External Pay Transparency and the Gender Wage Gap

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Abstract

We show that providing publicly available wage information in vacancies, so-called external pay transparency, can reduce the gender wage gap. There is an increasing interest in pay transparency policies as a tool to combat unequal pay. We exploit a reform of Austria's Equal Treatment Law to evaluate how providing wage information in vacancies affects the gender wage gap. To take into account that the value of providing such external pay information is likely to be heterogeneous along the wage distribution, we implement a Quantile Difference-in-Difference model. The reform led to a small overall reduction of the gender wage gap. Our main results highlight that reductions in the wage gap are larger in circumstances where women are likely to hold misspecified beliefs about their labor market options and when needing to make job acceptance decisions under pressure. The reduction in the gender wage gap was caused by an increase in women's earnings, particularly at the lower part of the distribution. Earnings of men, on the other side, remained largely constant. Our results lend support to policy proposals aimed at increasing external pay transparency.

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1 Introduction

Over the past years, many countries have introduced pay transparency measures to close the gender pay gap (see e.g. Cullen 2023 for an extensive discussion). Despite their popularity, it is still an open question whether these policies work as intended. For example, internal wage transparency, where workers can obtain information about their co-workers' pay, either has had only limited impact on reducing gender pay gaps or has led to a fall in wages.[1] Recently, there has been an increasing interest in policies demanding pay transparency already prior to the hiring stage. Many job postings come without any wage information and there is evidence that many applicants have no precise knowledge about the pay before their first interview (Hall and Krueger, 2012).[2] An increasing number of U.S. states and cities, such as Colorado, California, New York City, and Washington, as well as European countries like Austria, Slovakia, Latvia, and Lithuania have introduced laws mandating firms to provide explicit wage information in their job advertisements.[3] Providing wage information during the initial application stage may affect workers' beliefs about their pay and labor market options, and therefore their wage bargaining, which ultimately has an impact on the gender wage gap. Despite the importance and the increasing interest in such external pay transparency policies, evidence of their impact on workers' wages and the gender wage gap is still scant.

In this paper, we evaluate how mandating firms to provide wage information in job vacancies affects workers' wages and the gender pay gap, exploiting a reform of Austria's Equal Treatment Law and using a unique matched vacancy-employer-employee data set. The pay transparency law requires that all vacancies posted with private or public employment agencies after March 1, 2011 have to include (i) a posted wage and, unique to the Austrian pay transparency law, (ii) a reference to whether the firm is willing to overpay. The stated willingness to overpay is often connected to qualification and experience.

Requiring wage information in vacancies makes the employer's willingness to pay and the value of outside options more salient to job applicants. In addition, by requiring a statement about the willingness to overpay, the law also implicitly reveals the lower bound of returns to specific characteristics (e.g. experience) in possible wage negotiations. This news content in pay information is likely to be positive and large for workers with inaccurate beliefs about wages and their labor market options, such as those in lower paying jobs and women (see e.g. Cullen

[1] Böheim and Gust (2021) and Gulyas et al. (2023) evaluate such an internal pay transparency reform in Austria and find neither an effect on the gender pay gap nor on overall wages. In contrast, Bennedsen et al. (2022) for Denmark, Duchini et al. (2020) and Blundell (2021) for the UK, Mas (2017) and Obloj and Zenger (2022) for the US, and Baker et al. (2023) for Canada all find that internal pay transparency can reduce the gender pay gap. These works also find that the reduction is associated with a negative effect on wages. Cullen and Pakzard-Hurson (2023) suggest a model with bargaining under incomplete information which can reconcile these heterogeneous effects.

[2] In the US, around 20 to 40% of all job postings contain wage information (Marinescu and Wolthoff, 2020; Arnold et al., 2022). Similarly, in Slovakia, on average 20% of job ads contained information about pay prior to the introduction of a pay transparency policy (Škoda, 2022). As we will show later, in Austria less than 10% of job advertisements provided any information about pay before the introduction of the Equal Treatment Law.

[3] Cullen (2023) calls this external pay transparency.

and Perez-Truglia 2018, Glassdoor 2016, and Jäger et al. 2022). As more information about pay and negotiation possibilities at various job opportunities becomes available, workers with inaccurate beliefs about their labor market options update their beliefs.[4] As a consequence, they are also more motivated to negotiate or look for better jobs elsewhere. Providing workers with information about wages and whether wages are negotiable can therefore lower gender differences in pay (see also Bowles et al. 2005, Mazei et al. 2015, Leibbrandt and List 2015, as well as Biasi and Sarsons 2022 and Roussille 2021).[5]

While external pay transparency measures have the potential to reduce gender wage gaps, this may not necessarily lead to a reduction in general wages. Observing posted wages in other firms, workers can use this information to ask their (potential) employers to renegotiate wages. If the employer refuses, workers can credibly threaten to search for a better match elsewhere. If hiring is costly and there is a positive match surplus to be shared, firms will increase wages (or at least leave them constant). This stands in contrast to the impact of internal pay transparency measures, where providing more (internal) information can reduce wages by shifting bargaining power toward firms (Cullen and Pakzard-Hurson, 2023).

To study the impact of the external pay transparency law on wages and the gender pay gap, we use linked vacancy-employer-employee data containing information about (i) the characteristics of the posted vacancies including the posted wages and the advertisement texts, (ii) the establishments posting the vacancies, and (iii) the labor market histories of the workers finally filling the vacancies. The vacancy data are from the Austrian public employment service (AMS) and contain all vacancies posted through the AMS. The AMS is one of the most important vacancy platforms, covering the majority of all postings. It also captures the universe of all vacancies posted in Austria reasonably well (see also Mueller et al., forthcoming). For all vacancies filled through the mediation of the AMS, the data can be linked to the labor market histories of the workers finally filling the vacancies through an anonymized identifier. We are therefore able to investigate the impact of pay transparency at the worker level.

Using the AMS vacancy data, we first show that firms indeed complied with the new wage posting rule. Prior to the introduction of the reform, less than 5% of posted vacancies contained any wage information. This increased from 20% to 40% during a transition period lasting from the beginning of March 2011 until the end of June 2011. From the end of June 2011 onward, the AMS required all postings to comply with the pay transparency law. As a consequence, we observe posted wages for almost all vacancies posted after this point in time (see Figure 1).[6]

[4]For example, Jäger et al. (2022) show that workers incorrectly anchor their beliefs about outside options in their current wages (see Winter-Ebmer 1998 for an early study). This implies that workers with lower pay should benefit more from external transparency laws. Schmidpeter (2023) shows that publicly available information about job opportunities leads individuals to update their expectations about outside offers and wage growth in the current job.

[5]Posting wages can increase the number of applicants for a position and affect where people apply (e.g. Belot et al., 2022; Škoda, 2022). Our data at hand does not allow us to investigate application behavior in detail. We do not observe any difference in characteristics of workers filling the vacancy post and prior to introduction of the privacy law. But, as we discuss later, we find evidence for efficiency improvement in the search process.

[6]In general, there was a transition period until January 2012, during which violations were not sanctioned.

This near universal compliance also implies that selection into what type of firms decide to post wages is likely not a concern in our analysis.[7]

The above discussion implies that providing pay information is more valuable for workers who are less informed about their true labor market options, such as those in the lower part of the wage distribution. To capture this heterogeneity, we estimate the impact of the Austrian external pay transparency law on the whole distribution of wages, implementing an extended Quantile Difference-in-Difference (QDiD) estimator (Callaway et al., 2018; Callaway and Li, 2019). The QDID estimator allows to capture the effect of providing external pay information along the entire wage distribution, like in standard quantile regressions. At the same time, we can account for unobserved but time-variant characteristics affecting firm-worker matching and therefore wages, similar as in linear Difference-in-Difference models.

Using the linked vacancy-employer-employee data, we find evidence that the introduction of the external pay transparency law in Austria decreased the gender pay gap somewhat, but the reduction is statistically insignificant for most parts of the distribution. The reduction is driven by women earning more after the pay transparency law is introduced, while men tend to earn slightly less. Nevertheless, we see our baseline estimates as encouraging as they show that external pay transparency has the potential of reducing gender wage gaps without affecting general wages.

Given the positive but noisy baseline estimates, we consider different scenarios where pay information is potentially more valuable. We first analyze situations where firms explicitly specify their willingness to overpay in the job posting, which we interpret as a bargaining signal.[8] Postings which include such a bargaining signal are more likely to be found in "male-dominated" jobs like blue-collar production occupations. It is therefore possible that women were both not well informed about the prevailing wages in such positions and also held misspecified beliefs about specific returns to their qualifications and labor market experiences. Beliefs may be misspecified, for example, as women do not have strong co-worker networks in such occupations. Such networks are important, however, to transmit information about new job opportunities and pay, and ultimately affect wage negotiations (e.g. Caldwell and Harmon, 2019; Laffèrs and Schmidpeter, 2022).

We find that in this scenario, the introduction of the external pay transparency law indeed led to a significant reduction of the gender wage gap in the lower half of the wage distribution. Our estimated effects are quite substantial. For example, external pay transparency together

Afterward, firms violating the law can be fined up to €360.

[7]The near universal compliance is also different to the setting in Škoda (2022) and Arnold et al. (2022) who study pay transparency laws in Slovakia and Colorado, respectively. Škoda (2022) observes wage information for around 80% of all postings after the law, citing data issues as likely explanation for the less-than-perfect compliance in the data. Arnold et al. (2022) observe for roughly 70% of postings wage information after the law.

[8]Notice that we are agnostic to why firms decide to specify whether they are willing to overpay in our work. Schmidpeter and Tô (2023) show, however, that firms with negotiable wage postings tend to be more selective but also more efficient in their hiring process than firms with non-negotiable postings.

with the bargaining signal lowered the gender wage gap by around 10 percentage points at the lower tercile. The impact is decreasing at the upper part of the wage distribution. Our results show that the reduction in the gender pay gap is caused by a wage increase for women. Mirroring the impact of our estimates for the gender wage gap, we estimate large and highly significant effects at the lower part of the distribution. Consistent with our conjecture that higher paying individuals are better informed about their labor market opportunities, we estimate a fade out of the effect at the upper part of the wage distribution. For men, in contrast, we do not find that the introduction of the pay transparency law led to a significant reduction in wages. Moreover, we do not find the same effects in cases where no bargaining signal was provided in the posting.

Next, we consider situations where job offers are likely to have relatively short deadlines or where exploding offers may occur. Holding misspecified beliefs about pay and labor market options in such high-pressure scenarios may amplify gender wage differences. Our analysis is motivated by the findings in Cortés et al. (forthcoming) who show that female college graduates accept job offers earlier than men and that these jobs pay less. Women accept offers too early as they tend to be both more risk averse than men and hold less optimistic beliefs about future job offers. Providing external information may lead to a correction of these beliefs and, therefore to a reassessment of what type of job offer is deemed acceptable. To capture such a scenario with short deadlines, we use vacancies where the desired start date lies within the next seven days of the posting date.

