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# Does occupational licensing impact incomes? A replication study for the German crafts case

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**Abstract**

Large variation in the estimated income premium of occupational licensing can be found in the existing literature. I revisit the natural experimental design of a change in the German crafts regulation in 2004, which removed the traditional licensing requirement for self-employment in certain trades, using official survey data and difference-in-differences estimation. Previous studies of this deregulation have found significant, yet small effects on the incomes of employees in deregulated trades. I focus on the incomes of the self-employed and find no robust effects. Multiple channels through which occupational licensing may affect incomes such as price and quality competition in the regulated market and possible competitive pressure from outsiders are identified, which may also explain why the effects of occupational licensing on incomes appear to be context-specific.

**Keywords:** Labor markets, Occupational licensing, Monopoly rent, Natural experiment, Craftsmanship

**JEL Classification:** I28, I39, J24, L51

## 1 Introduction

### 1.1 Background

Occupational licensing (i.e. requiring minimum levels of human capital investment by professionals) is the strictest form of regulation to access professions, which blocks market access for individuals without the necessary credentials. National occupational licensing schemes have been steadily on the rise since the Second World War, in both Europe and the US (see Kleiner, 2006; Kleiner and Krueger 2010).

In the German crafts as the sector of interest in this paper, the German regulator liberalized the traditional occupation licensing scheme in 2004. With the aim of fostering labor market flexibility and employment,[1] the rule stipulating that only craftsmen holding a Meister title (an advanced vocational training certificate) could found a company was removed or alleviated for a number of trades.

The reform was in line with European institutions' efforts to create a common market area that also includes the free flow of labor. National labor market regulations, and licensing in particular, have been found to disproportionately negatively affect the labor market prospects of minorities (see Dorsey 1983; Federman et al. 2006; Gomez et al. 2015; and McDonald et al. 2015). Indeed, Runst (2018) shows that the liberalization of the crafts regulation in 2004 has increased the proportion of self-employed migrants as well as migrants employed in the crafts sector.

It should here be mentioned that while the traditional licensing scheme in the crafts sector discouraged foreigners from settling in Germany, it encouraged them to use the freedom of establishment lied down in the Treaty establishing the European Community (TEC). Indeed,

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[1] For an explanation of the aims of the reform, see e.g. Deutscher Bundestag (2003).

the Monopolies Commission[2] pointed out in 2001 that domestic crafts companies were penalized as foreign non-Meister companies could supply crafts services in Germany without the costs accompanying the qualification. Hence, removing the occupational licensing scheme actually alleviated two forms of discrimination embedded in the pre-reform regulatory framework.

Nonetheless, the 2004 reform has had its opponents. Studies have pointed out that removing the Meister obligation has caused a decline in the level of vocational training (Runst and Thomä 2019). It may have also caused a decline in quality (Runst et al. 2018a) in the presence of information asymmetries that may (Fredriksen et al. 2019) or may not (Rupieper and Proeger 2019) be resolved through market endogenous mechanisms.

Importantly for this paper, already in its 2001 report, the Monopolies Commission pointed out that removing the Meister obligation in the German crafts could reduce prices and incomes. Should this be the case, there is a strong incentive to lobby for perpetuating licensing requirements (see Rottenberg 1962). Therefore, it is worth striving for an informed policy debate on how the Meister obligation (and its suspension) actually affect incomes, which is the aim of the present paper.

There is already a large body of literature exploring the link between occupational licensing and incomes. One may therefore appropriately question whether another study on the topic is necessary. This main contribution of this study is its replicative character. The value of replication studies has long been subject to discussion and it is not difficult to find evidence of their usefulness (see e.g. Leimer and Lesnoy 1982; Dewald et al. 1986; Antonovics and Goldberger 2005). I thereby consider that replications do not exclusively serve to overturn or confirm existing findings, but that they are also useful to add new aspects to discussions in the economic literature.

### 1.2 Literature review and replicative contribution

The studies reviewed for this paper mostly examine licensing practices in the US and Germany (see summary in tabular format in Appendix). Kleiner (2006) notes that there are considerable variations in the findings and according to Redbird (2017) the estimated wage premium varies from 0 to 35%. For the German crafts case, the literature finds a noticeable income difference of 13% between regulated and deregulated trades [see Bol (2014)] using qualified professions economy-wide as a control group, and negative yet small effects of the deregulation in 2004 on earnings [see Damelang et al. (2018), Lergetporer et al. (2018)].

An important limitation of empirical studies on occupational licensing is that it is difficult for researchers to construct a suitable comparison group. In most cases, being subject to occupational licensing is not random, meaning that effect estimates will be biased unless all other factors influencing individual income are controlled for. However, it is difficult to perfectly control for individual human-level characteristics and differences in occupation-level productivity.

Empirical studies on the effects of occupational vary in their methodological approach to this challenge. Some estimate differences in national cross-section incomes between licensed and unlicensed professions (e.g. Bol and Weeden 2015; Redbird 2017), whereas others focus on a specific occupation and exploit geographical differences in licensing practices (e.g. Kleiner and Kudrle 2000; Timmons and Thornton 2008, 2013). The latter approach is less vulnerable to unobserved heterogeneity arising from occupation specificities, yet unobserved heterogeneity may still be present due to state effects or other individual-specific effects.

In a few cases, researchers have tried to improve the counterfactual at hand. Ingram (2018) uses matching and Lergetporer et al. (2018) use entropy balancing to achieve a treatment and control group with similar characteristics. As a result, the size of their estimates is reduced, which could indicate that some unobserved heterogeneity biasing the results was thus removed. Bol (2014) tries to solve the problem of unobserved heterogeneity by constructing three occupational-level indicators of skill requirements (physical abilities, technical skills, and complex mental processing skills), which are based on a workforce survey conducted by the Federal Institute for Vocational Education and Training and the Federal Institute for Occupational Safety and Health. It is difficult to judge whether this correction is successful. It is also worth noting that only differences in productivity between occupations are thus captured, leaving differences between individuals unaddressed.

The German crafts reform appears particularly well suited to studying the effects of occupational licensing, since the deregulation in 2004 resembles a natural experiment. Since the reform, market entry in certain trades[3] has been open. However, in the remaining trades,

[2] The Monopolies Commission is a permanent, independent expert committee that advises the German government and legislature in the areas of competition policy-making, competition law, and regulation.

[3] Examples of fully-deregulated trades are tiles and mosaic layers, coppersmiths and tailors.

the licensing requirement remains fully[4] or partially[5] intact,[6] which in theory makes for the perfect control group. Hence, difference-in-differences estimation can be used, which eliminates the problem of unobserved heterogeneity, given a key assumption that is discussed in part 2.3 of this paper.

