CESifo WORKING PAPERS

8960
2021

March 2021

The Austrian Pay Transparency Law and the Gender Wage Gap

René Böheim, Sarah Gust

CESifo

Impressum:

CESifo Working Papers
ISSN 2364-1428 (electronic version)
Publisher and distributor: Munich Society for the Promotion of Economic Research - CESifo GmbH
The international platform of Ludwigs-Maximilians University's Center for Economic Studies and the ifo Institute
Poschingerstr. 5, 81679 Munich, Germany
Telephone +49 (0)89 2180-2740, Telefax +49 (0)89 2180-17845, email office@cesifo.de
Editor: Clemens Fuest
https://www.cesifo.org/en/wp
An electronic version of the paper may be downloaded
· from the SSRN website: www.SSRN.com
· from the RePEc website: www.RePEc.org
· from the CESifo website: https://www.cesifo.org/en/wp

CESifo *Working Paper No. 8960*

The Austrian Pay Transparency Law and the Gender Wage Gap

Abstract

In Austria, a gender pay transparency law was introduced in 2011, requiring companies with more than 1,000 employees to publish a pay report every other year. Firms with 500, 250, and 150 employees were subject to this requirement at later years. We estimate the impact of the law on men's wages, women's wages, and the gender pay gap using administrative data. The results from a regression discontinuity design suggest that the wage transparency law did not change wages or the gender wage gap. In larger firms, the wage of newly hired women increased more due to the reform than of newly hired men, suggesting that the gender wage gap decreased among newly hired workers. Our estimates of the effect of the law on employment growth or turnover are small, and statistically insignificant. For larger firms, we estimate that the transparency law led to a lower share of women in treated firms. These results are robust to several additional specifications.

JEL-Codes: J310.

Keywords: wage transparency, gender wage gap.

René Böheim
Department of Economics
Johannes Kepler University
Linz / Austria
Rene.Boeheim@jku.at

Sarah Gust
ifo Institute – Leibniz Institute for Economic Research at the University of Munich / Germany
Gust@ifo.de

March 2021

1 Introduction

The European Commission (2013) calls the lack of equal pay in the European member states one of the most problematic areas and, in 2014, the European Commission requested its member states to introduce at least one instrument to enhance wage transparency (Aumayr-Pintar, 2018). Among the recommended instruments are the right of employees to obtain information on wage levels, the employers' duty to report wage levels, wage audits at firms or making equal wage part of the collective bargaining process (European Commission, 2017).

In 2011, Austria introduced a wage transparency law requiring firms to provide all employees with a wage report every two years. Firms that had 1,000 or more employees were required to publish reports from 2011. Firms with 500 or more employees have had to publish reports since 2012. Since 2013, firms with 250 or more employees have also been required to publish wage reports, and since 2014, firms with more than 150 employees. The minimum requirements for a report are of the publication of the number of men and women in each remuneration group and their mean and median wages. Employees must not communicate the results with third parties and employers may sue employees for breach of confidentiality.

We analyze the effects of the transparency law on men's wages, women's wages, and the gender wage gap. We apply an RDD and use difference-in-discontinuities specifications (Diff-in-Disc) proposed by Grembi, Nannicini and Troiano (2016) to exploit the variation between firms around the employee thresholds and the staggered application of the law to firms of different sizes. In our analyses, we focus on the threshold at 1,000 employees. Firms with more than 1,000 employees were the first firms which had to publish a gender wage report in 2011 when the law

was enacted. These firms were fairly unexpectedly subjected to the requirements. In contrast, smaller firms might have anticipated the requirements as the law was announced in 2011 and required them to publish wage reports as of 2012 or even later. Our analyses are similar to Gulyas, Seitz and Sinha (2021) who also study the effects of the transparency law in Austria.

The expected effects of wage transparency requirements on wages are not clear *a priori*. Wage transparency might decrease information asymmetries if employees have limited information about the wage structure or about outside options. Such frictions could allow firms monopsony power over wages, especially if wage information spreads in networks (Ioannides and Soetevent, 2006).

Several studies suggest that wage transparency reduces wage gaps in firms as reports reduce information asymmetries and lead to more equal wages (Mas, 2017). Since wage reports do not link wages to the productivity of employees, a reported wage gap could increase the wage gap if the most productive employees of the supposedly underpaid employees leave the firm or if men and women renegotiate their wages differently (Baker, Halberstam, Kroft, Mas and Messacar, 2019). Firms that want to avoid conflicts among their employees or to prevent increased turnover could aim to close the wage gap (Mas, 2017).

Bennedsen, Simintzi, Tsoutsoura and Wolfenzon (2019) find that the gender wage transparency law in Denmark dampened the wage growth of male employees and thus decreased the gender wage gap. Baker et al. (2019) find that wage disclosure at Canadian universities decreased the gender wage gap through an increase of the wages of female employees. According to Vaccaro (2018), the wage monitoring law in Switzerland decrease the adjusted gender wage gap. Duchini, Simion and Turrell (2020) show that the UK pay transparency law increased the

probability that women are hired in above-median-wage occupations and decreased real wages for men.

Roussille (2020) finds that providing job seekers with information on the median wage, conditional on their experience and qualification, lowers the gender ask gap in wages. (See also Artz, Goodall and Oswald (2018).) Evidence by Biasi and Sarsons (2020) suggest that women tend to be less aware of their colleagues' wages than men, but knowing others' pay is positively associated with a renegotiation of wages. In this respect, transparency laws might lead to a lower gender wage gap.

However, there are good reasons to be doubtful about any effect of gender wage reports on wages. First, employees might not be aware of the wage reports and thus might not make use of the information. The Federal Ministry for Education and Women (2015) suggests that wage reports are not salient among employees in Austria. Second, even if wage reports are salient, they might not increase the available information. Third, even if wage reports increase the information about a firm's wage structure, if men and women use the information in the same way, it might leave the gender wage gap unchanged (Baker et al., 2019). Fourth, wages might not change as employees could be unable to renegotiate their wages due to labour market frictions. Such frictions could however affect the wages of new employees since it is perhaps easier to adjust their wages.

Because the law has only limited consequences, the effects of the Austrian law of 2011 might be moderate. If a firm does not publish a report, the works council or the employees may take legal steps to enforce their inspection rights. Firms which do not publish a report can be forced by the Labour Court to publish a report. There are no legal consequences if a wage reports indicates a gender wage gap.

Indeed, Gulyas et al. (2021) find that the transparency law in Austria did not have any effects on the gender wage gap for smaller firms with 150 to 225 employees in 2014. We replicate the results of Gulyas et al. (2021) and show that wages, and the gender wage gap, did not react to the reform. While Gulyas et al. (2021) use a staggered difference-in-difference design, we use a RDD approach and use and use difference-in-discontinuitiesspecifications (Diff-in-Disc). Our main results show that the wage transparency law did not reduce the gender wage gap in Austria. In larger firms, which were arguably more surprised by the law than smaller firms, wages of men and women increased due to the reform, in particular, among newly hired employees. Our estimates of the effect of the law on employment growth or turnover are small, and statistically insignificant. For larger firms, we estimate that the share of women in treated firms declined relative to untreated firms.

Our conclusions are based on a set of RDD specifications and results from various robustness checks support them. Results from difference-in-discontinuities specifications, where we exploit the variation of wages and the gender wage gap over time in firms that are close to the threshold, also support our conclusions.

2 Background

The "Austrian National Action Plan for Gender Equality in the Labour Market" was adopted in 2010 (Federal Ministry for Women and the Civil Service, 2010). The action plan proposes several measures to increase gender equality in the labour market, including wage reports. The Austrian government approved a corresponding amendment of the Equal Treatment Act in January 2011, which led to the commencement of the legislative change in March 2011 (Austrian Parlament,

2011).

The law requires all private firms with more than 1,000 employees to publish a wage report every other year, starting from 2011. Smaller firms were subject to the law at later years. A report has to publish at least (Österreichischer Gewerkschaftsbund, 2011):

a. The number of women and men in each collective bargaining or remuneration group in each calendar year;

b. If applicable, the number of women and men in each seniority level;

c. The mean and median wages of women and men in each remuneration group and, if applicable, for each seniority level; and

d. The wages of part-time employees have to be projected for full-time employment and the wages of employees who are employed less than one year to annual employment.

An identification of part-time employees and job-changers is recommended but not compulsory. Agency workers, home workers, and freelance workers are excluded, but workers in marginal employment and apprentices are included in the reports.[1]

The firms have to submit the report to the works council or, if there is no works council, to all employees within the first quarter following the reporting year.[2] If the employer does not provide a wage report, the works council or, in

[1]Employees who earned less than €374.02 per month in 2011 were considered marginally employed. Marginally employed employees are exempt from compulsory social security contributions. The threshold is adjusted on an annual basis (Böheim and Weber, 2011).

