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Working Paper No. 2202

April 2022

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Mandatory Wage Posting, Bargaining and the Gender Wage Gap*

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April 4, 2022

Abstract

We evaluate whether revealing wage information in job vacancies is able to change the gender wage gap. In 2011, the Austrian Equal Treatment Law mandated every vacancy to include a minimum wage offer. This mandatory wage information makes the employer's willingness to pay and the value of outside options more salient to job applicants, thus changing bargaining options. Our general results show a small effect of the provision of wage information, reducing the gender gap somewhat. Taking up the bargaining argumentation, we split the sample into vacancies where a higher or a lower bargaining power of firms is to be expected and find a strong and significant reduction of the gender wage gap for jobs which are immediately available and need to be filled urgently. The effect is driven by an increase in female wages. There is no such effect for jobs positions which are not urgently vacant. There is no evidence for changes in vacancy characteristics, meaning the estimated effects come from the provision of wage information rather than different job descriptions and amenities offers. We also show that effects are unlikely to come from changes in the composition of employees and firms as well as from increased returns to labor market experience.

JEL Classification: J31, J23 , J63.
Keywords: mandatory wage posting, pay transparency law, gender wage gap, job postings, quantile DID.

* *Corresponding author*: Rene Wiesinger, Johannes Kepler University of Linz, Department of Economics; email: rene.wiesinger@jku.at. We thank Kajetan Schweighofer for valuable work on the textual analysis of job postings. For helpful discussions and comments we would further like to thank Anja Prummer and Matthias Fahn as well as seminar participants at University of Hohenheim and participants of ESPE, EALE, IIPF, VfS and NOeG 2022.This research was funded by the Austrian Anniversary Fund 18289.

1 Introduction

Gender pay differences – and their long-term persistence – cannot be fully explained by human capital differences (Blau and Kahn, 2017). Women are found to be less risk-prone than men (Eckel and Grossman, 2008) and less willing to enter highly competitive settings (Azmat and Petrongolo, 2014). Still, these differences in preferences are only able to explain around one fifth of the wage differential (Manning and Swaffield, 2008). Another promising explanation for the gender wage gap are gender differences in bargaining and negotiation results (Laschever and Babcock, 2003; Hernandez-Arenaz and Iriberri, 2018). Women are less likely to ask for a pay increase (Laschever and Babcock, 2003) and initiate negotiations (Leibbrandt and List, 2015; Small et al., 2007), they communicate less about wages (Goldfarb and Tucker, 2011; Cullen and Pakzard-Hurson, 2020) and they are also less confident in their knowledge of salaries than men (Cullen and Perez-Truglia, 2018; Glassdoor, 2016). Whether or not a job seeker knows the minimum wage offer of a firm in advance, may have an impact on the bargaining game, as bargaining about wages depends crucially on knowledge about bargaining power and the respective rent to be shared. Evidence suggests that gender differences are smaller when negotiators are provided with information about bargaining ranges (Bowles et al., 2005; Mazei et al., 2015) or the fact that wages are negotiable (Leibbrandt and List, 2015).

In this paper, we evaluate whether revealing wage information in vacancies is able to change the gender wage gap and how this is related to the bargaining situation of workers. In 2011, the Austrian Equal Treatment Law was changed: every vacancy posted with private or public employment agencies after March 1, has to include a minimum wage offer[1]. This mandatory wage information makes the employer's willingness to pay and the value of outside options more salient to job applicants.[2]. It also implicitly reveals the lower bound in possible wage negotiations. If women feel less informed about salaries, the provision of wage information may reduce the information gap relative to men and may, thus, decrease gender differences in negotiation outcomes (Mazei et al., 2015; Bowles et al., 2005). We would therefore expect women's wages to increase. The benefit to men might be limited because they generally negotiate more (even in absence of wage information) and tend to be more successful at doing so (Hernandez-Arenaz and Iriberri, 2018). The impact could even be negative, as firms could interpret the wage information as a take-it-or-leave-it offer, which could reduce the opportunity for wage negotiations. This would mean that men lose some of their bargaining advantage and suffer wage losses. Both effects should lead to a narrowing of the gender gap. Hence, we expect pay transparency to be particularly relevant for vacancies where firms either actively signal their willingness to bargain in the job posting or the bargaining power is shifted towards the worker, i.e. in case

[1]This minimum wage offer is not necessarily equal to the collectively bargained minimum wage. Firms can voluntarily overpay and signal with the job posting. Firms may also express their willingness to bargain in case of job experience or higher skills.

[2]New York City will introduce a similar pay transparency law for job listings in May 2022; see, for example, `https://www.wsj.com`.

of labor supply shortages or firms need to fill a vacancy urgently or have problems filling their vacancies. [3]

Implementing an extended difference-in-differences model for quantiles (Callaway et al., 2018; Callaway and Li, 2019) and using the universe of vacancies posted with the large public employment office, we evaluate male-female gender wage gaps across the entire distribution. Going beyond mean wage differentials seems worthwhile here, because giving additional information for the job vacancy might have quite different effects along the wage distribution. We separately consider vacancies where (i) firms actively signal their willingness to bargain or not, and (ii) where a higher or a lower bargaining power of firms is to be expected. We find a strong and significant reduction of the gender wage gap due to the provision of wage information if firms signal the willingness to bargain. Similarly, we find a strong and significant reduction of the gender wage gap for jobs which are immediately available and need to be filled urgently. These effects are driven by an increase in female wages. There are no such effects in the absence of a bargaining signal or for job positions which are not urgently vacant.

This paper makes several contributions to the literature. First, we mainly contribute to the existing literature on the relationship between wage information, bargaining and the gender wage gap. Roussille (2021) uses data from an online job platform to show that gender differences in salary requests explain a substantial part of gender differences in wage offers. This "ask gap" disappears when job seekers are provided with information about salary demands of other comparable candidates. The effect arises from adjustments by both women and men: While women begin to ask for more, men lower their salary requests. These results are consistent with an earlier study by Rigdon (2012). The author implements a modified version of the ultimatum game, in which one side can demand a certain amount of money before the other makes an offer, to show that gender differences in demands vanish if information about men's demands is provided. The reduction is mainly due to women making higher demands. In the situation where information is provided, women also received higher offers than in the baseline scenario. Bowles et al. (2005) argue that providing information about bargaining limits reduces the ambiguity of the bargaining situation, which should lead to smaller gender differences in bargaining outcomes. Using survey data, the authors find no differences in accepted salaries between female and male MBA students in industries where wage information is more readily available. In an experiment, they further find that there are no gender differences in negotiation outcomes when the applicants are given information about bargaining limits.

Leibbrandt and List (2015)...

A meta-analysis by Mazei et al. (2015) confirms the negative relationship between the degree of ambiguity in the bargaining situation and gender differences in negotiation outcomes in favor of men. In contrast to these papers we study the effect of a minimum wage information

[3] Alternatively, wage information could also reduce bargaining opportunities of men and they might "correct" their upward-biased expectations (e.g. Cortés et al., 2021) and beliefs about outside options (Jäger et al., 2022). This might even lead to actual wage reductions of men.

and information on the willingness of firms to bargain in job postings and focus on changes in bargaining power of workers and firms as a mechanism for the effect on the gender wage gap. Additionally we are the first paper to consider effect heterogeneity along the entire wage distribution of workers.

Second, we also contribute to the literature on how gender differences in salary expectations are affected by the provision of salary information. In one of the most recent contributions, Brandl et al. (2018) use a set of phrases typically found in Austrian job advertisements to indicate wages and evaluate these scenarios against the baseline situation in which no salary information is provided. They find that women expect to earn less than men on average in all scenarios, but that this gap is smaller in the scenarios where information is provided. In an earlier study, Major et al. (1984) found that gender differences in self-chosen remunerations are smaller when participants are provided with information about how other participants were rewarded. In contrast, the studies by Kaman and Hartel (1994) and Martin (1989) found no statistically significant effects on gender differences in pay expectations. In this study, we show that factual information about wages may change the bargaining situation and bargaining outcomes.

Third, our paper is also related to the growing number of studies focusing on the effects of pay transparency on the gender wage gap. Studies by Böheim and Gust (2021) and Gulyas et al. (2021) evaluating pay transparency measures implemented in Austria fail to find significant effects on the gender wage gap. Results for other countries, including the U.S. (Baker et al., forthcoming), the U.K. (Blundell, 2021; Duchini et al., 2020; Gamage et al., 2020), and Denmark (Bennedsen et al., 2019), suggest that pay transparency measures lead to smaller gender wage gaps. In this literature, however, pay transparency is usually understood as employees learning about the distribution of salaries and the gender wage gap within a given firm rather than actual wage information in job postings. So our work differs from these studies because we consider a different form of information treatment. The reform we use for the purpose of this paper does not lead to applicants being informed about the pay structure in general or the gender differences in salaries within a firm. Instead, it makes entry wages in different firms more salient to job seekers.

Finally, we also contribute to the growing literature using information from vacancy data to study different aspects of the labor market, such as how firms attract workers (e.g. Marinescu and Wolthoff, 2020; Mueller et al., 2020; Derencourt et al., 2021). Our data allows us to study how wage transparency laws affect entry wages of successful job applicants. Importantly, using detailed information about vacancy characteristics using the unstructured advertisement text we can also assess if the introduction of the law changed the characteristics of posted vacancies, such as changes in both cognitive and non-cognitive skill requirements, and therefore point toward an adjustment of firms' recruitment policy. With our data we can also explore whether the law changed the quality of the hired workers. Taken together, this allows us to evaluate different competing underlying mechanisms, such as wage posting versus wage bargaining (see also Lachowska et al. (2021) for similar considerations in a setting with dual jobholders). We are therefore able to provide a comprehensive picture of how wage transparency laws can affect the gender gap.

The remainder of the paper is structured as follows: Section 2 provides more information about the reform evaluated in this paper and the data employed in the estimation. The empirical strategy is outlined in Section 3. The main results, along with a discussion of potential mechanisms, are presented in Section 4. Section 5 concludes the paper.

2 Institutional Background and Data Sources

2.1 Mandatory Wage Posting in Austria

To determine the effect of wage information on the gender wage gap we exploit exogenous variation in wage posting in job vacancies introduced by a 2011 reform of Austria's Equal Treatment Law. The reformed law requires that all vacancies posted after March 1, 2011 must include the minimum wage under the relevant collective bargaining agreement or other laws and norms, as well as a reference to a possible willingness to overpay. The regulation applies to firms as well as private and public employment agencies. A transitional period was set for enforcement until January 2012, during which violations were not sanctioned. After that date, the list of sanctions comprises an admonition in case of the first violations and fines of up to € 360 for all subsequent violations. Although this would mean that the law did not go into effect only in January 2012, some firms began providing the required wage information as early as March 2011, and the Austrian employment agency mandated that jobs advertised there must comply with the updated requirements as of June 2011 (Der Standard, 2011). This implementation pattern can be observed in our data. Figure 1 plots the share of vacancies that post a wage six months before and after the reform date in March 2011. In the weeks leading up to the law change, there were virtually no job openings that posted a wage. In the week that the reform took effect, the proportion increases to about 20 %. As of mid-June, almost all job ads meet the new requirements.

Together with the reform described above, a second amendment to the Equal Treatment Law was enacted, requiring firms to regularly submit wage reports to employees. The first reports had to be prepared by firms with more than 1,000 employees by the end of July 2011. Smaller firms were not affected until later years. For details on this reform, see Böheim and Gust (2021) and Gulyas et al. (2021).

2.2 Data Sources and Sample Definition

Data Sources To construct our main data set, we combine two sources of Austrian administrative data. Data from the Austrian public employment agency cover all vacancies posted at the agency between 2005 and 2012 and contain a comprehensive set of characteristics of the advertised jobs (including the full text), the individuals who filled the advertised job and the firm that advertised the job. We use this data set to select the relevant vacancies based on their posting date[4].

