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ABSTRACT

Entry Barriers and the Labor Market Outcomes of Incumbent Workers: Evidence from a Deregulation Reform in the German Crafts Sector*

We study the labor market outcomes of a deregulation reform in Germany that removed licensing requirements to become self-employed in some occupations. Using longitudinal social security data, we implement a matched difference-in-differences design with entropy balancing to account for observable characteristics and unobserved individual heterogeneity. The reform tripled the number of businesses within ten years and led to slower earnings growth and higher unemployment for incumbent workers in deregulated occupations. However, the reform effect seems rather small, which we attribute to the relatively low competitiveness of new businesses. Supporting this view, the reform did not lead to overall employment growth.

JEL Classification: J31, J24, L11

Keywords: deregulation, entry barriers, self-employment, labor market outcomes, entropy balancing, matched difference-in-differences

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1 Introduction

Regulations make opening a business an expensive and time-consuming endeavor in many countries. Potential entrepreneurs incur costs that range from the time needed to do the paperwork to the effort of acquiring occupational licenses required to run a business (Djankov, 2009; Ciccone and Papaioannou, 2007; Kleiner, 2000). Entry barriers are often justified based on the presumption that they secure quality standards of products and services by discouraging the entry of low-quality firms (Arruñada, 2007). However, many entry barriers serve primarily the interests of incumbent firms to create rents for themselves (Stigler, 1971), and they often share these rents with their workers (e.g., Card et al., 2014; Blanchflower et al., 1996; Hildreth and Oswald, 1997). Because incumbent firms with market power also find it optimal to restrict output and therefore also employment,[1] governments around the world have started to deregulate entry barriers to increase the efficiency of the economy (Djankov, 2009). While numerous papers show that reducing entry barriers triggers entrepreneurial activity, innovation, employment, and productivity, most of them are based on country-level, region-level, or firm-level (panel) data.[2] Much less is known about the labor market consequences of reducing entry barriers for incumbent workers, requiring longitudinal worker-level data to track individuals over time.

In this paper, we use longitudinal administrative social security data to study the labor market consequences of a deregulation reform for incumbent workers in Germany. In 2004, the German government passed a law that reformed the German Trade and Crafts Code *(Handwerksordnung)* to foster entrepreneurial activity and to reverse the negative employment trend in craft occupations. The law removed the requirement to hold the educational certificate of a master craftsman *(Meisterbrief)* to establish a business in 52 out of 93 craft occupations (see Appendix Table A-1 for a full list of craft occupations). The reform was very successful in increasing the number of businesses in deregulated occupations: two years after the reform, the number of businesses had doubled, and it had tripled after ten years (see also, e.g., Rostam-Afschar, 2014; Runst et al., 2018b).[3] Occupations remained regulated if the specific trade was hazardous and quality controls were needed to prevent dangers to health or life of third parties, or if apprenticeship education activity in the trade was high. Deregulated occupations include costume tailors, weavers, tile layers, and millers, while occupations such as plumbers, electrical technicians, hairdressers, and butchers remained regulated. Overall, the reform affected a large part of the German economy because the crafts sector makes up 14% of

[1] See, e.g., Bertrand and Kramarz (2002); Blanchard and Giavazzi (2003); Felbermayr and Prat (2011).

[2] Djankov (2009) provides an overview. Section 3 presents a summary of the literature.

[3] There is a small, but growing literature on the effects of the reform of the German Trade and Crafts Code on various outcomes (see Runst et al. (2018a) for an overview). Most of the studies are concerned with the number of firms entering the market after the reform (Rostam-Afschar, 2014; Koch and Nielen, 2016; Müller, 2014, 2016; Runst et al., 2018b; Zwiener, 2017).

social security employment, constitutes 16% of all businesses, and generates 10% of all revenue in the economy (Federal Statistical Office, 2016).

Our main analysis compares labor market outcomes of the same incumbent worker before and after the reform in deregulated (treatment group) and regulated (comparison group) occupations using a matched difference-in-differences approach (Heckman et al., 1997, 1998; Todd, 2008). Compared to workers in regulated occupations, those in deregulated occupations are, among other things, more likely to be female, foreign-born, older, less educated, have lower average earnings, and more likely to be employed in manufacturing. Therefore, we use entropy balancing (Hainmueller, 2012) to construct weights for each worker in the comparison group such that the average characteristics of the comparison group match *exactly* the average characteristics of the treatment group before the treatment. Compared to conventional matching, entropy balancing has the advantage that it does not take the detour via estimating a propensity score to construct a comparison group but instead implements a nonparametric matching algorithm that reweights the comparison group observations such that they satisfy prespecified balancing requirements. The procedure accounts for observable differences between the groups, and the regression adjustment eliminates remaining unobserved individual heterogeneity.

We find that the daily gross earnings of incumbent workers in deregulated occupations grew significantly less than those of workers in regulated occupations after the reform. Over the period from 2004 to 2014, workers in deregulated occupations experienced a negative average effect on their earnings of approximately 2.3% (4% when using the nonmatched comparison group) relative to workers in regulated occupations. Year-specific estimates show that the treatment effect becomes gradually larger over time to -4.3% in 2014 (non-matched sample: -7%). We also find that unemployment among incumbent workers increased by 0.7 percentage points (approximately 20% of the unemployment rate in our analytical sample) more in the deregulated occupations than in the regulated occupations (non-matched sample: one percentage point). Because firms in the crafts sector are usually rather small, we also examine heterogeneity by the workers' employer firm size and find that the effects for earnings and unemployment are largest among firms with fewer than 20 employees. The results are robust to a series of identification checks. For example, we find that prereform earning trends are very similar between the treatment and the comparison groups (matched *and* nonmatched), lending credibility to the common trend assumption.[4] We also carefully evaluate the effects of EU enlargement in 2004 and 2007 and the economic crisis in 2009, which both may have affected workers in deregulated and regulated occupations differently.

While the earnings and unemployment effects on incumbent workers are economically relevant, we argue that they are relatively small when factoring in the large increase

[4]This makes it also unlikely that occupations were deregulated due to their pre-reform economic performance.

in the number of new firms. For example, Bruhn (2011) finds that a similar reform in Mexico decreased the income of incumbent businesses by 3% (similar to our effects) despite increasing the number of new firms by only 5% (compared to over 300% in our setting). U.S. studies that examine deregulation reforms in the airline industry (Card, 1986, 1998; Hirsch and Macpherson, 2000) and the trucking industry (Hirsch, 1993; Rose, 1987) document relative wage decreases of up to -10% to -15% for workers in deregulated industries. The comparison suggests that the new firms in our study that emerged after the reform in 2004 did not generate strong competitive pressure. Nevertheless, the large increase in the number of firms was sufficient to cause a slow adjustment in the pay scheme and employment structure of incumbent firms over time. The slow pace of the adjustment process is likely due to the low reform-induced competitive pressure and relatively strong labor market institutions in Germany.

Alternatively, it could be that increased labor demand by incumbent and new firms increased wage rates; this increase would have attenuated the negative reform effects for incumbent workers. However, we find significantly *declining* employment trends (similar to Koch and Nielen, 2016; Zwiener, 2017) and almost constant average wage trends in deregulated occupations compared to regulated occupations. Additional analyses using cross-sectional data from the German microcensus confirm these results for trends in net monthly income for both employed and self-employed craftsmen (self-employed are not included in the social security records) (see also Fredriksen, 2017). The nonpositive employment trends after the reform that are in contrast to theoretical predictions and most of the empirical literature most likely arise because newly established firms remained one-man businesses with very little apprenticeship activity and low survival rates: approximately 60% of newly established firms had already disappeared after five years (Müller, 2014, 2016). We also discuss firm adjustments over price decreases without affecting earnings due to, for example, investments in technology, physical capital, and innovation. However, none of these economic factors seems to have changed significantly after the reform in incumbent firms. The most plausible reasons for these noneffects are that customers do not perceive goods and services offered by firms with and without a master craftsman certificate to be perfect substitutes (Fredriksen et al., 2018). Furthermore, we provide suggestive evidence that former employees who likely become self-employed are not positively selected from the workforce (in terms of earnings in 2003); this outcome may also explain higher failure rates relative to their established and experienced competitors. Overall, the results of this study caution the deregulation of product markets where new competitors would be too weak to exert strong competitive pressure on incumbent firms.

Our study contributes primarily to the literature that examines labor market effects of deregulation policies within countries.[5] Almost all of these studies find that reducing entry

[5]Section 3 provides a detailed literature review.

barriers leads to increasing entrepreneurial activity and overall employment growth.[6] In contrast to our paper, most previous studies use regional- and firm-level variation in the exposure to a deregulation reform and do not exploit worker-level information. We also relate to a large U.S. literature on the effect of reducing entry barriers in specific industries on wages (see Fortin and Lemieux (1997) and Peoples (1998) for overviews). These studies show that industry wages decrease significantly after deregulating market entry. However, the evidence often relies on cross-sectional data from the current population survey (CPS) that, other than our data, does not allow us to control for unobserved worker characteristics.

The remainder of the paper is structured as follows. Section 2 provides the institutional background and explains the reform in detail. Section 3 discusses the theoretical background and the related literature. Section 4 introduces the data. Section 5 presents our main analysis of the reform effects on incumbent workers' labor market outcomes. Section 6 sets our findings in perspective to the overall reform effects on employment and wage growth, and on self-employed labor market outcomes. Section 7 provides a discussion of our findings and connects them with the current literature. Section 8 concludes.

2 Reform of the German Trade and Crafts Code 2004

Crafts occupations such as carpenters and stonemakers have historically played an important role in the German economy and continue to do so to this day (Federal Statistical Office, 2016). Since the middle ages (with short interruptions), craft occupations have been strictly regulated and monitored by guilds (Brenke, 2008). The role of the guilds was to regulate the supply of craftsmen through licenses and apprenticeship training, protect their members through contract enforcement, reduce information asymmetries on quality as well as invest in human capital and technology (Ogilvie, 2004, 2014).

In more recent years and until 2004, the supply of craftsmen had been regulated in two ways in Germany. First, only individuals with a "master craftsman certificate" *(Meisterbrief)* were able to open their own business and offer services in their trade. Second, only master craftsmen were allowed to educate apprentices. The master craftsman certificate is a professional qualification administered by the chambers of crafts and trade in the respective federal states. The certificate requires roughly two years of coursework, with the costs for the examination varying between two to ten thousand Euros (German Confederation of Skilled Crafts, 2014). As prerequisite for the master craftsman certificate

[6] See studies by Aghion et al. (2008), Bertrand and Kramarz (2002), Branstetter et al. (2014), Bruhn (2011), Kaplan et al. (2011), Mullainathan and Schnabl (2010), and Yakovlev and Zhuravskaya (2013).

exams, craftsmen must successfully finish their apprenticeship in their respective craft trade and usually must acquire some work experience as a journeymen.[7]

In 2004, the German government deregulated access to self-employment by removing the requirement to hold a master craftsman certificate for 52 out of 93 craft occupations (see Appendix Table A-1 for a full list of regulated and deregulated craft occupations). The other 41 occupations remained regulated with the exception that journeymen who had worked for at least six years in their occupation (four of them in a leadership position) also received the option to open their own business in 35 occupations.[8] The reform goals for the amendment of the German Trade and Crafts Code were to reverse the negative development in the number of craftsmen and apprenticeships, ease entry into entrepreneurial activity, and ensure future employment. The reform was first announced at Chancellor Gerhard Schröder's government declaration[9] in March 2003 and then quickly came into force on January 1, 2004, less than one year after the initial declaration.

The reform of the German Trade and Crafts Code was a less prominent and less widely known part of a comprehensive welfare and labor relation reform package aimed at enhancing economic growth and increasing competitiveness referred to as "Agenda 2010" or the "Hartz Reforms". Implemented by the coalition government between the Social Democrats and the Green Party, the Hartz reforms were a reaction to a decade of disappointing economic performance in the postreunification area.[10] The Hartz reforms did not affect institutions involved in the wage setting process but were primarily concerned with limiting unemployment benefits, liberalizing temp agency work, reforming active labor market policies, and reorganizing the Federal Labor Agency. While some argue that the reform explains at least some of the improvements in competitiveness of the German industrial sector, others argue that the Hartz reforms were not essential (Dustmann et al., 2014).

Occupations remained regulated for two reasons. First, saftey concerns for customers prevented the deregulation of occupations with tasks that require high quality control. In total, 31 occupations remained regulated for this reason. Second, 11 occupations remained regulated because they had already a high apprenticeship education activity. The second restriction argument was added as a compromise following political negotiations between opposing political parties, representatives of employers, labor unions, and employees. As an overall result of these negotiations, deregulated occupations represent only 11% of

[7]Journeymen get their name from the old custom that after passing their apprenticeship examination, young craftsmen left their master to go on their journey and work in different locations to acquire new knowledge and techniques. In modern times, this custom has mostly ceased to exist.

[8]However, Figure 1 indicates that this weaker deregulation did not affect the number of firms in these occupations.

[9]Gerhard Schröder's Government Declaration: `http://gerhard-schroeder.de/2003/03/14/regierungserklarung-agenda-2010/`

[10]At that time, Germany was "the sick man of Europe" (The Economist, 2004). In 2003, the year before the reform was introduced, the German economy was in recession, with high labor costs and an unemployment rate of 11.6% (Federal Statistical Office, 2018a).

craft businesses in 2003. Further anecdotal evidence suggests that regulated occupations were prominently represented in the constituencies of the governing politicians. This is consistent with the fact that some occupations are very similar to each other, yet one has remained regulated (e.g., metal worker) and the other has been deregulated (e.g., metal and bell founder).

The reform had strong effects on business formation and entrepreneurial activity. Using aggregated firm registry data from the German Confederation of Skilled Crafts *(Zentralverband des Deutschen Handwerks (ZDH))*, Figure 1 displays the changes in the number of businesses in the deregulated occupations and in the regulated occupations (relative to 2003). While the number of businesses in the still regulated occupations remained relatively constant, the number of businesses in deregulated occupations doubled between 2003 and 2006 and more than tripled between 2003 and 2014. On average, the stock of businesses in deregulated occupations increased annually by 26% in the first three years and then increased further by approximately 5% each year from 2007 to 2014.[11]

Using data from the German Microcensus[12], Figure 2a shows the shares of self-employed individuals in both the deregulated and regulated occupations (see Section 4.2 for a description of the data). The picture reveals an increase in the share of self-employed craftsmen in deregulated occupations.[13] As may be expected, another effect of the reform was a drastic and immediate decline in the number of master craftsman certificate exams in the deregulated occupations. As shown in Figure 2b, master craftsman certificate exams decreased by 60% after the reform (see also Koch and Nielen, 2016). Examinations do not fall to zero because they are still offered and considered to have value by signaling high quality standards (Fredriksen et al., 2018).

3 Labor Market Effects of Entry Barriers: Related Literature and Some Theory

Entry barriers are costs that new entrants must incur to become active in a given product market. These costs can range from standardization of products, minimum capital requirements, and time-consuming registration procedures to licenses that are required to run a business (Djankov, 2009; Kleiner, 2000). Advocates for entry barriers argue that

[11]Supporting evidence on the role of entry barriers in the German crafts sector on the likelihood of becoming self-employed comes from Prantl and Spitz-Oener (2009). They use the German reunification as a natural experiment to show that the entry barriers impeded entry into self-employment in East Germany compared to entry in West Germany. Prantl (2012) further shows that entry barriers suppressed short-lived and long-lived entrants.

[12]Research Data Centres of the Federal Statistical Office and the statistical offices of the Länder, Microcensus, census years 1998-2012.

[13]Columns (7) and (8) of Table 8 confirm an increase of more than 2% among self-employed individuals in deregulated compared to regulated occupations after the reform. This finding confirms Rostam-Afschar (2014) and Runst et al. (2018b) who study the effects of the reform on entry into self-employment more rigorously. See also Koch and Nielen (2016); Müller (2014, 2016); Zwiener (2017).

they can secure and improve the quality of the products and services provided (Arruñada, 2007). For example, Anderson et al. (2016) show that occupational licensing in medical professions can be highly beneficial because the licensing of midwives in the early 20th century in the United States led to reductions in infant and mother mortality. However, opponents argue that entry barriers lead to inefficient allocations of resources because they restrict competition and create rents for incumbent firms (Peltzman, 1976; Posner, 1975; Stigler, 1971).[14] With entry barriers in place, theory usually predicts that incumbent firms produce less output and charge higher prices than they would in a more competitive environment.[15]

While the effects of reducing entry barriers on wages and employment are theoretically ambiguous and seem to depend on a multitude of factors, such as the level of initial competition, labor market institutions, and political economy factors as well as the technology and the specific industry in question (see Djankov (2009) for a survey of the literature), standard economic theory predicts that reducing entry barriers for firms lead to increasing entrepreneurial activity through increasing the number of active firms. Evidence in favor of this theory comes from numerous cross-country studies,[16] as well as from studies examining deregulation reforms within countries by using regional and time variation in the reform exposure.[17] For example, Bruhn (2011) and Kaplan et al. (2011) document that a reform in Mexico, which simplified the local registration process for new businesses, has increased the number of new businesses by 5%. Branstetter et al. (2014) report an increase of 17% for a similar reform in Portugal. Aghion et al. (2008) report an average increase in the number of factories by 6% after a large deregulation reform in India; this reform removed licensing requirements to establish a new factory, expand capacity, start a new product line, or change location for entire industries.

Because incumbent firms with market power find it optimal to restrict output and therefore also employment (Bertrand and Kramarz, 2002; Blanchard and Giavazzi, 2003; Felbermayr and Prat, 2011), deregulation and new firm entry should predict increases in employment levels. Most of the literature confirms this link at the country and regional level. For example, (Bertrand and Kramarz, 2002) and Ciccone and Papaioannou (2007) show that excessive entry barriers harm employment growth. Other studies show that stringent anticompetitive product market regulation increases unemployment (Bassanini and Duval, 2006; Feldmann, 2008; Griffith et al., 2007). If incumbent firms and new

[14]De Soto (1989) argues that entry barriers may also benefit bureaucrats and politicians by collecting bribes from entrants. Mukoyama and Popov (2014) provide a political economy model for the existence and dynamics of entry barriers.

[15]See, e.g., Djankov (2009); Gittleman et al. (2018); Kleiner and Krueger (2013); Weeden (2002).

[16]See, e.g., Ciccone and Papaioannou (2007); Dreher and Gassebner (2013); Djankov et al. (2002); Fisman and Sarria Allende (2010); Klapper et al. (2006).

[17]Studies exist for Belgium (Schaumans and Verboven, 2008), France (Bertrand and Kramarz, 2002), Peru (Mullainathan and Schnabl, 2010), Portugal (Branstetter et al., 2014), Mexico (Bruhn, 2011; Kaplan et al., 2011), India (Aghion et al., 2008), and Russia (Yakovlev and Zhuravskaya, 2013).

entrants are effective competitors, meaning that they engage in similar markets (market commonality) and compete for similar resources (resource similarity) (Chen, 1996), then incumbent firms may react to increased competition by increasing investments (Alesina et al., 2005) and innovative activity (Aghion et al., 2015; Griffith et al., 2010) to keep their long-run competitive advantage. While new firm entry can also be detrimental to innovation and growth by diminishing rents and thereby decreasing incentives to innovate and invest (Aghion et al., 2005),[18] evidence for the United Kingdom and cross-country studies show a positive effect of deregulation on innovation (Aghion et al., 2009; Blundell et al., 1999). Thus, it is not surprising that most studies eventually find beneficial effects of deregulation reforms on productivity and economic growth.[19] For consumers, the literature shows that deregulation reforms lead to lower prices in the affected industries and sectors. For example, Bruhn (2011) confirms that prices decrease after deregulation in Mexico. Schivardi and Viviano (2011) study the market effects of barriers to entry in the Italian retail market and find that higher entry barriers lead to higher consumer prices. Bertrand and Kramarz (2002) also confirm this outcome for the French retail industry. Thus, an overwhelming part of the literature shows that there are mostly positive economic effects from removing entry barriers.