[2]Employees have the right to elect a works council if at least 5 employees are employed by the firm. In firms with 1,000 employees, a works council consists of 13 elected representatives (Oesingmann, 2015).

case no works council exists, an employee may take legal steps at the Labour and Social Court to enforce their inspection and control rights (Österreichischer Gewerkschaftsbund, 2011). This can lead to coercive penalties for the employer (Österreichischer Gewerkschaftsbund, 2011) but, to the best of our knowledge, there has been no corresponding case. The Federal Ministry for Education and Women (2015) assessed the compliance of firms with the wage transparency law based on interviews of managers, employees, and work councils. According to their assessment, most firms stuck to the legal minimum of reporting.

In contrast to the wage transparency laws in other countries, the Austrian gender wage transparency law is weak. Initially it obligated only firms with more than 1,000 employees and since 2014 it applies to all firms with more than 150 employees, whereas Swiss or Danish laws apply also to much smaller firms.[3] Canadian universities disclose individual salaries online, while for Austria the data are aggregated to at least five employees per group due to data protection. The UK chose a higher aggregation level than Austria but all indicators are posted publicly online. There is no systematic monitoring of the firms and non-compliance is therefore cheap, while employees are bound to secrecy. If employees violate the confidentiality of the wage reports, they may face a fine of up to €360. There are no legal consequences if the gender wage reports reveal unequal wages. In Switzerland, firms can be excluded from public tender if they fail to reduce their adjusted gender wage gap below 5%.

[3]In Denmark, firms with more than 35 employees have to publish a report (Bennedsen et al., 2019), in Switzerland the cut-off is 50 employees (Vaccaro, 2018).

3 Data

The Austrian Social Security Database (ASSD) contains detailed information on employees' earnings and employment history (Zweimüller, Winter-Ebmer, Lalive, Kuhn, Wuellrich, Ruf and Büchi, 2009). For the analysis, we use firm-level data for the years 2009 to 2017. The data contain the number of employees per firm by gender, contract type (most are either blue-collar or white-collar employees), and nationality. Our indicator for firm size is the average number of employees in the first quarter of a year since firms have to publish the wage reports in the first quarter. We restrict the data to the private sector as the legal situation for civil servants is different.[4] We exclude firms that have fewer than 10 female or fewer than 10 male employees. All monetary values are in 2010 prices, deflated using the consumer price index (Österreichische Nationalbank, 2020).

While the data are administrative data and thus more reliable than survey data (Roth and Slotwinski, 2018), two aspects of the data limit our analyses. First, the data do not contain the hours of work but the wage which is used to calculate the social security contribution. Social security contributions are capped at a monthly gross wage (€4,200.00 in 2011) and monthly gross wages that are above this threshold are not recorded in the data. In other words, monthly wages are top-coded and we have no information on hourly wages. For these reasons, we use firm-level median wages by gender as these are insensitive to top-coding.[5]

Table 1 reports the number of firms by size and year. Overall, there are 23,085

[4] We exclude NACE sectors 72 (scientific research and development i.e., universities), 84 (public administration and defence; compulsory social security), and 85 (education).

[5] If wage reports change the wage rates of men or women, the resulting effect on the gender differences in monthly wages is uncertain as the income and substitution effect might lead to different adjustments along the intensive margins.

firm-year observations for the years 2009 to 2017. There are markedly fewer larger than smaller firms. While there were around 9,200 to 10,700 firms each year which had fewer than 150 employees, there were around 170 to 200 firms with more than 1,000 employees each year.

Table 1: Number of firms, by number of employees and year.

Year	<150	150-249	250-499	500-999	≥1,000	Sum
2009	9,245	1,069	801	335	173	11,623
2010	9,327	1,054	773	334	171	11,659
2011	9,531	1,086	801	328	175	11,921
2012	9,672	1,114	827	342	178	12,133
2013	9,935	1,107	815	348	179	12,384
2014	10,106	1,126	822	339	187	12,580
2015	10,264	1,102	829	347	187	12,729
2016	10,383	1,135	855	342	192	12,907
2017	10,656	1,110	878	353	195	13,192
Total	18,507	2,272	1,466	573	267	23,085

Notes: Firm size is the average number of employees in the first quarter of each year. We exclude firms with fewer than 10 female or fewer than 10 male employees. We restrict the data to private sector firms.

Table 2 shows that large firms hire relatively fewer women than small firms. The percentage of women employed in firms with fewer than 150 employees is on average 48%. The share of women in firms with 150 to 249 employees is 35.9% and it is 33.8% in firms with 250 to 499 employees. In firms with 500 to 999 employees, the share of women is 36.2% and in firms with more than 1,000 employees, the share is 42.8%.

Larger firms pay more than smaller firms. In firms with fewer than 150 employees, the median wage of women is €63.88 per day and it is €82.44 per day for

men. In firms with more than 1,000 employees, women earn €80.80 and men earn €102.16 per day.

The unadjusted gender wage gap is relatively constant over firm sizes. In firms with fewer than 150 employees, it is 21.5% at the median. In firms with 150 to 249 employees, the gender wage gap is 20.6%, in firms with 250 to 499 employees it is 21.7%, in firms with 500 to 999 employees it is 21.5%, and in the largest firms it is 21.0%.[6]

Table 2: Wages, by gender and firm size.

Firm Size	Women (%)	Women's wages	Men's wages	Wage Gap (%)
<150	48.00	63.88	82.44	21.47
150-249	35.89	70.92	89.27	20.61
250-499	33.80	73.74	95.90	21.65
500-999	36.15	77.43	100.79	21.45
>=1,000	42.77	80.80	102.16	21.03

Notes: Percentage of women in firms, median daily gross wages of women and men, and the unadjusted gender wage gap, averages for the years 2009 to 2017. Firm size is the number of employees in the first quarter of each year. Wages are deflated to the year 2010 using the CPI (Österreichische Nationalbank, 2020). 111,618 firm-year observations.

Table 3 shows that on average around 46.5% of the employees were women and this number stayed roughly constant between 2009 and 2017. The median daily wage rate of men and women were at €65.63 for women and €86.10 for men in 2009. Real wages decreased until 2012 and increased thereafter. In 2017, the median wage for women was €66.81 and it was €96.96 for men. The unadjusted gender wage gap decreased from 22.57% in 2009 to 20.37% in 2017.

[6]See Böheim, Fink and Zulehner (2020) for the development of the unadjusted and the adjusted gender wage gap in Austria, 2005–2017.

Table 3: Median wages, by gender and year.

Year	Women (%)	Wages Women	Wages Men	Wage Gap (%)
2009	46.43	65.63	86.10	22.57
2010	46.67	65.21	85.49	22.25
2011	46.43	64.53	84.17	22.09
2012	46.52	64.81	84.10	21.57
2013	46.51	65.33	84.42	21.30
2014	46.67	65.88	84.74	21.23
2015	46.67	66.62	85.45	20.91
2016	46.45	66.93	85.39	20.49
2017	46.43	66.81	85.09	20.37

Notes: Percentage of women, median daily gross wages of women and men, and the unadjusted gender wage gap, averages for the years 2009 to 2011. Wages are deflated to year 2010 using the CPI (Österreichische Nationalbank, 2020). 111,618 firm-year observations.

4 Method

We apply a regression discontinuity design (RDD) as summarized in e.g., Lee and Lemieux (2010), in which the assignment variable is firm size. A RDD allows to estimate the average treatment effect (ATE) of whether a firm is required to publish a wage report or not, T_i, on some outcome y of firm i. The assumption used to identify the effect of the wage transparency law on wages is that a firm's size around the threshold is as good as random. (We discuss and present evidence on this assumption below.) The assignment rule is based on firm size S_i and determines if a firm is treated or not:

$$T_i = \begin{cases} 1 & \text{if } S_i \geq 1,000 \\ 0 & \text{if } S_i < 1,000 \end{cases} . \tag{1}$$

The estimating sample consists of all observations in the interval $(1,000-h, 1,000+h)$, where h is the data driven bandwidth parameter proposed by Calonico, Cattaneo and Titiunik (2014).

Following Cattaneo, Idrobo and Titiunik (2019), we use a local linear regression to estimate the ATE.[7] A global approximation may induce biases or errors at the boundary points through distant points or outliers. We therefore use triangular kernel smoothing to give a higher weight to observations that are closer to the cut-off.