Like in the case of bargaining signals, we find that providing external pay information is valuable for women who face short offer deadlines. The introduction of the law decreased the gender wage gap substantially, with the largest reductions occurring again at the lower part of the income distribution. Interestingly, we now also find that women at the upper part of the earnings distribution benefited from pay transparency. Interpreting these results through the lens of the job search model of Cortés et al. (forthcoming), which incorporates gender differences in overconfidence and risk aversion, they imply that providing women with pay information leads to updating of beliefs about future job offers and, therefore, whether the current offer is acceptable.

We provide additional evidence that the positive impact of external pay transparency on women's earnings is indeed caused by the reduction of information asymmetries about pay and jobs, and not by better firm-worker matches.[9] The introduction of the pay transparency law did not change the posting and hiring behavior of firms or the characteristics of the hired workers. For example, the law had neither an effect on the composition of firms' posting vacancies nor on the required qualifications mentioned in the ads. There is also no change in observable characteristics of workers filling the vacancies, such as previous labor market experience and

[9]In concurrent work, Bamieh and Ziegler (2022) also analyze the Austrian pay transparency law, but focus on sorting into occupations as a mechanism, rather than the gender wage gap. As we, they do not find any impact on firm-worker sorting.

education, after the law.

We do find, however, that the law improved search efficiency. Open positions were filled slightly faster after the introduction of the law, although our estimates are rather imprecise. These results are in line with those in Škoda (2022) for Slovakia. Being able to track the interests of potential job applicants, the author finds that the introduction of the external pay transparency law increased the number of clicks received by ads with posted wages. Similar to us, he also does not find that the introduction of the law has led to positive selection effects.

Our work is related to two main strands of literature. First, we contribute to the growing literature on the impact of pay transparency laws on wages and gender gaps. Many of these studies focus on internal pay transparency laws, where firm-level wage information is only observable for employees, and find either no effect on pay inequality or an associated fall in wages (see the above citations and Bennedsen et al. 2023 as well as Cullen 2023 for an overview). In contrast, only a small number of studies have investigated the impact of external pay transparency laws, where pay information becomes publicly available (Arnold et al., 2022; Bamieh and Ziegler, 2022; Škoda, 2022). These studies find in general a positive impact on wages but a muted impact on the gender wage gap. We show that external pay transparency laws can improve women's wages and lower the gender wage gap in situations where women are likely to hold misspecified beliefs about their labor market options or where they have to make decisions under pressure, such as when receiving exploding offers. There is no evidence that these gains come at the cost of general wage compression and a fall of wages for men. We provide additional evidence that the new information content created by the introduction of the law is indeed the main driver of our findings and not changes in firms' posting behavior or firm-worker sorting. The latter aspect is also found in Škoda (2022) and Bamieh and Ziegler (2022).

Second, we also contribute to the increasing literature on how (misspecified) beliefs about labor market outcomes can affect wages, careers, and job search (e.g. Brandl et al., 2018; Cortés et al., forthcoming; Jäger et al., 2022; Schmidpeter, 2023).[10] Persistence in beliefs and lack of updating can contribute to increasing inequality. While there exists (mostly experimental) evidence that providing individuals with relevant information can lead to belief updating, such as about gender norms (Cortés et al., 2022), the question remains how such information treatment could be scaled up. In addition, individuals do not necessarily have an incentive to act on their updated beliefs. Our results show that a simple pay transparency law, mandating firms to specify basic wage information in job postings, can improve earnings of those likely to be the least informed and reduce pay gaps. At the same time, we do not find that the introduction of the external pay transparency law has reduced general wages, a finding also confirmed for pay transparency settings in other countries (Arnold et al., 2022; Škoda, 2022).

The remainder of the paper is structured as follows: Section 2 provides more information

[10]Related to this point are also works in psychology, such as Major et al. (1984), Martin (1989), and Kaman and Hartel (1994).

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 Sections 4 and 5. Section 6 concludes the paper.

2 Institutional Background and Data Sources

2.1 The Austrian Wage Posting Law

In 2011, as a part of an Equal Treatment Law reform, Austria introduced measures to enhance pay transparency. Under the new law, all vacancies posted after March 1, 2011 must include information about the expected minimum pay. In addition, firms have to state in the posting whether they are willing to pay more than the advertised wage. These statements are usually connected to qualification and labor market experience.[11]

The wage posting law affects virtually all vacancy postings and applies to firms as well as private and public employment agencies. After an initial transition period lasting until January 2012, during which non-disclosure of wages was not sanctioned, firms violating the law have been fined up to € 360 for non-compliance. The law has been strictly enforced by private and public employment agencies. In fact, the Austrian public employment service (AMS), whose data we use in this analysis and which we describe in the next section, mandated all job postings after June 2011 to comply with the new law.[12]

Prior to the introduction of the law, less than 5% of all postings contained any information about pay (see Figure 1) and it is likely that applicants had no or incorrect beliefs about what pay to expect (see also Hall and Krueger, 2012). By requiring firms to post the minimum expected wage and an indication about the willingness to overpay, the Austrian law is likely to have increased available information about outside options. The law also implicitly reveals the lower bound of returns to specific characteristics, such as experience, in possible wage negotiations. Therefore, the introduction of the Austrian law generated *external* pay transparency, available to all market participants (see Cullen, 2023).

The new information content created is potentially more valuable for individuals who are least informed about their labor market options. The law therefore has the potential to reduce pay inequality in situations where workers hold misspecified beliefs about pay or have asymmetric information. For example, in situations where prospective employers give only short deadlines to accept job offers, such a policy can improve information about what type of offer is deemed acceptable. As women tend to accept job offers too early (Cortés et al., forthcoming), such a policy can reduce the gender wage gap. At the same time, requiring external pay transparency is likely to leave general wages unaffected. When workers observe posted wages,

[11] A typical advertisement with a stated willingness to overpay after the law would read: "*Given relevant qualifications or experience, the actual pay can be higher than stated.*"

[12] As we will discuss in the next section, the AMS job posting is representative for all job postings in Austria. We will also show that in our data, we indeed observe a near universal compliance with the law.

they can use this information to ask their (potential) employers to renegotiate wages. If a firm refuses, workers can credibly threaten to search for a better match elsewhere.

Notice that the mandatory wage posting law considered in this paper is different from the widely discussed and evaluated internal pay transparency measures. Internal pay transparency measures allow workers to obtain information about the wages of co-workers (e.g. mean wages within a certain age-experience group). There is ample evidence that internal pay transparency has no effect on wage inequality or, at the same time, reduces overall wages (Bennedsen et al., 2023; Cullen, 2023). This is because providing only internal pay information can reduce wages by shifting bargaining power toward firms (Cullen and Pakzard-Hurson, 2023). Information about pay under the law we consider is, in contrast, observable to all market participants.[13]

2.2 Data & Sample

Data Sources: We use linked vacancy-employer-employee data containing information about (i) the characteristics of the posted vacancies including the posted wages and the advertisement texts, (ii) the establishments posting the vacancies, and (iii) the labor market histories of the workers finally filling the vacancy.

The vacancy data are provided by the AMS and contain all vacancies posted through the AMS. The AMS is one of the most important vacancy platforms, covering the majority of all postings. It also captures the universe of all vacancies posted in Austria reasonably well (see Mueller et al., forthcoming). In our AMS data, we observe specific vacancy characteristics, such as the advertised wage (if one was posted) as well as the exact advertisement texts.

We use the advertisement text to determine a firm's willingness to overpay. This is what we call the *bargaining signal*. Specifically, we consider a vacancy to contain a bargaining signal if the text indicates that either the wage is subject to mutual agreement or the actual wage can be higher than the posted one. We do so by searching for specific phrases related to negotiation and overpay in the advertisement text.[14]

All vacancies filled through the mediation of the AMS can be linked to the Austrian Social Security Database (ASSD), using an anonymized person identifier.[15] The ASSD includes administrative records to verify pension claims and is 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

[13] A second amendment to the Equal Treatment Law, also requiring internal pay transparency, was enacted in July 2011. Initially, all firms with more than 1,000 employees were required to submit wage reports to employees. Firms below that threshold were exempt until the following years. For details on this reform, see Böheim and Gust (2021) and Gulyas et al. (2023). In the appendix, we show that our results are unaffected by the introduction of the external transparency law.

[14] For the first category, we search for combinations of words related to pay and negotiations. Words indicating bargaining include *nach Vereinbarung*, *nach Absprache*, *vereinbart* and *verhandelbar*. Words related to the willingness to overpay include *Überzahlung*, *Überbezahlung*, *Mehrzahlung*, *Mehrbezahlung*, *überkollektivvertraglich*, *übertariflich*, *über KV*, *über Kollektiv* or *über Kollektivvertrag*. We then combine them with words related to pay, such as, *Entlohnung*, *Gehalt*, *Lohn*, *Verdienst*, *Entgelt* and *Bezahlung*.

[15] We disregard vacancies filled by individuals who cannot be matched with the ASSD.

employer (Zweimüller et al., 2009).[16] The ASSD allows us to obtain individuals' labor market histories and their daily earnings.

Treatment and Control: To define our treatment and control groups as well as post- and pre-treatment periods we use the posting date of the vacancy as the main criterion. All vacancies posted (and the individuals filling these vacancies) in a symmetric window of six months around the introduction of the law on March 1, 2011 are in our treatment group. The six months prior to March 1, 2011 are the pre-treatment period, while the six months after March 1, 2011 constitute the post-treatment period. To define the control group, we follow the same logic but use the previous year as reference point. We now select all vacancies posted (and the individuals filling these vacancies) in a symmetric window of six month around March 1, 2010. Defining the control group in such a way allows us to account for specific seasonality in vacancy posting which can also affect pay, while also ensuring that postings are comparable between our two groups. At the same time, by choosing a window of six month we also ensure that wages of individuals in the control group were unlikely affected by the pay transparency law. Figure 2 provides an overview of the definitions of our treatment and control group.

The construction of the treatment and control group implicitly assumes that posted vacancies and individuals filling the vacancies in our control group are comparable to postings and individuals in our treatment group in the pre-treatment period. This assumption would be violated, for example, if there were specific structural and timed shocks only affecting one of our two groups. As we will show in detail in the next section and when assessing the robustness of our results, we do not find evidence suggesting that this assumption is violated. Both the control and treatment group are comparable in terms of background characteristics as well as labor market outcomes.