In this study, we are mainly interested in understanding potential earnings and employment outcomes of workers in a firm that is affected by a deregulation. In many cases, the main beneficiaries of entry regulations are incumbent firms, which are protected from competition by entry barriers and can therefore raise economic rents through charging markups on prices.[20] There is strong evidence that firms share their economic rents with their employees, causing higher wages in many regulated markets and industries.[21] To protect their product market position, firms may try to save costs following a deregulation reform by revising wages of their workers. They may do that by re-negotiating labor contracts and collective bargaining agreements with labor unions (Peoples, 1998). However, to retain their competitive advantage, firms may also choose to invest in the human capital of their workforce because firms with more skilled labor work more efficiently. Evidence on this channel comes from Fernandes et al. (2014) and Guadalupe (2007) who show that returns to skills increase after a deregulation and from Bassanini and Brunello (2011) who show that firms invest more in the training of their employees. Earnings for incumbent workers may also rise when labor demand increases

[18] Aghion et al. (2009) argue that it depends on the specific industry and its' technological advancement how firms react to increasing competition. Thus, a higher entry threat can encourage innovation in sectors that are initially close to the technological frontier, whereas it may discourage innovation in sectors that are initially far below the technological frontier.

[19] See, for example, (Aghion et al., 2004, 2005, 2008, 2009; Barseghyan, 2008; Dawson and Seater, 2013; Djankov et al., 2006; Schivardi and Viviano, 2011).

[20] See, e.g., Djankov (2009); Gittleman et al. (2018); Kleiner and Krueger (2013); Weeden (2002).

[21] See, for example, Arai and Heyman (2009); Card et al. (2014); Christofides and Oswald (1992); Blanchflower et al. (1996); Guertzgen (2009); Hildreth and Oswald (1997); Rusinek and Rycx (2013).

because of increasing product demand. Furthermore, because incumbent firms may invest more in technology and innovation, they might be able to keep their competitive advantage without reducing the earnings of their workers. Thus, theoretically, whether deregulation increases or decreases wages of incumbent workers is ambiguous. Moreover, some labor markets (e.g., Germany) are characterized by substantial downward rigidity of wages (e.g., through strong labor unions or high minimum wages).[22] If firms cannot easily revise wages, they may choose to adjust their labor costs over the employment margin.

Thus, while the economy-wide effects of reducing entry barriers are likely positive for employment, innovation, and productivity, those policies may harm incumbent workers by decreasing their wages and perhaps increase unemployment. However, in the longer run, incumbent workers may benefit from additional employment opportunities offered by new firms, raising average wages in the labor market due to increasing labor demand. Thus, it is important to distinguish conceptually between long- and short-run welfare effects for incumbent workers because the reduction and redistribution of rents through deregulation may also induce firms to adjust over longer periods (Blanchard and Giavazzi, 2003).

There is a large U.S. literature on the effect of reducing entry barriers in specific industries on earnings (see Fortin and Lemieux (1997) and Peoples (1998) for overviews).[23] Most of these studies show that industry wages decrease significantly after deregulating market entry. For example, workers in the airline industry (Card, 1986, 1998; Hirsch and Macpherson, 2000) and the trucking industry (Hirsch, 1993; Rose, 1987) see their wages decrease by between 10% to 15% after a major entry deregulation. MacDonald and Cavalluzzo (1996) find that wages in the railroad industry first increase, but then decrease substantially—indicating the importance of studying long-run patterns. However, the evidence often relies on cross-sectional data from the current population survey (CPS), which does not allow unobserved worker characteristics to be controlled. Moreover, because reform effects depend on the predetermined characteristics of the labor and product market, labor market effects from the deregulation of one entry barrier cannot easily be generalized to the deregulation of other entry barriers in other markets. Other studies that evaluate deregulation reforms within countries rely on regional variation in the exposure to a deregulation reform. For example, Bruhn (2011) provide some evidence that deregulation may also reduce wages of incumbent employees in Mexico. The reform that increased the number of new firms by only 5% decreased the income of incumbent businesses by 3%, which is comparable to our effect size. Bertrand and Kramarz (2002) also suggest negative earnings effects for a deregulation in the French

[22]Fiori et al. (2012) document an interaction between product market and labor market regulation in a panel of OECD countries. They show that product market deregulation is more effective at the margin when labor market regulation is high. See also Blanchard and Giavazzi (2003), Ebell and Haefke (2009), Felbermayr and Prat (2011), and Koeniger and Prat (2007) for theoretical contributions.

[23]Studies include sectors such as banking (Black and Strahan, 2001; Cuñat and Guadalupe, 2009; Wozniak, 2007), airline (Card, 1986, 1998; Hirsch and Macpherson, 2000), trucking (Hirsch, 1993; Rose, 1987), railroads (MacDonald and Cavalluzzo, 1996), and telecommunication (Majumdar, 2015).

retail sector. Hardly any evidence exists about the long-run employment prospects of incumbent workers after a deregulation reform, and none of these studies use longitudinal (administrative) worker-level data.

Based on the theoretical and empirical findings from the literature, we can derive some expectations about the labor market effects of the reform of the German Trade and Crafts Code. Compared to other deregulation reforms studied in the literature, the evidence that we present in Section 2 indicates that the reform led to a massive increase in the number of new firms. Under the assumption that those firms compete with incumbent firms by targeting similar consumers and providing comparable goods and services, we should expect that the reform placed competitive pressure on prices in the same craft occupations. If incumbent firms want to retain their profit margins, we may observe decreasing earnings over time as firms obtain the option to re-negotiate wage contracts. We may also observe increasing unemployment. However, because most craft services are labor intensive, we should observe that employment is increasing in deregulated craft occupations with the establishment of new firms. Incumbent firms may try to retain and train their workforce, and doing so may lead to average earnings growth in the long run. Therefore, the overall reform effect is an empirical question.

A set of related studies already looks at the economic effects of the reform of the German Trade and Crafts Code (Runst et al., 2018a, provide an overview). The burgeoning literature finds surprisingly little positive effects on overall employment and wage growth in deregulated occupations for self-employed and employees (Koch and Nielen, 2016; Fredriksen, 2017; Zwiener, 2017). However, these studies are based on either cross-sectional or firm-level data, which do not allow them to track the same individual over time. Some sociological studies use cross-sectional German microcensus data to look at wages of workers in occupations that are regulated versus those that are not regulated (Bol, 2014; Bol and Weeden, 2015). However, this approach informs us about wage-level differences across those occupations but does not inform us about the causal effects of the deregulation reform. Another study from sociology comes from Damelang et al. (2018). Using SIAB data and a simple difference-in-differences design, they concentrate on incumbent worker earnings, but do not study unemployment. Their analysis stops already four years after the reform and they do not provide year-specific treatment effects. However, the literature implies that it is important to distinguish conceptually between long- and short-run welfare effects because the reduction and redistribution of rents through deregulation may also induce firms to adjust over longer periods. Moreover, we analyze pretreatment trends in detail and correct for different sample compositions, which matter for the results.

4 Data

4.1 Sample of Integrated Labor Market Biographies (SIAB)

Our main analysis uses the German Sample of Integrated Labour Market Biographies (SIAB) from the Institute for Employment Research (IAB) (Antoni et al., 2016).[24] The dataset provides detailed administrative longitudinal earnings records on a two percent random sample of individuals who are subject to social security. We also have information on the detailed occupation, educational background, industry, employment status, and some information about the firm.

The analytical sample consists of full-time employees between 25 and 55 years old in the year of the deregulation, 2003, who work in either a deregulated or regulated craft occupation in 2003. To avoid (potentially endogenous) switching before the reform, we additionally require that the individual has held the same occupation within the same firm for three years prior to the reform. We also only look at individuals who report more than ten euros in daily wages in the five years before the treatment, i.e., from 1998 to 2003. This should not be a binding restriction for full-time regular employees (Card et al., 2013). Finally, we drop employees who work in large firms, i.e., firms with more than 1,500 employees, because craftsmen firms are usually small to medium firms. It is likely that this sample may still include firms that are not registered in the crafts sector (e.g., firms may belong to the industrial sector or the public sector) because the SIAB data do not allow us to exactly identify those firms. Because those firms are not directly affected by the reform, removing entry barriers in the craft sector should not have a direct effect on their workers. However, the reform may have an indirect effect on those workers in the long-run because workers within the same occupation are able to switch across industry boundaries. This is why we also include workers who work in larger firms and who potentially do not work in the crafts sector.[25] In further analyses, we show results by firm size because small firms (e.g., with fewer than 20 employees) are most likely to be crafts firms.

To assign whether an individual is employed in a regulated or deregulated craft occupation, we take the official Trade and Crafts Code, listing the names of occupations that remain regulated (*Annex A*) and were deregulated (*Annex B1*). We map these

[24]This study uses the weakly anonymous Sample of Integrated Labor Market Biographies (years 1975-2014). Data access was provided via on-site use at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) and subsequently remote data access.

[25]Runst et al. (2018b) propose to circumvent this assignment problem by including only occupations where the share of craft apprentices within each occupational code exceeds 60%. Data for this exercise come from the Federal Institute for Vocational Education and Training. Replicating the results of Rostam-Afschar (2014), they find (as expected) stronger reform effects on self-employment in the German microcensus. However, dropping entire occupations from the sample because of low apprenticeship activity may lead to an endogenous sample selection. We prefer to split the sample by firm size to capture firms registered in the crafts sector.

occupations into the 3-digit German Classification of Occupations 1988 (KldB88), which the SIAB data use to classify occupations. Appendix Tables A-2 and A-3 show the mapping of occupations from the Trade and Crafts Code into the KldB88. For 38 occupations, the occupation name in the Trade and Crafts Code and in the Classification of Occupations 1988 are identical.[26] A further 39 occupations can be matched without any difficulties because the occupation names only differ slightly, such as using a more modern word for the occupation. Thirteen occupations where the mapping is not so obvious are mapped from the Crafts and Trade Code using digressions and further research. These include cases where the 3-digit KldB88 is not detailed enough to list the specific occupation separately. For them, we look at the 4-digit level of the classification to see in which code the occupation is included. Only four small occupations cannot be matched at all.

To study also economy-wide reform effects (see Section 6), we also use the SIAB data to look at overall changes in employment and earnings in deregulated and regulated occupations before and after the reform. For this analytical sample, we keep all employees who are employed or unemployed and are between 20 and 60 years old. We again drop workers who work in firms that have more than 1,500 employees. For the year 2003, this sample shows that approximately 11.7% work in craft occupations.[27] The data further show that 9.3% of all employment subject to social security is in regulated occupations, 2.4% is in deregulated occupations, 14% are unemployed, and 74% work in other occupations not related to the craft sector.

4.2 Microcensus

Because the SIAB data only contain dependent employees who are subject to social security, we also use data from the German Microcensus to provide evidence on the earnings effects on self-employed individuals (FDZ der statistischen Ämter des Bundes und der Länder, 2012). Moreover, we can check the robustness of our results from the SIAB data by studying the impact of deregulation on the net income of employees.

The German Microcensus is a compulsory household survey, which contains a 1% random sample of persons and households in Germany.[28] The survey provides basic sociodemographic and labor market data in the form of yearly repeated cross sections. The advantage of this survey is that answering the questions is mandatory, thus avoiding selective nonresponse.

[26] This also counts cases in which the Trade and Crafts Code occupations matches two occupations in the KldB88, such as "Bricklayer and Concreter", which is one occupation in the Craft and Trade Code but corresponds to the occupations of "bricklayer" and "concreter" separately in the KldB88.

[27] The number is comparable to official statistics for employment in the crafts sector for the year 2014 (Federal Statistical Office, 2016).

[28] We use the full 1% sample of the scientific use file (SUF), which is only available for on-site use at Research Data Centers of the Federal Statistical Office and the statistical offices of the Länder.

For our analysis, we use the waves from 1997 to 2012. The microcensus does not report income by different income sources. To avoid the inclusion of income that is raised through capital and real estate, for example, we keep only individuals who report labor earnings as their primary source of net income. The analytical sample consists of individuals who work in either regulated or deregulated craft occupations, are between 18 and 65 years of age, and work full-time. The microcensus uses the Classification of Occupations 1992, which is similar to the 1988 classification in the SIAB. Based on the 4-digit 1992 classification of occupations, we use the same procedure to classify occupations into deregulated and regulated reform occupations (see Appendix Table A-4 for the mapping).

5 Reform Effects on Incumbent Workers

5.1 Basic Empirical Setup

Using the administrative SIAB data, we track the labor market performance of individuals who worked in deregulated occupations before the reform in the year 2003 and compare them to individuals who worked in regulated occupations. According to our sample setup, workers are in the treatment group if they held an occupation in the years 2001, 2002, and 2003 that was eventually deregulated. Workers are in the comparison group if they have worked in an occupation that remained regulated. Equation (1) shows the basic setup of the resulting difference-in-differences model.

$$y_{it} = \alpha_1 + \alpha_2 \text{deregulated}_{j(i)} \times \text{post}_{2003} + \mu_t + \mu_i + \epsilon_{it} \tag{1}$$

where y are labor market outcomes (log daily earnings and unemployment) for worker i at time t. The variable *deregulated* is an indicator variable that is one if occupation j is deregulated and zero otherwise. Assignment of occupation j to individual i is based on the last year before the reform in 2003. *Post* is one if year t is after the reform in 2004 and zero for years before the reform. We are mainly interested in the interaction between *deregulated* and *post*, where the coefficient α_2 gives the average reform effect for workers who work in a deregulated occupation compared to someone who works in a regulated occupation. The effect is causal if the treatment group would have developed in the same way as the comparison group had there been no treatment *(common trend assumption)*. μ_i represents individual-fixed effects, which partial out unobserved individual heterogeneity. μ_t represents time-fixed effects that take out common time specific effects. ϵ_{it} is an error term that is clustered at the individual level.

5.2 Covariate Balancing before Treatment

Table 1 displays the mean and variance for a large set of individual covariates for the treatment and comparison group. Comparing means across the two samples, the table

shows that the treatment and comparison group are very different in their characteristics prior to the reform. Individuals in regulated occupations compared to individuals in deregulated occupations constantly earn on average six to seven percent more over the entire period from 1998 to 2003. Some reasons for this persistent earnings gap are that individuals in deregulated occupations, compared to those in regulated occupations, are more likely to be female, have a foreign citizenship, and be less well educated. Because employees in deregulated occupations are older on average than employees in regulated occupations, they also have higher job and firm tenure. Another difference is that 77% of workers in deregulated occupations work in the manufacturing sector, where this is only the case for 38% in regulated occupations. Workers in regulated occupations are very likely to work in the construction sector (34% versus 6% in deregulated occupations) because the construction sector comprises many occupations with hazardous crafts. This also explains why the tasks of regulated compared to deregulated occupations are more likely to be categorized as complex specialized tasks than professional tasks.

The table also documents that regulated and deregulated occupations differ in terms of covariate variances. For example, the standard deviation of the log earnings variables show that variances in the treatment group are larger than in the comparison group across all periods. Studying earnings growth prior to the treatment at the bottom of the table, we observe only small differences in mean earnings growth. From 1998 to 2003, earnings in the deregulated occupations have increased by 23.3% on average. Earnings growth in the regulated occupations was very similar at 24.3%. However, the variance in the earnings growth is twice as large in the regulated compared to the deregulated occupations. This difference becomes even stronger when looking at the earnings growth since 1994.

5.3 Regression-adjusted Matched Difference-in-Difference

Large compositional differences between the deregulated and regulated groups are a potential threat to the common trend assumption of the simple difference-in-difference estimator because it is unclear whether the two groups would have developed similarly without the reform. Moreover, because reform effects may be driven by a particular population group, compositional differences could also complicate the interpretation of the results. While the overrepresentation of an affected population group does not bias estimates of the causal reform effects (given that the common trend assumption holds), it may undermine external validity because the effects are scaled up or down depending on the relative weights of the population group that is affected most. Thus, an analysis that is based on samples that are comparable before the reform may more likely reflect the reform effect for a randomly drawn employee from one of the deregulated occupations.

To allow for an apples-to-apples comparison that is not driven by compositional effects, we use entropy balancing to make the comparison group as similar as possible to the treatment group before the reform. Hainmueller (2012) formulates entropy balancing as

a nonparametric data preprocessing method for binary treatment studies. The method incorporates covariate balancing in the first and second moments (and potentially higher moments) of the covariate distribution directly into a maximum entropy reweighting function. By doing that, the approach constructs weights $w(i,k)$ (with i indicating individuals in the treatment group and k in the comparison group) for each observation in the comparison group such that prespecified balancing constraints are fulfilled precisely. In this study, we require exact balancing on all covariates reported in Table 1. Showing covariate balancing after reweighting becomes redundant because means and variances of all covariates of the comparison group are by construction identical to the means and variances of the treatment group. The advantages of entropy balancing compared to classical propensity score matching are that (i) iterative and somewhat arbitrary searches for a matching function become redundant; (ii) the method retains all observations and merely reweights them, which is helpful in reducing standard errors in the estimation; and (iii) most importantly, it can also handle differences in the variance of covariates. Especially the last point seems to be important because Table 1 reveals large differences in the earnings variance prior to the reform between the treatment and comparison group.

Using the weights $w(i,k)$, Equation (2) formulates the estimator for the treatment effect (Heckman et al., 1997, 1998; Todd, 2008). In this setting, 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 equal to $w(i,k)$.

$$\widehat{\alpha}_2 = \frac{1}{n_1} \sum_{i \in I_1} \left[(Y_{1i}^{after} - Y_{0i}^{before}) - \sum_{k \in I_0} w(i,j)(Y_{0j}^{after} - Y_{0j}^{before}) \right] \tag{2}$$

The estimator is implemented in a difference-in-differences regression (see Equation (1)) with each individual weighted by $w(i,k)$ and standard errors clustered at the individual level. The advantage of the regression adjustment is that we can additionally partial out time-invariant selection on unobservables by including individual-fixed effects.

5.4 Main Results

We start by plotting average earnings of the treatment group and the (nonmatched and matched) comparison group over time in Figure 3. Prior to 2003, we observe that workers in our analytical sample perform reasonably well with steadily increasing nominal earnings.[29] Supporting the common trend assumption, both groups perform very similar over the entire period from 1994 to 2003. After the reform, however, earnings trajectories start to diverge. While earnings in regulated occupations seem to further increase, the

[29]Note that the steady earnings growth is likely an artefact of the sample selection where we require that each individual reported strictly positive earnings from 1998 to 2003. Earnings are also not adjusted for inflation.

earnings growth in the deregulated occupations is notably flatter. Compared to the hypothetical earnings growth of the matched comparison group, we observe a growing earnings gap between treatment and comparison group.

Using Equation (1) to estimate the average earnings effect of the reform from 2004 to 2014, Table 2 reports that gross daily earnings of incumbent workers in deregulated occupations decrease relative to the earnings of workers in regulated occupations by approximately 4% (Column (1)). Weighting the comparison group by matching weights, the effect halves to -2.3% (Column (2)). In Figure 4a, we plot coefficient estimates and 95% confidence intervals of yearly treatment effects to study the dynamics of the effect over time (see also Appendix Table A-5). In the nonmatched sample, we see that the negative effect on daily earnings becomes significant in 2007, three years after the reform. Daily earnings are 2% lower for incumbent workers in deregulated than in regulated occupations at this time. The effect becomes more negative over time and decreases to -7% in 2014. The reform effect becomes gradually larger over time because new firms may need some time to be established and incumbent firms need some time to adapt to the reform. The reform effect may also become larger because wage rates that are negotiated in collective bargaining agreements need some time until they can be changed and renegotiated. The results for the matched sample are generally smaller, but not significantly different from the nonmatched results. In this sample, we already observe a significant negative coefficient directly in the year after the reform. While estimates show some recovering in the years 2005 and 2006, earnings decrease to -4.3% in 2014. Thus, it seems that the reform has caused a lower earnings growth for incumbent workers in the deregulated occupations compared to the earnings growth in the regulated occupations.

Effect sizes are comparable to bargained earnings increases in collective bargaining agreements, which are recently around two to three percent of earnings.[30] However, the effects of deregulation appear rather modest when compared to the effects identified by Goldschmidt and Schmieder (2017) who document wage losses of approximately 10% to 15% for jobs that have been outsourced. The same is true when compared to wage losses after deregulation reforms in the U.S. airline industry (Card, 1986, 1998; Hirsch and Macpherson, 2000) and the trucking industry (Hirsch, 1993; Rose, 1987), which document wage decreases of between 10% to 15% after deregulation. The results are more comparable to the findings of other deregulation reforms, e.g., in Mexico (Bruhn, 2011; Kaplan et al., 2011), only that the increase in the number of businesses due to the deregulation reform is much higher in our study. Moreover, when clustering standard errors at the occupation level to allow for arbitrary correlation of errors across workers within the same occupation, we cannot reject that the effects are significantly different

[30]The yearly average collective agreement wage increase for all industries from the years 1998 to 2016 lies at 2.3%, at 2.1% in the construction industry and at 2.5% in the raw materials and capital goods industry (see collective agreement database of the Hans-Böckler Foundation (2018)).

from zero. This casts first serious doubts on whether the reform has induced strong competitive pressure.

