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## ORIGINAL ARTICLE

# Do outside options drive wage inequalities in retained jobs? Evidence from a natural experiment

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This article uses the Linked Employer Employee Data of the IAB (LIAB9310, longitudinal version). Data access was via guest research spells at FDZ and afterwards via controlled remote data access at FDZ.

## Abstract

We provide evidence that suggests that a reduction in outside wage options reduces wage increases in retained jobs. We use the natural experiment of a reform that reduced outside wage options for employees in deregulated crafts occupations in comparison to employees in not reformed crafts occupations. To avoid estimation biases from general reform effects on wages, we concentrate on employees active in crafts occupations who worked for employers in the industry and commerce sectors and exclude employees in the crafts sector. Four years after the reform, the wages of treated employees in deregulated crafts were 5 per cent lower than wages of employees in not reformed occupations (control group). The reform, therefore, led to wage differentiation between comparable employees. The wage effects are concentrated in employers with high general wage increases after the reform and they can be found even at individual employers.

## 1 | INTRODUCTION

Changes in their outside wage options may drive the wages of employees. However, the following questions have not been answered so far in the literature: Do the wages of employees in retained jobs react to changes in their outside wage options? Do employers accept an increase in wage

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inequality if only a sub-group of their workforce faces a change in outside options? Do employers even decrease wages when their employees' outside options are diminished? A series of recent articles has measured the causal impact of changes in outside wage options on employee wages (Beaudry et al., 2012; Caldwell & Danieli, 2022; Caldwell & Harmon, 2019; Hafner, 2022; Staiger et al., 2010). These contributions mainly used classic Nash bargaining models between employers and employees (Mortensen & Pissarides, 1999; Pissarides, 2000, chapter 1; Cahuc et al., 2006; Hall & Milgrom, 2008; Jäger et al., 2020; Michelacci & Suarez, 2006) that 'give primacy to the bargainers' outside options' as wage determinants (Hall & Krueger, 2010: p. 8). However, these articles could not include the employer dimension, and thus, it remains unclear which employer type reacts more strongly to outside wage changes and whether the wage differentiation induced by outside wage changes can also be found at a single employer. In the present work, we can analyse heterogeneities in the wage effect between different employer types for the first time because we use linked employer-employee data. We can also identify whether employers differentiate wages among comparable employee groups in their workforce. Finally, we examine a reduction in outside wage options of a sub-group of employees and can identify whether employers even reduce wages when employees face lower outside wage options. In contrast, all previous articles have examined exogenous increases in outside wage offers or improvements in labour market options.

In traditional wage bargaining models, non-employment (e.g. invalidity or early retirement pensions) or unemployment benefits are posited as workers' outside options (Lachowska, 2017; Ljungqvist & Sargent, 2017; Pissarides, 2000). Thus, according to these models, changes in outside wage options should not affect employee wages in retained jobs, although some authors argue that outside wage offers are the relevant outside options for employees in retained jobs, rather than unemployment benefits (Bagger et al., 2014; Cahuc et al., 2006; Caldwell & Harmon, 2019; Manning, 2003; Postel-Vinay & Robin, 2002).

To identify the causal impact of changes in outside wage options, recent empirical articles use the spill-over effects of a change in wage-setting choices of a clearly defined group of retained employees at employers unaffected by these changes. They analyse whether deviations from 'market' wages in one employee group have spill-over effects on other employees' wages (Card, 2022). Staiger et al. (2010) emphasized that an exogeneous wage increase for registered nurses at the US Department of Veterans Affairs hospitals induced wage increases for nurses at neighbouring hospitals. Beaudry et al. (2012) showed that high wage premia in one economic sector spill over to other economic sectors in the same US city. Caldwell and Harmon (2019) revealed that retained employees with improved information on job opportunities at former employers can increase their wages in Denmark. Caldwell and Danieli (2022) used the increase in the labour market options for retained employees who live in small German towns near newly built high-speed commuter train stations. They found that the better outside labour market options made available by a reduction in commuting times and a strong growth in the local industry sector led to higher wages. Hafner (2022) highlighted that lifting cross-border labour market barriers between France and Switzerland increased wages for retained employees in French border regions compared with employees in regions further away from the border.

The main challenge in assessing the impact of changes in outside wage options in retained jobs is that employees' outside options usually cannot be observed (Caldwell & Danieli, 2022). In addition, a change in employees' outside options is frequently correlated with changes in the competitive situation of their employer, and consequently also with the productivity of employees, the employer's surplus, and the bargaining power of employer and employee (Beaudry et al., 2012), which also influence wages (Guiso et al., 2005). To avoid biases in the measurement of the impact

of changes in outside wage options on wages in retained jobs, we exploit situations in which the productivity and bargaining power of the employer are arguably not affected by the change in outside options of the treated employees.

This article analyses the effects of a shock to outside wage options on wage changes in retained jobs for a clearly defined sub-group of employees compared with wage changes of a similar sub-group of employees unaffected by the shock. We use the 2004 crafts reform in Germany that removed the requirement to hold a ‘master craftsman’ certificate (*Meisterbrief*) to establish a business in about half of the crafts occupations. The reform decreased the wages of incumbent employees in the deregulated occupations in crafts businesses directly affected by the reform (our treatment group) compared with employees in crafts occupations that remained regulated (our control group) (Damelang et al., 2018; Lergetporer et al., 2018; Rostam-Afschar, 2014).<sup>1</sup> We separate the effects of changes in outside options from changes in productivity or bargaining power by exploiting a peculiarity of the German labour market. Many skilled employees exercise their crafts occupation not in the crafts sector but in industry or commerce firms. To avoid that the general impact of the crafts reform biases our results, we reduce the sample to employees active in crafts occupations who worked for employers in the industry and commerce sector and exclude employees in the crafts sector.

We show that wages in the treatment group increased less dynamically compared with wages in the control group. Four years after the reform, wage differences between both groups amounted to about 5 per cent of the total wage increase during this period (a wage difference of 0.5 per cent in a 10 per cent total wage increase). This result seems remarkable given the strong downward wage rigidity in Germany (Dustmann & Schönberg, 2009; Hirsch & Zwick, 2015). We argue that the wage differences are driven by higher wage increases for employees in the control group. Employees in the treatment group do not suffer from wage renegotiations initiated by their employers, but they are less successful in their wage bargaining attempts.

## 2 | A MODEL FOR WAGE BARGAINING IN RETAINED JOBS

We model the circumstances under which changes in outside wage options spill over into changes in wages in retained jobs. We use a wage bargaining model with on-the-job bargaining and assume that employers compete *à la* Bertrand for workers by wage negotiations (compare Bartolucci, 2012; Cahuc et al., 2006; Postel-Vinay & Robin, 2002; and Caldwell & Harmon, 2019). Workers search while employed and when they receive an outside offer that is higher than the previous outside option, they can use it to renegotiate wages in their job or move directly from job to job. Most wage bargaining models concentrate on employees who translate higher outside options into higher wages in retained jobs by initiating wage renegotiations. Our model complements this mechanism and includes employers who use lower outside options of their employees for renegotiations that lead to lower wages. The renegotiation power of the employees and the firms is limited by the fact that contract renegotiations with retained employees are only based on mutual agreement; thus, wage renegotiation is only possible for employers and employees with a credible threat of ‘leaving the match’ (Bartolucci, 2012). If the credible threat is non-permanent, we assume that the matching partner still honours the negotiated wage after the credible threat disappears. Consequently, retained workers can ratchet up their wages when they have one or more temporary higher outside wage offers during their career, and employers can reduce wages for retained jobs permanently even when outside wage options recover.