One study to adopt this approach is Damelang et al. (2018), focusing on the indirect effects of removing the occupational licensing criteria for the self-employed on the wages of employees. The underlying theory is that economic rents that might arise with occupational licensing will (given certain conditions) be shared between employers and employees. The authors estimate a two-way fixed effect model using official survey data comprising a two per cent random sample of all employees subject to social security contributions in Germany (the SIAB). According to their findings, employees in the deregulated trades experienced an average wage loss of 0.65%[7] as a result of the deregulation. As the authors hypothesized, they find that this effect is concentrated among older and unskilled workers who they claim to have worse labor market prospects and hence lower bargaining power against their employer. The authors explain the low magnitude of the estimate compared with e.g. Bol (2014) by the fact that they investigate effects on the income of employees and not the self-employed, while they also only study within-subject changes.

Lergetporer et al. (2018) also examine the reform effects with difference-in-differences estimation on the wages of employed craftsmen using the SIAB dataset. They find that over the period from 2004 to 2014, workers in deregulated occupations experienced a negative average effect on their earnings of approximately 2.3% as a result of the deregulation. The authors note that the reform effect appears with a lag.

Despite being stronger than the effects found in Damelang et al. (2018), the authors still characterize the size of their estimates as "rather modest". This is confirmed when the authors use official survey data (microcensus) to gage the effects on both self-employed and employed. In this case, the estimated reform effect on the incomes of employees is even slightly positive, whereas that for the self-employed is negative but insignificant.

Lergetporer et al. (2018) offer several explications for the weak reform effect. Two general theses stand out, namely that the new firms that entered as the market barrier in the crafts market declined did not pose a real threat to incumbents, and German labor market institutions (minimum wage and collective bargaining agreement) both reduce and delay reductions in wages.

Finally, inspired by existing studies on the income effects of the 2004 deregulation on employees and the self-employed, Sonntag and Lutter investigate the following three hypotheses about the effects of the reform through difference-in-differences estimates using microcensus data from 2002 to 2007: (1) the reform caused a more pronounced decline in the incomes of deregulated self-employed craftsmen compared with their employees; (2) it negatively affected the income development of employees with a Meister title in deregulated crafts; whereas (3) it positively affected it for employees without a Meister title in the same crafts. However, their results are not in line with their prior expectations. The reform appears to have neither affected the incomes of the self-employed nor employees. In terms of different effects among employees, their models do not show consistent findings, and hence the authors are hesitant to draw clear conclusions.

In this study, I again exploit the natural experiment provided by the German craft sector to shed light on the link between occupational licensing and incomes using difference-in-differences estimation. Hamermesh (2007) distinguishes between two types of replications: 'pure replications' as duplicates of an existing scientific experiment, and 'scientific replications' that may use a different sample, different population, and perhaps a similar but not identical model to examine the same question. Since Lergetporer et al. (2018), Damelang et al. (2018) and Sonntag and Lutter (2018) analyze the link between occupational licensing and incomes in the German crafts, this study clearly has a scientific replicative character.

Whether one can speak of a pure replication is debatable. In contrast to Damelang et al. (2018), who explore the effects of the 2004 reform on the incomes of employees, I focus solely on the incomes of the self-employed. As such, this study deepens the second part in Lergetporer et al. (2018) as well as Sonntag and Lutter (2018), who also looks at income effects on the self-employed. However, some differences between these studies and mine still exist. For identifying the crafts, I use the same method as Sonntag and Lutter (2018),[8] which is somewhat different

[4] Examples of fully-regulated trades are optometrists, orthopedic and dental technicians.

[5] Examples of partially-regulated trades are roofers, gunsmiths and plumbers.

[6] It was the political intention that occupations considered hazardousness and/or providing a significant contribution vocational training in Germany should remain regulated. However, the minutes of the negotiations also provide evidence of interest group lobbying (see Bundestag 2011; Bulla 2012).

[7] Meaning that an employee in the reformed segment who earned €2000 before the reform saw his/her wage decrease to €1987 (i.e. a decrease of €13) following the reform.

[8] However, I fully align with the method defined in Runst et al. (2018a, b) and exclude the occupation of 'cleaners' from the analysis, whereas Sonntag and Lutter (2018) only do so in one (not the main) specification.

from Lergetporer et al. (2018). The time span considered also varies between my study and the existing literature (as well as between existing studies). Finally, although all studies use difference-in-differences, they consider slightly different specifications. Particularly notable is the choice of covariates, which implicitly affects the interpretation of the reform effect obtained. Whereas I focus on incomes pocketed, other studies examine the reform's effects on incomes for a given work effort.

The subsequent section outlines the methodology that I employ to study the income effects of removing occupational licensing in the German crafts and details the respective strengths and weaknesses of the data analysis. The final section of the paper reveals the results of the estimation and discusses possible explanations for the empirical findings.

## 2 Methods

### 2.1 Dataset

I use the microcensus dataset for 2000–2010, which is a representative official sample survey of the German population.[9] This dataset has many attractive features for economic researchers: one percent of the German population (approximately 800,000 individuals) are sampled, a broad range of economic and socio-demographic variables are covered, the data collection is executed by statistical officials through personal interviews and a response is compulsory for most questions. The latter two features contribute to a very low unit non-response rate of 3%.

The usual problems with data collection for income studies still hold for the microcensus. Notably, the very top or very bottom of the distribution have a smaller chance of being included in income surveys and well-off individuals have incentives to under-report their income (see Hoeller et al. 2012). Nevertheless, the personal character of the data collection should limit these biases in the case of the microcensus. Furthermore, the extremes of the income distribution are probably less relevant when studying the craft sector.

In order to assess the implications of a particular policy change in the crafts sector, it is paramount that especially the treatment group only comprises individuals within this sector. As highlighted in the introduction, like most other datasets the microcensus does not contain a separate crafts variable. This is particularly troublesome in the case of the German crafts, as Sonntag and Lutter (2018) highlight that there is no general definition of what 'the crafts' are. Perhaps unsurprisingly, Runst et al. (2018b) show that identifying the crafts solely based on occupation is too broad: while it certainly includes many of the occupations that German craftsmen would practice, it also contains a large proportion of non-crafts individuals who are unaffected by the policy reform. The inclusion of non-craftsmen is especially pronounced in the treatment group.