Our empirical strategy (outlined in detail in Section 3) requires a panel structure. Constructing the data as outlined above implies that we do not generally observe the same individual taking up the same job within the same firm before and after the law was introduced. The data set therefore is a repeated cross-section. To give the data the necessary panel structure, we categorize vacancies into fine cells based on the following observable characteristics: a 4-digit occupation code, firm industry, firm location based on federal states, collar and full- or part time jobs.[17]

Sample: Our main interest in this work is whether external pay information can structurally affect wages and pay inequality. We therefore exclude jobs and postings which are likely to be only seasonal in nature, either as defined by the AMS or by their industry, postings from temporary help and staffing agencies as well as postings for marginal employment and apprenticeships.[18] We also exclude vacancies with an unusually long filling time of more than one

[16] A drawback of the ASSD, as in many administrative data sets, is the lack of information on the number of contracted or actual hours.

[17] We only keep those cells that are observed in both the treatment and control group, before and after March 1 and for women and men.

[18] Seasonal industries are construction as well as food and accommodation (tourism). In addition, we also exclude

year.

Imposing all our restrictions, we have in total 13,131 individuals. The observations are distributed over 136 cells, implying that one cell contains on average approximately 96 observations.[19] Table 1 provides summary statistics for the entire estimation sample and separately by treatment status. In the first half of the table, we report background characteristics of individuals filling the vacancy. The majority of the vacancies in our sample are filled by prime age women, but we do not see any differences by treatment status. Individuals in our treatment group are more likely to be non-Austrian and have slightly less labor market experience. In general, however, we observe only small difference in personal characteristics and labor market outcomes between individuals in our treatment and control group.

The second half of the table reports information on the characteristics of the posted vacancies. The majority of postings are for full-time positions in the production or retail sector. The vast majority of open positions require low- to middle-education.

Finally, Table 2 summarizes pre-reform gender gaps in daily starting wages. Overall the raw unadjusted wage gap is 30 percent, which is reduced to 16.6 percent when adjusted for individual characteristics, industry, region, time and occupation fixed-effects. These raw gender gaps are larger for jobs with a bargaining signal (31.1 percent) and for immediately available jobs (34.5 percent).

3 Estimation Strategy

3.1 The Gender Gap across the Wage Distribution

We argue that external pay transparency laws are most beneficial for workers with misspecified beliefs about their labor market outcomes. The results in Jäger et al. (2022) imply that workers wrongly anchor their beliefs in their current wages. 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.

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

$$\Delta^{GG}(\tau) = \Delta^{W}(\tau) - \Delta^{M}(\tau) \tag{1}$$

where $\Delta^{W}(\tau)$ and $\Delta^{M}(\tau)$ are the estimated impacts of the mandatory wage posting law on women's and men's wages respectively at each quantile τ. Defining the effect on the gender wage gap as in Equation (1) allows us to identify potentially heterogeneous impacts of the wage posting law.

postings from firms operating in agriculture and mining, as we only observe very few postings in these industries.

[19] The smallest cell includes nine observations, while the largest cell contains 617 observations.

Under the strong assumption that the wage posting law was randomly introduced (conditional on covariates), one could obtain the counterfactual outcomes using the "standard" quantile treatment effect approach (e.g. Imbens and Wooldridge, 2009). It is likely, however, that time-invariant unobserved vacancy characteristics, such as associated occupational stress, are correlated with both the wage posting law and wages in our setting. Such a correlation would bias our estimates. To account for unobserved time-invariant characteristics, we therefore make use of the Quantile Difference-in-Difference (QDiD) approach of Callaway et al. (2018) and Callaway and Li (2019).

To define the QDiD more formally, let Y_s be wages observed in period s, where $s \in \{t-1, t\}$. Denote by D_t the 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 wage a unit would receive under the law at time s. Likewise, let $Y_s(0)$ be the potential wage absent the law at time s (see e.g. Imbens and Wooldridge 2009 for a discussion on potential outcomes). Then, we can define the impact of the wage posting law on wages at quantile τ for either women ($j = W$) or men ($j = M$) as

$$\Delta^j(\tau) = F^{-1,j}_{Y_t|D_t=1}(\tau) - F^{-1,j}_{Y_t(0)|D_t=1}(\tau) \tag{2}$$

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 (2) can be interpreted as the QDiD treatment effect, evaluating how mandatory wage posting affects wages of 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 $F^{-1,j}_{Y_t(0)|D=1}(\tau)$, however, cannot be estimated from the data. Callaway et al. (2018) show that one can identify the counterfactual outcomes using two time periods under two assumptions: (A1) Conditional Distributional Difference-in-Difference and (A2) Conditional Copula Invariance.

As we discuss in detail further below, both assumptions require that our treated vacancies are similar both in a distributional sense and in terms of how outcomes evolve over time in the absence of the wage information law. These assumptions concern the whole distribution and are therefore stronger than the usual assumptions imposed in mean Difference-in-Difference (DiD) models. We will provide some additional evidence to show that both assumptions are satisfied in our setting.

Define $\Delta Y_{vt}(0) = Y_{vt}(0) - Y_{vt-1}(0)$ as the difference in untreated potential outcomes of a filled vacancy v between time t and $t-1$.[20] The Conditional Distributional Difference-in-Difference Assumption can be stated formally as:

[20] Remember that, in practice, we define a vacancy v and therefore the unobserved time invariant vacancy fixed effect on the occupation x full-/part-time x industry x state x collar level. To make this clear we formally define our assumptions using v, noting that one vacancy can be filled by multiple individuals. Therefore, our assumptions also implicitly require that characteristics of individuals filling the vacancies do not change after the introduction of the law. We find evidence that this is the case in our setting (see Section 5).

Assumption A1: Conditional Distributional Difference-in-Difference.

$$\Delta Y_{vt}(0) \perp\!\!\!\perp D_{vt}|X_v \tag{A1}$$

Assumption (A1) requires that once we take differences, conditional on covariates, the potential outcomes if the law had not been enacted do not depend on whether a filled vacancy v belongs to the treatment or the control group. Intuitively, it extends the standard "parallel trends" assumption in mean DiD models to hold over the whole distribution. This assumption is different and stronger compared to the requirement in linear models, where the parallel trends assumption has to hold only for the mean. While not directly testable, we provide evidence that Assumption (A1) holds in our setting by estimating Equation (1) using only outcomes prior to the enactment of the law. This type of placebo test is akin to assessing pre-trends in the "standard" mean DiD setting, but concentrating on the outcome over the whole distribution.

The second assumption, the Conditional Copula Invariance, is more specific to the QDiD approach. It requires some structure on the dependence of how counterfactual outcomes change between the two time periods 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 is the same for the treatment and the control group. More intuitively, Assumption (A2) requires that changes in the outcome of control vacancies at certain parts of the distribution also happen with a similar likelihood for the potential outcomes of treated vacancies. For example, under Assumption (A2), if we observe that the largest outcome changes for control vacancies happens at the lower part of the distribution, we must also expect the largest changes to happen at the lower part of the distribution for treated units in the absence of treatment.[21] In summary, neither the treatment itself nor any structural shifts may change each vacancy's position in the wage distribution.

Callaway et al. (2018) show that under Assumptions (A1) and (A2), one can estimate the counterfactual cumulative distribution function $F^j_{Y(0)|D_t=1}(y)$ from the data as

$$F^j_{Y_t(0)|D_t=1}(y) = n^{-1}_{\mathcal{D}^C} \sum_{v\in\mathcal{D}^C} \mathbb{1}\{\Delta Y_{vt} + F^{-1,j}_{Y_{t-1}|X,D_t=1}(F^j_{Y_{t-1}|X,D_t=0}(Y_{it-1})) \leq y\} \tag{3}$$

[21] The copula invariance assumption is loosely related to rank invariance in quantile treatment effect models (see e.g. Imbens and Wooldridge 2009 for a discussion). Under the modified parallel trends assumption and the copula invariance assumption, our QDiD can be thought of as a series of DiD estimates for different quantiles.

where $\mathcal{D}^C$ is the set of control vacancies and $n_{\mathcal{D}}$ its cardinality.[22]

Under Assumptions (A1) and (A2), we can also use our distributional approach to obtain a (unconditional) DiD estimator for group j as

$$\Delta^{DiD,j} = E^j[\Delta Y_t(1)|D = 1] - E^j[\Delta Y_t(0)|D = 1] \tag{4}$$

Notice that $E^j[\Delta Y_t(1)|D = 1]$ is directly identified from the data. We estimate the missing counterfactual mean outcome $E^j[\Delta Y_t(0)|D = 1]$ for group j by[23]

$$E[Y(0)|D = 1] = n_{\mathcal{Q}}^{-1} \sum_{\tau \in \mathcal{Q}} F^{-1,j}_{Y_t(0)|D_t=1}(\tau) \tag{5}$$

where $F^{-1,j}_{Y_t(0)|D_t=1}(\tau)$ is the quantile function based on $F^{j}_{Y_t(0)|D_t=1}(y)$, as specified in Equation (3). $\mathcal{Q}$ is the set collecting all quantiles used in our estimation and $n_{\mathcal{Q}}$ its cardinality.[24] Having obtained the DiD estimator for women and men respectively, we then obtain the mean impact of external pay transparency analogous to Equation (1).

3.2 Estimation of the Gender Gap

When estimating Equation (3), we impose an additional assumption on the conditional quantile function. Specifically, we assume that it is linear in parameters β.

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 (3). Specifically, under Assumption (A3) we can obtain the missing quantities on the right-hand side of Equation (3) by using predictions from linear quantile regressions. To obtain the counterfactual $F^{-1,j}_{Y_t(0)|D_t=1}(\tau)$ in Equation (2), we invert the estimate of $\widehat{F}^{j}_{Y_t(0)|D_t=1}(y)$.

One additional challenge arises in our setting, as it is possible that one posted vacancies v can be filled by more than one worker within a time period. The QDiD approach, however, uses each vacancy v exactly once in time t and once in time $t-1$.

To incorporate that we observe more than one individual filling a vacancy v in our approach, we do the following: First, we randomly draw one individual for each filled vacancy v for both

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

[23] Note that our DiD estimator here is based on stronger assumptions compared to the "standard" case. For comparability, we base our DiD estimates on the QDiD approach.

[24] In practice, we estimate our counterfactuals over a fine grid of quantiles (see also the next section). Also note that when obtaining our DiD estimator, we allow for selection on covariates in our first step and then integrate over the distribution of covariates in a second step to obtain unconditional estimates (see e.g. Abadie 2005 for a similar, semi-parametric approach).

our treatment and control group. We do this separately for women and men. This gives us a balanced sample where each vacancy is observed once before and once after the introduction of the law. Then, for each vacancy in our randomly drawn sample r, we obtain additional background characteristics on the individual filling the vacancy, such as whether the person is Austrian, age, experience, unemployment duration and the number of children.[25] We also obtain the wage at time t and at time $t-1$. Then, for the sample r, we calculate the treatment effect for both women and men as in Equation (2) as well as the effect on the gender gap $\widehat{\Delta}^{GG,r}$ as specified in in Equation (1).