The model can be written as

$$y_i = S_i + T_i(\varphi_0 + \varphi_1 f(S_i)) + \eta_i, \tag{2}$$

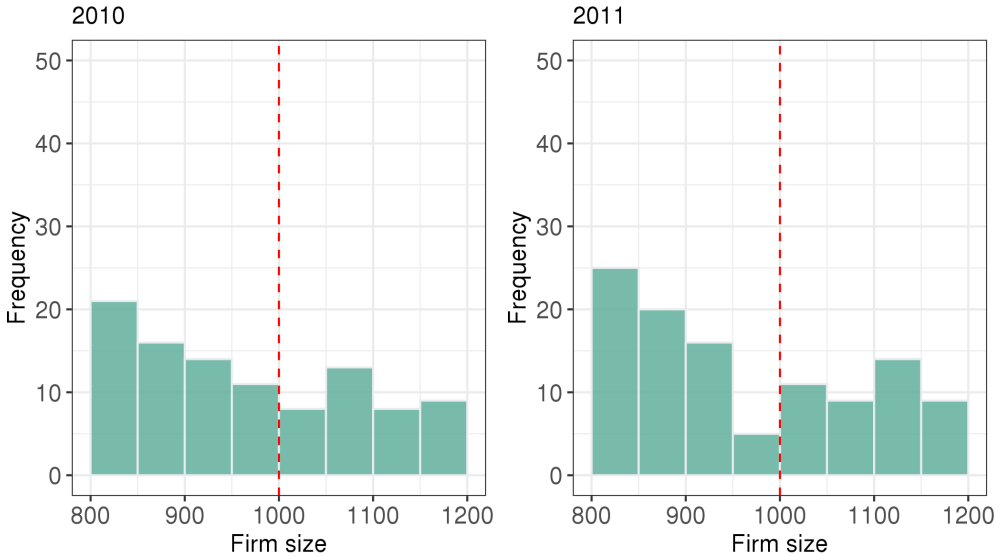
where y_i are firm level outcomes. The outcome indicators we focus on are men's and women's median wages, and the firms' gender pay gap. $f(S_i)$ is a function of firm size, η_i is the error term, and ρ_0 is the parameter of interest.

4.1 Identification

Firms may want to avoid the publication of wage reports, even if the information is only disclosed to their own employees. The direct costs of preparing the reports are relatively low, but firms could be concerned about indirect costs, for example, arising from wage increases or greater turnover. If firms choose their size strategically to avoid being affected by the law, the estimates from the RDD are biased.

[7] Gelman and Imbens (2019) caution against the use of higher order polynomials in RDD.

Figure 1: Firm Size Distribution.



Notes: Firm size distribution for 2010 (pre-treatment) and 2011 (first year of treatment) around the cut-off of 1,000 employees.

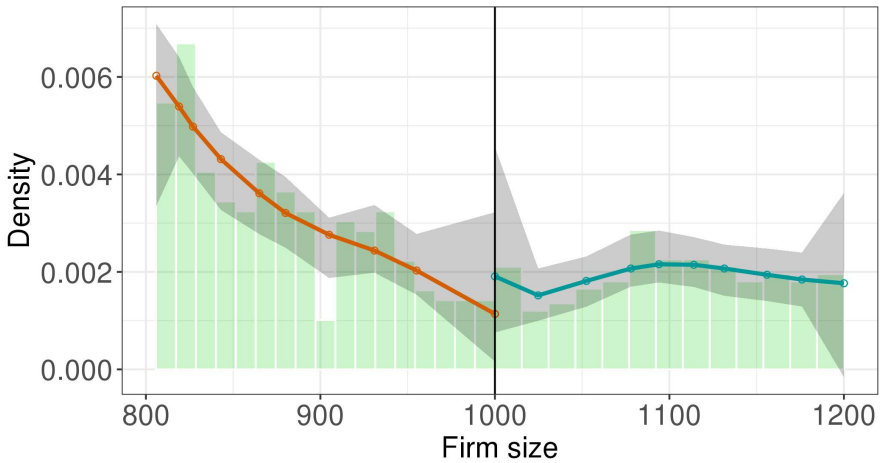
We think it unlikely that firms strategically choose their size in response to the law. At the time large firms became subject to the law, it was also announced that smaller firms with 500 to 999 employees had to publish reports as of 2012. Thus, a large firm which remained just below the 1,000 employee threshold in 2011 would have avoided the publication of a report for only one more year. Alternatively, it would have had to lower its size to fewer than 500 employees to avoid publishing wage reports in the next year. Or, it would have to reduce its size to fewer than 150 employees to avoid publication after 2014. This seems an excessive, and perhaps very costly, response to the law. Moreover, the law does not provide for penalties if a firm fails to publish the reports or if there are systematic differences in pay by gender.

To test for the presence of strategic firm size formally, we analyze the distri-

bution of firms' sizes. In the absence of a strategic decision to remain small, a discontinuity in the distribution of firms' sizes around the cut-off suggests strategic sorting of firms (Vaccaro, 2018). Figure 1 shows the distribution of firms' sizes in the pre-announcement year (2010) and in the year when the law became effective (2011). Figure 1 suggests that there were fewer, not more, firms with 950-999 employees in 2011 than in 2010.

McCrary (2008) offers a formal test where the logarithmic density of the running variable is regressed on the running variable on each side of the cut-off. A test for the whether the intercepts are significantly different from zero or not allows a formal assessment of the manipulation of the running variable. Figure 2 shows the graphic result of the test. The test cannot reject the null of continuity in the running variable around the cut-off of 1,000 employees in 2011. The difference in the two polynomials at the threshold is 0.77 with a p-value of 0.44.

Figure 2: McCrary Density Test.



Notes: The test cannot reject the null of continuity in the running variable around the cut-off of 1,000 employees in 2011. The difference in the two polynomials at the threshold is 0.77 with a p-value of 0.44.

5 Main results

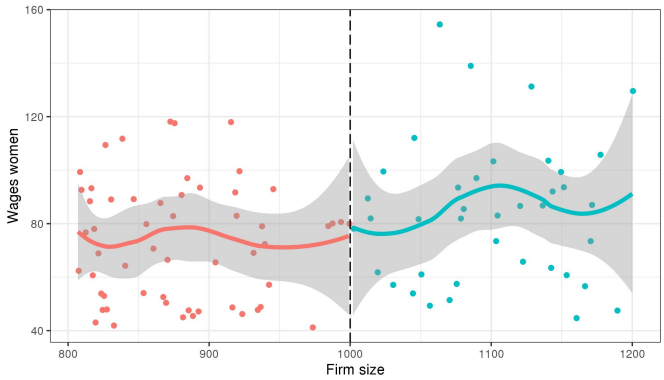
Figure 3 shows the results from fitting a regression on either side of the cut-off for women's wages, men's wage, and the gender wage gap in 2011. The vertical line represents the cut-off at the firm size of 1,000 employees, with a bin size is equal to one firm. These results do not suggest that wages differed in firms just above or just below the threshold.

In our RDD estimates, we us a local linear regression with a small bandwidth around the cut-off (Cattaneo et al., 2019). Panel A of Table 4 tabulates the estimation results for the cut-off in 2011. We tabulate the results from three different approaches to estimate standard errors. We use the data driven bandwidth selection proposed by Calonico et al. (2014) and, in consequence, each estimation uses a different bandwidth. The different bandwidths imply different sample sizes for different specifications. The bias correction and the robust confidence intervals follow Calonico et al. (2014) and we use a local polynomial bias estimator of order 2. The estimated effects suggest that both men and women earned more because of the transparency law, and that the gender wage gap narrowed. All estimated coefficients are statistically insignificant at conventional levels. Note that the sample sizes are small and the power of the test is thus low.

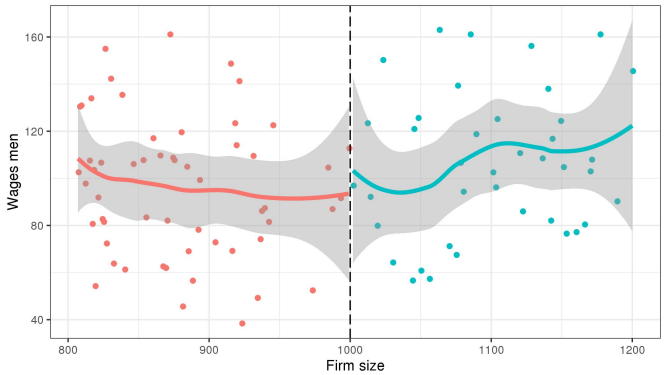
Panels B, C, and D of Table 4 provide the estimates for the years 2012, 2013, and 2014, where we use the cut-offs for smaller firms. Similar to the estimates for 2011, all estimated effects are statistically insignificant. The results for 2014 mirror the results of Gulyas et al. (2021) who also find no statistically significant effect of the transparency law on the wages for men or women.

Figure 3: Discontinuity Plots.

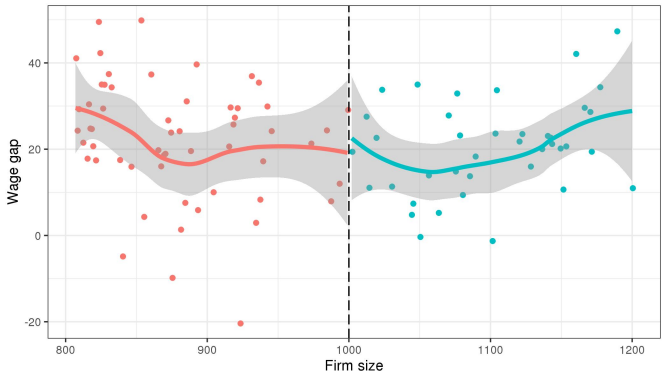
(a) Women's Wages in 2011



(b) Men's Wages in 2011



(c) Gender Wage Gap in 2011



Notes: The graph presents the regression discontinuity. The vertical line represents the cut-off and each dot is the median value in that bin.

