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Pitfalls of pay transparency: Evidence from the lab and the field*

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Abstract

Wage transparency regulation is widely adopted to reduce the gender wage gap. Combining field and laboratory evidence, we investigate how wage transparency can be effective and explore the role of belief adjustments as a mechanism. In the field, this paper studies a German wage transparency policy that allows employees to request wage information of comparable employees. Exploiting variation across firm size and time, we provide causal evidence that this regulation does not affect the wage gap. In an online laboratory experiment, we study whether the failure of this policy hinges on two aspects: (1) the endogenous availability of wage information, and (2) the absence of performance information. Both factors are essential. In contrast to endogenously acquired wage information, exogenously provided wage information increases overall wages. So does the provision of performance information. However, neither type of information reduces the gender wage gap. Wage information even deters women from entering negotiations.

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1 Introduction

Despite advances in promoting equal pay, wage discrepancies between men and women still characterize the overwhelming majority of labor markets (Blau and Kahn, 2017). The EU-wide gender pay gap amounted to 13% in 2020 (Eurostat, 2021). Gender differences in negotiations are deemed one possible source for the remaining pay gap (see Recalde and Vesterlund, 2022, for a recent overview). In response, the European Commission has become vocal in urging all member states to adopt pay transparency legislation to aid employees in bargaining for a wage they deem fair (European Commission, 2021). Several EU countries and multiple states in the U.S., have already adopted various measures against pay secrecy.[1] These measures include pay information for job seekers, the right to access pay information for workers in similar positions, information on wages of top earners, and company-level gender pay gap reporting duties. However, it remains unclear what determines the effectiveness of such instruments. In this paper, we examine the impact of a wage transparency measure introduced in Germany in 2017 and adopt an online experiment to explore potential mechanisms that determine the impact of such regulation.

Wage transparency policies serve, on the one hand, to publicly reveal pay inequalities and potentially discriminatory practices, and, on the other hand, to correct employee's misguided beliefs about co-workers' wages. This paper focuses on the second dimension. Transparency reduces the informational asymmetry between employees and firms and the co-workers' wages can provide a benchmark in negotiations. Therefore, wage transparency could be utilized in negotiations by both men and women. However, since women tend to have more pessimistic beliefs about average and future wages and there is a substantial gender difference in earnings expectations (see e.g. Kiessling et al., 2019; Briel et al., 2021; Boneva et al., 2022), wage information may prove particularly beneficial to women and thus contribute to a reduction in the gender pay gap. The awareness of sizable potential gains may even push women towards negotiating more.

In 2017, Germany introduced a law that allows workers to request wage information for comparable workers. It gives employees in firms with more than 200 employees the right to request information about the compensation that comparable workers receive. This unique German wage transparency measure lends itself particularly well to isolate the effect of wage transparency on negotiations. In contrast to wage transparency regulation introduced in several other countries,[2] the German regulation permits workers to ask for wage information of the median worker with comparable work. Compared

[1] For the EU, see the fact sheet of the European Commission on pay transparency measures across the EU (European Commission, accessed December 2021). See the Women's Bureau of the U.S. Department of Labor for an overview of state measures for pay transparency (U.S. Department of Labor, accessed December 2021)

[2] See Section 2 for a discussion of studies analyzing different types of wage transparency measures.

to wage statistics aggregated at a higher level, this information appears more relevant for wage negotiations. By definition, the work is comparable, allowing workers to argue for comparable compensation. No information is published to the public, which implies that forces such as public pressure are absent, and any impact on wages is likely driven through private negotiations and responses to information.

We combine field and laboratory data to address the empirical success of current wage transparency law in Germany and study the requirements for an effective wage transparency regulation. Leveraging German administrative employer-employee matched data, our identification strategy exploits variation in the transparency policy based on firm size and over time. We employ both a difference-in-difference analysis and a difference-in-discontinuities analysis to provide a quasi-experimental evaluation of the impact of wage transparency on the gender pay gap.

We do not find any evidence that the introduced wage transparency regulation decreased the gender pay gap in Germany. Both wages of men and women are unaffected and this finding is robust and independent of the specification we consider. We can estimate this null effect with high precision. In our preferred specification, we can exclude in our 95% confidence interval that the treatment effect of the introduction of the wage transparency law is larger than a 1.29 percentage point reduction in the gender pay gap, with a point estimate smaller than 0.1 percentage points. This result remains qualitatively the same for subgroups that we expect to benefit the most from the transparency regulation. Moreover, the regulation also does not push employees to leave their current employer.

Given this result, we set out to better understand the determinants of when and how wage transparency measures can deliver on their promise to reduce gender differences in wage negotiations. We do so theoretically and experimentally. In a simple theoretical model, we propose a novel mechanism that captures the impact of wage and performance information as an information shock that corrects misguided beliefs. Since both men and women can request wage information, it is, ex-ante, not clear that women benefit more from transparency measures. Our model shows how providing both wage and performance information can decrease the gender pay gap in a Nash bargaining framework. We assume the worker cares about receiving a wage he or she perceives as fair, formalized as a preference for receiving a piece rate similar to the worker's beliefs about the piece rates of comparable workers.

In our experiment, we address potential barriers to the effectiveness of the type of wage transparency policies currently implemented in Germany. It considers how a key feature of the German legislation, the fact that information is available only on request, may limit the usefulness of such regulation. Furthermore, we examine whether wage transparency works differently in environments that also allow for performance comparison. We study this in the laboratory, as there is no naturally occurring exogenous variation in the types of transparency regulation. In the experiment, workers and firms negotiate bilaterally over the split of resources they have produced in a task. Between treatments, we vary the information provided to the worker.

As a first barrier, we consider whether requiring that wage information is actively requested diminishes the potency of this type of intervention. In Germany, only 4% of eligible employees had requested wage comparison a year after the implementation of the wage transparency regulation.[3] To analyze whether automatic access to wage information can increase its effectiveness, our experiment varies whether wage information is absent, provided upon request for a small fee, or exogenously provided. If wage information is available, the experiment informs workers of the wage of a worker who was previously paired with the same firm and did the same task.

Second, we examine the type of environments that facilitate the use of wage information. We argue that wage information is particularly useful in settings where employees are aware of their relative performance. Think of settings where performance is easily measurable and information on this is accessible, such as in sales departments, compared to a setting where it is less well observable, such as in HR departments. In the absence of performance information, employees cannot evaluate whether wage differences are due to differences in performance. This, however, may be crucial information when bargaining. Therefore, we hypothesize that the joint provision of wage and performance information has the strongest effect on gender wage differences.

In line with our findings from the field, our experimental results show that workers do not earn significantly more if wage information is provided endogenously. However, we show that workers obtain a higher wage if wage information is provided exogenously. Removing the barrier to wage information thus helps workers overall. This effect is not gender-specific. Changing beliefs about wages, therefore, does not narrow the gender pay gap. Similarly, the provision of performance information increases workers' wages. Workers' wages mirror performance differences more closely if these are observable, resulting in a reduced variance in piece rates between workers. The effect of performance information is, however, also not different between men and women. These findings suggest that decreasing the informational asymmetry between workers and firm increases the workers' bargaining power. In our setting, this increase in bargaining power is not larger for women than for men.

Our experiment also shows that different types of wage transparency regulations can have unintended consequences. First, we observe that if wage information is provided on request only, employees requesting this information receive lower wages than employees being provided with this information exogenously. High-performing individuals are more

[3]See Report by the Federal Government on the effectiveness of the Act to Promote Transparency in Wage Structures among Women and Men (Germany Federal Ministry for Family Affairs, Senior Citizens, Elderly and Youth, accessed July 2022)

likely to request wage information, but less likely to benefit from it. Second, receiving wage information reduces women's propensity to enter negotiations. While the share of decisions to opt out of negotiations in our experiment is low enough such that this does not translate into a significant change in the wage gap, opting out of wage negotiations is associated with a substantial expected wage loss. Hence, wage transparency regulation might also backfire by deterring women from negotiating at all.

The effects we find in the laboratory are modest and may, in particular, not generalize to settings where wage information is disclosed publicly instead of privately. Nevertheless, our results have policy implications for the design of wage transparency regulations. We underline the importance of studying the distinct features of wage transparency regulations before rolling out future policies. First, the analyzed 'pay information right' regulation, which allows employees to request wage information, has been unsuccessful so far. 'Pay reporting duties', which require employers to provide this information, sometimes even publicly, might fare better (Bennedsen et al., 2022; Duchini et al., 2020). Advantages of providing pay information to everyone have, however, to be weighed against potential downsides, such as possibly lower job satisfaction (Card et al., 2012). Second, policymakers may need to consider distinct wage transparency policies depending on their specific goal. We see in our experiment that workers overall might benefit from transparency, but this does not reduce the gender wage gap. As women are deterred from entering negotiations by wage transparency, potentially due to the social comparison it entails, providing wage information may have adverse effects.

This paper proceeds as follows: Section 2 reviews the related literature. Section 3 provides an overview of the institutional setting and the analysis of the field data examining the effects of the German wage transparency law. We turn to the experiment in Section 4, first explaining our theoretical predictions, then the experimental design and the results from the experiment. Section 5 briefly concludes.

2 Related literature

Our results aim to contribute to two strands of the literature. First, we contribute to the growing literature on the impact of wage transparency laws in different settings. So far, no consensus has been reached on the effects of transparency measures.

The literature shows that wage information significantly reduced the gender pay gap among academics in Canadian and British universities (Baker et al., forthcoming; Gamage et al., 2020). A particular focus has so far been on the study of 'pay reporting duties', where companies are required to disclose gender-specific wage statistics. These policies are often implemented based on a size threshold and only affect firms with sufficiently many employees, an assignment rule that has been exploited in other studies. Such a reform in the U.K. resulted in more women being hired in above-median-wage jobs and a reduction in the male hourly wages (Duchini et al., 2020). The reform resulted in a decrease in the gender pay gap (Blundell, 2021). These findings are in line with evidence from Denmark, where slower wage growth for men drove a significant decrease in pay inequality (Bennedsen et al., 2022).

There are, however, not only success stories of wage transparency regulations. Publicly disclosed wages reduced the managers' compensation in California (Mas, 2017) and wage transparency can reduce job satisfaction (Card et al., 2012). More closely related to our research, the Austrian Pay Transparency Law did not impact wages (Gulyas et al., forthcoming; Böheim and Gust, 2021). Wage information in Austrian job advertisements also did not affect gender sorting into better-paid jobs (Bamieh and Ziegler, 2022). Greater transparency in the U.S. private sector has even reduced overall wages (Cullen and Pakzad-Hurson, 2021).

Our study contributes another data point to the conflicting results in this growing literature. Our aim, however, is broader than this. So far, there is little evidence on what could make a transparency law effective[4]. One contribution of this study is to investigate the unique transparency policy implemented in Germany that mandates the provision of wage information of co-workers in comparable positions on request, rather than the publication of firm-wide wage averages. Therefore, we do not study transparency measures classified as 'pay reporting duties', but a different class of measures coined 'pay information rights'. We analyze this endogeneity of receiving wage information more closely in our experiment. Furthermore, the information on wages paid to workers in similar positions could plausibly be more useful in wage negotiations than aggregate wage statistics. Therefore, our contribution is to investigate a setting in which wage information particularly lends itself to be used in negotiations.

The second strand of literature we contribute to is the experimental literature that studies gender differences in negotiations. Wage negotiations are seen as one source of the gender pay gap. Women enter negotiations less often, ask for lower wages (Roussille, 2020), and, depending on the exact setting, receive worse negotiation outcomes, see e.g. Bowles et al. (2005), Azmat and Petrongolo (2014), Mazei et al. (2015), Hernandez-Arenaz and Iriberri (2018) or Recalde and Vesterlund (2022) for overviews. In particular, settings with high ambiguity over the possibility to negotiate, that are competitive, and in which women have to negotiate on behalf of themselves (Bowles et al., 2005; Amanatullah and Tinsley, 2013) are prone to result in lower wages for women. Field evidence is in line with these findings. Flexible wage policies that allow for wage bargaining increase the gender wage gap among public school teachers (Biasi and Sarsons, 2022); women have a lower propensity to enter negotiations (Greig, 2008), especially if there is ambiguity (Leibbrandt and List, 2015); and female graduates request lower wages in their starting-

[4]An exception is the study by Cullen and Pakzad-Hurson (2021), which shows that unionization rates mute the negative impact of transparency laws on wages.

wage negotiations (Säve-Söderbergh, 2019).

Closest to our work is the literature that considers how information and interventions in negotiations affect gender differences. One possible intervention is to force women to negotiate more. Laboratory evidence, however, suggests that this does not benefit women. If women are forced to enter negotiations, they have to enter negotiations that are not profitable (Exley et al., 2020). The other extreme would be a negotiation ban, which appears to be more successful. Banning negotiations reduces inequalities between men and women in an experiment (Gihleb et al., 2020).

There is a small literature explicitly focusing on transparency interventions in negotiations. The literature shows that providing wage information can affect employees' behavior. In a field experiment, employees exert more effort if they find out that their managers earn more than expected (Cullen and Perez-Truglia, 2022). There is no evidence of any gender-specific impacts of this information. Some laboratory studies consider the effect of the provision of social information on wage negotiations. Focusing on the dynamic response of firms to the requirement of providing wage information, recent evidence points to higher and more equal wage offers with exogenous compared to endogenous information (Werner, 2019). In contrast to our study, Werner (2019) does not study gender-specific effects and focuses on firm behavior. In an ultimatum bargaining experiment that varies whether information on previous pay requests and average offers are provided, the gender gap in negotiated wages disappears if information is available (Rigdon, 2012). In contrast to our study, the information provided here stems from male participants only.

We add to this strand of literature by examining both the difference between endogenous and exogenous information provision and the interaction of wage and performance information. Furthermore, we focus on the effect of information on gender wage differences and take a closer look at the mechanisms that drive the effect of information provision by studying how beliefs are corrected. Specifically, we capture the role of confidence and beliefs about others' wages.

3 Field data

In this section, we will first introduce the institutional setting relevant for the wage transparency law in Section 3.1, then describe the data used in our analysis in Section 3.2, explain our identification strategy in Section 3.3 and finally discuss our results in Section 3.4. We provide robustness checks in Section 3.5.

3.1 Institutional setting

Germany has one of the largest gender wage gaps in the EU, with women earning on average 18.3% less than men in 2020.[5] In March 2017, the German federal parliament passed new legislation to battle gender-based wage inequality. This legislation was adopted in June of that year as the 'Gesetz zur Förderung der Transparenz von Entgeltstrukturen' (BGBl. I S. 2152, referred to here as 'wage transparency law'). The goal of this law is to eliminate inequalities across gender in wages for the same work. This law includes several instruments that are in place to enforce this ban of unequal pay. We focus on the pay information rights that are part of this law, which came into effect on January 06, 2018.

The pay information rights prescribe that employees in establishments with more than 200 employees working for the same employer can request information about the median (full-time equivalent) wage of an employee of the opposite gender doing comparable work. This comparison group has to comprise at least six individuals to prompt the provision of wage comparison. The request will be handled by the works council or the employer itself if no works council exists. Employees can use this right every two years or more frequently if working conditions substantially change.

The German wage transparency regulation differs in several aspects from wage transparency regulations implemented in other countries. First, workers have to actively ask their employer or works council to provide the information ('pay information right'). This is in contrast to transparency regulation implemented in e.g. Denmark, the U.K. or Austria ('pay reporting duties'). Second, employees receive a different type of information than in several other countries. Instead of receiving wage statistics that are aggregated at the company level, such as in Austria or the U.K., the employee can request wage information on a worker in a comparable position. This second point makes this transparency regulation particularly interesting to study in relation to wage negotiations; in contrast to company-wide wage statistics, wage information of an employee with a comparable task is an instrument that allows women to argue for a comparable wage.

On the one hand, there is some anecdotal evidence that this regulation has an impact on women's wages. For instance, a female head of department won a discrimination lawsuit in the Federal labor Court using information obtained through the wage transparency law.[6] On the other hand, survey data point to low uptake among employees in eligible firms (cf. fn 3). So far, no thorough analysis of the overall effects of this regulation exists.

3.2 Data description

Our primary data source stems from the German Institute for Employment Research (IAB). We utilize the Linked-Employer-Employee-Data of the IAB (LIAB). This employer-

[5]Source: Eurostat, 2022

[6]Source: Deutsche Welle, 2021

employee matched data set combines administrative data with an annual establishment survey. We observe the complete employment histories of 1,688,101 employees at firms surveyed in the IAB Establishment Panel, a representative sample of nearly 15,500 German establishments.

Our primary analysis will use only the administrative data on individuals and establishments from LIAB. This data encompasses employee-level demographic information, including age, completed education and whether the work was part-time. Data at the establishment level, including the total number of employees, are obtained from the linked Establishment-History-Panel (BHP). A detailed description of LIAB is available in Ruf et al. (2021).

The main analysis focuses on employment spells from 2011-2019. As we will exploit exogenous variation around the cutoff in firm size at 200 employees, we only use observations from firms with between 150 and 250 employees in 2018. For employment spells that did not last an entire year, we keep all observations that include the 30th of June, the date on which the size of firms is recorded. We discard all observations with a zero wage, indicating employment interruptions. This leaves 861,673 relevant observations from 241,372 individuals at 13,330 firms in our main sample. Table 1 reports summary statistics of this sample.[7] We observe that workers of the same gender in control firms are comparable to those in the treated firms in terms of age, education and the share of part-time workers.

We illustrate the observed gender differences in wages in the raw data in Figure A1. However, this gender pay gap could be the results of gender-specific occupational sorting, while there are no earnings differences for workers in comparable positions. As the information that employees receive concerns the wages of employees in comparable positions, this distinction is important. The LIAB data set does not allow us to identify this comparison group. It does contain information about the occupation of employees. We therefore estimate the gender pay gap within firm-occupation cells. We use three-digit occupation codes, distinguishing approximately 330 different occupations as a proxy for the relevant comparison group. The estimated gender wage gap within a firm-occupation cell is 13.08% in the pre-treatment year 2017, 5.58% if we include employee characteristics as controls. The regression results are provided in Table A2. Therefore, there is still a significant pay gap within firm-occupation cells.

One limitation of LIAB is the lack of administrative data on hourly wages. Instead, daily wages are calculated based on employer-reported fixed-period wages. The wage data is top-coded for individuals who earn more than the upper earnings limit for statutory pension insurance. In our main analysis, we do not take the censoring into account,

[7]Source DOI: 10.5164/IAB.FDZD.1906.en.v1, own calculations. We use these data for all results in Section 3.

	Men		Women	
	Large firms (1)	Small firms (2)	Large firms (3)	Small firms (4)
Daily Wage	94.05	94.32	75.22	72.41
	(50.51)	(52.91)	(45.22)	(43.23)
Age	41.27	41.29	42.72	42.54
	(12.64)	(12.71)	(12.45)	(12.63)
College educated	18.04%	18.08%	18.44%	17.67%
Part-time	15.03%	13.71%	48.61%	49.51%
Firms	4,746	7,743	4,301	6,935
Individuals	59,651	83,663	57,544	60,486
Observations	199,332	285,228	167,662	209,451

Notes: This table reports unconditional means and standard deviations in parentheses of key variables for individuals in large and small firms, split by gender. The descriptive statistics include all data in our panel from 2011 to 2019 in firms with 150 to 250 employees in 2018. 'Age' refers to the employee's age in years, 'College educated' is an indicator of whether the employee has at least some university or university of applied sciences education, and 'Part-time' is an indicator of whether the employee works part time.

Table 1: Summary statistics

but include a robustness check where all censored employment spells are discarded.[8] Although we do not know how many hours an employee worked per week, we do observe whether they worked full-time or part-time. We control for part-time workers in our main regression specifications.