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 women's and men's wages as well as R different estimates for the effect on the gender gap. We then average over all R simulated values to obtain estimates for $\widehat{\Delta}^{M}(\tau)$, $\widehat{\Delta}^{W}(\tau)$, and $\widehat{\Delta}^{GG}$. 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 $\widehat{\Delta}^{W,r}(\tau)$.

In practice we set R to 500. We base inference on the bootstrap using 500 replications. To be more precise, within each bootstrap replication we conduct our simulation approach 500 times as discussed above.[26]

4 Estimation Results

4.1 Main Results

Overall Effects: We first estimate the impact of the pay transparency law on women's and men's log daily wages and the gender gap using the full sample. The results of this analysis are shown in Figure 3. Panel (a) shows the impact on the gender gap, as defined in Equation (1). Panels (b) and (c) show the effect of the wage posting law on log daily wages for women and men respectively.

We find suggestive evidence that the introduction of the pay transparency law reduced the gender pay gap mostly at the lower and the upper part of the wage distribution, as well as around the median. Our estimates are rather noisy, however. This can also be seen from the (unconditional) DiD estimates, presented by the dashed line and shown in the upper right corner of each graph. While the point estimate is positive, we also estimate a relatively large standard error.

Looking at the results separately for women and men in Panels (b) and (c), we see that the law led to a slight, but imprecisely estimated, increase of wages for women at the same part of the distribution where we observe a narrowing of the gender gap. In contrast, for men we

[25]In the estimation, we also include a quadratic term in age as well as month of year fixed-effects.
[26]Our results remain virtually unchanged when increasing R beyond 500 replications.

estimate a downward shift in log daily wages. But, again, these estimates are rather noisy.

Overall, the results imply that the general impact of requiring firms to provide pay information is not a priori clear from both a theoretical (Cullen and Pakzard-Hurson, 2023) and an empirical point of view. Unlike results from internal transparency measures (see Cullen 2023 for an overview), our results are encouraging in that they show that external pay transparency can potentially reduce gender gaps for women at the lower end of the wage distribution, while leaving men's wages unaffected.

These results also imply that posting publicly available wage information may help those that are likely to be least informed about their possible outside options in the labor market. Therefore, in a next step, we investigate how possible bargaining signals in job advertisement together with wage postings can affect wages.[27]

Bargaining Signal: As described in Section 2, we use firms' wording in the advertisement texts to deduce their potential willingness to negotiate over wages. We use these signals as a potential additional source of information for applicants. For example, some workers may have misspecified beliefs about returns to their labor market experience and may use the posted wage and information about bargaining to update their beliefs. Such misspecified beliefs are likely more pronounced among lower paid workers (see e.g. Jäger et al. 2022).

The results from splitting our sample by vacancies with and without a bargaining signal are shown in Figure 4. The figure follows the same structure as above: We first present the impact on the gender gap and then the results separately for women's and men's wages. The panels on the left-hand side of Figure 4 show the estimates for vacancies including a bargaining signal. The panels on the right-hand side show the results for those vacancies that do not contain a bargaining signal.

Looking at the left-hand panel, two features become apparent. First, we observe a substantial reduction in the overall gender pay gap (see Panel (a)). The pay transparency law reduced the average gender wage gap by around 10 percentage points for individuals filling a vacancy containing a bargaining signal. The narrowing is, however, more pronounced at the lower quartile of the wage distribution. As Panel (b) shows, the reduction is primarily driven by women earning more. In contrast, we do not find any effects on men's wages (see Panel (c)).

These results indeed suggest that lower-earning women likely held misspecified beliefs about their outside options. These beliefs can arise when there is no or only noisy information available about job opportunities, such as from co-worker networks. As can be seen in Table A.1, job postings with a bargaining signal can be predominantly found in "male-dominated" occupations and industries. Wage posting, together with the information whether the posted wage is a lower bound, helps to remedy these information asymmetries. There is no effect on higher earning workers as they are, in general, better informed about their labor market opportunities. We want to highlight, however, that we do not observe whether the worker actually negotiated with

[27] We do not investigate why firms post bargaining signals.

the firm.

We do not find similar results for vacancies without a bargaining signal (see the panels on the right-hand side of Figure 4). In contrast, we find that the posting law has even slightly increased the gender gap at the lower quartile. This is mainly driven by a reduction in wages of women. One explanation for this negative effect is that firms may use their wage posting as a take-it-or-leave it offer. By being required to post wages publicly, these firms can credibly claim to walk away from negotiations if the worker demands a higher wage (in the spirit of Cullen and Pakzard-Hurson 2023).[28]

Note that we are largely agnostic to why firms decide to post any bargaining signals and a thorough analysis is out of scope of our paper. One possible reason is that firms posting bargaining signals are more efficient in hiring. In line with this argument, Schmidpeter and Tô (2023) show that firms with negotiable wage postings tend to be more selective but are able to hire faster than firms with non-negotiable postings.

Availability of Position: The results in Cortés et al. (forthcoming) suggest that women tend to do particularly bad in situations where prospective employers only give short deadlines to accept job offers ("exploding offers"). Such a scenario is likely to occur when a job is available relatively shortly after the advertisement was posted. We define a job to be immediately available when the starting date lies within the next seven days after the vacancy was posted. Similarly, not immediately available jobs are all jobs with a potential starting date of more than one week after the posting date.

We present the results of this exercise in Figure 5. The left-hand side of the figure shows our estimates for immediately available positions. The right-hand side contains the estimates for jobs with a longer duration until the job is available.

Looking at Panel (a), we see an almost reversed pattern compared to our bargaining sample. While there is again a reduction in the gender gap at the very bottom of the distribution, we also observe a reduction at the upper part. The effect is again mostly driven by women earning more after the introduction of the law. Interpreted through the job search model of Cortés et al. (forthcoming), which incorporates gender differences in overconfidence and risk aversion, providing women with pay information helps them to update their beliefs about future job offers. The additional information therefore enables them to decide whether the current job offer is acceptable.

In line with our information argument, we do not find any effects in situations where decisions are unlikely to be made under pressure. Using our sample of vacancies where the job is not immediately available we find neither an impact on the gender gap nor on gender specific wages. All of our estimates along the wage distribution are concentrated tightly around zero.

[28] If firms can lower wages by providing wage information, a question is why we did not see many wage postings prior to the law. One explanation is that without enforcement, firms could not credibly claim that the posted walk is indeed the highest possible wage rate (see Cullen 2023).

4.2 Sensitivity Checks

We conduct several checks to assess the robustness of our results. First, we provide evidence that the main identification assumptions of the QDiD estimator hold in our setting. In standard linear DiD models, one would use an event-study type analysis and assess whether any effect prior to the introduction of the law is zero. If this is the case, researchers use this as support for their identifying assumptions. Doing so is, however, not possible when using QDiD. Instead, we follow the placebo approach suggested by Callaway et al. (2018) and Callaway and Li (2019) to assess potential violations of the (conditional) Distributional Difference-in-Difference Assumption A1.

To do so, 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 we now shift all dates by one and two years to the past. In each placebo sample, we then define the treatment group as all those vacancies posted six month before and after March 2010 for the first sample, and March 2009 for the second sample. We define the control group analogously. If our results are driven by some spurious correlations in the data and not the transparency law, we would expect to find some effects for our placebo estimates as well. Likewise, if we do not find any effects, we see this as support for our research design.

As one can see in Figure 6, we do not find evidence that our estimates are caused by patterns in the data unrelated to the transparency law. Over large parts of the distribution, our placebo estimates are tightly concentrated around zero and none is statistically significant on any conventional level, regardless which placebo sample we are using. These insignificant results are also reflected in the zero mean effects we find when using the unconditional DiD approach.

Second, we also show that our results are not affected by the internal wage transparency law implemented in July 2011. This internal pay transparency law required firms with more than 1,000 employees to provide internal pay statistics (see Section 2 and Böheim and Gust 2021 as well as Gulyas et al. 2023 for an empirical evaluation[29]). We re-run our analysis excluding firms with more than 1,000 employees at the time of the vacancy posting. The results are shown in Figure 7.

The estimates are virtually identical to the main results discussed in the previous section. The QDiD exhibits very similar patterns and the estimates are of similar magnitude. As before, we estimate the strongest decrease in the gender pay gap at the bottom of the wage distribution in settings where firms specify their willingness to bargain in their postings. At the top, we find the largest decrease in the gender pay gap in situations where women likely need to make a job acceptance decisions under pressure. Overall, our results here do not indicate that the internal pay transparency law had any impact on our estimates.

Lastly, we also show that our results hold when using a "standard" DiD approach, commonly

[29] Both works do not find any or only imprecise effects of the internal wage transparency law on wages and gender wage gaps.

applied in such settings:

$$\begin{aligned} y_{ivt} = \alpha + \beta_1 \cdot T_v + \beta_2 \cdot Post_t + \beta_3 \cdot T_v \times Post_t + \beta_4 \cdot T_v \times Post_t \times Fem_i + \\ \beta_5 \cdot T_v \times Fem_i + \beta_6 \cdot Post_t \times Fem_i + \boldsymbol{\gamma\Gamma} + \varepsilon_{ivt} \end{aligned} \tag{6}$$

where the coefficient of the triple-interaction β_4 is a direct estimate of the treatment effect on the gender wage gap. $\boldsymbol{\Gamma}$ consists of a set of additional covariates also used for the quantile analysis, i.e. indicators for white-collar job, full-time vacancy and migration status, a second-order polynomial of age, experience, unemployment duration and number of children, as well as occupation, industry, federal state and month of year fixed-effects. All variables are interacted with a binary indicator for women.[30] Table 3 summarizes estimates for the impact of the pay transparency law on the gender gap and the overall wages using our linear DiD estimator.

The results in the table confirm our main results and conclusions. Looking at the results in the first column, external wage transparency has led to a small (and insignificant) improvement of the gender pay gap, similar to what we have found using our QDiD approach. In our linear DiD specification, we find strong evidence, however, that when firms post bargaining signals (see the second column) or when needing to make job acceptance decisions under pressure (see the fourth column), wage transparency leads to a narrowing of the gender wage gap.[31]

5 Information, Changes in Postings or Sorting

Our results show that external pay transparency laws can increase women's wages and decrease the gender pay gap. At the same time, we do not find evidence that the reduction of the gender gap comes at the cost of reducing wages of men. The reduction in the gender pay gap is particularly pronounced in situations where women are likely to hold misspecified beliefs about their labor market options.