I therefore use the method proposed in Runst et al. (2018b) for identifying the crafts, which combines the occupation codes in the microcensus in line with the KldB 1992[10] with data from the Federal Institute for Vocational Education and Training on the share of crafts apprentices within each occupational code. Only occupations for which this share exceeds 60% are considered as crafts, which excludes individuals in the agricultural, industrial or service sectors of the economy that are unaffected by changes in crafts legislation.[11]

As highlighted in the introduction, I restrict my analysis to the self-employed. Before as well as after the reform, craftsmen could seek employment regardless of their professional degree in all trades. The traditional licensing requirement concerns only craftsmen founding a company.

I distinguish between men and women as well as between part- and full-time workers, as labor market studies tend to focus on homogeneous groups with respect to labor market participation (see e.g. Becker and Blossfeld 2017). Furthermore, I exclude monthly net incomes below 300 Euros (which are likely only noise) from the full-time working sample.

Finally, the total sample comprises 30,691 observations, of which 17% work in a fully-deregulated trade, which form the treatment group. According to official German craft statistics ("*Handwerkszahlung*"), my sample accounts for 7% of the total German craft population, where the share of workers in a deregulated trade amounts to 20%. Partially-deregulated and fully-regulated trades form the control group. Table 1 provides a descriptive summary of the sample in this study.

Over the period considered, the average craftsman in Germany received 2066 Euros net per month. As Fig. 1 clearly shows, for all of the sub-population considered and across the entire time span considered, craftsmen in deregulated trades have on average lower incomes than those working in the still-regulated trades.

[9] For detailed information about the microcensus, see e.g. Schwarz (2001).

[10] The Classification of Occupations 1992 issue (KldB 92) is a version of the 1975 issue of the occupations classification, updated by the Federal Statistical Office. Further information can be found at: https://www.klassifikationsserver.de/klassService/jsp/variant/variantInfo.jsf. (Accessed 13.09.2019).

[11] The classification scheme in the microcensus (KldB1992) merges about seven activity profiles related to cleaning into one code (934). According to the crafts classification scheme recently developed by the Federal Employment Agency (BAA, 2014), only three of these seven occupations belong to the crafts sector. Like Runst et al. (2018b), I therefore do not include cleaners in the analysis.

**Table 1 Descriptive statistics of the variables by treatment and control group. Source: Microcensus 2000–2010**

| | Treatment group | | | | Control group | | | |
|---|---|---|---|---|---|---|---|---|
| | Pre-treatment | | Post-treatment | | Pre-treatment | | Post-treatment | |
| | Mean | St.d. | Mean | St.d. | Mean | St.d. | Mean | St.d. |
| Individual net total income | 1907.81 | 1678.78 | 1817.93 | 1559.85 | 2139.98 | 2120.46 | 2090.67 | 1750.11 |
| Age | 44.91 | 10.29 | 44.48 | 9.80 | 44.29 | 9.95 | 44.96 | 9.42 |
| Female | 0.28 | 0.41 | 0.24 | 0.43 | 0.12 | 0.33 | 0.13 | 0.34 |
| Migrant | 0.08 | 0.28 | 0.12 | 0.32 | 0.04 | 0.21 | 0.07 | 0.25 |
| Hours worked | 48.40 | 14.86 | 46.37 | 13.97 | 52.18 | 14.59 | 50.76 | 13.14 |
| Part-time | 0.07 | 0.26 | 0.12 | 0.33 | 0.03 | 0.17 | 0.05 | 0.21 |
| Time in current job | 12.62 | 12.62 | 11.32 | 9.85 | 12.75 | 10.29 | 12.18 | 9.61 |
| Number of employees | 1.26 | 2.32 | 2.20 | 2.32 | 2.00 | 3.01 | 3.62 | 3.01 |
| Last labor market status | | | | | | | | |
| Unemployed | 0.01 | 0.07 | 0.02 | 0.14 | 0.01 | 0.08 | 0.01 | 0.11 |
| Employed | 0.41 | 0.49 | 0.90 | 0.30 | 0.43 | 0.50 | 0.93 | 0.26 |
| Student | 0.00 | 0.06 | 0.01 | 0.07 | 0.00 | 0.02 | 0.00 | 0.04 |
| Other/missing | 0.57 | 0.50 | 0.01 | 0.11 | 0.56 | 0.50 | 0.05 | 0.23 |
| Marital status | | | | | | | | |
| Single | 0.23 | 0.42 | 0.26 | 0.44 | 0.18 | 0.39 | 0.21 | 0.41 |
| Married | 0.66 | 0.47 | 0.63 | 0.48 | 0.74 | 0.44 | 0.69 | 0.46 |
| Widow(er) | 0.02 | 0.13 | 0.02 | 0.12 | 0.01 | 0.11 | 0.01 | 0.10 |
| Divorced | 0.09 | 0.29 | 0.10 | 0.30 | 0.07 | 0.25 | 0.09 | 0.28 |
| Number of children | | | | | | | | |
| None | 0.64 | 0.48 | 0.62 | 0.49 | 0.57 | 0.50 | 0.59 | 0.49 |
| One child | 0.17 | 0.38 | 0.19 | 0.39 | 0.20 | 0.40 | 0.20 | 0.40 |
| Two children | 0.15 | 0.35 | 0.16 | 0.36 | 0.18 | 0.39 | 0.17 | 0.37 |
| Three + children | 0.05 | 0.21 | 0.04 | 0.19 | 0.06 | 0.23 | 0.05 | 0.21 |
| General education | | | | | | | | |
| Lower secondary school | 0.57 | 0.50 | 0.46 | 0.50 | 0.66 | 0.48 | 0.58 | 0.49 |
| Intermediate secondary school | 0.23 | 0.42 | 0.28 | 0.45 | 0.24 | 0.43 | 0.29 | 0.46 |
| University entrance qualification | 0.20 | 0.40 | 0.26 | 0.44 | 0.11 | 0.31 | 0.13 | 0.34 |
| Vocational qualification | | | | | | | | |
| None | 0.03 | 0.16 | 0.03 | 0.18 | 0.02 | 0.12 | 0.02 | 0.14 |
| Vocational training | 0.49 | 0.50 | 0.57 | 0.50 | 0.31 | 0.46 | 0.38 | 0.48 |
| Advanced vocational training | 0.42 | 0.42 | 0.31 | 0.46 | 0.65 | 0.48 | 0.58 | 0.49 |
| University | 0.07 | 0.08 | 0.10 | 0.29 | 0.03 | 0.16 | 0.02 | 0.15 |
| Yearly dummies | | | | | | | | |
| 2000 | 0.15 | 0.36 | N/A | N/A | 0.85 | 0.36 | N/A | N/A |
| 2001 | 0.17 | 0.37 | N/A | N/A | 0.83 | 0.37 | N/A | N/A |
| 2002 | 0.17 | 0.38 | N/A | N/A | 0.83 | 0.38 | N/A | N/A |
| 2003 | 0.16 | 0.37 | N/A | N/A | 0.84 | 0.37 | N/A | N/A |
| 2004 | 0.16 | 0.37 | N/A | N/A | 0.84 | 0.37 | N/A | N/A |
| 2005 | N/A | N/A | 0.18 | 0.39 | N/A | N/A | 0.81 | 0.39 |
| 2006 | N/A | N/A | 0.19 | 0.39 | N/A | N/A | 0.81 | 0.39 |
| 2007 | N/A | N/A | 0.18 | 0.38 | N/A | N/A | 0.82 | 0.39 |
| 2008 | N/A | N/A | 0.20 | 0.40 | N/A | N/A | 0.80 | 0.40 |
| 2009 | N/A | N/A | 0.18 | 0.39 | N/A | N/A | 0.81 | 0.39 |
| 2010 | N/A | N/A | 0.20 | 0.40 | N/A | N/A | 0.80 | 0.40 |
| N (max–min) | 2331–2913 | 2331–2913 | 2929–3488 | 2929–3488 | 2818–12,018 | 2818–12,018 | 2929–15,061 | 2929–15,061 |

