DISCUSSION PAPER

// VERONIKA LUKESCH AND THOMAS ZWICK

Outside Options Drive Wage Inequalities in Continuing Jobs: Evidence From a Natural Experiment

Leibniz
Association

ZEW

Outside Options Drive Wage Inequalities in Continuing Jobs: Evidence from a Natural Experiment

Veronika Lukesch, University of Würzburg

Thomas Zwick, University of Würzburg, ROA and ZEW[1]

January 2021

Abstract

The literature on wage bargaining so far mainly argues that unemployment benefits are relevant outside options for employees. This paper demonstrates that also a change in outside wage options drives wages in continuing jobs. We use the natural experiment of a crafts reform that reduces outside wage options for a clearly defined treatment group of employees in deregulated crafts occupations in comparison to employees in crafts occupations that have not been reformed. Five years after the reform, the wages of employees in deregulated crafts increased by five per cent less than wages of employees in the other group. Reform effects are concentrated in employers with high increases in their median wage level after the reform. Wage differences therefore seem to be the result of wage renegotiations initiated by employees, rather than renegotiations initiated by employers. Works councils or collective bargaining, firm size, firm profits or regional unemployment have no impact on wage differentiation after taking wage increases into account. We show for the first time that changes in outside options induce wage differentiation at the employer level even in the tightly regulated German labour market. We use entropy matching on the basis of a large representative administrative linked employer-employee panel data set to guarantee homogeneous treatment and control groups before the reform. We isolate the outside wage option effect from other wage determinants by restricting our sample to employers not affected by the crafts reform.

JEL Codes: J24, J31, J44, L11

Key Words: wage bargaining, outside options, diff-in-diff estimation, matching

Introduction

Many papers explore wage determinants based on the classic Nash bargaining model (Mortensen and Pissarides 1999; Burdett and Mortensen, 1998; Pissarides 2000, chapter 1; Michelacci and Suarez, 2006; Cahuc et al., 2006; Jäger et al., 2020). These wage bargaining and wage posting models "give primacy to the bargainers' outside options", in comparison to bargaining power and productivity as wage determinants (Hall and Krueger 2010). In most of these models, non-employment value or unemployment benefits are posited as workers´ outside options (Pissarides, 2000; Shimer, 2006; Ljungqvist and Sargent, 2017; Lachowska, 2017). Some wage bargaining models however argue that outside wage options are the relevant outside options for employees in continuing jobs, rather than unemployment benefits (Manning, 2003; Postel-Vinay and Robin, 2002; Cahuc et al., 2006; Bagger et al., 2014; Caldwell and Danieli, 2018).[2]

The empirical evidence on the impact of the value of non-employment or unemployment on wages indeed is thin. One of the few papers that assess the impact of a change in unemployment benefits on wages, Jäger et al. (2020), shows no impact. The authors argue that wages of employees in continuing jobs may be influenced by outside wages options rather than the unemployment benefits. Only a handful of papers have examined the impact of changes in outside wage options on wages, however. Beaudry et al. (2012) show in instrumental variables regressions that wages in an economic sector-city cell in the US depend on the share of sectors with high wage premia in the city. Wages in one economic sector therefore spill over to other sectors in the same city. Caldwell and Harmon (2019) argue that increases in the labour demand at former co-workers´ current firms are an indicator of better outside options not related to productivity of the employee. They show that improved information on job opportunities at former employers increases wages in continuing jobs in Denmark. Caldwell and Danieli (2018) use the increase in the relevant labour market options for employees who live in small German towns near to newly-built high-speed commuter train stations and regional differences in industry growth trends as exogenous variation. They argue that the inclusion of small towns in the high-speed train network does not affect the product market of firms located in these areas and therefore does not affect the productivity of those who remain in their jobs. They find that the higher outside options induced by a reduction in commuting times and a strong growth in the local industry sector are translated into higher wages. Hafner (2020) examines the wage effects of a lift in cross-border labour market barriers between France and Switzerland. The

---

[2] In the so-called alternating-offer wage bargaining model proposed by Binmore et al. (1986), Shimer (2006), and Hall and Milgrom (2008), outside wage options, instead of non-employment benefits, are the relevant outside option. Outside options have only a small impact on wages however because threatening to unilaterally quit the wage bargain (even for a brief instant) and look for another bargain is not considered as a credible threat.

extension of the potential labour market increases wages for employees in continuing jobs in French border regions, in comparison to employees in French control regions further away from the border.[3]

The main challenge in assessing the impact of changes in outside wage options in continuing jobs is that employees´ outside options cannot usually be observed (Caldwell and Danieli, 2018). In addition, a change in employees´ outside options is frequently correlated with other changes in the competitive situation of an employer and, as a consequence, with the productivity of employees, the employer´s surplus and the bargaining power of employer and employee (Beaudry et al., 2012). Productivity, employer´s surplus and bargaining power however also influence wages (Guiso et al., 2005). In order to avoid biases in the measurement of the impact of changes in outside wage options on wages, the literature proposes to exploit situations in which productivity and bargaining power of the employer are not affected by the change in outside options of (some of) the employees.

This paper calculates wage changes in continuing jobs after an exogenous shock on outside wage options for a clearly defined sub-group of employees at employers whose economic situation is not affected by the shock. More specifically, we use the 2004 crafts reform in Germany that removed the requirement to hold a 'master craftsman' certificate (*Meisterbrief*) to establish a business in 52 out of 93 crafts occupations. The reform led to a boom in newly-established enterprises that crowded out employment in established crafts businesses (Koch and Nielen, 2017) and decreased wages of incumbent employees in the deregulated occupations in crafts businesses affected by the reform, in comparison to employees in crafts occupations that remained regulated (Damelang et al., 2018; Lergetporer et al., 2018).[4] In order not to confound changes in outside options of employees in deregulated occupations with changes in productivity or bargaining power of their employers, we exploit a peculiarity of the German labour market. Skilled employees exercise their crafts occupation not only in the crafts sector but also in businesses not affected by the reform. We reduce the sample to establishments registered with the Chambers for Industry and Commerce and hereby ensure that the main business activity of the employers in our sample is not in the crafts sector. We show that only a small minority of the employees working for the employers in our sample are active in regulated and deregulated crafts and that their share is not related to business indicators such as median income, profits or employment level after the reform. As we concentrate on wage changes in continuing jobs before and after the reform, we can for the first time show that wage differentiation within employers between comparable employee groups can be explained by exogeneous changes in

---

[3] Lachowska (2017) chooses a different approach to the papers presented above. She shows that outside wage options measured by the perceived ease of the worker finding an alternative, adequate job and the ease of the employer to find a replacement for the worker have an impact on wages in Sweden.
[4] The effects of the German crafts reform therefore mirror wage decreases measured after deregulations in specific industries in other countries (Fortin and Lemieux, 1997; Peoples, 1998).

outside wage options. Our results complement studies that show a general increase in wage inequality for highly regulated German labour market around the crafts reform in 2004, compare Dustmann et al., (2009). We in addition analyse whether there are differences between the reform impact by employer type.

Our analysis shows that employers react to changes in the outside options of their employees by differentiating wages for comparable employee groups. Four years after the reform, wage differences between treatment and control groups amount to about five per cent of the total wage increase during this period. We argue that the wage differences are driven by negotiations initiated by employees with higher outside wage options. Employers however do not use the opportunity to renegotiate wages when employees experience reduced outside options, because wages are downwardly rigid. This argument is based on the finding that the reform effect is positively correlated with overall average wage increases on the part of the employers. General earnings increases therefore seem to offer employers the wiggle room to differentiate wages, when selected employees use their higher outside options for renegotiations. Our data also permit us to show that there is wage differentiation within a single employer. The reform effect does not differ by employer size, works councils or collective bargaining status, when the median wage increase at the employer level is accounted for. Regional unemployment levels and the profits situation of the employer show no clear reform effect pattern.

The paper is structured as follows. We first sketch a model of how outside wage options can influence wages in continuing jobs and discuss heterogeneities in the wage effects between employer types. We then discuss the 2004 craft reform and its consequences on crafts businesses. After describing our identification strategy, we present our sample of employees working for the same industry and commerce employer before and after the reform. Next, the empirical approach and our findings are presented, followed by a discussion of the results, and a conclusion.

A Model on Wage Bargaining in Continuing Jobs

Our model shows under which circumstances outside wage options have an influence on wages in continuing jobs in a standard wage bargaining model, with employers that compete for workers by wage negotiations, compare the seminal wage bargaining models with on-the-job bargaining developed by Postel-Vinay and Robin (2002), Cahuc et al. (2006), and Bartolucci (2012).[5]

Our wage setting mechanism allows firms to compete for workers *à la* Bertrand in wages. Workers are allowed to search on the job and when they receive an outside offer, which is higher than the

---

[5] Variants of this model also have been used by Caldwell and Harmon (2019).

previous outside option, they can use it to renegotiate wages in their continuing job or move directly from job to job. Most wage bargaining models to date concentrate on employees who translate higher outside options into higher wages in continuing jobs by initiating earnings renegotiations. Our model complements this mechanism and also includes employers that translate lower outside options of their employees into lower wages, by initiating renegotiations. The employee´s and the firm´s renegotiation powers are limited by the fact that contract renegotiations of continuing employment relationships only are possible by mutual agreement. A renegotiation of wages is therefore only possible for employers and employees with a credible threat to 'leave the match' (Bartolucci, 2012). If the credible threat is non-permanent, it has to be assumed that the matching partner still honours the negotiated wage after the credible threat disappears. Workers therefore can ratchet up their wages when they have one or more temporary higher outside wage offers, even if they do not switch jobs.

The Nash wage bargaining equilibrium is the weighted average of the inside value of a job (its productivity p) and the worker´s best outside option N, plus the share ϕ of the job surplus $(p - N)$ with ϕ denoting the employee bargaining power relative to the employer bargaining power (Jäger et al., 2020). The 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. Assume that the match- or firm-specific productivity at the current firm is p_o. The maximum value obtainable in a continuing job at the current firm accordingly 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 illustrated in equation (1):

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

Assume that the worker currently obtains a wage below the maximum possible, $w_o < p_o$. 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_n as the new outside option:

$$V(p_o, w(p_o, p_n)) = \phi \times V(p_o, p_o) + (1 - \phi) \times V(p_n, w_n).[6] \qquad (2)$$

---

[6] For this solution, we need the following further assumptions: productivity is perfectly observable by every agent in the economy, the opportunity cost of recruiting a worker and job searching costs are zero the risk-neutral worker in a firm with productivity p produces p (all workers are homogenous and provide one efficiency unit per period).

The logic behind this result is that the worker can take the new wage offer and switch to the outside firm. This move however is not a credible threat, because the utility obtainable by discontinuing the new job is smaller than the utility obtainable in the current job if $p_n < p_o$. The worker however can take the better outside offer and move for an insignificant time span. The worker then contacts the previous employer again to set 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 as long as $w_n < p_o$ and therefore the new wage in the continuing job is negotiated in light of the higher outside option.[7]

Note that in this scenario the current wage is determined by the best outside option used for a successful wage renegotiation in the job on condition that the employer never initiates a wage renegotiation. Note also that not all outside wage options are credible threats that lead to a renegotiation and a wage increase, because the previous employer may not be willing or able to re-hire the employee after the insignificant time span needed to realize the higher outside option. The occasional "bidding war" therefore serves to transfer portions of the match surplus from the employer to the employee over the course of a job (Postel-Vinay and Robin, 2002; Cahuc et al., 2006). Under the assumption that wages cannot be changed unilaterally, increases in outside options that are higher than non-employment transfers have long-lasting effects on wages in continuing jobs. On-the-job search and employer competition models have found empirical support for the hypothesis that higher negotiated wages are honoured in continuing jobs, even when the higher outside options are no longer valid (Caldwell and Danieli, 2018; Caldwell and Harmon, 2019; Jäger et al., 2020; Hafner, 2020).

The renegotiation mechanism works analogously for employers that initiate wage renegotiations. After an outside option has disappeared or has been reduced, the employer can threaten to lay off the worker for an insignificant time span and renegotiate a new wage, based on the lower outside option. This scenario implies that usually unemployment benefits are the outside option if employers initiate a renegotiation. Employees do not have valid outside wage offers all the time and employers therefore can wait until the only valid outside offer is unemployment benefits (Cahuc et al., 2008; Bartolucci, 2012; Flinn and Mabli, 2009). Postel-Vinay and Robin (2002), Flinn (2006), and Cahuc et al. (2006) for example 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. As a consequence, even a long-term reduction in outside wage options has no influence on equilibrium wages, as long as employers regularly renegotiate wages, because

---

[7] If $p_n > p_o$, the employee is poached and we do not observe an earnings change in a continuing job (Caldwell and Harmon, 2019).

unemployment benefits are the relevant outside option in wage renegotiations induced by employers.