The Nash wage bargaining equilibrium is the weighted average of the inside value of a job with productivity  $p$  and the worker's best outside option  $N$ , plus the share  $\phi$  of the job surplus  $(p - N)$  with  $\phi$  denoting the employee bargaining power relative to the employer bargaining power (Jäger et al., 2020). Function  $V(p,w)$  is the lifetime expected utility of a worker who is employed at a firm with productivity  $p$  and receives wage  $w$ . It is assumed that the match- or firm-specific productivity at the current firm is  $p_o$ . Thus, the maximum value obtainable in a retained job at the current employer is  $V(p_o, p_o)$ . If non-employment transfers  $N$  are the best outside option, the highest utility the employee can receive in the current job is

$$V(p_o, w(p_o, N)) = \phi \times V(p_o, p_o) + (1 - \phi) \times N. \quad (1)$$

It is assumed that the worker currently has a wage below the maximum possible wage,  $w_o < p_o$ . If the worker receives an outside offer from a firm with productivity  $p_n < p_o$ , the worker does not move to the new employer because the expected lifetime utility of the outside job is lower than the lifetime utility at the present job. Nevertheless, the worker can renegotiate the current wage with the external offer  $w_o \geq w_n > N$  as the new, higher outside option of<sup>2</sup>

$$V(p_o, w(p_o, p_n)) = \phi \times V(p_o, p_o) + (1 - \phi) \times V(p_n, w_n). \quad (2)$$

The logic behind this result is that the worker can take the new wage offer and switch to the outside firm. A return to the old job, however, increases utility if  $p_n < p_o$ . The worker, therefore, can contact the previous employer after a short period of time to agree a new contract that takes  $V(p_n, w_n)$  as the realized outside option. For the previous employer, it is still profitable to re-hire the worker if  $w_n < p_o$ , and thus, the new wage in the retained job is higher.<sup>3</sup>

In this scenario, the current wage is determined by the best outside option the employee uses for a successful wage renegotiation in the job, assuming that the employer never initiates a wage renegotiation. Furthermore, not all higher outside wage options are credible threats that lead to a renegotiation and wage increase because the previous employer may not be willing or able to re-hire the employee after the short time needed to realize the higher outside option. Therefore, the occasional bidding war serves to transfer portions of the match surplus from the employer to the employee over the course of a job (Cahuc et al., 2006; Postel-Vinay & Robin, 2002). If wages cannot be changed unilaterally, increases in outside options that are higher than non-employment transfers can have long-lasting effects on wages in retained jobs. In the Introduction, we describe the results of empirical articles that showed that higher negotiated wages are honoured in retained jobs, even when outside options exceeding non-employment transfers are no longer valid (Beaudry et al., 2012; Caldwell & Danieli, 2022; Caldwell & Harmon, 2019; Hafner, 2022; Staiger et al., 2010).

The mechanism described above works analogously for employers that initiate wage renegotiations and can also identify employer wage-setting power. After an outside option has disappeared or has been reduced, the employer can threaten to lay off the employee for a short time and renegotiate a new wage based on the lower next-best outside option currently available for the employee. This scenario implies that unemployment benefits are usually the outside option if employers regularly initiate a renegotiation. Employees do not have valid outside wage offers continually and employers can wait until the only valid outside offer is unemployment benefits (Bartolucci, 2012; Cahuc et al., 2008; Flinn & Mabili, 2009). For example, Postel-Vinay and Robin (2002), Flinn (2006) and Cahuc et al. (2006) show that there are large gaps between outside wage offers during

which employees are not protected against an employer that uses  $N$  as the best outside option in a renegotiation.

Our model predicts that even a long-term reduction in outside wage options,  $w_n > N$ , has no effect on wages in retained jobs if employers regularly renegotiate wages. Unemployment benefits are always the relevant outside option in wage renegotiations initiated by employers and the reduction in outside wage options is irrelevant for wages in retained jobs. However, our model also predicts that a reduction in outside wage options in a sub-group of employees can induce wage differences between employee groups. The employee group that is not affected by the reduction in outside wage options can ratchet up wages if the labour market environment improves in general, whereas the treatment group loses bargaining power and opportunities to initiate wage renegotiations until outside wage options reach the level before the reform. This argument implies that the reform effect is induced by successful renegotiations initiated by the control group. Therefore, the reform effect should be measurable only in an environment that allows general wage increases. The reform should not have a wage effect in a negative labour market environment in which the control group also cannot realize wage increases based on higher outside wage options.

Our model concentrates on the consequences of a reduction in outside wage options on wages in retained jobs. More elaborate wage bargaining models include additional wage determinants, such as the speed at which outside wage offers arrive, employee productivity or changes in unemployment benefits (Jäger et al., 2020). These alternative wage determinants are only relevant to our study if they differ between our treatment and comparison groups. Accordingly, we argue that changes in non-employment benefits and the speed of outside wage offers were similar for employees in regulated and deregulated occupations.

### 3 | HETEROGENEITY IN REACTIONS TO CHANGES IN OUTSIDE WAGE OPTIONS

The standard wage bargaining models *à la* Pissarides assume employer and employee heterogeneity with respect to productivity, whereas differences in wage renegotiation behaviour between employers are barely discussed in this literature (Cahuc et al., 2006). We propose arguments about why changes in the outside wage option of retained employees can cause different wage changes by employer type.

In Germany, industrial relations are characterized by a dual system of worker representation. Trade unions mainly negotiate collective wage agreements. Works councils give the interests of employees a voice and facilitate communication between management and the shopfloor. Works councils and unions prefer and promote equal pay for comparable jobs and compress wage differentiation (Addison et al., 2010; Card et al., 2004; Dell’Aringa & Pagani, 2007; Dustmann et al., 2009; Hirsch & Müller, 2020). Many collective bargaining agreements do not allow for wage differentiation between occupation groups, and instead, the agreements set a wage floor for each job level and determine uniform wage increases for each job level for all employees covered in an economic sector. Consequently, the employer should change wages for all employees at a certain job level uniformly. Many collective bargaining contracts even explicitly exclude differences in wage levels or wage increases between homogeneous employee sub-groups (Brenzel et al., 2014). Moreover, unions may also foster implicit contracts that shield all employees against wage reductions (Gürtzgen, 2009; Gürtzgen, 2014). The only way for an employer to undercut the wage floor set by collective bargaining is to use proper exit clauses from collective bargaining agreements and, for example, guarantee not to dismiss employees for a certain time-period in official deals with works



councils and unions. Unions and works councils must approve the design of wage reductions in these exit clauses. Consequently, unions and works councils would be unlikely to accept differing wage reductions initiated by employers for employee groups with different outside options, and we assume that firms with works councils and covered by collective bargaining allow less wage differentiation when individual employees try to renegotiate their wages (*Hypothesis 1*).

Firms with high profitability can be expected to avoid wage cuts induced by a reduction in outside options for some employees and to share their profits with their employees instead (Gürtzgen, 2009). Thus, employees with an increase in outside options may be more successful in increasing their individual wages during times in which their employers enjoy high profits (*Hypothesis 2*).

Employer size may also affect wage renegotiations: small employers are frequently exempted from labour regulation, or, when enforcement of the regulation is weak, small firms may not comply (Boeri & Jimeno, 2005). Small employers may use the opportunity to reduce the wages of selected employee groups when the outside options of these groups decline, even if wage reductions for selected employee groups are seen as critical by their workforce. Analogously, Guiso et al. (2005) noted that large employers have easier access to financial markets in downturns, and thus can mitigate against outside shocks better and provide insurance for their employees. When it comes to wage renegotiations initiated by employees, large employers have higher wage bargaining power because they can decline wage increases for employees who previously worked for them or work at another establishment in the same company (Jarosch et al., 2019). We should also consider that in Germany, larger employers are the first choice for high-ability labour market entrants and skilled workers. Smaller employers may be forced to accept higher wages selectively for their few high potentials to retain them. Finally, larger employers may also be less able to disentangle the individual contributions of their employees to corporate success than smaller employers (Lallemand & Rycx, 2006). Consequently, larger employers may be less willing to react to wage renegotiations initiated by a sub-group of their employees because the productivity and contribution of the sub-group to the employer's economic success is unknown to the employer. In summary, *Hypothesis 3* posits that smaller firms are better able and willing to differentiate among employees in wage renegotiations as a reaction to changes in their outside options (Du Caju et al., 2012; Guiso et al., 2005; Jäger et al., 2020).

The regional unemployment situation may also affect wage renegotiations. A higher unemployment incidence is a general indicator of lower outside options. An employee who demands a wage renegotiation based on an attractive outside option risks dismissal or may not receive a counter-offer from the current employer. Especially if employees mainly want to ratchet up the wage at the current employers instead of accepting the outside offer, they may be reluctant to initiate wage renegotiations in times of high unemployment (Beaudry & DiNardo, 1991). However, employers may use the fear of unemployment or low re-employment chances as a pretext to demanding wage concessions when the current employee wage is based on outside options higher than unemployment benefits. Thus, we assume that regional unemployment is negatively related to spill-over effects to wages in retained jobs when outside wage options increase (*Hypothesis 4*).

Germany has a labour market with strong downward wage rigidity implying that employers can hardly initiate wage renegotiations if the outside wage or non-employment options of their employees decline. In other words, the reform wage effect may mainly be induced by the higher wage bargaining power of employees in the control group instead of wage renegotiations of employers. Consequently, we expect reform effects mainly in establishments with a strong general wage increase. These employers can grant employees in the control group higher wage increases than employees in the treatment group because the employees in the control group are

in a stronger bargaining position. However, employers with low general wage increases abstain from wage differentiation between both groups because this would imply wage reductions for the treatment group (*Hypothesis 5*).