An alternative explanation is that pay transparency has not (only) led to belief updating about outside options, but has changed which firms are posting a vacancy and with whom they match.[32] For example, external pay transparency can direct more workers toward applying for a certain position in a firm. An increasing number of applicants also increases the chances for a firm to obtain a better match. At the same time, as more workers apply, the chance of getting hired decreases for each single applicant. Firms therefore need to compensate the prospective employees with higher wages for the higher risk of not getting the job, leading only more productive and potentially larger firms to post new jobs. It is also possible that firms anticipate that wage information revealed by pay transparency triggers an increase in

[30] Strictly speaking, the DiD estimator in Equation (6) does not recover the unconditional average treatment effect on the treated, unlike our DiD estimator in Equation (4) based on the QDiD approach.

[31] Notice that unlike internal pay transparency, our linear estimator does suggest that pay transparency is reducing overall wages, but wage gains for women and slight wage losses for men.

[32] We want to stress again that our work is silent to why a firm chooses its action.

competition for hiring workers and may therefore try to direct workers' applications by offering other amenities in their postings or change skill requirements.[33] Note that such alternative explanation is neither exclusive to nor rules out our main updating and bargaining hypothesis. Investigating such a potential channel further, however, allows us to better understand our main results.

To evaluate whether the reform changed postings and matches, we use a linear DiD design:

$$y_{ivt} = \alpha + \beta_1 \cdot T_v + \beta_2 \cdot Post_t + \beta_3 \cdot T_v \times Post_t + \lambda_o + \lambda_j + \lambda_s + \varepsilon_{ivt} \tag{7}$$

where y_{ivt} is the characteristic of a vacancy v that was posted at time t and filled with worker i, D_v is the treatment indicator being 1 if the vacancy is posted between September 2010 and August 2011, and $Post_t$ is the indicator for vacancies posted after March. In this DiD setting, β_3 is the parameter of interest and shows the change in a certain vacancy characteristic due to the reform. We also add occupation (λ_o), industry (λ_j) and federal state (λ_s) fixed effects.

We use a wide range of vacancy characteristics to obtain a comprehensive picture of possible changes associated with the law. For example, we use information about the required education and specific skills (e.g. problem-solving, social skills or management skills), but also about additional amenities (e.g. fringe benefits, offered contract duration, and working hours). Figure 8 plots the DiD estimates for these outcomes. Our results show that firms' vacancy posting has not been affected by the introduction of the pay transparency law. For example, we do not observe that firms posting after the law ask for a different set of skills. We also do not find that firms adjust posted fringe benefits after the reform. Interestingly, we also do not find that the reform changed the usage of our bargaining signal in postings.

We also do not find that there was a change in the type of firms posting vacancies after the introduction of the law (see Table 4). There is no change in firm size which would suggest that only more productive firms are posting after the law. We also do not find that firms change their age structure or share of female employment. Also the composition of firm industries and occupations has not changed as a result of the reform (see Figure 9).

While there is no evidence that the pay transparency law has changed what firm posts what type of vacancy, it may have affected firm-worker matching. To explore this possibility, we re-estimate Equation (7) using several worker characteristics, including gender, education, commuting distance, and age, as outcomes. Extending our example above, being possibly able to choose from a wider pool of applicants, firms may try to hire only higher educated workers after the transparency law. The results are shown in Table 4.[34] As in the case of vacancy characteristics, we do not find that the pay transparency law has changed the hiring behavior of firms. We neither observe that the hired workers are more educated nor that they are more

[33] Related to our examples, see Wu (2020) for a model with partial directed search and limited information.

[34] Table A.2 shows that gender differences in worker and firm characteristics as well as in job search outcomes were unaffected by the reform. For these results, we estimate a model similar to that in Equation (7) where all variables are interacted with a binary indicator for women.

likely to be female. Our results also do not indicate that hired workers now commute longer to work, implying that the pay transparency law is unlikely to have altered the geographic pool of applicants. We find, however, support for a small increase in search efficiency. Looking at how long it takes between posting and filling of the vacancy, we see that after the law, positions were filled slightly faster, although the estimate is rather imprecise. Related to this, Škoda (2022) finds that the pay transparency law in Slovakia has increased interest for open positions posting wages.[35]

Overall, our results here support our hypothesis that the external pay transparency law has led to a correction of misspecified beliefs and bargaining, specifically for the least informed. We do not find evidence that the positive wage effects we find are driven by changes in firms or firm-worker matches. In that sense, they are also in line with Bamieh and Ziegler (2022) who show that the Austrian law has not affected (gender) sorting into firms. The absence of selection effects are also found in Škoda (2022), evaluating an external pay transparency reform in Slovakia.

6 Conclusions

To reduce wage inequality, providing workers with the possibility to obtain information about their peers' pay has become a popular policy tool in many countries. But despite their popularity, it is still an open question whether pay transparency policies work as intended. For example, internal pay transparency, which gives workers access to information about their co-workers' wages, either had no effect on wage inequality or, if it does, it also compresses the overall wage structure.

In our work, we evaluate the introduction of an Austrian pay transparency law. The pay transparency law requires that all vacancies posted with private or public employment agencies after March 1, 2011 have to include (i) a posted wage and, unique to the Austrian pay transparency law, (ii) a reference whether the firm is willing to overpay. It therefore makes the employer's willingness to pay and the value of outside options more salient to both job applicants and incumbent workers.

Using linked vacancy-employer-employee data, we first show that the share of vacancies with posted wages increased from around 10% prior to the law to almost 100% after the pay transparency law was introduced. Firms fully complied with the law and there was no room for strategically selection into wage postings.

Looking at the full distribution of wages and the gender wage gap we find that the reform led to a small overall reduction of the gender wage gap. Moreover, our main results suggest that reductions in the wage gap are larger in situations where firms either specified their willingness to bargain over wages or when women likely had to make the job acceptance decision under

[35] Schmidpeter and Tô (2023) find that the efficiency argument is particularly true for firms with negotiable wage postings.

(time) pressure. In all these cases we find that the reductions in the gender gap are due to women earning more, whereas men's wages remain largely constant.

Our results stand in contrast to findings on the effects of internal wage transparency laws. Requiring external pay transparency has the potential of reducing the gender wage gap without compressing overall wages. One potential channel why external pay transparency laws can work may be that firms need to balance information conveyed via wage postings and hiring efficiency. Internal transparency laws create incentives for firms to lower wages in order to limit information spill-overs, as described in Cullen and Pakzard-Hurson (2023), without being concerned about potential external hiring. As external transparency laws make pay information available to both job applicants and incumbent workers, firms face a trade-off between posting low wages and the likelihood of filling the positions. While the reform has had no impact on firm-worker sorting, we find evidence that it has increased hiring efficiency (see also Škoda 2022 and Schmidpeter and Tô 2023).

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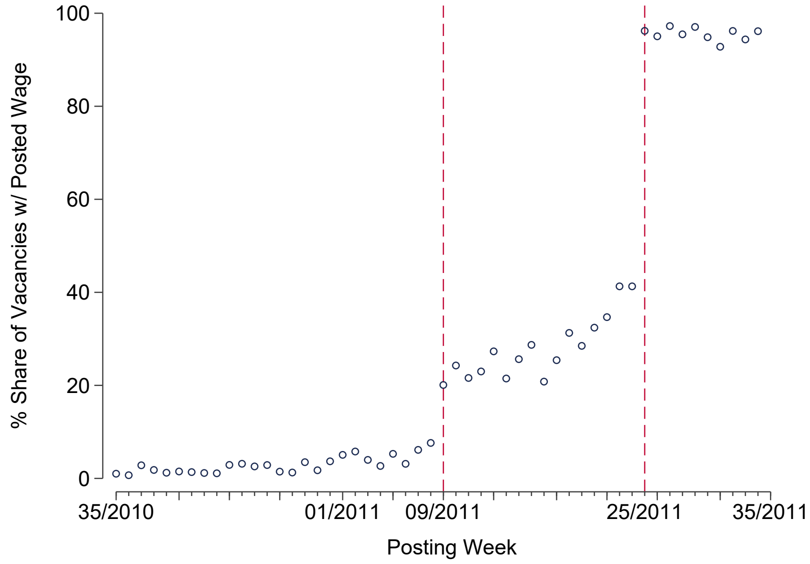
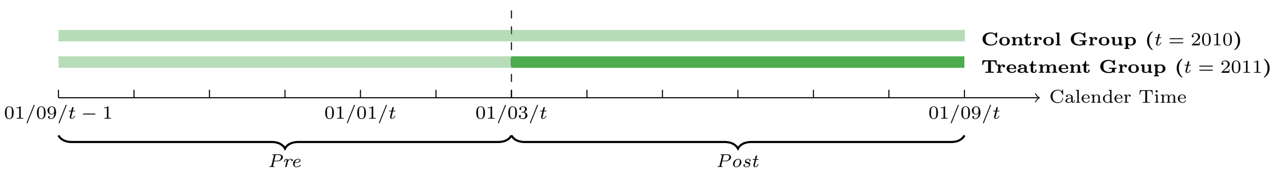
7 Figures (to be placed in the article)

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



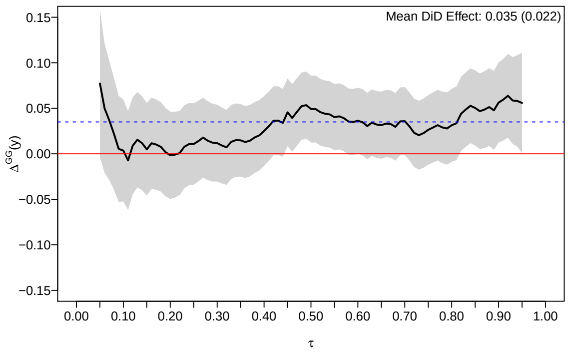
Note — Share of vacancies with a posted wage six months before and after the Equal Treatment Law reform by posting week. The first vertical line represents marks the date of the law reform (March 1, 2011), the second marks the enforcement date of reform by the employment agency (June 20, 2011).

Figure 2: Definition of treatment and control group

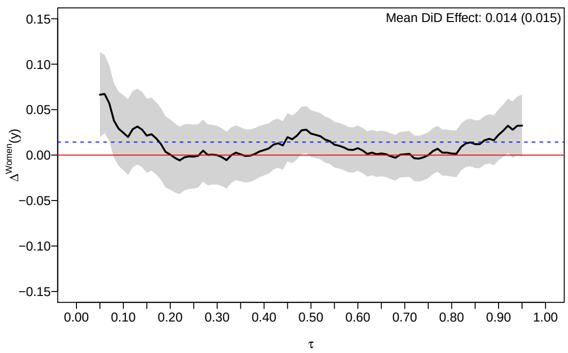


Note — The figure visualizes the construction of the treatment and control group. The treatment group comprises all vacancies posted six months before and after the reform date (March 1, 2011). For the control group, we us all vacancies posted in the same time window but one calendar year before (i.e. six months before and after March 1, 2010). We then split the sample in a pre- and post-period by assigning the vacancies posted before March 1 of the respective year as the pre-period and the vacancies posted thereafter as the post-period. The dark green bar marks the observation period that has actually been affected by the reform.