Our model concentrates on the consequences of changes in outside wage options on wages in continuing jobs. More elaborate wage bargaining models include additional wage determinants, such as the speed at which outside wage offers arrive or changes in unemployment benefits (Jäger et al., 2020). These wage determinants only play a role in our study if they differ between our treatment and comparison groups. We will show that non-employment benefits and the speed of outside wage offers did not change differently for employees in regulated and deregulated occupations and we therefore do not have to include these additional wage determinants in our model.

Heterogeneity in reactions to changes in outside earnings options

The standard wage bargaining models *à la* Pissarides assume heterogeneity between employers with respect to productivity. Differences in wage renegotiation behaviour by employers are however barely discussed in this literature (Cahuc et al., 2006; Shimer, 2006). We therefore argue now why some employer types may use their bargaining power differently from other employer types during wage renegotiations.

In Germany, industrial relations are characterised by a dual system of worker representation by trade unions and works councils. In firms covered by collective agreements or having a works council, employees may possess more bargaining power, enabling them to prevent wage cuts. Moreover, unions may also foster implicit contracts that shield employees against wage reductions (Gürtzgen, 2009; Gürtzgen, 2014). Works councils and unions additionally prefer and promote equal pay for comparable jobs and compress wage differentiation (Gosling and Machin, 1994; Card et al., 2004; Dell´Aringa, C. and Pagani, L., 2007; Dustmann et al., 2009; Addison et al. 2010; Hirsch and Müller, 2020). Many collective bargaining agreements do not allow for wage differentiations between occupation groups. They instead set a minimum wage level for each job level and determine uniform wage increases for each job level for all employees covered in a sector (or at the employer level). Many collective bargaining contracts even explicitly exclude differences in wage levels or wage increases between homogeneous employee sub-groups (Brenzel et al., 2014). We therefore assume that firms with works councils and firms covered by collective bargaining allow less wage differentiation when individual or sub-groups of employees try to renegotiate their individual wages. We also expect fewer and less effective wage renegotiations initiated by the employer as a reaction to lower outside options for subgroups of their employees.

7

Firms with high profitability can be expected to shy away from wage cuts induced by lower outside options of some employees and to share profits with their employees instead (Arai, 2003; Gürtzgen, 2009). Employees with an increase in outside options therefore may be more successful in increasing their individual wages during periods of time in which their employers enjoy high profits. We expect fewer and less effective wage reductions initiated by employers, but more and higher wage increases initiated by employees at employers with high profits.

Employer size may also play a role for wage renegotiations: small employers are frequently exempted from labour regulation, or, when enforcement of regulation is weak, small firms do not comply (Boeri and Jimeno, 2005). Small employers therefore may use the opportunity to reduce the earnings of selected employee groups when the outside options of these groups decline even if wage reductions for selected employee groups are not supported by collective wage bargains or works councils. Analogously, Guiso et al. (2005) note that large employers have an easier access to financial markets in downturns. They can therefore better buffer outside shocks and provide insurance for their employees. We also should take into account that in Germany, larger employers are the first choice for high-ability labour market entrants and skilled workers (Soskice, 1994). Smaller employers therefore may be forced to selectively offer higher wages to their few high potentials in order to retain them. Finally, larger employers may also be less able to disentangle individual contributions of their employees to corporate success than smaller employers (Oi and Idson, 1999; Lallemand and Rycx, 2006). They therefore may be less willing to react on wage renegotiations initiated by their employees because they do not know their productivity and potential. In summary, smaller firms may be better able and willing to differentiate between employees in wage renegotiations according to their outside options (Guiso et al., 2005; Du Caju et al., 2012; Jäger et al., 2020).

The regional unemployment situation may also play a role for the wage effect of renegotiations. A higher unemployment incidence weakens the bargaining power of employees and therefore the wage effect of higher outside options. Renegotiation demands may also trigger the risk of dismissals and therefore employees may be more reluctant to initiate wage renegotiations in times of high unemployment (Beaudry and DiNardo, 1991; Blanchflower and Oswald, 1994). Employers however may use the fear of unemployment or low re-employment chances as a pretext to demanding wage concessions of their employees when their current wage is based on outside options higher than unemployment benefits. We therefore assume that wage renegotiations initiated by employees are more frequent and lead to higher wage increases in regions with lower unemployment. Employers less frequently initiate wage renegotiations in these regions.

Finally, we have to take into account that there may be an asymmetry between the incidence of wage renegotiations initiated by employers and renegotiations initiated by employees, given a change in outside wage options for a clearly defined group. The reason is that downward wage rigidity in continuing jobs is strong in Germany, taken from an international perspective (Dickens et al., 2007; Bauer et al., 2007; Hirsch and Zwick, 2015). Unions and works councils usually make sure that collective bargaining agreements are not undercut by the employer and many employers without these industrial relations institutions copy such behaviour.[8] Wage floors set by collective bargaining agreements however differ by sector, region or employer (if the employer has a collective agreement on the firm level (*Haustarifvertrag*)). They never differ however for employee groups within one employer. The only way for an employer to undercut the wage floor 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 (Stettes, 2009). Unions and works councils have to approve the design of wage reductions in these exit clauses and it seems improbable that they accept official wage reductions that differ for employee groups with different outside options. We therefore conclude that wage reductions in continuing jobs and especially wage reductions for sub-groups of employees are improbable in Germany. Please also note that our wage bargaining model predicts that reductions in outside wage options should not induce changes in wages if employers initiate wage renegotiations. Employers use unemployment benefits rather than outside wage offers as relevant outside options for their wage renegotiations.

If indeed wage renegotiations initiated by employers as a reaction to lower outside wage options are improbable, we may assume that differences in wage developments induced by changes in outside options are mainly observed within employers with sufficient "wiggle-room" to increase earnings more substantially for some employee groups than others. In other words, wage differences between the employee groups with differences in their outside options should mainly occur at employers with a relatively strong increase in their average earnings level for all employees.

The 2004 crafts reform in Germany

Traditionally, all craftsmen working in the German crafts sector were regulated by occupational licensing. Specifically, 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

---

[8] Glitz (2012) for example argues that union influence prohibited a reduction of wages for workers in regional labor markets with a strong influx of ethnic Germans in the period 1996-2001 also for employers without collective wage agreements.

qualified craftsmen (*Handwerksrolle*).[9] In 2000, the European Court ruled that Germany's crafts regulations were not compatible with the single European market. In response, Germany liberalised the entry regulations for establishing a business in selected crafts in 2004. After the reform, a master craftsman certificate and registration in the register of qualified craftsmen were no longer prerequisites to establishing and operating a business in 53 out of the 94 crafts. In the remaining 41 crafts (our control group), the master craftsman certificate remained the central requirement to establish a business.[10] The reform turned deregulated crafts (our treatment group) 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 and it has many traits of a random decision. The political bargaining process had the consequence that until shortly before the reform bill was passed, it remained unclear which occupations would be deregulated and which not. More specifically, politicians originally planned to keep hazard-related crafts regulated and deregulate crafts jobs with low risks for consumers. This strategy however was diluted shortly before the reform (Gathmann and Lembcke, 2019; Damelang et al., 2018). As a consequence, many similar crafts occupations can be found in the regulated and deregulated groups. Examples are tile, slab and mosaic layer, and cast stone and terrazzo maker (deregulated), and bricklayer and concreter (regulated), or metal former, galvanizer, and metal and bell founder (deregulated) and metal worker (regulated). The reform bill was passed on December 19th, 2003 and the reform took effect on January 1st, 2004. The crafts reform therefore can be characterised as a quasi-natural experiment with a very short announcement period. It consequently hardly had any anticipation effects (Lergetporer et al., 2018).

Many observers argue that occupational licensing is a device to generate profits for licensed practitioners (Kleiner and Krueger, 2013) because the fixed entry costs to obtain the necessary qualifications reduce their supply (Kleiner, 2006; Branstetter et al., 2014). In line with this theory, the number of newly-established businesses in the deregulated occupations increased by more than 30

---

[9] Only few craftsmen were allowed to start a business without a master craftsman certificate before the reform, most prominently applicants with crafts education certificates from other countries and apprenticeship completers without a master craftsman certificate but at least six years of experience in the craft and four years in a management position (*Altgesellenregelung*), compare (Koch and Nielen, 2017).

[10] The entry restrictions for the 41 regulated occupations became somewhat less restrictive after 2004 as well. An official German degree in a related field, like an engineering title or degrees from technical professional schools, was now considered equivalent to a master craftsman. In addition, a craftsman who had at least six years of work experience, of which four had to be in a management position, also became eligible to register a business. That last rule did not extend to trades with a potentially high risk for the customer, however. A final legal change induced by the 2004 reform was that the master craftsman degree was no longer tied to the business owner, as long as the facility manager was a master craftsman (Koch and Nielen, 2017).

per cent directly after the reform. (Kleiner, 2006; Lergetporer et al., 2018, Fig. 1). Two years after the reform, the number of businesses in deregulated occupations had doubled, and ten years after the reform, it had tripled (Rostam-Afschar, 2014; Runst et al., 2019). In the regulated occupations, the number of businesses and the number of start-ups remained roughly the same between 2004 and 2008[11] however (Rostam-Afschar 2014; Koch and Nielen, 2017; Runst et al., 2019; Damelang et al., 2018; Lergetporer et al., 2018).

Besides the number of newly-established businesses in deregulated crafts, their character also changed after the reform. Rostam-Afschar (2014) shows that the probability of becoming self-employed increased by approximately 20 per cent in deregulated crafts, compared to regulated crafts. As a consequence, the number of self-employed increased by about 30 per cent in the deregulated crafts during the first five years after the reform. The number of self-employed in the regulated crafts however increased by only 10 per cent (Lergetporer et al., 2018). Many of the new self-employed businesses operated as solo entrepreneurs, and the owners frequently had a migrant background from Middle and Eastern Europe (Koch and Nielen, 2017).[12] The five year business survival rate gradually declined from 70 per cent before 2004 to 45 per cent in 2007 in the deregulated crafts businesses. The survival rate in the regulated occupations however remained constant at about 70 per cent during this period (Müller, 2016; 2018). In addition, the massive start-up boom in the deregulated crafts led to a reduction in profitability of established enterprises in these crafts (Fredriksen, 2018). The increased start-up dynamic induced by the reform was not mirrored by an increase in the creation of jobs for craftsmen in deregulated crafts. In the five years after the reform, employment numbers in the deregulated crafts were only somewhat higher than employment in regulated crafts: the number of jobs decreased by three per cent in deregulated crafts and by six per cent in the regulated crafts (Lergetporer et al., 2018).[13]

Most importantly for this paper, wages of employees in the deregulated crafts occupations decreased on average, compared to wages in the regulated crafts occupations.[14] Damelang et al. (2018) measure average earnings differences between regulated and deregulated crafts employees

---

[11] The number of enterprises in the deregulated crafts was 75.000 (11.3% of all craft enterprises) in 2003. This number increased to 175.000 enterprises in 2008 (22.6% of all craft enterprises).

[12] The share of solo businesses in the deregulated crafts was almost twice the share in the regulated crafts businesses (61% vs. 33%) in 2012 (Müller, 2016; 2018).

[13] Müller (2016, p. 14 Figure 4) normalises employment levels just before the reform (2003) at 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 until 92 in the deregulated crafts and 95 in the regulated crafts in 2005. Employment levels recovered to 100 in both groups in 2010.

[14] All papers use the Sample of Integrated Labour Market Biographies (SIAB), a two percent sample of all employees in Germany.

for the four-year periods before and after the reform. They do not control for time trends and do not match observations from regulated and deregulated occupation groups on observables. They find a negative deregulation effect on wages of 6.5 per cent. Lergetporer et al. (2018) include year-fixed effects in their individual-fixed effects difference-in-differences estimations. They use entropy balancing to control for industry-specific shocks. They find significant negative earnings effects of the reform of about 4 per cent for the unmatched sample and of 2.3 per cent after using entropy balancing for the period until 2014. Gathmann and Lembcke (2019) use nearest neighbour matching on the propensity score to be in a deregulated or a regulated occupation. In their difference-in-differences estimations, they control for industry-fixed effects, as well as industry-specific and occupation-specific trends. In contrast to the previous papers, they also include benefits during unemployment periods. They do not find a wage effect of the reform for the period until 2010.

