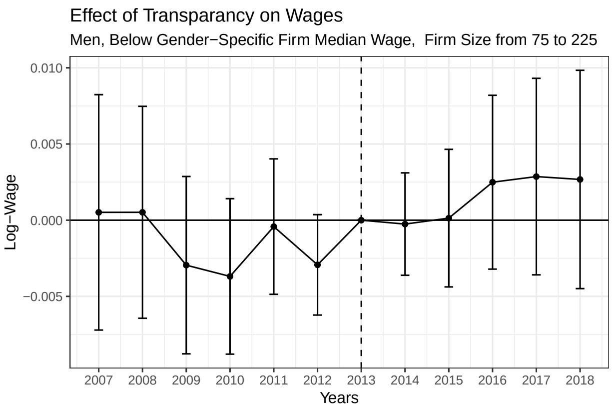
The figure below plots the effects of transparency on male and female wages, for workers who earn above (top panels) and below (bottom panels) their gender-specific firm-level median wage in 2013 (Eq. (1)), the year before treatment. Standard errors are clustered at establishment level. The standard error spikes represent 95% CI.



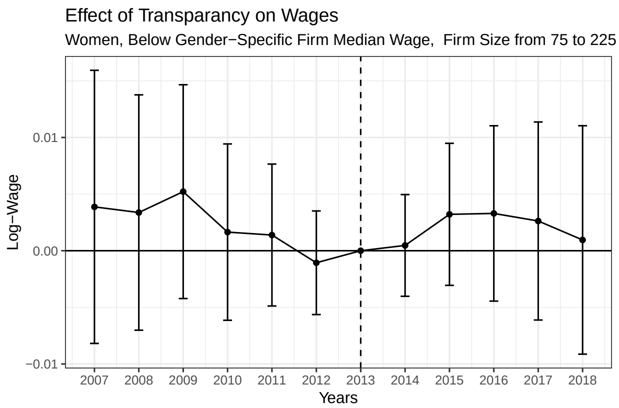
(a) Above Median Male



(b) Above Median Female



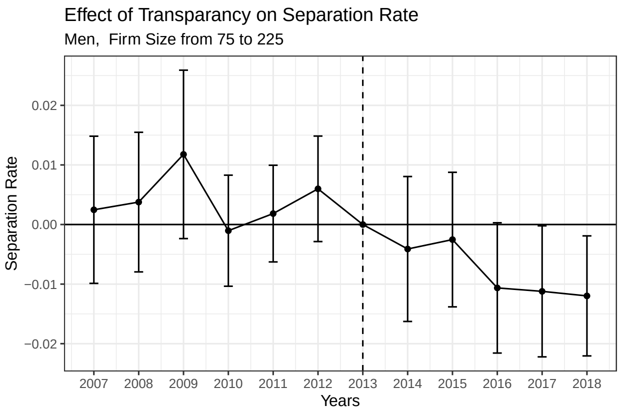
(c) Below Median Male



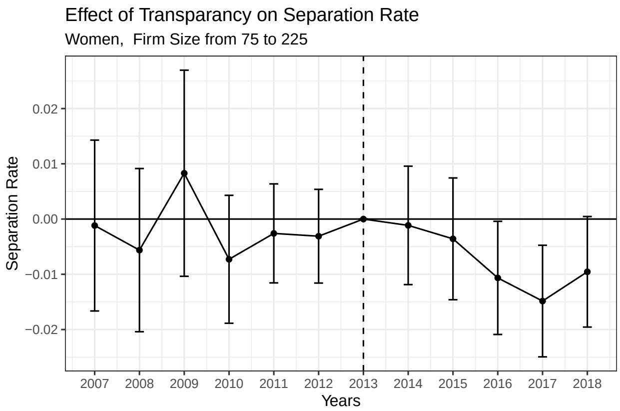
(d) Below Median Female

Figure A13: 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 (Eq. (7)). The sample is restricted to firms with 75-225 employees in 2013. Standard errors are clustered at the firm level. The standard error spikes represent 95% confidence intervals.



(a) Male



(b) Female