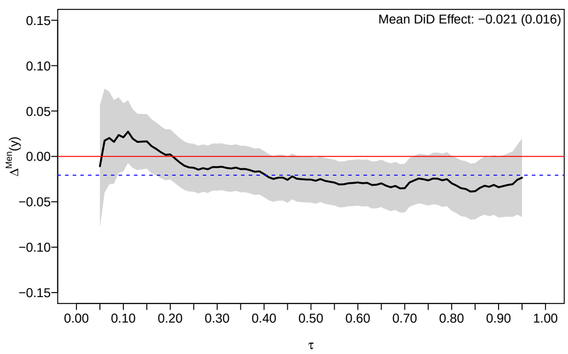
Figure 3: Main Results by Gender



(a) Gender Gap



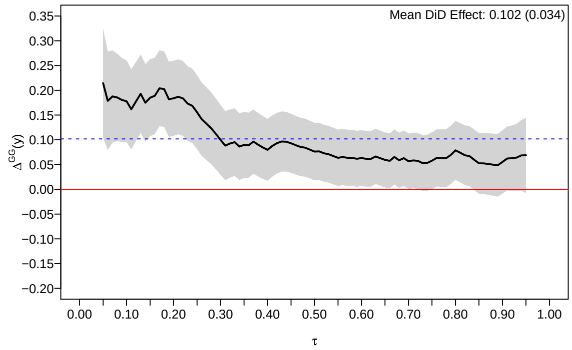
(b) Women



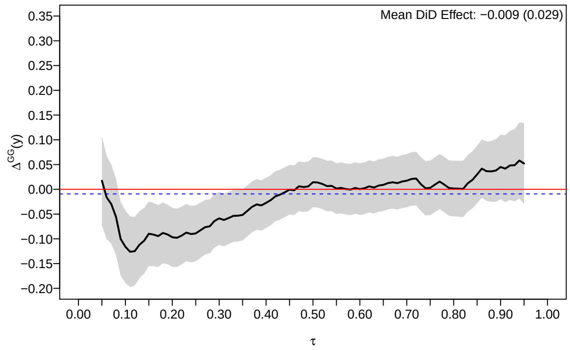
(c) Men

Note — Estimated quantile treatment effects for the gender wage gap, women's log daily wages and men's log daily wages. The coefficients for women's and men's wages can be interpreted in percent changes. The coefficients for the gender wage gap 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 Difference-in-Difference effect. It is also reported in the top right corner along with corresponding standard error in parentheses.

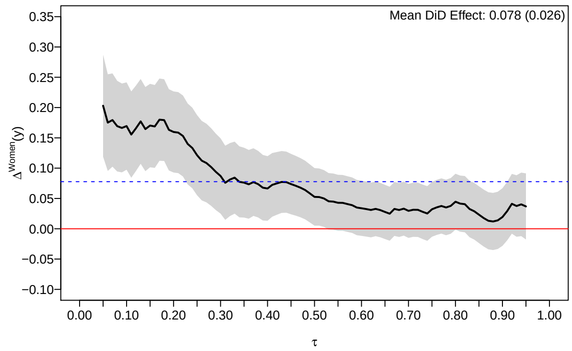
Figure 4: Results by Bargaining Signal



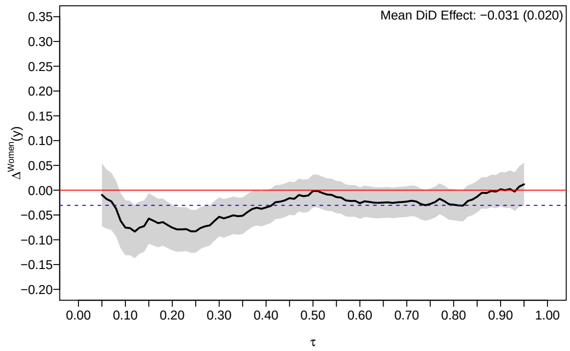
(a) Bargaining Signal - Gender Gap



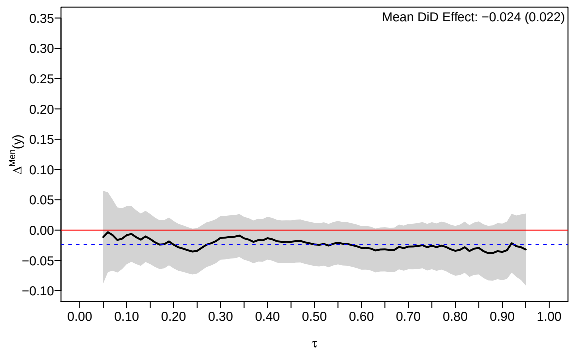
(b) No Bargaining Signal - Gender Gap



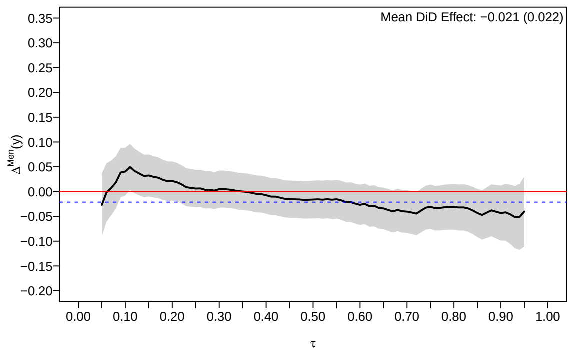
(c) Bargaining Signal - Women



(d) No Bargaining Signal - Women



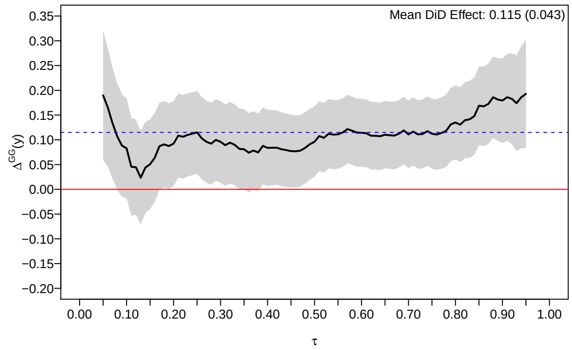
(e) Bargaining Signal - Men



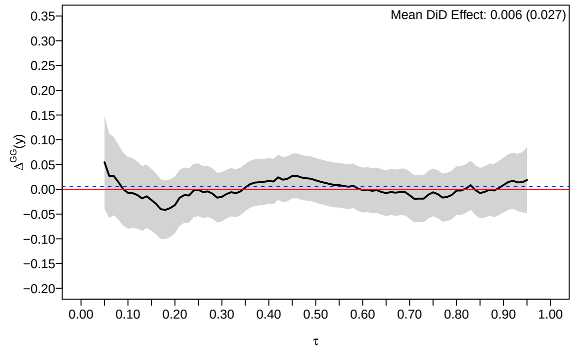
(f) No Bargaining Signal - Men

Note — Estimated quantile treatment effects for the gender wage gap, women's log daily wages and men's log daily wages. The coefficients for women's and men's wages can be interpreted in percent changes. The coefficients for the gender wage gap can therefore be interpreted as the percentage point change in the gender wage gap. The first column reports the results for vacancies that include a bargaining signal in the vacancy text, while the second shows the results for those vacancies where this is not the case. 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 Difference-in-Difference effect. It is also reported in the top right corner along with corresponding standard error in parentheses.

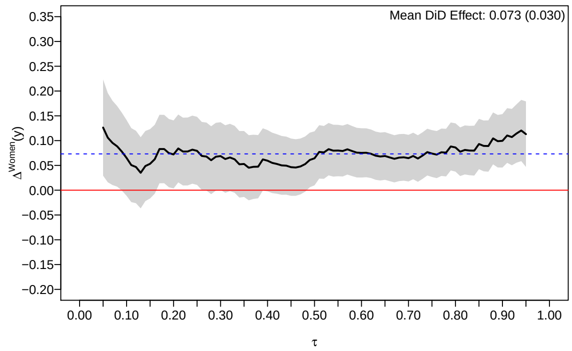
Figure 5: Results by Job Availability



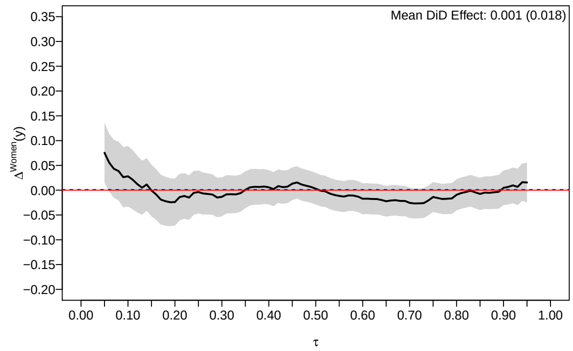
(a) **Immediately Available - Gender Gap**



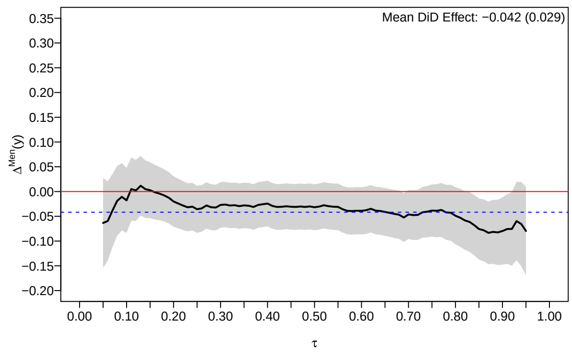
(b) **Not Immediately Available - Gender Gap**