The individuals and firms in the vacancy data can be directly matched with the *Austrian Social Security Database* (ASSD), which includes administrative records to verify pension claims and are structured as a matched employer-employee data set. These data cover all Austrian workers and provide detailed information on daily labor market activity. Information on individual earnings is available on an annual basis per employer. The data lacks information on the number of contracted hours, so we can only look at daily earnings (Zweimüller et al., 2009). The ASSD allows us to obtain our outcome variable of interest (daily earnings). We further draw on the ASSD to include information on the individuals' labor market history before they started the vacancy. The firm characteristics and employee demographics are used to enrich the information from the vacancy data.

Sample Definition We construct an employee-level data set of individuals who accepted a vacancy posted at the AMS. The unit of observation is a filled job posting with the posting date of a vacancy as the main inclusion criterion. Figure 2 provides an overview over the dates used for the sample and the definition of treatment and control groups. The starting point is all vacancies posted within six months before and after March 1, 2011 (i.e., between September 1, 2010 and August 31, 2011). We refer to these observations as the treatment group. For the control group, we apply the same window of six months to jobs advertised one year before the reform. Thus, it includes jobs advertised between September 1, 2009 and August 31, 2010. Within the treatment and the control group, the pre-period refers to vacancies posted between September and February, while the post-period includes vacancies posted between March and August.

The way we construct our treatment and control groups implies that the we observe vacancies and individuals at different times of the year and in different calendar years. To ensure the comparability of jobs both within and across the treatment and control group, we group the vacancies by a 4-digit occupation code, firm industry, firm location based on federal states, collar and full- or part time jobs. We only keep those groups of vacancies that are observed in both the treatment and control group, before and after March 1 and for men and women. To further increase comparability, we exclude vacancies for seasonal jobs. These include all job openings explicitly identified as seasonal by a binary indicator in the vacancy data as well as, all vacancies posted by firms in agriculture, mining, construction and food and accommodation. Furthermore, we drop all job openings from staff leasing companies, vacancies with a filling time of more than one year as well as job openings for marginal employment and apprenticeships. We keep only observations that can be matched with the ASSD and where the employment spell in the ASSD is either blue or white collar (this excludes civil servants).

[4] Say something on representativity of vacancies, cite kettemann et al.

2.3 Vacancy characteristics and worker sorting

We are interested in the effect of pay transparency in job postings on the gender gap in entry wages. However, apart from the mandatory provision of wage information in the posting, firms may also change their postings in other dimensions as well. In order to check whether the reform changed the characteristics of vacancies other than the wage information we run a simple linear difference-in-difference model,

$$y_{ivtojs} = \alpha + \beta_1 \cdot T_v + \beta_2 \cdot Post_t + \beta_3 \cdot T_v \times Post_t + \lambda_o + \lambda_j + \lambda_s + \varepsilon_{ivtojs} \tag{1}$$

with y_{ivtojs} as a vacancy characteristic, such as required skills, routinisation, bargaining signal, fringe benefits, contract characteristics or required education, T_v as the treatment indicator being 1 if vacancy is posted between September 2010 and August 2011, $Post_t$ as the indicator for vacancies posted after March. β_3 shows the difference in a certain vacancy characteristic due to the reform. We also add occupation, industry and federal state fixed effects.
Figure 4 plots the results for the interaction term for a set of skill requirements of the job (panel a), job amenities offered in the job posting (panel b) as well as contract characteristics (panel c). None of these estimates appear to be statistically significantly different from zero. We take this finding as evidence that apart from the mandatory wage information, other vacancy characteristics and job requirements did not systematically change with the reform.

The reform may also affect the sorting of workers into those vacancies. This might be particularly worrisome if different type of female workers select into certain jobs once they learn about the minimum offered wage. First, Bamieh and Ziegler (2022) find no evidence for such sorting. Second, with the identical empirical difference-in-difference model as before we test for compositional changes of workers filling those vacancies, i.e. y_{ivtojs} denote worker characteristics such as individual characteristics, commuting and moving distances, unemployment duration, tenure and a set of firm characteristics. Table 3 summarizes the estimation results for several worker characteristics and find no statistical significant changes, i.e. workers filling the vacancies have on average the same characteristics as before the reform. Table 4 extends the Differences-in-Difference model by adding a gender dummy interaction. Again, we do not find any significant triple-interactions and therefore no evidence for gender-specific sorting into vacancies due to the mandatory provision of wage information. Additionally, Figure 5 shows no evidence for gender difference in sorting into industry sectors after the reform. So these results are therefore in line with findings in Bamieh and Ziegler (2022).

3 Estimation Strategy

3.1 Descriptive Statistics

Table 1 reports sample means along with a comparison of means between the treatment and control group for several variables. Around 60 % of employees are female. The average age is approximately 35 years. Non-Austrian citizens make up around 18 % of the sample. Concerning job characteristics, around 45 % come from vacancies fore white collar jobs and more than 60 % come from vacancies for full-time jobs. Firms in the retail industry make up the largest part of the sample (39 %), followed by manufacturing (25 %), services (16 %) and health, education and public administration (14 %). The main outcome variable is the average daily wage excluding special payments measured in 2012 Euros. The average daily wage rate in the sample is € 49.737. The comparison of means between the treatment and control group reveals several statistically significant differences, although they are generally small. In addition to grouping the observations as described before, we also control for age, citizenship, labor market experience, unemployment duration before than vacancy, the number of children and a binary indicator for whether the job is immediately available when computing the treatment effects. BARGAINING VARIABLES ETC

3.2 The Gender Gap across the Wage Distribution

Recent evidence shows that there are substantial differences in the gender gap across the wage distribution (Blau and Kahn, 2017; Albrecht et al., 2003; Arulampalam et al., 2007; Hara, 2018). Figure 3 shows the raw log gender gap across the distribution for the full sample (a) and for full-time jobs (b) and reveals significant differences at the borders of the wage distribution. The gender gap is particularly pronounced at the lower bound.[5]. This suggests that providing wage information to prospective applicants may have a different impact on pay differentials between women and men, depending on their place in the distribution. For example, women at the lower part of the distribution may be less informed about the labor market relative to those at the upper part and therefore benefiting relatively more from wage information. One can also think of situations where women at the upper part may benefit more from wage transparency. For example, pay secrecy may be more prevalent in top jobs, exacerbating the disadvantages women face when negotiating. In all these cases, concentrating on mean effects will masks important heterogeneity.

Looking at the distribution, for each quantile $\tau \in (0, 1)$ our primary interest is how mandatory wage information affects the gender wage gap

$$\Delta^{GG}(\tau) = \Delta^{W}(\tau) - \Delta^{M}(\tau) \tag{2}$$

[5] Existing literature typically report larger differences at the upper part of the distribution. However, given our sample of vacancies offered at the labor agency, these high-paid jobs are typically not part of our sample

where $\Delta^W(\tau)$ and $\Delta^M(\tau)$ are the estimated impact of the mandatory wage posting law on women's and men's wages respectively at quantile τ. In other words, $\Delta^{GG}(\tau)$ estimates the impact of the mandatory wage posting law across the wage distribution and allows us to identify potential heterogeneous impacts.

Let Y_s be wages observed in period s, where $s \in t-1, t$. Denote by D_t our treatment indicator, with $D_t = 1$ if mandatory wage posting was enacted in period t and zero otherwise. Denote by $Y_s(1)$ the potential wages a unit would receive under the law and likewise $Y_s(0)$ the potential wages absent the law (see, for example Imbens and Wooldridge, 2009, for a discussion on potential outcomes). Then, we can define $\Delta^j(\tau)$ for $j \in \{W, M\}$ more formally as

$$\Delta^j(\tau) = F^{-1,j}_{Y_t|D_t=1}(\tau) - F^{-1,j}_{Y_t(0)|D_t=1}(\tau) \tag{3}$$

where $F^{-1,j}_{Y_t(d)|D_t}(\tau)$ is the quantile function conditional on D defined as $\inf\{y : F^j_{Y(d)|D_t}(y) \geq \tau\}$ for $d \in [0,1]$. Equation (3) is the Quantile Treatment Effect (QTE), evaluating how mandatory wage posting affect wages for women (men). Notice that we can estimate $F^{-1,j}_{Y_t|D=1}(\tau)$ directly from the data using the empirical quantiles. The counterfactual quantiles $F^{-1,j}_{Y_t(0)|D=1}(\tau)$ are, however, not observed.

Under the strong assumption that the wage posting law was randomly introduced, one could obtain the counterfactual outcomes using the "standard" QTE approach (e.g. Imbens and Wooldridge, 2009). Politicians certainly were concerned about the potential impact on wages when introducing the wage posting law, however. To estimate the impact of mandatory wage information on the distribution of the gender gap, we therefore use the quantile difference-in-difference (QDiD) approach suggested by Callaway et al. (2018) and Callaway and Li (2019) instead.

Callaway et al. (2018) show that one can identify the QDiD using two-time periods by imposing two assumptions on the counterfactual outcomes. Define $\Delta Y_{it}(0) = Y_{it}(0) - Y_{it-1}(0)$ as the difference in untreated potential outcomes. Our first assumption, the (conditional) Distributional Difference-in-Difference Assumption can be stated formally as

Assumption A1: Conditional Distributional Difference-in-Difference.

$$\Delta Y_{it}(0) \perp\!\!\!\perp D_{it}|X_i \tag{A1}$$

Assumption A1 requires that once we take differences, conditional on covariates, the potential outcomes if the law had not been enacted do not depend whether a unit i belongs to the treatment or control group. Intuitively, it extends the standard "parallel trend" assumption in mean DiD models to hold over the whole distribution. While not directly testable, we provide evidence that Assumption A1 holds in our setting, by estimating Equation (5) using only outcomes prior to the enactment of the law. This type of placebo test is akin to what is usually done in mean DiD settings, but concentrating on the outcome over the whole distribution.

The second assumption we need to impose is more specific to our QDiD approach. It requires some structure on the dependence of how counterfactual outcomes change between our two-time period across the distribution. More formally, it requires an invariance of the conditional copula with respect to D:

Assumption A2: Conditional Copula Invariance.

$$C_{\Delta Y_t(0),Y_{t-1}(0)|X,D_t=1}(u,v|X) = C_{\Delta Y_t(0),Y_{t-1}(0)|X,D_t=0}(u,v|X); \forall (u,v) \in [0,1]^2 \tag{A2}$$

On first sight, Assumption A2 is not very intuitive to grasp. It captures rank dependency between $\Delta Y_t(0)$ and $Y_{t-1}(0)$ and requires that the dependency of the variables are the same for treatment and control groups. More intuitively, Assumption A2 requires that changes in the outcome of control units at certain parts of the distribution also happen with a similar likelihood for potential outcomes of treated units. For example, under Assumption A2, if we observe that the largest changes for control units happen at the lower part of the distribution, we also expect to see the largest changes to happen at the lower part of the distribution for treated units in the absence of treatment. So, neither any structural shifts nor the treatment itself may change each vacancy's position in the wage distribution[6]. Also the treatment effect may differ in size along the distribution as long as the vacancies' position in the distribution remains constant[7].