Table 4: RDD results.

Model	Women's Wages	Men's Wages	Gender Gap
A. Cut-off 1000 in 2011			
Conventional	4.427	5.536	-1.473
	(9.452)	(13.262)	(5.139)
Bias-Corrected	3.557	5.296	-0.437
	(9.452)	(13.262)	(5.139)
Robust	3.557	5.296	-0.437
	(10.213)	(15.012)	(5.955)
Observations	120	160	172
B. Cut-off 500 in 2012			
Conventional	-1.047	1.964	6.594
	(5.275)	(7.387)	(5.179)
Bias-Corrected	-0.673	2.601	7.144
	(5.275)	(7.387)	(5.179)
Robust	-0.673	2.601	7.144
	(6.402)	(9.061)	(5.669)
Observations	593	474	383
C. Cut-off 250 in 2013			
Conventional	2.451	1.207	-2.839
	(3.185)	(4.758)	(3.946)
Bias-Corrected	1.669	-0.437	-4.265
	(3.185)	(4.758)	(3.946)
Robust	1.669	-0.437	-4.265
	(3.697)	(5.485)	(5.069)
Observations	887	842	2673
D. Cut-off 150 in 2014			
Conventional	-0.526	0.468	1.726
	(2.557)	(3.461)	(1.938)
Bias-Corrected	-1.149	0.089	2.233
	(2.557)	(3.461)	(1.938)
Robust	-1.149	0.089	2.233
	(3.029)	(4.128)	(2.281)
Observations	2025	1861	1540

Notes: The table presents the empirical results from the RDD with cut-off = 1,000 in 2011, cut-off = 500 in 2012, cut-off = 250 in 2013, and cut-off = 150 in 2014 using a local linear regression with triangular kernel smoothing and bandwidth selection by Calonico et al. (2014). Bias correction and the robust confidence intervals follow Calonico et al. (2014)). * p<0.1; ** p<0.05; *** p<0.01. Standard errors in parentheses.

Table 5: RDD results for new employees.

Model	Women's Wages	Men's Wages	Gender Gap
A. Cut-off 1000 in 2011			
Conventional	19.915*	6.337	-11.134
	(10.803)	(15.176)	(13.986)
Bias-Corrected	18.338*	3.441	-13.313
	(10.803)	(15.176)	(13.986)
Robust	18.338	3.441	-13.313
	(13.365)	(18.809)	(16.687)
Observations	142	141	58
B. Cut-off 500 in 2012			
Conventional	-2.823	12.444	11.992
	(8.851)	(8.659)	(7.64)
Bias-Corrected	-3.429	15.36*	14.573*
	(8.851)	(8.659)	(7.64)
Robust	-3.429	15.36	14.573
	(10.884)	(10.161)	(9.111)
Observations	271	230	105
C. Cut-off 250 in 2013			
Conventional	6.162	-9.187	5.141
	(6.284)	(7.527)	(11.136)
Bias-Corrected	7.553	-11.647	10.559
	(6.284)	(7.527)	(11.136)
Robust	7.553	-11.647	10.559
	(7.357)	(8.742)	(14.538)
Observations	192	265	189
D. Cut-off 150 in 2014			
Conventional	-15.43**	-4.956	-8.805
	(7.469)	(6.998)	(7.658)
Bias-Corrected	-18.63**	-7.474	-11.379
	(7.469)	(6.998)	(7.658)
Robust	-18.63**	-7.474	-11.379
	(8.811)	(8.103)	(10.522)
Observations	301	440	314

Notes: The table presents the empirical results from the RDD for new employees with cut-off = 1,000 in 2011, cut-off = 500 in 2012, cut-off = 250 in 2013, and cut-off = 150 in 2014 using a local linear regression with triangular kernel smoothing and bandwidth selection by Calonico et al. (2014). Bias correction and the robust confidence intervals follow Calonico et al. (2014)). * p<0.1; ** p<0.05; *** p<0.01. Standard errors in parentheses.

It is possible that employees who have been working for a firm may find it difficult to renegotiate their wages in the short-term. We therefore repeat the analysis for the wage levels of newly hired employees who are arguable more flexible when negotiating their wages. In addition, the information on wages and the distribution of wages within the firm might be more salient for newly hired employees. The estimation results are tabulated in Table 5. For the firms that hired new employees in 2011, we estimate that women had greater wages in firms just above the threshold than in firms just below the threshold. Note that the sample sizes are small, however, the effect is statistically significant at conventional levels in two of the three ways to calculate the standard errors. The estimated effect for newly hired women is in line with Leonardi and Pica (2013) and Vaccaro (2018) who both find that wages for new employees react more sensitively to legislative changes. Consequently, we also estimate a negative, if statistically insignificant, effect on the gender wage gap.

We tabulate the estimation results for the years 2012, 2013, and 2014 in Panels B, C, and D of Table 5. There is no clear pattern that emerges from these results. The estimation results suggest that the gender wage gap for newly hired employees in treated firms might have increased in firms which had just above 500 employees in 2012, relative to firms just below that threshold. The estimates for 2013 are all insignificant on conventional levels.

For 2014, we estimate that the wages of both newly hired men and newly hired women declined relative to the firms below the threshold. Although the sample sizes are relatively small, the estimated effect for newly hired women is statistically significant at conventional levels.

6 Results for growth rate, turnover, and share of women in a firm

Overall, our estimates suggest that the transparency law had no effect on the gender wage gap. The transparency law could have impacted on other labour market outcomes. For example, if the law increased firms' costs, we expect treated firms to grow less than untreated firms. Transparency laws could have led to different turnover if employees were positively or negatively surprised about their firms' wage structure. In particular, women might have decided to leave firms on learning about wage differences if they felt treated unfairly.

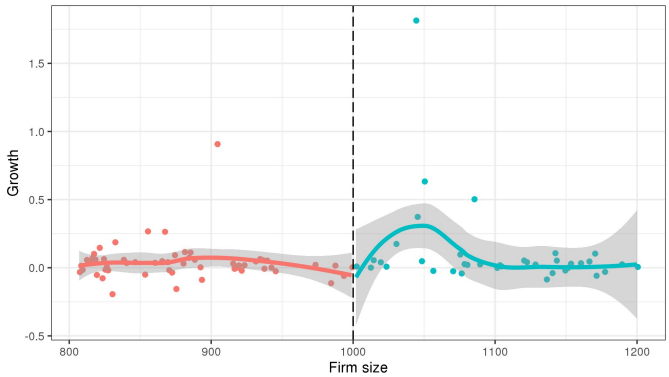
We examine the firms' growth rates, turnover, and the share of women in the firm. We calculate the turnover rate as the number of employees joining the firm in 2011 plus the number of employees leaving the firm in 2011 divided by the number of employees in 2010. The distribution of these outcomes are plotted in Figures 4 to 6. The distributions around the threshold do not suggest that the reform caused changes in treated firms relative to untreated firms.

We tabulate the estimated effect of the transparency law on firms' growth, turnover, and the share of female employees in Table 6. For 2011 in Panel A, the estimates suggest slightly lower growth rates of treated firms, however, the estimates are not statistically significant. The estimates also suggest that the law caused more turnover in treated firms, but again, note the small sample sizes and the lack of statistical significance. The last column of Panel A suggests that the share of female employees in firms above the threshold declined relative to firms below the threshold. The causal effect of the law on the share of female employees in treated firms is statistically significant at the 10 per cent error level, despite the

relatively small sample size.

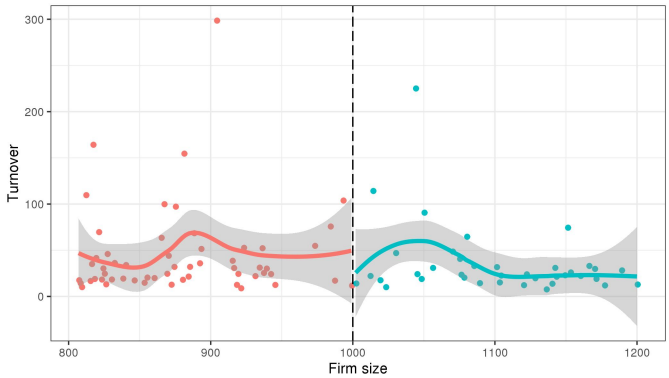
Panel B, C, and D present the results for 2012, 2013, and 2014 respectively. The sample size for 2013 and 2014 that is, for firms around the 250 and 150 employees cut-off are large. We can reject a significant effect of the gender pay transparency law on growth, turnover, and the share of female employees of these firms.

Figure 4: Firms' growth rates at the threshold.