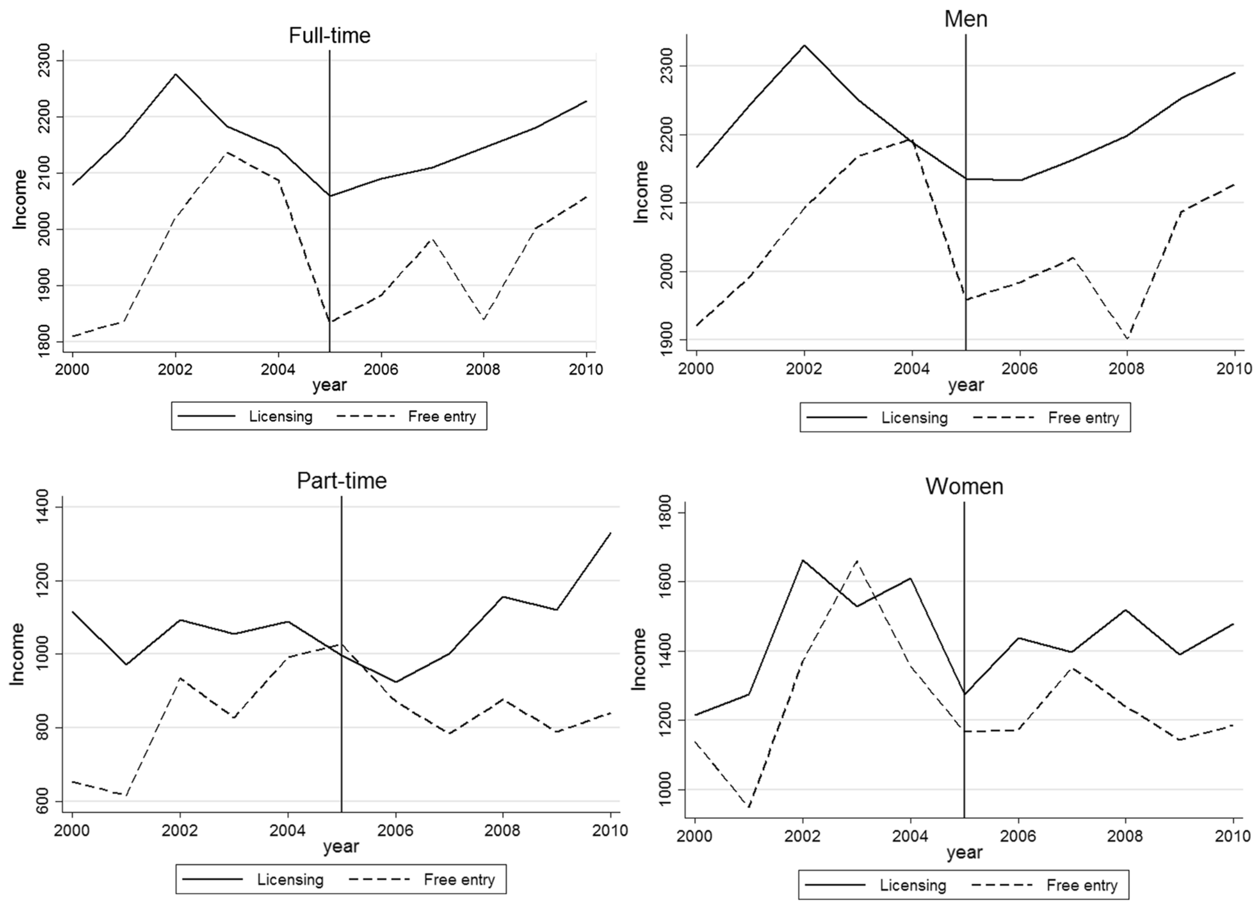


**Fig. 1** Evolution of incomes of self-employed (male, female, full-time and part-time) in the treatment and control group. Software: Stata 14.0 (Source: Microcensus 2000–2010)

Furthermore, a visual interpretation of Fig. 1 might suggest that deregulated occupations were more strongly affected by certain developments around the time of the 2004 reform than trades subject to occupational licensing for the full-time sample and the male sample. However, incomes among men and full-time workers were already on a downward-sloping trend as the reform took place in both deregulated and still-regulated trades No particular development around 2004 can be observed for the female and part-time sample. The rest of the paper is dedicated to investigating these intuitions using regression analysis.

## 2.2 Estimation strategy

By using difference-in-differences, researchers can benefit from the quasi-experimental research design of economic reforms that only affect certain groups. The average treatment effect of the treated is then calculated by comparing the average change over time in the outcome variable for the treatment group with the average change over time for the control group. I favor this approach to examine the income effects of the German crafts reform, whereby I estimate the following regression:

$$Y_{it} = \beta_1 + \beta_2(\text{treat}_i) + \beta_3(\text{time}_t) + \rho(\text{treat}_i \cdot \text{time}_t) + \gamma(\mathbf{X}\text{ vector of controls}) + \varepsilon_{it} \tag{1}$$

The dependent variable is the natural logarithm of monthly individual income. The treatment group is the fully-deregulated B-trades. Trades that are still fully or partially subject to occupational licensing form the control group. The reform effect is the interaction term between the treatment groups and time after 2004. Years after 2004 is chosen since microcensus data is collected in April every year and it is unlikely that any income effects of the reform—which entered into force on January 1, 2004—would have manifested during the first quarter of this year.