Another limitation of LIAB concerns the fact that the data is limited by the inclusion in the IAB Establishment panel, while administrative data is available for a broader set of firms. Therefore, we complement our data analysis with a larger data set, as explained in 3.5.3. This allows us to obtain even more precise estimates. The downside of this second data set is the time window of observation, as it only includes data up to and including 2018. With the German transparency policy being introduced in January 2018, this second sample only contains one year of post-treatment outcomes. Therefore, we primarily use the smaller LIAB data set.

3.3 Identification strategy

We aim to estimate the impact of the wage transparency law on the gender wage gap in affected firms. Our identification strategy relies on the implementation of the wage transparency measure based on the size of the firm. We compare control firms just below the threshold with treated firms just above the threshold, using a difference-in-difference (Diff-in-Diff) analysis.

Equation 1 gives the main specification for the Diff-in-Diff approach.

[8] Censored observations constitute only 1.29 % of our main sample.

$$
\begin{aligned}
Y_{ijt} =& \beta_1(Female_i \times Large_j \times Post_t) + \beta_2(Female_i \times Post_t) + \\
& \beta_3(Large_j \times Post_t) + \beta_4(Female_i \times Large_j) + \alpha_i + \alpha_j + \alpha_t + \delta X_{ijt} + u_{ijt}
\end{aligned}
\tag{1}
$$

The outcome Y_{ijt} is the log of the daily wage of individual i, working in firm j in year t. $Female$ is a gender dummy, $Post$ is a dummy indicating whether the observation is from 2018 or 2019 (when the transparency law was active) and $Large$ is a dummy for firms with 200 or more employees in 2018. Note that the right to request wages of comparable workers was only in effect for firms where $Large \times Post$ is equal to one. Throughout the paper, we will use the size of firms, referring to the number of employees observed in 2018 to determine treatment assignment. In a robustness check, we will use the size in the pre-treatment year 2017 instead to avoid any manipulation of size around the cutoff. α_i, α_j and α_t denote individual-, firm- and time-fixed effects. X_{ijt} controls for individual characteristics that vary over time (age squared, education and whether the employee worked part-time).

To study the differential effect of the wage transparency legislation on men and women, we include an interaction between $Female$ and the treated group. We will also report results from gender-specific Diff-in-Diff regressions to evaluate the impact of the policy on male and female wages separately.

β_1 is the coefficient of interest, capturing the change in the gender wage gap in treated firms compared to control firms in the treated period. The main identifying assumption is the parallel-trends assumption. It assumes that the gender wage gap in firms with 200-250 employees evolves over time in the same way as the gap in firms with 150-199 employees (Olden and Møen, 2020). We use an event study to address the plausibility of the parallel-trends assumption in this setting. A difference-in-discontinuity (Diff-in-Disc) approach is used as an additional robustness check, as in Grembi et al. (2016).

3.4 Results

Table 2 reports the results from our Diff-in-Diff regressions. Overall, we find no effect of the wage transparency law on wages. The first three columns report results of regressions including employee-level time-varying controls. Column (1) gives the results from our main Diff-in-Diff specification. While we confirm that the gender pay gap is reduced in the post-treatment years compared to earlier years (see the coefficient for the $Female \times Post$ interaction; $p < 0.001$), this cannot be attributed to the wage transparency regulation. The coefficient associated with $Female \times Large \times Post$ (β_1 in equation 1) is statistically insignificant, with a point estimate that is indistinguishable from zero ($p = 0.992$). This indicates that the law did not have an effect on the gender pay gap. In other words, the gender wage gap in firms bound by the wage transparency policy did not change in the

	Log of daily wage					
	Both gender (1)	Men (2)	Women (3)	Both gender (4)	Men (5)	Women (6)
Large × Post	0.0022	0.0009	0.0027	0.0051	0.0044	0.0022
	(0.46)	(0.17)	(0.42)	(0.82)	(0.71)	(0.31)
Female × Large × Post	-0.0001			-0.0028		
	(-0.01)			(-0.36)		
Female × Large	-0.0249			0.0037		
	(-0.83)			(0.17)		
Female × Post	0.0146***			0.0046		
	(3.30)			(0.91)		
Individual time-varying controls	✓	✓	✓			
Firm FE	✓	✓	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
Firm size	150-250	150-250	150-250	150-250	150-250	150-250
Observations	584,026	325,869	257,544	778,441	435,591	342,066

Notes: Impact of transparency regulation on the gender wage gap and the wages of men and women individually. Estimates from difference-in-difference specification. Individual time-varying controls include age squared, education and part-time occupation. Includes observations from 2011 to 2019. Standard errors are clustered at the firm level. T-statistics in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Diff-in-Diff estimates of impact of wage transparency law on daily wages

treated period in a different way than the gender wage gap in the control firms.

Columns (2) and (3) show the impact of the transparency law on male and female wages separately. The coefficients of interest are small and not statistically different from zero ($p = 0.863$ and $p = 0.675$, respectively). We can rule out an impact of more than a 1.5% change in wage for either gender in the 95% confidence intervals. In the joint sample of men and women, we can rule out that overall wages changed by more than 1%. The last three columns show that the estimated impact remains close to zero when individual time-varying controls are omitted. Overall, we do not find any evidence of an economically significant impact of the wage transparency regulations on wages.

Next, we will consider whether the law is effective in sub-groups of the German labor force, specifically for employees (not) covered by sectoral bargaining agreement, and whether the regulation resulted in employees seeking alternative employment.

3.4.1 The role of collective bargaining agreements

Unions play a prominent role in German industrial relations through bargaining sector-level collective agreements with employer associations and there is evidence that unionization can affect the success of transparency legislation (Cullen and Pakzad-Hurson, 2021). Almost half of all employees in Germany were covered by collective agreements in 2016 (Ellguth and Kohaut, 2019). In our sample of firms with 150 to 250 employees, 70.18% of male and 76.16% of female employees were employed in establishments bound by sectoral or firm-level bargaining agreements in 2018.

An exception for firms bound by a collective bargaining agreement outlined in the transparency law warrants a subgroup analysis by collective bargaining status. If bound by a collective bargaining agreement, it is assumed that workers who perform comparable activities, as defined by being in the same salary scale, receive adequate payments. This so-called 'presumption of adequacy' also applies to firms which use existing sectoral agreements for orientation without being formerly bound by them and implies that the transparency regulation does not allow employees to obtain additional wage information. Furthermore, there is less scope for employees covered by collective bargaining agreements to bargain with their employers individually, as wages and working conditions are set collectively. Even non-union members working for companies subjected to collective wage agreements are generally granted the same benefits. Thus, the transparency law potentially only has an effect in firms that do not adhere to collective bargaining agreements.

We leverage information from the IAB establishment panel to analyze the impact of wage transparency on firms either covered or not covered by a sectoral or firm-level collective bargaining agreement. Using our preferred specification with individual controls, the Diff-in-Diff estimates of interest are not statistically significant, see Figure 1. Both in establishments covered by collective bargaining agreement, see Table A12 in Appendix C.3, and for establishments not covered by collective bargaining agreement, see Table A13 in Appendix C.3, there is no clear evidence of an effect on wages for men ($p = 0.914$ and $p = 0.853$, respectively) nor women ($p = 0.578$ and $p = 0.397$, respectively)[9]. In other words, there also no significant treatment effects for the sub-sample where we expect the transparency law to be important. These estimates are based on a smaller sample than our main results, as we could only match the collective bargaining status for about half of our main sample.

3.4.2 The effect on employment changes

So far, our results demonstrate that wages are not affected by the transparency law. More precisely, we show that the wages in firms with more than 200 employees do not change more after the introduction of the transparency policy compared to wages in firms with fewer than 200 employees. However, the wage transparency regulation may affect workers in other ways. In particular, we investigate whether this regulation impacts the propensity of employees to leave their current employment. If wage information reveals that an employee's compensation is lower than the comparable other's, the employee

[9]As an additional robustness check, we also include firms that use existing sectoral agreements for orientation in the pool of observations that are affected by collective bargaining, as these firms also benefit from the 'presumption of adequacy'. However, the data coverage for this measure is considerably lower, which implies that this analysis is only based on 69 firms that are not affected by collective bargaining, compared to 162 firms if we do not consider orientation towards sectoral agreements. We again find no consistent evidence in our preferred specification of a differential effect on wages by gender if affected by collective bargaining agreements ($p = 0.150$), or not ($p = 0.964$), see Table A12 and A13.

might be inclined to search for alternative employment.

We employ the same Diff-in-Diff regression specification as outlined in Section 3.3, Equation 1. Instead of using the log of the daily wage as the outcome variable, we define a binary variable that is equal to one if the employee leaves the establishment in which they are employed within one year and zero otherwise. We see in our preferred specification that neither male nor female employees are more likely to leave their employment due to the transparency regulation ($p = 0.611$ and $p = 0.857$, respectively). See Table A3 for the regression results.

3.5 Robustness checks

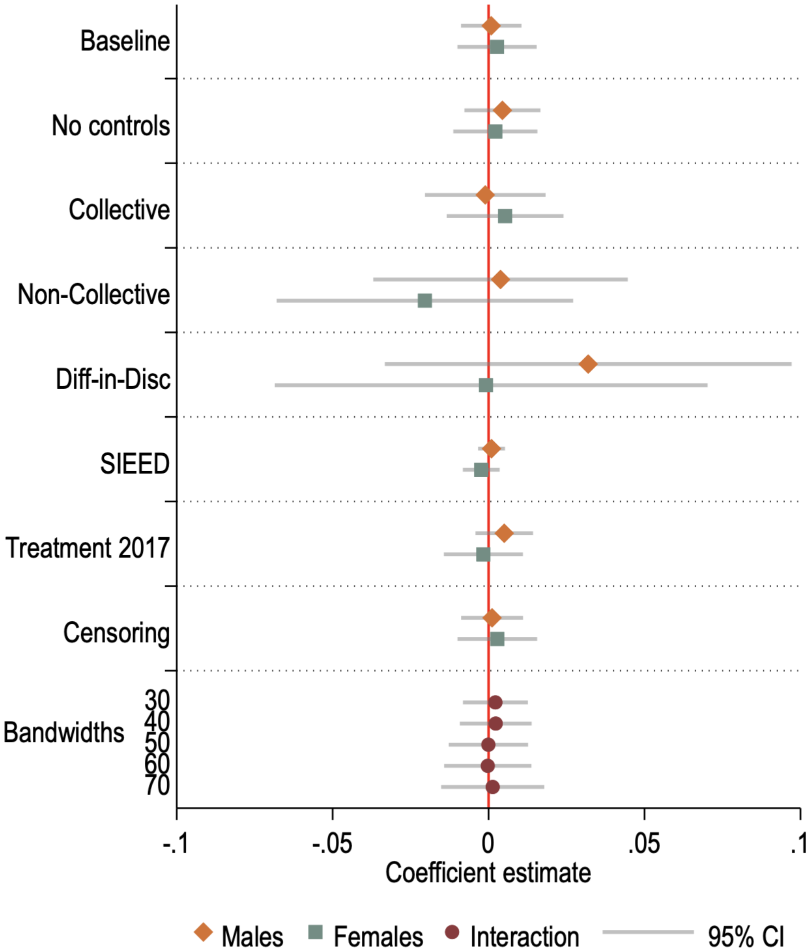
Using a Diff-in-Diff specification, Section 3.4 shows that the wage transparency law does not affect wages or the gender pay gap. In this section, we verify that our results are robust and not driven by the details of our specifications. Figure 1 provides a first overview of the coefficient estimates of our distinct analyses, demonstrating the robustness of our results. Next, we will lay out the specifics of the robustness checks that we perform.

3.5.1 Event study

First, we use an event study specification to evaluate the parallel trends assumption for the Diff-in-Diff specification. We estimate the following model, omitting 2017, the year prior to the reform:

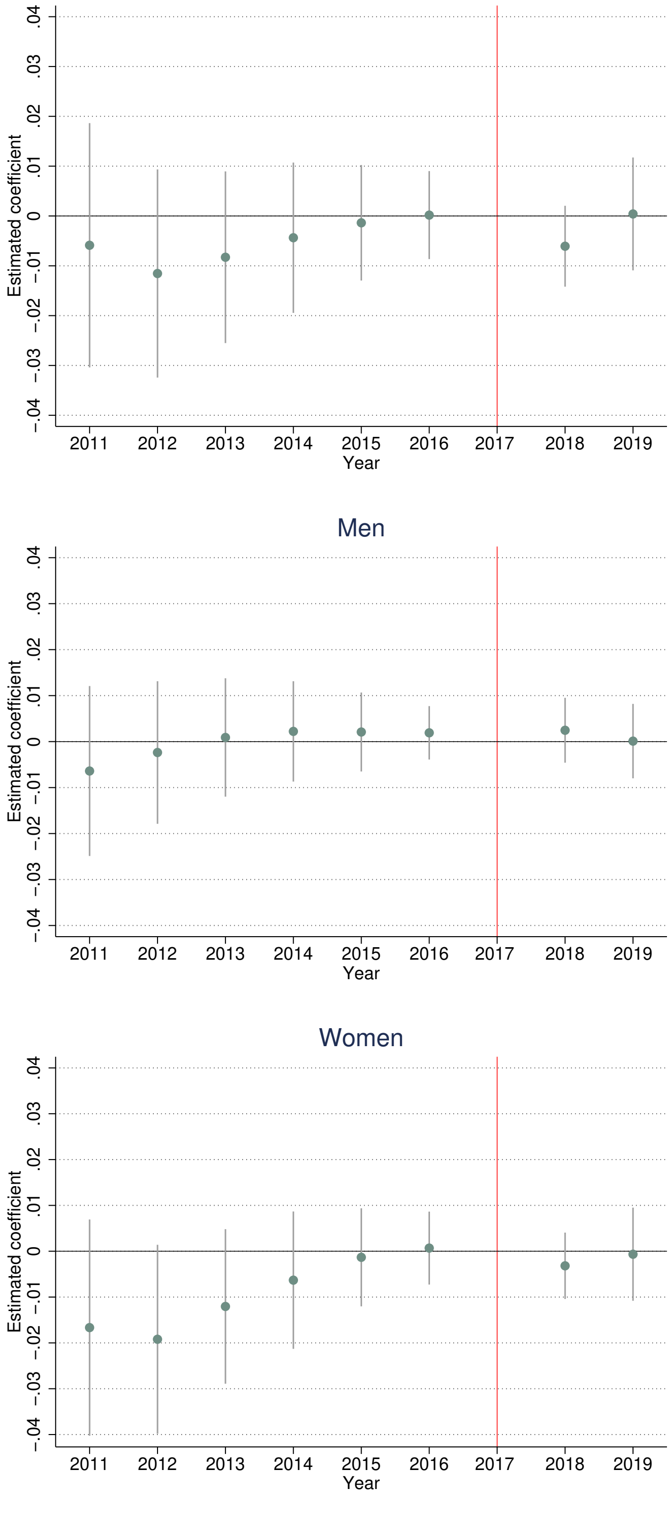
$$\begin{aligned} Y_{ijt} = &\sum_{k=2011}^{2019} \beta_k Female_i \times Large_j \mathbb{1}[t=k] + \sum_{k=2011}^{2019} \gamma_k Large_j \mathbb{1}[t=k] + \\ &\sum_{k=2011}^{2019} \pi_k Female_i + \alpha_t + \delta X_{ijt} + u_{ijt} \end{aligned} \tag{2}$$

If there are any pre-policy differences in trends between the treated and control firm, they will be captured by the coefficients β_k in pre-treatment years. The top panel in Figure 2 shows the estimated coefficients for β_k. We can see that the estimates are close to zero and do not seem to exhibit a trend in the period between 2011 and 2016, indicating support for the the parallel trends assumption. Furthermore, the estimated coefficients in the post-treatment periods are not statistically significant, suggesting that the transparency policy did not significantly impact the gender wage gap. We can exclude a treatment effect of more than 1.5 percentage points in our 95% confidence interval for both post-treatment years. The bottom two panels in Figure 2 display differences in wages in treated and control firms for men and women separately. These again indicate that the reform had no impact on the wages of either gender.



Notes: Coefficient estimates for the robustness checks outlined in Section 3.5. 'Baseline' refers to the estimates of the Diff-in-Diff regression in columns (2) and (3) of Table 2, 'No controls' to columns (5) and (6) of Table 2. 'Collective' gives the Diff-in-Diff estimates when the sample is restricted to employees covered by collective bargaining agreements in columns (2) and (3) of Table A12, 'Non-Collective' if the sample is restricted to employees not covered by these agreements. in columns (2) and (3) of Table A13. 'Diff-in-Disc' gives the estimates of the Diff-in-Disc analysis presented in columns (2) and (3) of Table A4. 'SIEED' gives the Diff-in-Diff estimates using the SIEED sample presented in columns (2) and (3) of Table A10. 'Treatment 2017' gives the Diff-in-Diff estimates if the number of employees in 2017 is used to determine treatment, see columns (2) and (3) in Table A5. 'Censoring' refers to estimates from the Diff-in-Diff analysis if top-coded observations are discarded, as in columns (2) and (3) in Table A6. 'Bandwidths' refers to the Diff-in-Diff estimates varying the bandwidths left and right of the cutoff, as presented in columns (1) to (5) in Table A7.

Figure 1: Overview of robustness checks



Notes: Event study analysis of the impact of wage transparency regulation on log daily wage. The top figure provides the estimates of the differential impact for women vs. men (β_k in Equation 2), the bottom two figures the yearly estimates of $Larger_j$ for separate event study specifications. Firms with more than 200 employees are classified as treated. Individual-, firm- and year-fixed effects are included. Time varying controls include age squared, education and part-time workers. 584,026 observations, including men and women. Error bars indicate the 95% confidence interval. Standard errors are clustered at the firm level.

Figure 2: Gender-specific effects of the transparency law

3.5.2 Difference-in-discontinuity

An alternative way to address potential biases from differential wage trends for small and large firms is using a Diff-in-Disc estimation introduced by Grembi et al. (2016). This methodology also allows us to control for the impact of any other policy changes at the threshold of 200 employees. In this alternative specification, we consider the following regression:

$$\begin{aligned} Y_{ijt} = & \beta_1 Size_j + Large_j \times (\gamma_0 + \gamma_1 Size_j) + \\ & Post_t[\delta_1 Size_j + Large_j \times (\lambda_0 + \lambda_1 Size_j)] + \alpha_t + \pi X_{ijt} + u_{ijt} \end{aligned} \tag{3}$$

$Size_j$ denotes the size of a firm in 2018. λ_0 is the Diff-in-Disc coefficient, which will be estimated separately for men and women. With the Diff-in-Disc estimator, we test whether the discrete jump at the cutoff when approaching from below compared to approaching from above is different for the treatment period compared to control periods. The key identifying assumption for a causal interpretation is the continuity of potential outcomes at the threshold of 200 employees.

Table A4 gives the results from our main Diff-in-Disc regression. The estimates for gender-specific difference-in-discontinuity coefficients are displayed in columns 2 and 3. The point estimate for the discontinuity in the male sample of 0.032 is statistically insignificant ($p = 0.337$), as is the point estimate for the female sample of 0.001 ($p = 0.981$). This result is also reflected when we interact the Diff-in-Disc estimator with a dummy for women (column (1) in Table A4), indicating that there are no gender differences in the treatment effect ($p = 0.487$). Overall, these results are qualitatively comparable but less precise than our main Diff-in-Diff specifications. The wage transparency law has no detectable effect on the gender pay gap.

3.5.3 Alternative data set

As a further robustness check, we conduct our primary analysis with a different, larger data set. For this, we use the Sample of Integrated Employer-Employee Data (SIEED) by the IAB. SIEED provides administrative data from the same data sources as in our primary analysis. It, however, covers 1.5% of all German establishments, which results in 1,842,584 relevant observations. This is substantially more than in our primary analysis. This larger data set allows us to obtain more precise estimates.

As of 2022, SIEED only includes one post-treatment year. This limits the meaningfulness of the results obtained with this data set, since the initiation of wage negotiations and the accompanying use of wage information might take some time. It is conceivable that we do not observe any impact because the availability of wage information only affects wages in later years. Thus, we do not use the SIEED as our primary sample.