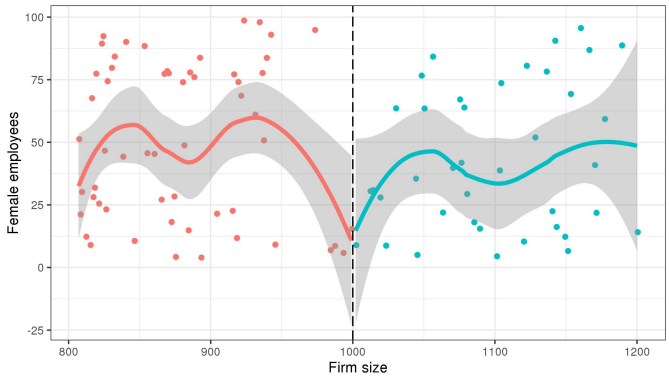
Notes: The graph presents the graphic results of the regression discontinuity. The vertical line represents the cut-off. The growth rate is the difference in the number of employees between 2011 and 2010 divided by the number of employees in 2010.

Figure 5: Firms' turnover rates at the threshold.



Notes: The graph presents the graphic results of the regression discontinuity. The vertical line represents the cut-off. The turnover rate is sum of employees joining and the number of employees leaving the firm in 2011 divided by the number of employees in 2010.

Figure 6: Firms' share of female employees at the threshold (in %).



Notes: The graph presents the graphic results of the regression discontinuity. The vertical line represents the cut-off. Firms with fewer than 10 male or fewer than 10 female workers are excluded from the sample.

Table 6: RDD Results for Growth, Turnover, and Share of Women.

Model	Growth	Turnover	Share of Women
A. Cut-off 1000 in 2011			
Conventional	-0.106	13.522	-14.799*
	(0.1)	(26.392)	(8.81)
Bias-Corrected	-0.142	11.574	-17.032*
	(0.1)	(26.392)	(8.81)
Robust	-0.142	11.574	-17.032*
	(0.119)	(31.92)	(9.478)
Observations	21	78	308
B. Cut-off 500 in 2012			
Conventional	0.035	5.616	1.972
	(0.076)	(18.091)	(5.125)
Bias-Corrected	0.049	4.702	1.896
	(0.076)	(18.091)	(5.125)
Robust	0.049	4.702	1.896
	(0.08)	(21.397)	(6.232)
Observations	68	166	794
C. Cut-off 250 in 2013			
Conventional	-0.022	-19.3	3.296
	(0.53)	(60.972)	(3.661)
Bias-Corrected	0.015	-16.578	4.215
	(0.53)	(60.972)	(3.661)
Robust	0.015	-16.578	4.215
	(0.666)	(73.709)	(4.335)
Observations	3256	1704	1088
D. Cut-off 150 in 2014			
Conventional	0.053	10.333	1.115
	(0.094)	(18.543)	(2.583)
Bias-Corrected	0.065	14.063	1.608
	(0.094)	(18.543)	(2.583)
Robust	0.065	14.063	1.608
	(0.121)	(23.071)	(3.073)
Observations	3600	1179	1672

Notes: The table presents the empirical results from the RDD using a local linear regression with triangular kernel smoothing and bandwidth selection by Calonico et al. (2014). The growth rate is the difference in the number of employees between treatment year t and t-1 divided by the number of employees in t-1. The turnover rate is sum of employees joining and the number of employees leaving the firm in the treatment year t divided by the number of employees in t-1. The share of women is the number of women divided by total firm size. * p<0.1; ** p<0.05; *** p<0.01. Standard errors in parentheses.

7 Robustness

7.1 Alternative RDD specifications

In Table 7, we present estimation results when we use different specifications of the RDD for firms around the 1000 employees cut-off. In particular, we use alternative bandwidth selection (Panel A), alternative kernel smoothing (Panel B), and a polynomial of order 2 (Panel C). All estimated results from these specifications are statistically insignificant at conventional levels and suggest that, overall, wages reacted only moderately to the law. The results from the alternative bandwidth selection and the alternative kernel smoothing suggest that women's wages and men's wages increased. The estimations based on a local polynomial of 2 suggest that wages for women declined because of the law. The effects of the law on the gender wage gap are mixed, in some specification we find a (statistically insignificant) small decrease and in other specifications we find a (statistically insignificant) small increase.

In Table B2, B3, B4 we report the estimation results with alternative RDD specifications for firms with 500, 250, and 150 employees respectively. All coefficients remain statistically insignificant. Our robustness checks confirm that the law had no significant effect on wages and the gender wage gap.

Table 7: RDD robustness for firms with 1000 employees.

Model	Women's Wages	Men's Wages	Gender Gap
A. CER-optimal bandwidth selector			
Conventional	0.471	1.387	-3.458
	(9.297)	(15.251)	(6.015)
Bias-Corrected	0.091	1.018	-3.114
	(9.297)	(15.251)	(6.015)
Robust	0.091	1.018	-3.114
	(9.521)	(15.926)	(6.288)
Observations	62	84	98
B. Epanechnikov kernel smoothing			
Conventional	5.091	6.354	-0.925
	(9.989)	(13.307)	(5.355)
Bias-Corrected	4.415	6.564	0.134
	(9.989)	(13.307)	(5.355)
Robust	4.415	6.564	0.134
	(10.952)	(15.284)	(6.198)
Observations	123	158	158
C. Polynomial of order 2			
Conventional	-1.593	6.151	-1.255
	(10.324)	(15.118)	(6.041)
Bias-Corrected	-3.05	5.991	0.297
	(10.324)	(15.118)	(6.041)
Robust	-3.05	5.991	0.297
	(10.935)	(16.667)	(6.722)
Observations	227	337	337

Notes: The table presents robustness checks for the RDD with cut-off = 1000 in 2011. The main specification uses a local polynomial of order 1, triangular kernel smoothing, and bandwidth selection by Calonico et al. (2014). * p<0.1; ** p<0.05; *** p<0.01. Standard errors in parentheses.

7.2 Difference-in-Discontinuities

Any difference between firms that have fewer than 1,000 and more than 1,000 employees might not arise because of the transparency law, but from systematic differences of the firms.[8] For that reason, we estimate difference-in-discontinuities (Diff-in-Disc) specifications as proposed by Grembi et al. (2016).

The Diff-in-Disc approach exploits both the discontinuity around the threshold and variation over time. Let $\hat{\tau}_{RD}(t < t_0)$ denote the cross-sectional RDD estimator before the introduction of the law and $\hat{\tau}_{RD}(t \geq t_0)$ the cross-sectional RDD estimator after the introduction of the law. The Diff-in-Disc estimator uses the difference of these two discontinuities to estimate the treatment effect $\hat{\tau}_{DD}$:

$$\hat{\tau}_{DD} = \hat{\tau}_{RD}(t \geq t_0) - \hat{\tau}_{RD}(t < t_0) \qquad . \tag{3}$$

That way, the Diff-in-Disc estimation controls for other changes that could have led to discontinuities in the outcome variable around the same threshold before the introduction of the wage transparency law. Grembi et al. (2016) show that $\hat{\tau}_{DD}$ identifies the average treatment effect in a sharp discontinuity design. An identifying assumption is that after 2010 there was no other law that affected the outcome variables around the threshold.[9]

The Diff-in-disc model can be written as:

$$y_{it} = \beta_0 + \beta_1 S^{\star}_{i,t} + T_i(\varphi_0 + \varphi_1 S^{\star}_{i,t}) + R_t[\alpha_0 + \alpha_1 S^{\star}_{i,t} + T_{it}(\gamma_0 + \gamma_1 S^{\star}_{it})] + \xi_{it}, \tag{4}$$

where y_{it} denotes the firm i's outcome at time t. We normalize the threshold to 0

[8] We are not aware of any other law that treats firms above or below this threshold differently.
[9] Grembi et al. (2016) provide precise identification assumptions for the Diff-in-Disc estimator.

and use the normalized firm size $S^{\star}_{it} = S_{it} - 1,000$. T_i is a binary indicator that equals 1 if $S^{\star}_{i,t} > 0$, and 0 otherwise. φ_0 denotes the RDD estimator that contains the base year and the post-treatment year in the Diff-in-Disc design. R_t is an indicator for the post-treatment period. Thus, the treatment in the Diff-in-Disc setting is $T_i * R_t$ and γ_0 is the parameter of interest that captures the treatment effect.

Table 8 presents the estimation results from the Diff-in-Disc specifications, where we use 2010 as the pre-treatment period and 2011 as the post-treatment period. Firms are treated if they are subject to the transparency law in 2011, and untreated if they are not. We exclude all observations that cross the threshold of 1,000 employees between 2010 and 2011. The estimated effects in Panel A for all employees are statistically insignificant at conventional levels. The point estimates, however, suggest that wages for both women and men were relatively lower because of the reform in treated firms. The gender wage gap appears to have declined due to the transparency requirement. The estimated effects for newly hired employees, tabulated in Panel B of Table 8, are not statistically significant. The point estimates would indicate that women's wages increased slightly, men's wages decreased, and that the gender wage gap in treated firms decreased because of the transparency law. Again, note that the power of our analyses is low due to small sample sizes for firms with more than 1000 employees.