Callaway et al. (2018) show that under Assumptions A1 andA2, one can identify the counterfactual cumulative distribution function $F^j_{Y(0)|D_t=1}(y)$ from the data using

$$F^j_{Y_t(0)|D_t=1}(y) = n^{-1}_{\mathcal{D}^C} \sum_{i \in \mathcal{D}^C} \mathbb{1}\{\Delta Y_{it} + F^{-1,j}_{Y_{t-1}|X,D_t=1}(F^j_{Y_{t-1}|X,D_t=0}(Y_{it-1})) \leq y\} \tag{4}$$

where $\mathcal{D}^C$ is the set of control units and $n_{\mathcal{D}}$ its cardinality.[8]

Equation (4) allows us to identify the distribution of the counterfactual outcome using information on control units. It requires that our treatment units are similar both in a distributional sense and how outcomes evolve over time in the absent of the wage information law to our control units. Thus, we need to impose assumptions concerning the whole distribution which tend to be stronger than the usual assumptions imposed in mean DiD models.

3.3 Estimation of the Gender Gap

To estimate Equation (4) from the data, we impose an additional assumption on the conditional quantile function. Specifically, we assume that it is linear in parameters β.

[6]For example, if collective bargaining agreements were less favorable for control units at the lower part of the distribution after the end of the recession. We do not find evidence, however, that this was the case.

[7]If a vacancy is e.g. on the 10^{th} percentile, the treatment may not shift this vacancy to a higher or lower percentile to ensure the comparison of identical vacancies

[8]One also needs to assume that the outcomes are continuous, otherwise the effects are not identified. As our outcome are wages, this assumption is satisfied trivially in our setting.

Assumption A3: Linear Conditional Quantile Function.

$$F^{-1}_{Y|X,D}(\tau) = X'\beta(\tau) \tag{A3}$$

Assumption A3 is standard in the literature concerning quantile treatment effects. It considerably facilitates estimation of Equation (4). Specifically, under Assumption A3 we can obtain the missing quantities on the right hand side of Equation (4) using prediction of linear quantile regressions (and the inverse thereof). To obtain the counterfactual $F^{-1,j}_{Y_t(0)|D_t=1}(\tau)$ in Equation (3), we invert the estimate of $\widehat{F}^{j}_{Y_t(0)|D_t=1}(y)$

One additional challenge arises in our setting, as we observe multiple units i within a time period. As discussed above, this is as we observe multiple vacancies posted with the same characteristics in our data, i.e. several firms might offer the same job in one of the time periods (e.g. as typical for office clerks, cleaning jobs...). Equation (4) requires, however, that we observe each unit i exactly once in time t and once in time $t-1$, hence we require each group of vacancy only once in each of the four defined time periods.

To incorporate multiple observed units i in our approach, we do the following: First, we randomly draw one observation for each group i for both our treatment and control group. We do this separately for men and women. For our random draw r, we obtain background characteristics as well as the wage in time t and time $t-1$. Then, we calculate both $\widehat{\Delta}^{M,r}(\tau)$ and $\widehat{\Delta}^{W,r}(\tau)$, as well as the gender gap $\widehat{\Delta}^{GG,r}$. It should be noted that when calculating these quantities we use the entire wage distribution. That is, the points y over which our distribution is evaluated when estimating $\widehat{\Delta}^{M,r}(\tau)$ are the same points when estimating $\widehat{\Delta}^{W,r}(\tau)$. We repeat this sampling procedure R times, each time drawing a random sample and where R is a large number. In other words, we obtain R different treatment effects for men and women as well as R different estimates of the gender gap. We then average over all R simulated values to obtain $\widehat{\Delta}^{M}(\tau)$, $\widehat{\Delta}^{W}(\tau)$, and estimates for the $\widehat{\Delta}^{GG}$.

In practice we set R to 500. We base inference on the bootstrap using 500 replications. To be more precise, within each bootstrap replications we conduct our simulation approach 500 times as discussed above[9].

4 Estimation Results

4.1 Main results

If providing salary information in job postings actually encourages applicants to negotiate over pay, we might expect the gender wage gap to narrow. In particular we expect effects in situations where the bargaining power of firms is low. One situation in which firms' bargaining power is lower is when a vacancy must be filled immediately. Figure 7 summarizes the treatment effect

[9] We also tried higher numbers R and of bootstrap replications without changes, so we stick to $R = 500$ and 500 bootstrap replications for computational reasons.

of the reform over the entire distribution for vacancies which are immediately available or not[10]. We argue that for immediately available jobs, firms are willing to pay higher wages or to engage in bargaining in order to quickly fill the vacancy. As panel (a) of Figure 7 shows, the gender gap is reduced for immediately available jobs significantly along the entire wage distribution and tend to be slightly larger above the median than below. The outcome variable is log daily wages, which allows to interpret the coefficients as percentage point changes in the gender wage gap. The coefficient is obtained by computing the QTE for women and men separately and then subtracting the effect for men from the effect for women. Positive coefficients therefore mean a narrowing of the gender gap, while negative coefficients are interpreted as a widening. On average, the reduction amounts to 11.5 percentage points, which is a reduction in the gender entry wage gap of approximately a third[11]. In contrast, for vacancies that are not immediately to be filled, such a treatment effect is absent for all percentiles τ (panel (b) of Figure 7). Consistent with our reasoning, we find that the narrowing of the gender gap is due to an increase in women's wages along the entire distribution (panel (c)). Men experience wage declines in most parts of the distribution, although the effects are not statistically significant for almost all quantiles (panel (e)).

Similarly, one might assume that the bargaining power of employers is smaller when the firm has difficulty filling a vacancy. Therefore, we divide the sample by filling time and consider the effects separately for immediately and not immediately filled vacancies.[12] We would expect larger effects for the latter group. Indeed, Figure 8 shows that a reduction in the gender wage gap along the entire distribution is only observed for jobs that are not immediately filled (panel (b). The mean effect is again statistically significant, but smaller compared to the treatment effects for immediately available jobs. Again, the effect stems from an increase in women's wages (panel d) and a decrease in men's wages (panel f)), with the mean effects being similar in absolute value. For immediately filled vacancies (panel (a)), we find no effect on the gender gap above the median. Below the median, our estimates suggest a (statistically insignificant) widening of the gender gap due to a decline in women's wages in this part of the distribution (panel (c)).

The consequences of increased bargaining by workers should also lead to larger effects in settings where employers directly signal a willingness to bargain to applicants through the vacancy text. Since the vacancy data also includes the full text, we can split the sample into observations from vacancies with and without a bargaining signal. A vacancy contains a

[10]Job availability is defined as the difference between the date the job is posted and the date the firm announces that the job is available. We define vacancies to be immediately available if there are not more than seven days between the posting date and the date to which the firm states that the job is available. Around 35 % of observations in our sample come from immediately available vacancies.

[11]The blue dashed line marks the mean effect. It is also indicated in the upper right corner of the plot with the t-statistic in parentheses.

[12]A vacancy is categorized as immediately filled if the filling time (i.e. the difference between the posting date and the filling date) is smaller or equal than seven days. Approximately 12 % of observations in our sample come from immediately filled vacancies.

bargaining signal, if the text includes phrases that indicate that the starting wage is subject to an agreement between the employer and the prospective employee or if the firm states a willingness to overpay. Approximately 40 % of observations come from vacancies that contain a bargaining signal. The results for the gender gap are reported in Figure 6. We find statistically significant reductions in the gender wage gap along the entire wage distribution, with larger point estimates in the lower end of the distribution (panel (a)). The mean effect of a reduction in entry wages of 10 percentage points is statistically significant on the 5 % level. In line with the argument that the wage information should increase bargaining by women, we again find that the reduction in the gender gap is driven by increases in women's wages (panel (c)). Men's decrease slightly, but none of the estimated coefficients are statistically significant(panel (e)). The results for observations from vacancies without a bargaining signal suggest a mean effect of zero(panel (b)).

4.2 Further findings

Table 2 summarizes the results of an identical analysis using a more standard linear difference-in-difference model. We estimate the following model:

$$y_{ivtojs} = \alpha + \beta_1 \cdot T_v + \beta_2 \cdot Post_t + \beta_3 \cdot T_v \times Post_t + \beta_4 \cdot T_v \times Post_t \times Fem_i + \Gamma_{ivtojs}\lambda_o + \lambda_j + \lambda_s + \varepsilon_{ivtojs} \tag{5}$$

where the triple-interaction with a gender dummy β_4 is a direct estimate of the treatment effect on the gender wage gap. Γ consists of a set of additional covariates also used for the quantile analysis, i.e. indicators white-collar job, , full-time vacancy and migration status, as well as a second-order polynomial of age, experience, unemployment duration and number of children. We further include occupation, industry, federal state and month of year fixed-effects. This analysis confirms our findings from the quantile differences-in-difference analysis. We find a significant reduction of approximately 4.5 percentage points the gender entry wage gap for vacancies including a bargaining signal as well as for jobs that are immediately available or could not be filled. There are no such significant effects in the opposite scenarios.

Finally, Figure A.1 presents the overall estimates for the gender gap for all $\tau \in [0.05, 0.95]$ irrespective of the bargaining situation of firms or bargaining signals. In general, the results indicate a reduction in the gender gap along the entire wage distribution. The point estimates show a clear upward shift for the quantiles above the median, while they are close to zero in the lower half of the distribution. We also find a sizable but rather imprecisely estimated effect for the lowest quantiles. Except for a few estimates around the median and the top 10^{th} percentile, the estimated coefficients are insignificant. The average estimate would imply a (statistically insignificant) 3.5 percentage point reduction in the gender wage gap (around € 1.70 per day).

Looking at the results separately for women and men in Figure A.2 reveals that the impact on the gender gap are mainly driven by wage reductions for men. While both women and men tend to experience wage gains at the lower end of the wage distribution (with women's gains exceeding those of men), the effects on men's wages are persistently negative for $\tau \geq 0.2$. Wage gains for women also emerge around the median and at the upper end of the wage distribution. Together with persistent reductions in men's wages, this explains the larger narrowing of the gender gap above the median. The estimates for almost all quantiles are statistically insignificant. The mean effects would imply a wage gain of 1.4 % (€ 0.874 per day) for women and a wage loss of 2.1 % (€ 0.855 per day) for men. Neither estimate is statistically significant.

4.3 Robustness

To check the validity of the main identifying assumptions, we perform placebo checks suggested by Callaway et al. (2018) and Callaway and Li (2019). For this purpose, we define two placebo samples containing only untreated observations. The samples are constructed based on the structure outlined in Figure 2 and described in Section 2, except that all dates are shifted one (for the first placebo sample) and two (for the second placebo sample) years into the past. We then re-estimate the QTE for both samples and would expect the coefficients for all quantiles to be indistinguishable from zero. The results of this exercise are presented in Panels (a) and (b) of Figure A.4. As expected, the estimated coefficients are close to zero and are not statistically significant for all τ. The mean effects are virtually zero in both samples. Together with the evidence presented in Figure 4 that the reform did not change the job descriptions, amenities offered and other vacancy characteristics, this strengthens the assumption that the estimated effects are indeed due to the provision of wage information in job postings.

Second, firms with more than 1,000 employees were potentially affected by the second reform implemented in July 2011 (see Section 2.1, Böheim and Gust (2021) and Gulyas et al. (2021)). Although there is no evidence of an effect of the second reform on the gender wage gap, these firms might have changed their wage transparency behavior also for entry wages. In a robustness check, we estimate the main results and placebo checks for a sample excluding the firms affected by the second reform and can confirm our main findings (see Figure 10).

Third, post-reform job advertisements often state that actual starting wages depend on previous labor market experience. This wording signals to applicants that their experience will increases their pay. As a result, effects on wages and the gender wage gap might also stem from higher returns to experience rather than a change in bargaining behavior. To check for this potential alternative explanation, we split the sample into individuals with above and below median labor market experience[13] and compute the effects for the two samples separately. We find slightly larger point estimates for the decline in the gender wage gap among indi-

[13]The median labor market experience in our sample is approximately seven years.

viduals with high labor market experience, although the effects are statistically insignificant for almost all quantiles (see Figure 11). The gender-disaggregated results show that there are no statistically changes in women's and men's wages. We interpret these results as evidence that effects on the gender wage gap do not come from higher returns to labor market experience.