#### 4 | THE CRAFTS REFORM

Traditionally, all craftsmen working in the German crafts sector were regulated by occupational licensing. In the crafts sector, a business only could be established after acquiring a master craftsman certificate (*Meisterbrief*) and a registration of the business in the register of qualified craftsmen (*Handwerksrolle*). In 2000, the European Court ruled that Germany's crafts regulations were not compatible with the European single market. In response, Germany liberalized the entry regulations for establishing a business in selected crafts in 2004. After the reform, the certificate and registration were no longer prerequisites to establishing and operating a business in 53 of the 94 crafts. These occupations are our treatment group and they are listed in online Appendix B1 in the Crafts Code (Lergetporer et al., 2018; Rostam-Afschar, 2014). For the remaining 41 crafts (our control group, listed in online Appendix A in the Crafts Code), the master craftsman certificate remained the central requirement for establishing a business. The reform turned deregulated crafts from a 'licensed' to a 'certified' occupation because a master certificate still had a positive signal value for the productivity and quality of the business owner after the reform (Rottenberg, 1980).

The crafts reform was the result of a political compromise. As a consequence, until shortly before the reform bill was passed, it remained unclear which occupations would be deregulated and which would not. Politicians originally planned to keep hazardous crafts regulated and to deregulate crafts jobs with low risks for consumers. However, this strategy was diluted shortly before the reform. Consequently, many similar crafts occupations can be found in the regulated and deregulated groups. For example, tile, slab, and mosaic layer, and cast stone and terrazzo maker are deregulated, whereas bricklayer and concreter are regulated, and metal former, galvanizer, and metal and bell founder are deregulated, whereas metal worker is regulated. The reform bill was passed on 19 December 2003, and the reform took effect on 1 January 2004. Therefore, the crafts reform can be characterized as a quasi-natural experiment with a short announcement period. It accordingly had hardly any anticipation effects (Rostam-Afschar, 2014).

Many studies show that occupational licensing reduces the number of businesses and increases profits for owners and employees in licensed occupations (Gittleman et al., 2018; Kleiner, 2000; Kleiner & Soltas, 2023; Timmons & Thornton, 2019). Fixed entry costs to obtain the necessary qualifications to establish an enterprise reduce supply (Kleiner & Krueger, 2013). In line with these findings, the number of newly established businesses in the deregulated occupations and the number of self-employed increased much more strongly than those in the regulated occupations (Rostam-Afschar, 2014; Koch & Nielen, 2017; Lergetporer et al., 2018; Figure 1). Many of the new self-employed businesses operated as solo entrepreneurs, and the owners frequently had a migrant background from Central and Eastern Europe (Koch & Nielen, 2017). The business survival rate and their profitability declined in the deregulated crafts businesses (Fredriksen, 2020; Müller, 2016; 2018). The number of jobs decreased less in deregulated crafts than in regulated crafts during the 4 years after the reform (Lergetporer et al., 2018).<sup>4</sup> Finally, the wages of employees in the deregulated crafts occupations increased less dynamically after the reform, compared with wages in the regulated crafts (Damelang et al., 2018; Lergetporer et al., 2018). This finding is



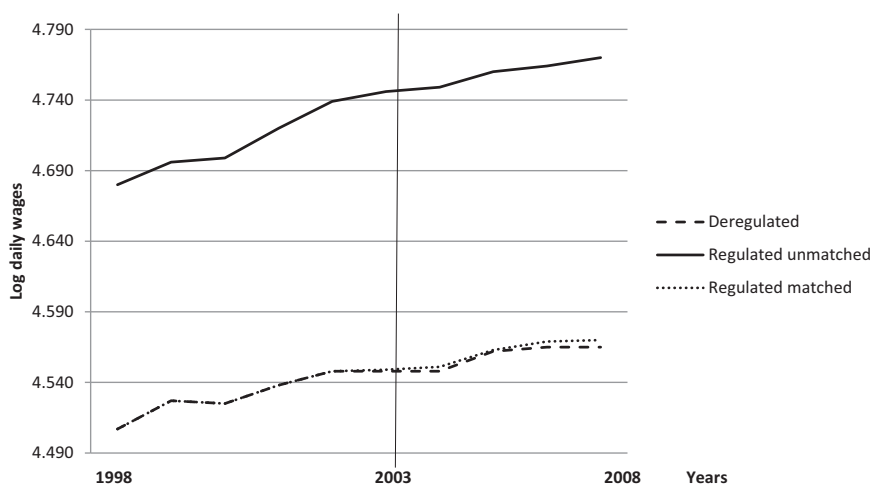


FIGURE 1 Log wages before and after the reform, 1999–2008. Notes: Average gross daily wages for employees in regulated and deregulated crafts included in our sample.

in accordance with literature that shows higher wages for licensed occupations (Kleiner & Soltas, 2023; Koumenta et al., 2022).

## 5 | IDENTIFICATION STRATEGY

The literature on the wage effects of the 2004 crafts reform uses the crafts reform as a quasi-experimental setting in difference-in-differences analyses (Damelang et al., 2018; Gathmann & Lembcke, 2020; Lergetporer et al., 2018; Rostam-Afschar, 2014). Most analyses include all employees in regulated and deregulated crafts occupations irrespective of whether employees stayed with their employers during the observation period. To capture the wage effects of the reform on retained jobs, we reduce our sample to observation periods in which employees did not change their employer and exclude all employees who switched occupations during the entire employment spell observed.

We want to ensure that the measured wage differences between the control and treatment group are induced by changes in outside wage options and not by other consequences of the crafts reform. Therefore, the sample is restricted to employees who worked for employers in the industry and commerce sector and excludes employees in the crafts sector. We show below that the main business activity of the employers in the industry and commerce sector is not affected by the crafts reform and that the share of employees who worked in crafts occupations is small in these employers. Therefore, third factors, such as differences in changes in productivity or bargaining power between treatment and control group, do not drive our results. Our article complements prior research on the impact of the 2004 crafts reform because previous articles have concentrated on the average wage effects of the crafts reform in the crafts sector or other businesses directly affected by the reform (Lergetporer et al., 2018).<sup>5</sup>

We use a peculiarity of the German crafts law (*Handwerksrecht*) to identify employers who were not affected by the crafts reform although they employed craftsmen. All businesses in Germany must be registered either with the regional Chamber of Crafts or with the regional Chamber of

Industry and Commerce. According to the crafts law, an enterprise must register with the Chamber of Crafts if the main product or service is an activity listed in online Appendices A or B1 of the crafts code (*Handwerksordnung*). In a first step, we reduce our sample to enterprises registered with the Chamber of Industry and Commerce. Thus, we know that these enterprises mainly pursued business activities unrelated to activities affected by the crafts reform.<sup>6</sup> A registration of an enterprise with the Chamber of Crafts, however, means that the main business activity of the enterprise perfectly overlaps with employees active in crafts in our treatment (online Appendix B1) and control group (online Appendix A).<sup>7</sup>

In a second step, we reduce the sample to employees who performed one of the occupations listed in online Appendices A and B1. We identify these employees based on their actual job activity as shown in their social security records. Most employees in crafts occupations with an apprenticeship or a master craftsman certificate work in adequate skilled jobs when they are employed in the industry and commerce sector. Only a small proportion work in unskilled or semi-skilled jobs unrelated to their education.

Craftsmen (i.e. employees who have completed a dual apprenticeship education [*Geselle*] or a master craftsman education [*Handwerksmeister*]) who work in their occupation in the industry or commerce sector are common in Germany because they received education certificates that enable them to work in all sectors of the German economy. More than 20 per cent of all master craftsmen and about 25 per cent of all skilled employees who have completed apprenticeship education in a crafts occupation prefer to work in the industry and commerce sector (Haverkamp & Gelzer, 2016).

We also include employees who worked in a crafts occupation, but who had not completed apprenticeship education or have an academic degree. Employees who did not complete an apprenticeship may have completed a professional education abroad that is not officially recognized in Germany, or they may just have had a short introduction to basic activities within the occupational domain and learnt the necessary skills on the job. Employees with an academic degree who work in a crafts occupation have usually completed an apprenticeship or a master craftsman certificate before their academic study.<sup>8</sup>

Jobs in the crafts sector only offer credible outside wage options if they are an attractive alternative. For about 10 per cent of our sample (2131 employees), we observe a move from an employer in the industry and commerce sector to another employer in a known economic sector. Only 36 movers (1.5 per cent) joined an employer in the crafts sector. The main reason for few switches from the industry and commerce sector to the crafts sector is that the average pay for the same kind of jobs is substantially higher in the industry and commerce sector than in the crafts sector. According to our model, the threat of a switch to an employer in the crafts sector may cause a wage increase in a retained job if the wage offer from a crafts employer is higher than the non-employment benefits.