In Table B1 we present the Diff-in-Disc results for smaller firms. 2010 is the pre-treatment period. 2012, 2013, and 1013 are the post treatment periods for firms with 500, 250, and 150 employees respectively. The results confirm our previous estimations: all effects remain small and statistically insignificant.

Table 8: Estimated treatment effects from Diff-in-Disc specifications.

	Women's Wages	Men's Wages	Gender Gap
A. All employees			
Post × Treat	-14.690	-11.190	-6.049
	(12.480)	(15.250)	(7.067)
Observations	263	422	557
B. New hires			
Post × Treat	1.465	-16.850	-16.860
	(15.25)	(19.02)	(19.40)
Observations	361	310	171

Notes: The table shows the coefficients from Diff-in-Disc specifications with bandwidth selection following Imbens and Kalyanaraman (2012). * p<0.1; ** p<0.05; *** p<0.01. Robust standard errors in parentheses. The threshold is at 1,000 employees, the pre-treatment period is 2010, and the post-treatment period is 2011.

7.3 Results for smaller firms in 2011

The law requires also firms with fewer than 1,000 employees to publish wage reports. However, while the law was announced in 2011, smaller firms were subject to this requirement at later years. From 2012, the law applied also to firms with 500 or more employees and from 2013 also to firms that employed 250 or more employees. Since 2014, all firms with more than 150 employees have to prepare biennial wage reports. In our main results above, we implicitly assume that firms had no forward looking behavior and firms reacted to the law only from the year in which they were required to publish the reports.

In Table 9, we tabulate the estimation results for smaller thresholds in 2011 as smaller firms might have reacted to the transparency law already in 2011 and did not wait until they formally had to publish wage reports. All estimated effects are relatively small and statistically insignificant.

Table 9: RDD for smaller cut-offs, 2011.

Model	Women's Wages	Men's Wages	Gender Gap
A. Cut-off 500 in 2011			
Conventional	4.523	2.596	-4.644
	(6.443)	(7.43)	(4.086)
Bias-Corrected	6.591	4.005	-5.356
	(6.443)	(7.43)	(4.086)
Robust	6.591	4.005	-5.356
	(7.499)	(8.532)	(4.649)
Observations	387	330	611
B. Cut-off 250 in 2011			
Conventional	1.104	2.487	1.351
	(2.984)	(3.746)	(2.622)
Bias-Corrected	2.304	3.301	0.888
	(2.984)	(3.746)	(2.622)
Robust	2.304	3.301	0.888
	(3.44)	(4.458)	(3.063)
Observations	1411	1351	1465
C. Cut-off 150 in 2011			
Conventional	1.582	-0.871	-2.742
	(2.357)	(3.314)	(1.872)
Bias-Corrected	2.095	-1.353	-2.718
	(2.357)	(3.314)	(1.872)
Robust	2.095	-1.353	-2.718
	(2.796)	(3.966)	(2.158)
Observations	2507	2037	1698

Notes: The table presents the empirical results from the RDD using a local linear regression with triangular kernel smoothing and bandwidth selection by Calonico et al. (2014). Bias correction and the robust confidence intervals follow Calonico et al. (2014). * p<0.1; ** p<0.05; *** p<0.01. Standard errors in parentheses.

8 Conclusion

We analyze the effect of the Austrian wage transparency law of 2011 on women's wages, men's wages, and the gender wage gap where we exploit the quasi-random assignment of firms according to their size. We obtain the treatment effect of the transparency law based on estimations in a regression discontinuity design. We provide a range of specification checks where we vary the sample selection and the econometric specifications. We also exploit the variation over time in firms which are in the vicinity of the threshold using a difference-in-discontinuities approach.

Our main results show that the wage transparency law did not reduce the gender wage gap. Only in three instances we reject the null-hypothesis of no effect. First, we find that for newly hired women in larger firms, the law resulted in a wage increase. (We also estimate that the gender wage gap for larger firms declined, however, this result is not statistically significant.) Second, the women's wages in firms with 150 or slightly more employees *declined* relative to those in firms with slightly fewer than 150 employees in our main RDD specification. However, the latter result is not robust and is not statistically significant in alternative specifications. Third, we find that the share of female employees declined in firms with more than 1,000 employees relative to slightly smaller firms. This result suggests that firms reacted to the (potential) increase in costs arising from paying female employees more by reducing their numbers. We leave a more detailed analysis of this "unintended consequence" for future research.

Our results complement Gulyas et al. (2021) who study the effect of the transparency law on workers in treated and untreated firms. They also find no statistically significant effect on the wages for men or women. However, our findings

contrast with previous studies in other countries where wage disclosure was successful in reducing wage gaps. Our interpretation is that the Austrian gender wage transparency law is relatively weak. Initially, it obligated only firms with more than 1,000 employees and as of 2014 it applies to all firms with more than 150 employees, whereas Swiss or Danish laws apply also to much smaller firms (Bennedsen et al., 2019; Vaccaro, 2018). Canadian universities disclose individual salaries online (Baker et al., 2019), while for Austria the data are aggregated to at least five employees per group due to data protection. The UK uses a higher aggregation level than Austria but all indicators are posted publicly online (Duchini et al., 2020). In Switzerland, firms can be excluded from public tender if they fail to reduce their adjusted gender wage gap below 5%.

In Austria, is no systematic monitoring of the firms and non-compliance is therefore cheap, while employees are bound to secrecy. If employees violate the confidentiality of the wage reports, they may face a fine of up to €360. There are no legal consequences if the gender wage reports reveal unequal wages. Thus, we conclude, that gender pay transparency can be effective in reducing gender wage gaps but the Austrian law demonstrates that they are no panacea.

References

Artz, Benjamin, Amanda H. Goodall and Andrew J. Oswald (2018), 'Do women ask?', *Industrial Relations: A Journal of Economy and Society* **57**(4), 611–636.

Aumayr-Pintar, Christine (2018), Pay transparency in Europe: First experiences with gender pay reports and audits in four member states, Technical report, Brussels.

Austrian Parlament (2011), 'Gesetz über die Gleichbehandlungskommission und die Gleichbehandlungsanwaltschaft u.a., Änderung', *BGBl. Nr. 7/2011* .
URL: *https://www.ris.bka.gv.at/eli/bgbl/I/2011/7/20110215*

Baker, Michael, Yosh Halberstam, Kory Kroft, Alexandre Mas and Derek Messacar (2019), Pay transparency and the gender gap, *NBER Working Paper* 25834, National Bureau of Economic Research.

Bennedsen, Morten, Elena Simintzi, Margarita Tsoutsoura and Daniel Wolfenzon (2019), Do firms respond to gender pay gap transparency?, *NBER Working Paper* 25435, National Bureau of Economic Research.

Biasi, Barbara and Heather Sarsons (2020), 'Flexible wages, bargaining, and the gender gap'.
URL: *http://conference.nber.org/conf_papers/f139855.pdf*

Böheim, René and Andrea Weber (2011), 'The effects of marginal employment on subsequent labour market outcomes', *German Economic Review* **12**(2), 165–181.

Böheim, René, Marian Fink and Christine Zulehner (2020), 'About time: The narrowing gender wage gap in Austria', *Empirica* .
URL: *https://doi.org/10.1007/s10663-020-09492-4*

Calonico, Sebastian, Matias D Cattaneo and Rocio Titiunik (2014), 'Robust nonparametric confidence intervals for regression-discontinuity designs', *Econometrica* **82**(6), 2295–2326.

Cattaneo, Matias D, Nicolás Idrobo and Rocio Titiunik (2019), *A Practical Introduction to Regression Discontinuity Designs: Foundations*, Cambridge University Press.

Duchini, Emma, Stefania Simion and Arthur Turrell (2020), 'Pay transparency and cracks in the glass ceiling', *arXiv preprint arXiv:2006.16099* .

European Commission (2013), 'Com(2013) 861 final', Online. Accessed (2020-04-29).
URL: *https://eige.europa.eu/docs/3050_com-2013-861-final_en.pdf*

European Commission (2017), 'Report on the implementation of commission recommendation on strengthening the principle of equal pay between men and women through transparency', Online. Accessed (2020-04-29).
URL: *ec.europa.eu/newsroom/just/document.cfm?action=display&doc_id=48361*

Federal Ministry for Education and Women (2015), Einkommenstransparenz gleiches Entgelt für gleiche und gleichwertige Arbeit, Technical report, Vienna.

Federal Ministry for Women and the Civil Service (2010), 'Nationaler Aktionsplan: Gleichstellung von Frauen und Männern am Arbeitsmarkt', Vienna.

Gelman, Andrew and Guido Imbens (2019), 'Why high-order polynomials should not be used in regression discontinuity designs', *Journal of Business & Economic Statistics* **37**(3), 447–456.

Grembi, Veronica, Tommaso Nannicini and Ugo Troiano (2016), 'Do fiscal rules matter?', *American Economic Journal: Applied Economics* **8**(3), 1–30.