A concern in difference-in-differences analysis is that the policy may have been anticipated by the concerned individuals and consequently they would have adjusted their behavior in the pre-treatment years. However, anticipation effects are very unlikely in the case of the German crafts deregulation. For a long time, it was uncertain which crafts trades were to be deregulated. The governing coalition presented a reform proposal in 2003, which was criticized and rejected by the Federal Assembly, the upper house of the German parliament. As a result, the final discussion in the mediation committee occurred on December 10, 2003, after which many of the trades intended for deregulation would remain regulated or only be partially deregulated (Runst 2018).

The vector of control variables includes general individual attributes, namely age, age squared, gender, having a migration background, state and city size. It also includes human capital measures for the highest general education obtained and vocational qualification.[12] I also control for a number of labor market attributes, namely the number of years spent in the current position, company size, occupation, previous labor market status as well as previous occupation. In addition, household characteristics are included, namely marital status and the number of children. The errors are clustered by occupation, as suggested by Bertrand et al. (2004).

Note that I do not control for hours worked since I consider that any change in hours worked to compensate for income developments affects is part of the reform effect to be estimated. By contrast, other studies that either control for hours worked (Sonntag and Lutter 2018) or use income per hours worked as the outcome variable (Damelang et al. 2018) obtain estimates that portray impacts on economic rents as a result of the reform.

Average effects may hide interesting developments for particular sub-populations. The construction sector may be a special case, in particular with respect to the impact of the business cycle. Therefore, I run two separate regressions: one including only construction occupations and one excluding all construction occupations. I also look more closely at the floor-tiling occupation due to its non-negligible size (20% of all construction craftsmen). Finally, I look at a sample comprising only Meister companies, as a means of looking at the effects of the reform on market incumbents. The existing literature on this reform investigating heterogeneous effects mostly find significant income reductions only for male crafts (see Lergetporer et al. 2018). Hence, these regressions focus on the full-time male craftsmen sample.

In order to test the robustness of my results, I also estimate the baseline regression with alternative control groups. One obvious choice here is individuals outside the crafts sector, as they were clearly not affected by the 2004 deregulation in the crafts sector. Additionally, I estimate an alternative specification where the partially-deregulated trades are removed from the control group. In the partially-deregulation trades, experienced employees without a Meister title can start a business since the deregulation. Moreover, a potential business owner without a sufficient degree was enabled to hire a company manager who possesses a Meister degree to start the business. As such, the reform could have had repercussions for these trades. However, in the baseline regressions, they are included in the control group since they are still strongly regulated and without them the crafts control group would be very small.

Finally, I also estimate one specification with yearly interaction terms, which is able to pick up lagged effects as it is possible to consider a situation in which reform effects take time to materialize. For instance, Lergetporer et al. (2018) find that significant income effects of the 2004 reform only appear with a 5-year lag.

### 2.3 Shortcomings of the empirical approach

Just like existing studies on the German crafts deregulation have their shortcomings, the approach adopted in this paper also has a number of methodological issues. The income variable in the microcensus survey is problematic in two respects. First, individual income in the microcensus is reported in 24 intervals and hence not as specific as could be wished. This character of the data will prevent picking up small changes in individual incomes over time.

In order to achieve a metric scale for the income variable, I use a method described by the Leibniz Institute for Social Sciences[13] where I assign the mid-point of the respective income interval for each observation. For the unbounded first and last interval, I assign 1.5 times the interval's lower bound and 0.75 times the upper bound, respectively. The more that incomes are normally distributed within the intervals, the more accurate that this transformation will be.

Second, the microcensus income variable only contains information on total net income and therefore no information on labor income. This is unfortunate since the reform would have only affected labor income and using total income will likely understate the reduction in income after the reform, given that public transfers will partly compensate for the reduction. Another negative implication of the absence of an earnings variable is that unobserved factors influencing other income sources

[12] The German terms regarding education/degree have been translated using a guide from the Federal Institute for Vocational Education and Training according to Batzel (2017).

[13] In: GESIS (Hg.): Mikrodaten-Informationssystem MISSY.

**Table 2 Main results, no covariates. Source: Microcensus 2000–2010**

| | (1) Men full-time | (2) Men part-time | (3) Women full-time | (4) Women part-time |
|---|---|---|---|---|
| Reform effect | − 0.052** (0.026) | − 0.057 (0.051) | − 0.096 (0.080) | − 0.13* (0.057) |
| $R^2$ | 0.00 | 0.00 | 0.00 | 0.01 |
| N | 24,083 | 660 | 3472 | 825 |

*, ** correspond to 10, 5, and 1 percent levels of statistical significance

may bias the results if they affect craft trades differently. In order to address this issue, I follow the approach in previous studies[14] and only include respondents in the sample who reported that labor earnings are the primary source of their net individual income.

Furthermore, since the microcensus survey is a repeated cross-section dataset that does not track individual workers over time, there is reason to fear that unobserved heterogeneity will bias the results. In this regard, the SIAB dataset used by Lergetporer et al. (2018) and Damelang et al. (2018) is more appropriate since its panel structure can better address unobserved heterogeneity at the individual level. However, this data cannot be used to examine the direct reform effects on the self-employed, which is the objective of this paper.

The advantage of difference-in-differences estimation is that it can still yield causal estimates, even without having a perfectly-randomized experiment, individual-level data or observing all of the relevant explanatory variables. However, the difference-in-differences estimator can only circumvent the problem of unobserved heterogeneity if the so-called parallel trends assumption holds. Difference-in-differences therefore only give a casual effect of a given policy intervention when both the treatment and control group would have followed the same trends over time in the absence of the treatment. This assumption fails when non-observable factors (either an exogenous shock or changes in the composition of the groups) affect the average incomes in the treatment and control groups differently.

Since the parallel assumption depends on the non-treatment outcome for the treatment group after an intervention—which by definition is not observable—it is in fact non-testable. Therefore, a major concern in the policy evaluation is whether the parallel trends assumption is plausible in practice (Ryan et al. 2015).

The most basic means of assessing the likelihood of parallel trends is to look at the plotted data in the pre-treatment period. In this case, it is unclear whether the treatment and control groups follow identical trends in the pre-treatment period (Fig. 1). For the male and full-time samples, while the general evolution pre-treatment appears similar, the rise in incomes at the onset of the 2000s appeared earlier in the regulated trades. For the female and part-time sample, it seems even more questionable that the parallel trends assumption holds.