We provide summary statistics and reproduce our results from Section 3.4 in Appendix C using SIEED. Both the Diff-in-Diff and Diff-in-Disc results are in line with the findings we presented previously, see Tables A10 and A11. The event study in Figure A3 underlines this. As Figure 1 shows, the wage transparency regulation neither significantly affects wages of women ($p = 0.435$) nor men ($p = 0.666$) in 2018. This sample allows us to rule out an effect of more than 1% on the wages of either gender.

3.5.4 Alternative regression specifications

We classify whether employees in firms have a right to wage information by the number of employees a firm had in 2018. However, if firms selectively manipulate their size in 2018 around the policy cutoff, the effect estimated in the previous section would be biased. A McCrary test for the continuity of the density of the variable $Size_j$ around the cutoff of 200 employees in 2018 provides no evidence of manipulation ($p = 0.712$). We illustrate the smoothness of the density around the cutoff in Figure A2. Nevertheless, we use the size of firms in the year prior to the reform as a proxy for treatment to calculate an intention-to-treat effect. Table A5 in the appendix shows the main outcomes of a Diff-in-Diff analysis using this alternative treatment assignment. The estimates are not significantly different from the main results presented in the last section and do not indicate any treatment effect on male or female wages in our main specification, see also Figure 1.

Wages in our sample are censored, as wages above the upper earnings limit for statutory pension insurance are top-coded. In our main specification, this only affects 1.82% of observations. We address censoring in Appendix C. Here, we discard all top-coded employment spells from our analysis. Independent of the exact specification, we also do not observe a significant impact of the wage transparency regulation if we remove top-coded observations. Table A6 provides an overview of our Diff-in-Diff analysis on this restricted sample.

Finally, to check whether our conclusions are sensitive to the chosen bandwidth, we provide additional robustness checks with different bandwidths in Appendix C. These confirm our main specification, as Figure 1 illustrates. In particular, we include specifications in the range of the optimal bandwidth selected by the data-driven method introduced by Calonico et al. (2020). This does not change our estimates in any meaningful way.

4 Experiment

Section 3 shows that the German wage transparency law has to date been unsuccessful in reducing the gender pay gap. We now explore potential drivers of this lack of success. Our online laboratory experiment studies the determinants of and potential barriers to a successful wage transparency policy. In this, we focus on how wage transparency can induce changes in beliefs about average wages and the consequences for wage inequality.

First, in Section 4.1, we pin down the intuitive arguments in favor of wage transparency as a tool to decrease the gender pay gap and analyze how its effectiveness may depend on the presence of performance information. This theoretical model will provide predictions for the experiment. Next, we outline the experiment designed to test how the endogenous nature of wage information and the environment in which wage information is available impacts the success of wage transparency regulation in Section 4.2 and discuss the results in Section 4.3.

4.1 Theoretical predictions

In this subsection, we examine why and when wage transparency could help decrease the gender wage gap and provide theoretical predictions for our online laboratory experiment. Assume a worker i bargains for a wage w_i with a firm j. In these negotiations, the worker and firm split a pie π between themselves. The worker believes he or she can contribute $\hat{c}_i$ to the firm. The worker further believes that the firm pays comparable workers, that is, workers performing comparable tasks, an average wage of $\hat{\bar{w}}_i$. He or she believes that the average contribution of the comparable workers to the firm is $\hat{\bar{c}}_i$. Consider worker preferences represented by utility $U_i^W(\mathbf{w}, \mathbf{c})$:

$$U_i^W(\mathbf{w}, \mathbf{c}) = w_i - \alpha_i \left(\frac{w_i}{\hat{c}_i} - \frac{\hat{\bar{w}}_i}{\hat{\bar{c}}_i} \right)^2$$

α_i measures a worker's aversion to perceived unfair payment. We define perceived unfair payment as a worker's belief that he or she receives a different piece rate $(w_i/\hat{c}_i)$ than comparable workers $(\hat{\bar{w}}_i/\hat{\bar{c}}_i)$. The worker is therefore not concerned with wage inequalities per se, but holds the meritocratic ideal that the same contribution should result in the same wage.[10] The firm's objective $U_j^F(w_i)$ is to minimize the wage to the worker:

$$U_j^F(w_i) = \pi - w_i$$

For simplicity, we assume that both worker and firm have an outside option of $d^F = d^W = 0$.

The wage w_i is part of the Nash bargaining solution if it solves the following optimization problem:

[10]This definition of an unfair wage is in line with the literature on fairness ideals that demonstrates that the source of an inequality matters for its acceptability. Inequalities that are based on merit are more likely to be deemed acceptable, see e.g. Konow (2000), Cappelen et al. (2007) and Almås et al. (2020).

$$\begin{aligned}
\max_{w_i} \quad & \left(w_i - \alpha_i \left(\frac{w_i}{\hat{c}_i} - \frac{\hat{\bar{w}}_i}{\hat{\bar{c}}_i}\right)^2\right)(\pi - w_i) \\
\text{s.t.} \quad & w_i \geq 0 \\
& \pi \geq w_i
\end{aligned}$$

In the absence of information on wages and contributions, a worker's beliefs about his or her contribution and the piece rate of comparable workers, captured by $\hat{c}_i$ and $\hat{\bar{w}}_i/\hat{\bar{c}}_i$, respectively, do not necessarily correspond to the true values, c_i and $\bar{w}/\bar{c}$. Assume that there are two types of workers, a pessimistic and an optimistic type. The first type, type F, has pessimistic beliefs $\hat{c}_i$ about his or her own contribution. The second type, type M, has optimistic beliefs. Type F also has pessimistic beliefs $\hat{\bar{w}}_i$ about the average wages, while M has optimistic beliefs.

Providing information on the true values of c_i and $\bar{w}/\bar{c}$ can shift beliefs. In particular, when receiving information about the true values c_i and $\bar{w}/\bar{c}$, F will update his or her beliefs about c_i and about $\bar{w}/\bar{c}$ positively, type M negatively.

To analyze the impact of belief shifts on wages in the Nash bargaining solution, we first posit that information on the average wage of comparable others only affects beliefs about exactly this average wage of others, $\bar{w}$, and not beliefs about the average contribution $\bar{c}$. Correspondingly, information on the average performance of comparable others does not affect beliefs about the average wage of others. Realize that this is not a trivial assumption. If a worker learns that others earn more than expected, s/he could reasonably infer that this higher pay may be a reward for higher than expected contributions. Unexpectedly high contributions may be perceived as an indication that wages are also higher than expected, to compensate. We will later relax this assumption.

Let w_i^* define the Nash bargaining solution. Inducing a shift in beliefs affects the w_i^*. We show in Appendix A that the Nash bargaining solution has the following properties: w_i^* (1) increases in $\hat{c}_i$, (2) decreases in $\hat{\bar{c}}_i$, and (3) increases in $\hat{\bar{w}}_i$. Intuitively, an increase in $\hat{c}_i$ implies that the own perceived piece rate relative to the comparable workers' decreases, which can be compensated by an increase in w_i. In contrast, if beliefs about comparable workers' average contributions $\hat{\bar{c}}_i$ increase, this entails a decrease in the perceived piece rate of comparable workers. To counteract the perceived inequality in piece rates, w_i needs to decrease. Last, if beliefs about the average wages of others increase, the reverse holds true. The perceived piece rate of comparable workers increases, which a higher w_i can counterbalance.

To derive testable hypotheses from this model, we assume that women are more frequently of the F type, and men more frequently of the M type. As discussed in the introduction, there is some empirical support for this assumption. Men are more confident in their own abilities (see e.g. Niederle and Vesterlund, 2007) and have more optimistic beliefs about average and future wages (Briel et al., 2021). Using this classification, the model permits the following hypotheses, for which we provide the theoretical proofs in Appendix A:

Hypothesis 1. *Providing information about a comparable worker's wage decreases the gender wage gap.*

The change in beliefs $\hat{\bar{w}}_i$ in response to information on $\bar{w}$ will be negative for type M and positive for type F. Since w_i^* increases in $\hat{\bar{w}}_i$, this implies that the wage of women will respond positively to information about a comparable worker's wage, but negatively for men, decreasing the gender wage gap. A similar reasoning leads to the next hypothesis.

Hypothesis 2. *Providing information about a worker's own performance relative to the comparable worker's performance decreases the gender wage gap.*

Given their pessimistic beliefs about their own compared to others' performance, women's beliefs react positively to information on the true value of c_i compared to $\bar{c}_i$. As w_i^* increases in $\hat{c}_i$, revealing the true value c_i compared to $\bar{c}_i$ induces a positive change in the wages of women, at the same time a negative effect is expected for men.

For our next hypothesis, we relax the assumption that information on average wages and contributions of comparable others cannot affect beliefs about average contributions and wages, respectively. Instead, we propose that if the average wage is higher than expected, $\hat{\bar{c}}_i$ will increase. If the average contribution is higher than expected, $\hat{\bar{w}}_i$ will increase. Workers thus expect that there is a positive correlation between the contributions and wages of other workers. For simplicity, we assume that this correlation is the same for types F and M. As a result, the effect of wage information on beliefs about the average piece rate of comparable workers $\bar{w}/\bar{c}$ is now smaller in absolute terms. We will continue to assume that the effect of positive information on $\bar{w}$ as well as negative information on $\bar{c}$ positively affects beliefs about $\bar{w}/\bar{c}$. Intuitively, if a worker learns about higher than expected wages of others, he or she will not decrease beliefs about the average piece rates.

With this more realistic assumption, the arguments brought forward in favor of Hypotheses 1 and 2 are still valid. However, the effects will be less pronounced. In turn, providing information on $\bar{c}$ and $\bar{w}$ simultaneously now distinctively impacts w_i^* in the Nash bargaining solution. Specifically, if both the true values of $\bar{c}$ and $\bar{w}$ are communicated to the worker, there is no adverse effect that reduces the impact on w_i^* of providing this information. Holding the true values $\bar{c}$ and $\bar{w}$ and prior beliefs about these values constant, the effect of providing information on $\bar{c}$ and $\bar{w}$ jointly on $\hat{\bar{w}}_i/\hat{\bar{c}}_i$ is stronger than the aggregate effects of providing information on $\bar{c}$ and $\bar{w}$ separately. As a consequence, given that w_i^* decreases in $\hat{\bar{c}}_i$ and increases in $\hat{\bar{w}}_i$, the effects on w_i^* are stronger when information is provided jointly. This informs our next hypothesis.

Hypothesis 3. *Providing information about a comparable worker's wage and relative performance jointly has a stronger effect on wages than providing this information separately.*

The intuitive implication is that workers cannot use wage information as effectively if they do not know about the corresponding contribution. Higher wages of others can be attributed to higher contributions, which warrant only a smaller increase in the wage of the worker him- or herself to match piece rates.

Our type classification implies that moving to a joint provision of wage and contribution information will benefit women more. This follows from the fact that type F individuals receive on average information that can shift their beliefs $\hat{c}_i$ and $\hat{\hat{w}}_i$ upwards, while it shifts these beliefs downwards for type M. If, however, part of this effect is offset by a change in the respective other belief, this diminishes the differential change in $\hat{\hat{w}}_i/\hat{\hat{c}}_i$ between F and M types. Therefore, F types benefit to a larger extent from joint information provision.

So far, we have interpreted potential differences in the use of information in terms of gender differences. However, we can also utilise the bargaining model to make predictions about the effect of information provision on the wages of type M versus type F using the classification based on beliefs, not gender. In this case, we do not require that the assumptions on male versus female beliefs hold true in our subject pool. Instead, in the analysis, we can classify the subjects based on beliefs and check whether information reduces wage differences between types F and M, irrespective of gender.

4.2 Experimental design

Our experiment mimics wage negotiations between a firm and a worker, varying whether and how wage information is provided and whether performance information is given.[11] The experiment consists of two main parts with four periods each. At the start of the experiment, participants are assigned to matching groups of eight. Four are assigned to be a worker, four to be a firm.[12] In each period, one worker is matched with one firm. After each period, subjects are re-matched. We employ a perfect stranger matching within parts. Between parts, the same groups of workers and firms are re-matched.

Figure 3 provides an outline of the experimental stages. At the end of the experiment, we elicit risk aversion using the Holt and Laury (2002) multiple price list, and subjects fill in a short questionnaire. We provide the experimental instructions in Appendix E.

[11] We pre-registered on the AEA RCT Registry (Brütt and Yuan, 2021).

[12] Our matching procedure ensures that men and women are distributed as equally as possible to the worker and firms roles within a matching group. The workers were gender balanced. 6 workers did not self-report their gender, or reported "other". We classify the gender of these workers based on the administration data from the laboratories.

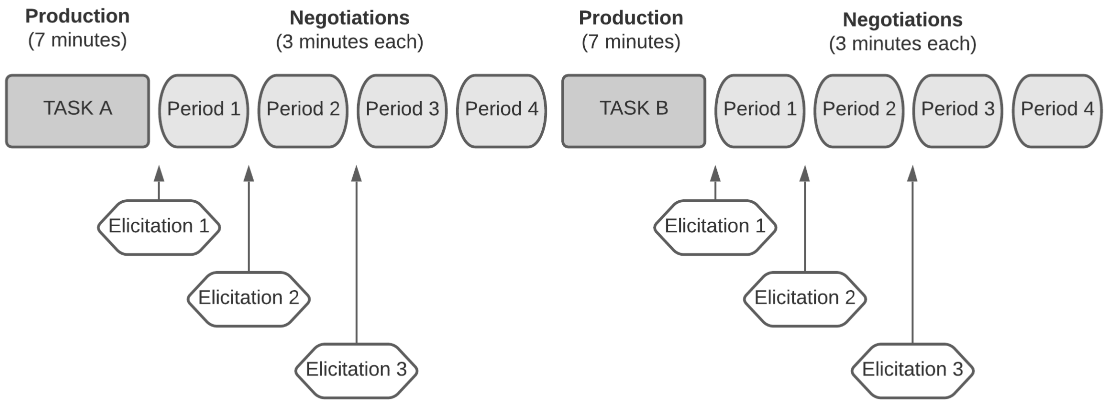


Figure 3: Experimental outline

4.2.1 Production stage

At the start of part 1 and part 2, there is a production stage. In the production stage, workers and firms produce a budget that can be allocated between them in the negotiations. The budget is the sum of the worker's contribution and a fixed firm contribution. Each firm contributes a firm-specific constant to the budget, which is a number drawn from a uniform distribution between 300 and 450 points. This constant is fixed within a part, but re-drawn for each of the two parts.

The worker's contribution is determined in a part-specific production task. The performance in this task determines the worker's contribution to the budget. Workers have to solve as many elements as possible within seven minutes in both tasks. In one part, workers have to produce in the maze task, in the other part in the matrix task.[13] We counter-balance the order of the tasks.

In the maze task, first used in Gneezy et al. (2003), workers have to navigate through mazes on their computer screen. We count the number of mazes they navigate successfully. In the matrix task, introduced by Weber and Schram (2017), workers have to find and then sum up the highest numbers from two matrices with 49 two-digit numbers each. We count the number of correct additions. For each correctly solved element in the production stage, the budget that can be split during negotiations increases by 35 points (for the matrix task) or 20 points (for the maze task).

Both tasks are chosen to be stereotypically male. While studies typically show little evidence for gender differences in the performance in these tasks, spatial reasoning and mathematical skills are often perceived to favor men (Sanchis-Segura et al., 2018).[14] We

[13]While firms cannot produce any output that is added to the budget in the production task, firms also experience the production stage to form an accurate impression of how the worker's contribution is generated.

[14]Studies such as Gneezy et al. (2003) and Schram et al. (2019) report no significant gender differences in performance with non-competitive payment and without status ranking, respectively. In an incentivized pre-study run with 100 participants on Prolific, we confirm that these tasks are indeed perceived

choose stereotypically male tasks to create an environment where gender differences in wages are likely to emerge from negotiations due to differences in beliefs as described in Section 4.1.

4.2.2 Negotiation stage

In the first period within a part, all workers enter negotiations[15]. In subsequent periods, workers first unilaterally decide whether they want to enter negotiations. If they do not enter negotiations, workers receive an outside option of 150 points, the remainder is allocated to the firm.[16]

If negotiations occur, workers and firms first submit an initial, non-binding wage proposal. This wage proposal is shown to their negotiation partner during negotiations. Afterward, they enter a three-minute, free-form chat. This stage mirrors the negotiation setup in Exley et al. (2020). Next to the chat, participants can submit and accept wages in a separate field. To agree on a wage, either the worker or the firm has to accept the other side's wage proposal. If the worker and firm agree on a wage, the worker receives this wage and the firm the remainder of the budget. If there is no agreement, both receive zero points.

During the negotiations, only the firm knows the size of the budget that can be split between worker and firm. This allows firms to avoid offering the focal point of an equal budget split. We furthermore do not disclose the exact size of the firm's fixed contribution to the firm or worker. In this way, firms cannot reveal the worker's contribution in the chat in treatments without performance information.

This negotiation stage is repeated three times after the first period in each part, with re-matching after each period.

4.2.3 Treatments

In a 3×2 design, we manipulate the information provided during the negotiations along two dimensions, wage information and performance information.

Wage information We vary the provision of wage information between-subjects. Wage information refers to the wage of a 'comparable worker'. We define a worker's comparable worker as the worker who was paired with the current worker's firm in period one. The

to favor male participants. See Appendix B for details.

[15]This ensures that the wage we observe of a comparable worker (see Section 4.2.3) for all subsequent periods is determined by wage negotiations, not an outside option.

[16]This outside option is set such that even if firms receive the lowest possible draw as their fixed contribution and the worker produces no output, an equitable split would still result in a wage that corresponds to the outside option for the worker. Thus, workers can expect that it is beneficial to enter negotiations.

wage of the comparable worker is comparable in two dimensions. First, as the comparable worker's wage refers to the wage that this worker received in the same part, s/he performed the same task. Second, both workers were paired with the same firm for the wage concerned.[17]

The three between-subject treatments differ in the availability of wage information. The baseline treatments do not provide wage information (*NoWage* treatments), representing the scenario without wage transparency regulation. The second type of treatments provide wage information endogenously (*EndoWage* treatments). Here, workers face the choice of receiving wage information before deciding on whether to enter wage negotiations. Acquiring wage information costs 10 points[18]. The information choice is communicated to the firm. In these treatments, we mimic the wage transparency regulation in Germany, which requires employees to approach their employer in order to acquire wage information. The third type of treatments provide wage information exogenously (*ExoWage* treatments). In contrast to the *EndoWage* treatments, workers here do not face the choice of acquiring wage information. Instead, this is provided for free before the negotiation entry decision. These treatments are closer to a setting where the duty of providing information lies with the employer.

As wage information is created in period one of each part, workers cannot obtain wage information in this period. Treatments, therefore, only differ in periods two to four of each part.

Performance information The treatments *Performance* and *NoPerformance* vary whether information about both own performance and the comparable worker's performance is provided. This variation occurs within-subject; participants face the *Performance* treatment in one part and the *NoPerformance* treatment in the other part. The order of the within-subject treatments and the combination of performance information and working on a specific task are counter-balanced.

4.2.4 Belief elicitations

We elicit workers' beliefs about performance and wages at several points during the experiment.[19] First, after each part's production stage, we elicit beliefs about the participant's own performance and the part's comparable worker's performance (*Elicitation 1*). We

[17]The German wage transparency law mandates that employers provide information about the median comparable worker, while we provide wage information on one worker and not the median wage of all previously matched workers of a firm. We opted to provide the same information in all periods to keep the informational value constant across periods. We can interpret the information that is provided as a signal of the wage of the median worker.

[18]This small but non-negligible cost ensures that we observe whether participants have a strict preference for receiving wage information.