Identification strategy

The literature on effects of the 2004 crafts reform so far analyses average wage effects on all employees in regulated and deregulated craft occupations irrespective of whether employees stay with their employers during the observation period or not. In order to capture wage effects of the reform on continuing jobs, we reduce our sample to employees who do not change their employer. In addition, we also exclude all employees who switch occupations during the entire employment spell observed. These sample restrictions avoid endogeneity biases from employees who switch to another employer or occupation in order to avoid negative effects from the crafts reform.[15]

We want to make sure 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. The second decisive sample restriction in this paper therefore is our concentration on employees who work for employers in the industry and commerce sector. We will show below that the main business activity of these firms is not affected by the crafts reform and that the proportion of employees in crafts occupations is small in these employers. As a consequence, third factors such as differences in changes in productivity or bargaining power between treatment and control group do not drive our results. Our paper therefore complements prior research on the impact of the German craft reform because previous papers concentrate on the average wage effects of the crafts

---

[15] Damelang et al. (2018) and Gathmann and Lembcke (2019) only exclude switchers between regulated and deregulated occupations, Lergetporer et al. (2018) exclude occupation switchers before the reform only.

reform in the crafts sector or for businesses directly affected by the reform (Lergetporer et al., 2018).[16]

We use a specificity of the German occupation regulation to identify employers that are not affected by the crafts reform although they employ craftsmen. All businesses in Germany have to be registered either with the regional Chamber of Crafts or with the regional Chamber of Industry and Commerce. According to the crafts law (*Handwerksrecht*), an enterprise has to register with the Chamber of Crafts if the main product or service is an activity listed in Appendix A or B1 of the Crafts Code (*Handwerksordnung*). Also the crafts reform identified crafts activities according to this list. The reform actually led to a split between the regulated crafts listed in Appendix A and the newly deregulated crafts activities listed in Appendix B1.[17] As a consequence, registration of an enterprise with the Chamber of Crafts means that the main business activity of the enterprise perfectly overlaps with crafts activities in our treatment and control group. Only if an enterprise has its main business activity not in one of the crafts activities specified by the crafts law, can it be registered with the Chamber of Industry and Commerce. We therefore know that enterprises registered with the Chamber of Industry and Commerce[18] mainly pursue business activities unrelated to the deregulation in founding a business and its consequences.[19]

We accordingly identify employees whose outside wage options are affected by the crafts reform and employees in the control group on the basis of their actual job activity, rather than their educational background. In other words, we only include employees whose employers indicate in their social security records that they currently perform one of the occupations listed in Appendix A and B1 of the Crafts Law. For example, we include a carpenter (regulated crafts occupation) who works for an employer in the chemical industry, or a modeller (deregulated crafts occupation) who works in the car development department of a car company. We do not however include people

---

[16] In order to concentrate on employers affected by the crafts reform, these papers for example only include small firms (Lergetporer et al., 2018) because crafts firms tend to be smaller than industry and commerce firms. Koch and Nielen (2017) and Runst et al. (2019) use data for crafts firms provided by the German Confederation of Skilled Crafts. Runst et al. (2018) suggest including only employers with a share of more than 60% in crafts occupations to obtain a sample of businesses affected by the reform.

[17] There is a list of 57 activities classified as similar to crafts (*handwerksähnlich*) that do not need a license or certificate. The enterprises mainly active in these crafts are listed in the Appendix B2 of the Crafts Code and also registered with the Chambers of Crafts. Activities listed in Appendix B2 are mainly basic jobs that do not require a lot of experience and are not in competition with crafts listed in the Appendices A and B1. Examples of these crafts are piano tuner, ice-cream manufacturer or floor layer. We exclude employees working in these occupations and also exclude the firms registered with the chamber of crafts because their main business activity is in the domain of Appendix B2.

[18] Note that all papers on the effects of the 2004 crafts reform so far use data that do not include information on chamber registration (SIAB and Microzensus). These papers therefore cannot identify whether the employer was affected by the crafts reform or not.

[19] There is a small share of businesses that is registered in both chambers. We exclude these firms from our sample in order to be sure that the employer is not affected by the crafts reform.

trained as carpenters or modellers who work for industrial employers in unskilled or semi-skilled activities unrelated to their professional education. The vast majority of employees in crafts occupations with an apprenticeship or a master craftsman certificate work in adequate skilled jobs in the industry and commerce sector. Only a small proportion work in unskilled or semi-skilled jobs not related to their education.[20]

Craftsmen (i.e. employees with a completed dual apprenticeship education (*Geselle*) or a complete master craftsman education (*Handwerksmeister*) who work in their occupation in the industrial or commerce sector are a common phenomenon in Germany, because they received education certificates that enable them to work in all sectors of the German economy. In fact, many master craftsmen and craftsmen prefer to work as employees in the industrial and commercial sector. More than 20 per cent of all master craftsmen and about one quarter of all skilled employees with completed apprenticeship education in a crafts occupation work in the industrial and commercial sector (Haverkamp and Gelzer, 2016).[21]

We also include employees who currently work in a crafts occupation, but do not have a completed apprenticeship education or have an academic degree. Employees who did not complete an apprenticeship, may have completed a professional education in another country that is not officially recognised 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 may have completed an apprenticeship or a master craftsman certificate before their academic study.[22]

Jobs in the crafts sector only offer credible outside wage options if we indeed observe employees in crafts occupations who switch (back) from the industrial and commerce sector to the crafts sector. We are not aware of representative studies on moves of this group of employees from the industry and commercial sector to the crafts sector. We can however use the employer movement incidence in our data set as indicator. We know the economic sector of both employers if the target employer

---

[20] There is a widely shared but biased perception that craftsmen mainly find work as semi-skilled employees in the industrial and commerce sector (Soskice, 1994), also see the quote: "For example, the leading trainer of bakers in Munich is the Ford Motor Company", Heckman (2004).

[21] Most craftsmen and master craftsmen obtain their certificates when working for a crafts firm. In some cases, they obtain their apprenticeship or master craftsmen education when working for an employer in industry and commerce. These employers usually use inter-company training facilities ("*überbetriebliche Ausbildung*") in order to cover the parts of the curricula that are not their core business. The value of the certificates in principle is the same irrespective of the economic sector the training firm is active in, because the minimum training contents are independent of the training firm. Also, the more theoretical training part at the professional school ("*Berufsschule*") is identical for all apprentices.

[22] We only observe the highest education and current occupational status in our dataset and therefore do not know all educational degrees.

is known (i.e. movers between two LIAB establishments). We observed 2131 moves of employees from an employer in the industry and commerce sector to another employer with known economic sector. In this sample, 36 movers joined an employer in the crafts sector. The proportion of 1.5 per cent is small. The main reason for few switches from the industrial and commerce to the crafts sector is that on average pay in the industry and commerce sector is substantially higher for the same kind of jobs than in the crafts sector (Soskice, 1994). The threat of a switch to an employer in the crafts sector may lead to a wage increase in a continuing job after renegotiations if the wage offer from a crafts employer is higher than the non-employment benefits according to our theoretical model.

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 plant-level survey data from the IAB Establishment Panel. The distinctive feature of the LIAB is that besides establishment level information, administrative information of the labour market histories of all individuals who work in the establishments is available. Our data set includes all establishments with three consecutive entries in the IAB Establishment Panel between 1999 and 2002 and all employees who worked at least one day in those establishments between 1997 and 2003. For these employees, the data report complete employment histories between 1993 and 2010 (Jacobebbinghaus, 2008), a period that perfectly fits with the observation period we are interested in. Besides the education background, nationality, age and gender of all employees, we know the current occupational activity, plus current wage, tenure, and labour market seniority with a daily accuracy. We also know for all employers whether they are registered at their local Chamber of Industry and Commerce or their Chamber of Crafts (or both), median earnings, a subjective profit assessment, revenues, the size and the characteristics of their workforce, and industrial relations arrangements, such as the application of collective bargaining (at sector or firm level) or the presence of works councils.

We only include full time employees because the LIAB does not report working hours. This avoids measurement errors induced because wages are changed with 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 1988 (*Klassifikation der Berufe*, KldB88).[23] We can identify 39 of the 41 regulated occupations and 49 of the 51 deregulated occupations (Appendix Tables A1 and A2).

---

[23] See the analogous identification strategies used in Lergetporer et al. (2018), Damelang et al. (2018), and Gathmann and Lembcke (2019).

The highest occupational status obtained (*Stellung im Beruf*) is known for four occupational status levels, un- and semi-skilled, skilled (having completed an apprenticeship training), master craftsmen, and academic. Occupations, education levels, and occupational status are reported by employers. This information carries a higher risk of misreporting than the earnings, tenure and employment type information that are also reported by the employer, because it is not relevant for the social benefit calculations (see for example Fitzenberger et al., 2006). As "atypical" employees in crafts occupations jobs may have a higher risk of misreporting than "typical" employees, we reduce our sample to skilled employees with a completed apprenticeship training and a master craftsman degree in a robustness check.

We analyse the individual wage development in continuing jobs between 1999 and 2008. We do not extend our observation period beyond the year 2008, because Germany was hit hard by the Great Recession in the year 2009. Large export-oriented manufacturing establishments in particular had to reduce hiring, scrap bonus payments, reduce training and send their employees into short-term work (Rinne and Zimmermann, 2012). The possibility cannot be excluded that the recession affected employees in the deregulated occupations stronger than employees in the regulated occupations (Lergetporer et al., 2018). Therefore wage changes in the years after 2008 may not be exclusively attributable to the crafts reform.

Empirical Analysis

Our identification strategy rests on the assumption that only the reduction in the outside wage options of employees in deregulated crafts occupations, in comparison to employees in regulated crafts occupations, drive the wage differences after the reform. Other wage determinants are unchanged or they change in tandem for employees working in regulated and deregulated crafts activities. We therefore have to eliminate observable differences between both employee groups before the reform (Gathmann and Lembcke, 2019; Lergetporer et al., 2018). More specifically, in order to make sure that the common trends assumption is met, we use entropy balancing (Hainmueller, 2012). This is a nonparametric data pre-processing method developed for binary treatment studies, so that the means of all covariates are exactly balanced between treatment and control groups. In addition, we show later 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 for employees in regulated and deregulated occupations moved in tandem after the reform.

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

$$y_{it} = \alpha_1 + \alpha_2 \text{ deregulated X post reform} + \mu_t + \mu_i + \varepsilon_{it} \tag{3}$$

with y log daily wages for worker i in year t. The variable *deregulated* is an indicator variable that equals one, if the employee works in one of the deregulated crafts occupations, and zero if the employee works in one of the regulated crafts occupations. The variable *post reform* equals one, if year t is in the period 2004-2008 and zero in the period 1999-2003. Our main coefficient of interest, α_2, gives us the average reform effect on wages for employees who work in a deregulated crafts occupation compared to employees who work in a regulated occupation. We include individual-fixed effects μ_i to partial out unobserved individual heterogeneity, time-fixed effects μ_t that take out common time specific effects[24], and we use standard errors clustered at the individual level.

In order to obtain homogeneous comparison groups, we use entropy balancing.[25] This matching method incorporates covariate balancing in the first and second moments of the covariate distribution directly into a maximum entropy reweighting function.[26] It hereby constructs weights $w(i,j)$ with i indicating individuals in the treatment group and j in the comparison group for each observation in the comparison group such that pre-specified balancing constraints are fulfilled precisely. In our case, means of all balancing variables are identical after matching. We use an extensive list of individual, employer, and regional labour market characteristics including individual wages for 1999-2003.[27] 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 $w(i,j)$:

$$\tilde{\alpha}_2 = (1/n_1) \sum_i \epsilon \, I_1 \, [(Y^{\text{after}}_{1i} - Y^{\text{before}}_{1i}) - \sum_j \epsilon \, I_0 \, w(i,j)(Y^{\text{after}}_{0j} - Y^{\text{before}}_{0j})] \tag{4}.$$

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

The interpretation of coefficient α_2 as the causal reform effect on wages mainly relies on three assumptions. First, the common trends assumption means that trends in the outcome variable would have been parallel in the absence of the treatment. In our case, a good indicator of this un-testable assumption is that the wage changes of both groups before the crafts reform were the same. Without matching, employees in the deregulated crafts occupations for example have significantly

---

[24] It is not possible to include establishment-fixed effects μ_j that take out establishment-specific earnings changes in the model in addition to individual fixed effects because we do not have employer movers in our data set. We therefore include establishment fixed effects into the regression instead of individual fixed effects in a robustness check.