This descriptive approach should be complemented by formal calculations. As is standard for difference-in-differences analyses, I also investigate the likelihood of the parallel trends assumption being verified through a placebo test regression, where I exclude all observations in the post-reform period and simulate a treatment in the years preceding the reform.

A final means of addressing the parallel trends problematic is a descriptive search of the data for changes in covariates between the pre- and post-treatment period that are dissimilar between the treatment and control group. In my case, this does not seem to be the case (see Table 1). Only the gender dummy and the variable for having a university entrance qualification are possibly problematic. Since I estimate my regressions separately for male and female samples, I am not worried about the change in the gender composition between the treatment and control group. In order to address the possible effect of the unobserved change in qualifications, I estimate the main regression (not presented in the paper) with a dummy that interacts the relevant qualification variable with the post-treatment dummy. The results do not change.

## 3 Results

### 3.1 Main findings

A first estimation without covariates shows negative effects of the reform that are significant for the male full-time working sample (see Table 2), in line with what Sonntag and Lutter (2018) also find. However, given that the problems caused by the cross-sectional nature of the microcensus data discussed in the previous section are exaggerated in this case, I do not place much faith in this specification.

In line with Sonntag and Lutter (2018) and contrary to what some other studies in the literature on occupational licensing may suggest, I find no immediate indication of a

[14] E.g. Bol (2014) and Lergetporer et al. (2018).

**Table 3 Main results, with covariates. Source: Microcensus 2000–2010**

| | (1) Men full-time | (2) Men part-time | (3) Women full-time | (4) Women part-time |
|---|---|---|---|---|
| Reform effect | −0.0071 (0.025) | 0.19 (0.17) | 0.096* (0.052) | 0.032 (0.063) |
| Deregulated trade | −0.13*** (0.021) | −0.35* (0.17) | −0.41*** (0.073) | −0.048 (0.19) |
| Post 2004 | 0.12*** (0.016) | −0.29* (0.15) | 0.15*** (0.038) | 0.051 (0.094) |
| Age | 0.027*** (0.005) | −0.012 (0.027) | 0.039*** (0.001) | −0.025 (0.018) |
| Age square | −0.00032*** (0.000) | 0.00011 (0.000) | −0.00050*** (0.000) | 0.00027 (0.000) |
| Migrant | −0.071*** (0.021) | −0.057 (0.092) | −0.070 (0.050) | −0.0091 (0.12) |
| Time in current job | 0.0069*** (0.000) | 0.0084** (0.004) | 0.0084*** (0.002) | 0.0065*** (0.001) |
| Number of employees | 0.045*** (0.002) | 0.011 (0.023) | 0.0031** (0.006) | 0.0020 (0.009) |
| Last labor market status | | | | |
| Unemployed | Comparison | Comparison | Comparison | Comparison |
| Employed | 0.088*** (0.032) | −0.17 (0.17) | 0.18*** (0.049) | 0.16 (0.11) |
| Student | −0.18*** (0.054) | −0.20 (0.25) | −0.20 (0.51) | 0.31* (0.15) |
| Other | 0.0029 (0.094) | −0.49 (0.35) | 0.21 (0.12) | −0.20* (0.10) |
| Marital status | | | | |
| Single | Comparison | Comparison | Comparison | Comparison |
| Married | 0.12*** (0.012) | 0.13 (0.095) | −0.016 (0.028) | −0.23*** (0.052) |
| Widow(er) | 0.048 (0.057) | 0.16 (0.21) | 0.21*** (0.058) | 0.24 (0.179) |
| Divorced | 0.038* (0.020) | −0.18 (0.15) | 0.11*** (0.024) | 0.045 (0.074) |
| Number of children | | | | |
| None | Comparison | Comparison | Comparison | Comparison |
| One child | 0.048*** (0.014) | −0.61 (0.13) | −0.026 (0.024) | −0.043 (0.050) |
| Two children | 0.11*** (0.014) | −0.13 (0.078) | −0.041 (0.029) | 0.0034 (0.053) |
| Three + children | 0.13*** (0.020) | −0.24 (0.29) | 0.13 (0.11) | 0.14* (0.074) |
| General education | | | | |
| Lower secondary school | Comparison | Comparison | Comparison | Comparison |
| Intermediate secondary school | 0.063*** (0.010) | 0.14 (0.13) | 0.058*** (0.020) | 0.084 (0.058) |
| University entrance qualification | 0.0061 (0.014) | 0.076 (0.13) | 0.050 (0.072) | 0.24* (0.13) |
| Vocational qualification | | | | |
| None | Comparison | Comparison | Comparison | Comparison |
| Vocational training | 0.010 (0.035) | 0.93 0.29) | −0.064 (0.075) | 0.36*** (0.069) |
| Advanced vocational training | 0.028 (0.041) | 0.21 (0.29) | −0.030 (0.073) | 0.32*** (0.068) |
| University | 0.090** (0.041) | 0.14 (0.25) | 0.057 (0.11) | 0.27 (0.27) |
| $R^2$ | *0.25* | 0.40 | 0.23 | 0.31 |
| N | 17,807 | 410 | 2506 | 547 |

*, **, *** correspond to 10, 5, and 1 percent levels of statistical significance

negative effect on incomes for male craftsmen as a result of the deregulation in 2004 when I included covariates. Surprisingly, a rather sizable positive income effect for female craftsmen emerges, which is significant at the 10% level (see Table 3, column 1 and 3). The adjusted $R^2$ is only 0.22, although a relatively low coefficient of determination is not uncommon in social sciences and does not exclude that the estimation yields relevant results (see Wooldridge 2002).

When looking more specifically at different subgroups of the male, full-time working crafts population, it becomes apparent that distinguishing between construction and non-construction occupations is important (see Table 4, column 1 and 2). The deregulation appears to have reduced incomes by 6% in the construction crafts, a result that has strong statistical significance. Symmetrically, when construction trades are excluded, there is no income impact of the deregulation for male full-time workers. Excluding the occupation of 'floor tilers' does not change from the baseline results (see Table 4, column 3).