## 6 | DATA

We use the Linked Employer-Employee Data Set of the IAB (LIAB9310, longitudinal version 2). The LIAB combines individual employment statistics from Social Security Records with establishment-level survey data from the IAB Establishment Panel. The distinctive feature of the LIAB is that besides establishment-level information, administrative information for the labour market histories of all individuals who work in the establishments is available. Our stratified data set includes all establishments with four consecutive entries in the IAB Establishment

Panel between 1999 and 2002. For these establishments, we know the complete employment histories of all employees between 1993 and 2010 (Jacobebbinghaus, 2008). Besides the education background, nationality,<sup>9</sup> age and gender of all employees, we know the current occupational activity, current daily gross wage, tenure and labour market seniority with daily accuracy. We also know for all employers whether they were registered at their local Chamber of Industry and Commerce or their Chamber of Crafts, median wages of all employees, a subjective profit assessment, revenues, the size and characteristics of the workforce, and industrial relations arrangements, such as the application of collective bargaining at the sector or firm level, or the presence of works councils. See Table 1 for descriptive statistics.

We only include full-time employees because the LIAB does not report working hours. We determine who works in the deregulated and regulated crafts occupations by matching the names of the occupations in the Annexes of the Crafts Code to those in our list of occupational title codes (3-digit classification of occupations from the year 1988; *Klassifikation der Berufe*, KldB88). We can precisely identify 39 of the 41 regulated occupations and 49 of the 51 deregulated occupations (see online Tables A1 and A2).

Occupations, education levels and occupational status are reported by employers, although because this information is not relevant for the social benefit calculations, it carries a higher risk of misreporting than the wages, tenure and employment type information (see, e.g., Fitzenberger et al., 2006). As ‘atypical’ employees in crafts occupations, jobs may have a higher risk of misreporting than ‘typical’ employees; thus, we reduce our sample to skilled employees in crafts occupations with completed apprenticeship training and a master craftsman degree in a robustness check.

We analyse the individual wage development in retained jobs between 1999 and 2008. We do not extend our observation period beyond 2008 because Germany was hit hard by the Great Recession in 2009. Large export-oriented manufacturing establishments had to reduce hiring, scrap bonus payments, reduce training and send their employees into short-term work. The possibility cannot be excluded that the recession affected employees in the deregulated occupations more strongly than employees in the regulated occupations (Lergetporer et al., 2018). Therefore, wage changes after 2008 may not be exclusively attributable to the crafts reform in 2004.

## 7 | EMPIRICAL ANALYSIS

Our identification strategy rests on the assumption that the exogenous reduction in the outside wage options of employees in deregulated crafts occupations compared with those of employees in regulated crafts occupations drives the wage differences after the reform. Accordingly, other wage determinants must be unchanged before and after the crafts reform or they must change in tandem for employees working in the treatment and control group. Thus, we eliminate observable differences between both employee groups before the reform (Lergetporer et al., 2018; Runst & Thomä, 2020). To ensure that the common trends assumption is met, we use entropy balancing (Hainmueller, 2012). This approach is a non-parametric data pre-processing method developed for binary treatment studies, so that the means and higher moments of an extensive list of observable relevant covariates are exactly balanced between the treatment and control groups. It avoids the challenge in propensity score models of locally and globally balancing the propensity score distributions of the control and treatment groups. In addition, we demonstrate that the business activities of the industry and commerce businesses in our sample are not affected by the share of employees in crafts occupations. Finally, we show that employment and unemployment levels

TABLE 1 Covariate balancing before matching, values for 1999–2003.

Variables	Deregulated occupation		Regulated occupation		Mean difference	
	Mean	Std. Err.	Mean	Std. Err.	Diff.	Sig.
	(1)	(2)	(3)	(4)	(5)	(6)
Log daily gross wages 1999a	4.507	0.001	4.680	0.001	−0.173	***
Log daily gross wages 2000	4.527	0.001	4.696	0.001	−0.169	***
Log daily gross wages 2001	4.525	0.001	4.699	0.002	−0.175	***
Log daily gross wages 2002	4.538	0.001	4.720	0.001	−0.181	***
Log daily gross wages 2003	4.548	0.001	4.739	0.001	−0.191	***
Female	0.094	0.001	0.048	0.001	0.046	***
German nationality	0.885	0.001	0.930	0.001	−0.045	***
Age: 16–35 years	0.180	0.002	0.168	0.001	0.012	***
Age: 36–45 years	0.398	0.002	0.380	0.001	0.018	***
Age: 46–55 years	0.333	0.002	0.357	0.001	−0.023	***
Age: 56–65 years	0.089	0.001	0.095	0.001	−0.006	***
Tenure: until 7 years	0.091	0.001	0.100	0.001	−0.009	***
Tenure: 8–14 years	0.440	0.002	0.379	0.001	0.061	***
Tenure: 15 years and more	0.403	0.002	0.449	0.001	−0.046	***
Experience: until 7 years	0.122	0.001	0.129	0.001	−0.007	***
Experience: 8–14 years	0.485	0.002	0.428	0.001	0.057	***
Experience: 15 years and more	0.393	0.002	0.443	0.001	−0.049	***
Education: no finished schooling	0.182	0.002	0.063	0.001	0.120	***
Education: secondary education	0.772	0.002	0.812	0.001	−0.039	***
Education: tertiary education	0.022	0.001	0.105	0.001	−0.083	***
Professional status: unskilled	0.357	0.002	0.164	0.001	0.193	***
Professional status: skilledb	0.568	0.002	0.424	0.001	0.144	***
Professional status: master craftsman	0.019	0.001	0.034	0.001	−0.015	***
Professional status: academic	0.054	0.001	0.377	0.001	−0.322	***
Economic sector: raw materials and manufacturing	0.430	0.002	0.288	0.001	0.142	***
Economic sector: mechanical engineering	0.546	0.002	0.581	0.001	−0.035	***
Economic sector: construction, trade and services	0.024	0.001	0.130	0.001	−0.106	***

(Continues)

TABLE 1 (Continued)

Variables	Deregulated occupation		Regulated occupation		Mean difference	
	Mean	Std. Err.	Mean	Std. Err.	Diff.	Sig.
	(1)	(2)	(3)	(4)	(5)	(6)
Employer size smaller than 250 employees	0.231	0.002	0.147	0.001	0.084	**
Employer size 250–1000	0.376	0.002	0.283	0.001	0.093	***
Employer size larger than 1000 employees	0.392	0.002	0.570	0.001	−0.178	***
Average wages level at employer	94.283	0.101	103.52	0.059	9.244	***
Average employee age at employer	41.642	0.012	41.54	0.009	0.099	***
Collective wage bargaining (sector level)	0.725	0.002	0.792	0.001	−0.067	***
Collective wage bargaining (employer level)	0.092	0.001	0.124	0.001	−0.032	***
Works council	0.906	0.001	0.942	0.001	−0.036	***
Profit situation good	0.340	0.001	0.369	0.001	0.029	***
Profit situation acceptable	0.434	0.002	0.405	0.001	0.029	***
Profit situation bad	0.195	0.002	0.159	0.001	0.036	***
Regional unemployment	12.056	0.021	11.487	0.013	0.569	***
Wages change 1999–2003	0.100	0.001	0.118	0.000	0.018	***
Wages change 2000–2003	0.065	0.000	0.088	0.000	0.022	***
Wages change 2001–2003	0.048	0.000	0.065	0.000	0.017	***
Wages change 2002–2003	0.021	0.000	0.030	0.000	0.009	***
Number of individuals	6532		15,461			

Notes: Significance levels: \*\*\* < 1% and \*\* < 5%. Data source: LIAB9310, own calculations.

<sup>a</sup>Nominal wages. Less than 3% of wage observations are top-coded at the social benefits threshold. These wage observations are imputed using the do-file `09_wages_imputation.do` as described, for example, in Dauth and Eppelsheimer (2020). As the share of top-coded observations is similar in our treatment and control group and the share of top-coded observations is small, the imputation does not change the results.

<sup>b</sup>Skilled employees have a completed apprenticeship training.

for employees in regulated and deregulated occupations moved in tandem after the reform. This evidence indicates that the bargaining power of the treatment and control groups was not affected by drivers other than the crafts reform.