5 Conclusions

In this paper, we evaluate whether revealing wage information in vacancies is able to reduce the gender wage gap. In 2011, the Austrian Equal Treatment Law was changed: every vacancy posted with private or public employment agencies after March 1, has to include a minimum wage offer. This mandatory wage information makes the employer's willingness to pay and the value of outside options more salient to job applicants.

Overall we show a small effect of the provision of wage information, reducing the gender gap somewhat. However, we find a strong and significant reduction of the gender wage gap for vacancies where firms signal willingness to bargain wages and where the bargaining power of firms is expected to be low, i.e. in jobs to be filled urgently. The effect is driven by an increase in female wages along the entire wage distribution. This suggests that the mandatory information on a minimum wage and the increased transparency on outside options potentially reduces a gender information gap and improves the bargaining outcomes of women relative to men.

There is no evidence for changes in vacancy characteristics, meaning the estimated effects come from the provision of wage information rather than different job descriptions and amenities offers. We also show that effects are unlikely to come from changes in the composition of employees and firms as well as from increased returns to labor market experience.

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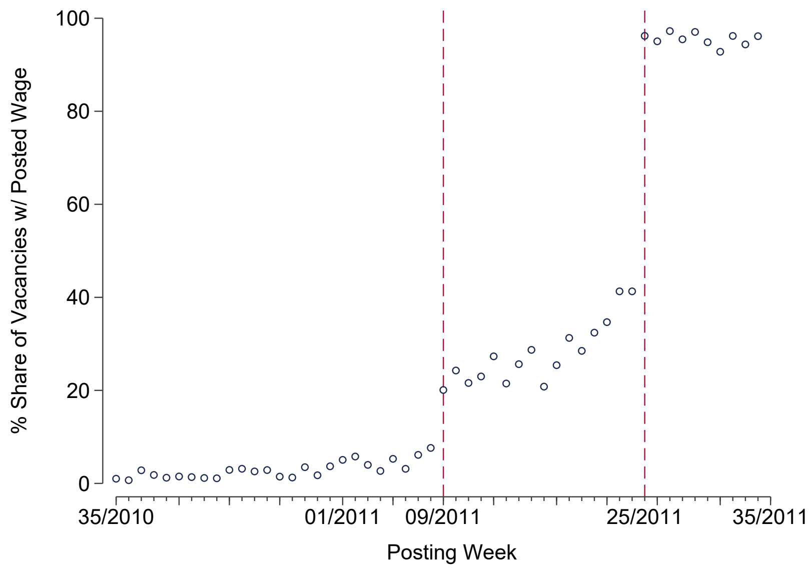
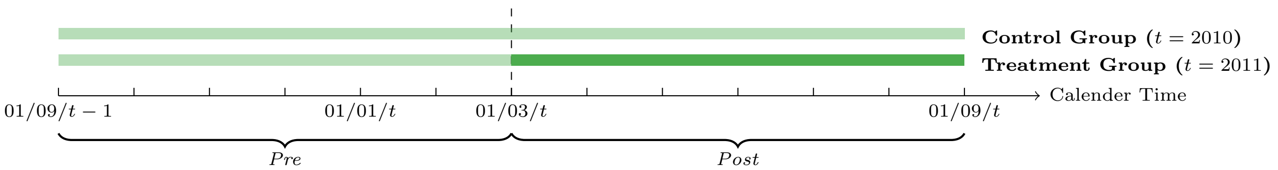
6 Figures (to be placed in the article)

Figure 1: Share of Vacancies with Posted Wage Around Reform Date



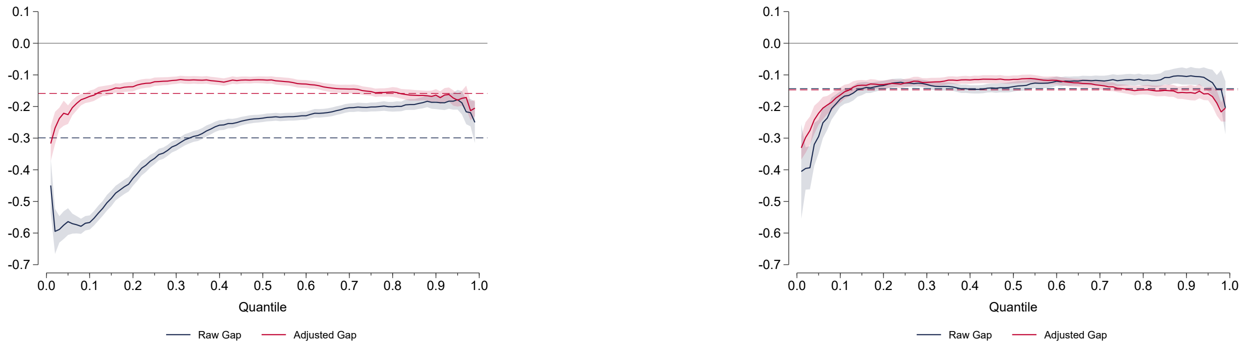
Note — Share of vacancies with a posted wage six months before and after the Equal Treatment Law reform by posting week. The first vertical line represents marks the date of the law reform (March 1, 2011), the second marks the enforcement date of reform by the employment agency (June 20, 2011).

Figure 2: Definition of treatment and control group



Note — XXXX

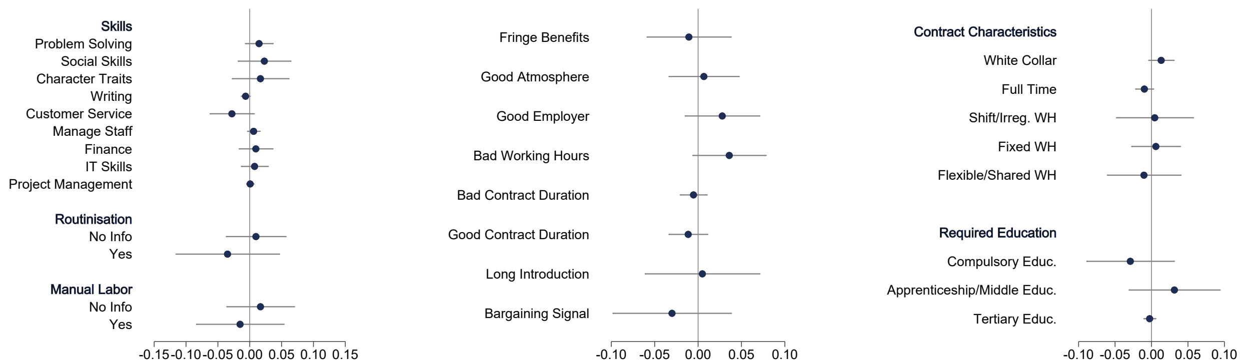
Figure 3: Gender gap along the wage distribution



(a) Log Gap - Full Sample **(b) Log Gap - Full-Time Only**

Note — xxx

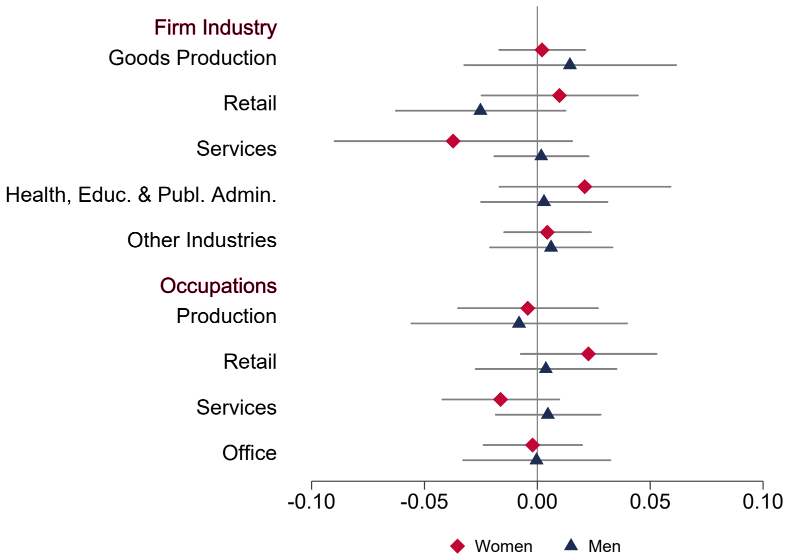
Figure 4: Changes in Vacancy Characteristics



(a) Skills **(b) Amenities** **(c) Contract Char. & Education**

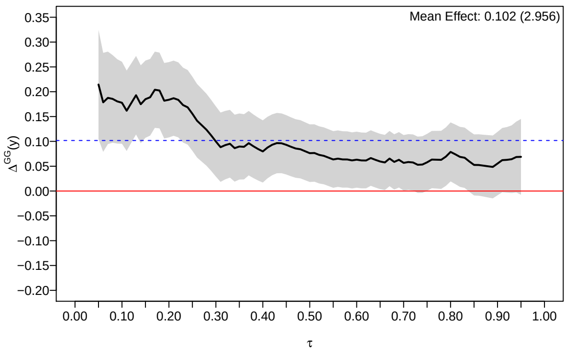
Note — Linear difference-in-difference estimates. The graphs only show the coefficients of Treatment × Post term. The regression additionally includes occupation, industry and federal state fixed effects. Blue dots represent the point estimates. Grey lines represent 95 % confidence intervals based on standard errors clustered on the occupation level.

Figure 5: Changes in Industry and Occupation Composition by Gender

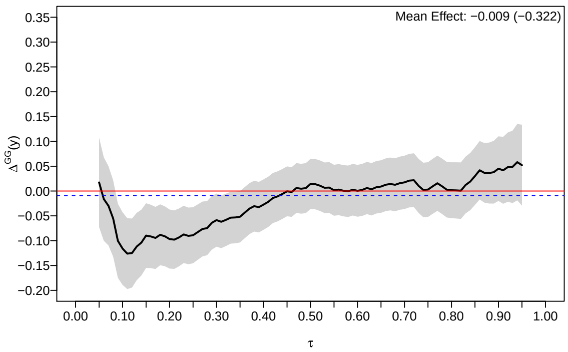


Note — Estimates are based on a linear difference-in-difference model with federal state fixed effects. The model has been estimated separately for women (red diamonds) and men (blue triangles). Binary indicators for each industry and occupation group were used as outcome variables. The estimates can therefore be interpreted as the percentage point change in the respective industry's/occupation group's % share in the sample. Grey lines represent the 95 % confidence intervals.