[25] The STATA command for the procedure is `ebalance`, compare Hainmueller and Xu (2013).

[26] We use the default tolerance level of 0.015 for achieving convergence in the optimization.

[27] Our list of matching variables is similar to the list used for example by Lergetporer et al. (2018), but it adds several employer characteristics.

lower schooling, professional status and wages and they work in smaller establishments that pay less for their average employee. These and further differences between treatment and control groups may have an impact on wage increases (Lergetporer et al., 2018; Müller, 2018). Entropy matching ensures that all relevant wage determinants in both groups before the crafts reform are the same. Second, we have to 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. In our case, a reform anticipation effect can be excluded 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 one year before the reform and did not change their occupation and employer during the entire observation period. Third, we need the stable unit treatment value assumption (SUTVA) that the outcome of the comparison group after the treatment would have been the same both in a world without the reform and in a world with the reform (Rubin, 1977). We only include employees who obtained their occupational degree in a regulated occupation before the reform, and did not change their occupation during the observation period. In addition, we only include employees who work for employers not affected by the crafts reform and continue to work for the same employer. Therefore it seems very improbable that the reform affected the employees in regulated crafts occupations. The SUTVA therefore should be met.

In order to analyse our hypotheses, we show possible heterogeneities in the reform effect for different groups of employers. We divide the employers into the following sub-samples: employers with and without works councils, with and without collective bargaining (at the sector level and the employer level), three employer size groups, employers in the three main industrial and commerce sectors, three profit levels, and four regional unemployment level quartiles. Finally, in order to check whether general wage level increases at the employer level are a pre-requisite for reform wage effects, we differentiate the employers by their median wage level increase and sort them into deciles. We also want to be sure that the results of our analysis can be transferred to a single employer. We therefore exclude all employers with only members of the treatment or control group respectively in a robustness check. In addition, we reduce our sample to just one large employer. Additional robustness tests show that our results are also 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 between the entire period between 1999 and 2003, reduce our sample to employees without atypical crafts employees (those without an occupational degree or an academic degree), and reduce our sample to employers with a share of employees in regulated and deregulated crafts

occupations smaller than 20 per cent, respectively. We finally show that pseudo treatment effects before and after the actual reform do not lead to measurable wage effects.

Findings

Employees in crafts occupations working for German industrial and commerce firms are concentrated in metalworking and electrotechnical occupations. These deregulated occupations have the highest proportions (proportions in all deregulated occupations are in brackets): cutting tool mechanic (32%), fitter (13%), and glass finisher/precision optician (8%). All other occupations have a share below 5 per cent, respectively. The most common regulated occupations are: electrician (27%), electrical engineering technician (10%), and motor vehicle maintenance technician (9%). Again, all other regulated crafts occupations have shares below five per cent, respectively.

Analogously to the previous literature on the crafts reform, we find that employees working in deregulated crafts occupations earn less and they are less educated, more frequently female, with a migration background, and younger than those employees working in regulated crafts occupations (see Table 1 and Lergetporer et al., 2018). We find in addition to the previous literature that employees in deregulated crafts occupations work for smaller employers that pay less on average and that are more profitable. The employers of employees in deregulated crafts occupations accordingly less frequently have a works council or participate in collective bargaining, they are more frequently active in raw materials, manufacturing and mechanical engineering, and less in construction, trade and services.

Entropy balancing works well and perfectly aligns the means of all variables in the treatment and control group in the period 1999-2003.[28] Table 1 therefore only shows employer and employee characteristics before entropy matching and omits the report of the matched variables. Note that Table 1 also indicates in accordance with the previous literature on the 2003 craft reform that employees in the deregulated occupations had a less dynamic earnings development in the years before the reform (see last four lines). As we include the wage level in each year between 1999 and 2003, the wage levels of the matched samples are identical. Figure 1 shows the development of matched and unmatched wages in regulated and deregulated crafts occupations by year. After the reform, wages of the matched employees in deregulated crafts occupations increase slightly less than wages of employees in regulated crafts.

---

[28] In contrast to the entropy matching for employment in Hafner (2020), our results are robust with respect to the inclusion of different sets of covariates in the matching list.

If we calculate the treatment effect of the crafts reform on wages between 2004 and 2008 in a difference-in-differences regression, we obtain a significantly flatter development of wages for employees in deregulated crafts occupations. The wage difference amounts to 1.8% in the five-year period (refer to column 1 in Table 2). After entropy matching, the treatment effect declines to 0.5% but it remains significant at the five per cent level. Average earnings increase in the period between 2004 and 2008 by 9.7 per cent. Our difference in earnings increases therefore translates into a relative wage disadvantage of employees in deregulated occupations of about five per cent. Table 3 shows that the treatment effects differ somewhat from year to year. The reform has a significantly negative wage effect in 2005, 2007 and 2008.[29] Not surprisingly, there are no wage differences before the craft reform in our matched sample.

Our results are robust to several changes in the econometric design (see Table 4). If we replace individual fixed effects by establishment fixed effects, we obtain the same treatment effect (column 1). The treatment effect is also unchanged if we replace nominal wage changes by real wage changes, taking into account inflation (column 3). Our 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 about doubles to more than 41,000 individuals (Table 4, column 2). The treatment effect is larger (0.9 per cent) in the larger sample (the treatment effect also is somewhat larger with 2.4 per cent for the unmatched larger sample).

In the next estimation steps, we look at differences in the treatment effects by employer groups.[30] We find that the treatment effect increases with employer size – there is no treatment effect for employers with less than 250 employees, but for employers larger than 1000 employees, the effect is 0.8 per cent, compare Table 5. This finding is not according to our hypothesis. Also contrary to our hypotheses, works councils and collective wage bargaining do not restrict wage differentiation between treatment and control groups. In fact, we only find a treatment effect for employers with works councils and 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. We also find treatment effects for employers with both a good and bad profit situation, but not for employers with a satisfactory profit situation (Table 8). In addition, we find that the negative treatment effect is concentrated in companies within the raw materials and manufacturing sector and we find no treatment effects for the other two sectors (see

---

[29] Lergetporer et al. (2018) also find significant treatment effects only for selected years (2004, 2007, 2008 and later).
[30] We also calculated the reform effects without entropy matching in the sub-samples. We in general obtain higher reform effects, the relative differences found for matched groups in the sub-samples presented in this section are also found, however. For example, reform effects are much smaller for small employers in comparison to large employers.

Table 9). There is no clear relationship between the regional unemployment level and the treatment effect size (Table 10). Finally, our hypothesis that wage differentiation is only possible if the wage level at an employer increases in general seems to be supported. If we sort all employers by their wage increase for the median worker and divide them into wage increase deciles[31], we only find treatment effects for employers with wage increases higher than the 6th decile (compare Tables 11a and 11b).

A problem of sample-splitting is that several mechanisms may be combined in one group. In particular, the negative reform effects at establishments with works councils, collective wage bargaining and large establishments that are not in accordance with our hypotheses may be driven by a third unobserved factor. The negative correlation between the effects of a general wage increase on wage differentiation after the reform suggests that the negative effects at the large employers with works councils and collective bargaining are mainly driven by their higher wage increases. This assumption is supported by Table A3 that shows average median wage increases between the years 2004 and 2008 for the employer groups presented in Tables 4-11. All employer groups with significantly negative reform effects (large employers, employers with works councils and with collective wage bargaining (especially at company level), employers active in the raw materials and manufacturing sector) have higher than average median earnings increases at the company level.

In order to test whether general wage increases at the establishment level mainly drive the treatment effects, 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 12). The interaction terms for firm size, works councils, and collective bargaining lose their significance, however. This means that unions and works councils have no impact on wage differentiation when the generally positive wage trend at employers is taken into account.

Robustness tests

In the following robustness checks, we want to ensure that our results are not driven by "problematic" employee or employer groups. In the first robustness check, we reduce the sample to

---

[31] 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. The wage measure also includes the majority of employees who are not active in crafts jobs. If an employer is not observed for the entire period 2004-2008, the longest observation period is used. The average yearly median earnings increases on employer level between the years 2004 and 2008 for our sample are by decile: 0.001, 0.008 0.013, 0.018, 0.023, 0.029, 0.032, 0.035, 0.043, and 0.077.

those employees with a completed crafts apprenticeship education or a master craftsman certificate. We find that the treatment effect for the reduced sample is comparable to the average effect for the full sample (first column in Table A4). In a second robustness check, we eliminate all employers with more than 20% of their employees in deregulated and regulated crafts occupations, respectively. We therefore exclude employers that potentially face the strongest impact from the crafts reform. Again, the treatment effect is comparable to that of the full sample (second column in Table A4).

Our identification strategy compares all employees in deregulated crafts jobs with all employees in regulated craft jobs. Our matching procedure therefore compares the wage development of employees who work for different employers. If differences in wage developments between treated and untreated employee groups also apply to employees within the same employer, this is an indicator for wage differentiation at the employer level. We therefore check whether a reform effect also can be detected on the employer level. In a first step, we exclude all employers with either no regulated or no deregulated crafts employees. We find that the reform effect is also present for the sub-group of employers with at least one regulated and deregulated crafts occupation employees (the reform effect is 0.3 per cent for this sample of firms, see third column in Table A4). This robust result is not too surprising given the fact that about 90% of the employees in our treatment and control group work for an employer that has employees from both groups. The next robustness step checks whether the reform effect can be measured even within a single employer. Only when the wage development of a sufficient number of employees can be compared, do we have the chance of observing a reform effect. We therefore 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 two 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 and therefore experiences a more dynamic wage development than the average employer in our sample. Even if we reduce our sample to this employer, we find a significant reform effect (the effect is 0.2 per cent) (see the fourth column in Table A4).

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

Finally, we want to make sure that the reform effects presented are mainly driven by changes in outside wages between regulated and deregulated crafts occupations after the crafts reform, rather

than by other potential drivers of wages related to our two occupation groups. We first analyse whether employment and unemployment develops differently for regulated and deregulated occupations between the years 1999 and 2008. Unfortunately, there are no data on occupation-specific employment and unemployment for all crafts occupations included in our sample. The internet resource "*Berufe im Spiegel der Statistik*" (occupations mirrored by statistics, see: http://bisds.infosys.iab.de/) provided by the Institute for Employment Research in Nuremberg however provides data on the employment and unemployment development for many occupations. The occupation selection is based on the Classification of Occupation Code (KldB88), the classification we also use in our data set. We therefore can obtain information about employment and unemployment levels for 25 out of 41 regulated and for 30 out of 51 deregulated craft occupations.[32] Employment decreases more strongly in regulated crafts occupations between 1999 and 2001 than employment in deregulated craft occupations (Figure A1). For the following years, until 2008, the employment development is similar in both occupation groups, however. Unemployment is somewhat higher in the years 1999 and 2000 for deregulated crafts occupations (Figure A2[33]). The relative unemployment rates then are roughly similar between 2001 and 2005. For the period 2006 to 2008, unemployment decreases somewhat more for regulated crafts than for deregulated crafts. The measured reform effect in the last three years of our observation window therefore partly may be a consequence of the small relative increase in unemployment risk for employees in deregulated crafts. We do not find a correlation between the average regional unemployment rate and the average reform effect (compare Table 10). We also argue above that the reform effect is mainly measured at employers with high median earnings increases. We therefore can assume that wage differentiation is mainly driven by wage renegotiations initiated by employees instead of wage renegotiations initiated by employers. Therefore we suppose that the effect of the somewhat more dynamic unemployment development in deregulated jobs after 2006 on wage development is small. This assumption is in accordance with the previous literature that shows that changes in unemployment have little effect on earnings (Hall and Milgrom, 2008).[34]

---

[32] The crafts occupations not covered in this data source are marked in Appendix Tables A1 and A2 by an asterisk.

[33] The unemployment rate for deregulated occupations was 19.5% and for regulated occupations 20.2% in 2003.