In a first step, we use a difference-in-differences approach that compares the wage development of employees in regulated and deregulated occupations before and after the crafts reform of

$$y_{it} = \alpha_1 + \alpha_2 \text{deregulated} \times \text{post reform} + \mu_i + \mu_t + \varepsilon_{it} \quad (3)$$

with  $y$  log daily wages for worker  $i$  in year  $t$ . The variable *deregulated* is an indicator variable that is 1 if the employee works in one of the deregulated crafts occupations, and 0 if the

employee works in one of the regulated crafts occupations. The variable *post reform* is 1 if year  $t$  is in the period 2004–2008 and 0 in the period 1999–2003. Our main coefficient of interest,  $\alpha_2$ , gives us the average reform effect on wages for employees who work in a deregulated crafts occupation compared with employees who work in a regulated occupation. We include individual fixed effects  $\mu_i$  to partial out unobserved individual heterogeneity, time fixed effects  $\mu_t$  that take out common time-specific effects,<sup>10</sup> and we use standard errors clustered at the occupation level.<sup>11</sup>

Entropy balancing<sup>12</sup> incorporates balancing in the first and second moments of the covariate distribution directly into a maximum entropy reweighting function for all covariates included.<sup>13</sup> The method constructs weights  $w(i,j)$ , where  $i$  and  $j$  indicate individuals in the treatment and comparison groups, respectively, for each observation in the comparison group so that pre-specified balancing constraints are fulfilled precisely. In our case, the means and variances of all balancing variables are identical after matching. We use an extensive list of individual, employer and regional labour market characteristics, including individual wages to be balanced for the period 1999–2003.<sup>14</sup> Equation (4) shows the estimator for the treatment effect. Here,  $n_1$  is the number of treated individuals, and group membership is indicated by  $I_1$  (treated) and  $I_0$  (comparison), respectively. The counterfactual comparison group is a weighted average of the change in outcome variables with weights  $w(i,j)$  of

$$\tilde{\alpha}_2 = (1/n_1) \sum_I \varepsilon I_1 \left[ (Y_{1i}^{\text{after}} - Y_{1i}^{\text{before}}) - \sum_j \varepsilon I_0 w(i,j) (Y_{0j}^{\text{after}} - Y_{0j}^{\text{before}}) \right]. \quad (4)$$

The estimator is implemented in the difference-in-differences regression in Equation (3).

The interpretation of coefficient  $\alpha_2$  as the causal reform effect on wages relies on three main assumptions. First, the common trends assumption means that trends in the outcome variable would have been parallel without the treatment. In our case, a good indicator of this untestable assumption is that the wage changes of both groups before the crafts reform are the same. Without matching, employees in the deregulated crafts occupations had significantly lower schooling, professional status and wages, and they also worked in smaller establishments that paid less for their average employee. These and further differences between the treatment and control groups may have affected wage increases after the reform (Lergetporer et al., 2018; Müller, 2018). Entropy matching ensures that all wage determinants in both groups before the crafts reform are the same. Second, we assume that there is no effect of the reform on the pre-treatment population. In other words, employers and employees working in crafts occupations did not change their behaviour in anticipation of the crafts reform. We can exclude a reform anticipation effect because the occupations affected by the reform were announced at short notice and the reform was implemented directly after its announcement. In addition, we only include employees who completed their education and training at least 1 year before the reform and did not change their occupation and employer during the entire observation period. Third, we use the stable unit treatment value assumption (SUTVA) that the outcome of the comparison group after the treatment would have been the same in worlds with and without the reform (Rubin, 1977). We only include employees who obtained their occupational degree in a regulated occupation before the reform and who did not change their occupation during the observation period. In addition, we only include employees who worked for employers unaffected by the crafts reform and worked for the same employer during the entire observation period. Therefore, it seems improbable that the reform affected the employees in regulated crafts occupations and that there was self-selection into occupations related to the potential reform effect. Therefore, the SUTVA should be met.



To analyse our hypotheses, we show possible heterogeneities in the reform effect for the following groups of employers: employers with and without works councils or collective bargaining (at the sector level and the employer level); three employer size groups; three profit levels; and in the regional-unemployment-level quartiles. Hypothesis 5 posits that we test whether there is a correlation between general wage increases and the reform effect for employers with employees from the treatment and control group, respectively. Lastly, we check whether general wage level increases at the employer level drive the reform wage effect by differentiating the employers by their wage level increase.

Finally, we analyse the robustness of our results for sub-samples of employers. First, we only include employers with employees from the treatment and control group. This robustness check reveals whether the reform effects obtained in the full sample are mainly driven by differences in wage changes at employers with only treatment or control group employees. Second, we replicate the analysis for just one large employer. If we also find a reform wage effect for individual employers, this indicates that employers are willing to allow wage differences between comparable groups of employees within their workforce as a reaction to changes in the outside wage options of these groups.

Additional robustness tests check whether our results are obtained for real wage changes instead of nominal wage changes. We also replace individual fixed effects by establishment fixed effects; increase our sample to all employees present in 2003 at the employer instead of all employees present over the entire period between 1999 and 2003; reduce our sample to employees without atypical crafts employees (employees without an occupational or an academic degree); and reduce our sample to employers with a share of employees in regulated and/or deregulated crafts occupations larger than 20 per cent. Finally, we show that placebo treatments before and after the actual reform do not have measurable wage effects.

## 8 | FINDINGS

Craftsmen who work for German industry and commerce firms are mainly active in metalworking and electrotechnical occupations. The occupations with the highest proportions in all deregulated occupations are cutting tool mechanic (32 per cent), fitter (13 per cent) and glass finisher/precision optician (8 per cent), and all other occupations have shares below 5 per cent. The most common regulated occupations are electrician (27 per cent), electrical engineering technician (10 per cent) and motor vehicle maintenance technician (9 per cent), and all other regulated crafts occupations have shares below 5 per cent.

Analogous to previous literature on the crafts reform, we find that employees working in deregulated crafts occupations earned less and they were less educated, more frequently female, with a migration background and younger than those employees who worked in regulated crafts occupations (Table 1). In addition to the previous evidence, we find that employees in deregulated crafts occupations worked for smaller employers that paid less on average and that were more profitable. Thus, the employers of employees in deregulated crafts occupations less frequently had a works council or participated in collective bargaining.

Entropy balancing works well and perfectly aligns the means of all variables in the treatment and control group in the period 1999–2003. Table 1 shows employer and employee characteristics before entropy matching, and the values of the matched variables are identical. Consistent with previous literature on the crafts reform, column 5 in Table 1 indicates that employees in the deregulated occupations had less dynamic wage development in the years before the reform

TABLE 2 Treatment effects.

Log daily wages	Non-matched (1)	Matched (2)
Deregulated X After reform	-0.018*** (0.002)	-0.005* (0.003)
Year 2000	0.029*** (0.002)	0.034*** (0.002)
Year 2001	0.050*** (0.001)	0.049*** (0.001)
Year 2002	0.082*** (0.001)	0.077*** (0.003)
Year 2003	0.109*** (0.002)	0.097*** (0.002)
Year 2004	0.131*** (0.001)	0.116*** (0.002)
Year 2005	0.146*** (0.001)	0.127*** (0.002)
Year 2006	0.169*** (0.001)	0.152*** (0.002)
Year 2007	0.203*** (0.002)	0.190*** (0.003)
Year 2008	0.230*** (0.001)	0.216*** (0.002)
Constant	4.542*** (0.001)	4.419*** (0.002)
Adj. $R^2$	0.90	0.88
Observations	187,801	187,801
Individuals	21,993	21,993

Note: Nominal wages, individual fixed effects, standard errors clustered at occupation level in parentheses. Significance levels: \*\*\* < 1% and \* < 10%. Data source: LIAB9310, own calculations.

(see last four rows). We include the wage level in each year between 1999 and 2003, and the wage levels and their increases are identical in the matched sample. Figure 1 shows the development of matched and unmatched wages in regulated and deregulated crafts occupations by year. Following the reform, wages of the employees in deregulated crafts occupations increased slightly less than wages of employees in regulated crafts after matching.

If we calculate the treatment effect of the crafts reform on wages between 2004 and 2008 in a difference-in-differences regression according to Equation (3), we obtain significantly lower wage increases for employees in deregulated crafts occupations. The wage difference amounts to 1.8 per cent in the 5-year period (refer to column 1 in Table 2). After entropy matching, the treatment effect declines to 0.5 per cent but it remains significant at the 5 per cent level. Average wages increase in the period between 2004 and 2008 by 9.7 per cent. Therefore, the difference in wage increases translates into a relative wage disadvantage of employees in deregulated occupations of about 5 per cent. Table 3 shows that the treatment effects differ from year to year. The reform

TABLE 3 Yearly treatment effects.

	Log daily wages Matched
Deregulated * Year 1999	0.001 (0.003)
Deregulated * Year 2000	0.000 (0.002)
Deregulated * Year 2001	0.001 (0.002)
Deregulated * Year 2002	0.002 (0.003)
Baseline Year 2003	
Deregulated * Year 2004	-0.004 (0.002)
Deregulated * Year 2005	-0.005** (0.002)
Deregulated * Year 2006	-0.001 (0.003)
Deregulated * Year 2007	-0.004** (0.002)
Deregulated * Year 2008	-0.011*** (0.004)
Adj. $R^2$	0.89
Observations	187,801
Individuals	21,993

Note: Significance levels: \*\*\* < 1% and \*\* < 5%. Data source: LIAB9310, own calculations.