Furthermore, it is interesting to note that in the sample only comprising Meister companies, the reform effect is far from significant (see Table 4, column 4). This implies

**Table 4 Results by sub-group. Source: Microcensus 2000–2010**

| | (1) Only construction sector | (2) Excluding construction sector | (3) Excluding tilers | (4) Only Meister craftsmen |
|---|---|---|---|---|
| Reform effect | −0.058*** (0.012) | 0.036 (0.039) | 0.010 (0.031) | 0.0089 (0.040) |
| Deregulated trade | 0.088*** (0.008) | −0.067** (0.032) | −0.14*** (0.026) | −0.19*** (0.032) |
| Post 2004 | 0.12*** (0.021) | 0.13*** (0.025) | 0.12*** (0.017) | 0.12*** (0.024) |
| Age | 0.033*** (0.005) | 0.020*** (0.007) | 0.028*** (0.005) | 0.026*** (0.007) |
| Age square | −0.00040*** (0.000) | −0.00026*** (0.0009) | −0.00033*** (0.000) | −0.00030*** (0.000) |
| Migrant | −0.064** (0.023) | −0.12*** (0.036) | −0.065*** (0.022) | −0.032 (0.039) |
| Time in current job | 0.0088*** (0.001) | 0.0067*** (0.000) | 0.0069*** (0.001) | 0.0056*** (0.001) |
| Number of employees | 0.045*** (0.002) | N/A | 0.046*** (0.002) | 0.049*** (0.003) |
| Last labor market status | | | | |
| Unemployed | Comparison | Comparison | Comparison | Comparison |
| Employed | 0.053* (0.027) | 0.21** (0.079) | 0.086** (0.035) | 0.13** (0.064) |
| Student | −0.27*** (0.064) | −0.15* (0.076) | −0.18*** (0.059) | −0.11 (0.13) |
| Other | −0.031 (0.073) | 0.17 (0.20) | 0.0036 (0.098) | −0.11 (0.15) |
| Marital status | | | | |
| Single | Comparison | Comparison | Comparison | Comparison |
| Married | 0.10*** (0.013) | 0.16*** (0.026) | 0.12*** (0.013) | 0.10*** (0.016) |
| Widow(er) | 0.11 (0.079) | −0.0076 (0.060) | 0.021 (0.053) | 0.030 (0.063) |
| Divorced | 0.045* (0.025) | 0.044 (0.034) | 0.034 (0.021) | 0.042* (0.022) |
| Number of children | | | | |
| None | Comparison | Comparison | Comparison | Comparison |
| One child | 0.042** (0.016) | 0.055** (0.022) | 0.048*** (0.015) | 0.046*** (0.017) |
| Two children | 0.10*** (0.019) | 0.13*** (0.019) | 0.11*** (0.015) | 0.092*** (0.016) |
| Three + children | 0.13*** (0.027) | 0.15*** (0.029) | 0.13*** (0.020) | 0.14*** (0.027) |
| General education | | | | |
| Lower secondary school | Comparison | Comparison | Comparison | Comparison |
| Intermediate secondary school | 0.070*** (0.011) | 0.068*** (0.014) | 0.065*** (0.011) | 0.056*** (0.015) |
| University entrance qualification | −0.0035 (0.016) | 0.037 (0.027) | 0.0069 (0.014) | 0.033* (0.019) |
| Vocational qualification | | | | |
| None | Comparison | Comparison | Comparison | Comparison |
| Vocational training | 0.0012 (0.046) | 0.0074 (0.034) | 0.0075 (0.037) | N/A |
| Advanced vocational training | 0.0079 (0.052) | 0.076** (0.030) | 0.021 (0.042) | N/A |
| University | 0.079 (0.053) | 0.10* (0.054) | 0.087** (0.043) | N/A |
| $R^2$ | 0.25 | 0.22 | 0.25 | 0.25 |
| N | 10,624 | 8709 | 17,052 | 10,379 |

*, **, *** correspond to 10, 5, and 1 percent levels of statistical significance

that those companies in the previously-regulated market have not seen their income decline overall as a result of the deregulation. This speaks against the predominant view of economic rents in the licensed occupations and favors the explanation that any decline in incomes results from changes in the average human capital of supply.

Turning to the control variables, they mostly have the expected significance and sign, which speaks for the chosen specification. In particular, it is interesting to note that the estimate of the income premium of working in one of the deregulated trades is almost identical to that obtained in Bol (2014), which uses the same dataset but a different control group [−10% in column 1, Table 2 and −13% in Bol (2014)]. However, it is worth reminding that this result is far from a casual effect of the reform and probably also not a perfect causal effect of working in a deregulated trade, since it is very difficult to perfectly control for all differences in individual human capital characteristics and productivity between firms.

The specifications with alternative control groups corroborate a non-existent reform effect. No significant estimate of the deregulation on the incomes of male craftsmen is detected regardless of whether non-craftsmen form the control group or if the partially-deregulated

**Table 5 Results using alternative control groups. Source: Microcensus 2000–2010**

| | Men full-time | | Women full-time | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | Non-craft | Only fully regul. | Non-craft | Only fully regul. |
| Reform effect full deregulation | −0.017 (0.020) | −0.00046 (0.062) | 0.085** (0.038) | 0.28*** (0.068) |
| Reform effect partial deregulation | N/A | 0.01 (0.060) | N/A | 0.19*** (0.058) |
| Fully deregulated | −0.034** (0.014) | −0.24*** (0.032) | −0.048** (0.021) | −0.83*** (0.13) |
| Partially deregulated | N/A | −0.11*** (0.028) | N/A | −0.39*** (0.092) |
| Post 2004 | 0.11*** (0.012) | 0.12* (0.058) | 0.090*** (0.019) | −0.025 (0.040) |
| Age | 0.020*** (0.003) | 0.024*** (0.005) | 0.027*** (0.004) | 0.035*** (0.008) |
| Age square | −0.00021*** (0.000) | −0.00029*** (0.000) | −0.00031*** (0.000) | −0.00045*** (0.000) |
| Migrant | −0.060*** (0.019) | −0.062*** (0.021) | −0.040** (0.020) | −0.082* (0.048) |
| Time in current job | 0.0094*** (0.001) | 0.0067*** (0.001) | 0.0098*** (0.001) | 0.0080*** (0.002) |
| Number of employees | 0.045*** (0.002) | 0.043*** (0.002) | 0.039*** (0.005) | 0.043*** (0.006) |
| Last labor market status | | | | |
| Unemployed | Comparison | Comparison | Comparison | Comparison |
| Employed | 0.24*** (0.025) | 0.076** (0.032) | 0.17*** (0.026) | 0.17*** (0.051) |
| Student | −0.022 (0.047) | −0.19*** (0.056) | −0.097* (0.059) | −0.23 (0.50) |
| Other | 0.16*** (0.054) | −0.0065 (0.093) | −0.0017 (0.054) | 0.21* (0.12) |
| Marital status | | | | |
| Single | Comparison | Comparison | Comparison | Comparison |
| Married | 0.14*** (0.010) | 0.11*** (0.012) | 0.020 (0.015) | −0.0053 (0.032) |
| Widow(er) | 0.073** (0.029) | 0.045 (0.054) | 0.12*** (0.021) | 0.22*** (0.052) |
| Divorced | 0.048*** (0.014) | 0.033 (0.020) | 0.067*** (0.011) | 0.11*** (0.021) |
| Number of children | | | | |
| None | Comparison | Comparison | Comparison | Comparison |
| One child | 0.042*** (0.006) | 0.049*** (0.014) | 0.022 (0.014) | −0.013 (0.023) |
| Two children | 0.12*** (0.01) | 0.11*** (0.014) | 0.044*** (0.012) | −0.028 (0.029) |
| Three + children | 0.18*** (0.013) | 0.13*** (0.020) | 0.090*** (0.028) | 0.13 (0.11) |
| General education | | | | |
| Lower secondary school | Comparison | Comparison | Comparison | Comparison |
| Intermediate secondary school | 0.11*** (0.012) | 0.058*** (0.010) | 0.14*** (0.013) | 0.059*** (0.0209) |
| University entrance qualification | 0.14*** (0.019) | 0.00029 (0.014) | 0.14*** (0.018) | 0.050 (0.072) |
| Vocational qualification | | | | |
| None | Comparison | Comparison | Comparison | Comparison |
| Vocational training | 0.067*** (0.018) | 0.017 (0.034) | −0.031* (0.017) | −0.038 (0.076) |
| Advanced vocational training | 0.087*** (0.018) | 0.025 (0.039) | −0.021 (0.019) | −0.014 (0.076) |
| University | 0.16*** (0.018) | 0.094** (0.040) | 0.070*** (0.026) | 0.071 (0.11) |
| $R^2$ | 0.35 | 0.26 | 0.33 | 0.26 |
| N | 74,968 | 17,807 | 26,941 | 17,807 |