Firms may also react over the extensive margin by laying off workers. Therefore, Columns (3) and (4) in Table 2 report the results from linear probability models on the average unemployment rate in our sample. Compared to the matched (nonmatched) comparison group, workers in deregulated occupations face a higher unemployment risk of 0.7 percentage points (one percentage point) on average. To be categorized as unemployed for this analysis, the employment records have to list the worker as unemployed or that the worker is searching for a job while currently employed.[31] Figure 4b shows point estimates and 95% confidence intervals for yearly treatment effects. While we do not observe effects for the first three years, in 2007 and 2008 the probability of being unemployed increases to statistically significant 0.8 to 0.9 percentage points.[32] The probability peaks in 2009 with 1.5 percentage points and then hovers around one percentage point until 2014.

In the interpretation of the effect size, we have to keep in mind that the sample consists only of individuals who held stable jobs within the same firm between 2001 and 2003. Thus, while the unemployment rate in this sample is zero in 2003, 3.5% of the workers experienced some unemployment in the period between 2003 and 2014. This indicates that the effect is approximately 20% (= 0.7/3.5) of the relative long-term unemployment probability of the sample. However, because overall employment in deregulated occupations is rather small (in 2003, only 2.4% of all employment subject to social security was in deregulated occupations; see Section 4.1.), the impact on the nation-wide unemployment rate may also be small.

In further analyses, Appendix Table A-6 shows that the reform tended to decrease full-time regular employment and increased marginal employment. However, the results do not differ significantly from zero when using the matched comparison group. Therefore, we conclude that this adjustment mechanism is mainly driven by the different sample compositions and less likely to be a direct effect of the reform.

5.5 Identification

The identifying assumption of these estimations is that earnings and employment in the deregulated occupations would have developed equally compared to earnings and employment in the regulated occupations in the absence of the reform *(common trend assumption)*. We already presented visual evidence that this seems to be the case in our sample (see Figures 3 and 4).[33] Table 1 also already documented that there is barely any economically meaningful difference in the average earnings growth between the treatment

[31] We do not consider individuals who drop out of the sample or report zero earnings.

[32] Effects are again not significant when clustering at the occupation level.

[33] Appendix Figure A-1 plots average gross daily log earnings of incumbent workers in the regulated and deregulated occupations relative to the year 2003. The figure reiterates that trends before the reform are very similar.

and comparison group.[34] Those nonsignificant pretreatment trends in the nonmatched sample suggest that differences between the matched and nonmatched reform effects are more likely due to different compositions of workers in regulated and deregulated occupations and not due to different prereform economic trends.

To investigate common trends in pretreatment periods more formally, we move the reform to the year 1998 to check whether the earnings pattern has already diverged between regulated and deregulated occupations before 2003 in our baseline sample.[35] Results in Appendix Table A-8 confirm the descriptive analysis from Figure 3 that there are no meaningful pretreatment trends in our baseline sample (Columns (1) and (2)). However, it could be that earnings have diverged between the two occupational groups more generally. This may also affect future earnings of workers in our sample. Therefore, we use a new sample of workers, imposing the same set of sample restrictions as in our baseline case with 1998 as the reference year. Matching is performed on the years 1993 to 1998. On average, we find that workers in deregulated occupations even have slightly *higher* earnings over time prior to the reform year in 2003. However, there are significant and negative effects in the nonmatched sample for earnings in 2002 and 2003 of about -0.8% and -1.6%, respectively. The matched sample does not show any economically significant earnings effects over the same period. This underscores the importance of using matching weights to correct for different sample compositions.

The common trend assumption may also be violated if the reform was anticipated by firms and workers, giving them the opportunity to self-select into one of the treatment groups. This is unlikely because the reform was passed and implemented within the year of 2003, making it difficult for workers to switch occupations and for firms to adjust their business model already prior to the reform. In addition, restricting the sample to regular employees who work in the same occupation and in the same firm for three years prior to the reform avoids our results being affected by endogenous switching. However, because the sample would be larger by about a third without these restrictions, effects may be subject to a sample selection bias. The results are qualitatively and quantitatively similar when we allow for occupational and firm switches before the reform and include nonregular employees (including unemployment and apprenticeship spells) (see Appendix Table A-9). This implies that the results do not strongly depend on the sample restriction and it also implies that switching before the reform is not systematically related to the reform.

[34] Appendix Table A-7 shows that matching successfully addresses remaining differences in the earnings growth between the two groups.

[35] The reference year 1998 is arbitrarily chosen. However, the analysis requires a sufficient number of years before and after any given pseudo treatment year. We use five pretreatment years as in the baseline case. By starting in the year 1993, the analysis avoids major economic changes that are due to the German reunification in 1989/90. The analysis stops in 2003 with the start of the true treatment. Thus, the years 1993 and 2003 establish the natural upper and lower bounds for a common trend analysis. In any case, showing yearly treatment effects make the exact year of the pseudo-treatment less important.

Another threat to identification comes from changes in the economy that affect deregulated occupations more negatively than regulated occupations in the period after 2004. As outlined in Section 2, the reform was part of a larger reform package that aimed at reforming labor market institutions (especially unemployment insurance systems and assistance) and the organization of the Federal Employment Agency. It is unlikely that these other reforms had differential impacts on regulated and deregulated occupations, especially because the wage setting process was not affected at all (Dustmann et al., 2014). We are more concerned about two economic events that happened closely after the reform. The first event is the enlargement of the European Union in 2004 and 2007, which granted citizens from new member countries access to the German labor market.[36] While the data clearly show that the share of foreign workers is twice as large in deregulated occupations as in regulated occupations (see Table 1), these shares do not change much over time. For example, the SIAB data show that overall 5.6% (10.7%) of workers in regulated (deregulated) occupations were foreign citizens in the year 2003. These numbers change to 5.7% and 10.8% in 2010, representing a negligible increase.[37] The most likely reason for this low increase in the foreign workforce is that Germany allowed unlimited access to the German labor market for the new 2004 (2007) member countries only from 2011 (2014) onwards. Nevertheless, we use a regional approach to test whether changes in the share of foreign workers at the county level affect the reform estimates. The share of foreign workers at the county level is computed based on all workers in the SIAB data. Columns (1) and (3) of Table 3 show the results of a triple difference-in-differences model, which interacts the yearly share of foreign workers with the treatment interaction. We do not observe that the share of foreign worker interacts in meaningful ways with the treatment effect.

A second event is the economic and financial crisis of 2009. While earnings already start to decrease more in deregulated occupations than in regulated occupations in 2007, Figure 4a suggests that workers in deregulated occupations are hit more by the crisis than workers in regulated occupations in 2009. However, the larger reform effect in 2009 seems to disappear again in 2010 with effect sizes that are almost the same as in 2008. The most likely reason for this seemingly transitory shock is that the crisis affected had its strong effects on the export-oriented manufacturing sector, which also comprised the largest share of individuals working in deregulated occupations (see Table 1).[38] Rinne and

[36]In 2004, ten countries joined the EU: Cyprus, the Czech Republic, Estonia, Hungary, Latvia, Lithuania, Malta, Poland, Slovakia, and Slovenia. In 2007, Bulgaria and Romania joined the EU.

[37]Occupation-level regression results confirm no relationship between the share of foreign workers in deregulated compared to regulated occupations and the reform (Column (7) in Table 7). We stop in 2010 because the Federal Employment Agency introduced a new occupational classification (KldB2010) in the year 2011, which is very close to the International Standard Classification of Occupations 2008. However, the major revision makes it impossible to clearly map old and new occupations, which basically invalidates comparisons over longer time periods.

[38]Rinne and Zimmermann (2012) document that the GDP in manufacturing decreased by 18% in 2009.

Zimmermann (2012) document that manufacturing production recovered quickly after the crisis, which may explain the transitory increase of the reform effect in 2009. Running regressions within industry groups, Appendix Table A-10 shows that the reform effects within manufacturing are in line with the baseline effects. The average effect seems more driven by the reform effects in the construction and trade, maintenance, and repair industries, which are nearly two times as large as the effect in manufacturing. This shows that it is important to match on industry groups in the entropy balancing such that the effect is not dominated by a particular characteristic of the sample.

However, Rinne and Zimmermann (2012) also document that the crisis hit some regions harder than others because economic activity and industries are unequally distributed across Germany. Using the unemployment share of the county as an indicator for the severity of the crisis, we estimate another triple difference-in-differences model to test the interaction of the crisis with the reform effect at the regional level (Columns (2) and (4) of Table 3). We again use all workers in a county to compute regional unemployment shares by using the SIAB data. While we do not find that reform-induced unemployment effects are stronger in regions with high unemployment growth, we do find that the earnings effect is slightly stronger with increasing unemployment: a one percentage-point increase in the regional unemployment rate (seven percent increase in the national unemployment rate in 2003)[39] increases the reform effect by 0.17 percentage-points. While this does not seem to be a strong effect when compared to the main reform effect of -2.34%, this indicates that local economic conditions matter to a certain extent for the outcome of the reform.

Economic shocks may also hit one particular occupation across industries (instead of several occupations within one industry). This may be a threat to our identification because it would make other occupations that do not experience the shock bad comparisons. In our main analysis, we match on industries in the entropy balancing because we are more concerned about industry-specific economic shocks (see above) than about occupation-specific shocks. Nevertheless, Appendix Table A-16 provides alternative results when matching on seven craft groups, categorizing occupations of the crafts sector along similar tasks (see Appendix Table A-1).[40] The classification of occupations comes from the German Confederation of Skilled Crafts and includes (i) building and interior finishes trades, (ii) electrical and metalworking trades, (iii) woodcrafts and plastic trades, (iv) clothing, textiles, and leather crafts and trades, (v) food crafts and trades, (vi) health and body care trades as well as the chemical and cleaning sector, and (vii) graphic design. Compared to the baseline specification, we find larger reform effects when matching on

[39]The average unemployment rate among all workers subject to social security payments in 2003 was equal to 14%. The national rate of unemployment (including the entire workforce) was somewhat lower at 11.6% in 2003 Federal Statistical Office (2018c).

[40]The overlap between industries and craft groups is not large enough to consider craft occupation by industry-specific effects.

craft groups instead of industry groups. This suggests that controlling for industry-specific shocks may be more important.

A further threat to identification is that workers in deregulated occupations switch systematically to close occupations in regulated occupations because they may receive higher earnings there. This would contaminate the comparison group because the comparison group would also be affected by the treatment. To shed some light on the extent of switching, we estimate whether workers switch occupations and firms more frequently in deregulated compared to regulated occupations after the reform. Based on the matched comparison group, Appendix Table A-11 reveals that occupational and firm switching is not systematically different between deregulated and regulated occupations (Columns (1) and (3)).[41] However, at a more aggregated level, workers are slightly more likely to switch from a deregulated occupation to a regulated occupation after the reform than the other way around (Column (2) in Appendix Table A-11). Given that we assign treatment status based on the occupation in 2003, occupational switching between occupational groups would lead to an underestimation of the reform effect (assuming that switchers change occupations to earn higher wages).

Another way to study the impact of potential switching on the reform effect is to drop regulated and deregulated occupations that are similar to each other. From the list of occupations, we identified three occupational groups that have to perform similar tasks. These occupational pairs are (i) tile, slab and mosaic layer and cast stone and terrazzo maker (both deregulated) and bricklayer and concreter (regulated), (ii) metal former, galvanizer, and metal and bell founder (all deregulated) and metal worker (regulated), and (iii) interior decorator (deregulated) and installer and heating fitter (regulated). Because of similar task requirements, skills should be more transferable and workers may find it particularly easy to switch between these occupations. As expected, dropping those occupations from the sample yields larger reform effects (Appendix Table A-12). In particular, dropping occupations in sample (ii) raises reform effects from -2.3% to -3.3% for earnings and from 0.7% to 1% for unemployment. One reason could be that workers can switch occupations between these craft occupations very easily. Another interpretation could be that because those occupations are often in the industrial manufacturing sector and not in the crafts sector, the reform would not affect earnings and unemployment in these occupations.

[41] In the non-matched sample, we observe that occupational switching is stronger in the deregulated occupations than in the regulated occupations after the reform. However, workers in deregulated compared to regulated occupations are less likely to change their firm after the reform. The results are not shown.

5.6 Heterogeneity by Firm Size

As mentioned earlier, the SIAB does not allow to distinguish between craft businesses and businesses in other industries. This may attenuate the reform effect if our sample comprises many firms that are not directly affected by the reform. Because craft businesses are usually rather small with no more than nine employees on average,[42] and because we know that the reform has mainly produced one-man businesses, we expect that the reform effect is strongest among small firms. The effects should also be considerable weaker in larger firms because they may absorb the reform effect more easily and workers who are not working in the craft sector can only be affected indirectly. Table 4 shows results by pretreatment average firm size of the individual worker (see Appendix Figures A-2 and A-3 for yearly treatment effects). The entropy balancing procedure is rerun within each subsample to ensure a valid comparison group. The findings confirm our expectations. For workers in firms with fewer than 20 employees, the reform effect in terms of earnings and unemployment is twice as large as in the overall sample. The effect fades out with increasing firm size.

5.7 Attrition

Overall, we observe that only 6.8% of all person-year observations are missing in our analysis. While workers may disappear from the social security records for various reasons (leave the labor force, migrate abroad, become a public servant, pass away), the reform should also have triggered attrition in the deregulated occupations because self-employed workers are not listed in the social security records. Dropouts account for 79% of missing person-year observations (5.4% of all person-year observations). Columns (1) and (2) of Table 5 document that workers drop out (slightly) more frequently in deregulated occupations than in regulated occupations. Appendix Table A-13 reveals that yearly treatment effects may also increase up to 1.7 percentage-points in the year 2012. The other 21% of missing person-year observations (1.4% of all person-year observations) are not in the analysis because workers report missing or zero earnings. This may happen when workers are still registered, but the employer does not report any earnings that are subject to social security payments. We do not know why no earnings are reported for these workers. However, it seems likely that some of them have become self-employed while still being registered with their former employer. Excluding dropouts, Columns (3) and (4) of Table 5 document the reporting of zero and missing earnings is also more frequent in deregulated occupations than in regulated occupations. Finally, Columns (5) and (6) of Table 5 show highly significant results for selective attrition, which is due to either dropping out or reporting of nonpositive earnings, respectively.

[42] For example, the Federal Statistical Office (2018b) reports that the average firm has about nine employees, ranging from two to 33 employees depending on the specific occupation.

To get an idea about the (average) ability of potential new business owners, we compare the earnings in 2003 of workers who we know drop out in future periods to the earnings of workers who do not drop out.[43] Appendix Table A-14 shows that dropouts who drop out at some year between 2004 and 2014 earn 11% less than nondropouts in 2003 (Column (1)). This negative selection is not different between workers in deregulated versus regulated occupations (Column (2)). However, when concentrating on workers who drop out immediately after the reform in the years 2004 to 2006 (2004 to 2008), we observe that the earnings disadvantage of dropping out of the social security records is five (three) log-points lower for workers in deregulated occupations than in regulated occupations. However, the earnings disadvantage for dropping out is also six (five) log-points larger than in the average over all years, leaving the overall negative earnings selection almost unchanged. Hence, while dropouts from deregulated occupations earn slightly higher earnings than dropouts from regulated occupations, they still earn much lower earnings than workers who do not drop out. It is also interesting to note that the earnings advantage completely vanishes for later dropouts in the years 2009 to 2014, indicating that the positive selection appears only in the early periods.

Dropping out of the sample can also be related to other background characteristics. Controlling for interactions between educational, personal, and job characteristics and whether the worker has worked in a deregulated occupation in 2003, further narrows the earnings gap between dropouts and nondropouts in deregulated occupations in 2003 (Appendix Table A-15). However, it is still the case that dropouts are negatively selected compared to nondropouts in deregulated occupations. These findings suggest that working in a high-paying job prior to the reform creates low incentives to start one's own business.

Attrition may also be an identification problem if it is not related to the reform. For example, we would also detect a reform effect if workers in deregulated occupations with an otherwise higher earnings growth dropped out of the sample. We already document above that this is unlikely given that those who drop out are those with below-average earnings. Nevertheless, we can test how large earnings of dropouts need to be for our results to go away. We bring missing observations back into the sample by imputing different percentiles where the percentiles are based on the yearly earnings distribution of workers in the crafts sector. In Table 6, we calculate the average reform effects with those imputations at the 10th, 25th, 50th, 75th, and 90th percentiles. Estimates are generally in the same ballpark as our baseline effect of -2.34%. Even imputing the 90th percentile for everyone how dropped out of the sample gives still a negative and significant reform effect of -1.3%.

[43]By dropouts, we refer to workers who drop out from the sample due to disappearing from the social security records or due to reporting zero or missing earnings.

6 Reform Effects on Overall Employment and Earnings

To set the results into perspective, we now turn to economy-wide effects of the reform. Theory suggests that the deregulation should have had beneficial effects for employment growth and perhaps also for earnings growth (with only incumbent workers seeing earnings and employment losses). We then also turn to a discussion about how the earnings of self-employed individuals have changed due to the reform.

6.1 Employment and Earnings of Employees (SIAB)

Using SIAB data, Figures 5a and 5b present the evolution of employees subject to social security before and after the reform in deregulated, regulated, and other occupations (relative to the year 2003). The analytical sample is based on workers who report valid occupational codes, work in firms with fewer than 1,500 employees and are between 20 and 60 years old. The analysis starts in the year 1999 and stops in the year 2010 due to changes in the occupational classifications, which do not allow a coherent comparison over time. While total employment in other occupations has increased by approximately 5% in 2010 and employment in regulated occupations has stabilized at some lower level, we observe ongoing *decreases* in employment levels in deregulated occupations. The results are confirmed by occupation-level regression using occupation-fixed effects (Columns (1) to (3) in Table 7). They show that employment in deregulated occupations has decreased by approximately 8% more on average than in regulated occupations.[44]

Average earnings for (new and incumbent) employees at the occupational level are lower in deregulated than in regulated occupations by approximately 1% to 1.6% (Column (5) and (6) in Table 7). However, the average effect is probably only due to the large drop in earnings in the year 2009 (see Figure 5c). The most likely reason for this drop is again that the economic crisis hit in particular the manufacturing sector, which employs the majority of workers in deregulated occupations. The analysis suggests that occupation-level regressions are not able to capture the reform effects on incumbent workers because they do not track individual workers over time.

One of the reform goals was to halt the negative trend in apprentice numbers in craft occupations through establishing new firms. However, opponents of the reform argued that the deregulation would do the opposite because the master craftsman certificate is seen as a qualification (and also unofficial obligation) to educate apprentices. Furthermore, the deregulation undermined firms' incentives to educate apprentices, because they now

[44]By contrast, Koch and Nielen (2016) use representative firm panel survey data to evaluate the reform and do not find significant effects on employment. They also do not find effects on lay-offs, limited-term contracts, part-time work, and the number of employees on union tariffs. While our study, which uses administrative individual-level worker data, clearly indicates negative employment effects, the bottom line is that it is highly unlikely that the reform has triggered positive employment growth.

could become direct competitors by founding their own business, upon completion of their apprenticeship. After the reform, business owners without a master craftsman certificate had to apply for the right to employ apprentices. Using SIAB data, Figure 5d indicates that the number of apprentices has decreased more in deregulated occupations than in regulated occupations. However, occupation-level regressions do not find a significant relationship between the number of apprenticeships in deregulated compared to regulated occupations and the reform (Column (4) in Table 7). These results are confirmed when examining firm registry data from the German Confederation of Skilled Crafts in Appendix Figure A-4. While there is a clear continued downward trend in apprentice numbers in craft occupations, the regulated and deregulated occupations do not actually differ in the numbers of apprenticeships and apprenticeship final examinations.[45]

Overall, the results imply that the newly established firms did not create new job opportunities. This is in strong contrast to other deregulation reforms studied elsewhere. Before we discuss likely reasons for these differences to other studies in the literature in the next section, we examine the performance of self-employed craftsmen. The analysis should give us an indication about the role of the entrepreneurs in explaining the reform effects.