TABLE 4 Treatment effects with establishment fixed effects, larger sample with all employees working in 2003 and log daily real wages.

	Log daily wages (matched)	Log daily real wages (matched)	
	Establishment fixed effects (1)	All employees in 2003 (2)	Individual fixed effects (3)
Deregulated X After reform	-0.005** (0.003)	-0.009*** (0.002)	-0.005** (0.003)
Adj. $R^2$	0.70	0.68	0.88
Observations	187,801	312,826	187,801
Individuals	21,993	41,697	21,993

Note: Nominal wages in columns 1 and 2, entropy balancing is rerun on each sub-sample, establishment fixed effects in first column and individual fixed effects in second and third columns, year dummies and constant included, real wages obtained by deflating wages by the inflation rate reported by the Federal Statistical Office, standard errors clustered at establishment level in parentheses (first column) and occupation level (second and third columns). Significance levels: \*\*\* < 1% and \*\* < 5%. Data source: LIAB9310, own calculations.

has a significantly negative wage effect in 2005, 2007 and 2008.<sup>15</sup> As expected, there are no wage differences before the crafts reform in our matched sample.

Our results are robust to several changes in the econometric design. If we replace individual fixed effects with establishment fixed effects, we obtain the same treatment effect (Table 4, column 1). Our preferred matching approach demands that all employees worked for the same firm since 1999. If we relax this condition and only require that an employee was employed in the firm since 2003, the sample size roughly doubles to more than 41,000 individuals (Table 4, column 2). The treatment effect is higher (0.9 per cent) in the larger sample.<sup>16</sup> The treatment effect is also unchanged if we replace nominal wage changes with real wage changes (Table 4, column 3).

TABLE 5 Treatment effects by employer size.

	Employer size < 250	Employer size 250–999	Employer size > 999
Log daily wages (matched)	(1)	(2)	(3)
Deregulated X After reform	0.002 (0.006)	−0.006* (0.003)	−0.006*** (0.002)
Adj. $R^2$	0.87	0.89	0.70
Observations	32,295	58,3508	97,165
Individuals	3915	6777	11,301

Note: Significance levels: \*\*\* < 1% and \* < 10%. Data source: LIAB9310, own calculations.

TABLE 6 Treatment effects by collective bargaining status.

	Collective wage agreement, sector level	Collective wage agreement, company level	No collective wage agreement
Log daily wages (matched)	(1)	(2)	(3)
Deregulated X After reform	−0.004** (0.002)	−0.010* (0.006)	−0.003 (0.003)
Adj. $R^2$	0.86	0.88	0.91
Observations	145,023	21,506	18,347
Individuals	13,171	5057	3765

Note: Significance levels: \*\* < 5% and \* < 10%. Data source: LIAB9310, own calculations.

The reform wage effect may be biased if the characteristics of those employees who move to another employer after the reform differ between treatment and control group. Lower outside options may make an employer move less attractive for highly productive members of the treatment group. However, the share of employer movers is stable for all observation years and it is comparable for the treatment and control groups. Consequently, we do not observe a reform effect on the propensity to change employer, and hardly any short-term employer switches as predicted by our theoretical model. The average share of job-to-job movers is 0.4 per cent (2131 job-to-job movers out of 498,213 employees who stay with their employers over the years) and is so small that including employer movers in our analysis after finishing their job at the focal employer does not affect the measured reform effect.<sup>17</sup>

Next, we check our hypotheses on differences in the treatment effects by employer groups.<sup>18</sup> The treatment effect increases with employer size; there is no treatment effect for employers with fewer than 250 employees. For employers with more than 999 employees, the effect is −0.6 per cent (Table 5). This finding is inconsistent with Hypothesis 2. Also contrary to Hypothesis 1, works councils and collective wage bargaining do not restrict wage differentiation between treatment and control groups. We only find a treatment effect for employers with works councils and for employers with collective wage bargaining (compare Tables 6 and 7). The treatment effect is larger for employers with collective wage bargaining at the company level than for employers with collective wage bargaining at the sector level. Contrary to Hypothesis 3, we find no correlation between treatment effects and profit situation (Table 8), and contrary to Hypothesis 4, there also is no clear relationship between the regional unemployment level and the treatment effect size (Table 9). However, according to Hypothesis 5, the reform effect increases with

TABLE 7 Treatment effects by works council status.

Log daily real wages (matched)	Works council	
	(1)	(2)
Deregulated X After reform	-0.006*** (0.002)	0.002 (0.008)
Adj. R <sup>2</sup>	0.86	0.94
Observations	174,939	12,862
Individuals	18,371	3622

Note: Significance level: \*\*\* < 1%. Data source: LIAB9310, own calculations.

TABLE 8 Treatment effects by profit situation.

Log daily wages (matched)	Profit situation		
	Profit situation good <sup>a</sup> (1)	Profit situation acceptable (2)	Profit situation bad (3)
Deregulated X After reform	-0.006*** (0.002)	-0.002 (0.004)	-0.007* (0.004)
Adj. R <sup>2</sup>	0.92	0.91	0.83
Observations	67,593	77,725	31,853
Individuals	15,664	18,955	9837

Notes: Significance levels: \*\*\* < 1% and \* < 10%. Data source: LIAB9310, own calculations.

<sup>a</sup>The profit situation is assessed based on the following question: 'Please give your assessment of the profit situation of your business in the last fiscal year. Profitability was very good, good, satisfactory, sufficient, unsatisfactory'. We combined 'very good' and 'good' to 'good' and 'satisfactory' and 'sufficient' to 'acceptable'.

TABLE 9 Treatment effects by regional unemployment.

Log daily wages (matched)	Regional unemployment			
	First quartile regional unemployment (1)	Second quartile regional unemployment (2)	Third quartile regional unemployment (3)	Fourth quartile regional unemployment (4)
Deregulated X After reform	-0.005 (0.005)	-0.005* (0.003)	-0.009** (0.004)	-0.003 (0.006)
Adj. R <sup>2</sup>	0.79	0.80	0.93	0.86
Observations	44,580	44,977	50,559	50,078
Individuals	8393	10,773	10,446	9,274

Note: Significance levels: \*\* < 5% and \* < 10%. Data source: LIAB9310, own calculations.

general wage increases. If we sort all employers by their wage increase for the median worker and divide employers by wage increase deciles,<sup>19</sup> we only find treatment effects for employers with wage increases higher than the sixth decile (Table 10).

A problem with sample-splitting is that several mechanisms may be present in one group. In particular, the negative reform effects at establishments with works councils, collective wage bargaining and large establishments that are not consistent with our hypotheses may be driven by a neglected third factor in the sample split. According to Hypothesis 5, this third factor may be a general higher wage increase at large employers with works councils and

TABLE 10 Treatment effects by establishment median wage increases after reform.

<b>Log daily wages (matched)</b>				
<b>Establishment median wage increase</b>				
	<b>Deregulated X</b>			
	<b>After Reform</b>	<b>Adj. R2</b>	<b>Observations</b>	<b>Individuals</b>
First decile	0.004 (0.004)	0.92	12,536	1470
Second decile	-0.001 (0.003)	0.80	14,762	1539
Third decile	-0.001 (0.006)	0.68	11,301	1283
Fourth decile	0.001 (0.006)	0.90	16,294	11,617
Fifth decile	0.001 (0.006)	0.91	11,617	1208
Sixth decile	-0.001 (0.006)	0.87	17,894	1935
Seventh decile	-0.004** (0.002)	0.89	15,842	1635
Eighth decile	-0.009*** (0.003)	0.85	17,394	1494
Ninth decile	-0.017*** (0.007)	0.82	16,783	1498
10th decile	-0.011** (0.006)	0.70	13,138	1307

Note: Significance levels: \*\*\* < 1% and \*\* < 5%. Data source: LIAB9310, own calculations.

collective bargaining. This assumption is supported by online Table A3, which shows average median wage increases between 2004 and 2008 at the employer level for the employer groups presented in Tables 4–10. All employer groups with significantly negative reform effects (large employers, employers with works councils and with collective wage bargaining, especially at the company level) have higher-than-average median wage increases.