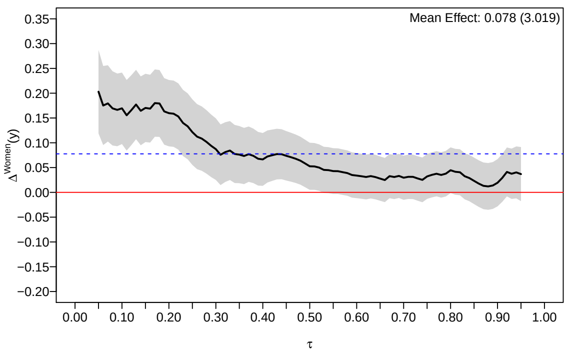
Figure 6: Results by Bargaining Signal



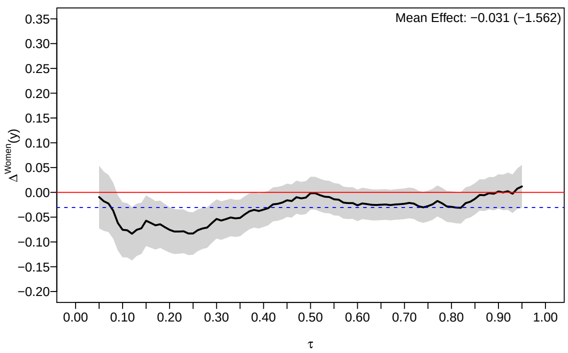
(a) Bargaining Signal - Gender Gap



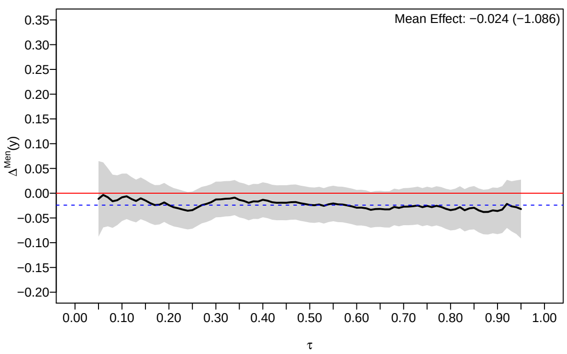
(b) No Bargaining Signal - Gender Gap



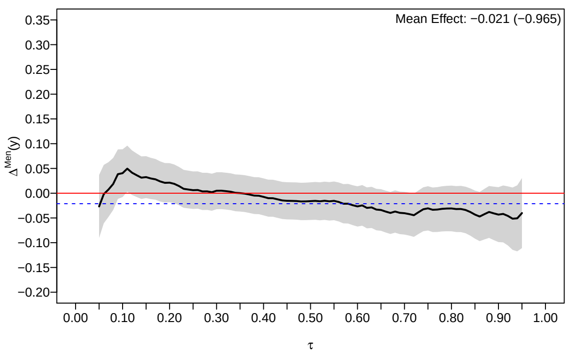
(c) Bargaining Signal - Women



(d) No Bargaining Signal - Women

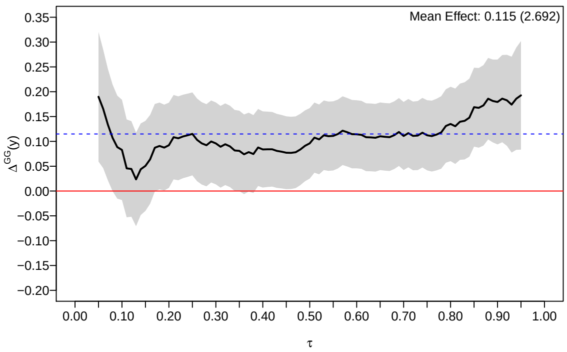


(e) Bargaining Signal - Men

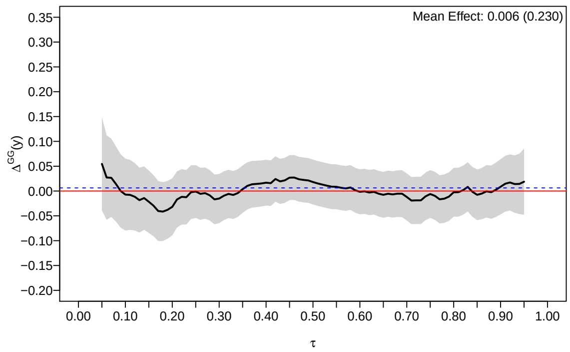


(f) No Bargaining Signal - Men

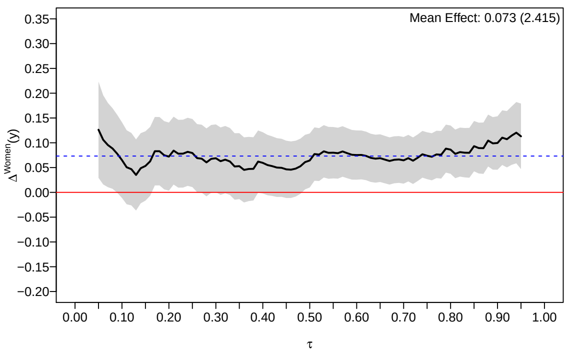
Figure 7: Results by Job Availability



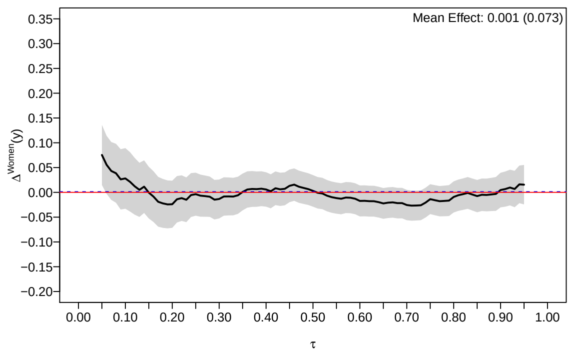
(a) Immediately Available - Gender Gap



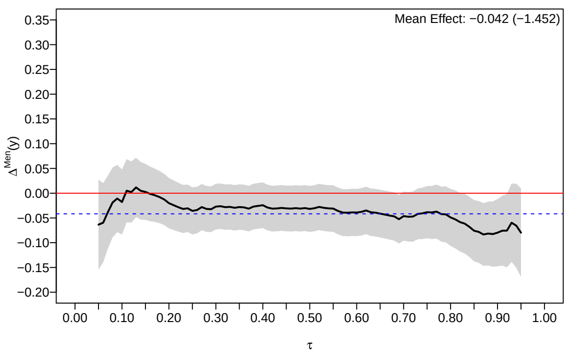
(b) Not Immediately Available - Gender Gap



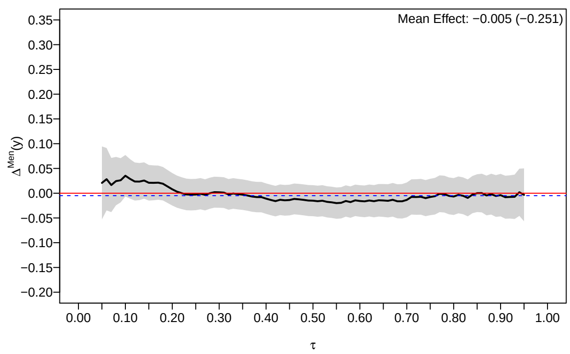
(c) Immediately Available - Women



(d) Not Immediately Available - Women

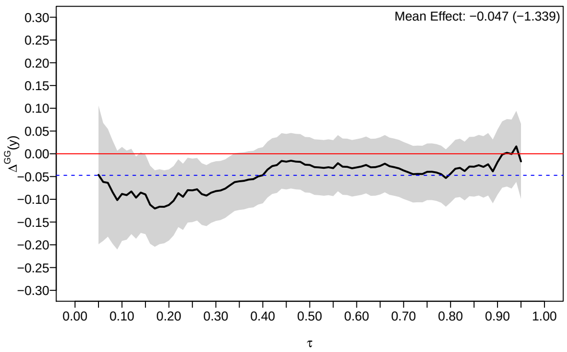


(e) Immediately Available - Men

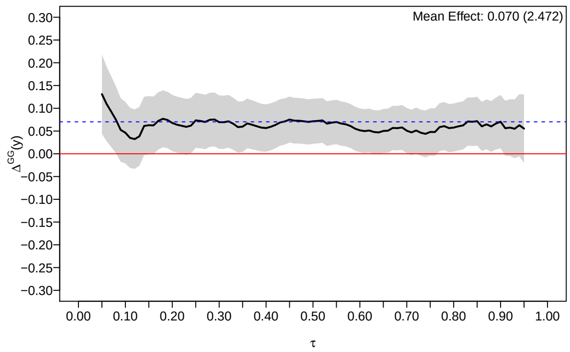


(f) Not Immediately Available - Men

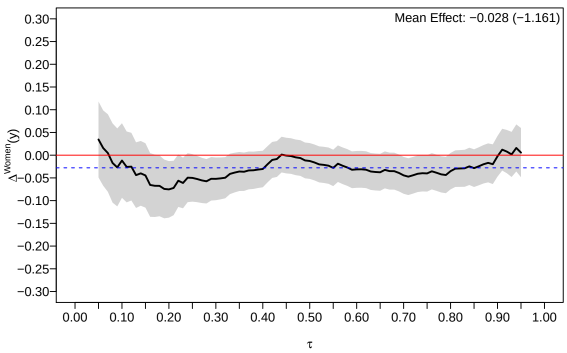
Figure 8: Results by Filling Time



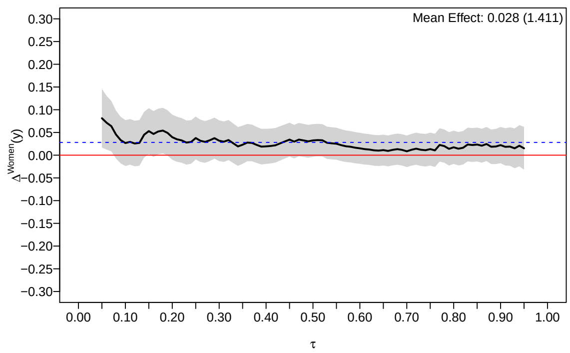
(a) Immediately Filled - Gender Gap



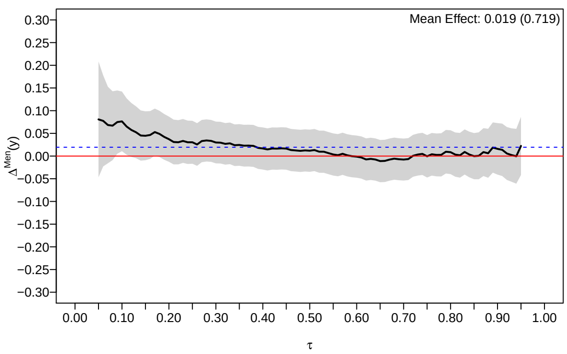
(b) Not Immediately Filled - Gender Gap



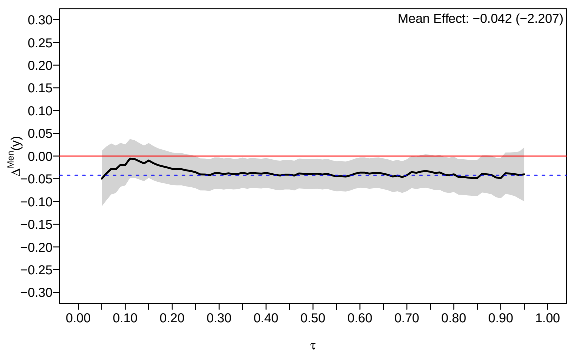
(c) Immediately Filled - Women



(d) Not Immediately Filled - Women

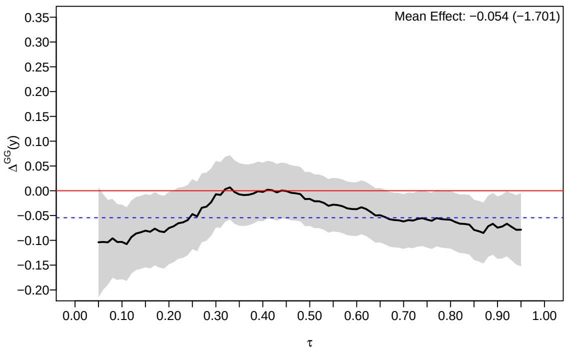


(e) Immediately Filled - Men

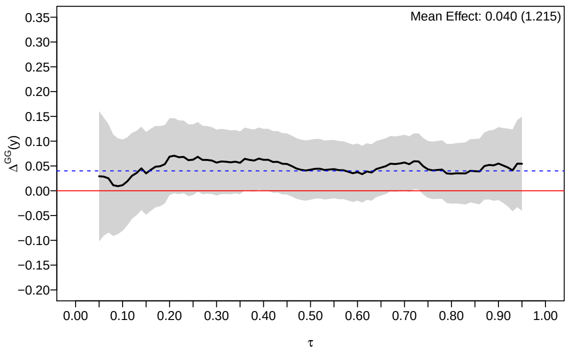


(f) Not Immediately Filled - Men

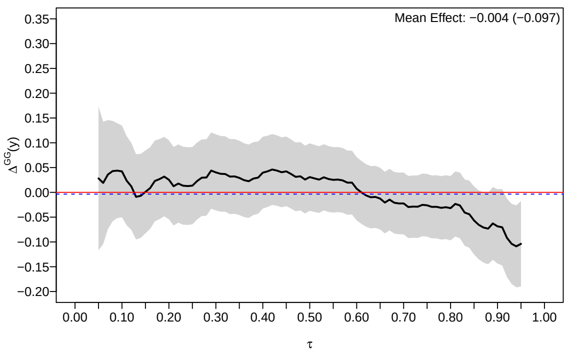
Figure 9: Placebo Checks for Gender Gap Results