6.2 Net Income of Self-Employed and Employees (Microcensus)

The SIAB data do not allow us to identify self-employed individuals because they are not subject to social security payments. To study reform effects on the income situation of self-employed individuals, we use repeated cross-sections from the German Microcensus. The large sample size enables us to concentrate on the labor market position of self-employed individuals in craft occupations. We can also verify the SIAB results on the overall economic situation of employees in craft occupations, using measures of net income instead of gross earnings. However, the cross-sectional nature of the survey does not allow us to track individuals over time, leaving us to compare yearly occupational averages. This raises concerns about common trends in pretreatment periods, which we try to mitigate by controlling for individual covariates and occupation fixed effects in the occupation-level regression analysis. Moreover, we cannot distinguish between newly self-employed individuals and individuals who were already self-employed before the reform.

While we find that the number of self-employed individuals has increased in deregulated compared to regulated occupations (see also Rostam-Afschar, 2014; Runst et al., 2018b), we do not find substantial income effects for workers who work in craft occupations (Columns (1) and (2) in Table 8). Studying the effects by employment status,

[45]Koch and Nielen (2016) find slightly positive effects on the number of apprentices in deregulated compared to regulated occupations when using firm survey data. The results may differ because the survey data contain only firms with at least one employee subject to social security, biasing the results toward larger firms.

we find negative but statistically nonsignificant effects on self-employed individuals, whereas the effects for employees are slightly positive (Columns (3) and (5) in Table 8).[46] However, controlling for age, gender, highest school degree, highest professional degree, and nationality, we do not detect any reform effects (Columns (4) and (6) in Table 8). The latter result confirms small overall earnings effects that we found with the SIAB data.

To sum up, the microcensus analysis suggests that the economy-wide economic position of self-employed and employees has not changed dramatically after the reform. This is surprising given that the reform substantially increased entrepreneurial activity. In the next section, we discuss likely reasons for the reform effects.

7 Discussion

The reform of the German Trade and Crafts Code in the year 2004 has led to a large increase in the foundation of new businesses. While there has been no notable growth in the number of new establishments in either occupational group, we documented that the number of deregulated businesses increased annually by 26% in the first three years and then increased further by approximately 5% each year from 2007 to 2014. This is substantially higher compared to other deregulation reforms that are studied in the literature. For example, similar reforms in France, Mexico, Portugal, and India led to increases of between 5% and 17% in the number of new firms (Aghion et al., 2008; Bertrand and Kramarz, 2002; Branstetter et al., 2014; Bruhn, 2011; Kaplan et al., 2011).

As theory would suggest, we find that the economic position of incumbent workers in deregulated occupations compared to workers in regulated occupations was negatively affected by the reform. On average, earnings increased less strongly by approximately 2.3% for deregulated occupations than for regulated occupations after the reform. The strength of the earnings reform effect is almost linearly increasing (in absolute terms) over time. For unemployment, we find an average increase of approximately 0.7%. As mentioned earlier, the earnings losses of incumbent workers seem to be rather small compared to the large increase in new firms. In contrast to the theory, however, we do not find increases in overall employment or average earnings for new employees. This is also not in line with almost all of the studies mentioned before because these studies also document that deregulation triggers employment growth and sometimes earnings growth for other employees.

What can explain the relatively small reform effect with respect to earnings? To have enduring and large labor market effects, the newly established businesses would have to represent stable competitors for incumbent firms. While the aggregate numbers in Figure 1 indicate that the stock of businesses is strictly increasing, findings by Müller

[46] Appendix Figure A-6 plots average log monthly personal income of employees and self-employed individuals in craft occupations over time.

(2014, 2016) indicate that the composition of firms has changed over time. Studying survival rates of newly founded businesses, he shows that five-year survival rates hovered approximately 70% in both regulated and deregulated occupations before the reform and dropped to approximately 50% in the deregulated occupations after the reform. Thus, approximately 60% of newly established firms had already disappeared after five years (Müller, 2014, 2016). This is in line with the theoretical model of Branstetter et al. (2014) who show that new firms have lower survival probabilities than incumbent firms. Their model further postulates that new entrepreneurs have relatively lower talent than those already in the market. While we cannot compare skills of incumbent entrepreneurs with the skills of new entrepreneurs, our dropout analysis in Section 5.7 suggests that new entrepreneurs are not a positive selection when compared to the average worker in the craft occupations. Accordingly, Runst et al. (2018b) show that both entry *and* exit rates of firms have increased in the deregulated crafts after the reform.

Furthermore, customers may have discriminated between crafts offered by newly established firms and crafts offered by incumbent firms because incumbent firms still hold a master craftsman certificate to signal superior quality of their goods and services. In other words, it could be that the two firms were not offering perfectly substitutable crafts, which would have mitigated competitive pressure for incumbent firms. There are some pieces of evidence that this was the case. For example, Fredriksen et al. (2018) show that customers still value the master craftsman certificate and that firms in deregulated occupations use the certificate as a quality signal. While effective competitive pressure should have reduced prices for customers, there is little evidence that aggregate price indicators for goods and services in the different occupations are largely affected by the reform (RWI, 2012). However, because detailed data on prices are not available, the results of this analysis remain rather speculative. However, if firms do not experience large pressure to lower prices, we should observe that the earnings disadvantage of the reform is smaller for a selected group of workers who remain with the same firm and stay in the same occupation after the reform. Columns (4) to (7) in Appendix Table A-11 indicate that this is the case. The earnings effect is only half of the average effect for workers who have not changed the firm or the occupation. Treatment effects are much larger for occupation and firm switchers. Moreover, incumbent firms may have reduced competitive pressure by investing more in technology and innovation. However, Koch and Nielen (2016) use data from a representative establishment panel and find that the reform had insignificant effects on investments (per establishment or employee) and product innovations.

What can explain the absence of a reform effect on overall employment and average earnings for other employees? Müller (2014, 2016) shows that the majority of newly founded firms were and remained one-man businesses with very little apprenticeship activity. He documents that shares of one-man businesses in the deregulated occupations increased from 24% in 1995 to 61% in 2012. The likely reason for limited employment

growth was that newly established firms were not able to compete at a large scale with incumbent firms. Therefore, they remained relatively small with little creation of further employment. The absence of higher labor demand through the new firms may also explain why there has not been an effect on average earnings growth for other employees in the economy.

8 Conclusion

In this paper, we study the labor market effects of the reform of the German Trade and Crafts Code in 2004. Abolishing the requirement to hold a master craftsman certificate to open a new business increased entrepreneurial activity massively, tripling the number of businesses within ten years. Using administrative longitudinal social security data and German Microcensus data, we find that the reform led to decreasing earnings and increasing unemployment for incumbent workers. However, contrary to the theory, the reform did not trigger employment and earnings growth for others in affected occupations. The most likely reason for this finding is that the newly established firms remained one-man business with low ability to compete against incumbent firms. This makes it also unlikely that the reform strongly affected prices in the crafts sector. While the reform may have increased choice for customers, we conclude that the reform led to an average welfare loss for incumbent craft workers in deregulated occupations because they experienced employment and earnings losses without having the opportunity to exploit new employment possibilities.

The study contributes to the current literature by documenting the labor market effects of a deregulation reform that failed to produce intended effects of increasing employment dynamics within an industry. This is the case even if the increase in entrepreneurial activity was much larger in the German reform than it was in reforms studied elsewhere. It is also one of the few studies that uses longitudinal administrative earnings data to track the same workers over long time periods. This allows us to sort out unobserved individual heterogeneity from the impact of different sample compositions.

While decreasing entry barriers should generally foster competition, entrepreneurial activity, innovation, and employment growth, policymakers should be aware that there may be unintended consequences when the newly created businesses do not compete with incumbent firms. If this is the case, it is likely that there are further (more important) barriers in place that hold back new firms from becoming stable competitors. Thus, each deregulation reform should collect and carefully evaluate possible industrial and occupational entry barriers before the reform is implemented. While it is difficult to identify all sorts of entry barriers in advance, the success of each reform must be constantly monitored and evaluated.

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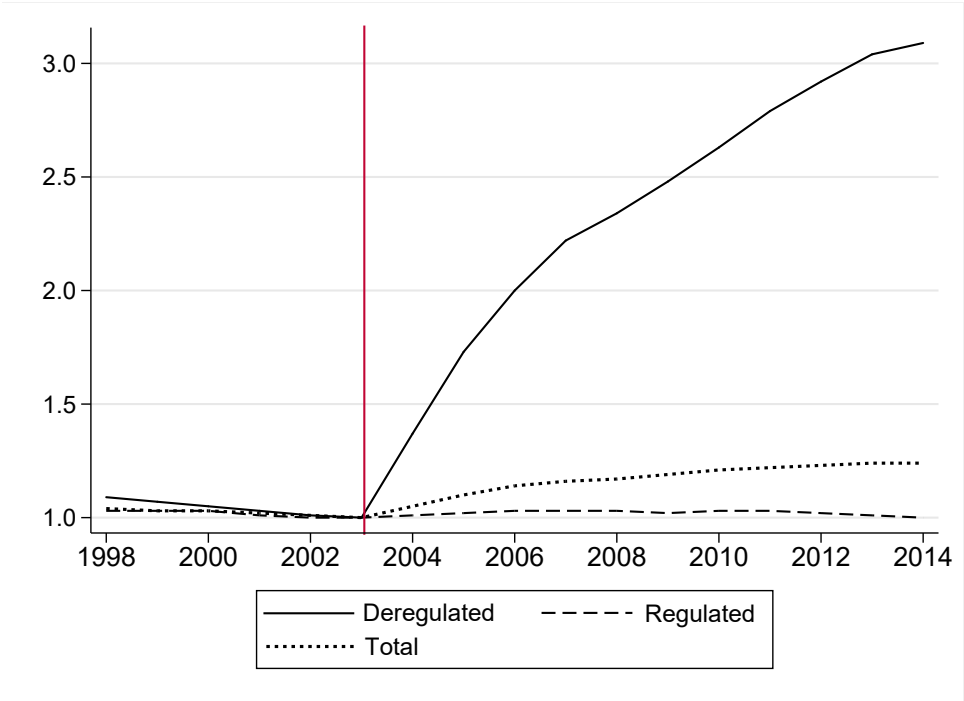
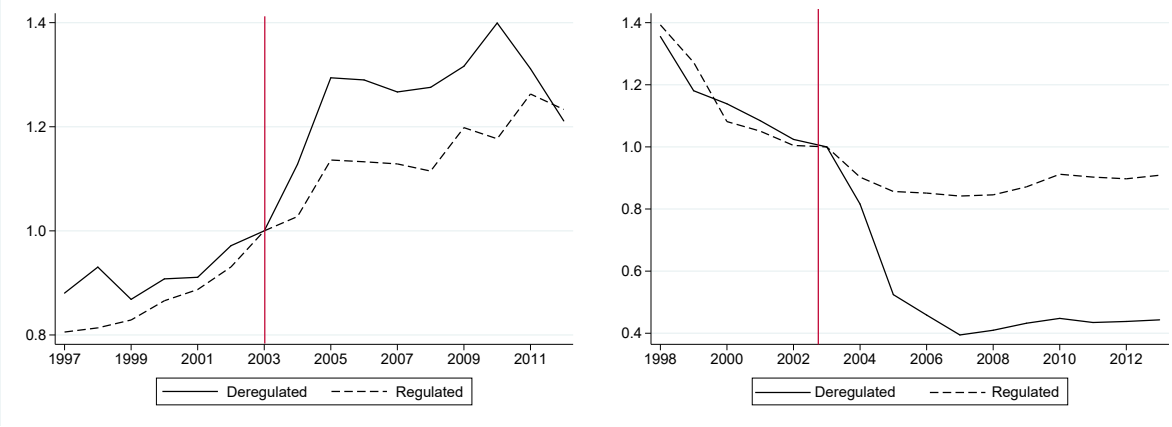
Figures and Tables

Figure 1: Changes in Number of Businesses (relative to 2003)



Notes: The figure shows the number of business in the deregulated and regulated crafts. In 2003, the number of businesses in the regulated (deregulated) crafts sector was 587,762 (74,940).
Source: German Confederation of Skilled Crafts

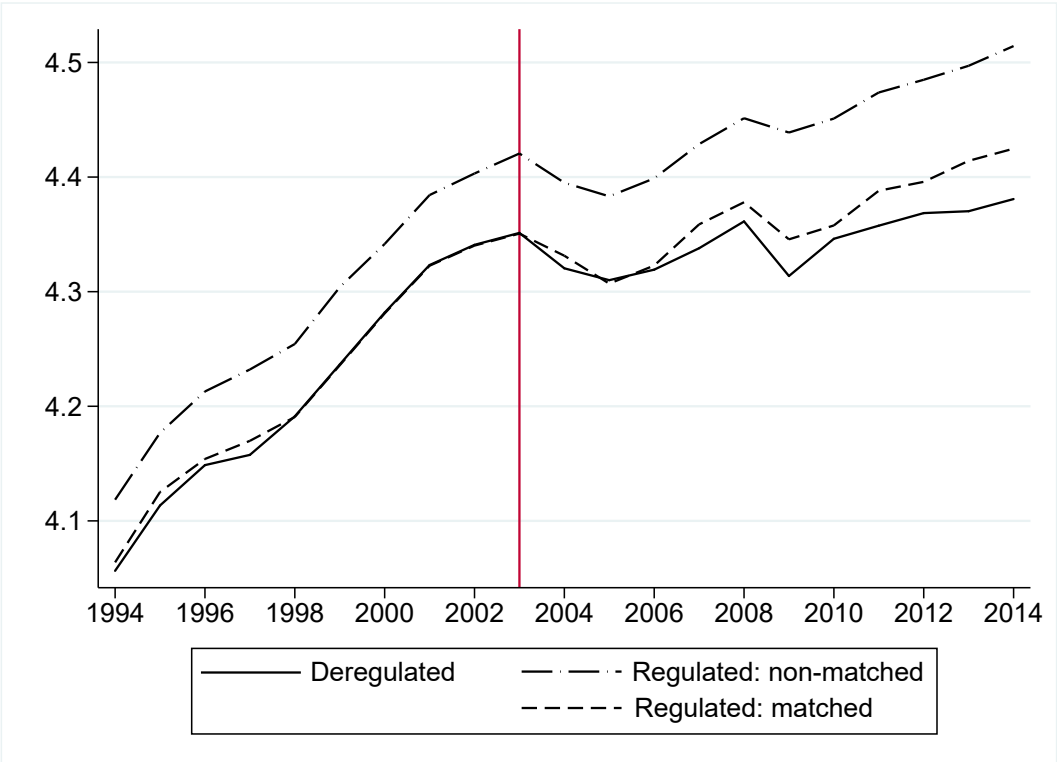
Figure 2: Self-Employment and Master Craft Examinations (relative to 2003)



(a) Self-employed (b) Master Craft Examinations

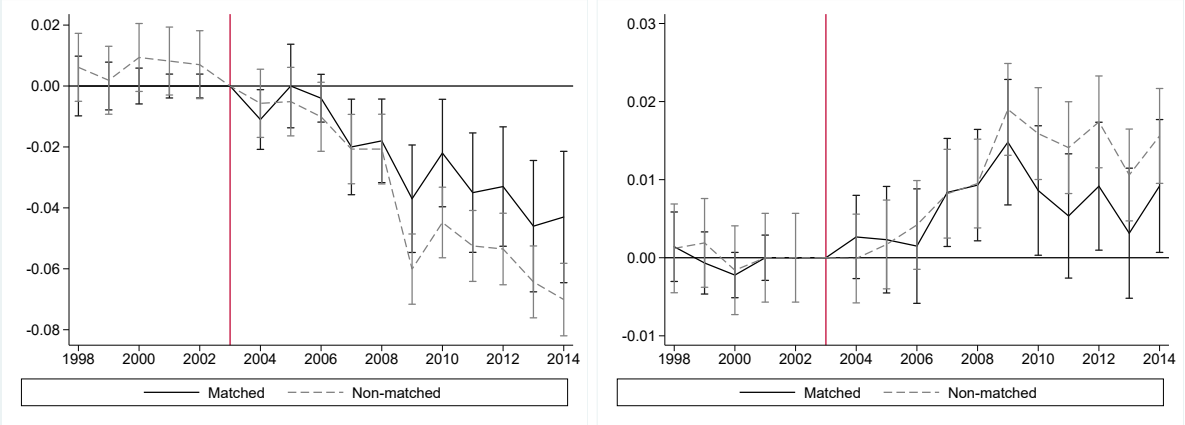
Notes: The figure shows the share of self-employed individuals in Figure (a) and the number of master craft examinations in Figure (b). Both figures show values relative to the year 2003.
Sources: Research Data Centres of the Federal Statistical Office and the statistical offices of the Länder, Microcensus, census years 1998-2012. (Figure (a)) and German Confederation of Skilled Crafts (Figure (b))

Figure 3: Log Earnings Before and After the Reform



Notes: The figure shows average log gross daily earnings for the sample of incumbent workers. Earnings are in current values and not adjusted for inflation.
Source: SIAB

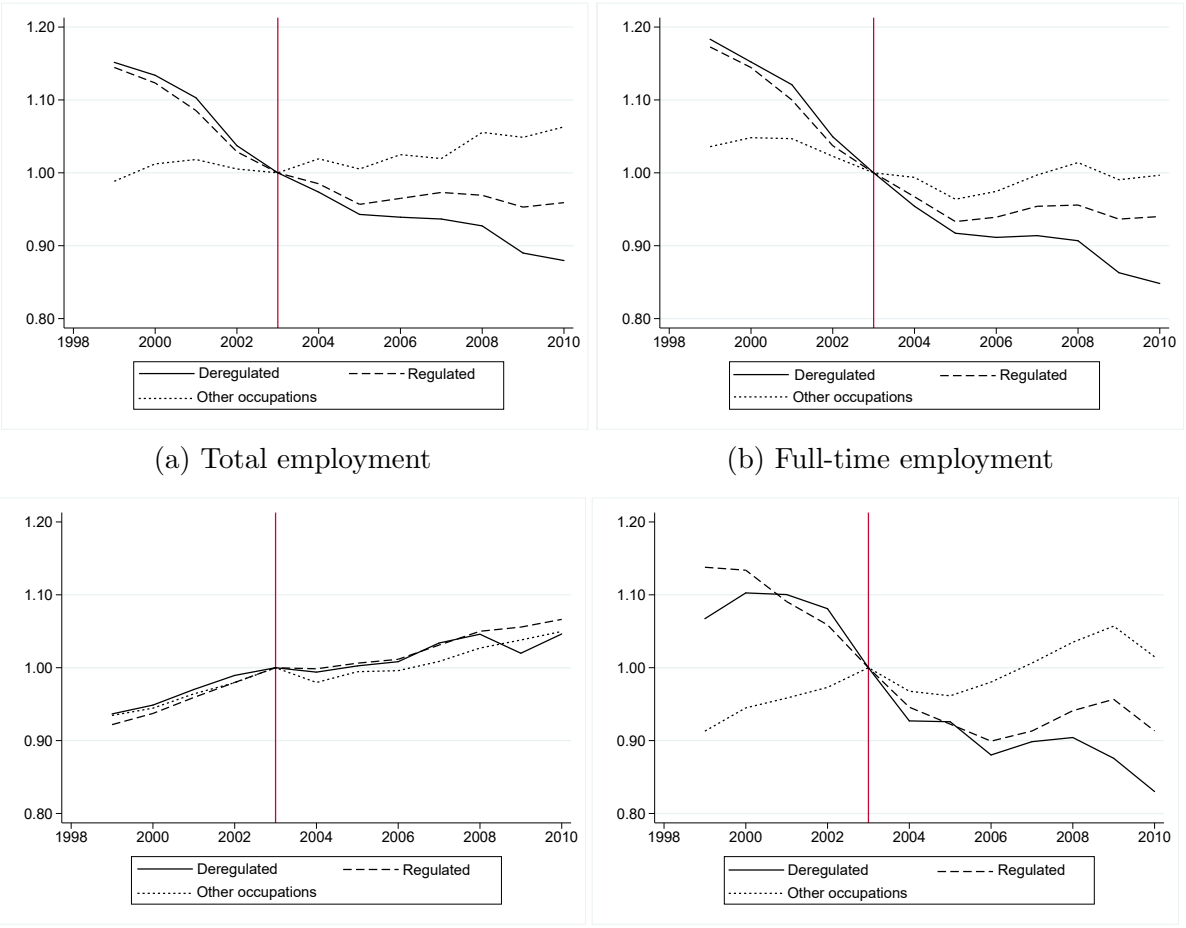
Figure 4: Effect on Log Earnings and Unemployment for Incumbent Workers over Time