[19]Aside from studying belief updating about performance and wages, we can also utilise the elicitations to classify participants into the types described in Section 4.1.

ask subjects to estimate how many elements were solved correctly. Second, after each part's first negotiation period, we elicit workers' beliefs about the comparable worker's wage (*Elicitation 2*). Third, there are treatment- and choice-contingent elicitations after the second negotiation period in each part (*Elicitation 3*). In treatments and periods without wage information but with performance information, we re-elicit a worker's belief about the comparable worker's wage. Similarly, we re-elicit performance beliefs in treatments and periods without performance information but with wage information.

We elicit beliefs using the binarized scoring rule (Hossain and Okui, 2013). The subjects' estimates are transformed via a quadratic loss function into a probability to win a prize of three Euros.[20] See Appendix E for the detailed procedures and instructions.

4.2.5 Experimental procedures

The experiment was conducted online in 24 sessions in May and June 2021, with participants from the subject pools of the CREED laboratory of University of Amsterdam in the Netherlands and the MELESSA laboratory of Ludwig Maximilian University of Munich in Germany. We recruited 528 subjects, 264 each from CREED and MELESSA. We collected observations from 22 matching groups per between-subject variation, eleven from CREED and MELESSA for each between-subject treatment. twotwo.

Recruiting participants for online experiments from subject pools of university laboratories ensures that participants are aware that practices commonly used at the laboratory, such as no deception, will also apply online. Furthermore, drop-out rates are low even in long experiments.[21] Participants had to correctly answer all comprehension checks about the experimental instructions before starting the experiment.

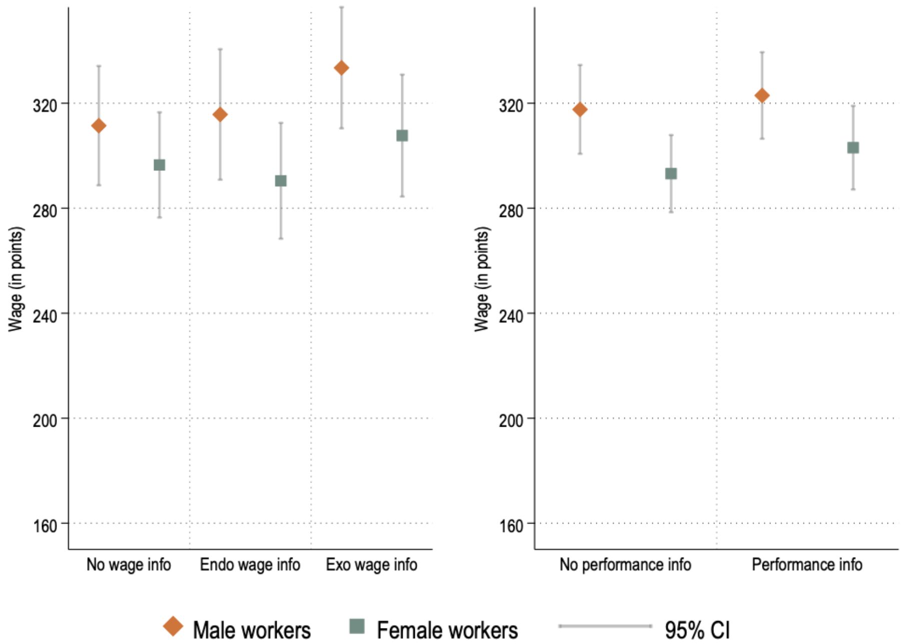
At the end of the experiment, point earnings were exchanged for Euro at a rate of one Euro per 25 points. We pay one randomly chosen period from one randomly chosen part, one randomly chosen belief elicitation for the workers, and the risk aversion elicitation. Subjects receive a show-up fee of six Euros and a fee of four Euros for filling out the questionnaire. On average, participants earned 26.59 Euros and the experiment lasted 88 minutes.

4.3 Experimental results

As outlined in the pre-analysis plan, we only consider negotiations after the first period, when there is a treatment variation in the available information. In the parametric

[20] In line with recent findings by Danz et al. (2020), we withhold information about the exact incentive structure of the binarized scoring rule to limit biased reporting. Instead, we state that subjects maximize their chance of winning the prize by providing their true beliefs. Subjects can receive more detailed information on the mechanism if they actively request this.

[21] In our experiment, only two participants dropped out after the experiment started. In total, observations from 36 periods had to be discarded from the analysis due to subjects experiencing technical difficulties. This amounts to 2.22% of the data.



Notes: Comparison of mean wages by gender, varying wage information (left) and performance information (right). Error bars indicate the 95% confidence interval. Standard errors are clustered at the matching-group level.

Figure 4: Treatment comparison of gender differences in wages

analysis, we will include controls for the worker's and firm's contributions, and laboratory, period and part fixed effects to test our hypotheses. To account for the dependence of observations within a matching group, we cluster standard errors at the matching group level. When comparing raw means, we will use permutation t-tests (*PmtT-test*).[22]

In the following sections, we will first discuss how wages and gender wage differences are affected by wage and performance information, then turn to the effects on negotiation entry. Subsequently, we will take a closer look at the empirical validity of the mechanisms suggested in Section 4.1.

4.3.1 The effect of transparency on wages

Figure 4 provides an overview of the average wages by gender in each wage-information treatment. Note that the worker's wage is equal to the outside option of 150 points if he or she did not enter negotiations. If the worker entered negotiations, it is equal to the agreed upon wage, minus the incurred costs of wage information in treatments with endogenous wage information.

[22]Here, we will average observations on an individual level or, for the comparison of wage differences, on a matching-group level. Permutation t-tests are more powerful than traditional t-tests (Moir, 1998; Schram et al., 2019).

The effects of wage information The left panel of Figure 4 illustrates wage differences by gender and wage-information treatment. Table 3 presents regression results. Including the described control variables and fixed effects, we employ a linear regression of the worker's wage on dummy variables for the wage information treatments (*EndoWage* and *ExoWage*), columns (1) and (2), an indicator for the worker being female, column (3), and the fully interacted variables of the worker's gender and treatment indicators, column (4).

We confirm in the laboratory that overall, wages are not significantly affected by the introduction of a wage transparency policy that requires workers to ask for wage information ($p = 0.643$; regression (1) in Table 3). On average, workers earn a wage of 303.98 points in *NoWage* and 302.95 points in *EndoWage* (*PmtT-test*; $p = 0.9159$). Overall, workers pay for wage information in 47.57% of the decisions. 83.91% of workers request wage information at least once. This documents a substantial demand for wage information if the associated monetary costs are low. Nevertheless, workers do not benefit from the introduction of the type of wage transparency policy that resembles the law discussed in Section 3.

Compared to these two treatments, the introduction of exogenous wage information in *ExoWage* has a positive, albeit only marginally significant, effect on wages ($p = 0.076$; regression (2) in Table 3). We estimate that exogenously provided wage information increases the workers' wages by 14.65 points. In *ExoWage*, workers earn 320.78 points, 6% more than in the other two wage-information treatments (*PmtT-test*; $p = 0.067$).

This suggests that the accessibility of wage information indeed matters. Note that we only implement a small cost of 10 points for obtaining this information in *EndoWage*. Yet, this treatment shows virtually identical outcomes for workers compared to *NoWage*. This is in line with the notion that providing this information only on request is a barrier to the utilization of wage information. Possible reasons are fear of backlash or wrong perceptions about its usefulness, which may limit uptake. In Section 4.3.3, we will discuss this second potential reason. Workers seem to take advantage of wage information only when it is provided exogenously.

Next, we consider gender-specific effects. Male workers earn on average a wage of 320.27 points in our experiment, female workers significantly less at 298.11 points (*PmtT-test*; $p = 0.029$). In regression (3) of Table 3, we see that this gap in our experiment disappears if we control for the worker's and firm's contribution ($p = 0.733$).[23] We can nevertheless study whether the treatments have a differential impact on male and female workers. In particular, our experiment provides a setting where women, on average, are paired with a comparable worker who obtained a wage that is 16.78 points higher than their own wage. In comparison, men face comparable workers who obtained a wage that

[23] The worker's contribution was about 10% lower for female workers in both the maze task (*PmtT-test*; $p = 0.005$) and the matrix task (*PmtT-test*; $p < 0.001$).

is 16.38 points lower than the average male worker's wage. This difference is significant (*PmtT-test*; $p = 0.018$). Gender differences in wages induce gender differences in the information that is provided. Furthermore, women are significantly more pessimistic about the wage of the comparable worker than men (*PmtT-test*; $p = 0.067$). Therefore, wage information has the potential to shift women's beliefs to a larger extent.

As in the field, a wage transparency policy that requires workers to ask for wage information themselves (in *EndoWage*) does not have a differential effect on male and female workers. It does not reduce the unconditional gender pay gap compared to the *NoWage* treatment without any wage information ($p = 0.948$; regression (4) in Table 3). In *NoWage*, male workers earn 5% more than female workers, in *EndoWage* 9% more. These wage gaps are statistically indistinguishable (*PmtT-test*; $p = 0.713$).

Our results so far confirm the findings from the field. As a next step, we want to study whether removing the barrier to wage information alleviates its ineffectiveness in our setting. We do not find any support for this. The unconditional gender wage gap in treatment *ExoWage* amounts to 8%, which is no reduction compared to the wage gap in *NoWage* (*PmtT-test*; $p = 0.671$) and similar to the gap in *EndoWage* (*PmtT-test*; $p = 0.976$). Together, our results provide evidence that in our context a move to more accessible wage information does increase overall wages, but this effect is not gender specific ($p = 0.760$; regression (4) in Table 3). Therefore, the accessibility of wage information on its own does not lead to a reduction in the gender pay gap.

Since our experimental design isolates the effect that wage transparency regulation has by changing beliefs about wages and the role of these beliefs in bargaining, we can conclude that there is no evidence in favor of this channel leading to a reduction in the gender pay gap. This holds irrespective of how wage information is provided. We therefore cannot reject the null hypothesis of no effect of wage information on the gender wage gap in favor of Hypothesis 1. Section 4.3.4 discusses the interaction of beliefs and information provision in more detail.

The effects of performance information The right panel of Figure 4 depicts wages by gender and performance-information treatment. In Table 3, we provide results of a linear regression of the worker's wage on a dummy variable for the performance information treatment (*Performance*), column (5), and the fully interacted variables of the indicator *Performance* with the indicator of the worker being female, column (6).

Overall, the workers' wages are slightly higher if they know their performance and the comparable worker's performance. Workers earn 312.99 points with performance information compared to 305.45 points without this information. While this difference is small, it yields a significant effect of performance information on wages in our parametric specification ($p = 0.039$; regression (5) in Table 3). We estimate that providing performance information increases the workers' wage by 10.73 points. This suggests that a

	Worker's wage							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Worker contribution	0.55***	0.55***	0.55***	0.55***	0.55***	0.55***	0.55***	0.55***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Firm contribution	0.24**	0.24**	0.23**	0.24**	0.24**	0.24**	0.25**	0.25**
	(0.09)	(0.09)	(0.09)	(0.09)	(0.10)	(0.10)	(0.09)	(0.10)
Endo wage	-4.92			-5.44			-1.40	-5.02
	(10.57)			(14.05)			(13.08)	(17.74)
Exo wage	12.18	14.65*		14.55			15.33	22.13
	(9.13)	(8.13)		(12.48)			(11.96)	(16.09)
Female			1.99	3.38		4.74		7.09
			(5.82)	(12.34)		(8.98)		(18.05)
Endo wage × Female				0.95				7.00
				(14.52)				(22.17)
Exo wage × Female				-4.80				-13.88
				(15.67)				(22.49)
Performance					10.73**	13.45*	15.25*	19.05
					(5.08)	(7.57)	(8.63)	(13.25)
Performance × Female						-5.43		-7.67
						(10.13)		(20.44)
Performance × Endo wage							-7.14	-1.19
							(12.34)	(18.45)
Performance × Exo wage							-6.32	-15.39
							(12.58)	(18.59)
Performance × Endo wage × Female								-11.70
								(25.31)
Performance × Exo wage × Female								18.53
								(25.94)
Constant	-0.44	-3.27	2.88	-2.92	-2.96	-6.27	-9.80	-14.16
	(35.10)	(34.11)	(35.77)	(37.43)	(35.92)	(37.10)	(34.78)	(38.10)
Part FE	✓	✓	✓	✓	✓	✓	✓	✓
Period FE	✓	✓	✓	✓	✓	✓	✓	✓
Laboratory FE	✓	✓	✓	✓	✓	✓	✓	✓
Observations	1548	1548	1548	1548	1548	1548	1548	1548
Clusters	66	66	66	66	66	66	66	66
R-squared	0.265	0.265	0.262	0.265	0.264	0.264	0.267	0.268

Notes: Results are from ordinary least squares regression of the worker's wage. Worker contribution is a control for the worker's contribution to the negotiation pie, Firm contribution for the firm's contribution to the negotiation pie. Endo wage and Exo wage are indicators of whether wage information was provided endogenously or exogenously, respectively. Female indicates whether a participant is female. Performance is an indicator of whether information of the workers' performances is provided. Standard errors are clustered at the matching-group level and shown in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: The effect of wage and performance information on wages

worker's bargaining power increases if the informational asymmetry between worker and firm is reduced. We also observe that workers receive wages that better reflect their performance in treatment *Performance*. In the presence of performance information, workers receive 0.14 points more for every point they have contributed to the negotiation budget ($p = 0.003$; regression (2) in Table A14).

As observed for wage information, performance information also has no significant effect on the gender pay gap in our experiment ($p = 0.593$; regression (6) in Table 3). Female workers do not exploit their knowledge of their relative performance in negotiations more than male workers do or vice versa. Therefore, we cannot reject a null effect of performance information on the gender wage gap in favor of Hypothesis 2.

The effects of a joint provision of wage and performance information We concluded that exogenously providing wage and performance information both have a small, but significant effect on wages. Now, we want to study whether the joint provision of these two types of information can enhance their effectiveness. This would point to wage transparency regulation working better in environments where performance is easily observable. In Table 3, we provide the fully interacted model including indicators of the treatments *Performance* with *EndoWage* and *ExoWage*, column (7), also including interactions with the indicator of whether the worker is female in column (8).

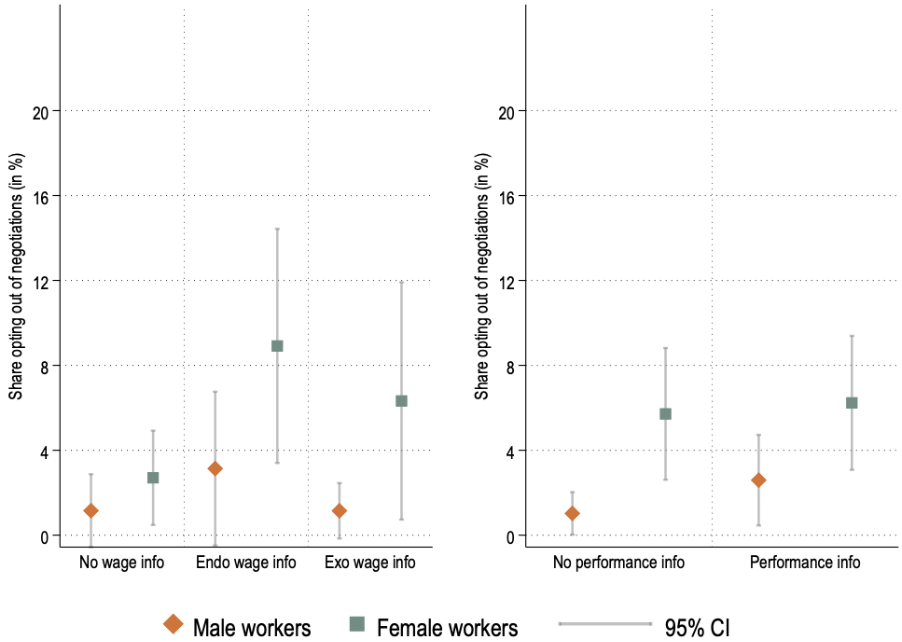
We do not find a meaningful interaction effect of performance and wage information. The effect of providing performance information is not significantly different with wage information compared to without wage information ($p = 0.565$ for *EndoWage* and $p = 0.617$ for *ExoWage*); regression (7) in Table 3). Moreover, there is no distinct interaction effect of joint information provision for women compared to men ($p = 0.646$ and $p = 0.478$, respectively; regression (8) in Table 3). Hence, we also cannot reject the null hypothesis of no effect of joint information provision in favor of Hypothesis 3.

4.3.2 The effect of transparency on negotiation entry

In this section, we analyze whether the availability of information affects the workers' willingness to negotiate. Considering negotiation entry is essential, as not entering negotiations usually entails negative payoff consequences. Controlling for differences in contributions by workers who do and do not select into negotiations, opting out of negotiations reduces the worker's wage by 101.01 points ($p < 0.001$). Figure 5 depicts the share of male and female workers opting out of negotiations in each treatment.

Whereas performance information does not significantly impact the worker's propensity to enter negotiations (*PmtT-test*; $p = 0.490$), wage transparency deters workers from entering negotiations. Compared to the *NoWage* treatment, significantly more workers opt out of negotiations in *EndoWage* and *ExoWage* (*PmtT-test*; $p = 0.068$, pooling observations from *EndoWage* and *ExoWage*). Importantly, this effect is gender specific. Our experiment replicates the common finding in the literature that women opt out of negotiations significantly more often. Female workers opt out of 6% of negotiations, male workers only out of 2% of negotiations (*PmtT-test*; $p = 0.004$). This gender difference is primarily driven by women's response to wage transparency. Without wage information, there are no gender differences in the willingness to enter negotiations (*PmtT-test*; $p = 0.379$), but differences emerge in the information treatments (*PmtT-test*; $p = 0.009$, pooling observations from *EndoWage* and *ExoWage*).[24] The results are in line with

[24]We observe this gender difference both in *EndoWage* (*PmtT-test*; $p = 0.094$), and *ExoWage* (*PmtT-test*; $p = 0.046$). The regression results for this subsection can be found in Table A15. It provides the results of OLS regressions of the participant's choice to opt out of negotiations on a gender dummy, the treatment indicators *Wage* and *Performance*, as well as their interactions.



Notes: Comparison of the share of workers opting out of negotiations by gender, varying wage information (left) and performance information (right). Error bars indicate the 95% confidence interval. Standard errors are clustered at the matching-group level.

Figure 5: Treatment comparison of gender differences in negotiation opt-outs

women avoiding the social comparison that negotiations with wage information entail. Wage information reveals crucial information on the worker's social status and ranking, which may result in women opting out of negotiations more often due to gender differences in status-ranking aversion (Brandts et al., 2020) and different responses to public self-assessments (Haeckl, 2022).

Note that in our experiment, the small share of decisions to opt out of negotiations means that the gender difference in entry decisions in *ExoWage* does not imply that the gender wage gap increases under wage transparency.[25] If women are more likely to forego the benefits from negotiations if wage information is freely available, this nevertheless results in substantial wage losses for these workers.

4.3.3 Endogenous wage information

Next, we turn to potential barriers to the usefulness of endogenous wage information. If requesting wage information is not beneficial for workers, wage policy that requires

[25] We show in Appendix D, Table A16, that the wage gap is not affected by treatments *EndoWage* or *ExoWage* if we only consider workers who enter negotiations. Therefore, the fact that gender differences in entry decisions under wage transparency do not result in an increase of the gender pay gap is not the result of a change in wages by women entering negotiations. These women do not benefit from wage information and thus do not compensate for the loss incurred by women who opt out. Instead, the share of choices to opt out of negotiations is too low to significantly affect the gender pay gap.

workers to ask for the information might fail. So we now focus on who is requesting and who is benefiting from wage information.[26] Table 4 presents regression results restricting the sample to observations from *EndoWage*. We regress the binary choice of requesting wage information on the worker's contribution and the usual set of controls in column (1), add an indicator for female in column (2), and split the sample by gender in columns (3) and (4).