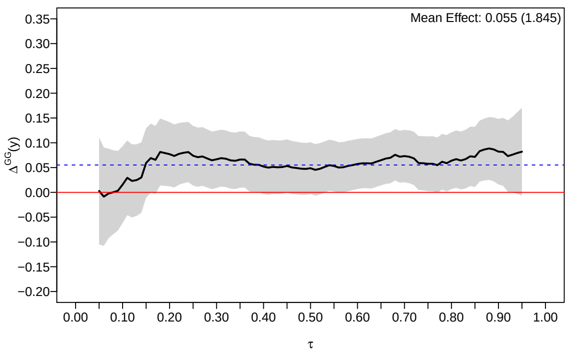
(a) Bargaining Signal



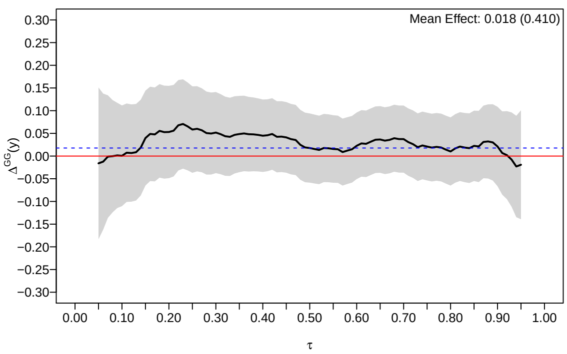
(b) No Bargaining Signal



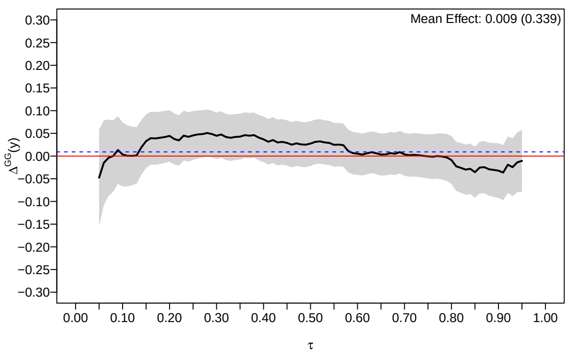
(c) Immediately Available



(d) Not Immediately Available

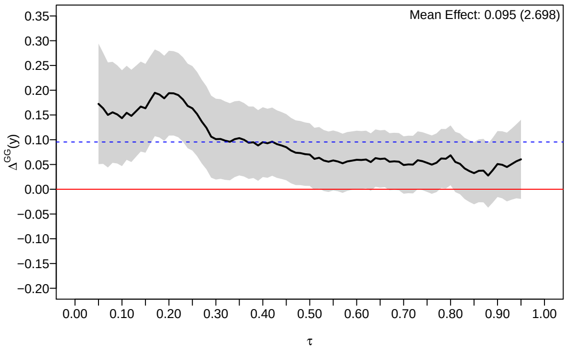


(e) Immediately Filled

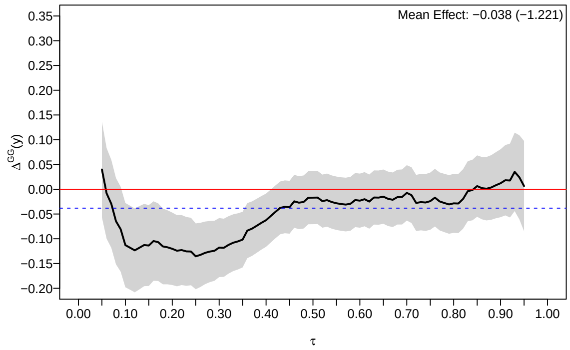


(f) Not Immediately Filled - Men

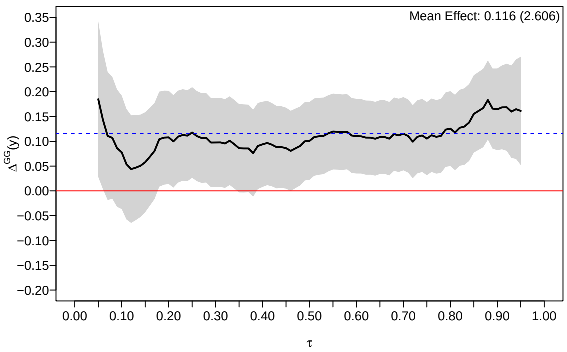
Figure 10: Gender Gap Results – Excluding Firms with More Than 1 000 Employees



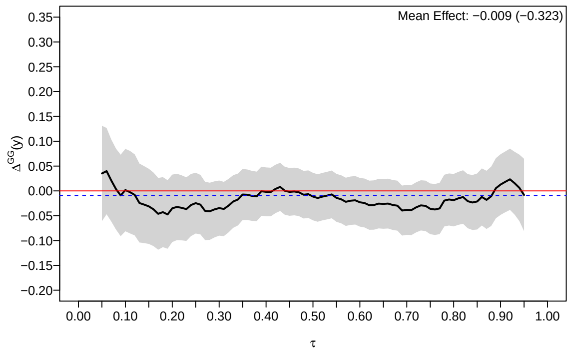
(a) **Bargaining Signal**



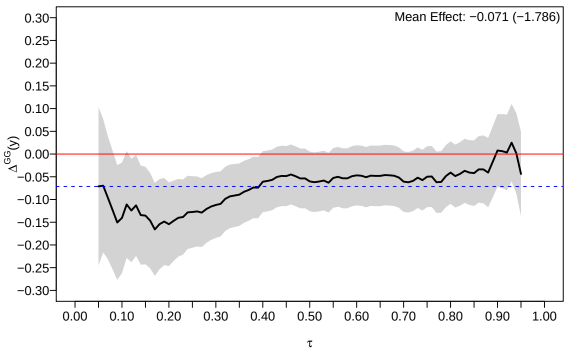
(b) **No Bargaining Signal**



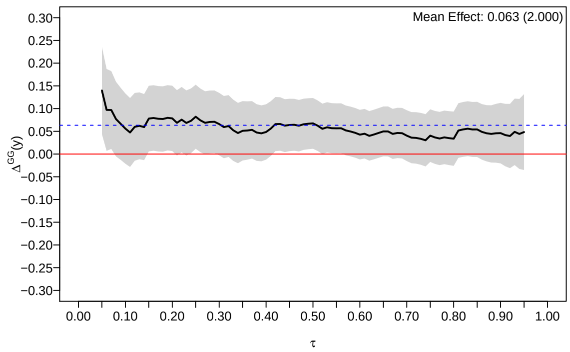
(c) **Immediately Available**



(d) **Not Immediately Available**

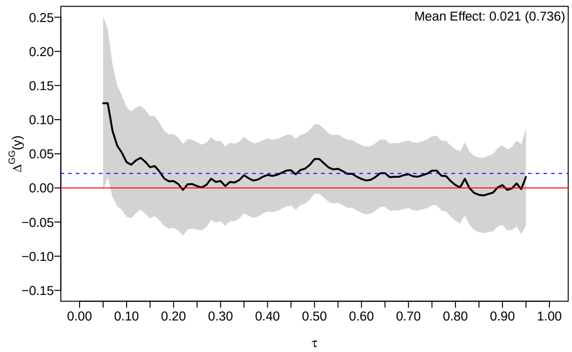


(e) **Immediately Filled**

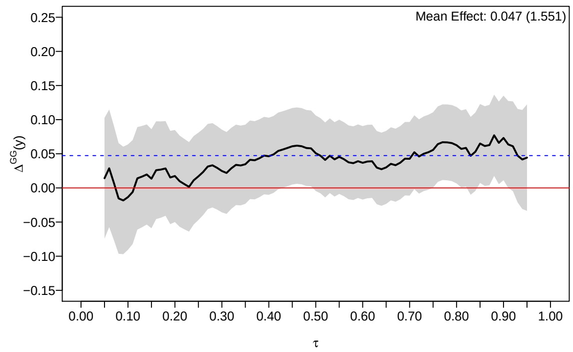


(f) **Not Immediately Filled - Men**

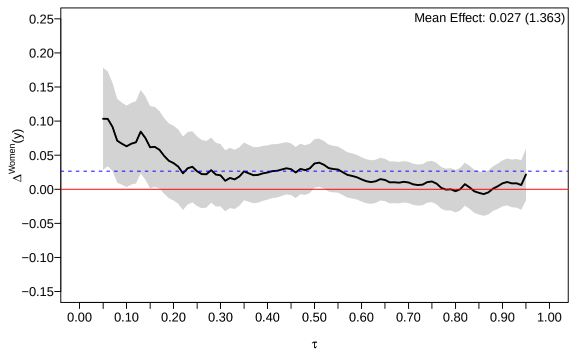
Figure 11: Results by Labor Market Experience