Gulyas, Andreas, Sebastian Seitz and Sourav Sinha (2021), 'Does pay transparency affect the gender wage gap? Evidence from Austria', *CRC TR 224 Discussion Paper Series, University of Mannheim* .

Imbens, Guido and Karthik Kalyanaraman (2012), 'Optimal bandwidth choice for the regression discontinuity estimator', *Review of Economic Studies* **79**(3), 933–959.

Ioannides, Yannis M and Adriaan R Soetevent (2006), 'Wages and employment in a random social network with arbitrary degree distribution', *American Economic Review* **96**(2), 270–274.

Lee, David S. and Thomas Lemieux (2010), 'Regression discontinuity designs in economics', *Journal of Economic Literature* **48**(2), 281–355.

Leonardi, Marco and Giovanni Pica (2013), 'Who pays for it? The heterogeneous wage effects of employment protection legislation', *Economic Journal* **123**(573), 1236–1278.

Mas, Alexandre (2017), 'Does transparency lead to pay compression?', *Journal of Political Economy* **125**(5), 1683–1721.

McCrary, Justin (2008), 'Manipulation of the running variable in the regression discontinuity design: A density test', *Journal of econometrics* **142**(2), 698–714.

Oesingmann, Katrin (2015), 'Workplace representation in Europe: Works councils and their economic effects on firms', *CESifo DICE Report* **13**(4), 59–64.

Roth, Anja and Michaela Slotwinski (2018), Gender norms and income misreporting within households, CESifo Working Paper 7298, CESifo, Munich.

Roussille, Nina (2020), 'The central role of the ask gap in gender pay inequality'. **URL:** *https://ninaroussille.github.io/files/Roussille_askgap.pdf*

Vaccaro, Giannina (2018), Using econometrics to reduce gender discrimination: Evidence from a difference-in-discontinuity design, paper presented at the 2nd iza workshop: Gender and family economics, New York.

Zweimüller, Josef, Rudolf Winter-Ebmer, Rafael Lalive, Andreas Kuhn, Jean-Philippe Wuellrich, Oliver Ruf and Simon Büchi (2009), 'Austrian social security database', *NRN working papers 2009-03* .
URL: *https://ideas.repec.org/p/jku/nrnwps/2009_03.html*

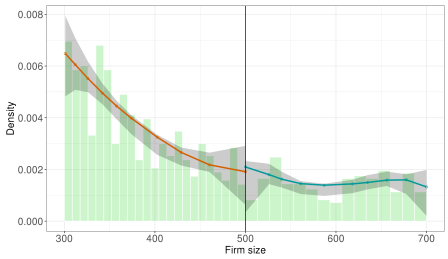
Österreichische Nationalbank (2020), 'Consumer price index', Online Table. Accessed (2020-04-29).
URL: *https://www.oenb.at/isaweb/report.do;jsessionid=0488B3220B90A6733D509077675A321B?report=6.4*

Österreichischer Gewerkschaftsbund (2011), Leitfaden zum Einkommensbericht in Unternehmen, Technical report, Vienna.

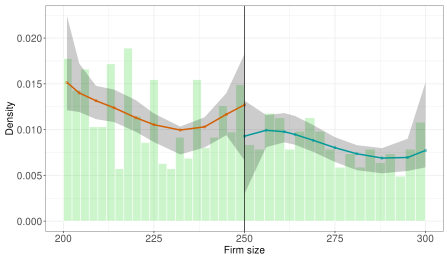
A Figures

Figure A1: McCrary Density Test

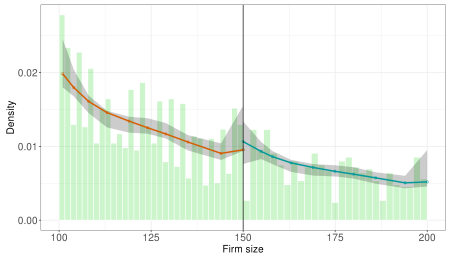
(a) Year 2012



(b) Year 2013



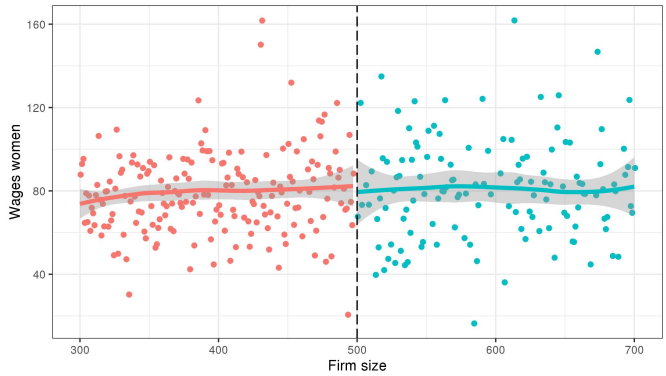
(c) Year 2014



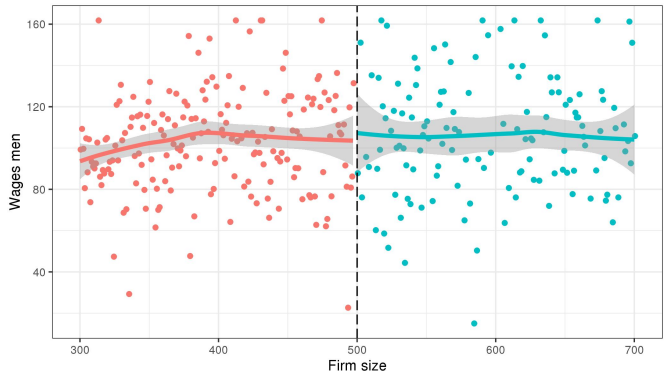
Notes: The test cannot reject the null of continuity in the running variable around the cut-off of 500 employees in 2012, 250 employees in 2013, and of 150 employees in 2014

Figure A2: Discontinuity Plots for the cut-off of 500 employees.

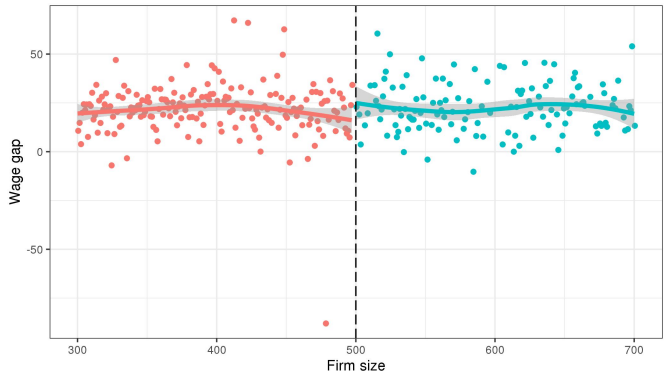
(a) Women's Wages in 2012



(b) Men's Wages in 2012



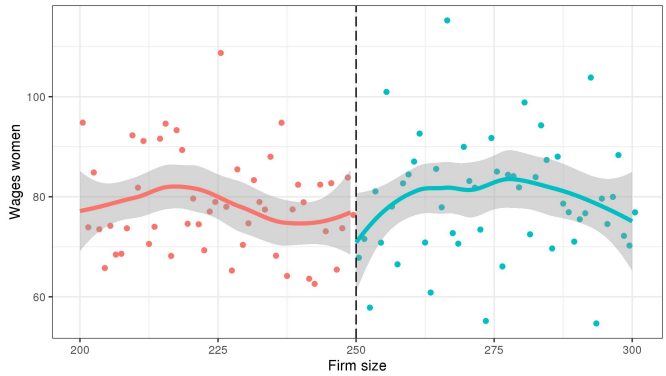
(c) Gender Wage Gap in 2012



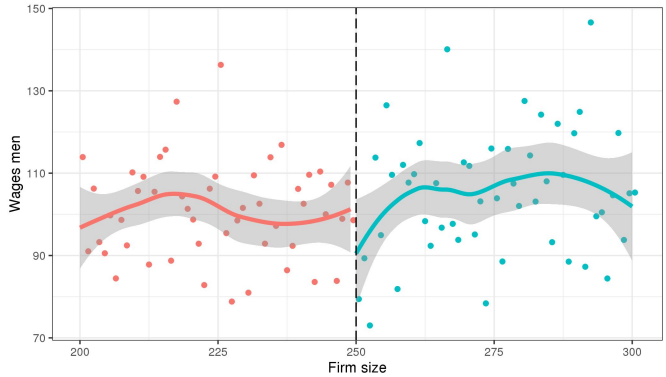
Notes: The graph presents the regression discontinuity for the cut-off of 500 employees in 2012. The vertical line represents the cut-off and each dot is the median value in that bin.

Figure A3: Discontinuity Plots for the cut-off of 250 employees.