Overall, women request wage information about five percentage points less often than men, a difference that is statistically insignificant ($p = 0.540$; regression (2) in Table 4). More productive workers, on the other hand, are significantly more likely to ask for wage information ($p = 0.050$; regression (1) in Table 4). Endogenous wage transparency policies, therefore, are more likely to have an impact on the negotiations of high-performing individuals. Interestingly, this effect is entirely driven by the behavior of male workers ($p = 0.079$ for men $p = 0.954$ for women, regression (3) and (4) in Table 4). This effect could be another consequence of (high-performing) men being more inclined to seek social comparisons, as discussed in Section 4.3.2.

	Requested wage information			
	(1)	(2)	(3)	(4)
Worker contribution	0.04**	0.04*	0.08*	-0.00
	(0.02)	(0.02)	(0.05)	(0.03)
Female		-0.05		
		(0.08)		
Constant	0.47***	0.51***	0.30	0.65***
	(0.10)	(0.12)	(0.19)	(0.12)
Part FE	✓	✓	✓	✓
Period FE	✓	✓	✓	✓
Laboratory FE	✓	✓	✓	✓
Sample	*EndoWage*	*EndoWage*	*EndoWage Men*	*EndoWage Women*
Observations	515	515	255	260
Clusters	22	22	22	22
R-squared	0.066	0.068	0.079	0.075

Notes: Results are from ordinary least squares regression of the worker's binary decision to request wage information. Worker contribution is a control for the worker's contribution to the negotiation pie (in hundred units), Female indicates whether a participant is female. Standard errors are clustered at the matching-group level and shown in parentheses. Sample refers to the treatment(s) from which the observations for the analysis stem
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: The determinants of requesting wage information

The rest of this subsection analyzes how the choice of requesting wage information affects wages. Endogenous wage transparency policies are only effective if individuals who request wage information actually benefit from this request. Table 5 gives the results of a linear regression with the previously outlined controls and fixed effects of the worker's wage on an indicator of the worker requesting wage information in treatment *EndoWage*, column (1), including an interaction of this choice with the worker's contribution in column (2). In columns (3) and (4), the analysis is split by gender. Column (5) only

[26]This section is of an exploratory nature and was not pre-registered.

includes observations from *ExoWage* and observations from individuals choosing wage information in *EndoWage*, regressing the worker's wage on an indicator for treatment *ExoWage* and the interaction of *ExoWage* with the worker's contribution.

	Worker's wage				
	(1)	(2)	(3)	(4)	(5)
Worker contribution	0.57***	0.70***	0.78***	0.64***	0.50***
	(0.03)	(0.05)	(0.05)	(0.08)	(0.03)
Firm contribution	0.06	0.07	0.11	-0.01	0.25**
	(0.18)	(0.18)	(0.21)	(0.17)	(0.12)
Info choice	-17.77	76.86**	76.07*	77.56*	
	(11.72)	(30.23)	(40.55)	(43.42)	
Info choice × Worker contribution		-0.25***	-0.26**	-0.25*	
		(0.08)	(0.10)	(0.14)	
Exo wage					26.14**
					(11.69)
Constant	67.34	19.77	-4.00	45.13	4.63
	(66.20)	(69.78)	(80.52)	(72.36)	(42.52)
Part FE	✓	✓	✓	✓	✓
Period FE	✓	✓	✓	✓	✓
Laboratory FE	✓	✓	✓	✓	✓
Sample	*EndoWage*	*EndoWage*	*EndoWage Men*	*EndoWage Women*	*WageInfo*
Observations	515	515	255	260	759
Clusters	22	22	22	22	44
R-squared	0.272	0.284	0.294	0.307	0.240

Notes: Results are from ordinary least squares regression of the worker's wage. Worker contribution is a control for the worker's contribution to the negotiation pie, Info choice indicates whether worker requested wage information in *EndoWage*, ExoWage is an indicator for *ExoWage*. Standard errors are clustered at the matching-group level and shown in parentheses. Sample refers to the treatment(s) from which the observations for the analysis stem, *WageInfo* refers to observations from *ExoWage* and individuals choosing wage information in *EndoWage*.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: The effect of requesting wage information on wages

We first test the effect of requesting wage information in the *EndoWage* treatment. There is no significant effect of requesting wage information on negotiated wages ($p = 0.144$; regression (1) in Table 5). If anything, the effect is more likely to be negative, with a point estimate of -17.77 points.

Although there is no overall effect of requesting wage information, this pooled analysis hides an important heterogeneity. Requesting wage information helps low performers and hurts high performers ($p = 0.006$; regression (2) in Table 5). This is the case both for male and female workers ($p = 0.015$ and $p = 0.096$; regression (3) and regression (4) in Table 5, respectively). Intuitively, the wage information provides an anchor for the negotiations, which is, on average, comparatively low for high-performing workers. Without wage information, highly productive workers earn more on average, and the comparable wage is likely to be lower than the wage they would receive without wage information. The reverse is true for low performers. Therefore, the anchor is favorable for low-performing individuals only. As we have previously seen that high-performing workers are more likely to request this information, endogenous wage transparency policies might then fail to improve overall wages.

Finally, we compare the wages of workers who request wage information in *EndoWage* to those that receive it exogenously in *ExoWage*. Here, both comparison groups acquire (endogenously or exogenously) wage information. Nevertheless, wages differ. We observe significantly higher wages (by 11%) in *EndoWage*, where the information acquisition does not result from an active choice (*PmtT-test*; $p = 0.018$). However, after controlling for the higher performance of those who request wage information, we estimate that the choice to acquire wage information (compared to the exogenous provision) reduces wages by 26.14 points ($p = 0.031$; regression (5) in Table 5).[27] This result hints that endogenous wage information will not only reach fewer workers (due to limited take-up), but the workers who do request the information may also benefit less from it than workers if wage information is provided exogenously. This therefore provides further evidence suggesting that endogenous wage transparency may not be optimal.

4.3.4 The role of beliefs

Our experiment addresses whether wage information can reduce wage inequality by correcting beliefs about others' wages and relative performance. We now zoom in on this mechanism. For this, we take a closer look at the effects of these types of information on beliefs and the role that incorrect beliefs play in determining negotiation outcomes.

Type classification Previously, we established that controlling for a worker's contribution reduces the gender wage gap in our experiment. This, however, does not necessarily mirror actual labor markets. To study whether belief changes can provide a channel through which wage transparency can be an effective tool, we now directly classify individuals based on their beliefs, not their gender.[28] Do pessimistic individuals benefit more from learning about others' wages and underconfident individuals more from performance information?

Following the theoretical analysis in Section 4.1, we utilize two types of beliefs for our classification. First, we use a subject's beliefs about the comparable worker's wage (from *Elicitation 2*). Subjects with beliefs about the comparable worker's wage that exceed the actual wage of the period's comparable worker are classified as 'Optimistic'.

[27] Any difference in the wages of these two groups reflects the costs of 10 points for acquiring wage information and could be driven by selection in *EndoWage*. The workers who choose wage information are a non-random subsample of the pool of workers. For instance, high-performing individuals are more likely to request wage information. Furthermore, it is possible that workers with low negotiation skills are more likely to request wage information, and that they would have received lower wages regardless of information provision. There is, however, some suggestive evidence that selection is not the main driver of this effect. We show in Table A17 that workers who do not request wage information in *EndoWage* receive comparable wages as workers in *NoWage*, so these two samples of individuals reach similar outcomes. This suggests that the interaction of receiving wage information and choosing to acquire wage information is crucial.

[28] We pre-registered this approach.

Second, we use a subject's belief about performance in the production task (from *Elicitation 1*). As information about own performance and the period's comparable worker's performance are always provided jointly, we classify subjects depending on whether they were 'Overconfident' in their relative performance.[29]

Belief updating After workers receive wage or performance information, we re-elicit their beliefs, as explained in Section 4.2. Now, we compare wage elicitations from *Elicitation 2* and *Elicitation 3*. This allows us to investigate whether information about wages informs beliefs about performance and vice versa. Indeed, beliefs about the comparable worker's performance are affected by wage information. If workers receive wage information, in *EndoWage* or in *ExoWage*, they update their beliefs more negatively about the comparable worker's performance the more they overestimated the comparable worker's wage ($p = 0.033$; regression (2) in Table A18). Thus, surprisingly low wages are partially attributed to lower-than-expected performance. Similarly, individuals that were too optimistic about the comparable worker's performance update their beliefs more negatively about the comparable worker's wage if performance information is provided ($p = 0.007$; regression (4) in Table A18). Therefore, it is important to consider the observability of performance when wage transparency is implemented. See Table A18 for the regression analysis.

The effects wage and performance information on negotiation outcomes We document the results of linear regressions of the worker's initial wage request (Table 6) and the worker's wage (Table 7) on the worker's type in columns (1) and (3), including interactions of the worker's type and treatment in columns (2) and (4), with the usual controls and fixed effects. For both tables, we use the full sample in columns (2) and (4) and restrict the sample to individuals in treatment *NoWage* in column (1) and individuals in treatment *NoPerformance* in column (3).

As a first test of whether the type classification predicts negotiation behavior in the hypothesized way, we analyze the effect of information on the workers' initial wage requests in negotiations. Studying initial wage requests allows us to see the different types' responses to information when this has not yet been affected by the firm's behavior or the negotiations in the chat. The classification of 'Overconfident' and 'Optimistic' workers predicts initial wage requests in the hypothesized way, even after controlling for the

[29] In other words, individuals classified as 'Overconfident' believe that the difference between their performance and the period's comparable worker's performance exceeds the actual difference. This definition is similar to the overplacement definition of overconfidence found in the literature (Moore and Healy, 2008), although it refers to overestimation of the relative number of questions solved, rather than overestimation of the relative rank. Outliers, with beliefs exceeding 60 correct answers, are excluded from this analysis. These participants likely reported their beliefs about worker contribution, rather than the number of correct answers, and constitute only 1% of observations. This does not affect the results of our analysis in any meaningful way.

	Worker's initial offer			
	(1)	(2)	(3)	(4)
Worker contribution	0.51***	0.52***	0.50***	0.55***
	(0.08)	(0.04)	(0.05)	(0.04)
Optimistic	40.38***	39.93***		
	(12.29)	(12.25)		
Endo wage		9.71		
		(13.22)		
Exo wage		11.24		
		(12.62)		
Endo wage × Optimisticic		-6.17		
		(14.77)		
Exo wage × Optimisticic		-38.39***		
		(14.05)		
Overconfident			28.77***	32.74***
			(7.61)	(7.29)
Performance info				26.81***
				(9.13)
Performance info× Overconfident				-45.70***
				(9.91)
Constant	202.03***	204.81***	215.62***	195.33***
	(28.49)	(18.42)	(21.86)	(18.02)
Part FE	✓	✓	✓	✓
Period FE	✓	✓	✓	✓
Laboratory FE	✓	✓	✓	✓
Observations	509	1486	743	1469
Clusters	22	66	66	66
R-squared	0.278	0.275	0.248	0.268

Notes: Results are from ordinary least squares regression of the worker's initial request. Worker contribution is a control for the worker's contribution to the negotiation pie. Endo wage and Exo wage are indicators of whether wage information was provided endogenously or exogenously, respectively. Performance is an indicator of whether information of the workers' performances is provided. Optimist indicates that a subject's beliefs about the comparable worker's wage are too optimistic, Overconfident indicates that a subject's beliefs about his or her own performance relative to the comparable worker's are too optimistic. Standard errors are clustered at the matching-group level and shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: The type-specific effect of performance and wage information on initial wage requests

worker's contributions. Both optimistic workers ($p = 0.004$, regression (1) in Table 6) and overconfident workers ($p < 0.001$; regression (3) in Table 6) request significantly higher wages in the absence of the relevant information, in line with our model's predictions.[30]

However, when information is provided to correct these misspecified beliefs, initial wage requests change in the expected direction: compared to the other type, optimistic workers reduce their initial demand by 38 points in *ExoWage* ($p = 0.008$; regression (2) in Table 6) and overconfident workers reduce their demand by 46 points in *Performance* ($p < 0.001$; regression (4) in Table 6). This gives a first indication of the potential power of information: After correcting beliefs, the initial wage requests of optimistic and overconfident individuals are no longer higher than the demands by other types. However, at this point, it is not clear whether this effect translates into a change in negotiated wages. Therefore, we will now study whether realized wages are affected in a

[30]Note that this initial proposal was made before the unstructured negotiations started, but after the provision of wage and/or performance information.

similar way.

We first consider heterogeneous treatment effects depending on whether beliefs about the wage of the comparable worker are too optimistic. Individuals with too optimistic beliefs earn significantly less in the absence of wage information ($p = 0.016$; regression (1) in Table 7). This is potentially driven by optimists negotiating for unrealistically high wages, which leads to a negotiation breakdown. In line with this, optimists are significantly more likely to face a breakdown of negotiations, where workers and firms fail to agree and both receive a payoff of zero ($p < 0.001$; regression (1) in Table A19). However, there is no evidence that wage information improves the outcomes for Optimists

	Worker's wage			
	(1)	(2)	(3)	(4)
Worker contribution	0.58***	0.58***	0.49***	0.55***
	(0.04)	(0.02)	(0.04)	(0.03)
Firm contribution	0.28	0.22**	0.33**	0.25**
	(0.17)	(0.09)	(0.13)	(0.10)
Optimistic	-33.19**	-33.78***		
	(12.71)	(12.28)		
Endo wage		-4.52		
		(11.87)		
Exo wage		7.53		
		(10.35)		
Endo wage × Optimistic		-0.06		
		(16.14)		
Exo wage × Optimistic		12.13		
		(16.45)		
Overconfident			5.70	10.55
			(9.72)	(9.61)
Performance info				21.06**
				(8.60)
Performance info× Overconfident				-18.07*
				(10.70)
Constant	-12.48	13.68	-18.05	-13.23
	(63.31)	(35.35)	(48.90)	(38.02)
Part FE	✓	✓	✓	✓
Period FE	✓	✓	✓	✓
Laboratory FE	✓	✓	✓	✓
Observations	519	1548	770	1530
Clusters	22	66	66	66
R-squared	0.292	0.278	0.247	0.263

Notes: Results are from ordinary least squares regression of the worker's wage. Worker contribution is a control for the worker's contribution to the negotiation pie, Firm contribution for the firm's contribution to the negotiation pie. Endo wage and Exo wage are indicators of whether wage information was provided endogenously or exogenously, respectively. Performance is an indicator of whether information of the workers' performances is provided. Optimistic indicates that a subject's beliefs about the comparable worker's wage are too optimistic, Overconfident indicates that a subject's beliefs about his or her own performance relative to the comparable worker's are too optimistic. Standard errors are clustered at the matching-group level and shown in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: The type-specific effect of performance and wage information on wages

($p = 0.997$ for *Endo Wage*, $p = 0.463$ for *Exo Wage*; regression (2) in Table 7). Thus, wage information only changes initial asks by overconfident individuals, without affecting the ultimate negotiation outcomes. Correcting beliefs about wages, therefore, only has an intermediate effect on those individuals in our sample that could benefit from this information.

Next, we consider the effect of performance information depending on whether an individual is overconfident or underconfident. The wages of underconfident individuals increase if performance information is provided ($p = 0.017$; regression (4) in Table 7). We estimate that underconfident individuals increase their wages by 21 points, whereas overconfident individuals are not affected by performance information (point estimate of $21.06 - 18.07 = 2.99$ points, $p = 0.096$ for the interaction effect). In line with our theoretical predictions, this suggests that underconfident individuals gain from performance information that corrects their pessimistic beliefs. In contrast to the effect of wage information, the correction of beliefs about relative performance is thus also powerful in affecting wages, not only intermediate outcomes such as initial wage requests.

5 Conclusion

Wage transparency regulation has become an increasingly popular policy tool. Studies on the diverse wage transparency policy landscape can guide the design of future regulations. This is particularly relevant in light of efforts by the EU to establish wage transparency standards. Ours is the first study to look into a unique wage transparency law introduced by Germany, where employees are given the right to request wage information. Using plausibly exogenous variation in whether firms have to comply with this regulation, we do not find any impact on wages or the gender pay gap.

In an online laboratory experiment, we examine several mechanisms underlying the policy's ineffectiveness that can inform future policies. We address the way in which wage information is currently provided, with employees needing to actively request this. If wage information is provided exogenously instead of endogenously, we see that wages increase. This suggests an increase in the workers' bargaining power if wage information is provided by default. In part, the ineffectiveness of endogenously compared to exogenously provided wage information is driven by workers requesting wage information who do not effectively utilize this information. Crucially, the gender wage gap, however, is also not affected by exogenously provided wage information. Moreover, female workers enter negotiations less often if wage information is provided exogenously, suggesting that wage transparency may also backfire.

As a complimentary transparency measure, we study performance information. Performance information increases workers' wages, but does not affect the gender pay gap. Our study underlines why it is nevertheless important to consider performance information when designing transparency regulations. When performance comparisons are difficult, the effect of wage transparency on correcting beliefs about a worker's fair compensation may be dampened. Individuals could attribute the news they receive about others' wages to performance differences instead of only updating their beliefs about wages.

Our research is a first step that indicates that 'pay information rights' do not perform as well as previously studied 'pay information duties', such as investigated by Duchini et al. (2020) and Bennedsen et al. (2022). As a next step, the effect of wage transparency regulation could be monitored over a longer horizon. We only observe two 'treated' years, and it is conceivable that the policy is more successful later on. Our analysis so far does not suggest an increased effect in 2019 compared to 2018. Nevertheless, employees might start seeking out wage information from their employers after hearing success stories of others using this information. If they fear backlash from requesting this information, this fear might diminish after observing that others successfully requested it.

While our experiment focuses on the effect of correcting beliefs about others' wages, future research could take a closer look at whether and how wage transparency can affect wages by spotlighting discriminatory practices. Firms with unequal compensation policies may face public pressure if periodic reporting of gender pay gaps becomes compulsory. Sorting of workers into different firms and industries might then be of particular interest. If wage information is easily accessible, it could reduce gender wage gaps by encouraging firms to increase the wages of women to attract female employees. The current German wage transparency regulation is, given that wage information is hard to access, not able to do so.