[34] We can exclude that there are differences in changes in non-employment earnings options between treatment and control group after the reform. Unemployment benefits are paid for a maximum of one year after losing a job. The benefits are set at about 60 per cent of the previous earnings (67 per cent of an employee with at least one dependent child) given previous earnings are below the so-called social contribution ceiling (*Sozialbeitragsbemessungsgrenze*). If the resulting unemployment benefit is lower than the social assistance level, it is increased to this level. After one year of unemployment, the unemployment benefits are reduced to the social assistance level (*Arbeitslosengeld II*). These rules have been introduced in 2005 and they replaced the more generous unemployment benefits (*Arbeitslosengeld*) and unemployment assistance (*Arbeitslosenhilfe*). All unemployment benefit rules apply to all employees and therefore there are

We finally 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 (Table A6[35]). It is not surprising that the share of deregulated and regulated crafts occupations employees hardly is correlated with the economic situation of employers in the industry and commerce sector. The share of employees in crafts occupations is small: about nine per cent of the employees work in deregulated and about 16 per cent work in regulated crafts occupations. Almost 90 per cent of the employers in our sample have fewer than 50 per cent of their workforce active in crafts occupations. The results of our robustness check in column 2 of Table A4 also show that the reform effects are not driven by employers with relatively high shares of crafts occupations employees.

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

Discussion

Our analysis provides empirical evidence for a positive impact of higher outside wage options to wage increases for selected groups of employees within employers. To the best of our knowledge, there are only two papers on the impact of outside wage options on wages that use natural experiments and therefore can claim to identify causal effects. Caldwell and Danieli (2018) use the same data set as our paper (LIAB). They exploit the introduction of high-speed commuter lines in a region as exogenous variation in labour market options on the regional level. In their main estimations, they include job movers and job stayers and find a positive effect of the treatment of wages. In a decomposition exercise, they also find significantly positive wage effects for employees who stay in the same job. These effects are however clearly smaller than the effects for employees who switch their job. Their estimations include industry fixed effects but no employer data or controls. Their findings therefore are compatible with homogeneous wage changes at the employer level. Employers affected by the extension of the regional labor market of their employees may

---

no differences between regulated and deregulated crafts education with respect non-employment outside options.

[35] The results are unchanged if we split the share of crafts employees in the interaction term into regulated and deregulated crafts shares.

increase wages more significantly, but symmetrically, for all of their employees than employers in the control region. Hafner (2020) also uses linked employer-employee data. He includes only job stayers. The exogenous variation in outside wage options exploited is the international labor market integration between France and Switzerland. The natural experiment therefore is at the regional level and no employer characteristics are controlled for. Again, the results are compatible with more significant, but homogeneous, wage changes at the employer level in French regions bordering Switzerland, in comparison with control regions without regional labor market interaction.

Our paper therefore for the first time provides evidence on selective wage changes at the employer level induced by changes in outside wage options. This result complements a vast literature on wage differences between similar workers in Germany. The literature so far concentrated on calculating wage differentiation between employee groups for the entire economy (Dustmann et al., 2009; Goldschmidt and Schmieder, 2017) or for employer wage premia (Gürtzgen, 2009; Card et al., 2013). These papers however do not take into account differences and changes in workers´ outside options (Hirsch and Müller, 2020) and they do not analyse wage differentials within single employers.

Probably the most important finding of this paper is that works councils and unions, which are institutions that usually champion wage equality, do not prevent wage differentiation at the employer level. In the rigid German wage bargaining system, employers are allowed to voluntarily offer a higher compensation than specified by the collective agreements. Legal ways to increase wages above collective agreements are the regrouping of selected employees into higher job categories, allowances for atypical or heavy work, vacancy pay, performance pay, and profit sharing. Effective wages therefore often exceed collective bargaining wages (Dustmann et al., 2009; Addison et al., 2010)[36]. Employers with works councils or collective wage agreements can use these channels in wage renegotiations initiated by employees from groups with improved outside wage options in order to retain their most prolific employees. Our results corroborate the findings by Cahuc et al. (2006). They stress that rent sharing between employer and employees in continuing jobs is driven by employer competition in France. Therefore also in France employers are willing to differentiate wages in order to retain employees if employees try to renegotiate their wages. France and Germany are countries in which employer associations and unions negotiate wages for the majority of workers at the industry level and both countries have institutional wage floors that induce strong downward wage rigidity.

Our results complement previous findings on the role of institutions on wage differentiation. Studies on the influence of institutions and regulations on wage compression typically use the entire wage

---

[36] Jäger et al. (2020) argue that the institutional environment leaves substantial room for idiosyncratic deviation from collectively bargained wage floors in Austria. The labour market institutions in Austria and Germany are similar.

distribution or returns for education (Fortin and Lemieux, 1997). These studies therefore also include wage changes of job movers and job entrants into the labour market. Dustmann et al., (2009) show a slight increase in wage inequality for Germany in the years around the 2004 crafts reform. Future research may separate the impact of continuing, new and separated jobs for the increase in wage inequality when assessing the role of unions and works councils for wage inequality.

Conclusions

This paper shows that an exogenous change in outside wage options drives wage differences between comparable employee groups in continuing jobs. The change in outside options was induced by a crafts reform that reduced wages of employees in deregulated crafts occupations in comparison to employees in crafts occupations that remained regulated. The reform effects amount to a wage difference of 0.5 per cent in the five years after the reform or about 5 per cent of the average wage increase during this period. Our results therefore provide empirical evidence for wage bargaining models that postulate an impact of outside wage options on wages in continuing jobs (Postel-Vinay and Robin, 2002; Cahuc et al., 2006; Bagger et al., 2014). In order to make sure that the changes in outside wage options drive the measured reform effects, we reduce the sample to employees who work for the same employer in the industry and commerce sector that was not affected by the crafts reform. Although few employees actually move from the industry and commerce sector to the crafts sector, wage offers in the crafts sector seem to influence the wage structure in the industry and commerce sector. This finding is according to the contestable markets theory (Doeringer, 1986) and findings by Caldwell and Harmon (2019) that outside wage options have a stronger effect on wage changes than employer changes.

We find that reform effects are concentrated within employers with relatively high increases in the wage level for all employees. More specifically, reform effects can be measured for the raw materials and manufacturing sectors, for large employers and employer with collective bargaining and works councils. After controlling for the general wage increase at the employer level, the reform effect and the presence of unions, works councils and employer size disappears. We do not find a clear reform effect pattern for the regional unemployment situation or the profit situation of the employer. Wage differences between employee groups therefore seem to be mainly a result of wage negotiations initiated by employees with an improved outside wage option. Given strong downward wage rigidity in Germany, employers seem not to trigger wage renegotiations when outside options decline for selected employee groups. We therefore argue that the basis of wage differentiation is the wiggle room only employers with increasing wage levels have to retain a sub-group of their employees with better outside wage options.

Several relevant papers on the impact of employer competition on wage differentiation include regional labour markets and commuting costs (Beaudry et al., 2012; Caldwell and Harmon, 2019; Hafner, 2020). Most products and services offered by employees in crafts occupations are only locally distributed. We therefore 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. It would therefore be an interesting extension of the present analysis to look at the regional dimension of the spill-over effects of the change in competition on the regional labour markets between businesses in the crafts sector on wages for employees working in crafts occupations in the industry and commerce sector.

References

Addison, J., Teixeira, P., and Zwick, T. (2010) German Works Councils and the Anatomy of Wages, *Industrial and Labor Relations Review* 63(2), 247-270.

Arai, M. (2003), Wages, profits, and capital intensity: evidence from matched worker–firm data, *Journal of Labor Economics*, 21(3), 593–618.

Bagger, J., Fontaine, F., Postel-Vinay, F. and Robin, J.-M. (2014), Tenure, Experience, Human Capital, and Wages: A Tractable Equilibrium Search Model of Wage Dynamics, *American Economic Review,* 104(6), 1551-1596.

Bartolucci, C. (2012), Credible Threats in a Wage Bargaining Model with on-the-job Search, *Economic Letters,* 117(3), 657-659.

Bauer, T., Bonin, H., Götte, L. and Sunde, U. (2007), Real and Nominal Wage Rigidities and the Rate of Inflation: Evidence from West-German Micro Data, *Economic Journal* 117, F508-F529.

Beaudry, P. and DiNardo, P. (1991), The Effect of Implicit Contracts on the Movement of Wages Over the Business Cycle: Evidence from Micro Data. *Journal of Political Economy* 99(4), 665–688

Beaudry, P., Green, D. and Sand, B. (2012), Does Industrial Composition Matter for Wages? A Test of Search and Bargaining Theory, *Econometrica* 80(3), 1063-1104.

Binmore, K., Rubinstein, A. & Wolinsky, A. (1986), The Nash Bargaining Solution in Economic Modelling, *RAND Journal of Economics* 17(2), 176-188.

Blanchflower, D. and Oswald, A. (1994), The Wage Curve, MIT Press.

Boeri, T. & Jimeno, J. (2005), The Effects of Employment Protection: Learning from Variable Enforcement, *European Economic Review* 49(8), 2057-2077.

Brenzel, H., Gartner, H. & Schnabel, C. (2014), Wage Bargaining or Wage Posting? Evidence from the Employers´ Side, *Labour Economics,* 29(C), 41-48.

Branstetter, L., Lima, F., Taylor, L. & Venancio, A (2014), Do Entry Regulations Deter Entrepreneurship and Job Creation? Evidence from Recent Reforms in Portugal, *The Economic Journal,* 124(577), 805-832.

Burdett, K., and Mortensen, D. (1998) Wage Differentials, Employer Size, and Unemployment, *International Economic Review* 39, 257-273.

Cahuc, P., Postel-Vinay, F. and Robin, J-M. (2006) Wage Bargaining with On-the-Job Search: Theory and Evidence, *Econometrica* 74(2), 323-364.

Cahuc, P., Marque, F. and Wasmer, E. (2008), A Theory of Wages and Labor Demand with Intra-Firm Bargaining and Matching Frictions, *International Economic Review* 49(3), 943-972.

Caju, du P., Fuss, C. and Wintr, L. (2012), "Sectoral Differences in Downward Real Wage Rigidity: Workforce Composition, Institutions, Technology and Competition", *Zeitschrift für ArbeitsmarktForschung* 45(1), 7-22.

Caldwell, S. and Danieli, O. (2018), Outside Options in the Labor Market, mimeo, Harvard University.

Caldwell, S. and Harmon, N. (2019), Outside Options, Bargaining, and Wages: Evidence from Coworker Networks, mimeo, MIT.

Card, D., Lemieux, T. and Riddell, W. (2004), Unions and Wage Inequality, *Journal of Labor Research* 25, 519-559.

Card, D. Heining, J. and Kline, P. (2013), Workplace Heterogeneity and the Rise of West German Wage Inequality, *Quarterly Journal of Economics* 128(3), 967-1015.

Damelang, A., Haupt, A. & Abraham, M. (2018), Economic Consequences of Occupational Deregulation – Natural Experiment in the German Crafts, *Acta Sociologica* 51(1), 34-49.

Dell´Aringa, C. and Pagani, L. (2007) Collective Bargaining and Wage Dispersion in Europe, *British Journal of Industrial Relations*, 45(1), 29-54.

Dickens, W., L. Goette, E. Groshen, S. Holden, J. Messina, M. Schweitzer, J. Turunen, and M. Ward (2007), How Wages Change: Micro Evidence from the International Wage Flexibility Project, *Journal of Economic Perspectives* 21(2), 195-214

Doeringer, P. (1986) Internal Labor Markets and Noncompeting Groups, *American Economic Review P&P* 76(2), 48-52.

Dustmann, C., Ludsteck, J. & Schönberg, U. (2009) Revisiting the German Wage Structure, *Quarterly Journal of Economics* 124(2), 843-881.

Dustmann, C., Fitzenberger, B., Schönberg, U., and Spitz-Oener, A. (2014) From Sick Man of Europe to Economic Superstar: Germany's Resurgent Economy, *Journal of Economic Perspectives* 28(1):167-188.

Fitzenberger, B., Osikominu, A. and Völter, R. (2006) Imputation Rules to Improve the Education Variable in the IAB Employment Subsample, *Journal of Applied Social Science Studies* 136(3), 405-436.

Flinn, C. (2006) Minimum Wage Effects on Labor Market Outcomes under Search, Matching, and Endogenous Contact Rates, *Econometrica,* 74(4), 1013-1062.