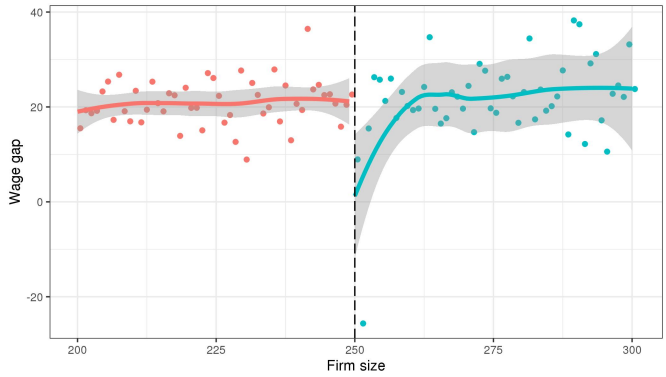
(a) Women's Wages in 2013



(b) Men's Wages in 2013



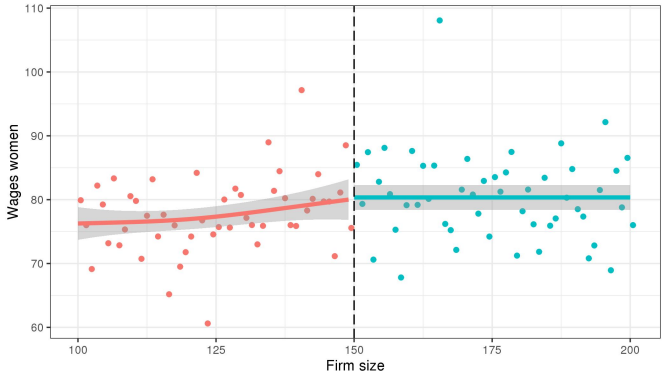
(c) Gender Wage Gap in 2013



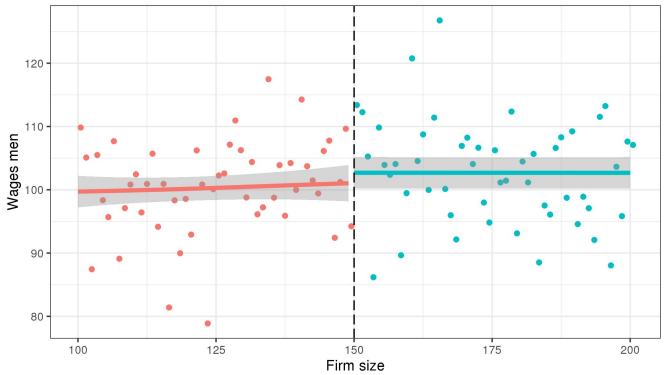
Notes: The graph presents the regression discontinuity for the cut-off of 250 employees in 2013. The vertical line represents the cut-off and each dot is the median value in that bin.

Figure A4: Discontinuity Plots for the cut-off of 150 employees.

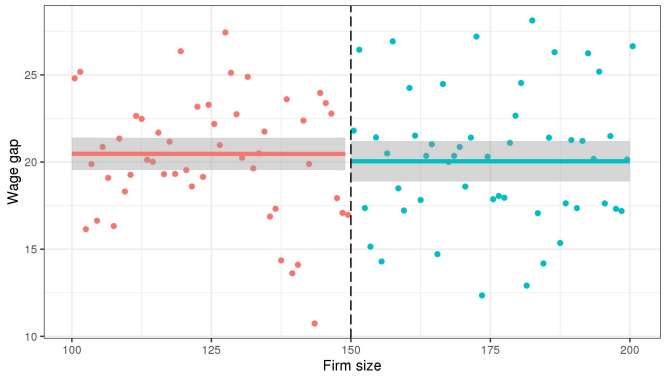
(a) Women's Wages in 2014



(b) Men's Wages in 2014



(c) Gender Wage Gap in 2014



Notes: The graph presents the regression discontinuity for the cut-off of 150 employees in 2014. The vertical line represents the cut-off and each dot is the median value in that bin.

B Additional Tables

Table B1: Diff-in-Disc results for smaller cut-offs.

Model	Women's Wages	Men's Wages	Gender Gap
Cut-off 500 in 2012			
Post X Treat	-1.604	-4.417	0.127
	(7.867)	(10.23)	(5.173)
Observations	1,543	1,423	2,103
Cut-off 250 in 2013			
Post X Treat	0.442	-3.554	-14.270
	(5.231))	(6.974)	(9.744)
Observations	1,776	1,836	2,689
Cut-off 150 in 2014			
Post X Treat	-2.019	1.186	4.673
	(4.173)	(5.607)	(2.972)
Observations	4,004	3,716	3104

Notes: The table shows the coefficients from the Diff-in-Disc estimation with bandwidth selection following Imbens and Kalyanaraman (2012). * p<0.1; ** p<0.05; *** p<0.01. Standard errors in parentheses. Cut-offs are 1,000 for 2011, 500 for 2012, 250 for 2013 and 150 for 2014. The pre-treatment year is 2010 for all estimations.

Table B2: RDD robustness for firms with 500 employees.

Model	Women's Wages	Men's Wages	Gender Gap
A. CER-optimal bandwidth selector			
Conventional	-2.435	2.252	7.06
	(7.073)	(9.973)	(5.914)
Bias-Corrected	-2.297	2.493	7.28
	(7.073)	(9.973)	(5.914)
Robust	-2.297	2.493	7.28
	(7.51)	(10.634)	(6.096)
Observations	297	264	230
B. Epanechnikov kernel smoothing			
Conventional	-1.375	2.3	5.419
	(5.368)	(7.34)	(4.757)
Bias-Corrected	-0.75	2.764	5.776
	(5.368)	(7.34)	(4.757)
Robust	-0.75	2.764	5.776
	(6.475)	(8.985)	(5.259)
Observations	529	441	436
C. Higher order polynomial			
Conventional	-0.939	2.638	4.89
	(6.64)	(9.297)	(4.994)
Bias-Corrected	-0.657	3.832	5.821
	(6.64)	(9.297)	(4.994)
Robust	-0.657	3.832	5.821
	(7.997)	(11.22)	(5.714)
Observations	1063	881	1240

Notes: The table presents robustness checks for the RDD with cut-off = 500 in 2012. The main specification uses a local polynomial of order 1, triangular kernel smoothing, and bandwidth selection by Calonico et al. (2014). * p<0.1; ** p<0.05; *** p<0.01. Standard errors in parentheses.

Table B3: RDD robustness for firms with 250 employees.

Model	Women's Wages	Men's Wages	Gender Gap
A. CER-optimal bandwidth selector			
Conventional	0.999	-1.358	-5.514
	(4.014)	(6.051)	(5.609)
Bias-Corrected	0.709	-1.975	-6.106
	(4.014)	(6.051)	(5.609)
Robust	0.709	-1.975	-6.106
	(4.232)	(6.344)	(6.081)
Observations	549	515	1369
B. Epanechnikov kernel smoothing			
Conventional	3.209	1.7	-2.178
	(3.233)	(4.819)	(3.522)
Bias-Corrected	2.424	0.031	-3.441
	(3.233)	(4.819)	(3.522)
Robust	2.424	0.031	-3.441
	(3.759)	(5.551)	(4.565)
Observations	802	771	2609
C. Higher order polynomial			
Conventional	0.753	-1.873	-7.885
	(3.813)	(5.659)	(7.243)
Bias-Corrected	0.069	-2.953	-10.219
	(3.813)	(5.659)	(7.243)
Robust	0.069	-2.953	-10.219
	(4.323)	(6.431)	(9.025)
Observations	1418	1399	2426

Notes: The table presents robustness checks for the RDD with cut-off = 250 in 2013. The main specification uses a local polynomial of order 1, triangular kernel smoothing, and bandwidth selection by Calonico et al. (2014). * p<0.1; ** p<0.05; *** p<0.01. Standard errors in parentheses.

Table B4: RDD robustness for firms with 150 employees.

Model	Women's Wages	Men's Wages	Gender Gap
A. CER-optimal bandwidth selector			
Conventional	-0.995	1.945	1.646
	(3.197)	(4.289)	(2.366)
Bias-Corrected	-1.215	1.82	1.851
	(3.197)	(4.289)	(2.366)
Robust	-1.215	1.82	1.851
	(3.399)	(4.591)	(2.519)
Observations	1202	1119	949
B. Epanechnikov kernel smoothing			
Conventional	-0.825	-0.058	1.877
	(2.587)	(3.606)	(1.965)
Bias-Corrected	-1.449	-0.689	2.429
	(2.587)	(3.606)	(1.965)
Robust	-1.449	-0.689	2.429
	(3.048)	(4.264)	(2.304)
Observations	1810	1540	1393
C. Higher order polynomial			
Conventional	-1.782	0.722	2.033
	(3.507)	(4.615)	(2.291)
Bias-Corrected	-1.995	0.919	2.238
	(3.507)	(4.615)	(2.291)
Robust	-1.995	0.919	2.238
	(3.998)	(5.27)	(2.587)
Observations	2271	2219	2415

Notes: The table presents robustness checks for the RDD with cut-off = 150 in 2014. The main specification uses a local polynomial of order 1, triangular kernel smoothing, and bandwidth selection by Calonico et al. (2014). * p<0.1; ** p<0.05; *** p<0.01. Standard errors in parentheses.