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Online Appendix

A Proofs

A worker and a firm split a pie π. The worker believes he or she can contribute $\hat{c}_i$ to the firm and that the firm pays comparable workers, that is, workers performing comparable tasks, an average wage of $\hat{\bar{w}}_i$. S/he believes that the average contribution of comparable workers to the firm is $\hat{\bar{c}}_i$. The wage in the Nash bargaining solution w_i^* is the w_i characterized by

$$\begin{aligned} \max_{w_i} \quad & \left(w_i - \alpha_i \left(\frac{w_i}{\hat{c}_i} - \frac{\hat{\bar{w}}_i}{\hat{\bar{c}}_i} \right)^2 \right) (\pi - w_i) \\ \text{s.t.} \quad & w_i \geq 0 \\ & \pi \geq w_i \end{aligned}$$

This gives the following objective function

$$L\left(w_i; \alpha_i, \hat{c}_i, \hat{\bar{w}}_i, \hat{\bar{c}}_i, \pi\right) = \left(w_i - \alpha_i \left(\frac{w_i}{\hat{c}_i} - \frac{\hat{\bar{w}}_i}{\hat{\bar{c}}_i} \right)^2 \right) (\pi - w_i) - \lambda (\pi - w_i)$$

The first order conditions for a local maximum are given by

$$\frac{\partial L}{\partial w_i} = -w_i + \alpha_i \left(\frac{w_i}{\hat{c}_i} - \frac{\hat{\bar{w}}_i}{\hat{\bar{c}}_i} \right)^2 + (\pi - w_i) \left(1 - \frac{2\alpha_i \left(\frac{w_i}{\hat{c}_i} - \frac{\hat{\bar{w}}_i}{\hat{\bar{c}}_i} \right)}{\hat{c}_i} \right) + \lambda = 0$$

$$\lambda (\pi - w_i) = 0, \qquad \lambda \geq 0$$

We require $\lambda = 0$, as otherwise we get $L\left(w_i; \alpha_i, \hat{c}_i, \hat{\bar{w}}_i, \hat{\bar{c}}_i, \pi\right) = 0$, which is not a local maximum. Thus, w_i^* is characterized by

$$-w_i^* + \alpha_i \left(\frac{w_i^*}{\hat{c}_i} - \frac{\hat{\bar{w}}_i}{\hat{\bar{c}}_i} \right)^2 + (\pi - w_i^*) \left(1 - \frac{2\alpha_i \left(\frac{w_i^*}{\hat{c}_i} - \frac{\hat{\bar{w}}_i}{\hat{\bar{c}}_i} \right)}{\hat{c}_i} \right) = 0$$

This gives an implicit function of w_i^* in terms of the agent's beliefs ($\hat{c}_i, \hat{\bar{w}}_i$, and $\hat{\bar{c}}_i$) and aversion to unfair wages (α_i). Solving this expression for w_i^*, we obtain as the only solution that also satisfies the second order condition of $\frac{\partial^2 L}{\partial w_i^2} < 0$:

$$w_i^* = \frac{\pi + \frac{2\hat{c}_i \hat{\bar{w}}_i}{\hat{\bar{c}}_i} + \frac{\hat{c}_i^2}{\alpha_i} - \sqrt{\frac{\hat{c}_i^4}{\alpha_i^2} + \frac{\hat{c}_i^2 (4\hat{c}_i \hat{\bar{w}}_i - \hat{\bar{c}}_i \pi)}{\alpha_i \hat{\bar{c}}_i} + \frac{(\hat{c}_i \hat{\bar{w}}_i - \hat{\bar{c}}_i \pi)^2}{\hat{\bar{c}}_i^2}}}{3} \tag{4}$$

Our hypotheses from Section 4.1 follow from comparative statics predictions about w_i^* and the assumptions on gender differences in the agent's beliefs ($\hat{c}_i, \hat{\bar{w}}_i$, and $\hat{\bar{c}}_i$) outlined in Section 4.1. For the first two results, we assume that information on average wages of comparable others does not affect beliefs $\hat{\bar{c}}$ about average contributions. Information on average performances of comparable others does not affect beliefs about average wages of others. Formally, this means $\frac{\partial \hat{c}_i}{\partial \hat{\bar{w}}_i} = \frac{\partial \hat{\bar{c}}_i}{\partial \hat{\bar{w}}_i} = \frac{\partial \hat{\bar{w}}_i}{\partial \hat{c}_i} = \frac{\partial \hat{\bar{w}}_i}{\partial \hat{\bar{c}}_i} = 0$.

Result 1. *Providing information about a comparable worker's wage increases wages by women compared to men.*

The wage maximizing the Nash product defined in Equation 4 has the property that $\frac{\partial w_i^*}{\partial \hat{\bar{w}}_i} > 0$. We assume that women F have pessimistic beliefs about others' wages, so $\hat{\bar{w}}_i^F < \bar{w}$. Men have optimistic beliefs $\hat{\bar{w}}_i^M$, so $\hat{\bar{w}}_i^M > \bar{w}$. After observing information on the correct value $\bar{w}$, beliefs will be updated such that both for men and women $\hat{\bar{w}}_i^F = \hat{\bar{w}}_i^M = \bar{w}$.

Given these assumptions, we consider how making wages transparent (T_w), changes the wage from the Nash bargaining solution for women. We denote this change by $\Delta_{T_w} w_i^{*F}$ and compare this to the change for men, which we denote by $\Delta_{T_w} w_i^{*M}$. This change $\Delta_{T_w} w_i^*$ is defined as the difference in the equilibrium wage if wages are transparent, $w_i^{*T_w}$, compared to when wages are secret, $w_i^{*S_w}$. For this, see that given the assumption $\frac{\partial \hat{c}_i}{\partial \hat{\bar{w}}} = \frac{\partial \hat{\bar{c}}_i}{\partial \hat{\bar{w}}_i} = 0$, we can write

$$\Delta_{T_w} w_i^* = w_i^* \left(\hat{\bar{w}}_i^{T_w}; \cdot\right) - w_i^* \left(\hat{\bar{w}}_i^{S_w}; \cdot\right) = \int_{\hat{\bar{w}}_i^{S_w}}^{\hat{\bar{w}}_i^{T_w}} \underbrace{\frac{\partial w_i^*}{\partial \hat{\bar{w}}_i}}_{>0} d\hat{\bar{w}}_i$$

Here, we use the integral notation to illustrate the dependence of this difference on $\frac{\partial w_i^*}{\partial \hat{\bar{w}}_i}$ and the change in beliefs $\hat{\bar{w}}_i$, which serve as limits of integration.

Since $\bar{w} = \hat{\bar{w}}_i^{T_w} > \hat{\bar{w}}_i^{S_w}$ for women, but $\bar{w} = \hat{\bar{w}}_i^{T_w} < \hat{\bar{w}}_i^{S_m}$ for men, this implies

$$\Delta_{T_w} w_i^{*W} > \Delta_{T_w} w_i^{*M}$$

$\square$

Result 2. *Providing information about a worker's own performance relative to the comparable worker's performance increases wages by women compared to men.*

This proof follows along similar lines as the previous. The wage maximizing the Nash product defined in Equation 4 has the property that $\frac{\partial w_i^*}{\partial \hat{c}_i} > 0$ and $\frac{\partial w_i^*}{\partial \hat{\bar{c}}_i} < 0$. Information on c_i and $\bar{c}$ is simultaneously provided. We assume that women have pessimistic beliefs about their performance, denoted by $\hat{c}_i^F$, so $\hat{c}_i^W < c_i$, while men have optimistic beliefs $\hat{c}_i^M > c_i$. After observing information on the correct value c_i, beliefs will be updated such that both for men and women $\hat{c}_i^F = \hat{c}_i^M = c_i$.

Given these assumptions, performance information (T_p) changes the wage from the Nash bargaining solution for women. We denote this change by $\Delta_{T_p} w_i^{*F}$ and compare this to the change for men, which we denote by $\Delta_{T_p} w_i^{*M}$. This change $\Delta_{T_p} w_i^{*}$ is defined as the difference in the equilibrium wage if performance is transparent, $w_i^{*T_p}$, compared to when performance is secret, $w_i^{*S_p}$.

For this, see that given the assumption $\frac{\partial \hat{\hat{w}}_i}{\partial \hat{c}_i} = \frac{\partial \hat{w}_i}{\partial \hat{\hat{c}}_i} = 0$ we can write

$$\begin{aligned}
\Delta_{T_p} w_i^* &= w_i^*\left(\hat{\hat{c}}_i^{T_p}, \hat{c}_i^{T_p}; \cdot\right) - w_i^*\left(\hat{\hat{c}}_i^{S_p}, \hat{c}_i^{S_p}; \cdot\right) \\
&= \int_{\hat{\hat{c}}_i^{S_p}}^{\hat{\hat{c}}_i^{T_p}} \underbrace{\frac{\partial w_i^*\left(\hat{c}_i = \hat{c}_i^{S_p}\right)}{\partial \hat{\hat{c}}_i}}_{<0} d\hat{\hat{c}}_i + \int_{\hat{c}_i^{S_p}}^{\hat{c}_i^{T_p}} \underbrace{\frac{\partial w_i^*\left(\hat{\hat{c}}_i = \hat{\hat{c}}_i^{T_p}\right)}{\partial \hat{c}_i}}_{>0} d\hat{c}_i
\end{aligned}$$

Ceteris paribus, since $c_i = \hat{c}_i^{T_p} > \hat{c}_i^{S_p}$ for women and $c_i = \hat{c}_i^{T_p} < \hat{c}_i^{S_p}$ for men, this implies

$$\Delta_{T_p} w_i^{*F} > \Delta_{T_p} w_i^{*M}$$

□

Next, we relax the assumption of $\frac{\partial \hat{c}_i}{\partial \hat{w}_i} = \frac{\partial \hat{\hat{c}}_i}{\partial \hat{\hat{w}}_i} = \frac{\partial \hat{\hat{w}}_i}{\partial \hat{c}_i} = \frac{\partial \hat{w}_i}{\partial \hat{\hat{c}}_i} = 0$ and instead posit that $\frac{\partial \hat{\hat{c}}_i}{\partial \hat{\hat{w}}_i} > 0$ and $\frac{\partial \hat{\hat{w}}}{\partial \hat{\hat{c}}_i} > 0$. In this case, if a worker is told that another worker is more productive than anticipated, they will also update beliefs about the wage of the other worker in the same direction.

Result 3. *Providing information about a comparable worker's wage and relative performance jointly has a stronger effect on wages than providing this information separately.*

If $\frac{\partial \hat{\hat{c}}_i}{\partial \hat{\hat{w}}_i} > 0$ and $\frac{\partial \hat{\hat{w}}_i}{\partial \hat{\hat{c}}_i} > 0$, $\Delta_{T_w} w_i^* \neq \int_{\hat{\hat{w}}_i^{S_w}}^{\hat{\hat{w}}_i^{T_w}} \frac{\partial w_i^*}{\partial \hat{\hat{w}}_i} d\hat{\hat{w}}_i$. Instead, we can write that if no performance information is provided, the effect of wage transparency on wages in the Nash bargaining solution is characterized by

$$\Delta_{T_w} w_i^* = \int_{\hat{\hat{w}}_i^{S_w}}^{\hat{\hat{w}}_i^{T_w}} \frac{\partial w_i^*(\hat{\hat{c}}_i = \hat{\hat{c}}_i^{S_w})}{\partial \hat{\hat{w}}_i} d\hat{\hat{w}}_i + \int_{\hat{\hat{c}}_i^{S_w}}^{\hat{\hat{c}}_i^{T_w}} \underbrace{\frac{\partial w_i^*(\hat{\hat{w}}_i = \hat{\hat{w}}_i^{T_w})}{\partial \hat{\hat{c}}_i}}_{<0} d\hat{\hat{c}}_i$$

$\Delta_{T_w} w_i^*$ is therefore decreasing in $\hat{\hat{c}}_i^{T_w} - \hat{\hat{c}}_i^{S_w}$. Since $\frac{\partial \hat{\hat{c}}_i}{\partial \hat{\hat{w}}_i} > 0$, we know that if and only if $\hat{\hat{w}}_i^{T_w} > \hat{\hat{w}}_i^{S_w}$, also $\hat{\hat{c}}_i^{T_w} > \hat{\hat{c}}_i^{S_w}$ must hold. In other words, providing wage information alone results in a smaller change of equilibrium wages if it also leads to updating of beliefs about performance.

Similarly, the effect of providing performance information is then characterized by

$$\Delta_{T_p} w_i^* = \int_{\hat{\hat{c}}_i^{S_p}}^{\hat{\hat{c}}_i^{T_p}} \frac{\partial w_i^* \left(\hat{c}_i = \hat{c}_i^{S_p} \right)}{\partial \hat{\hat{c}}_i} d\hat{\hat{c}}_i + \int_{\hat{c}_i^{S_p}}^{\hat{c}_i^{T_p}} \frac{\partial w_i^* \left(\hat{\hat{c}}_i = \hat{\hat{c}}_i^{T_p} \right)}{\partial \hat{c}_i} d\hat{c}_i + \int_{\hat{\hat{w}}_i^{S_p}}^{\hat{\hat{w}}_i^{T_p}} \underbrace{\frac{\partial w_i^* (\hat{\hat{c}}_i = \hat{\hat{c}}_i^{T_p})}{\partial \hat{\hat{w}}_i}}_{>0} d\hat{\hat{w}}_i$$

$\Delta_{T_p} w_i^*$ is therefore increasing in $\hat{\hat{w}}_i^{T_p} - \hat{\hat{w}}_i^{S_p}$. Since $\frac{\partial \hat{\hat{w}}_i}{\partial \hat{\hat{c}}_i} > 0$, we know that if and only if $\hat{\hat{c}}_i^{T_p} > \hat{\hat{c}}_i^{S_p}$, also $\hat{\hat{w}}_i^{T_p} > \hat{\hat{w}}_i^{S_p}$ must hold.

Note that if wage and performance information are provided jointly, we are back in the cases considered in Result 1 and Result 2, as the respective beliefs about wages or performance will be fixed.

This implies that the joint effect of providing wage and performance information on equilibrium wages is larger for women than the sum of the effects of providing the two types of information separately. Given $\hat{\hat{w}}_i^{T_w} > \hat{\hat{w}}_i^{S_w}$ and $\hat{\hat{c}}_i^{T_p} < \hat{\hat{c}}_i^{S_p}$, $\Delta_{T_w} w_i^*$ and $\Delta_{T_p} w_i^*$ are smaller if provided separately than if provided jointly. For men, the reverse holds true. The effect is thus muted if the information is provided separately compared to provided simultaneously.

□

B Prolific pre-study

We conducted a pre-study before running the experiment described in Section 4.2. This pre-study is designed to inform us on which tasks are perceived to favor male participants. We recruited 100 participants on Prolific. We selected participants from the Netherlands in an age bracket from 18 to 30 years to match the subject pool from the University of Amsterdam.

The survey asks participants whether the average number of correctly solved tasks was 5% higher for men, 5% higher for women or the average numbers of correctly solved tasks of men and women were within 5% of each other.

We asked participants about their estimates about men's and women's performance in three tasks. The first two tasks are the maze and the matrix task, described in Section 4.2. The third task are Raven's matrices.

On top of a one Pound base payment, we use a bonus payment to incentivize this task. If the participant's answer matches the results of a corresponding experimental study, the participant receives 30 pence per correct answer. To incentivize accurate beliefs in the matrix task, we use Schram et al. (2019) for the matrix task, Gneezy et al. (2003) for the maze task, and Crucian and Berenbaum (1998) for the Raven's matrices.

Table A1 provides the shares of respondents who believe that men or who believe that women solve at least 5% more tasks correctly.

	Raven's task	Matrix task	Maze task
Men	27%	45%	33%
Women	28%	20%	23%

Table A1: Overview of pre-study results

For Raven's matrices, we see that there is an almost equal share of participants that believe that men versus women perform better in this task (27% versus 28%, respectively). These shares are not significantly different (t-test; $p = 0.894$).

45% of the respondents believe that men solve significantly more elements correctly in the matrix task, while only 20% believe that women do so. This difference is significant (t-test; $p = 0.002$).

The pattern is similar for the maze task. Here, 33% of the respondents believe that men perform better, 23% believe that women perform better. While these shares differ by 43%, this difference is not statistically significant (t-test; $p = 0.183$).

Given this evidence, we do not include Raven's matrices in our experiment, as this task does not appear to respondents as favoring male participants.

C Additional analyses of field data

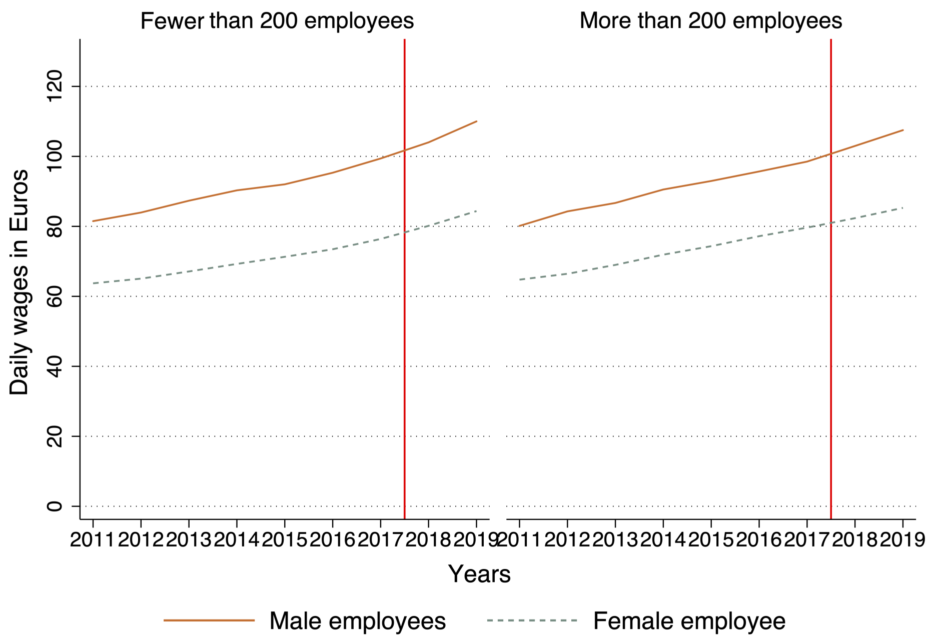
This appendix complements the analysis from Section 3. We will first give additional tables and figures using LIAB data, then provide the main analysis using data from SIEED, and finally present some heterogeneity analysis.

C.1 Additional results using LIAB

	Log of daily wage	
	(1)	(2)
Female	-0.1308***	-0.0558***
	(-12.67)	(-7.30)
Individual time-varying controls		✓
Firm × Occupation FE	✓	✓
Firm size	150-250	150-250
Observations	82,766	62,059

Notes: Gender gaps within firm-occupation cells in 2017 in firms with 150 to 250 employees. Individual time-varying controls include age, age squared, education and part-time occupation. Standard errors are clustered at the firm level. T-statistics in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Gender pay gap within firm-occupation cells in 2017



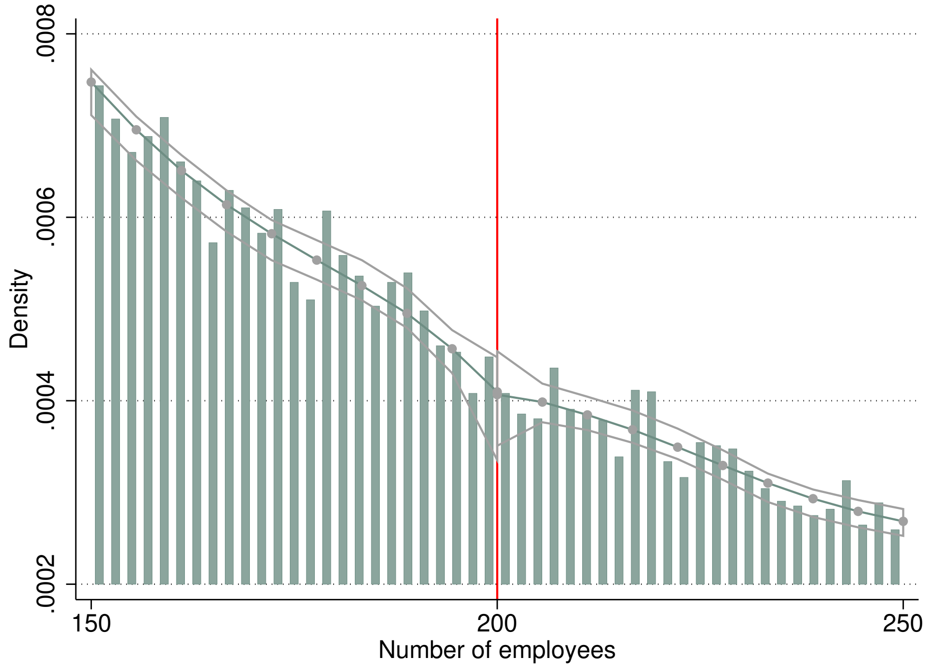
Notes: Raw data of daily wages from 2011 to 2019 by gender and by firm size. Includes observations from firms with 150 to 250 employees in 2018. The red vertical line indicates the introduction of the wage transparency regulation.

Figure A1: The gender gap in wages in firms with fewer vs. in firms with at least 200 employees

	Indicator of employment change					
	Both gender (1)	Men (2)	Women (3)	Both gender (4)	Men (5)	Women (6)
Large × Post	0.0090	0.0094	0.0027	0.0128	0.0131	0.0009
	(0.49)	(0.51)	(0.18)	(0.74)	(0.75)	(0.06)
Female × Large × Post	-0.0060			-0.0118		
	(-0.39)			(-0.82)		
Female × Large	0.0477			0.04229**		
	(1.62)			(2.37)		
Female × Post	-0.0083			-0.0108		
	(-0.99)			(-1.35)		
Individual time-varying controls	✓	✓	✓			
Firm FE	✓	✓	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
Firm size	150-250	150-250	150-250	150-250	150-250	150-250
Observations	508,410	282,588	225,179	685,693	383,751	301,094

Notes: Impact of transparency regulation on an indicator variable equal to one if the employee leaves the current establishment they work at by the next year and zero otherwise. Estimates from difference-in-difference specification. Individual time-varying controls include age squared, education and part-time occupation. Includes observations from 2011 to 2019. Standard errors are clustered at the firm level. T-statistics in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3: Diff-in-Diff estimates of impact of wage transparency law on the propensity to leave current employment



Notes: Plot of the share of firms by firm size, measured by the number of employees, in the range of 150 to 250, split by the cutoff of 200 (red vertical line). The center line indicates the estimated density, the gray lines indicate the 95% confidence interval around this.