Flinn, C. and Mabli, J. (2009) On-the-Job Search, Minimum Wages, and Labor Market Outcomes in an Equilibrium Bargaining Framework, New York University, mimeo.

Fortin, N. and Lemieux, T. (*1997)* Institutional Changes and Rising Wage Inequality: Is There a Linkage? *Journal of Economic Perspectives,* 11 (2): 75-96.

Fredriksen, K (2018) Does Occupational Licensing Impact Incomes? The German Crafts Case, ifh Working Paper 11, Göttingen.

Gathmann, C. and Lembcke, F. (2019) From Occupational Licensing to Certification: Labor Market Effects of Liberalization, University of Heidelberg Working Paper, Heidelberg.

Glitz, A. (2012) The Labor Market Impact of Immigration: A Quasi-Experiment Exploiting Immigrant Location Rules in Germany, *Journal of Labor Economics* 30(1), 175-213.

Goldschmidt, D. and Schmieder, J. (2017) The Rise of Domestic Outsourcing and the Evolution of the German Wage Structure, *Quarterly Journal of Economics* 132(3), 1165-1217.

Gosling, A. and Machin, S. (1994) Trade Unions and the Dispersion of Earnings in British Establishments, 1980-90, *Oxford Bulletin of Economics and Statistics*, 57, 167-184.

Guiso, L., Pistaferri, L. and Schivardi, F. (2005) Insurance within the Firm, *Journal of Political Economy* 113(5), 1054-1087.

Gürtzgen, N. (2009) Rent-sharing and collective bargaining coverage: evidence from linked employer–employee data, *Scandinavian Journal of Economics* 111(2), 323–349.

Gürtzgen, N. (2014) Wage Insurance within German Firms: Do Institutions Matter? *Journal of the Royal Statistical Society, Series A* 177(2), 345-369.

Hafner, F. (2020) Labor Market Competition, Wages and Worker Mobility, mimeo Universitat Pompeu Fabra, Barcelona.

Hainmueller, J. (2012) Entropy Balancing for Causal Effects: A Multivariate Reweighting Method to Produce Balanced Samples in Observational Studies, *Political Analysis* 20(1), 25-46.

Hainmueller, J. and Xu, Y. (2013) ebalance: A Stata Package for Entropy Balancing, *Journal of Statistical Software* 54(7), 1-18.

Hall, R. and Krueger, A. (2010) Evidence on the Determinants of the Choice between Wage Posting and Wage Bargaining, NBER Working Paper 16033, Cambridge, MA.

Hall, R. and Milgrom, P. (2008) The Limited Influence of Unemployment on the Wage Bargain, *American Economic Review* 98(4), 1653-1674.

Haverkamp, K. and Gelzer, A. (2016) Verbleib und Abwanderung aus dem Handwerk – die Arbeitsmarktmobilität von handwerklichen Nachwuchskräften, Göttinger Beiträge zur Handwerksforschung 10, Göttingen.

Heckman, J. (2004) U.S. Education and Training Policy: A Reevaluation of the Underlying Assumptions Behind the "New Consensus", in: Henry, M. (ed.): Race, Poverty, and Domestic Policy, Yale University Press, New Haven, 367-404.

Hirsch, B. and Müller, S. (2020) Firm Wage Premia, Industrial Relations, and Rent Sharing in Germany, *Industrial and Labor Relations Review* 73(5), 1119-1146.

Hirsch, B. and Zwick, T (2015) How Selective are Real Wage Cuts? A Micro-analysis Using Linked Employer-Employee Data, *LABOUR* 29(4), 327-347.

Jacobebbinghaus, P. (2008), LIAB-Datenhandbuch, Version 3.0., *FDZ Datenreport 03/2008,* Nuremberg.

Jäger, S., Schoefer, B., Young, S. and Zweimüller, J. (2020), Wages and the Value of Nonemployment, *Quarterly Journal of Economics*, forthcoming, https://doi.org/10.1093/qje/qjaa016.

Kleiner, M. and Krueger, A. (2013), Analyzing the Extent and Influence of Occupational Licensing on the Labor Market, *Journal of Labor Economics,* 31(2), 173-202.

Kleiner, M. (2006) Licensing Occupations: Ensuring Quality or Restricting Competition? Upjohn Institute for Employment Research, Kalamazoo, MI.

Koch, A. and Nielen, S. (2017) Ökonomische Wirkungen der Handwerksnovelle 2004: Ergebnisse einer Kontrollgruppenanalyse, *Perspektiven der Wirtschaftspolitik,* 18(1), 72-85.

Lachowska, M. (2017) Outside Options and Wages: What can we Learn from Subjective Assessments? *Empirical Economics,* 52(1), 79-121.

Lallemand, T. and Rycx, F. (2006) Establishment Size and the Dispersion of Wages, *Applied Economics Quarterly*, 52, 309-336.

Lergetporer, P., Ruhose, J. and Simon, L. (2018) Entry Barriers and the Labor Market Outcomes of Incumbent Workers: Evidence from a Deregulation Reform in the German Crafts Sector, IZA Discussion Paper 11857, Bonn.

Ljungqvist, L. and Sargent, T. (2017) The Fundamental Surplus, *American Economic Review* 107(9), 2630–2665.

Manning, A. (2003) Monopsony in Motion, Princeton: Princeton University Press.

Michelacci, C. and Suarez, J. (2006) Incomplete Wage Posting, *Journal of Political Economy* 114(6), 1098-1123.

Mortensen, D. and Pissarides, C. (1999) New Developments in Models of Search in the Labor Market, in: Ashenfelter, O. and Card, D. (eds.) Handbook of Labor Economics vol. 3, chapter 39, 2567-2627.

Müller, K. (2016) Economic Effects of Deregulation – Using the Example of the Revised Trade and Crafts Code 2004, ifh Working Papers 5, Göttingen.

Müller, K. (2018) Neue Daten zu den Auswirkungen der Teilderegulierung des Handwerks 2004, Göttinger Beiträge zur Handwerksforschung 19, Göttingen.

Oi, W. and Idson, T. (1999) Firm size and wages, *in* O.C. Ashenfelter and D.E. Card (eds.), *Handbook of Labor Economics*, vol. 3B, pp. 2165–2214, Amsterdam: Elsevier.

Peoples, J. (1998) Deregulation and the Labor Market, *Journal of Economic Perspectives* 12(3), 111-130.

Pissarides, C. (2000) Equilibrium Unemployment Theory (2nd ed.), MIT Press, Cambridge.

Postel-Vinay, F. and Robin, J.-M. (2002) Equilibrium Wage Dispersion with Worker and Employer Heterogeneity, *Econometrica* 70(6), 2295-2350.

Rinne, U. and Zimmermann, K. (2012) Another Economic Miracle? The German Labor Market and the Great Recession, *IZA Journal of Labor Policy* 1(3), 1-21.

Rostam-Afschar, D. (2014) Entry Regulation and Entrepreneurship: A Natural Experiment in German Craftsmanship, *Empirical Economics* 47, 1067-1107.

Rottenberg, S. (1980) Occupational Licensure and Regulation, American Enterprise Institute for Public Policy Research, Washington.

Rubin, D. (1977) Assignment to Treatment Group on the Basis of a Covariate', *Journal of Educational and Behavioral Statistics* 2(1), 1-26.

Runst, P., Fredriksen, K., Proeger, T., Haverkamp, K., and Thomä, J. (2018) Handwerksordnung: ökonomische Effekte der Deregulierung von 2004, *Wirtschaftsdienst* 98(5): 365-371.

Runst, P. Thomä, J, Haverkamp, K., Müller, K. (2019) A Replication of "Entry Regulation and Entrepreneurship: A Natural Experiment in German Craftsmanship", *Empirical Economics* 56(6), 2225-2252.

Shimer, R. (2006) On-the-job Search and Strategic Bargaining, *European Economic Review* 50, 811-830.

Stettes, O. (2009) Wage Flexibility and Collective Bargaining – the Case of Germany, European Working Conditions Observatory, Eurofound, Dublin.

Soskice, D. (1994) Reconciling Markets and Institutions: The German Apprenticeship System, in L. Lynch (ed.), Training and the Private Sector: International Comparisons, University of Chicago Press, Chicago.

Tables

Variables	Deregulated		Regulated		Mean difference	
	Mean	Std. Err.	Mean	Std. Err.	Δ	Sig.
	I	II	III	IV	V	VI
Log daily gross earnings 1999	4.507	0.001	4.680	0.001	-0.173	***
Log daily gross earnings 2000	4.527	0.001	4.696	0.001	-0.169	***
Log daily gross earnings 2001	4.525	0.001	4.699	0.002	-0.175	***
Log daily gross earnings 2002	4.538	0.001	4.720	0.001	-0.181	***
Log daily gross earnings 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 degree: unskilled	0.357	0.002	0.164	0.001	0.193	***
Professional degree: skilled	0.568	0.002	0.424	0.001	0.144	***
Professional degree: tertiary vocational	0.019	0.001	0.034	0.001	-0.015	***
Professional degree: white collar	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	***
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 earnings 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	***
Earnings change 1999-2003	0.100	0.001	0.118	0.000	0.018	***

Earnings change 2000-2003	0.065	0.000	0.088	0.000	0.022	***
Earnings change 2001-2003	0.048	0.000	0.065	0.000	0.017	***
Earnings change 2002-2003	0.021	0.000	0.030	0.000	0.009	***
Number of individuals		6,532		15,461		

Tab. 1. Covariate Balancing before Matching, values for 1999-2003.

Notes: Significance levels (for this and all following tables): ***<1%, **<5%, *<10%. Source: LIAB9310, own calculations.

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

Tab. 2. Treatment Effects.

Notes: Nominal earnings, individual fixed effects, standard errors clustered at the individual level in parentheses. Source: LIAB9310, own calculations.

	Log daily earnings
	Matched
Deregulated * Year 1999	0.001
	(0.004)
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***
	(0.003)
Adj. R^2	0.89
Observations	187,801
Individuals	21,993

Tab. 3. Yearly treatment effects.
Notes: Nominal earnings, individual fixed effects, year dummies and constant included, standard errors clustered at the individual level in parentheses. Source: LIAB9310, own calculations.

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

Tab. 4. Treatment effects with establishment fixed effects, larger sample with all employees working in 2003, and log daily real earnings.
Notes: Nominal earnings in columns 1 and 2, entropy balancing is rerun on each subsample, establishment fixed effects in first column and individual fixed effects in second and third columns, year dummies and constant included, standard errors clustered at the establishment level in parentheses (first column) and individual level (second and third columns). Source: LIAB9310, own calculations.

	Log daily earnings (matched)		
	Employer size <250	Employer size 250-999	Employer size >999
Deregulated X After reform	0.002	-0.006*	-0.006***
	(0.004)	(0.002)	(0.002)
Adj. R²	0.87	0.89	0.70
Observations	32,295	58,3508	97,165
Individuals	3,915	6,777	11,301

Tab. 5. Treatment effects by employer size.
Notes: Nominal earnings, entropy balancing is rerun on each sample, individual fixed effects, year dummies and year dummies and constant included, standard errors clustered at individual level in parentheses. Source: LIAB9310, own calculations.

	Log daily earnings (matched)		
	Collective wage agreement, sector level	Collective wage agreement, company level	No collective wage agreement
Deregulated X After reform	-0.004**	-0.010**	-0.003
	(0.002)	(0.005)	(0.003)
Adj. R²	0.86	0.88	0.91
Observations	145,023	21,506	18,347
Individuals	13,171	5,057	3,765

Tab. 6. Treatment effects by collective bargaining status.
Notes: Nominal earnings, entropy balancing is rerun on each sample, individual fixed effects, year dummies and constant included, standard errors clustered at individual level in parentheses. Source: LIAB9310, own calculations.

	Log daily real earnings (matched)	
	Works council	No works council
Deregulated X After reform	-0.006***	0.002
	(0.001	(0.008)
Adj. R²	0.86	0.94
Observations	174,939	12,862
Individuals	18,371	3,622

Tab. 7. Treatment effects by works council status.
Notes: Nominal earnings, entropy balancing is rerun on each sample, individual fixed effects, year dummies and constant included, standard errors clustered at individual level in parentheses. Source: LIAB9310, own calculations.