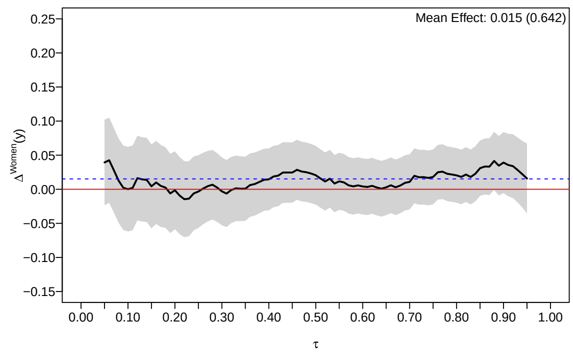
(a) Below Median - Gender Gap



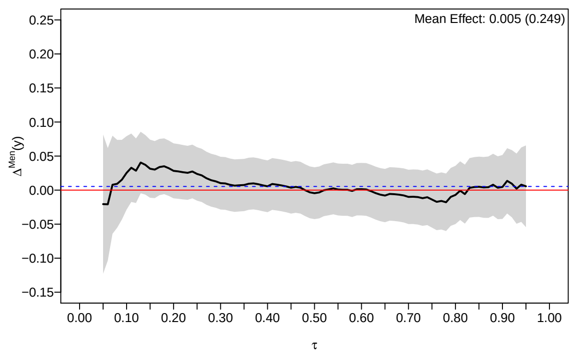
(b) Above Median - Gender Gap



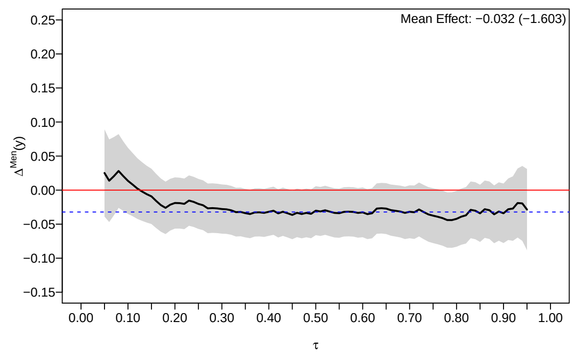
(c) Below Median - Women



(d) Above Median - Women



(e) Below Median - Men



(f) Above Median - Men

7 Tables (to be placed in the article)

Table 1: Descriptive Statistics

	Ø Full Sample	Ø Control	Ø Treatment	Diff.	p-value
Person Characteristics					
Female	0.614	0.619	0.610	-0.009	0.283
Age at Vacancy	35.327	35.273	35.385	0.113	0.550
Foreigner	0.183	0.170	0.197	0.026***	0.000
No. of Children	0.839	0.853	0.824	-0.029	0.142
Labour Market Outcomes					
Experience in Years	9.788	9.935	9.634	-0.301**	0.035
Unemployment Duration in Months	3.094	3.224	2.958	-0.266***	0.000
Real Daily Wage Excl. Special Payments	49.737	50.186	49.265	-0.920***	0.002
Contract Characteristics					
White Collar	0.446	0.459	0.433	-0.026***	0.002
Ad for Full-Time Job	0.618	0.611	0.626	0.015*	0.077
Job is Immediately Available	0.354	0.371	0.336	-0.034***	0.000
Occupations					
Production	0.282	0.273	0.291	0.018**	0.025
Retail	0.296	0.301	0.292	-0.009	0.264
Services, Education & Health	0.307	0.306	0.308	0.002	0.851
Office	0.115	0.120	0.110	-0.010*	0.066
Industry					
Goods Production	0.251	0.248	0.254	0.006	0.415
Retail	0.394	0.402	0.386	-0.016*	0.068
Services	0.167	0.157	0.178	0.021***	0.001
Health, Education & Public Administration	0.140	0.149	0.130	-0.020***	0.001
Other Industries	0.048	0.044	0.052	0.008**	0.035
Firm Location					
Eastern Austria	0.418	0.430	0.405	-0.026***	0.003
Southern Austria	0.196	0.188	0.205	0.017**	0.012
Western Austria	0.386	0.382	0.390	0.008	0.333

Note — p-values are based on robust standard errors. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Linear Difference-in-Difference Estimates

	Bargaining Signal		Job Immediately Available		Job Immediately Filled	
	Yes	No	Yes	No	Yes	No
Treatment × Post × Female	0.044*	-0.022	0.046**	-0.014	-0.026	0.047**
	(0.024)	(0.023)	(0.022)	(0.022)	(0.033)	(0.021)
Treatment × Post	-0.020	-0.004	-0.035**	0.004	-0.000	-0.024
	(0.019)	(0.024)	(0.016)	(0.022)	(0.029)	(0.015)
Covariates:						
White Collar	Y	Y	Y	Y	Y	Y
Vacancy for Full Time	Y	Y	Y	Y	Y	Y
Age	Y	Y	Y	Y	Y	Y
Age^2	Y	Y	Y	Y	Y	Y
Experience in Years	Y	Y	Y	Y	Y	Y
Unemployment Duration in Months	Y	Y	Y	Y	Y	Y
Number of Children	Y	Y	Y	Y	Y	Y
Foreigner	Y	Y	Y	Y	Y	Y
Fixed Effects:						
Occupation FE	Y	Y	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y	Y	Y
Federal State FE	Y	Y	Y	Y	Y	Y
Month of Year FE	Y	Y	Y	Y	Y	Y
Constant	Y	Y	Y	Y	Y	Y
N	5,380	7,740	4,639	8,478	7,100	6,019
Adj. R^2	0.442	0.495	0.486	0.459	0.447	0.499

Note — Standard errors are clustered on the occupation level (4-digit level) and given in parenthesis below the coefficients. All covariates and fixed effects are interacted with the female binary indicator. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Changes in Other Variables

Outcome	Treatment × Post	Treatment	Post	N	Outcome Mean
Person Characteristics					
Age at Vacancy	-0.630*	0.360	0.301	13,131	35.327
	(0.377)	(0.372)	(0.273)		
Foreigner	-0.029*	0.038***	0.022*	13,131	0.183
	(0.017)	(0.014)	(0.011)		
Academic	-0.000	0.002	0.000	13,131	0.005
	(0.002)	(0.001)	(0.001)		
Female	0.006	0.002	-0.023**	13,131	0.614
	(0.012)	(0.007)	(0.010)		
No. of Children	0.033	-0.051**	-0.034	13,131	0.839
	(0.028)	(0.024)	(0.023)		
Age of Firstborn at Vacancy	-0.091	-0.099	0.065	5,805	14.927
	(0.376)	(0.351)	(0.207)		
Parental Leave Returner	0.001	0.003	0.004	13,131	0.040
	(0.007)	(0.005)	(0.004)		
Experience in Years	0.191	-0.441	-0.190	13,131	9.788
	(0.391)	(0.347)	(0.279)		
Durations					
Filling Time (Rel. to Posting)	-1.256	3.880	-0.533	13,131	45.602
	(2.183)	(4.095)	(2.052)		

Table 3: Changes in Other Variables (Cont.)

Outcome	Treatment × Post	Treatment	Post	N	Outcome Mean
Filling Time (Rel. to Available)	0.897	-0.890	-5.508**	13,131	9.533
	(3.131)	(1.349)	(2.576)		
Unemployment Duration	0.201*	-0.298***	-0.073	13,131	3.094
	(0.108)	(0.091)	(0.112)		
Days Since Last Employment	-7.073	7.063	-11.281	12,791	258.083
	(16.084)	(9.759)	(12.335)		
Job Immediately Available	0.035*	-0.063***	-0.035**	13,123	0.354
	(0.020)	(0.012)	(0.017)		
Tenure in Months	-1.525	-0.383	0.489	13,131	20.949
	(1.289)	(0.618)	(1.147)		
Commuting					
Commuter	-0.012	0.008	0.006	13,131	0.806
	(0.013)	(0.016)	(0.010)		
Commuting Duration (Min.)	0.674	0.048	-0.489	12,856	19.707
	(0.700)	(0.675)	(0.747)		
Commuting Distance (km)	1.211	-0.001	-0.584	12,856	18.828
	(1.164)	(0.845)	(1.108)		
Moved Residence	0.007	-0.009	0.001	12,743	0.131
	(0.014)	(0.010)	(0.013)		
Moving Duration (Min.)	0.729	-0.165	0.078	12,674	5.731
	(0.794)	(0.751)	(0.671)		
Moving Distance (km)	0.921	-0.055	0.125	12,674	6.890
	(1.082)	(1.040)	(0.889)		
Firm Characteristics					
Log Firm Size	-0.049	0.025	0.088	13,081	4.617
	(0.073)	(0.037)	(0.074)		
Firm Age	-0.048	-0.117	-0.151	13,131	18.669
	(0.846)	(0.695)	(0.517)		
% Female Employees	0.397	-0.335	-0.757	13,081	61.043
	(0.727)	(0.438)	(0.588)		
Ø Staff Age	-0.048	0.198	0.064	13,081	38.344
	(0.164)	(0.143)	(0.111)		
% Austrian Employees	0.211	-1.116***	-0.646**	13,081	74.366
	(0.673)	(0.362)	(0.323)		

Note — Linear difference-in-difference estimates. The regression additionally includes occupation, industry and federal state fixed effects. Standard errors are given in parentheses and are clustered on the occupation level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Changes in Gender Differences in Other Variables

Outcome	Treatment × Post × Female	Treatment × Post	N	Outcome Mean
Person Characteristics				
Age at Vacancy	-0.428	-0.304	13,131	35.327
	(0.702)	(0.646)		
Foreigner	-0.036	-0.009	13,131	0.183
	(0.026)	(0.015)		
Academic	-0.003	0.001	13,131	0.005
	(0.004)	(0.004)		
No. of Children	0.010	0.028	13,131	0.839

Table 4: Changes in Gender Differences in Other Variables (Cont.)

Outcome	Treatment × Post × Female	Treatment × Post	N	Outcome Mean
	(0.061)	(0.045)		
Age of Firstborn at Vacancy	-0.713	0.458	5,803	14.927
	(0.997)	(0.866)		
Parental Leave Returner	0.007	-0.003	13,131	0.040
	(0.013)	(0.004)		
Experience in Years	-0.005	0.251	13,131	9.788
	(0.619)	(0.652)		
Durations				
Filling Time (Rel. to Posting)	-2.676	0.671	13,131	45.602
	(3.996)	(3.846)		
Filling Time (Rel. to Available)	6.383	-2.852	13,131	9.533
	(4.014)	(2.963)		
Unemployment Duration	-0.038	0.225	13,131	3.094
	(0.189)	(0.169)		
Days Since Last Employment	24.282	-22.286	12,791	258.083
	(29.078)	(19.940)		
Job Immediately Available	0.039	0.010	13,123	0.354
	(0.036)	(0.038)		
Tenure in Months	0.145	-1.517	13,131	20.949
	(2.925)	(2.481)		
Commuting				
Commuter	-0.002	-0.010	13,131	0.806
	(0.029)	(0.022)		
Commuting Duration (Min.)	0.813	0.250	12,856	19.707
	(2.191)	(1.177)		
Commuting Distance (km)	1.757	0.230	12,856	18.828
	(3.331)	(1.780)		
Moved Residence	-0.006	0.010	12,743	0.131
	(0.018)	(0.017)		
Moving Duration (Min.)	0.197	0.651	12,674	5.731
	(1.519)	(1.342)		
Moving Distance (km)	-0.072	1.043	12,674	6.890
	(2.102)	(1.824)		
Firm Characteristics				
Log Firm Size	-0.203	0.076	13,081	4.617
	(0.161)	(0.133)		
Firm Age	-1.313	0.741	13,131	18.669
	(1.507)	(1.483)		
% Female Employees	3.305***	-1.772***	13,081	61.043
	(1.060)	(0.686)		
Ø Staff Age	-0.468	0.257	13,081	38.344
	(0.315)	(0.271)		
% Austrian Employees	1.490	-0.745	13,081	74.366
	(1.083)	(1.027)		

Note — Linear difference-in-difference estimates where all covariates were interacted with a binary indicator for women. The coefficient of the term Treatment × Post × Female can therefore be interpreted as the effect on the gender difference and the coefficient of the term Treatment × Post is the estimated effect for men. The regression additionally includes occupation, industry and federal state fixed effects. Standard errors are given in parentheses and are clustered on the occupation level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Web Appendix

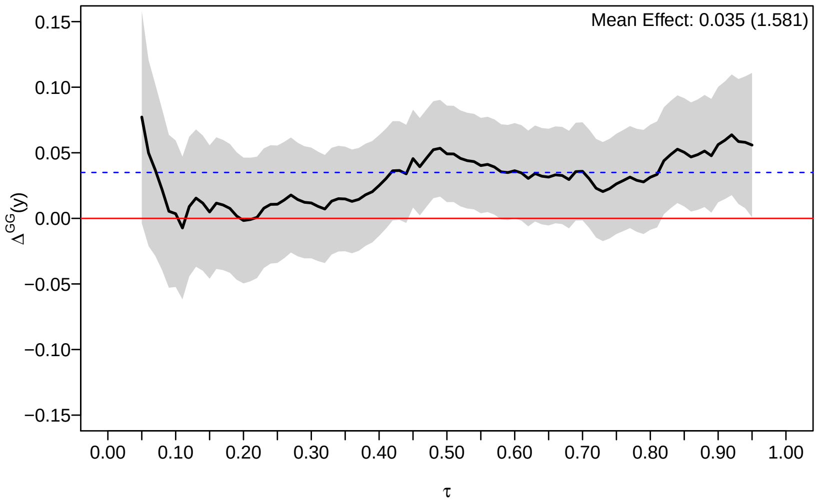
This Web Appendix (not for publication) provides additional material discussed in "Mandatory Wage Posting and the Gender Wage Gap" by Wolfgang Frimmel, Bernhard Schmidpeter, Rene Wiesinger and Rudolf Winter-Ebmer.