(a) Log daily earnings (b) Unemployment

Notes: The figure shows coefficient estimates and 95% confidence intervals of the reform effect on log gross daily earnings in Figure (a) and on unemployment in Figure (b) for the sample of incumbent workers.
Source: SIAB

Figure 5: Overall Employment and Earnings (relative to 2003)



(a) Total employment

(b) Full-time employment

(c) Gross daily earnings

(d) Apprenticeships

Notes: The figure shows average employment and gross daily earnings in deregulated, regulated, and other occupations. The analytical sample is based on workers who report valid occupational codes, work in firms with less than 1,500 employees, and are between 20 and 60 years old. Figure (a) plots total employment. Figure (b) restricts the sample to workers who are employed as regular full-time employees. Figure (d) restricts the sample to apprentices. Figure (c) plots average daily earnings over all individuals who report positive earnings. Earnings are in current values and not adjusted for inflation. All time series are relative to the year 2003.
Source: SIAB

Table 1: Covariate Balancing Before Matching, 2003

	(1)	(2)	(3)	(4)	(5)	
	Deregulated		Regulated		Mean difference	
	Mean	Variance	Mean	Variance	Δ	Sig.
Log daily gross earnings 2003	4.351	0.166	4.421	0.148	-0.070	***
Log daily gross earnings 2002	4.340	0.156	4.403	0.135	-0.063	***
Log daily gross earnings 2001	4.323	0.156	4.384	0.133	-0.061	***
Log daily gross earnings 2000	4.281	0.175	4.342	0.152	-0.061	***
Log daily gross earnings 1999	4.236	0.192	4.304	0.168	-0.068	***
Log daily gross earnings 1998	4.191	0.211	4.254	0.192	-0.063	***
Female	0.229	0.177	0.089	0.081	0.140	***
Foreigner	0.086	0.079	0.042	0.040	0.044	***
Age: 12-34	0.183	0.150	0.231	0.178	-0.048	***
Age: 35-45	0.463	0.249	0.474	0.249	-0.011	***
Age: 46-55	0.354	0.229	0.294	0.208	0.060	***
Training: none	0.181	0.148	0.056	0.052	0.125	***
Training: vocational	0.764	0.181	0.872	0.112	-0.108	***
Training: university	0.018	0.018	0.042	0.041	-0.024	***
Training: missing info	0.037	0.036	0.031	0.030	0.006	***
Schooling: Haupt-/Realschule	0.920	0.271	0.895	0.094	0.025	***
Schooling: FH-Reife / Abitur	0.042	0.040	0.074	0.069	-0.032	***
Schooling: missing info	0.038	0.037	0.031	0.030	0.007	***
Ever unemployed	0.035	0.034	0.034	0.033	0.001	
Ever marginally employed	0.002	0.002	0.001	0.001	0.001	***
Changed firm within 6 years before reform	0.201	0.161	0.254	0.190	-0.053	***
Changed occupation within 6 year pre reform	0.145	0.124	0.134	0.116	0.011	**
Industry: manufacturing	0.773	0.176	0.383	0.236	0.390	***
Industry: construction	0.062	0.058	0.338	0.224	-0.276	***
Industry: wholesale and retail trade	0.061	0.057	0.090	0.082	-0.029	***
Industry: real estate and business activities	0.071	0.066	0.057	0.054	0.014	***
Industry: other industries	0.033	0.032	0.132	0.114	-0.099	***
Tasks: professional tasks	0.949	0.049	0.808	0.155	0.141	***
Tasks: complex specialized tasks	0.051	0.049	0.192	0.155	-0.141	***
Job tenure: 0-3 years	0.127	0.111	0.185	0.151	-0.058	***
Job tenure: 4-7 years	0.283	0.203	0.299	0.210	-0.016	***
Job tenure: 8-14 years	0.365	0.232	0.334	0.223	0.031	***
Job tenure: 15-39 years	0.226	0.175	0.181	0.148	0.045	***
Firm tenure: 0-3 years	0.105	0.094	0.142	0.122	-0.037	***
Firm tenure: 4-7 years	0.249	0.187	0.270	0.197	-0.021	***
Firm tenure: 8-14 years	0.360	0.230	0.346	0.226	0.014	**
Firm tenure: 15-39 years	0.286	0.204	0.242	0.183	0.044	***
Employees at firm (2001-2003 average)	241	98,528	175	85,241	66	***
Median wage at firm (2001-2003 average)	85.4	631.6	86.7	934.8	-1.3	***
Regional unemployment 2003	0.115	0.002	0.119	0.003	-0.004	***
Regional foreigner share 2003	0.070	0.003	0.065	0.003	0.005	***
Regional average daily earnings 2003	79.83	79.11	79.44	87.00	0.390	***
Growth regional unemployment 99-03	0.138	0.027	0.123	0.025	0.015	***
Growth regional foreigner share 99-03	0.061	0.109	0.083	0.142	-0.022	***
Growth regional average daily earnings 99-03	0.084	0.0004	0.085	0.0004	-0.001	***
Growth log daily gross earnings 98-03	0.233	0.291	0.243	0.571	-0.010	
Growth log daily gross earnings 98-00	0.139	0.196	0.134	0.444	0.005	
Growth log daily gross earnings 00-03	0.093	0.088	0.103	0.306	-0.010	**
Growth log daily gross earnings 94-03	0.509	0.982	0.539	2.036	-0.030	
Observations	6,521		24,636			

Notes: The table shows mean and variance of covariates for the cross-section of workers in the year 2003 unless stated otherwise. All earnings variables are in current Euros and not adjusted for inflation. Column (5) indicates the significance of a t-test for equality of means in the two samples. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table 2: Reform Effects on Incumbent Workers

	(1)	(2)	(3)	(4)
	Log daily earnings		Unemployment	
	Non-matched	Matched	Non-matched	Matched
$\text{Deregulated}_{j(i)} \times \text{post}_{2003}$	-0.0404***	-0.0234***	0.0099***	0.0069***
	(0.0021)	(0.0063)	(0.0010)	(0.0024)
Constant	4.256***	4.206***	0.0171***	0.0172***
	(0.0016)	(0.0027)	(0.0008)	(0.0012)
R-squared	0.034	0.022	0.017	0.021
Observations	496,084	493,568	503,705	501,156
Individuals	31,327	31,157	31,327	31,157

Notes: The table shows average effects of the reform on log of gross daily earnings in Columns (1) and (2) and on being unemployed in Columns (3) and (4). Appendix Table A-5 shows yearly treatment effects. Columns (1) and (3) refer to results using the non-matched comparison group and Columns (2) and (4) refer to results using the matched comparison group. All regressions include year and individual fixed effects. Average log daily earnings are equal to 4.406 (4.351) in the non-matched (matched) sample in 2003. The unemployment rate of our sample is equal to zero in 2003 by construction and equal to 3.6% for the period from 2003 to 2014. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table 3: Reform Effect Heterogeneity by Regional Characteristics

	(1)	(2)	(3)	(4)
	Log daily earnings		Unemployment	
	Foreign	Unemployed	Foreign	Unemployed
$\text{Deregulated}_{j(i)} \times \text{post}_{2003} \times$ share foreign	0.0067		-0.0331	
	(0.1458)		(0.0506)	
$\text{Deregulated}_{j(i)} \times$ share foreign	-0.1882		-0.0634	
	(0.1414)		(0.0591)	
$\text{Post}_{2003} \times$ share foreign	-0.1606		0.0929**	
	(0.1201)		(0.0370)	
Share foreign	0.2065*		0.0776*	
	(0.1092)		(0.0404)	
$\text{Deregulated}_{j(i)} \times \text{post}_{2003} \times$ share unemployed		-0.1730*		0.0270
		(0.0995)		(0.0539)
$\text{Deregulated}_{j(i)} \times$ share unemployed		0.1860**		-0.0243
		(0.0930)		(0.0651)
$\text{Post}_{2003} \times$ share unemployed		0.0974		0.0695*
		(0.0658)		(0.0371)
Share unemployed		-0.7440***		0.3720***
		(0.0658)		(0.0474)
$\text{Deregulated}_{j(i)} \times \text{post}_{2003}$	-0.0221***	-0.0265***	0.0075***	0.0072***
	(0.0061)	(0.0065)	(0.0023)	(0.0026)
Constant	4.206***	4.186***	0.0173***	0.0285***
	(0.0027)	(0.0030)	(0.0012)	(0.0018)
R-squared	0.022	0.024	0.021	0.025
Observations	493,430	493,430	501,018	501,018
Individuals	31,157	31,157	31,157	31,157

Notes: The table shows average effects of the reform on log of gross daily earnings in Columns (1) and (2) and on being unemployed in Columns (3) and (4). *Share foreign* refers to the percentage of foreign workers at the county level in the SIAB data. *Share unemployed* refers to the percentage of unemployed individuals at the county level in the SIAB data. Regional measures are based on all individuals in the SIAB who are between 20 and 60 years old. Both measures are computed separately by year and are demeaned to facilitate interpretation. All regressions include year and individual fixed effects. Workers in the comparison group are weighted by matching weights. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table 4: Reform Effect Heterogeneity by Firm Size

	(1)	(2)	(3)	(4)
	Firm size (Number of employees)			
	Less than 20	20–100	100–250	More than 250
	Panel A: log daily earnings			
$\text{Deregulated}_{j(i)} \times \text{post}_{2003}$	-0.0451***	-0.0363***	-0.0270*	-0.0125
	(0.0140)	(0.0110)	(0.0157)	(0.0105)
Constant	4.069***	4.182***	4.186***	4.321***
	(0.0063)	(0.0046)	(0.0071)	(0.0047)
R-squared	0.012	0.017	0.031	0.055
Observations	152,366	146,645	77,975	116,582
Individuals	9,835	9,246	4,867	7,209
	Panel B: unemployment			
$\text{Deregulated}_{j(i)} \times \text{post}_{2003}$	0.0145***	0.0078*	0.0051	0.0027
	(0.0054)	(0.0046)	(0.0068)	(0.0041)
Constant	0.0302***	0.0160***	0.0162***	0.0120***
	(0.0032)	(0.0023)	(0.0031)	(0.0022)
R-squared	0.023	0.022	0.023	0.018
Observations	155,446	148,999	79,014	117,697
Individuals	9,835	9,246	4,867	7,209

Notes: The table shows average effects of the reform on log gross daily earnings in Panel A and on being unemployed in Panel B. Sample splits, indicated in the column header, are based on the average individual firm size from 2001 to 2003. All regressions include year and individual fixed effects. Entropy balancing is rerun on each subsample and workers in the comparison group are weighted by these matching weights. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table 5: Attrition from Sample

	(1)	(2)	(3)	(4)	(5)	(6)
	Dropped out		Missing earnings		Both	
	Non-matched	Matched	Non-matched	Matched	Non-matched	Matched
$\text{Deregulated}_{j(i)} \times \text{post}_{2003}$	0.0022*	0.0058*	0.0052***	0.0044**	0.0071***	0.0091**
	(0.0013)	(0.0030)	(0.0007)	(0.0018)	(0.0014)	(0.035)
Constant	0.0000	0.0000	-0.0023***	-0.0020***	0.0000	0.0000
	(0.0010)	(0.0009)	(0.0006)	(0.0005)	(0.0014)	(0.0011)
R-squared	0.089	0.092	0.019	0.021	0.098	0.103
Observations	532,559	529,669	503,705	501,156	532,559	529,669
Individuals	31,327	31,157	31,327	31,157	31,327	31,157

Notes: The table documents the extent of sample attrition. In Columns (1) and (2), the dependent variable is an indicator variable that is one if the individual dropped out from the social security records, zero otherwise. In Columns (3) and (4), the dependent variable is an indicator variable that is one if the individual reports missing/zero earnings, zero otherwise. In Columns (5) and (6), the dependent variable is an indicator variable that is one if the individual reports missing/zero earnings or dropped out of the sample, zero otherwise. Appendix Table A-13 shows the results for yearly treatments. Columns (1), (3), and (5) refer to results using the non-matched comparison group and Columns (2), (4), and (6) refer to results using the matched comparison group. All regressions include year and individual fixed effects. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table 6: Reform Effects: Accounting for Attrition

	(1)	(2)	(3)	(4)	(5)
	Imputed earnings percentile				
	10th	25th	50th	75th	90th
$\text{Deregulated}_{j(i)} \times \text{post}_{2003}$	-0.0274***	-0.0218***	-0.0184***	-0.0157***	-0.0131**
	(0.0065)	(0.0055)	(0.0053)	(0.0054)	(0.0056)
Constant	4.191***	4.191***	4.191***	4.191***	4.191***
	(0.0029)	(0.0025)	(0.0025)	(0.0026)	(0.0026)
R-squared	0.018	0.020	0.035	0.052	0.066
Observations	529,669	529,669	529,669	529,669	529,669
Individuals	31,157	31,157	31,157	31,157	31,157

Notes: The outcome variable is imputed log gross daily earnings of incumbent workers. The imputation is being done for workers who have missing earnings due to attrition (drop out of the sample or report zero/none earnings) from social security records. Missing earnings are imputed by the percentile indicated in the column header. Yearly earnings distributions are calculated based on daily earnings (including zero earnings) in the working age population (20 to 60 years old). Imputing percentiles obtained from earnings distributions that are based on log earnings yield almost identical estimates (results not shown). All regressions include year and individual fixed effects. Workers in the comparison group are weighted by matching weights. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.
Source: SIAB

Table 7: Economy-wide Reform Effects on Occupational Level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Log employment				Log daily earnings		Share foreign workers
	Total	Other occupations	Full-time	Apprentices			
$\text{Deregulated}_j \times \text{post}_{2003}$	-0.0835***	-0.1920***	-0.1010***	0.0196	-0.0105	-0.0162	-0.0032
	(0.0147)	(0.0160)	(0.0158)	(0.0459)	(0.0180)	(0.0199)	(0.0029)
$\text{Regulated}_j \times \text{post}_{2003}$		-0.1090***					
		(0.0183)					
Constant	5.692***	5.950***	5.539***	2.959***	3.967***	1.732***	0.0956***
	(0.0124)	(0.0098)	(0.0133)	(0.0388)	(0.0095)	(0.2050)	(0.0024)
Control variables	no	no	no	no	no	yes	no
R-squared	0.436	0.080	0.506	0.017	0.294	0.428	0.052
Observations	1,116	4,100	1,116	1,016	801,355	801,355	1,116
Occupations	93	342	93	90	93	93	93

Notes: The table shows regressions at the occupational level. The analytical sample is based on workers who report valid occupational codes, work in firms with less than 1,500 employees, and are between 20 and 60 years old. For regressions in Columns (1) to (4) and (7), we collapse the dataset at the occupation level. In Columns (1) to (4), the dependent variable is the log average number of workers in the occupation. Column (1) use all observations in deregulated and regulated occupations. Column (2) uses all occupations that are available. Column (3) uses only workers who are employed as regular full-time employees. Column (4) restricts the sample to apprentices. In Column (7), the dependent variable is the share of migrants within the sample of individuals with valid occupational codes. In Columns (5) and (6), the dependent variable is log daily earnings and estimation takes place on the microdata. *Control variables:* age, age squared, gender, education (8 cat.), and foreign citizenship. The analysis is restricted to the years between 1999 and 2010. All regressions include occupation and year fixed effects. Standard errors, clustered at the occupational level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.
Source: SIAB

Table 8: Reform Effects on Monthly Income and Self-Employment

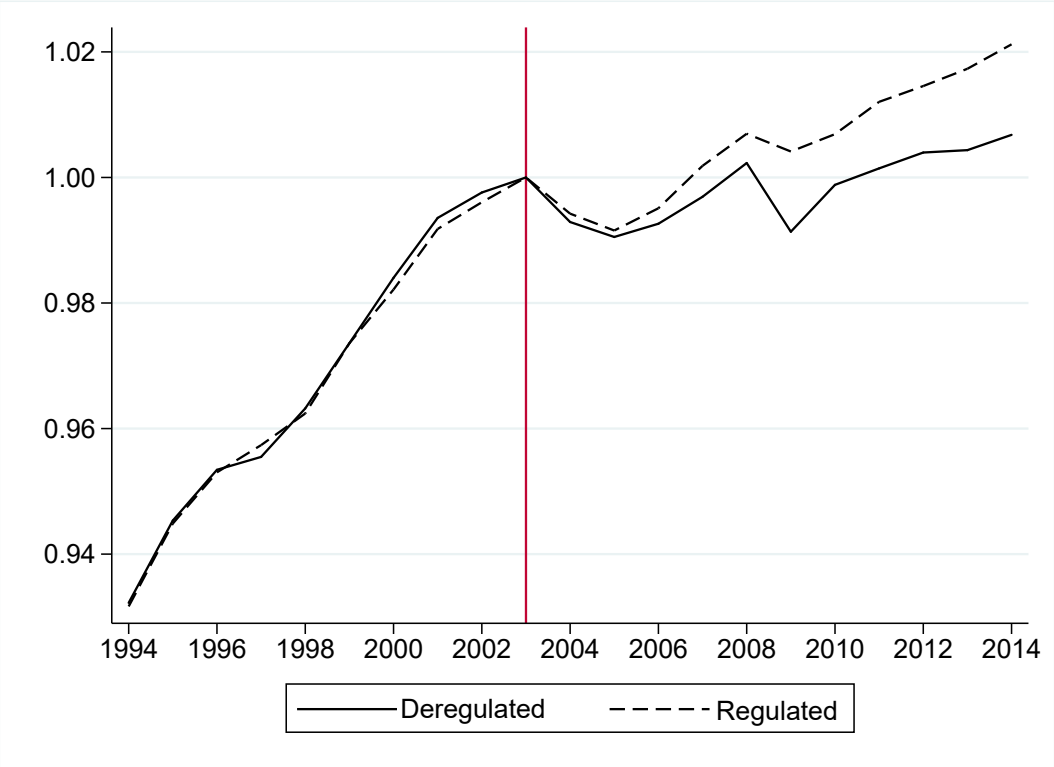
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log monthly income						Self-employed	
	All		Employees		Self-employed			
$\text{Deregulated}_{j(i)} \times \text{post}_{2003}$	0.0117	0.0084	0.0113	0.0002	-0.0326	0.0014	0.0267	0.0223
	(0.0107)	(0.0134)	(0.0108)	(0.0087)	(0.0353)	(0.0352)	(0.0199)	(0.0193)
Constant	7.071***	5.503***	7.052***	5.581***	7.355***	5.817***	0.068***	-0.613***
	(0.0045)	(0.0373)	(0.0047)	(0.0149)	(0.0874)	(0.0557)	(0.0042)	(0.1290)
Control variables	no	yes	no	yes	no	yes	no	yes
R-squared	0.102	0.252	0.136	0.320	0.067	0.111	0.0751	0.1944
Observations	451,098	304,321	396,363	260,666	54,735	43,655	476,892	322,635

Notes: The table shows reform effects on log monthly net personal income in Columns (1) to (6) and on an indicator variable that is one if the individual is self-employed and zero otherwise in Columns (7) and (8). The analytical sample is based on individuals aged between 18 and 65, working full-time, reporting a craft as gainful occupation in the respective year, and reporting labor earnings as their primary source of income. All regressions include occupation and year fixed effects. *Control variables:* age, age squared, gender, highest school degree (8 cat.), highest professional degree (7 cat.), and nationality group (3 cat.). Standard errors, clustered at the occupational level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: Research Data Centres of the Federal Statistical Office and the statistical offices of the Länder, Microcensus, census years 1998-2012.