Figure A2: Density plot of the firm size

	Log of daily wage					
	Both gender (1)	Men (2)	Women (3)	Both gender (4)	Men (5)	Women (6)
Diff-in-disc	0.0307	0.0320	0.0008	0.0024	0.0025	-0.0578
	(0.88)	(0.96)	(0.02)	(0.04)	(0.04)	(-1.11)
Female × Diff-in-disc	-0.0251			-0.0603		
	(-0.70)			(-1.02)		
Individual time-varying controls	✓	✓	✓			
Time FE	✓	✓	✓	✓	✓	✓
Firm size	150-250	150-250	150-250	150-250	150-250	150-250
Observations	639,395	357,630	281,765	852,465	478,000	374,465

Notes: Impact of transparency regulation on the gender wage gap and the wages of men and women individually. Estimates from difference-in-discontinuity specification. Time-varying controls include age squared, education and an indicator for part-time workers. Includes observations from 2011 to 2019. Standard errors are clustered at the firm level. T-statistics in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Diff-in-Disc estimates of impact of wage transparency law on daily wages

	Log of daily wage					
	Both gender (1)	Men (2)	Women (3)	Both gender (4)	Men (5)	Women (6)
Large × Post	0.0055	0.0050	-0.0017	0.0040	0.0112*	0.0011
	(1.18)	(1.07)	(-0.26)	(0.64)	(1.79)	(0.16)
Female × Large × Post	-0.0073			-0.0064		
	(-1.12)			(-0.82)		
Female × Large	-0.1361***			-0.0481**		
	(-3.88)			(-2.10)		
Female × Post	0.0187***			0.0060		
	(4.24)			(1.25)		
Individual time-varying controls	✓	✓	✓			
Firm FE	✓	✓	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
Firm size	150-250	150-250	150-250	150-250	150-250	150-250
Observations	585,822	333,183	252,051	778,441	446,733	340,632

Notes: Impact of transparency regulation on the gender wage gap and the wages of men and women individually. Estimates from difference-in-difference specification, using firm sizes recorded in 2017. Individual time-varying controls include age squared, education and part-time occupation. Includes observations from 2011 to 2019. Standard errors are clustered at the firm level. T-statistics in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Diff-in-Diff estimates for impact of wage transparency on daily wages, based on firm size in 2017

	Log of daily wage					
	Both gender (1)	Men (2)	Women (3)	Both gender (4)	Men (5)	Women (6)
Large × Post	0.0025	0.0011	0.0028	0.0049	0.0042	0.0023
	(0.50)	(0.22)	(0.43)	(0.77)	(0.67)	(0.33)
Female × Large × Post	-0.0002			-0.0023		
	(-0.03)			(-0.30)		
Female × Large	-0.0236			0.0049		
	(-0.78)			(0.22)		
Female × Post	0.0146**			0.0033		
	(3.25)			(0.65)		
Individual time-varying controls	✓	✓	✓			
Firm FE	✓	✓	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
Firm size	150-250	150-250	150-250	150-250	150-250	150-250
Observations	573,375	317,070	255,689	766,757	425,915	340,058

Notes: Impact of transparency regulation on the gender wage gap and the wages of men and women individually. Sample excludes employment spells with top-coded observations. Estimates from difference-in-difference specification. Individual time-varying controls include age squared, education and part-time occupation. Includes observations from 2011 to 2019. Standard errors are clustered at the firm level. T-statistics in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Diff-in-Diff estimates of impact of wage transparency law on daily wages excluding top-coded observations

	Log of daily wage				
	(1)	(2)	(3)	(4)	(5)
Large × Post	0.0009	-0.0008	0.0022	0.0031	0.0015
	(0.22)	(-0.19)	(0.46)	(0.56)	(0.22)
Female × Large × Post	0.0022	0.0023	-0.0001	-0.0003	0.0013
	(0.41)	(0.39)	(-0.01)	(-0.04)	(0.16)
Female × Large	-0.0425*	-0.0442	-0.0249	-0.0283	-0.0208
	(-1.87)	(-1.42)	(-0.83)	(-0.71)	(-0.38)
Female × Post	0.0152***	0.0164***	0.0146***	0.0123**	0.0144**
	(4.38)	(4.21)	(3.30)	(2.45)	(2.28)
Individual time-varying controls	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓
Firm size	130-270	140-260	150-250	160-240	170-230
Observations	852,267	707,938	584,026	464,504	333,935

Notes: Impact of transparency regulation on the gender wage gap and the wages of men and women individually. Estimates from difference-in-difference specification. Individual time-varying controls include age squared, education and part-time occupation. Includes observations from 2011 to 2019. Standard errors are clustered at the firm level. T-statistics in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7: Diff-in-Diff with different bandwidths

	Log of daily wage				
	(1)	(2)	(3)	(4)	(5)
Diff-in-disc	0.0289	0.0279	0.0307	0.0348	0.0573
	(1.07)	(0.91)	(0.88)	(0.85)	(1.24)
Female × Diff-in-disc	-0.0246	-0.0117	-0.0251	-0.0392	-0.0586
	(-0.86)	(-0.36)	(-0.70)	(-0.97)	(-1.29)
Time FE	✓	✓	✓	✓	✓
Firm size	130-270	140-260	150-250	160-240	170-230
Observations	926,022	772,753	639,395	508,662	368,058

Notes: Impact of transparency regulation on the gender wage gap and the wages of men and women individually. Estimates from difference-in-discontinuity specification. Individual time-varying controls include age squared, education and part-time occupation. Includes observations from 2011 to 2019. Standard errors are clustered at the firm level. T-statistics in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8: Diff-in-Disc with different bandwidths

C.2 Results from SIEED

Our secondary data source is the German Sample of Integrated Employer-Employee Data (SIEED). This employer-employee matched administrative data set covers 1.5% of all German establishments and contains information on employment spells of all employees. Employee-level demographic information includes age, completed education and whether the work was part-time. Data at the establishment level, including the total number of employees, are obtained from the linked Establishment-History-Panel (BHP). A detailed description of SIEED can be found in Schmidtlein et al. (2020).

We observe employment spells from 2011 to 2018. We again discard all observations with a zero wage, indicating employment interruptions. This leaves 1,842,584 relevant observations from 544,437 individuals at 16,049 firms in our main sample, substantially more than in our primary analysis. Table A9 reports summary statistics for this data set, Table A10 the Diff-in-Diff analysis, Table A11 the Diff-in-Disc analysis and Figure A3 provides the event study specification.[31]

	Men		Women	
	Large firms (1)	Small firms (2)	Large firms (3)	Small firms (4)
Daily Wage	98.18	96.96	67.75	67.65
	(54.18)	(52.62)	(45.88)	(45.25)
Age	43.66	43.56	43.67	44.00
	(11.98)	(12.01)	(11.80)	(11.80)
College educated	17.34%	16.18%	15.87%	15.72%
Part-time	16.00%	15.20%	53.73%	52.99%
Firms	5,755	10,162	5,623	9,840
Individuals	126,111	179,476	106,102	152,222
Observations	415,813	594,111	340,610	492,050

Notes: This table reports unconditional means and standard deviations in parentheses of key variables for individuals in large and small firms, split by gender. The descriptive statistics include all data in our SIEED panel from 2011 to 2018 in firms with 150 to 250 employees in 2018. 'Age' refers to the employee's age in years, 'College educated' is an indicator of whether the employee has at least some university or university of applied sciences education, and 'Part-time' is an indicator of whether the employee works part time.

Table A9: Summary statistics using SIEED

[31]Source DOI: 10.5164/IAB.FDZD.2014.en.v1, own calculations. We use these data for all results in Section C.2.

	Log of daily wage					
	Both gender (1)	Men (2)	Women (3)	Both gender (4)	Men (5)	Women (6)
Large × Post	0.0006	0.0009	-0.0024	0.0023	0.0023	0.0018
	(0.0022)	(0.0022)	(0.0030)	(0.0027)	(0.0026)	(0.0030)
Female × Large × Post	-0.0032			-0.0004		
	(0.0034)			(0.0036)		
Female × Large	-0.0134			0.021		
	(0.0158)			(0.0142)		
Female × Post	0.0213***			0.0139***		
	(0.0021)			(0.0022)		
Individual time-varying controls	✓	✓	✓			
Firm FE	✓	✓	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
Firm size	150-250	150-250	150-250	150-250	150-250	150-250
Observations	1,137,638	632,974	504,269	1,652,424	909,136	742,997

Notes: Impact of transparency regulation on the gender wage gap and the wages of men and women individually. Estimates from difference-in-difference specification. Individual time-varying controls include age squared, education and part-time occupation. Includes observations from 2011 to 2018 in SIEED. Standard errors are clustered at the firm level and shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

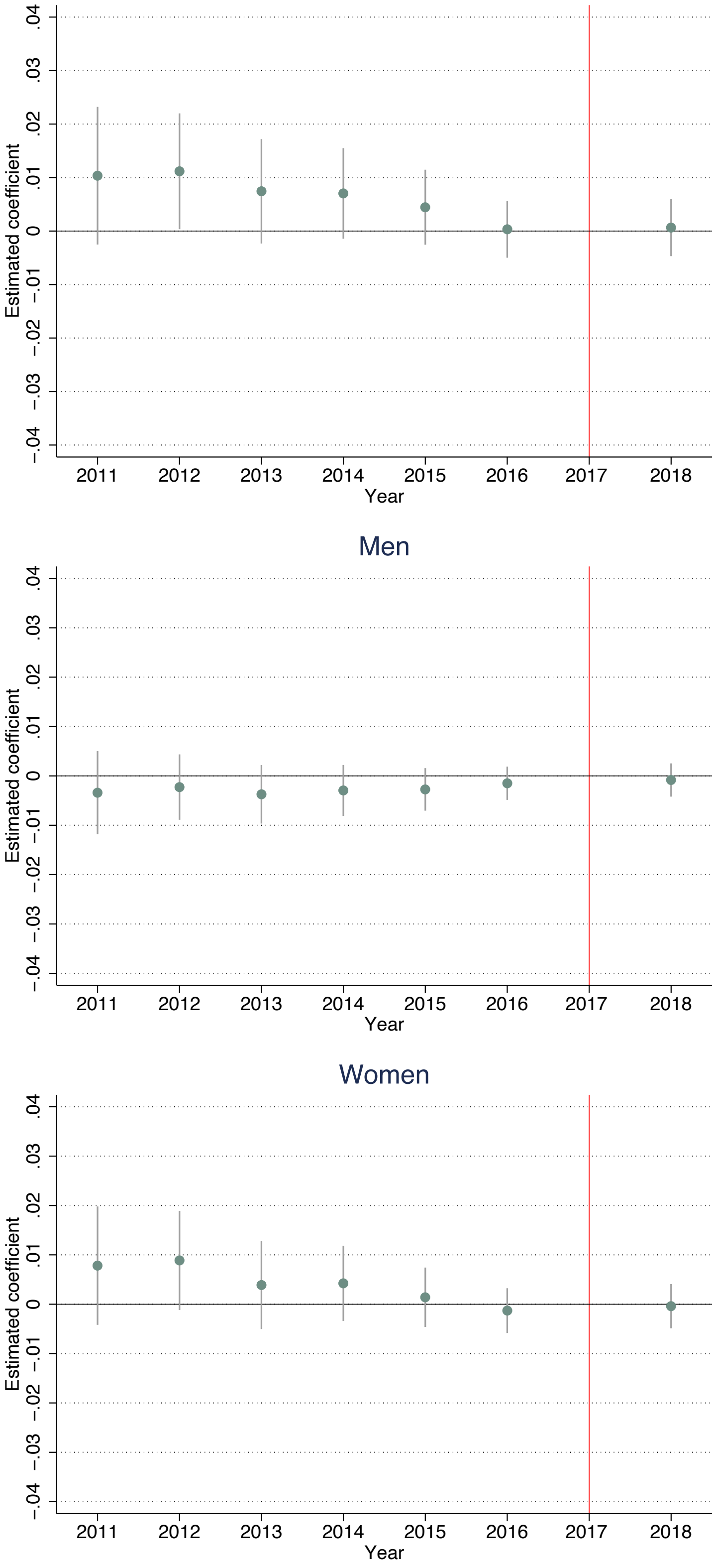
Table A10: Diff-in-Diff estimates using SIEED

	Log of daily wage		
	Both gender (1)	Men (2)	Women (3)
Diff-in-disc	-0.0028	-0.0028	-0.0024
	(0.0209)	(0.0209)	(0.0221)
Female × Diff-in-disc	0.0002		
	(0.0192)		
Time FE	✓	✓	✓
Firm size	150-250	150-250	150-250
Observations	1,833,178	1,006,963	826,215

Notes: Impact of transparency regulation on the gender wage gap and the wages of men and women individually. Estimates from difference-in-discontinuity specification. Individual time-varying controls include age squared, education and part-time occupation. Includes observations from 2011 to 2018 in SIEED. Standard errors are clustered at the firm level and shown in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A11: Diff-in-Disc estimates using SIEED



Notes: Event study analysis of the impact of wage transparency regulation on log of daily wage. The top figure provides the differential impact for women vs. men, the bottom two figures separate event study specifications. Firms with more than 200 employees are classified as treated. Individual-, firm- and year-fixed effects are included. Time varying controls include age squared, education and part-time workers. 1,137,638 observations, including both men and women. Error bars indicate the 95% confidence interval. Standard errors are clustered at the firm level.

Figure A3: Gender-specific effects of the transparency law

C.3 Heterogeneity analysis

	Log of daily wage						
	Both gender (1)	Men (2)	Women (3)	Both gender (4)	Men (5)	Women (6)	Both gender (7)
Large × Post	0.0002	-0.0011	0.0053	0.0053	0.0052	0.0114	0.0245
	(0.03)	(-0.11)	(0.56)	(0.43)	(0.42)	(1.05)	(0.98)
Female × Large × Post	0.0038			0.0062			-0.0399
	(0.34)			(0.45)			(-1.45)
Female × Large	-0.0170			-0.0513			
	(-0.07)			(-0.21)			
Female × Post	0.0100			0.0014			0.0219
	(1.25)			(0.15)			(0.88)
Ind. time-varying controls	✓	✓	✓				✓
Firm FE	✓	✓	✓	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓
Firm size	150-250	150-250	150-250	150-250	150-250	150-250	150-250
Observations	191,844	101,765	90,078	234,541	122,689	111,851	41,699

Notes: Impact of transparency regulation on the gender wage gap and the wages of men and women individually in establishments bound by an industry-wide or firm-level wage agreement in columns (1) - (6). Column (7) also considers establishments that are not bound by collective bargaining agreements, but base their wages on these agreements. Estimates from difference-in-difference specification. Individual time-varying controls include age squared, education and part-time occupation. Includes observations from 2011 to 2019. Standard errors are clustered at the firm level. T-statistics in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A12: Diff-in-Diff estimates for impact of wage transparency on daily wages in establishments bound by wage agreements

	Log of daily wage						
	Both gender (1)	Men (2)	Women (3)	Both gender (4)	Men (5)	Women (6)	Both gender (7)
Large × Post	0.0051	0.0038	-0.0204	0.0137	0.0138	-0.0300	-0.0218
	(0.24)	(0.19)	(-0.85)	(0.42)	(0.42)	(-1.30)	(-1.28)
Female × Large × Post	-0.0218			-0.0435			0.0010
	(-0.97)			(-1.44)			(0.04)
Female × Large	-0.0066			0.1147			0.0066
	(-0.07)			(0.98)			(0.07)
Female × Post	0.0233			0.0083			0.0277*
	(1.33)			(0.47)			(1.92)
Ind. time-varying controls	✓	✓	✓				✓
Firm FE	✓	✓	✓	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓
Firm size	150-250	150-250	150-250	150-250	150-250	150-250	150-250
Observations	62,822	36,392	26,430	80,801	48,730	32,070	21,108

Notes: Impact of transparency regulation on the gender wage gap and the wages of men and women individually in establishments not bound by an industry-wide or firm-level wage agreement in columns (1) - (6). Column (7) only considers establishments that also do not base their wages on these agreements. Estimates from difference-in-difference specification. Individual time-varying controls include age squared, education and part-time occupation. Includes observations from 2011 to 2019. Standard errors are clustered at the firm level. T-statistics in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A13: Diff-in-Diff estimates for impact of wage transparency on daily wages in establishments not bound by wage agreements

D Additional analyses of laboratory data

	Worker's wage	
	(1)	(2)
Worker contribution	0.55***	0.48***
	(0.04)	(0.03)
Firm contribution	0.24**	0.24**
	(0.09)	(0.10)
Endo wage	-10.81	
	(18.82)	
Exo wage	21.52	
	(18.95)	
Endo wage × Worker contribution	0.02	
	(0.05)	
Exo wage × Worker contribution	-0.03	
	(0.06)	
Performance		-41.81**
		(16.98)
Performance × Worker contribution		0.14***
		(0.05)
Constant	-1.71	20.47
	(35.84)	(35.42)
Part FE	✓	✓
Period FE	✓	✓
Laboratory FE	✓	✓
Observations	1548	1548
Clusters	66	66
R-squared	0.265	0.268

Notes: Results are from ordinary least squares regression of the worker's wage, restricting the sample to periods in which subjects enter negotiations. Worker contribution is a control for the worker's contribution to the negotiation pie, Firm contribution for the firm's contribution to the negotiation pie. Endo wage and Exo wage are indicators of whether wage information was provided endogenously or exogenously, respectively. Female indicates whether a participant is female. Performance is an indicator of whether information of the workers' performances is provided. Standard errors are clustered at the matching-group level and shown in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A14: The interaction of wage and performance information with the worker's contribution

	Worker's decision to opt out of negotiations				
	(1)	(2)	(3)	(4)	(5)
Worker contribution	-0.034***	-0.037***	-0.035***	-0.036***	-0.034***
	(0.007)	(0.008)	(0.007)	(0.007)	(0.007)
Female	0.026*		0.004		0.028**
	(0.013)		(0.015)		(0.014)
Wage info		0.033***	0.016		
		(0.012)	(0.013)		
Wage info × Female			0.033		
			(0.025)		
Performance info				0.009	0.011
				(0.010)	(0.010)
Performance info × Female					-0.004
					(0.016)
Constant	0.125***	0.126***	0.116***	0.141***	0.119***
	(0.027)	(0.027)	(0.028)	(0.029)	(0.025)
Part FE	✓	✓	✓	✓	✓
Period FE	✓	✓	✓	✓	✓
Laboratory FE	✓	✓	✓	✓	✓
Observations	1546	1546	1546	1546	1546
Clusters	66	66	66	66	66
R-squared	0.061	0.063	0.070	0.058	0.062

Notes: Results are from OLS regression of the participant's (binary) decision to opt out of negotiations. Worker contribution is a control for the worker's contribution to the negotiation pie (in hundred units). Female indicates whether a participant is female. Wage info is an indicator of whether wage information was (potentially) provided. Performance is an indicator of whether information of the workers' performances is provided. Standard errors are clustered at the matching-group level and shown in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A15: The effect of information on opting out of negotiations