Table A.1: Descriptive Statistics by Job Availability

	Ø Full Sample	Ø Not Imm. Avail.	Ø Imm. Avail.	Diff.	p-value
Person Characteristics					
Female	0.614	0.620	0.604	-0.016*	0.073
Age at Vacancy	35.328	34.896	36.118	1.223***	0.000
Foreigner	0.183	0.164	0.219	0.055***	0.000
No. of Children	0.839	0.811	0.889	0.078***	0.000
Labour Market Outcomes					
Experience in Years	9.790	9.785	9.798	0.013	0.929
Unemployment Duration in Months	3.093	3.302	2.711	-0.591***	0.000
Real Daily Wage Excl. Special Payments	49.736	50.546	48.258	-2.288***	0.000
Contract Characteristics					
White Collar	0.446	0.488	0.370	-0.118***	0.000
Ad for Full-Time Job	0.618	0.607	0.638	0.030***	0.001
Occupations					
Production	0.281	0.276	0.292	0.016*	0.059
Retail	0.296	0.299	0.292	-0.006	0.441
Services, Education & Health	0.307	0.290	0.337	0.047***	0.000
Office	0.115	0.135	0.079	-0.056***	0.000
Industry					
Goods Production	0.251	0.247	0.258	0.012	0.138
Retail	0.394	0.416	0.354	-0.062***	0.000
Services	0.167	0.117	0.259	0.142***	0.000
Health, Education & Public Administration	0.140	0.173	0.079	-0.094***	0.000
Other Industries	0.048	0.047	0.050	0.003	0.495
Firm Location					
Eastern Austria	0.418	0.476	0.312	-0.164***	0.000
Southern Austria	0.196	0.168	0.248	0.081***	0.000
Western Austria	0.386	0.357	0.439	0.083***	0.000
Required Education					
Compulsory Education	0.548	0.491	0.651	0.161***	0.000
Apprenticeship/Middle Educ.	0.408	0.456	0.321	-0.135***	0.000
Tertiary Education	0.044	0.053	0.027	-0.026***	0.000
Job Characteristics					
Problem Solving	0.041	0.044	0.035	-0.009***	0.009
Social Skills	0.170	0.201	0.114	-0.087***	0.000
Character Traits	0.395	0.417	0.355	-0.062***	0.000
Writing	0.021	0.025	0.014	-0.011***	0.000
Customer Service	0.302	0.337	0.239	-0.099***	0.000
Manage Staff	0.033	0.035	0.029	-0.006*	0.061
Finance	0.082	0.104	0.042	-0.062***	0.000
IT Skills	0.092	0.106	0.066	-0.040***	0.000
Project Management	0.020	0.022	0.017	-0.005**	0.038
Routinisation					
No Information	0.516	0.473	0.594	0.122***	0.000
Yes	0.779	0.760	0.822	0.062***	0.000
Manual Labor					
No Information	0.610	0.574	0.675	0.101***	0.000
Yes	0.672	0.646	0.734	0.089***	0.000

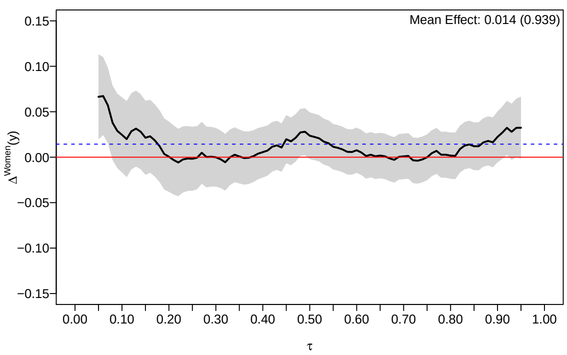
Note — p-values are based on robust standard errors. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure A.1: Main Results for Gender Gap

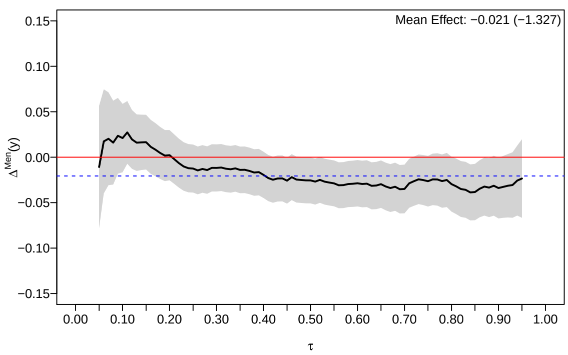


Note — Estimated quantile treatment effects for the gender wage gap, which is obtained by subtracting the treatment effect on men's log daily wages from the effect on women's log daily wages. The displayed coefficients can therefore be interpreted as the percentage point change in the gender wage gap. The horizontal axis denotes the percentiles τ of the entire wage distribution. The black solid line represents the point estimates. The gray area represents the 95 % confidence interval. Effects are only displayed for $\tau \in [0.05, 0.95]$. The blue dashed line marks the estimated mean effect. It is also reported in the top right corner along with corresponding t-statistic in parentheses.

Figure A.2: Main Results by Gender



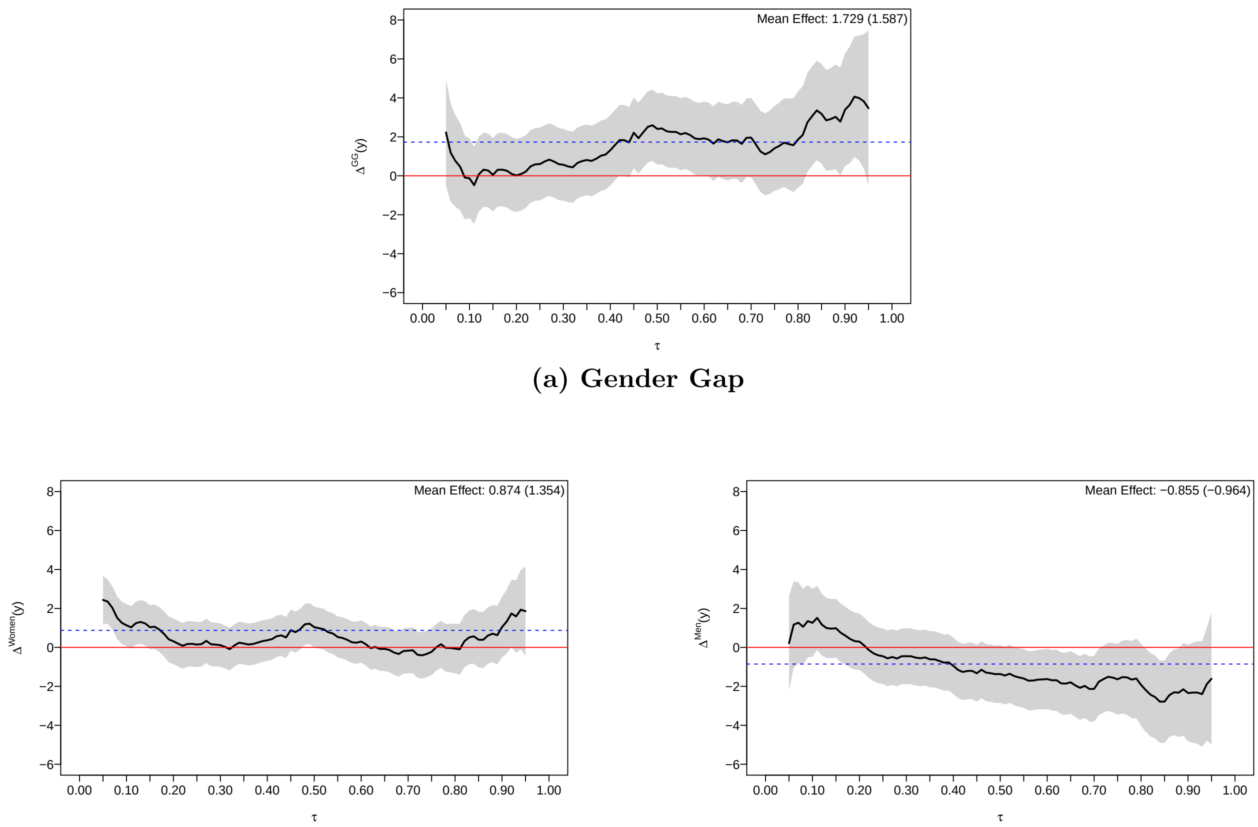
(a) Women



(b) Men

Note — Estimated quantile treatment effects for log daily wages by gender. Panel (a) displays the results for women, Panel (b) those for men. The displayed coefficients can be interpreted as the % change in the respective group's daily wages. The horizontal axis denotes the percentiles τ of the entire wage distribution. The black solid line represents the point estimates. The gray area represents the 95 % confidence interval. Effects are only displayed for $\tau \in [0.05, 0.95]$. The blue dashed line marks the estimated mean effect. It is also reported in the top right corner along with corresponding t-statistic in parentheses.

Figure A.3: Main Results in Levels

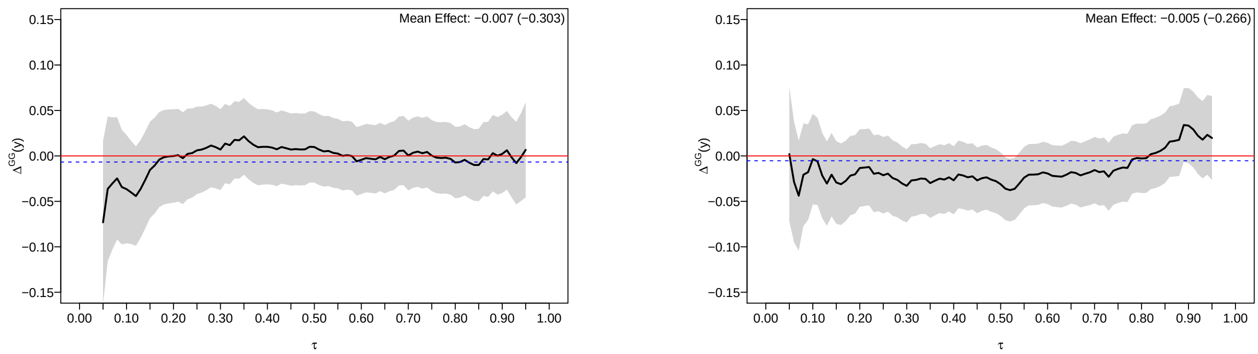


(a) Gender Gap

(b) Women **(c) Men**

Note — Estimated quantile treatment effects for daily wages measured in 2012 euros. Panel (a) displays the result for the gender gap, Panels (b) & (c) those for women and men respectively. The displayed coefficients can be interpreted as the change in the gender gap/wages in € per day. The horizontal axis denotes the percentiles τ of the entire wage distribution. The black solid line represents the point estimates. The gray area represents the 95 % confidence interval. Effects are only displayed for $\tau \in [0.05, 0.95]$. The blue dashed line marks the estimated mean effect. It is also reported in the top right corner along with corresponding t-statistic in parentheses.

Figure A.4: Placebo Checks



(a) Sample -1 **(b) Sample -2**

Note — Estimated quantile treatment effects for the gender wage gap, which is obtained by subtracting the treatment effect on men's log daily wages from the effect on women's log daily wages. The displayed coefficients can therefore be interpreted as the percentage point change in the gender wage gap. The estimates are based on two placebo samples that contain only untreated observations. The horizontal axis denotes the percentiles τ of the entire wage distribution. The black solid line represents the point estimates. The gray area represents the 95 % confidence interval. Effects are only displayed for $\tau \in [0.05, 0.95]$. The blue dashed line marks the estimated mean effect. It is also reported in the top right corner along with corresponding t-statistic in parentheses.