To explore the hypothesis that several mechanisms may interact in split samples further, we include interaction terms between all employer groups with the deregulated occupations dummy and the post-reform dummy. We find that the interaction terms have the same direction as the differences between the groups in the split samples (Table 11, column 1), although the interaction terms for firm size, works councils and collective bargaining lose their significance. The negative reform wage effect for treated employees at employers with the highest general wage increases remains significant if we include the other interaction terms. To support the hypothesis that general wage increases are the main driver of the reform effect, we include the interaction term between the linear average log wage increase at the employer level and the deregulation dummy as an explanatory variable. We obtain a positive significant impact for general wage increases on the reform wage effect. The coefficient of the wage increase measured in Table 11 (column 2) suggests that an increase in average wages at the employer level by 1 per cent increases the reform effect for employees by about 1 per cent.



TABLE 11 Treatment effects with interaction terms.

	Log daily wages (matched)			
	(1)		(2)	
Deregulated X After reform	-0.005***	(0.001)	-0.005**	(0.002)
Deregulated X After reform X Employer size < 250	0.003	(0.002)	0.002	(0.002)
Deregulated X After reform X Employer size 250–999	0.002	(0.002)	0.001	(0.002)
Deregulated X After reform X Collective wage bargaining on sector level	0.002	(0.004)	0.002	(0.003)
Deregulated X After reform X Collective wage bargaining on company level	-0.001	(0.003)	-0.001	(0.003)
Deregulated X After reform X Works council	-0.003	(0.002)	-0.006	(0.005)
Deregulated X After reform X Profit situation good	0.000	(0.001)	0.001	(0.001)
Deregulated X After reform X Profit situation acceptable	0.004***	(0.002)	0.009***	(0.002)
Deregulated X After reform X first quartile regional unemployment	-0.001	(0.002)	-0.001	(0.002)
Deregulated X After reform X second quartile regional unemployment	-0.002	(0.002)	-0.004	(0.003)
Deregulated X After reform X third quartile regional unemployment	-0.010	(0.006)	-0.011	(0.007)
Deregulated X After reform X second decile establishment median wage increase	-0.002	(0.002)		
Deregulated X After reform X third decile wage increase	0.000	(0.002)		
Deregulated X After reform X fourth decile wage increase	-0.001	(0.002)		
Deregulated X After reform X fifth decile wage increase	-0.002	(0.002)		
Deregulated X After reform X sixth decile wage increase	-0.002	(0.004)		
Deregulated X After reform X seventh decile wage increase	-0.001	(0.002)		
Deregulated X After reform X eighth decile wage increase	-0.004**	(0.002)		
Deregulated X After reform X ninth decile wage increase	-0.006***	(0.001)		
Deregulated X After reform X 10th decile wage increase	-0.005***	(0.002)		
Deregulated X Yearly log wage increase			-0.995***	(0.008)
Raw materials and manufacturing	-0.011*	(0.006)	-0.009*	(0.006)
Mechanical engineering	-0.000	(0.007)	-0.002	(0.007)
Adj. $R^2$	0.88		0.85	
Observations	187,801		187,801	
Individuals	21,993		21,993	

Note: Nominal wages, reference groups for difference in differences effects: employer size > 999, profit situation bad, highest quartile regional unemployment, construction, trade and services; individual fixed effects, year dummies and constant included, standard errors clustered at occupational level in parentheses. Significance levels: \*\*\* < 1%, \*\* < 5% and \* < 10%. Data source: LIAB9310, own calculations.

## 9 | ROBUSTNESS TESTS

In our first robustness check, we make sure that our results are not driven by outliers. Thus, we reduce the sample to those employees with a completed crafts apprenticeship education or a master craftsman certificate. The treatment effect for the reduced sample is comparable to the average effect for the full sample (online Table A4, column 1). In a second robustness check, we eliminate all employers with less than 20 per cent of their employees in deregulated and/or regulated crafts occupations. Thus, we check whether employers who potentially face the strongest exposure to employees in the treatment and control group drive the results. This group may be vulnerable to the general effects by the craft reform. Again, the treatment effect is comparable to that in the full sample (online Table A4, column 2). We conclude that the reform effect does not seem to be concentrated at the establishments with a large share of employees in crafts occupations.

Our identification strategy compares all employees in deregulated crafts jobs with all employees in regulated craft jobs. Hence, our matching procedure compares the wage development of groups of employees who potentially work for different employers. If differences in wage development between treated and untreated employee groups also apply to employees within the same employer, this finding indicates wage differentiation at the employer level. We check whether the reform effect can also be detected on the employer level and that the effect is not driven, for example, by differences in the wage dynamics of firms with only treatment or control group employees. First, we only include employers with at least one regulated and at least one deregulated crafts occupation employee. We find a reform effect of 0.3 per cent for this sub-sample of firms (online Table A4, column 3). The robustness of the result is unsurprising given that about 90 per cent of the employees in our main sample work for an employer who has employees from the treatment and control groups.

The next robustness test goes a step further and checks whether the reform effect can be measured even within a single employer. A reform effect can only be observed when the wage development of a sufficient number of employees can be compared; thus, we take the employer with the largest number of employees in crafts occupations in our sample as a case study. This employer is active in mechanical engineering, has a works council and participates in collective bargaining at the sector level. The employer has about 21,000 employees and a share of 2 per cent of employees in deregulated crafts and of 15 per cent of employees in regulated crafts. Most importantly, the employer has an increase in the median wage in the workforce of 11 per cent between 2004 and 2008, so experiences more dynamic wage development than the average employer in our sample. We find a significant wage effect of 0.2 per cent at this large employer (online Table A4, column 4).

Our last robustness tests check whether a placebo reform in 2001 for the period before the reform and a placebo reform in 2006 for the period after the reform show no treatment effects. As expected, both placebo reforms do not lead to measurable 'effects' (online Table A5).

Next, we want to ensure that the reform effects presented are mainly driven by differences in the development of outside wage options between regulated and deregulated crafts occupations after the crafts reform, rather than by other drivers of wages related to our two occupation groups. We first analyse whether employment and unemployment levels develop differently for regulated and deregulated occupations between 1999 and 2008. Differences in employment chances or unemployment risks could also influence the bargaining power of the treatment and control groups. There are no data on occupation-specific employment and unemployment shares for all crafts occupations included in our sample. The internet resource '*Berufe im Spiegel der*

*Statistik*' (occupations mirrored by statistics, <http://bisds.infosys.iab.de/>) provides data on the employment and unemployment development for many treatment and control occupations. The occupations in the data set are identified according to the 3-digit classification of occupations from the year 1988. Therefore, we can obtain information about employment and unemployment levels for 25 out of 41 regulated craft occupations and 30 out of 51 deregulated craft occupations.<sup>20</sup> Employment decreases more strongly in regulated crafts occupations between 1999 and 2001 than employment in deregulated craft occupations (online Figure A1). For the following years, until 2008, employment development is similar in both occupation groups. Unemployment is higher in 1999 and 2000 for deregulated crafts occupations (online Figure A2<sup>21</sup>), the relative unemployment rates are roughly similar between 2001 and 2005, and for the period 2006–2008, unemployment decreases more for regulated crafts than for deregulated crafts. Therefore, the measured reform effect in the last 3 years of our observation window may partly be a consequence of the small relative increase in unemployment risk for employees in deregulated crafts. We find no correlation between the average regional unemployment rate and the average reform effect (Table 9). Therefore, we suppose that the effect of the more dynamic unemployment development in deregulated jobs after 2006 on wage development is small. This assumption is consistent with the finding that unemployment has little effect on wages (Hall & Milgrom, 2008).

Finally, we check whether the economic situation of the employers in our sample might be affected by the share of employees in regulated and deregulated occupations. We show that the median employee income, number of employees, and subjective profit situation assessment before and after the crafts reform are not related to the share of employees in regulated and deregulated crafts occupation jobs. None of the three economic situation indicators are correlated with the dummy 'after the reform' multiplied by the share of employees in the treatment and control groups (online Table A6). It is unsurprising that the shares of employees in deregulated and regulated crafts occupations are hardly correlated with the economic situation of employers in the industry and commerce sectors. The share of employees in crafts occupations is small: about 9 per cent of the employees work in deregulated crafts occupations and about 16 per cent work in regulated crafts occupations. Almost 90 per cent of the employers in our sample have less than 50 per cent of their workforce active in crafts occupations. The results of our robustness check in column 2 of online Table A4 also show that the reform effects are not driven by employers with high shares of employees in crafts occupations.

The 2004 crafts reform was part of a larger reform package aimed at reforming labour market institutions and the German Federal Employment Agency. However, it is unlikely that other parts of the reform implemented in 2004 had a different impact on regulated and deregulated occupations because the wage-setting process was not affected by these reforms (Dustmann et al., 2014).