A Appendix Figures and Tables

Figure A-1: Log Earnings Before and After the Reform Relative to 2003



Notes: The figure plots average log gross daily earnings for the sample of incumbent workers in the regulated and deregulated occupations relative to the year 2003.
Source: SIAB

Figure A-2: Effect Heterogeneity by Firm Size: Log Daily Earnings

(a) Firm Size less than 20 Employees

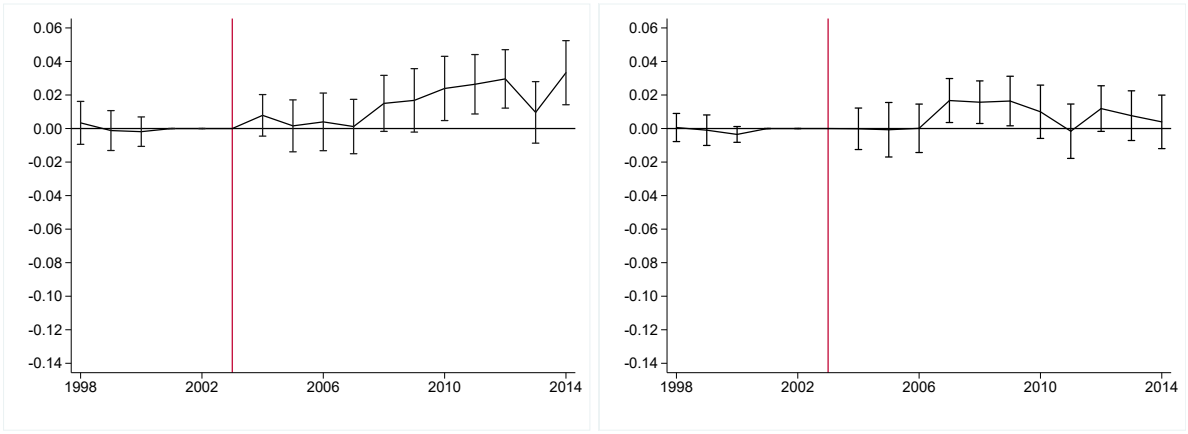
(b) Firm Size 20-100 Employees

(c) Firm Size 100-250 Employees

(d) Firm Size more than 250 Employees

Notes: The figure shows coefficient estimates and 95% confidence intervals of the reform effect on gross daily log earnings using the matched comparison group for incumbent workers from 1998 to 2014 by the size of the firm in the year 2003.
Source: SIAB

Figure A-3: Effect Heterogeneity by Firm Size: Unemployment



(a) Firm Size less than 20 Employees

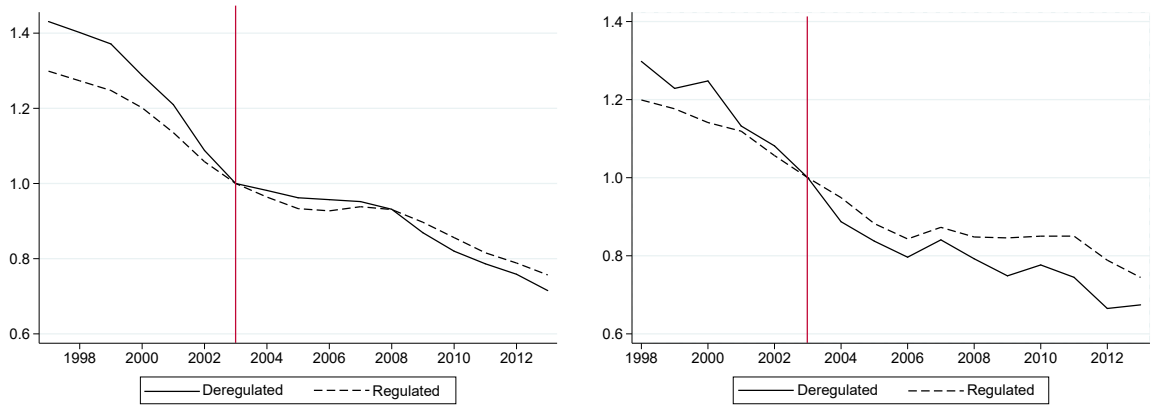
(b) Firm Size 20-100 Employees

(c) Firm Size 100-250 Employees

(d) Firm Size more than 250 Employees

Notes: The figure shows coefficient estimates and 95% confidence intervals of the reform effect on unemployment using the matched comparison group for incumbent workers from 1998 to 2014 by the size of the firm in the year 2003.
Source: SIAB

Figure A-4: Apprenticeships and Apprenticeship Examinations

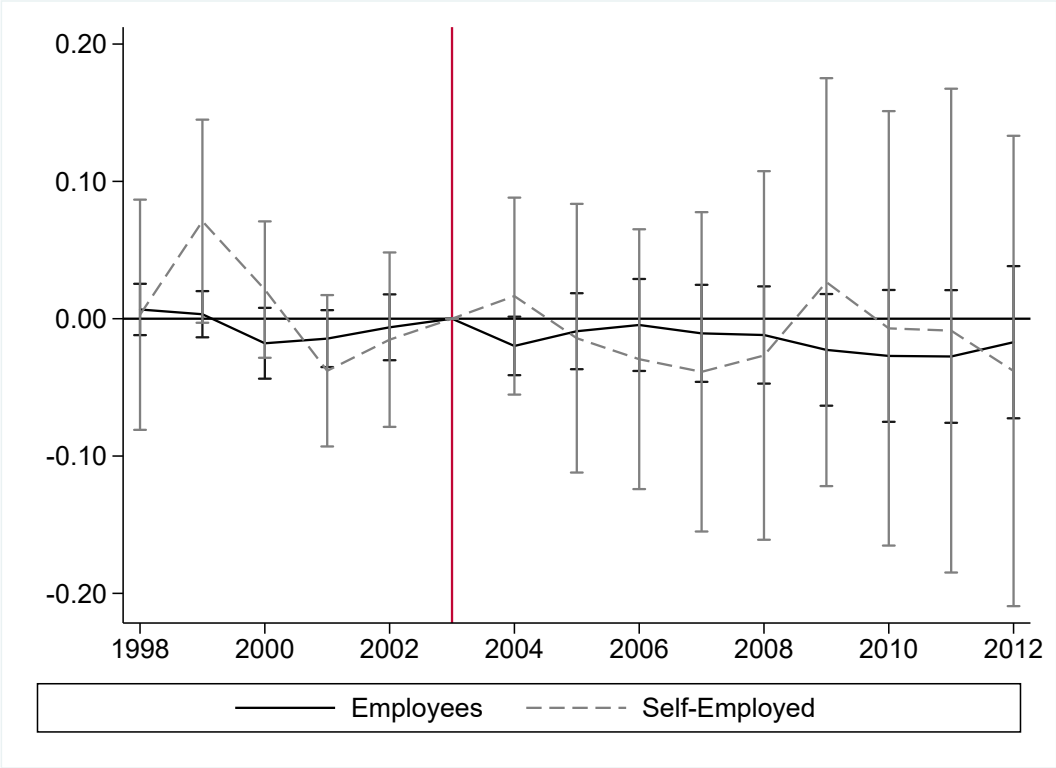


(a) Apprenticeships

(b) Apprenticeships Examinations

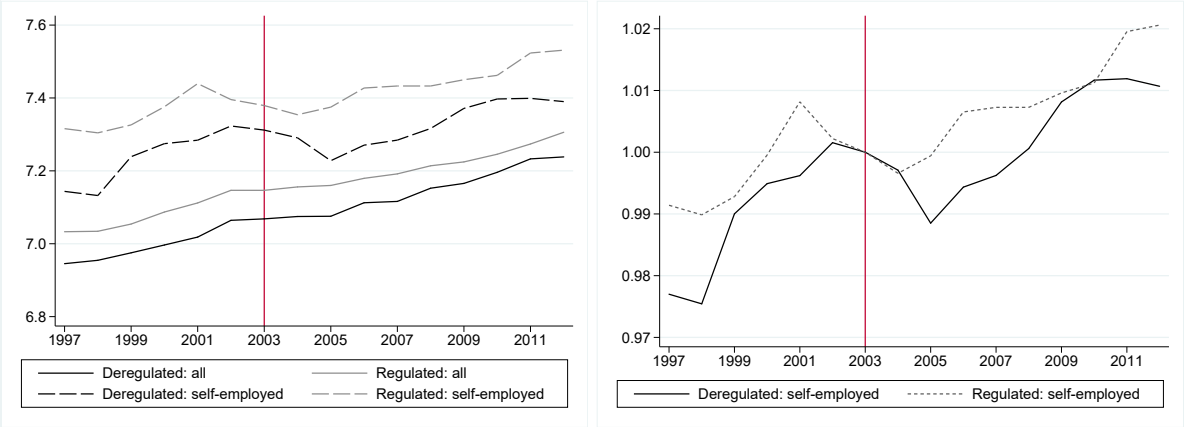
Notes: The figures show the numbers of apprenticeships (Figure (a)) and the number of apprenticeship final examinations (Figure (b)) relative to 2003. Statistics based on firm registry data.
Source: German Confederation of Skilled Crafts

Figure A-5: Effects on Net Monthly Income of Self-Employed and Employees



Notes: The figure shows coefficient estimates and 95% confidence intervals of the reform effect on log net monthly income for self-employed individuals and employees who work in craft occupations.
Source: Research Data Centres of the Federal Statistical Office and the statistical offices of the Länder, Microcensus, census years 1998-2012.

Figure A-6: Net Monthly Income of Self-Employed and Employees



(a) Log levels (b) Relative to 2003

Notes: The figure plots average log monthly personal income of employees and self-employed individuals in craft occupations. Figure (a) plots log levels for all craft workers (incl. employees and self-employed individuals) and for self-employed individuals. Figure (b) plots average income levels for self-employed individuals relative to 2003.
Source: Research Data Centres of the Federal Statistical Office and the statistical offices of the Länder, Microcensus, census years 1998-2012.

Table A-1: List of Regulated and Deregulated Craft Occupations

Deregulated occupations

No	Group	Name	No	Group	Name
1	I	Tile, slab, and mosaic layer	27	V	Miller
2	I	Cast stone and terrazzo maker	28	V	Brewer and maltster
3	I	Screed layer	29	V	Wine cellarperson
4	II	Vessel and equipment constructor	30	VI	Textile cleaner
5	II	Clockmaker	31	VI	Wax chantler
6	II	Engraver	32	VI	Glass finisher
7	II	Metal former	33	VI	Building cleaner
8	II	Galvanizer	34	VII	Glass finisher
9	II	Metal and bell founder	35	VII	Precision optician
10	II	Cutting tool mechanic	36	VII	Glass and china painter
11	II	Goldsmith and silversmith	37	VII	Precious stone engraver and cutter
12	III	Parquet layer	38	VII	Photographer
13	III	Shutter mechatronics technician	39	VII	Bookbinder
14	III	Model builder	40	VII	Typesetter and printer
15	III	Turner (ivory carver) and wooden toy maker	41	VII	Screen printer
16	III	Wood carver	42	VII	Flexographer
17	III	Cooper	43	VII	Ceramist
18	III	Basket maker	44	VII	Organ and harmonium maker
19	IV	Costume tailor	45	VII	Piano and harpsichord maker
20	IV	Embroiderer, weaver, knitter	46	VII	Reed an organ musical instrument maker
21	IV	Milliner	47	VII	Violin maker
22	IV	Sailmaker	48	VII	Bow maker
23	IV	Furrier	49	VII	Metal wind instrument maker
24	IV	Shoemaker	50	VII	Wooden wind instrument maker
25	IV	Saddler	51	VII	Plucked instrument maker
26	IV	Interior decorator	52	VII	Gilder

Regulated occupations

No	Group	Name	No	Group	Name
1	I	Bricklayer and concreter	22	II	Gunsmith
2	I	Stove and air heating mechanic	23	II	Plumber
3	I	Carpenter	24	II	Installer and heating fitter
4	I	Roofer	25	II	Electrics technician
5	I	Road construction worker	26	II	Electrical machine engineer
6	I	Thermal and acoustic insulation fitter	27	III	Joiner
7	I	Well sinker	28	III	Boat builder
8	I	Stonemason	29	IV	Rope maker
9	I	Plasterer	30	V	Baker
10	I	Painter and lacquerer	31	V	Pastry-cook
11	I	Scaffolder	32	V	Butcher
12	I	Chimney sweep	33	VI	Dispensing optician
13	II	Metal worker	34	VI	Hearing aid acoustician
14	II	Surgical instrument maker	35	VI	Orthotic technician
15	II	Coachbuilder	36	VI	Orthopaedic shoemaker
16	II	Precision engineer	37	VI	Dental technician
17	II	Motorbike and bicycle mechanic	38	VI	Hairdresser
18	II	Refrigeration mechanic	39	VII	Glazier
19	II	Communication technician	40	VII	Glass blower and glass apparatus maker
20	II	Automotive mechatronics technician	41	VII	Mechanic for tyres and vulcanization
21	II	Mechanic for agricultural machinery			

Notes: The table shows deregulated craft occupations *(zulassungsfreies Handwerk)* that enter in Annex B1 of the German Trade and Crafts Code and regulated craft occupations *(zulassungspflichtiges Handwerk)* that enter in Annex A of the German Trade and Crafts Code. The group number groups craft occupations into that perform similar tasks. Group I: building and interior finishes trades *(Bau- und Ausbauhandwerke)*. Group II: electrical and metalworking trades *(Elektro- und Metallhandwerke)*. Group III: woodcrafts and plastic trades *(Holzhandwerke)*. Group IV: clothing, textiles and leather crafts and trades *(Bekleidungs-, Textil- und Lederhandwerke)*. Group V: food crafts and trades *(Nahrungsmittelhandwerke)*. Group VI: health and body care trades as well as the chemical and cleaning sector *(Gesundheits- und Körperpflege-, chemische und Reinigungshandwerke)*. Group VII: graphic design *(Glas-, Papier-, keramische und sonstige Handwerke)*.

Table A-2: Mapping of Occupations from Crafts and Trade Code to Classification of Occupations 1988: Deregulated Occupations

Crafts and Trade Code – Annex B1		Classification of Occupations 1988 (KldB88)	
Code	Name	Code	Name
1	Fliesen-, Platten- und Mosaikleger	483	Fliesenleger
2	Betonstein- und Terrazzohersteller	112	Formstein-, Betonhersteller
3	Estrichleger	486	Estrich-, Terrazzoleger
4	Behälter- und Apparatebauer	252	Behälterbauer, Kupferschmiede und verwandte Berufe
5	Uhrmacher	286	Uhrmacher
6	Graveure	232	Graveure, Ziseleure
7	Metallbildner	193	Metallzieher
7	Metallbildner	213	Sonstige Metallverformer (spanlose Verformung)
7	Metallbildner	225	Metallschleifer
7	Metallbildner	233	Metallvergüter
7	Metallbildner	244	Metallkleber und übrige Metallverbinder
8	Galvaniseure	234	Galvaniseure, Metallfärber
9	Metall- und Glockengießer	202	Formgießer
10	Schneidwerkzeugmechaniker	291	Werkzeugmacher
11	Gold- und Silberschmiede	302	Edelmetallschmiede
12	Parkettleger	–	n/a
13	Rollladen- und Sonnenschutztechniker	627	Übrige Fertigungstechniker
14	Modellbauer	306	Puppenmacher, Modellbauer, Präparatoren
15	Drechsler (Elfenbeinschnitzer) und Holzspielzeugmacher	183	Holzwarenmacher
16	Holzbildhauer	182	Holzverformer und zugehörige Berufe
17	Böttcher	503	Stellmacher, Böttcher
18	Korb- und Flechtwerkgestalter	184	Korb-, Flechtwarenmacher
19	Maßschneider	351	Schneider
20	Textilgestalter (Sticker, Weber, Klöppler, Posamentierer, Stricker)	342	Weber
20	Textilgestalter (Sticker, Weber, Klöppler, Posamentierer, Stricker)	346	Textilverflechter
20	Textilgestalter (Sticker, Weber, Klöppler, Posamentierer, Stricker)	354	Sticker
20	Textilgestalter (Sticker, Weber, Klöppler, Posamentierer, Stricker)	352	Oberbekleidungsnäher
20	Textilgestalter (Sticker, Weber, Klöppler, Posamentierer, Stricker)	356	Näher, a.n.g.
20	Textilgestalter (Sticker, Weber, Klöppler, Posamentierer, Stricker)	357	Sonstige Textilverarbeiter
21	Modisten	355	Hut-, Mützenmacher

Notes: The table is continued on the next page.

Table A-2, cont'd

Crafts and Trade Code – Annex B1		Classification of Occupations 1988 (KldB88)	
Code	Name	Code	Name
22	Segelmacher	362	Textilausrüster
23	Kürschner	378	Fellverarbeiter
24	Schuhmacher	372	Schuhmacher
25	Sattler und Feintäschner	374	Groblederwarenhersteller, Bandagisten
25	Sattler und Feintäschner	375	Feinlederwarenhersteller
25	Sattler und Feintäschner	376	Lederbekleidungshersteller und sonstige Lederverarbeiter
26	Raumausstatter	491	Raumausstatter
27	Müller	432	Mehl-, Nährmittelhersteller
29	Brauer und Mälzer	422	Brauer, Mälzer
29	Weinküfer	421	Weinküfer
30	Textilreiniger	932	Textilreiniger, Färber und Chemischreiniger
31	Wachszieher	–	n/a
32	Gebäudereiniger	934	Glas-, Gebäudereiniger
33	Glasveredler	135	Glasbearbeiter, Glasveredler
34	Feinoptiker	135	Glasbearbeiter, Glasveredler
35	Glas- und Porzellanmaler	514	Kerammaler, Glasmaler
36	Edelsteinschleifer und -graveure	102	Eselsteinbearbeiter
37	Fotografen	837	Photographen
38	Buchbinder	163	Buchbinderberufe
39	Drucker	173	Buchdrucker (Hochdruck)
39	Drucker	174	Flach-, Tiefdrucker
40	Siebdrucker	175	Spezialdrucker, Siebdrucker
41	Flexografen	172	Druckstockhersteller
42	Keramiker	121	Keramiker
43	Orgel- und Harmoniumbauer	305	Musikinstrumentenbauer
44	Klavier- und Cembalobauer	305	Musikinstrumentenbauer
45	Handzuginstrumentenmacher	305	Musikinstrumentenbauer
46	Geigenbauer	305	Musikinstrumentenbauer
47	Bogenmacher	305	Musikinstrumentenbauer
48	Metallblasinstrumentenmacher	305	Musikinstrumentenbauer
49	Holzblasinstrumentenmacher	305	Musikinstrumentenbauer
50	Zupfinstrumentenmacher	305	Musikinstrumentenbauer
51	Vergolder	235	Emaillierer, Feuerverzinker und andere Metalloberflächenveredler
52	Schilder- und Lichtreklamehersteller	834	Dekorationen-, Schildermaler

Table A-3: Mapping of Occupations from Crafts and Trade Code to Classification of Occupations 1988: Regulated Occupations

Crafts and Trade Code – Annex A		Classification of Occupations 1988 (KldB88)	
Code	Name	Code	Name
1	Maurer und Betonbauer	441	Maurer
1	Maurer und Betonbauer	442	Betonbauer
2	Ofen- und Luftheizungsbauer	484	Ofensetzer, Luftheizungsbauer
3	Zimmerer	451	Zimmerer
4	Dachdecker	452	Dachdecker
5	Straßenbauer	462	Straßenbauer
6	Wärme-, und Kälte- und Schallschutzisolierer	482	Isolierer, Abdichter
7	Brunnenbauer	465	Kultur-, Wasserbauwerker
8	Steinmetzen und Steinbildhauer	101	Steinbearbeiter
9	Stukkateure	481	Stukkateure, Gipser, Verputzer
10	Maler und Lackierer	511	Maler, Lackierer (Ausbau)
10	Maler und Lackierer	512	Warenmaler, -lackierer
11	Gerüstbauer	453	Gerüstbauer
12	Schornsteinfeger	804	Schornsteinfeger
13	Metallbauer	301	Metallfeinbauer, a.n.g.
14	Chirurgiemechaniker	285	Sonstige Mechaniker
15	Karosserie- und Fahrzeugbauer	285	Sonstige Mechaniker
16	Feinwerkmechaniker	284	Feinmechaniker
17	Zweiradmechaniker	285	Sonstige Mechaniker
18	Kälteanlagenbauer	285	Sonstige Mechaniker
19	Informationstechniker	628	Sonstige Techniker
20	Kraftfahrzeugtechniker	621	Maschinenbautechniker
21	Landmaschinenmechaniker	621	Maschinenbautechniker
22	Büchsenmacher	211	Blechpresser, -zieher, -stanzer
23	Klempner	211	Blechpresser, -zieher, -stanzer
24	Klempner	261	Feinblechner
24	Installateur und Heizungsbauer	262	Rohrinstallateure
25	Elektrotechniker	311	Elektroinstallateure, -monteure
25	Elektrotechniker	622	Techniker des Elektofaches
26	Elektromaschinenbauer	314	Elektrogerätebauer
27	Tischler	501	Tischler
28	Boots- und Schiffbauer	275	Stahlbauschlosser, Eisenschiffbauer
29	Seiler	332	Spuler, Zwirner, Seiler
30	Bäcker	391	Backwarenhersteller
31	Konditoren	392	Konditoren
32	Fleischer	401	Fleischer
33	Augenoptiker	304	Augenoptiker
34	Hörgeräteakustiker	–	n/a
35	Orthopädietechniker	628	Sonstige Techniker
36	Orthopädieschuhmacher	–	n/a
37	Zahntechniker	303	Zahntechniker
38	Friseure	901	Friseure
39	Glaser	485	Glaser
40	Glasbläser und Glasapparatebauer	132	Hohlglasmacher
40	Glasbläser und Glasapparatebauer	133	Flachglasmacher
40	Glasbläser und Glasapparatebauer	134	Glasbläser (vor der Lampe)
41	Vulkaniseure und Reifenmechaniker	144	Vulkaniseure