	Worker's wage							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Worker contribution	0.53***	0.53***	0.53***	0.53***	0.53***	0.54***	0.53***	0.53***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Firm contribution	0.25**	0.25**	0.24**	0.25**	0.24**	0.24**	0.25**	0.25**
	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)
Endo wage	-0.04			-2.46			1.11	-2.84
	(10.06)			(14.00)			(12.86)	(17.79)
Exo wage	14.37	14.39*		15.19			18.63	24.01
	(8.99)	(7.93)		(12.65)			(11.63)	(16.06)
Female			5.79	4.88		8.65		9.90
			(6.04)	(12.21)		(9.09)		(18.02)
Endo wage × Female				4.90				7.85
				(14.96)				(22.68)
Exo wage × Female				-1.67				-11.43
				(15.75)				(22.49)
Performance					11.82**	14.63*	15.36*	20.59
					(5.31)	(7.49)	(8.48)	(13.24)
Performance × Female						-5.78		-10.77
						(9.94)		(20.46)
Performance × Endo wage							-2.17	0.36
							(12.87)	(18.01)
Performance × Exo wage							-8.49	-18.08
							(12.90)	(18.73)
Performance × Endo wage × Female								-5.24
								(25.03)
Performance × Exo wage × Female								20.22
								(25.57)
Constant	3.17	3.15	5.86	-1.23	2.36	-4.00	-5.81	-12.44
	(36.91)	(36.09)	(37.87)	(39.25)	(37.75)	(38.90)	(36.57)	(39.82)
Part FE	✓	✓	✓	✓	✓	✓	✓	✓
Period FE	✓	✓	✓	✓	✓	✓	✓	✓
Laboratory FE	✓	✓	✓	✓	✓	✓	✓	✓
Observations	1486	1486	1486	1486	1486	1486	1486	1486
Clusters	66	66	66	66	66	66	66	66
R-squared	0.247	0.247	0.245	0.248	0.246	0.247	0.249	0.251

Notes: Results are from ordinary least squares regression of the worker's wage, restricting the sample to periods in which subjects enter negotiations. Worker contribution is a control for the worker's contribution to the negotiation pie, Firm contribution for the firm's contribution to the negotiation pie. Endo wage and Exo wage are indicators of whether wage information was provided endogenously or exogenously, respectively. Female indicates whether a participant is female. Performance is an indicator of whether information of the workers' performances is provided. Standard errors are clustered at the matching-group level and shown in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A16: The effect of wage and performance information conditional on negotiation entry

	Worker's wage				
	(1)	(2)	(3)	(4)	(5)
Worker contribution	0.57***	0.70***	0.61***	0.50***	0.55***
	(0.03)	(0.05)	(0.03)	(0.03)	(0.04)
Firm contribution	0.06	0.07	0.23*	0.25**	0.35***
	(0.18)	(0.18)	(0.13)	(0.12)	(0.10)
Info choice	-17.77	76.86**			
	(11.72)	(30.23)			
Info choice × Worker contribution		-0.25***			
		(0.08)			
Endo wage			4.79		
			(11.80)		
Exo wage				26.14**	23.32
				(11.69)	(19.38)
Exo Wage × Worker contribution					-0.03
					(0.06)
Constant	67.34	19.77	-17.90	4.63	-41.58
	(66.20)	(69.78)	(48.85)	(42.52)	(39.47)
Part FE	✓	✓	✓	✓	✓
Period FE	✓	✓	✓	✓	✓
Laboratory FE	✓	✓	✓	✓	✓
Sample	*EndoWage*	*EndoWage*	*No wage info*	*Wage info*	*NoWage & ExoWage*
Observations	515	515	789	759	1033
Clusters	22	22	44	44	44
R-squared	0.272	0.284	0.303	0.240	0.272

Notes: Results are from ordinary least squares regression of the worker's wage. Worker contribution is a control for the worker's contribution to the negotiation pie, Firm contribution for the firm's contribution to the negotiation pie. Endo wage and Exo wage are indicators of whether wage information was provided endogenously or exogenously, respectively. Info choice indicates whether the participant chose to receive wage information. Standard errors are clustered at the matching-group level and shown in parentheses. Sample refers to the treatment(s) from which the observations for the analysis stem; *No Wage info* refers to observations from *NoWage* and individuals choosing no wage information in *EndoWage*, *Wage info* refers to observations from *ExoWage* and individuals choosing wage information in *EndoWage*.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A17: Effects of requesting wage information on wages

	Difference in beliefs			
	(1)	(2)	(3)	(4)
Error in belief of other's wage		-0.02**		
		(0.01)		
Error in belief of other's performance				-7.25***
				(2.56)
Constant	2.89	4.36	43.96***	44.49***
	(7.63)	(8.00)	(15.07)	(14.97)
Part FE	✓	✓	✓	✓
Laboratory FE	✓	✓	✓	✓
Observations	144	144	128	126
Clusters	44	44	41	41
R-squared	0.012	0.020	0.039	0.077

Notes: Results are from ordinary least squares regression of the difference in beliefs between *Elicitation 2* and *Elicitation 3*. Error in belief of other's wage is defined as the difference between the subject's beliefs about the comparable worker's wage and the comparable worker's actual wage. Error in belief of other's performance is defined as the difference between the subject's beliefs about the comparable worker's number of correctly solved in the production tasks and the comparable worker's actual number correctly solved elements. Standard errors are clustered at the matching-group level and shown in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A18: Changes in beliefs between *Elicitation 2* and *Elicitation 3*

	Negotiation breakdown	
	(1)	(2)
Worker contribution	-0.01	-0.00
	(0.01)	(0.01)
Firm contribution	-0.04*	-0.05**
	(0.02)	(0.02)
Optimistic	0.06***	
	(0.02)	
Overconfident		-0.00
		(0.02)
Constant	0.27***	0.30***
	(0.09)	(0.09)
Part FE	✓	✓
Period FE	✓	✓
Laboratory FE	✓	✓
Observations	1548	1545
Clusters	66	66
R-squared	0.026	0.014

Notes: Results are from ordinary least squares regression of an indicator that negotiations broke down and resulted in zero payoff for worker and firm. Worker contribution is a control for the worker's contribution to the negotiation pie (in hundred units), Firm contribution for the firm's contribution to the negotiation pie (in hundred units). Optimist indicates that a subject's beliefs about the comparable worker's wage are too optimistic, Overconfident indicates that a subject's beliefs about his or her own performance relative to the comparable worker's are too optimistic. Standard errors are clustered at the matching-group level and shown in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A19: Negotiation breakdown by type

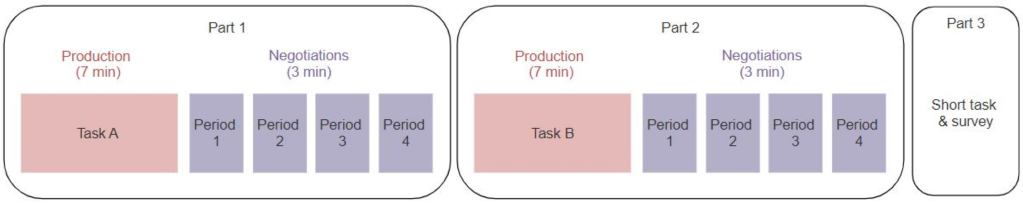
E Experimental instructions

Instructions

Please read these instructions carefully.

If you follow the instructions carefully, you may earn a considerable amount of money. Your earnings will depend on your decisions and may depend on other participants' decisions as well as chance.

This experiment consists of 3 parts. In the first two parts, a firm and a worker negotiate a wage for the worker for producing output. First, the worker produces an output. Then, firms and workers will be randomly matched and negotiate over a wage for the worker. There will be **4 negotiation rounds in every part** after each production stage. In part 3, there will be a short task and a survey. Part 3 is independent of part 1 and 2. The graph below shows the flow of the experiment.



Production stage

The production stage determines the total number of points that workers and firms can split during negotiations, called the budget. The budget is determined by the sum of the firm's and the worker's contributions. The firm knows the size of the budget, the worker does not. It is generated as follows:

Firm For each part, every firm draws a random number between between 3000 points and 450 points as the **fixed firm contribution**. This fixed contribution cannot be influenced by the firm and remains the same for the firm during a part. Each firm has a different draw for the firm contribution.

Worker Every worker has to perform a task. They are asked to solve as many elements as possible in seven minutes. At a later stage, more detailed instructions about these tasks will be provided. There will be different tasks for part 1 and part 2. We will call the number of elements solved in a task the **worker's performance**. The more elements the worker solves, so the higher the worker's performance is, the more points can be split between the worker and the firm. The worker increases the budget by by **35/20 points in part 1** for each correctly solved element, and by **35/20 points in part 2**.

Although firms do not have to perform the tasks, they will be shown the task that workers have to perform. The performance of firms in these tasks does not have any consequences for the budget or anyone's payoff.

In sum, the budget is the number of correctly solved elements by the worker multiplied by 35/20 (part 1) or 35/20 (part 2), plus the fixed contribution by the firm.

Negotiation stage

There are four negotiation periods in each negotiation stage. Every period, a worker and a firm are randomly paired and negotiate to split the budget they generated together. Within a part, you have a new negotiation partner in each period. This means that if a firm and a worker are paired in a period, they will be paired with someone else the next period of that part. Since a new pair is formed each period, the budget that can be split between a firm and a worker differs from period to period.

Timeline In period 1, all workers automatically enter negotiations. At the start of the subsequent periods 2-4, the worker must decide whether or not to enter negotiations. If the worker decides not to enter negotiations, the worker will receive 150 points and the firm receives the remainder. If the worker does not enter negotiations, both the worker and the firm will have to wait for other pairs, while they negotiate; the new period starts after all negotiations have ended.

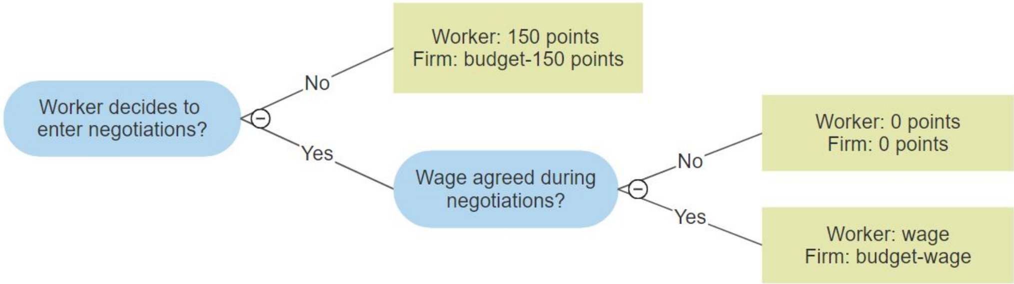
In period 1 and in later periods, if the worker decides to enter negotiations:

1. Both the worker and the firm submit an **initial wage proposal**. The firm can offer to the worker a wage between zero and the budget generated for this period. The worker can request a positive wage. These initial wage proposals are not binding. Workers and firms still need to submit binding wage proposals later on.

2. The worker and the firm have 3 minutes to negotiate a wage. For the negotiation, they will use a **chat**. No personally identifiable information such as names, age or gender is allowed in the chat. They can enter binding wage proposals in a separate entry field. If they agree on a wage proposed by their negotiation partner, they can **click on 'accept'**.

3. If a wage agreement is reached, this wage is implemented. If there is no agreement, that is, neither the firm nor the worker accepted the other's wage proposal, both the worker and the firm receive a payoff of zero points for this period.

See below for a graphical outline of the negotiation stage.



What do you know when you negotiate?

Workers and firms have different information when negotiating. Workers also have different information in part 1 and part 2.

After the first period of each part, the worker has certain information about a **comparable worker**. In periods 2,3 and 4, the comparable worker is always the worker that was paired to the same firm in period 1 as the worker is paired to in the current period. The comparable worker did the same production task.

For example, if worker A was paired to firm X in period 1 and worker B gets paired to firm X in period 2, then worker A will be the comparable worker for worker B in period 2. If worker C is paired to firm X in period 3, worker A will be the comparable worker for worker C in period 3.

Performance information [ORDER DEPENDS ON TREATMENT:

In part 1, the worker and the firm know the worker's performance in this part's production task as well the comparable worker's performance in the same production task.

In part 2, neither the worker nor the firm receive any information about the worker's performance in that part's production task. The worker and the firm also do not know the comparable worker's performance.]

Wage information [ENDOWAGE: In both part 1 and part 2, the worker can decide whether he or she wants to receive information on the comparable worker's wage. Buying this information costs 10 points. **If the worker acquires information, he or she will be told the wage that the comparable worker received**. This is the wage that the firm with which the worker is currently paired to paid another worker in the first period.]

[NOWAGE: In both part 1 and part 2, **the worker does not know the wage of the comparable worker**.]

[EXOWAGE: In both part 1 and part 2, **the worker will be told the wage that the comparable worker received**. This is the wage that the firm with which the worker is currently paired to paid another worker in the first period.]

There is no information on the firm's fixed contribution. As stated before, workers do not know the size of the budget that can be split in each period.

In contrast, the firm always knows the size of the budget. Firms also know all other information that is provided to the worker, including information about the comparable worker.

Payoff summary

For this experiment, you will be paid a show-up fee of 6 Euros. Additionally, you will be paid based on your decisions in the experiment.

To summarize, the payoffs for the worker and the firm in a period are the following:

- If the worker does not enter negotiations: 150 points for the worker, the budget minus 150 points for the firm.
- If the worker enters negotiations: The agreed upon wage for the worker and the budget minus the wage for the firm if an agreement is reached, zero points for both worker and firm if no agreement is reached.

One period from either part 1 or part 2 is randomly selected for payment. All periods are equally likely to be selected. Your decisions do not have any influence on the probability that a certain period is selected for payment.

[WORKER: Furthermore, you will be paid based on your estimate of wages and of performances and for the short task in part 3. You will receive detailed information about the payment of these task later on. You will also receive 4 Euros for completing the questionnaire at the end.]

At the end of the experiment, points will be converted to Euros. 25 points will be converted to one Euro. So each point is worth 0.04 Euros.

Your role

You will have the role of [WORKER: **a worker**] [FIRM: **a firm**].

Instruction Summation Task

In this task you have to find the largest numbers in two different matrices and sum them up.

Each element contains two matrices. Every matrix contains exactly 49 numbers, displayed in seven rows and seven columns. The numbers are randomly generated by the computer. First, find the largest number in each of the two matrices. Then, find the sum of these two numbers and enter your answer.

As an example, see the two matrices below. In the left matrix, the largest number is 85. In the right matrix, the largest number is 79. The sum of 85 and 79 is 164. The correct answer for this example is therefore 164.

17	59	23	31	11	35	53
40	53	57	11	18	61	20
42	84	12	29	43	45	28
29	23	33	45	30	25	38
20	24	85	15	72	21	47
36	36	16	58	45	16	26
76	15	60	52	29	14	26

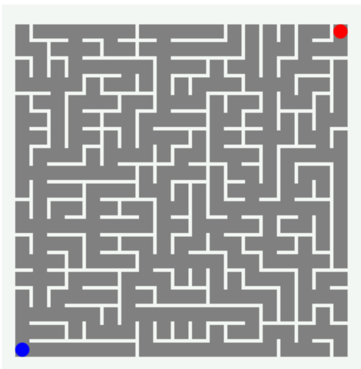
17	39	22	21	34	16	41
74	79	31	13	31	21	13
22	19	17	16	27	41	14
51	58	60	17	40	60	17
27	37	50	79	75	19	34
78	47	51	75	25	58	16
12	10	21	15	70	44	28

Your goal is to solve as many elements as you can within 7 minutes (you can answer up to 50 questions in total). For every question that you solve correctly, the negotiation-stage budget is increased by 35 points.

Instruction Maze Task

In this task you must navigate through a maze. Your current position in indicated by a blue dot, which always starts in the bottom-left corner of the maze. The end of the maze is indicated by a red dot, which always appears in the upper-right corner of the maze.

You can move the blue dot using the arrow keys on your keyboard. Walls of the maze are shown in white. An example maze is shown below.



Your goal is to solve as many mazes as you can within 7 minutes by moving the blue dot onto the exit marked in red. For every maze that you solve, the negotiation-stage budget is increased by 20 points.

Belief elicitation[32]

Estimates about performance

Please provide an estimate of your performance and the performance of another (randomly chosen) worker in the [summation task] [maze task]. Please enter below how many elements you think that you and the randomly chosen worker solved correctly.

At the end of the experiment, one of the questions about your estimates will be chosen for payment. You will receive a bonus of 3 Euros if your guess is close enough to the actual answer. It is in your interest to provide accurate guesses, as this increases the probability of receiving the bonus. If you would like to know more about the mechanism we use to determine whether you receive this bonus, feel free to click on the button below.

Optional: Click here for information about the mechanism

[IF CLICKED: If a question is chosen for payment, the probability that you receive a bonus payment of 3 Euros will depend on your prediction error. This prediction error is the distance between your estimate and the correct number. The closer your estimate is to the correct answer, the larger is the probability that you will receive the bonus.

Assume that your actual performance is X solved [summations] [mazes] and you guessed that you had Y solved [summations] [mazes]. In this case your squared prediction error is $(X - Y)^2$. To determine the probability of receiving the bonus, the computer first draws a number between 0 and 20, let's call this number T. Then this number T is compared to your squared prediction error. If T is larger than the squared error, you will receive the bonus payment for this question. If your squared prediction error is larger than or equal to T, you will not receive a bonus for this question.]

Estimates of wage of others

Please provide an estimate of the wage of another (randomly chosen) worker in the [summation task] [maze task]. Please enter below how many points you think that the randomly chosen worker received in the last negotiation period.

At the end of the experiment, one of the questions about your estimates will be chosen for payment. You will receive a bonus of 3 Euros if your guess is close enough to the actual answer. It is in your interest to provide accurate guesses, as this increases the probability of receiving the bonus. If you would like to know more about the mechanism we use to determine whether you receive this bonus, feel free to click on the button below.

Optional: Click here for information about the mechanism

[IF CLICKED: If a question is chosen for payment, the probability that you receive a bonus payment of 3 Euros will depend on your prediction error. This prediction error is the distance between your estimate and the correct number. The closer your estimate

[32] The instructions for the belief elicitation are adapted from Babcock et al. (2017).

is to the correct answer, the larger is the probability that you will receive the bonus.

Assume that your actual performance is X solved [summations] [mazes] and you guessed that you had Y solved [summations] [mazes]. In this case your squared prediction error is $(X - Y)^2$. To determine the probability of receiving the bonus, the computer first draws a number between 0 and 40000, let's call this number T. Then this number T is compared to your squared prediction error. If T is larger than the squared error, you will receive the bonus payment for this question. If your squared prediction error is larger than or equal to T, you will not receive a bonus for this question.]

Estimates about performance

You previously estimated the performance of another worker in the [summation task] [maze task]. Now you know your comparable worker's wage.

- **Your estimate was that a worker solved [x] elements correctly.**
- **The wage your comparable worker received in the previous negotiation period was [y] points.**

We would like to know your estimate of your comparable worker's performance. Please enter below how many elements you now think the comparable worker solved correctly.

At the end of the experiment, one of the questions about your estimates will be chosen for payment. You will receive a bonus of 3 Euros if your guess is close enough to the actual answer. It is in your interest to provide accurate guesses, as this increases the probability of receiving the bonus. If you would like to know more about the mechanism we use to determine whether you receive this bonus, feel free to click on the button below.

Optional: Click here for information about the mechanism

[IF CLICKED: If a question is chosen for payment, the probability that you receive a bonus payment of 3 Euros will depend on your prediction error. This prediction error is the distance between your estimate and the correct number. The closer your estimate is to the correct answer, the larger is the probability that you will receive the bonus.

Assume that your actual performance is X solved [summations] [mazes] and you guessed that you had Y solved [summations] [mazes]. In this case your squared prediction error is $(X - Y)^2$. To determine the probability of receiving the bonus, the computer first draws a number between 0 and 20, let's call this number T. Then this number T is compared to your squared prediction error. If T is larger than the squared error, you will receive the bonus payment for this question. If your squared prediction error is larger than or equal to T, you will not receive a bonus for this question.]