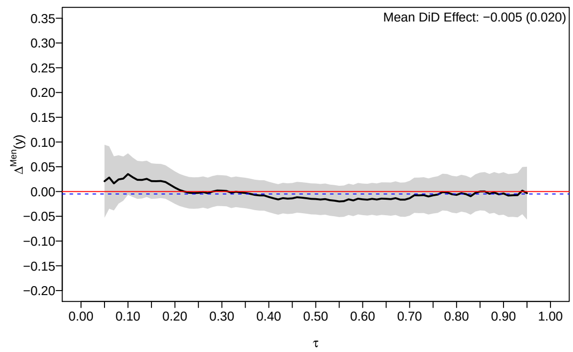
(c) **Immediately Available - Women**



(d) **Not Immediately Available - Women**



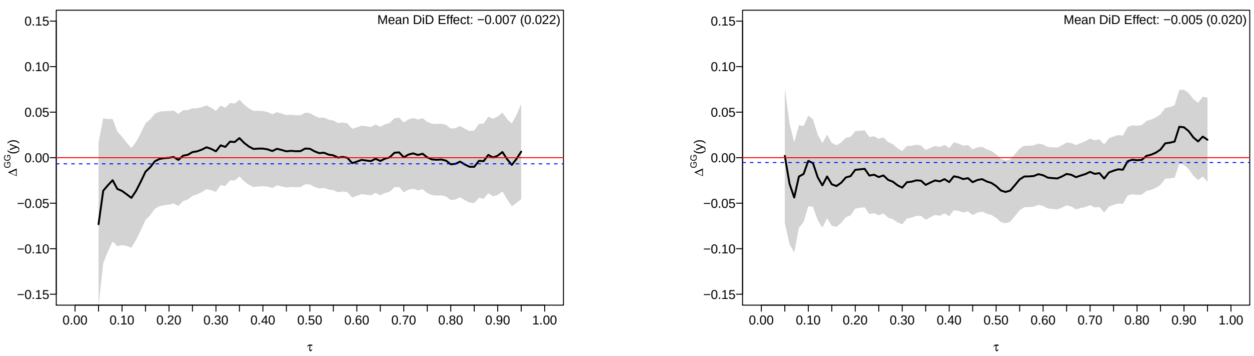
(e) **Immediately Available - Men**



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

Note — Estimated quantile treatment effects for the gender wage gap, women's log daily wages and men's log daily wages. The coefficients for women's and men's wages can be interpreted in percent changes. The coefficients for the gender wage gap can therefore be interpreted as the percentage point change in the gender wage gap. The first column reports the results for jobs that are immediately available (difference between availability date and posting date ≤ 7 days), while the second shows the results for those vacancies where this is not the case. 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 Difference-in-Difference effect. It is also reported in the top right corner along with corresponding standard error in parentheses.

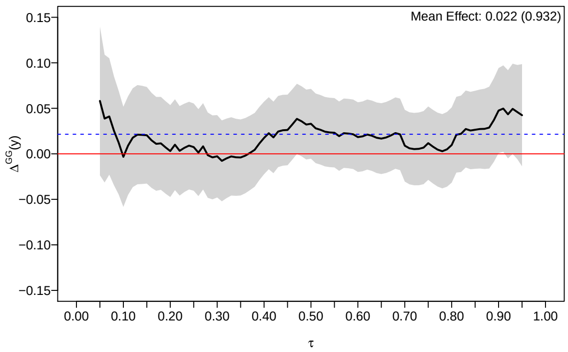
Figure 6: Placebo Checks



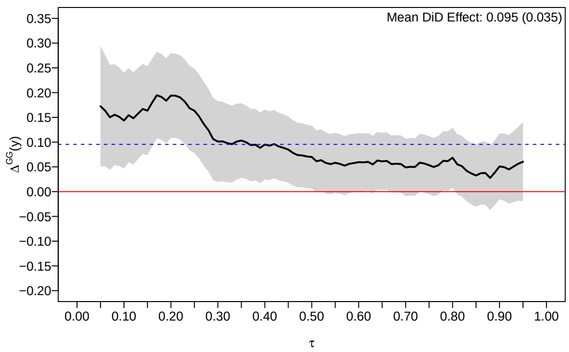
(a) Sample -1 **(b) Sample -2**

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 Difference-in-Difference effect. It is also reported in the top right corner along with corresponding standard error in parentheses.

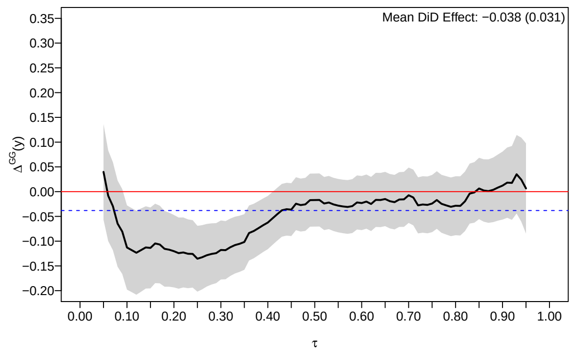
Figure 7: Gender Gap Results – Excluding Firms with More Than 1 000 Employees



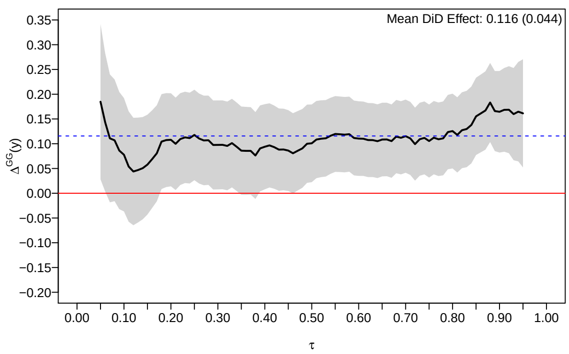
(a) **Full Sample**



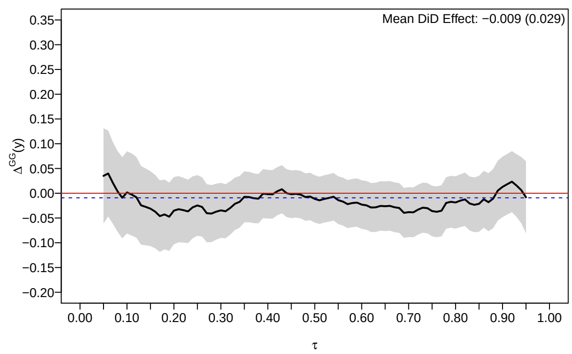
(b) **Bargaining Signal**



(c) **No Bargaining Signal**



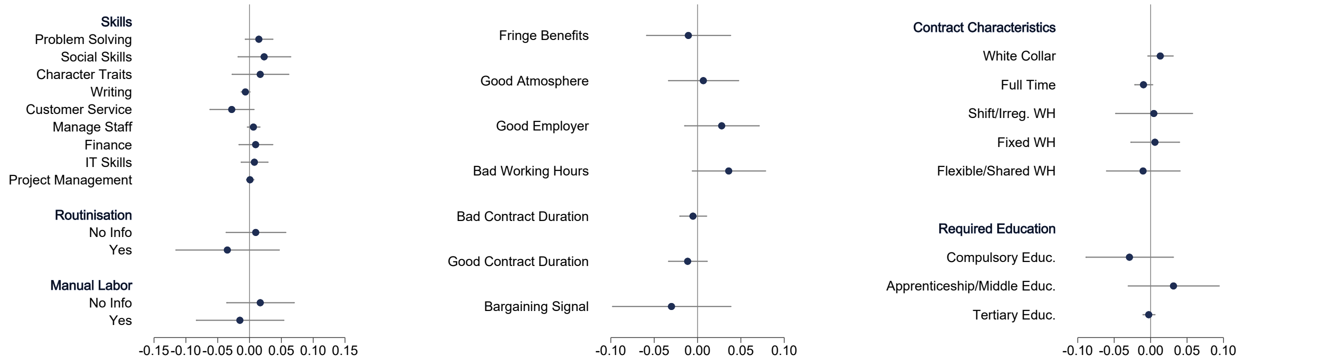
(d) **Immediately Available**



(e) **Not Immediately Available**

Note — Estimated quantile treatment effects for the gender wage gap in the three main sub-samples. The coefficients for the gender wage gap are obtained by subtracting the effect on women's log daily wages from the effect on men's log daily wages. They can therefore be interpreted as the percentage point change in the gender wage gap. The estimates are based on a main estimates sample that excludes all firms with more than 1 000 employees as they were affected by a second reform implemented at the same time as the reform we study in this paper. 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 Difference-in-Difference effect. It is also reported in the top right corner along with corresponding standard error in parentheses.

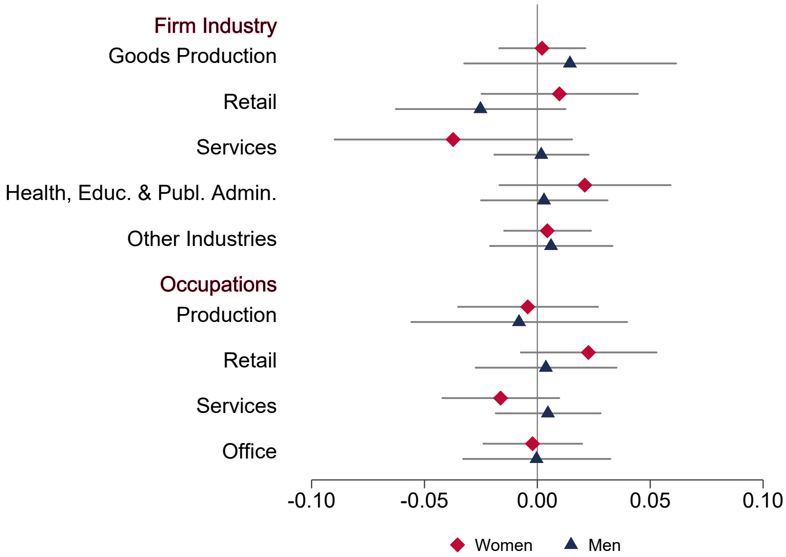
Figure 8: Changes in Vacancy Characteristics



(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 9: 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.