	Log daily earnings (matched)		
	Profit situation good	Profit situation acceptable	Profit situation bad
Deregulated X After reform	-0.006***	-0.002	-0.007**
	(0.002)	(0.004)	(0.004)
Adj. R^2	0.92	0.91	0.83
Observations	67,593	77,725	31,853
Individuals	15,664	18,955	9,837

Tab. 8. Treatment effects by profit situation.
Notes: Nominal earnings, entropy balancing is rerun on each sample, individual fixed effects, year dummies and constant included, standard errors clustered at individual level in parentheses. Source: LIAB9310, own calculations.

	Log daily earnings (matched)		
	Raw materials and manufacturing	Mechanical engineering	Construction, trade and services
Deregulated X After reform	-0.009***	-0.001	0.003
	(0.003)	(0.003)	(0.009)
Adj. R^2	0.90	0.85	0.91
Observations	62,074	107,237	18,490
Individuals	7,179	12,590	2,224

Tab. 9. Treatment effects by economic sector.
Notes: Nominal earnings, entropy balancing is rerun on each sample, individual fixed effects, year dummies and constant included, standard errors clustered at individual level in parentheses. Source: LIAB9310, own calculations.

	Log daily earnings (matched)			
	1st quartile regional unemployment	2nd median regional unemployment	3rd quartile regional unemployment	4th quartile regional unemployment
Deregulated X After reform	-0.005	-0.005*	-0.009***	-0.003
	(0.005)	(0.003)	(0.003)	(0.005)
Adj. R^2	0.79	0.80	0.93	0.86
Observations	44,580	44,977	50,559	50,078
Individuals	8,393	10,773	10,446	9,274

Tab. 10. Treatment Effects by regional unemployment.
Notes: Nominal earnings, entropy balancing is rerun on each sample, individual fixed effects, year dummies and constant included, standard errors clustered at individual level in parentheses. Source: LIAB9310, own calculations.

	Log daily earnings (matched)				
	Establishment median wage increase				
	1st decile	2nd decile	3rd decile	4th decile	5th decile
Deregulated X After reform	0.004	-0.001	-0.001	0.001	0.001
	(0.004)	(0.002)	(0.005)	(0.006)	(0.005)
Adj. R^2	0.92	0.80	0.68	0.90	0.91
Observations	12,536	14,762	11,3016	16,294	11,617
Individuals	1,470	1,539	1,283	1,815	1,208

Tab. 11a. Treatment Effects by establishment median wage increases after reform.
Notes: Nominal earnings, entropy balancing is rerun on each sample, individual fixed effects, year dummies and constant included, standard errors clustered at individual level in parentheses. Source: LIAB9310, own calculations.

	Log daily earnings (matched)				
	Establishment median wage increase				
	6th decile	7th decile	8th decile	9th decile	10th decile
Deregulated X After reform	-0.001	-0.004**	-0.009***	-0.017***	-0.011**
	(0.006)	(0.002)	(0.003)	(0.006)	(0.006)
Adj. R^2	0.87	0.89	0.85	0.82	0.70
Observations	17,894	15,842	17,394	16,783	13,138
Individuals	1,935	1,635	1,494	1,498	1,307

Tab. 11b. Treatment Effects by establishment median wage increases after reform.
Notes: Nominal earnings, entropy balancing is rerun on each sample, individual fixed effects, year dummies and constant included, standard errors clustered at individual level in parentheses. Source: LIAB9310, own calculations.

	Log daily earnings (matched)	
Deregulated X After reform	-0.005***	(0.001)
Deregulated X After reform X Employer size <250	0.003	(0.002)
Deregulated X After reform X Employer size 250 – 999	0.002	(0.002)
Deregulated X After reform X Collective wage bargaining on sector level	0.002	(0.003)
Deregulated X After reform X Collective wage bargaining on company level	-0.001	(0.003)
Deregulated X After reform X Works council	-0.003	(0.002)
Deregulated X After reform X Profit situation good	0.000	(0.001)
Deregulated X After reform X Profit situation acceptable	0.004***	(0.002)
Deregulated X After reform X 1st quartile regional unemployment	-0.001	(0.002)
Deregulated X After reform X 2nd quartile regional unemployment	-0.002	(0.002)
Deregulated X After reform X 3rd quartile regional unemployment	-0.010***	(0.003)
Deregulated X After reform X 1st decile establishment median wage increase	0.006***	(0.000)
Deregulated X After reform X 2nd decile wage increase	0.006***	(0.000)
Deregulated X After reform X 3rd decile wage increase	0.002**	(0.000)
Deregulated X After reform X 4th decile wage increase	0.002***	(0.000)
Deregulated X After reform X 5th decile wage increase	0.002***	(0.000)
Deregulated X After reform X 6th decile wage increase	0.001**	(0.000)
Deregulated X After reform X 7th decile wage increase	0.000	(0.000)
Deregulated X After reform X 8th decile wage increase	-0.006***	(0.001)
Deregulated X After reform X 9th decile wage increas	-0.005***	(0.000)
Raw materials and manufacturing	-0.011*	(0.006)
Mechanical engineering	-0.000	(0.006)
Adj. R^2	0.88	
Observations	187,801	
Individuals	21,993	

Tab. 12 Treatment Effects with interaction terms.

Notes: Nominal earnings, 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 individual level in parentheses. Source: LIAB9310, own calculations.

Appendix

Crafts and Trade Code (Annex A)	Name	Classification of Occupation Code (KldB88)
1	Bricklayer and Concretor	441, 442
2	Stove and Air Heating Mechanic	484
3	Carpenter	451
4	Roofer	452
5	Road Construction Worker	462
6	Thermal and Acoustic Insulation Fitter	482
7	Well Sinker	465*
8	Stonemason	101
9	Plasterer	481
10	Painter and Lacquerer	511,512
11	Scaffolder	453
12	Chimney Sweeper	804
13	Metal Worker	301
14	Surgical Instrument Maker	285*
15	Coachbuilder	285*
16	Precision Engineer	284
17	Motorbike and Bicycle Mechanic	285*
18	Refrigeration Mechanic	285*
19	Communication Technician	628*
20	Automotive Mechatronics Technician	281, 621
21	Mechanic for Agricultural and Construction Machinery	621
22	Gunsmith	211*
23	Plumber	211*
24	Installer and Heating Fitter	261, 262*
25	Electrical Engineering Technician	311, 622*
26	Electrical Machine Engineer	314*
27	Joiner	501
28	Boat Builder	275*
29	Rope Maker	332*
30	Baker	391
31	Pastry-cook	392
32	Butcher	401
33	Dispensing Optician	304
34	Hearing Aid Acoustician	/*
35	Orthotic Technician	628*
36	Orthopaedic Shoemaker	/*
37	Dental Technician	303
38	Hairdresser	901
39	Glazier	485
40	Glass Blower and Glass Apparatus Maker	132,133,134
41	Mechanic for Tyres and Vulcanization	144*

Tab. A1. List of regulated (licensed) craft occupations (Annex A).
We can identify 39 occupations. Missing are hearing aid acoustician (34) and orthopaedic shoemaker (36), also compare Damelang et al. (2018) and Lergetporer et al. (2018). * denotes the 16 occupations not included in Figures A1 and A2.

Crafts and Trade Code (Annex B1)	Name	Classification of Occupation Code (KldB88)
1	Tile, Slab and Mosaic Layer	483
2	Cast Stone and Terrazzo Maker	112*
3	Screed Layer	486
4	Vessel and Equipment Constructor	252
5	Clockmaker	286
6	Engraver	232*
7	Metal Former	193, 213, 225, 233, 244, 271
8	Galvaniser	234
9	Metal and Bell Founder	202
10	Cutting Tool Mechanic	291*
11	Goldsmith and Silversmith	302
12	Parquet Layer	/*
13	Shutter and Sunshade Mechatronics Technician	627*
14	Model Builder	306
15	Turner (Ivory Carver) and Wooden Toy Maker	183
16	Wood Carver	182
17	Cooper	503*
18	Basket Maker	184
19	Costume Tailor	351
20	Embroiderer	342, 346, 352, 354, 356, 357
21	Milliner	355
22	Sailmaker	362*
23	Furrier	378
24	Shoemaker	374, 375, 376
25	Saddler	374, 375, 376
26	Interior Decorator	491
27	Miller	432*
28	Brewer and Maltster	422
29	Wine Cellarperson	421
30	Textile Cleaner	932
31	Wax Chandler	/*
32	Building Cleaner	934
33	Glass Finisher	135
34	Precision Optician	135*
35	Glass and China Painter	514*

36	Precious Stone Engraver and Cutter	102
37	Photographer	837
38	Bookbinder	163
39	Typesetter and Printer	173, 174
40	Screen Printer	175
41	Flexographer	172*
42	Ceramist	121
43	Organ and Harmonium Maker	305*
44	Piano and Harpsichord Maker	305*
45	Reed an Organ Musical Instrument Maker	305*
46	Violin Maker	305*
47	Bow Maker	305*
48	Metal Wind Instrument Maker	305*
49	Wooden Wind Instrument Maker	305*
50	Gilder	235*
51	Sign and Illuminated Advertisement Maker	834*

Tab. A2. List of deregulated (certified) craft occupations (Annex B1).
We can identify 49 occupations. Missing are parquet layer (12) and wax chandler (31), also compare Damelang et al. (2018) and Lergetporer et al. (2018). * denotes the occupations not included in Figures A1 and A2.

Average wage increases			
Significantly negative treatment effect		No treatment effect	
Establishment size 250-999	Establishment size >1000	Establishment size <250	
0.099 (0.136)	0.114 (0.134)	0.081 (0.141)	
Works Council		No works council	
0.105 (0.137)		0.073 (0.126)	
Collective Bargaining (Sector)	Collective Bargaining (Company)	No Collective Bargaining	
0.109 (0.133)	0.097 (0.130)	0.075 (0.157)	
Raw materials and manufacturing		Mechanical engineering	Construction, trade and services
0.113 (0.137)		0.096 (0.141)	0.068 (0.095)
Profit situation good	Profit situation bad	Profit situation acceptable	
0.111 (0.149)	0.098 (0.119)	0.090 (0.140)	

Tab. A3. Average wage increases on establishment level in period 2004-2008 by employer groups with and without significant treatment effects.
Source: LIAB9310, own calculations.

	Log daily earnings (matched)			
	Employees with completed crafts apprenticeship or master craftsman degree	Establishments with less than 20% regulated and 20% deregulated craft jobs	Establishments with regulated and deregulated craft jobs	One establishment
Deregulated X After reform	-0.006**	-0.004*	-0.003**	-0.002*
	(0.002)	(0.002)	(0.001)	(0.001)
Adj. R^2	0.87	0.83	0.86	0.87
Observations	93,368	108,060	165,155	26,427
Individuals	10,813	14,908	19,878	3,571

Tab. A4. Treatment effects for restricted sample of employees with completed crafts apprenticeship or master craftsman degree, for enterprises with less than 20% regulated and non-regulated craft employees, respectively, establishments with at least one regulated and deregulated craft jobs, and within one employer.

Notes: Notes: Nominal earnings, entropy balancing is rerun on each sample, individual fixed effects, year dummies and constant included, standard errors clustered at individual level in parentheses. Source: LIAB9310, own calculations.

	Log daily earnings (matched)	
	"Reform" in 2001, period 1999-2003	"Reform" in 2006, period 2004-2008
Deregulated X after "Reform"	0.000	0.000
	(0.002)	(0.001)
Adj. R^2	0.89	0.90
Observations	108,103	99,698
Individuals	21,931	19,465

Tab. A5. Treatment "effects" in pseudo treatments.

Notes: Nominal earnings, entropy balancing is rerun on each sample, individual fixed effects, year dummies and constant included, standard errors clustered at individual level in parentheses. Source: LIAB9310, own calculations.