Table A-4: Mapping of Occupations from Crafts and Trade Code to Classification of Occupations 1992 in the Microcensus

Deregulated		Regulated	
Crafts and Trade Code	KldB92	Crafts and Trade Code	KldB92
1. Fliesen-, Platten- und Mosaikleger	4830, 4831, 4832, 4833, 4837, 4839	1. Maurer und Betonbauer	4410, 4411, 4412, 4413, 4414, 4415, 4416, 4417, 4419, 4420, 4421, 4422, 4423, 4424, 4425
2. Betonstein- und Terrazzohersteller	1120, 1121, 1122, 1123, 1124, 1125, 1127, 1129	2. Ofen- und Luftheizungsbauer	4840, 4841, 4842, 4843, 4847
3. Estrichleger	4860, 4861, 4862, 4863, 4864, 4867, 4869	3. Zimmerer	4870, 4871, 4872, 4873, 4874, 4875, 4876, 4877, 4879
4. Behälter- und Apparatebauer	2520, 2521, 2522, 2527, 2529	4. Dachdecker	4880, 4881, 4882, 4883, 4884, 4885, 4887, 4889
5. Uhrmacher	3080, 3081, 3082, 3083, 3084, 3086, 3087, 3089	5. Straßenbauer	4610, 4611, 4612, 4613, 4614, 4615, 4617, 4619
6. Graveure	2940, 2941, 2942, 2943, 2944, 2947, 2949	6. Wärme-, Kälte- und Schall- schutzisolierer	4820, 4821, 4822, 4823, 4824, 4825, 4826, 4827, 4828, 4829
7. Metallbildner	3231, 3232, 3237, 3239, 2016	7. Brunnenbauer	4662
8. Galvaniseure	2340, 2341, 2342, 2343, 2347, 2349	8. Steinmetzen und Steinbildhauer	1010, 1011, 1012, 1013, 1014, 1015, 1016, 1017, 1018, 1019
9. Metall- und Glockengießer	2010, 2011, 2012, 2013, 2014, 2015, 2016, 2017, 2018, 2019	9. Stukkateure	4810, 4811, 4812, 4813, 4814, 4817, 4819
10. Schneidwerkzeugmechaniker	2951, 2952, 2953, 2954, 2957, 2959	10. Maler und Lackierer	5101, 5102, 5103, 5107, 5109, 5110, 5111, 5112, 5113, 5114, 5115, 5116, 5117, 5119, 5120, 5121, 5122, 5123, 5124, 5125, 5126, 5127, 5128, 5129
11. Gold- und Silberschmiede	3021, 3022	11. Gerüstbauer	4431, 4437
12. Parkettleger	4915, 4916	12. Schornsteinfeger	8041, 8042
13. Rollladen- und Sonnenschutztechniker	2591	13. Metallbauer	2540, 2541, 2542, 2543, 2547, 2549, 2550, 2551, 2552, 2553, 2557, 2559, 2560, 2561, 2562, 2563, 2567, 2591, 2599
14. Modellbauer	5021, 5022, 5023, 5024, 5025, 5026, 5027, 5028, 5029	14. Chirurgiemechaniker	8570
15. Drechsler und Holzspielzeugmacher	1851, 1855	15. Karosserie- und Fahrzeugbauer	2870, 2871, 2872, 2873, 2877, 2879
16. Holzbildhauer	1852	16. Feinwerkmechaniker	3000
17. Böttcher	5062	17. Zweiradmechaniker	2813
18. Korb- und Flechtwerkgestalter	1858	18. Kälteanlagenbauer	2661, 2662, 2667
19. Maßschneider	3510, 3511, 3512, 3513, 3514, 3515, 3516, 3517, 3518, 3519	19. Informationstechniker	3171
20. Textilgestalter (Sticker, Weber, Klöppler, Posamentierer, Stricker)	3520, 3521, 3522, 3523, 3524, 3525, 3526, 3527, 3529, 3591, 3410, 3411, 3412, 3413, 3414, 3415, 3416, 3417, 3419, 3418, 3440	20. Kraftfahrzeugtechniker	2810
21. Modisten	3541, 3542, 3543	21. Landmaschinenmechaniker	2821
22. Segelmacher	3581	22. Büchsenmacher	3003
23. Kürschner	3780, 3781, 3782, 3783, 3784, 3787, 3789	23. Klempner	2610, 2611, 2612, 2613, 2614, 2617, 2619

Notes: The table is continued on next page.

Table A-4, cont'd

Deregulated		Regulated	
Crafts and Trade Code	KldB92	Crafts and Trade Code	KldB92
24. Schuhmacher	3720, 3721	24. Installateur und Heizungsbauer	2680, 2681, 2682, 2687, 2690, 2691, 2697, 2699
25. Sattler und Feintäschner	3741, 3742, 3743, 3744, 3745, 3747	25. Elektrotechniker	6220, 6221, 6222, 6223, 6224, 6225, 6226, 6228, 6229
26. Raumausstatter	4910, 4911	26. Elektromaschinenbauer	3130, 3131, 3132, 3133, 3134, 3137, 3139
27. Müller	4351	27. Tischler	5010, 5011, 5012, 5013, 5014, 5015, 5016, 5017, 5018, 5019
28. Brauer und Mälzer	4210, 4211, 4212, 4217, 4219	28. Boots- und Schiffbauer	5063, 5064, 5065
29. Weinküfer	4233	29. Seiler	3323
30. Textilreiniger	9310, 9311, 9312, 9313, 9314, 9315, 9317, 9318, 9319	30. Bäcker	3910, 3911, 3912, 3913, 3914, 3915, 3917, 3918, 3919
31. Wachszieher	1418	31. Konditoren	3920, 3921, 3922, 3923, 3924, 3925, 3927, 3929
32. Gebäudereiniger	9340, 9341, 9342, 9343, 9349	32. Fleischer	4010, 4011, 4012, 4013, 4014, 4015, 4017, 4018, 4019
33. Glasveredler	1350,1351, 1352, 1353, 1354, 1355	33. Augenoptiker	3041
34. Feinoptiker	1356, 1357, 1358, 1359	34. Hörgeräteakustiker	3153
35. Glas- und Porzellanmaler	5140, 5141, 5142, 5143, 5144, 5145, 5146, 5147, 5149	35. Orthopädietechniker	3071
36. Edelsteinschleifer und -graveure	1018	36. Orthopädieschuhmacher	3722
37. Fotografen	8370, 8371, 8372, 8373, 8374, 8375, 8376, 8378, 8379	37. Zahntechniker	3031, 3032, 3037
38. Buchbinder	1780, 1781, 1782, 1783, 1784, 1785, 1789	38. Friseure	9010, 9011, 9012, 9013, 9014, 9015, 9016, 9017, 9018, 9019
39. Drucker	1740, 1741, 1742, 1743, 1749, 1750	39. Glaser	4850, 4851, 4852, 4853, 4854, 4855, 4856, 4857, 4859
40. Siebdrucker	1751	40. Glasbläser und Glasapparatebauer	1310, 1311, 1312, 1313, 1314, 1315, 1316, 1317, 1318, 1319
41. Flexografen	1736	41. Vulkaniseure und Reifenmechaniker	1450, 1451, 1452, 1453, 1454, 1456, 1457, 1458, 1459, 1500, 1501, 1507
42. Keramiker	1210, 1211, 1212, 1213, 1214, 1215, 1216, 1217, 1218, 1219		
43. Orgel- und Harmoniumbauer	3052		
44. Klavier- und Cembalobauer	3051		
45. Handzuginstrumentenmacher	3058		
46. Geigenbauer	3054		
47. Bogenmacher			
48. Metallblasinstrumentenmacher	3053		
49. Holzblasinstrumentenmacher	3056		
50. Zupfinstrumentenmacher	3055		
51. Vergolder	5126		
52. Schilder- und Lichtreklamehersteller	8390, 8391, 8392, 8393, 8394, 8395, 8397, 8399		

Table A-5: Yearly Treatment Effects

	(1)	(2)	(3)	(4)	(5)	(6)
	Log daily earnings		Unemployment		Attrition	
	Non-matched	Matched	Non-matched	Matched	Non-matched	Matched
$\text{Deregulated}_{j(i)} \times I(t = 2014)$	-0.0701***	-0.0431***	0.0156***	0.0091**	0.0150***	0.0039
	(0.0060)	(0.0114)	(0.0030)	(0.0043)	(0.0040)	(0.0071)
$\text{Deregulated}_{j(i)} \times I(t = 2013)$	-0.0643***	-0.0458***	0.0106***	0.00314	0.0188***	0.0071
	(0.0060)	(0.0105)	(0.0030)	(0.0042)	(0.0040)	(0.0068)
$\text{Deregulated}_{j(i)} \times I(t = 2012)$	-0.0535***	-0.0334***	0.0174***	0.0091**	0.0178***	0.0182***
	(0.0059)	(0.0101)	(0.0030)	(0.0041)	(0.0040)	(0.0063)
$\text{Deregulated}_{j(i)} \times I(t = 2011)$	-0.0525***	-0.0350***	0.0141***	0.0053	0.0114***	0.0132**
	(0.0059)	(0.0095)	(0.0029)	(0.0040)	(0.0040)	(0.0060)
$\text{Deregulated}_{j(i)} \times I(t = 2010)$	-0.0448***	-0.0224**	0.0159***	0.0086**	0.0097**	0.0144**
	(0.0059)	(0.0094)	(0.0029)	(0.0042)	(0.0040)	(0.0056)
$\text{Deregulated}_{j(i)} \times I(t = 2009)$	-0.0601***	-0.0374***	0.0190***	0.0148***	0.0067*	0.0169***
	(0.0058)	(0.0090)	(0.0029)	(0.0041)	(0.0040)	(0.0052)
$\text{Deregulated}_{j(i)} \times I(t = 2008)$	-0.0207***	-0.0180**	0.0094***	0.0093**	-0.0008	0.0103**
	(0.0058)	(0.0074)	(0.0029)	(0.0036)	(0.0040)	(0.0048)
$\text{Deregulated}_{j(i)} \times I(t = 2007)$	-0.0207***	-0.0198***	0.0081***	0.0083**	-0.0014	0.0051
	(0.0058)	(0.0076)	(0.0029)	(0.0035)	(0.0040)	(0.0044)
$\text{Deregulated}_{j(i)} \times I(t = 2006)$	-0.0101*	-0.0042	0.0042	0.0014	0.0030	0.0094**
	(0.0057)	(0.0071)	(0.0029)	(0.0037)	(0.0040)	(0.0039)
$\text{Deregulated}_{j(i)} \times I(t = 2005)$	-0.0051	0.0001	0.0017	0.0023	0.0018	0.0056*
	(0.0057)	(0.0069)	(0.0029)	(0.0034)	(0.0040)	(0.0031)
$\text{Deregulated}_{j(i)} \times I(t = 2004)$	-0.0056	-0.0114**	-0.00007	0.0026	-0.0034	-0.0033
	(0.0056)	(0.0045)	(0.0028)	(0.0027)	(0.0040)	(0.0024)
Baseline: $t = 2003$						
$\text{Deregulated}_{j(i)} \times I(t = 2002)$	0.0070	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.0056)	(0.0018)	(0.0028)	(0.0000)	(0.0040)	(0.0000)
$\text{Deregulated}_{j(i)} \times I(t = 2001)$	0.0082	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.0056)	(0.0022)	(0.0028)	(0.0000)	(0.0040)	(0.0000)
$\text{Deregulated}_{j(i)} \times I(t = 2000)$	0.0093*	0.0000	-0.0016	-0.0022	0.0000	0.0000
	(0.0056)	(0.0032)	(0.0028)	(0.0014)	(0.0040)	(0.0000)
$\text{Deregulated}_{j(i)} \times I(t = 1999)$	0.0018	0.0000	0.0019	-0.0006	0.0000	0.0000
	(0.0056)	(0.0040)	(0.0028)	(0.0020)	(0.0040)	(0.0000)
$\text{Deregulated}_{j(i)} \times I(t = 1998)$	0.0061	0.0000	0.0011	0.0014	0.0000	0.0000
	(0.0056)	(0.0047)	(0.0028)	(0.0022)	(0.0040)	(0.0000)
Constant	4.254***	4.206***	0.0169***	0.0165***	0.0000	0.0000
	(0.0020)	(0.0034)	(0.0010)	(0.0015)	(0.0014)	(0.0000)
R-squared	0.035	0.022	0.018	0.021	0.098	0.103
Observations	496,084	493,568	503,705	501,156	532,559	529,669
Individuals	31,327	31,157	31,327	31,157	31,327	31,157

Notes: The table shows yearly treatment effects of the reform on log of gross daily earnings in Columns (1) and (2), on being unemployed in Columns (3) and (4), and on attrition in Columns (5) and (6). The outcome variable is log gross daily earnings. Column (1) refers to the non-matched sample, Column (2) to the matched sample from entropy balancing. All regressions include year and individual fixed effects. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table A-6: Other Labor Market Adjustment Mechanisms

	(1)	(2)	(3)	(4)
	Full-time regular employee		Marginal employment	
	Non-matched	Matched	Non-matched	Matched
Panel A: average effects				
$\text{Deregulated}_{j(i)} \times \text{post}_{2003}$	-0.0140***	-0.0029	0.0074***	0.0026
	(0.0015)	(0.0044)	(0.006)	(0.0019)
Constant	0.942***	0.926***	-0.0008	-0.0008
	(0.0012)	(0.0021)	(0.0005)	(0.0006)
R-squared	0.050	0.058	0.016	0.020
Panel B: yearly effects				
$\text{Deregulated}_{j(i)} \times I(t = 2014)$	-0.0252***	-0.0033	0.0149***	0.0069*
	(0.0043)	(0.0076)	(0.0019)	(0.0040)
$\text{Deregulated}_{j(i)} \times I(t = 2013)$	-0.0173***	0.0042	0.0142***	0.0101***
	(0.0043)	(0.0073)	(0.0019)	(0.0035)
$\text{Deregulated}_{j(i)} \times I(t = 2012)$	-0.0239***	0.0006	0.0110***	0.0037
	(0.0043)	(0.0072)	(0.0019)	(0.0036)
$\text{Deregulated}_{j(i)} \times I(t = 2011)$	-0.0267***	-0.0019	0.0098***	0.0025
	(0.0043)	(0.0071)	(0.0019)	(0.0034)
$\text{Deregulated}_{j(i)} \times I(t = 2010)$	-0.0209***	-0.0056	0.0086***	0.00007
	(0.0042)	(0.0066)	(0.0019)	(0.0033)
$\text{Deregulated}_{j(i)} \times I(t = 2009)$	-0.0242***	-0.0149**	0.0107***	0.0041
	(0.0042)	(0.0062)	(0.0019)	(0.0031)
$\text{Deregulated}_{j(i)} \times I(t = 2008)$	-0.0112***	-0.0091*	0.0067***	0.0015
	(0.0042)	(0.0055)	(0.0018)	(0.0025)
$\text{Deregulated}_{j(i)} \times I(t = 2007)$	-0.0072*	-0.0067	0.0056***	0.0009
	(0.0042)	(0.0053)	(0.0018)	(0.0024)
$\text{Deregulated}_{j(i)} \times I(t = 2006)$	-0.0004	-0.0006	0.0033*	-0.0004
	(0.0041)	(0.0051)	(0.0018)	(0.0022)
$\text{Deregulated}_{j(i)} \times I(t = 2005)$	0.0041	0.0011	0.0014	-0.0003
	(0.0041)	(0.0046)	(0.0018)	(0.0019)
$\text{Deregulated}_{j(i)} \times I(t = 2004)$	0.0032	-0.0014	0.0009	0.0014
	(0.0041)	(0.0034)	(0.0018)	(0.0013)
Baseline: $t = 2003$				
$\text{Deregulated}_{j(i)} \times I(t = 2002)$	0.0010	-0.0014	0.0000	0.0000
	(0.0041)	(0.0013)	(0.0018)	(0.0000)
$\text{Deregulated}_{j(i)} \times I(t = 2001)$	0.0001	-0.0017	0.0000	0.0000
	(0.0041)	(0.0016)	(0.0018)	(0.0000)
$\text{Deregulated}_{j(i)} \times I(t = 2000)$	0.0020	0.0003	0.0005	-0.0002
	(0.0041)	(0.0028)	(0.0018)	(0.0006)
$\text{Deregulated}_{j(i)} \times I(t = 1999)$	0.0004	0.0005	0.0004	-0.0001
	(0.0041)	(0.0035)	(0.0018)	(0.0006)
$\text{Deregulated}_{j(i)} \times I(t = 1998)$	0.0035	-0.0011	0.0000	–
	(0.0041)	(0.0041)	(0.0018)	–
Constant	0.942***	0.9270***	-0.0008	-0.0008
	(0.0014)	(0.0029)	(0.0006)	(0.0006)
R-squared	0.050	0.058	0.016	0.020
Observations	503,705	501,156	503,705	501,156
Individuals	31,327	31,157	31,327	31,157

Notes: The table shows reform effects on being in full-time regular employment in Columns (1) and (2) and on being in marginal employment in Columns (3) and (4) from linear probability models. Marginal employment is recorded from the year 1999 onwards in the SIAB data. Columns (1) and (3) refer to results using the non-matched comparison group and Columns (2) and (4) refer to results using the matched comparison group. All regressions include year and individual fixed effects. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table A-7: Average Earnings Growth After Matching

(1)	(2)	(3)	(4)	(5)
Period	Deregulated	Regulated	Difference	
			Δ	p-value
1998-2003	0.233	0.234	-0.001	0.858
1998-2000	0.139	0.136	0.003	0.673
2000-2003	0.093	0.096	-0.003	0.556
1994-2003	0.510	0.523	-0.013	0.544

Notes: The table shows average growth rates of log daily gross earnings after matching.
Source: SIAB

Table A-8: Checking Common Trends in Pretreatment Periods

	(1)	(2)	(3)	(4)
	Baseline sample		New sample	
	Non-matched	Matched	Non-matched	Matched
	Panel A: average effects			
$\text{Deregulated}_{j(i)} \times \text{post}_{1998}$	0.0010	0.0042	0.0043**	0.0056
	(0.0019)	(0.0041)	(0.0019)	(0.0040)
Constant	4.030***	3.991***	4.141***	4.088***
	(0.0013)	(0.0029)	(0.0012)	(0.0018)
R-squared	0.245	0.240	0.049	0.069
	Panel B: yearly effects			
$\text{Deregulated}_{j(i)} \times I(t = 2003)$	-0.0061	0.0000	-0.0162***	-0.0003
	(0.0043)	(0.0047)	(0.0045)	(0.0028)
$\text{Deregulated}_{j(i)} \times I(t = 2002)$	0.0008	0.0000	-0.0087*	-0.0028
	(0.0043)	(0.0043)	(0.0045)	(0.0061)
$\text{Deregulated}_{j(i)} \times I(t = 2001)$	0.0020	0.0000	0.0047	0.0111**
	(0.0043)	(0.0042)	(0.0045)	(0.0049)
$\text{Deregulated}_{j(i)} \times I(t = 2000)$	0.0032	0.0000	0.0029	0.0099**
	(0.0043)	(0.0041)	(0.0044)	(0.0039)
$\text{Deregulated}_{j(i)} \times I(t = 1999)$	-0.0042	0.0000	-0.0010	0.0042
	0.0043	(0.0037)	(0.0044)	(0.0031)
Baseline: $t = 1998$				
$\text{Deregulated}_{j(i)} \times I(t = 1997)$	-0.0111**	-0.0100**	-0.0074*	0.0000
	0.0044	(0.0040)	(0.0044)	(0.0012)
$\text{Deregulated}_{j(i)} \times I(t = 1996)$	-0.0059	-0.0062	-0.0080*	0.0000
	0.0044	(0.0048)	(0.0044)	(0.0016)
$\text{Deregulated}_{j(i)} \times I(t = 1995)$	-0.0048	-0.0081	-0.0070	0.0000
	0.0044	(0.0059)	(0.0044)	(0.0026)
$\text{Deregulated}_{j(i)} \times I(t = 1994)$	-0.0018	-0.0022	-0.0166***	0.0000
	0.0045	(0.0070)	(0.0044)	(0.0036)
$\text{Deregulated}_{j(i)} \times I(t = 1993)$	0.0136***	0.0013	-0.0082*	0.0000
	0.0045	(0.0075)	(0.0044)	(0.0041)
Constant	4.027***	3.990***	4.142***	4.088***
	(0.0016)	(0.0047)	(0.0015)	(0.0024)
R-squared	0.245	0.240	0.049	0.069
Observations	332,153	330,383	374,522	357,047
Individuals	31,327	31,157	34,924	32,945