8 Tables (to be placed in the article)

Table 1: Descriptive Statistics

	Ø Control	Ø Treatment	Difference (SE)
Person Characteristics			
Female	0.619	0.610	0.009 (0.008)
Age at Vacancy	35.273	35.385	-0.113 (0.188)
Foreigner	0.170	0.197	-0.026 (0.007)
No. of Children	0.853	0.824	0.029 (0.020)
Labour Market Outcomes			
Experience in Years	9.935	9.634	0.301 (0.143)
Unemployment Duration in Months	3.224	2.958	0.266 (0.069)
Real Daily Wage Excl. Special Payments	50.186	49.265	0.920 (0.297)
Vacancy Characteristics			
White Collar	0.459	0.433	0.026 (0.009)
Ad for Full-Time Job	0.611	0.626	-0.015 (0.008)
Job is Immediately Available	0.371	0.336	0.034 (0.008)
Vacancy is Difficult-to-Fill	0.462	0.456	0.006 (0.009)
Bargaining Signal	0.435	0.384	0.051 (0.009)
Occupations			
Production	0.273	0.291	-0.018 (0.008)
Retail	0.301	0.292	0.009 (0.008)
Services, Education & Health	0.306	0.308	-0.002 (0.008)
Office	0.120	0.110	0.010 (0.006)
Industry			
Goods Production	0.248	0.254	-0.006 (0.008)
Retail	0.402	0.386	0.016 (0.009)
Services	0.157	0.178	-0.021 (0.007)
Health, Educ. & Public Admin.	0.149	0.130	0.020 (0.006)
Other Industries	0.044	0.052	-0.008 (0.004)
Firm Location			
Eastern Austria	0.430	0.405	0.026 (0.009)
Southern Austria	0.188	0.205	-0.017 (0.007)
Western Austria	0.382	0.390	-0.008 (0.009)
Required Education			
Compulsory Education	0.542	0.553	-0.010 (0.009)
Apprenticeship/Middle Educ.	0.414	0.403	0.011 (0.009)
Tertiary Education	0.043	0.044	-0.001 (0.004)
Job Characteristics			
Problem Solving	0.035	0.047	-0.012 (0.003)
Social Skills	0.151	0.189	-0.038 (0.007)
Character Traits	0.372	0.419	-0.048 (0.009)
Writing	0.023	0.019	0.003 (0.003)
Customer Service	0.310	0.294	0.015 (0.008)
Manage Staff	0.026	0.040	-0.014 (0.003)
Finance	0.092	0.071	0.021 (0.005)
IT Skills	0.088	0.097	-0.009 (0.005)
Project Management	0.020	0.020	0.000 (0.002)
Routinisation			
No Information	0.530	0.501	0.030 (0.009)
Yes	0.783	0.773	0.011 (0.010)
Manual Labor			
No Information	0.615	0.605	0.010 (0.009)
Yes	0.703	0.639	0.064 (0.013)
No. of Observations	6,730	6,401	

Note — The first and second column show the averages for the control and the treatment group respectively. The third column reports the difference in means between these to groups. The standard error is given in parentheses next to the difference. Inference is based on robust standard errors.

Table 2: Pre-Reform Gender Gaps in Log Daily Starting Wages

	Full Sample		With Bargaining Signal		Imm. Avail. Jobs	
	Raw	Adjusted	Raw	Adjusted	Raw	Adjusted
Female	-0.300***	-0.166***	-0.311***	-0.182***	-0.345***	-0.173***
	(0.013)	(0.011)	(0.021)	(0.019)	(0.023)	(0.022)
Covariates:						
White Collar	N	Y	N	Y	N	Y
Ad for Full-Time Job	N	Y	N	Y	N	Y
Age	N	Y	N	Y	N	Y
Age^2	N	Y	N	Y	N	Y
Experience	N	Y	N	Y	N	Y
Unemployment Duration	N	Y	N	Y	N	Y
No. of Children	N	Y	N	Y	N	Y
Foreigner	N	Y	N	Y	N	Y
Job Immediately Available	N	Y	N	Y	N	Y
Fixed Effects:						
Industry	N	Y	N	Y	N	Y
Federal State	N	Y	N	Y	N	Y
Occupation	N	Y	N	Y	N	Y
Month of Year	N	Y	N	Y	N	Y
Constant	Y	Y	Y	Y	Y	Y
N	3,010	3,006	1,296	1,295	1,000	1,000
Adj. R^2	0.145	0.450	0.138	0.395	0.167	0.485
Sample Mean	3.839	3.839	3.838	3.838	3.794	3.794

Note — The table reports the estimated pre-reform gender gap in log daily starting wages for the full sample, for vacancies that contain a bargaining signal and for jobs that are immediately available. The raw gap is obtained from a simple linear OLS regression including only a binary indicator that is one for women and a constant on the left-hand side. For the adjusted gap, we additionally control for the variables indicated in the bottom of the table. The sample includes only pre-reform observations in the treatment group. Robust standard errors are given in parentheses below the coefficients. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Linear Difference-in-Difference Estimates

	Full Sample	Bargaining Signal		Job Immediately Available	
		Yes	No	Yes	No
Treatment × Post × Female	0.009	0.044*	-0.022	0.046**	-0.014
	(0.016)	(0.024)	(0.023)	(0.022)	(0.022)
Treatment × Post	-0.013	-0.020	-0.004	-0.035**	0.004
	(0.016)	(0.019)	(0.024)	(0.016)	(0.022)
Covariates:					
White Collar	Y	Y	Y	Y	Y
Vacancy for Full Time	Y	Y	Y	Y	Y
Age	Y	Y	Y	Y	Y
Age^2	Y	Y	Y	Y	Y
Experience	Y	Y	Y	Y	Y
Unemployment Duration	Y	Y	Y	Y	Y
Number of Children	Y	Y	Y	Y	Y
Foreigner	Y	Y	Y	Y	Y
Job Immediately Available	Y	Y	Y	Y	Y
Fixed Effects:					
Occupation	Y	Y	Y	Y	Y
Industry	Y	Y	Y	Y	Y
Federal State	Y	Y	Y	Y	Y
Month of Year	Y	Y	Y	Y	Y
Constant	Y	Y	Y	Y	Y
N	13,123	5,380	7,740	4,639	8,478
Adj. R^2	0.473	0.442	0.495	0.486	0.459

Note — Linear Triple Difference-in-Difference estimates for the reform impact on the gender gap in log daily starting wages. Estimates are reported for the full sample and the sub-samples where bargaining should be more likely to occur. In all regressions we control for the covariates and fixed effects indicated in the table. All covariates and fixed effects are interacted with a binary indicator that is one for women. Standard errors are clustered on the occupation level (4-digit level) and given in parenthesis below the coefficients. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: 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)		

Table 4: Changes in Other Variables (Cont.)

Outcome	Treatment × Post	Treatment	Post	N	Outcome Mean
Experience in Years	0.191	-0.441	-0.190	13,131	9.788
	(0.391)	(0.347)	(0.279)		
Job Search Outcomes					
Durations					
Filling Time (Rel. to Posting)	-1.256	3.880	-0.533	13,131	45.602
	(2.183)	(4.095)	(2.052)		
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 for the reform impact on several worker and firm characteristics as well as job search outcomes. 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 "External Pay Transparency and the Gender Wage Gap" by Wolfgang Frimmel, Bernhard Schmidpeter, Rene Wiesinger and Rudolf Winter-Ebmer.

Table A.1: Descriptive Statistics by Bargaining Situation

	Ø Full Sample	Bargaining Signal			Job Immediately Available		
		Ø Yes	Ø No	SE	Ø Yes	Ø No	SE
Person Characteristics							
Female	0.614	0.563	0.650	0.009	0.604	0.620	0.009
Age at Vacancy	35.327	35.225	35.399	0.192	36.118	34.896	0.198
Foreigner	0.183	0.167	0.194	0.007	0.219	0.164	0.007
No. of Children	0.839	0.801	0.865	0.020	0.889	0.811	0.021
Labour Market Outcomes							
Experience in Years	9.788	10.284	9.444	0.146	9.798	9.785	0.149
Unemployment Duration in Months	3.094	2.801	3.298	0.069	2.711	3.302	0.071
Real Daily Wage Excl. Special Payments	49.737	50.388	49.285	0.301	48.258	50.546	0.313
Vacancy Characteristics							
White Collar	0.446	0.406	0.474	0.009	0.370	0.488	0.009
Ad for Full-Time Job	0.618	0.734	0.537	0.008	0.638	0.607	0.009
Job is Immediately Available	0.354	0.402	0.321	0.009	1.000	0.000	0.000
Vacancy is Difficult-to-Fill	0.459	0.463	0.456	0.009	0.711	0.321	0.008
Bargaining Signal	0.410	1.000	0.000	0.000	0.466	0.380	0.009
Occupations							
Production	0.282	0.365	0.224	0.008	0.292	0.276	0.008
Retail	0.296	0.326	0.276	0.008	0.292	0.299	0.008
Services, Education & Health	0.307	0.209	0.375	0.008	0.337	0.290	0.009
Office	0.115	0.101	0.125	0.006	0.079	0.135	0.005
Industry							
Goods Production	0.251	0.313	0.208	0.008	0.258	0.247	0.008
Retail	0.394	0.416	0.380	0.009	0.354	0.416	0.009
Services	0.167	0.134	0.190	0.006	0.259	0.117	0.007
Health, Educ. & Public Admin.	0.140	0.074	0.185	0.006	0.079	0.173	0.006
Other Industries	0.048	0.063	0.038	0.004	0.050	0.047	0.004
Firm Location							
Eastern Austria	0.418	0.286	0.510	0.008	0.312	0.476	0.009
Southern Austria	0.196	0.266	0.148	0.007	0.248	0.168	0.008
Western Austria	0.386	0.448	0.343	0.009	0.439	0.357	0.009
Required Education							
Compulsory Education	0.548	0.562	0.537	0.009	0.651	0.491	0.009
Apprenticeship/Middle Educ.	0.409	0.402	0.413	0.009	0.321	0.456	0.009
Tertiary Education	0.044	0.035	0.050	0.004	0.027	0.053	0.003
Job Characteristics							
Problem Solving	0.041	0.038	0.043	0.003	0.035	0.044	0.004
Social Skills	0.170	0.110	0.213	0.006	0.114	0.201	0.006
Character Traits	0.395	0.358	0.421	0.009	0.355	0.417	0.009
Writing	0.021	0.015	0.026	0.002	0.014	0.025	0.002
Customer Service	0.302	0.265	0.329	0.008	0.239	0.337	0.008
Manage Staff	0.033	0.024	0.040	0.003	0.029	0.035	0.003
Finance	0.082	0.053	0.102	0.005	0.042	0.104	0.004
IT Skills	0.092	0.090	0.094	0.005	0.066	0.106	0.005
Project Management	0.020	0.012	0.026	0.002	0.017	0.022	0.002
Routinisation							
No Information	0.516	0.629	0.436	0.009	0.594	0.473	0.009
Yes	0.778	0.869	0.736	0.010	0.822	0.760	0.011
Manual Labor							
No Information	0.610	0.678	0.562	0.009	0.675	0.574	0.009
Yes	0.672	0.745	0.634	0.013	0.734	0.646	0.014
No. of Observations	13,131	5,385	7,746		4,644	8,479	

Note — Comparison of observations by whether the vacancy text contains a bargaining signal and job availability. The first column reports the full sample average. Within each of the following categories, the first column reports the averages of observations for which a criterion is fulfilled, the second the averages of those for which it is not fulfilled. The standard error of the difference in means is given in the third column. Inference is based on robust standard errors.

Table A.2: 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
	(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)		
Job Search Outcomes				
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 Triple Difference-in-Difference estimates for the reform impact on the gender gap in several person and firm characteristics as well as job search outcomes. The regression additionally includes occupation, industry and federal state fixed effects. All covariates were interacted with a binary indicator that is one for women. Standard errors are given in parentheses and are clustered on the occupation level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$