## 10 | DISCUSSION AND CONCLUSIONS

Our analysis provides empirical evidence that an exogenous reduction of outside wage options for employees working in deregulated crafts occupations in the industry and commerce sector reduces wage increases in this group compared with wage increases for employees working in regulated occupations in this economic sector. We use a reform in Germany that can be characterized as a natural experiment in which outside wages were reduced for employees by a deregulation of their occupations. The reform effect amounts to a wage difference of 0.5 per cent in the 4 years after the reform between employees treated by the reform and a comparable group of employees in a control group. This difference corresponds to about 5 per cent of the average wage increase

during this period. To ensure that the changes in outside wage options drive the reform effects, we reduce the sample to employees who work for the same employer in the industry and commerce sector that was not directly affected by the deregulation in the crafts reform. Although few employees move from the industry and commerce sector to the crafts sector, wage offers in the crafts sector can influence wages in the industry and commerce sector. This result is consistent with findings by Caldwell and Harmon (2019) that outside wage options have a stronger effect on wage changes than actual employer changes.

We find evidence that wage differentiation induced by the reform is mainly present at employers with high general wage increases after the reform. In addition, contrary to our assumptions, employers with works councils and collective bargaining, and large employers have the largest reform wage effect. In other words, works councils and unions do not seem to prevent wage differentiation. In the rigid German wage bargaining system, employers are allowed to offer voluntarily higher compensation than that specified by collective agreements. Legal ways to increase wages above collective agreements include regrouping of selected employees into higher job categories, allowances for atypical or heavy work, vacancy pay, performance pay and profit sharing. Therefore, effective wages for selected employee groups often exceed collective bargaining wages, and bonus payments for sub-groups of the workforce are accepted by works councils (Addison et al., 2010; Dustmann et al., 2009). Employers with works councils or collective wage agreements can use the flexibility in wage renegotiations initiated by employees with improved outside wage options to retain them. The positive impact of works councils and unions disappears if we control for the general wage increase at the employer level. Thus, we argue that works councils and unions do not actually drive wage differentiation, but they tolerate it if selected employee groups renegotiate their wages. An additional indicator that the higher bargaining option of the control group mainly drives our results instead of the lower bargaining position of the treatment group is that the regional unemployment rate does not affect wage differentiation. Therefore, employers do not seem to use a higher unemployment risk to renegotiate wages. General wage increases seem to offer employers the opportunity to differentiate between comparable employee groups in wage bargaining initiated by employees.

Previous articles on the impact of changes in outside wage options on wages of retained employees could not control for employer characteristics (Caldwell & Harmon, 2019; Caldwell & Danieli, 2022; Hafner, 2022) or did not have control groups at the employer level (Staiger et al., 2010). Our work can identify wage differentiation on the employer level. First, we show that the reform wage effect can be measured in a sample that contains only employers with employees in both the treatment and control groups. Second, we demonstrate that the effect can be measured at one large employer in our sample. The result that employers use wage differentiation in their workforce complements a vast body of literature on wage differences among similar workers in Germany. The literature concentrates on wage differentiation among employee groups for the entire economy (Dustmann et al., 2009; Goldschmidt & Schmieder, 2017) or for employer wage premia (Card et al., 2013; Gürtzgen, 2009). However, these articles do not consider workers' outside options (Hirsch & Müller, 2020) and they do not analyse wage differentials within employers.

Several articles on wage differentiation include indicators about regional labour markets and commuting costs (Beaudry et al., 2012; Caldwell & Harmon, 2019; Hafner, 2022). Most products and services offered by employees in crafts occupations are only locally distributed. Hence, we may assume that there are differences in regional changes in competition for employers in the crafts sector, depending on the regional incidence of new business foundations. Unfortunately, our data do not include start-up activities; thus, it would be an interesting extension of the present analysis to examine potential heterogeneities in wage effects between entrepreneurs in both groups. In addition, it may be interesting to check whether a substantial

number of new employee entrepreneurs in the deregulated crafts come from the industry and commerce sector. A final open question that is beyond the scope of this article is whether there are differences in entry wage levels between groups induced by the reform.

We find evidence that employers accept wage differentiation between comparable employee groups as a reaction to a change in outside wage options. However, our indicators are indirect because we do not observe directly how employers negotiate wages with retained employees. Employers may change working hours or pay temporary bonus payments to counter the higher bargaining power of specific employee groups. Another interesting extension of our analysis would be a qualitative analysis of how employers renegotiate wages with individual employees who demand pay raises when they have better outside wage options.

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## DATA AVAILABILITY STATEMENT

The data set used in the publication (LIAB9310, longitudinal version) is publicly available and can be accessed via the web pages of the Research Data Centre of the German Federal Employment Agency at the Institute for Employment Research.

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## ENDNOTES

- <sup>1</sup>The effects of the German crafts reform mirror wage decreases measured after deregulation in specific industries in other countries.
- <sup>2</sup>This solution requires the following further assumptions: productivity is perfectly observable by every agent in the economy; the opportunity cost of recruiting a worker, namely job search intensity, is constant; and job searching costs are zero. All workers are homogenous and provide one efficiency unit per period, and consequently, a risk-neutral worker in a firm with productivity  $p$  produces  $p$ .
- <sup>3</sup>If  $p_n > p_o$ , the employee is poached and we do not observe a wage change in a retained job (Caldwell & Harmon, 2019).
- <sup>4</sup>Müller (2016, p. 14 fig. 4) normalizes employment levels just before the reform (2003) to 100. In both employee groups, the employment level was about 112 in 2000. Employment dropped somewhat quicker in the deregulated crafts than in the regulated crafts until the reform. Employment decreased further to 92 in the deregulated crafts and to 95 in the regulated crafts in 2005. Employment levels recovered to 100 in both groups in 2010.
- <sup>5</sup>Previous articles concentrate on employers affected by the crafts reform and only include small or craft firms (Koch & Nielen, 2017; Lergetporer et al., 2018; Runst et al., 2019).
- <sup>6</sup>We exclude the few firms that are registered in both chamber types from our sample.
- <sup>7</sup>There is a list of 57 activities classified as 'similar to crafts' (*handwerksähnlich*) that do not need a license or certificate. Enterprises mainly active in these crafts are listed in online Appendix B2 of the Crafts Code. We exclude employees working in these occupations because they usually require lower skills than those listed in online Appendices A and B1 (Rostam-Afschar, 2014).
- <sup>8</sup>We only observe the highest education and current occupational status and do not know all educational degrees.
- <sup>9</sup>The EU enlargements in 2004 and 2007 may have increased the supply of non-German applicants for jobs in crafts occupations (Rostam-Afschar, 2014; Koch & Nielen, 2017). We, therefore, include a nationality dummy.
- <sup>10</sup>It is not possible to include establishment fixed effects  $\mu_j$  in addition to individual fixed effects because we do not have employer movers in our data set. We include establishment fixed effects instead of individual fixed effects in a robustness check.

- <sup>11</sup> Wage differences induced by the crafts reform are measured on the occupation level (Abadie et al., 2023).
- <sup>12</sup> The STATA command for the procedure is `ebalance` (Hainmueller & Xu, 2013).
- <sup>13</sup> We use the default tolerance level of 0.015 for achieving convergence in the optimization.
- <sup>14</sup> Our balancing variables are as follows: gender, German nationality, age (five groups), tenure (three groups) experience (three groups), school education (three groups), occupational status (four groups), economic sector (three groups), average employee wage at employer, average employee age, collective bargaining status (three groups), works council, economic situation of employer (three groups), log (wage) (separately for 1999–2003) and regional unemployment level per year (compare the list of variables in Table 1).
- <sup>15</sup> Lergetporer et al. (2018) also find significant treatment effects only for selected years.
- <sup>16</sup> The treatment effect is also larger, with 2.4 per cent for the unmatched larger sample.
- <sup>17</sup> We include an employer move dummy in this regression. Results of this exercise are available on request.
- <sup>18</sup> We also calculate the reform effects without entropy matching in the sub-samples by employer groups. Although we generally obtain higher reform effects, the relative differences found for matched groups in the sub-samples are robust. Thus, we do not report the results for the unmatched samples here.
- <sup>19</sup> We take the average increase of the median wage per year on the employer level. We use the median because the measure is not influenced by relatively high wage changes for a small minority of employees. If an employer is not observed for the entire period of 2004–2008, the longest observation period is used. The following average yearly median wage increases on the employer level between the observation years for our sample by decile are 0.001, 0.008, 0.013, 0.018, 0.023, 0.029, 0.032, 0.035, 0.043 and 0.077.
- <sup>20</sup> The crafts occupations not covered are marked with an asterisk in online Tables A1 and A2.
- <sup>21</sup> The unemployment rate in 2003 for deregulated occupations was 19.5 per cent and for regulated occupations was 20.2 per cent.

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## SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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