Notes: The table checks common trends in pretreatment periods. The dependent variable in all regressions is log gross daily earnings. Columns (1) and (2) use the same analytical sample that we use in our baseline regression without further restrictions. Columns (3) and (4) use a new analytical sample, pretending that the reform has happened in the year 1998. Matching is based on the same set of covariats as in the baseline analysis. All regressions include year and individual fixed effects. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table A-9: Labor Market Effects for Incumbent Workers: Dropping Sample Restrictions

	(1)	(2)	(3)	(4)	(5)	(6)
	Earnings		Unemployment		Attrition	
	Non-matched	Matched	Non-matched	Matched	Non-matched	Matched
$\text{Deregulated}_{j(i)} \times \text{post}_{2003}$	-0.0353***	-0.0246***	0.0059***	0.0045*	0.0012	0.0043
	(0.0021)	(0.0064)	(0.0010)	(0.0026)	(0.0014)	(0.0036)
Constant	4.113***	4.135***	0.0476***	0.0421***	0.0130***	0.0000
	(0.0017)	(0.0032)	(0.0008)	(0.0017)	(0.0011)	(0.0011)
R-squared	0.034	0.016	0.011	0.013	0.051	0.106
Observations	812,246	657,423	836,388	671,225	914,090	711,382
Individuals	53,770	41,846	53,770	41,846	53,770	41,846

Notes: The table shows results from regressions on the sample that includes occupational and firm switches before the reform and non-regular employees (including unemployment and apprenticeship spells). All regressions include year and individual fixed effects. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table A-10: Effect Heterogeneity by Industry Group

	(1)	(2)	(3)	(4)
	Industry group			
	Manufacturing	Construction	Trade, maintenance, and repair	Real estate, renting, and other business activities
	Panel A: log daily earnings			
$\text{Deregulated}_{j(i)} \times \text{post}_{2003}$	-0.0222***	-0.0469**	-0.0408*	-0.0119
	(0.0070)	(0.0187)	(0.0240)	(0.0326)
Constant	4.261***	4.178***	4.026***	3.909***
	(0.0030)	(0.0092)	(0.0114)	(0.0133)
R-squared	0.026	0.016	0.013	0.015
Observations	232,001	136,192	41,630	83,745
Individuals	14,475	8,726	2,627	5,329
	Panel B: unemployment			
$\text{Deregulated}_{j(i)} \times \text{post}_{2003}$	0.0055**	0.0096	0.0184*	0.0069
	(0.0026)	(0.0086)	(0.0094)	(0.0132)
Constant	0.0138***	0.0288***	0.0259***	0.0283***
	(0.0013)	(0.0050)	(0.0053)	(0.0059)
R-squared	0.020	0.026	0.024	0.028
Observations	235,033	138,910	42,206	85,007
Individuals	14,475	8,726	2,627	5,329
	Panel C: attrition			
$\text{Deregulated}_{j(i)} \times \text{post}_{2003}$	0.0042	0.0491***	0.0187	0.0270*
	(0.0039)	(0.0148)	(0.0144)	(0.0162)
Constant	0.0000	0.0000	0.0000	0.0000
	(0.0012)	(0.0047)	(0.0046)	(0.0052)
R-squared	0.095	0.151	0.119	0.125
Observations	246,075	148,342	44,659	90,593
Individuals	14,475	8,726	2,627	5,329

Notes: The table shows average effects of the reform on log gross daily earnings in Panel A, on being unemployed in Panel B, and on dropping out of the sample (incl. missing/zero earnings) in Panel C. Sample splits, indicated in the column header, are based on the industry group in the year 2003. *Trade, maintenance, and repair* contains wholesale and retail trade, repair of motor vehicles, motorcycles and personal and household goods. Due to small samples, *real estate, renting, and other business activities* also contains other industries that we do not further distinguish. All regressions include year and individual fixed effects. Entropy balancing is rerun on each subsample and workers in the comparison group are weighted by these matching weights. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table A-11: Reform Effects by Occupational and Firm Switches

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Same			Log daily earnings			
				Occupation		Firm	
	Occupation	Occupational group	Firm	Same	Different	Same	Different
$\text{Deregulated}_{j(i)} \times \text{post}_{2003}$	0.0060	0.0134***	-0.0040	-0.0098**	-0.0428**	-0.0119***	-0.0396***
	(0.0050)	(0.0047)	(0.0056)	(0.0043)	(0.0174)	(0.0045)	(0.0136)
Constant	0.0865***	0.0691***	0.1270***	4.238***	4.119***	4.234***	4.157***
	(0.0025)	(0.0023)	(0.0027)	(0.0025)	(0.0063)	(0.0027)	(0.0051)
R-squared	0.105	0.090	0.160	0.095	0.045	0.121	0.028
Observations	355,020	355,020	353,079	357,464	136,104	292,099	201,469
Individuals	31,157	31,157	31,157	21,529	9,628	17,544	13,613

Notes: In Column (1), the dependent variable is an indicator variable that is one if the worker has the same occupation as in the year 2003 and zero if the worker has changed the occupation. In Column (2), the dependent variable is an indicator variable that is one if the worker is in the same occupational group (deregulated occupation versus regulated occupation) as in the year 2003 and zero if the worker has changed the occupation group. In Column (3), the dependent variable is an indicator variable that is one if the worker works for the same firm as in the year 2003 and zero if the worker has changed the firm. The analysis in these two columns is restricted to the years 1999 to 2010 because of non-comparable occupational classifications in the other years. In Columns (4) to (7), the dependent variable is log daily gross earnings. Columns (4) and (5) split the sample by whether the worker works in the same or in a different occupation in 2010 compared to 2003. Columns (6) and (7) split the sample by whether the worker works in the same or in a different firm in 2010 compared to 2003. All regressions include year and individual fixed effects. Workers in the comparison group are weighted by matching weights. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table A-12: Excluding Occupations with Similar Tasks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Deregulated occupations:	Tile, slab and mosaic layer and cast stone and terrazzo maker		Metal former, galvanizer, and metal and bell founder		Interior decorator		All	
Regulated occupations:	Bricklayer and concreter		Metal worker		Installer and heating fitter		All	
	Earnings	Unemployed	Earnings	Unemployed	Earnings	Unemployed	Earnings	Unemployed
$\text{Deregulated}_{j(i)} \times \text{post}_{2003}$	-0.0231***	0.0067***	-0.0339***	0.0102***	-0.0238***	0.0068***	-0.0353***	0.0104***
	(0.0066)	(0.0025)	(0.0063)	(0.0024)	(0.0063)	(0.0024)	(0.0068)	(0.0026)
Constant	4.201***	0.0171***	4.189***	0.0169***	4.210***	0.0169***	4.186***	0.0162***
	(0.0028)	(0.0013)	(0.0027)	(0.0013)	(0.0027)	(0.0012)	(0.0030)	(0.0013)
R-squared	0.023	0.020	0.020	0.021	0.022	0.021	0.021	0.021
Observations	453,326	460,035	477,717	485,070	461,223	468,362	405,130	411,155
Indivudals	28,563	28,563	30,172	30,172	29,109	29,109	25,530	25,530

Notes: The table shows regressions for log daily earnings and unemployment in subsamples that drop occupations indicated in the column header. Columns (7) and (8) drop all occupations mentioned in the other columns. All regressions include year and individual fixed effects. Entropy balancing is rerun on each subsample and workers in the comparison group are weighted by these matching weights. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table A-13: Attrition from Sample: Yearly Treatment Results

	(1)	(2)	(3)	(4)	(5)	(6)
	Dropped out		Missing earnings		Both	
	Non-matched	Matched	Non-matched	Matched	Non-matched	Matched
$\text{Deregulated}_{j(i)} \times I(t = 2014)$	0.00755**	0.00051	0.0088***	0.0041	0.0150***	0.0039
	(0.0037)	(0.0067)	(0.0023)	(0.0034)	(0.0040)	0.0071
$\text{Deregulated}_{j(i)} \times I(t = 2013)$	0.0121***	0.0045	0.0079***	0.0036	0.0188***	0.0071
	(0.0037)	(0.0064)	(0.0022)	(0.0033)	(0.0040)	0.0068
$\text{Deregulated}_{j(i)} \times I(t = 2012)$	0.0096***	0.0168***	0.0096***	0.0033	0.0178***	0.0182***
	(0.0037)	(0.0058)	(0.0022)	(0.0034)	(0.0040)	0.0063
$\text{Deregulated}_{j(i)} \times I(t = 2011)$	0.0071*	0.0125**	0.0049**	0.0022	0.0114***	0.0132**
	(0.0037)	(0.0055)	(0.0022)	(0.0033)	(0.0040)	0.0060
$\text{Deregulated}_{j(i)} \times I(t = 2010)$	0.0004	0.0085*	0.0098***	0.0071**	0.0097**	0.0144**
	(0.0037)	(0.0049)	(0.0022)	(0.0035)	(0.0040)	0.0056
$\text{Deregulated}_{j(i)} \times I(t = 2009)$	-0.0013	0.0107**	0.0082***	0.0079**	0.0067*	0.0169***
	(0.0037)	(0.0045)	(0.0022)	(0.0031)	(0.0040)	0.0052
$\text{Deregulated}_{j(i)} \times I(t = 2008)$	-0.0078**	0.0034	0.0069***	0.0080**	-0.0008	0.0103**
	(0.0037)	(0.0039)	(0.0022)	(0.0031)	(0.0040)	0.0048
$\text{Deregulated}_{j(i)} \times I(t = 2007)$	-0.0018	0.0049	0.0008	0.0018	-0.0014	0.0051
	(0.0037)	(0.0035)	(0.0022)	(0.0029)	(0.0040)	0.0044
$\text{Deregulated}_{j(i)} \times I(t = 2006)$	-0.0005	0.0018	0.0037*	0.0082***	0.0030	0.0094**
	(0.0037)	(0.0029)	(0.0021)	(0.0027)	(0.0040)	0.0039
$\text{Deregulated}_{j(i)} \times I(t = 2005)$	-0.0003	0.0012	0.0022	0.0046**	0.0018	0.0056*
	(0.0037)	(0.0021)	(0.0021)	(0.0022)	(0.0040)	0.0031
$\text{Deregulated}_{j(i)} \times I(t = 2004)$	-0.0004	-0.0009	-0.0030	-0.0023*	-0.0034	-0.0033
	(0.0037)	(0.0020)	(0.0021)	(0.0013)	(0.0040)	0.0024
Baseline: $t = 2003$						
$\text{Deregulated}_{j(i)} \times I(t = 2002)$	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.0037)	(0.0000)	(0.0021)	(0.0000)	(0.0040)	(0.0000)
$\text{Deregulated}_{j(i)} \times I(t = 2001)$	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.0037)	(0.0000)	(0.0021)	(0.0000)	(0.0040)	(0.0000)
$\text{Deregulated}_{j(i)} \times I(t = 2000)$	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.0037)	(0.0000)	(0.0021)	(0.0000)	(0.0040)	(0.0000)
$\text{Deregulated}_{j(i)} \times I(t = 1999)$	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.0037)	(0.0000)	(0.0021)	(0.0000)	(0.0040)	(0.0000)
$\text{Deregulated}_{j(i)} \times I(t = 1998)$	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.0037)	(0.0000)	(0.0021)	(0.0000)	(0.0040)	(0.0000)
Constant	0.0000	0.0000	-0.0023***	-0.0020***	0.0000	0.0000
	(0.0010)	(0.0009)	(0.0007)	(0.0005)	(0.0014)	(0.0011)
R-squared	0.089	0.092	0.019	0.021	0.098	0.103
Observations	532,559	529,669	503,705	501,156	532,559	529,669
Individuals	31,327	31,157	31,327	31,157	31,327	31,157

Notes: The table documents the extent of sample attrition. In Columns (1) and (2), the dependent variable is an indicator variable that is one if the individual dropped out from the social security records, zero otherwise. In Columns (3) and (4), the dependent variable is an indicator variable that is one if the individual reports missing/zero earnings, zero otherwise. In Columns (5) and (6), the dependent variable is an indicator variable that is one if the individual reports missing/zero earnings or dropped out of the sample, zero otherwise. All regressions include year and individual fixed effects. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table A-14: Earnings of Workers Dropping Out of the Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	2004–2014		2004–2006		2004–2008		2009–2014	
Outsample_i	-0.1138***	-0.1157***	-0.1500***	-0.1769***	-0.1486***	-0.1646***	-0.1033***	-0.1018***
	(0.0082)	(0.0123)	(0.0159)	(0.0261)	(0.0117)	(0.0179)	(0.0084)	(0.0124)
$\text{Outsample}_i \times \text{Deregulated}_{j(i)}$		0.0037		0.0517		0.0315		-0.0029
		(0.0165)		(0.0320)		(0.0234)		(0.0169)
$\text{Deregulated}_{j(i)}$		-0.0007		-0.0030		-0.0037		0.0014
		(0.0089)		(0.0078)		(0.0080)		(0.0089)
Constant	4.388***	4.388***	4.362***	4.364***	4.371***	4.373***	4.380***	4.380***
	(0.0044)	(0.0067)	(0.0038)	(0.0057)	(0.0040)	(0.0059)	(0.0044)	(0.0067)
R-squared	0.017	0.017	0.010	0.010	0.015	0.016	0.013	0.013

Notes: The dependent variable is log gross daily earnings. Sample is restricted to the year 2003. *Outsample* is an indicator variable that is one if the worker disappears from the analysis (due to dropping out from the records or due to missing and zero earnings), zero otherwise. In Columns (1) and (2), we consider all dropouts from the sample over all years from 2004 to 2014. In Columns (3) and (4), we consider early dropouts from the sample over the first two years from 2004 to 2006. In Columns (5) and (6), we consider early dropouts from the sample over the years from 2004 to 2008. In Columns (7) and (8), we consider late dropouts from the sample over the years from 2009 to 2014. $N = 31,157$. Workers in the comparison group are weighted by matching weights. Robust standard errors in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table A-15: Earnings of Workers Dropping Out of the Sample: Controlling for Individual Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	All dropouts: 2004–2014			Early dropouts: 2004–2006		
Outsample_i	-0.1039***	-0.0651***	-0.0573***	-0.1696***	-0.1139***	-0.1048***
	(0.0118)	(0.0111)	(0.0110)	(0.0251)	(0.0214)	(0.0209)
$\text{Outsample}_i \times \text{Deregulated}_{j(i)}$	0.0104	0.0151	0.0292**	0.0629**	0.0511*	0.0714***
	(0.0160)	(0.0148)	(0.0144)	(0.0310)	(0.0267)	(0.0255)
$\text{Deregulated}_{j(i)}$	-0.0210	-0.0427**	-0.0225	-0.0208	-0.0413**	-0.0207
	(0.0206)	(0.0192)	(0.0259)	(0.0197)	(0.0190)	(0.0259)
Constant	4.289***	4.481***	4.405***	4.261***	4.472***	4.398***
	(0.0170)	(0.0157)	(0.0198)	(0.0163)	(0.0156)	(0.0199)
Education control variables	yes	yes	yes	yes	yes	yes
Personal control variables	no	yes	yes	no	yes	yes
Job control variables	no	no	yes	no	no	yes
R-squared	0.055	0.319	0.319	0.051	0.319	0.384

Notes: The dependent variable is log gross daily earnings. Sample is restricted to the year 2003. *Outsample* is an indicator variable that is one if the worker disappears from the analysis (due to dropping out from the records or due to missing and zero earnings), zero otherwise. Columns (1) to (3) consider all dropouts from the sample over all years from 2004 to 2014. Columns (4) to (6) consider early dropouts from the sample over the first two years from 2004 to 2006. Control characteristics include the linear effect of the control variable and interactions between the control variable and *deregulated*. *Education control variables:* training (4 cat.) and schooling (3 cat.). *Personal control variables:* age (demeaned), age (demeaned) squared, gender, foreign citizenship. *Age* is demeaned to facilitate interpretation. *Job control variables:* industry (5 cat.), job tenure (4 cat.), and firm tenure (4 cat.). $N = 31,157$. Workers in the comparison group are weighted by matching weights. Robust standard errors in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB

Table A-16: Treatment Effects with Matching on Craft Groups

	(1)	(2)	(3)
	Earnings	Unemployment	Attrition
	Panel A: average effects		
$\text{Deregulated}_{j(i)} \times \text{post}_{2003}$	-0.0347***	0.0086	0.0097
	(0.0108)	(0.0053)	(0.0067)
Constant	4.205***	0.0169***	0.0000
	(0.0045)	(0.0025)	(0.0021)
R-squared	0.024	0.021	0.105
	Panel B: yearly effects		
$\text{Deregulated}_{j(i)} \times I(t = 2014)$	-0.0756***	0.0085	0.0008
	(0.0156)	(0.0084)	0.0139
$\text{Deregulated}_{j(i)} \times I(t = 2013)$	-0.0577***	0.0022	0.0041
	(0.0202)	(0.0094)	0.0133
$\text{Deregulated}_{j(i)} \times I(t = 2012)$	-0.0416**	0.0020	0.0224**
	(0.0186)	(0.0113)	0.0109
$\text{Deregulated}_{j(i)} \times I(t = 2011)$	-0.0419**	0.0054	0.0152
	(0.0172)	(0.0087)	0.0166
$\text{Deregulated}_{j(i)} \times I(t = 2010)$	-0.0324*	0.0169**	0.0154
	(0.0184)	(0.0077)	0.0103
$\text{Deregulated}_{j(i)} \times I(t = 2009)$	-0.0519***	0.0191***	0.0130
	(0.0164)	(0.0071)	0.0098
$\text{Deregulated}_{j(i)} \times I(t = 2008)$	-0.0164	0.0127**	0.0101
	(0.0159)	(0.0055)	0.0086
$\text{Deregulated}_{j(i)} \times I(t = 2007)$	-0.0317***	0.0051	0.0063
	(0.0094)	(0.0078)	0.0075
$\text{Deregulated}_{j(i)} \times I(t = 2006)$	-0.0154	0.0076	0.0121*
	(0.0111)	(0.0068)	0.0065
$\text{Deregulated}_{j(i)} \times I(t = 2005)$	-0.0157*	0.0095*	0.0084*
	(0.0086)	(0.0055)	0.0050
$\text{Deregulated}_{j(i)} \times I(t = 2004)$	-0.0180***	0.0040	-0.0013
	(0.0054)	(0.0048)	0.0023
Baseline: $t = 2003$			
$\text{Deregulated}_{j(i)} \times I(t = 2002)$	0.0000	0.0000	0.0000
	(0.0018)	(0.0000)	(0.0000)
$\text{Deregulated}_{j(i)} \times I(t = 2001)$	0.0000	0.0000	0.0000
	(0.0025)	(0.0000)	(0.0000)
$\text{Deregulated}_{j(i)} \times I(t = 2000)$	0.0000	-0.0008	0.0000
	(0.0040)	(0.0018)	(0.0000)
$\text{Deregulated}_{j(i)} \times I(t = 1999)$	0.0000	-0.0023	0.0000
	(0.0065)	(0.0047)	(0.0000)
$\text{Deregulated}_{j(i)} \times I(t = 1998)$	0.0000	0.0022	0.0000
	(0.0065)	(0.0041)	(0.0000)
Constant	4.205***	0.0158***	0.0000
	(0.0063)	(0.0040)	(0.0021)
R-squared	0.024	0.021	0.105
Observations	493,568	501,156	529,669
Individuals	31,157	31,157	31,157

Notes: The table shows average effects of the reform when matching on craft groups instead of industries. All regressions include year and individual fixed effects. Workers in the comparison group are weighted by matching weights. Standard errors, clustered at the individual level, in parenthesis. Significance level: * p<0.10, ** p<0.05, *** p<0.01.

Source: SIAB