Year	Median income	Number of employees	Profit situation
Share deregulated occupations*after reform	0.070	-0.078	0.105
	(0.101)	(0.127)	(0.427)
Share regulated occupations*after reform	-0.068	0.047	.0.281
	(0.117)	(0.143)	(0.488)
Share deregulated occupations	-0.212***	-0.266***	0.562*
	(0.078)	(0.095)	(0.317))
Share regulated occupations	-0.026	-0.078	0.315
	(0.061)	(0.127)	(0.251)
Median age	0.003	-0.009***	-0.028***
	(0.002)	(0.002)	(0.008)
Raw materials and manufacturing	-0.065***	0.029	-0.007
	(0.021)	(0.025)	(0.088)
Mechanical engineering	0.013	0.121***	-0.130
	(0.022)	(0.026)	(0.093)
Collective bargaining	0.128***	0.111***	0.064
	(0.021)	(0.025)	(0.086)
Works council	0.137***	0.183***	0.086
	(0.022)	(0.026)	(0.093)
Employer size <250	-0.190***		0.070
	(0.030)		(0.131)
Employer size 250-1000	-0.101***		-0.036
	(0.002)		(0.128)
Regional unemployment	-0.018***	-0.005**	-0.025***
	(0.002)	(0.002)	(0.007
Year 2000	-0.001	-0.085	-0.302
	(0.047)	(0.058)	(0.199)
Year 2001	0.083*	-0.065	-0.224
	(0.044)	(0.054)	(0.183)
Year 2002	0.039	-0.073	0.209
	(0.042)	(0.052)	(0.179)
Year 2003	0.076*	-0.035	0.321*
	(0.041)	(0.051))	(0.173)
Year 2004	0.136***	-0.124**	0.274
	(0.046)	(0.056))	(0.193)
Year 2005	0.155***	-0.010	-0.043
	(0.046)	(0.056)	(0.196)
Year 2006	0.178***	-0.110*	-0.106
	(0.048)	(0.059)	(0.207)
Year 2007	0.160***	-0.086	-0.217
	(0.050)	(0.061)	(0.216)
Year 2008	0.160***	-0.032	-0.719***
	(0.055)	(0.067)	(0.233)
Adj. R^2	0.35	0.30	0.08
N	1,196	1,196	1,104

Tab. A6. Association between share of employees in deregulated and regulated crafts occupations with median employee income, number of employees and profit situation.

Notes: Reference values for employer size more than 1000 employees, year 1999, economic sectors construction, trade and services. Source: LIAB9310, own calculations.

Number of employees in deregulated (blue) and regulated (red) craft occupations (2003=100)
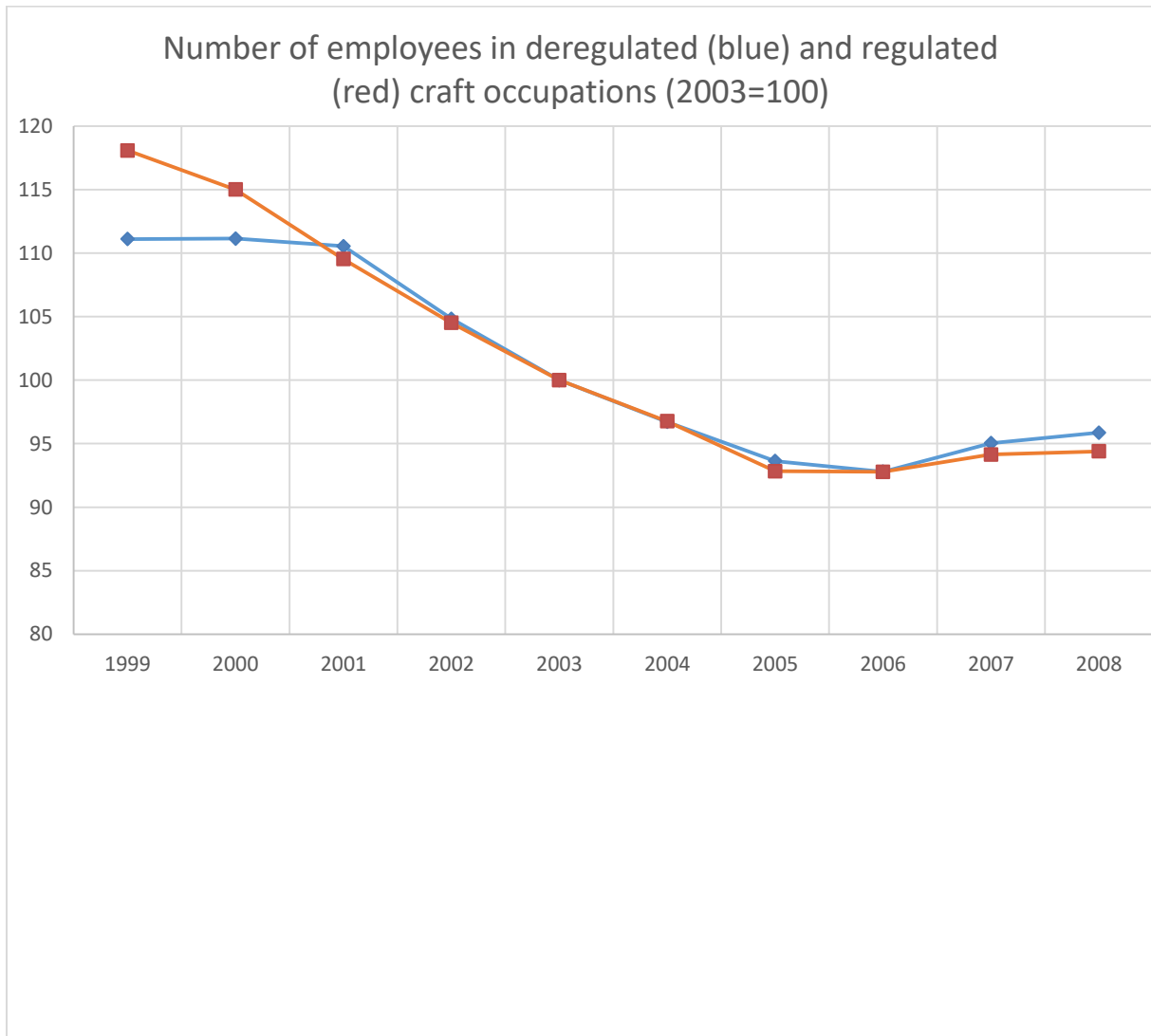
Fig. A1: Employment in deregulated and regulated crafts occupations 1999-2008.
Values normalized to 100 in year 2003, own calculations. Data source: Berufe im Spiegel der Statistik
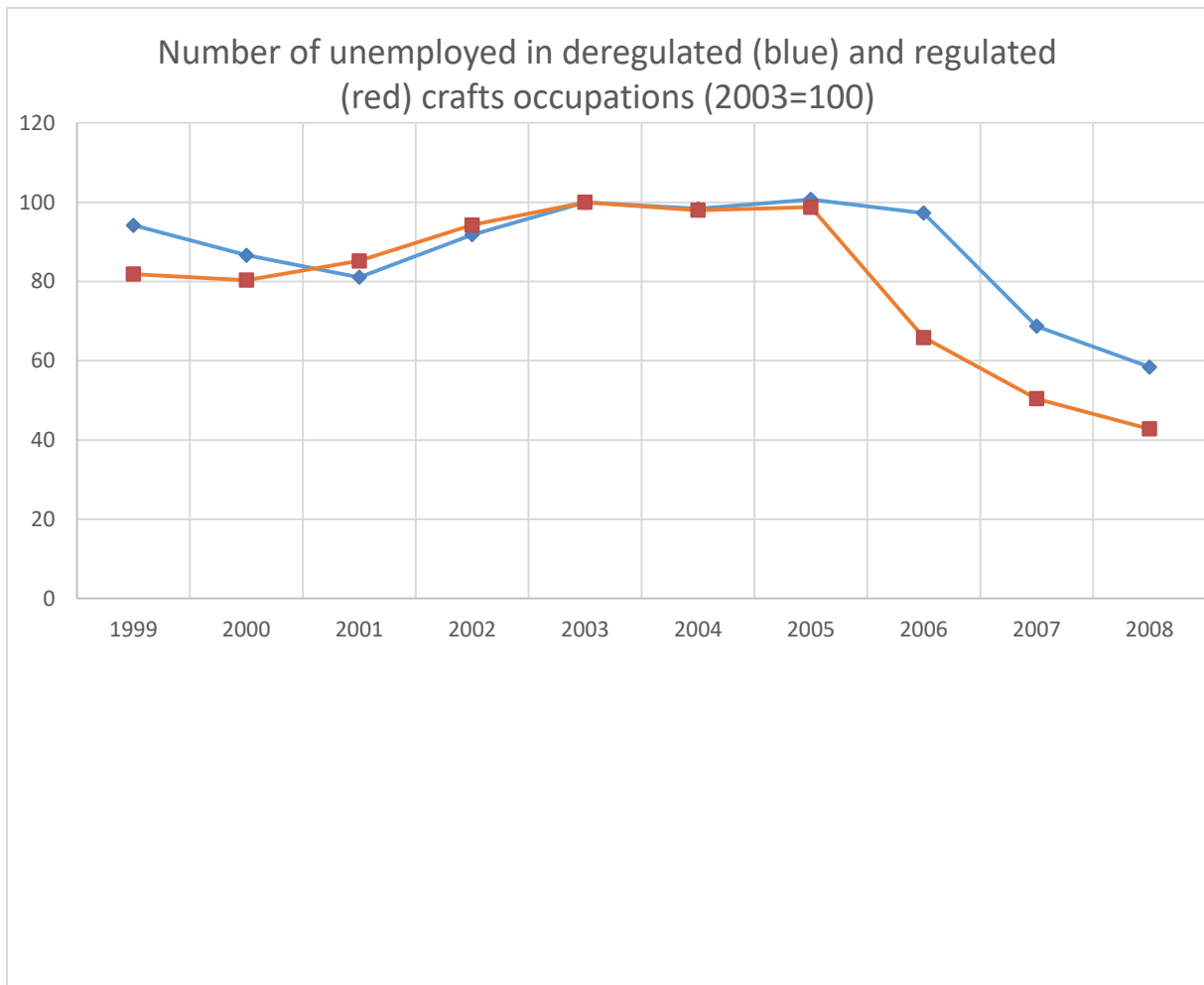(occupations mirrored by statistics) (http://bisds.infosys.iab.de/)
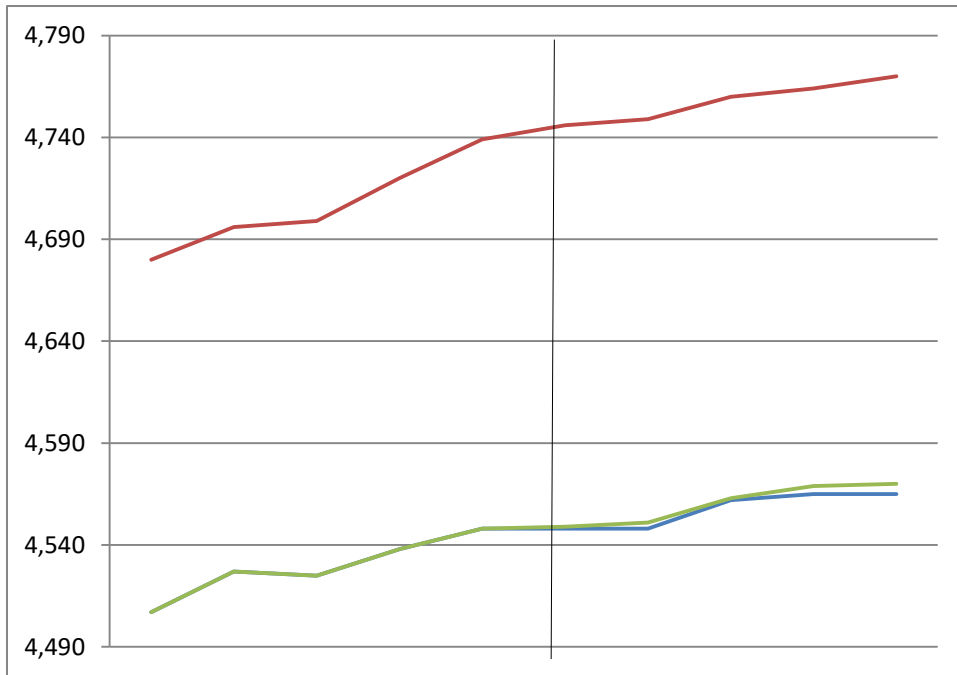
Fig. A2: Unemployment in deregulated and regulated crafts occupations.
Values normalized to 100 in year 2003 own calculations. Data source: Berufe im Spiegel der Statistik (occupations mirrored by statistics) (http://bisds.infosys.iab.de/)

Fig 1: Log earnings before and after the reform, 1999-2008
Notes: Average gross daily earnings for employees in regulated and deregulated crafts included in our sample. Unmatched earnings in regulated crafts are in red, matched earnings in green, and earnings in deregulated crafts are in blue. The horizontal line indicates the reform year 2003.

//

IMPRINT