*, **, *** correspond to 10, 5, and 1 percent levels of statistical significance

trades are switched from the control group to the treatment group (see Table 5, column 1 and 2).

While a significant positive income effect for women is still found, (see Table 5, column 3 and 4), these specifications should be interpreted with caution due to the sample size. Women are already under-represented in the sample and the number of fully-deregulated trades is very small.

In general, interpreting lagged effects in challenging. On the one hand, in some cases it seems plausible that the effects of economic reforms take time to materialize. On the other hand, the more that the analysis moves away from the reform implementation in time, the more concerned one should be that the estimates may be picking up irrelevant developments affecting the outcome variable. In this case, including yearly effects over a three-year period after the reform does not yield any new

**Table 6 Results with yearly interaction terms. Source: Microcensus 2000–2010**

| | Men full-time | | Women full-time |
|---|---|---|---|
| | (1) | (2) | (3) |
| | Yearly interaction terms all sectors | Yearly interaction terms only construction | Yearly interaction terms all sectors |
| Reform effect 2000 | Comparison | Comparison | Comparison |
| Reform effect 2001 | 0.025 (0.031) | 0.084*** (0.027) | − 0.16** (0.072) |
| Reform effect 2002 | 0.0094 (0.037) | 0.022 (0.037) | − 0.10 (0.069) |
| Reform effect 2003 | 0.039 (0.037) | 0.073* (0.037) | 0.05 (0.039) |
| Reform effect 2004 | 0.027 (0.040) | 0.032 (0.038) | − 0.07 (0.064) |
| Reform effect 2005 | 0.011 (0.030) | − 0.0051 (0.036) | 0.09 (0.080) |
| Reform effect 2006 | 0.011 (0.029) | − 0.016 (0.027) | 0.05 (0.079) |
| Reform effect 2007 | 0.049 (0.043) | 0.020 (0.035) | 0.02 (0.12) |
| Reform effect 2008 | − 0.023 (0.034) | − 0.074*** (0.026) | 0.05 (0.069) |
| Reform effect 2009 | N/A | N/A | N/A |
| Reform effect 2010 | N/A | N/A | N/A |
| Deregulated trade | − 0.16*** (0.025) | 0.052** (0.025) | − 0.39*** (0.066) |
| Age | 0.024*** (0.005) | 0.031*** (0.005) | 0.035*** (0.008) |
| Age square | − 0.00029*** (0.000) | − 0.00037*** (0.000) | − 0.00045*** (0.000) |
| Migrant | − 0.062*** (0.021) | − 0.051* (0.025) | − 0.078*** (0.052) |
| Time in current job | 0.0066*** (0.001) | 0.0084*** (0.001) | 0.0080*** (0.002) |
| Number of employees | 0.043*** (0.002) | 0.042*** (0.003) | 0.043*** (0.006) |
| Last labor market status | | | |
| Unemployed | Comparison | Comparison | Comparison |
| Employed | 0.076** (0.032) | 0.041 (0.026) | 0.18*** (0.048) |
| Student | − 0.19*** (0.056) | − 0.27*** (0.074) | − 0.22 (0.50) |
| Other | − 0.0049 (0.093) | − 0.032 (0.071) | 0.20* (0.12) |
| Marital status | | | |
| Single | Comparison | Comparison | Comparison |
| Married | 0.11*** (0.012) | 0.094*** (0.012) | − 0.0090 (0.029) |
| Widow(er) | 0.045 (0.054) | 0.11 (0.076) | 0.21*** (0.053) |
| Divorced | 0.034* (0.020) | 0.040 (0.025) | 0.12*** (0.020) |
| Number of children | | | |
| None | Comparison | Comparison | Comparison |
| One child | 0.049*** (0.014) | 0.043** (0.016) | − 0.014 (0.024) |
| Two children | 0.11*** (0.014) | 0.10*** (0.019) | − 0.029 (0.030) |
| Three + children | 0.13*** (0.020) | 0.13*** (0.026) | 0.14 (0.11) |
| General education | | | |
| Lower secondary school | Comparison | Comparison | Comparison |
| Intermediate secondary school | 0.058*** (0.010) | 0.062*** (0.011) | 0.059*** (0.018) |
| University entrance qualification | 0.00036 (0.014) | − 0.010 (0.016) | 0.045 (0.070) |
| Vocational qualification | | | |
| None | Comparison | Comparison | Comparison |
| Vocational training | 0.017 (0.033) | 0.011 (0.044) | − 0.044 (0.073) |
| Advanced vocational training | 0.024 (0.038) | 0.0058 (0.048) | − 0.019 (0.069) |
| University | 0.093** (0.040) | 0.085 (0.051) | 0.075 (0.11) |
| $R^2$ | 0.26 | 0.26 | 0.24 |
| N | 17,807 | 10,624 | 2506 |

The variables of interest are (for each year in the sample) the interaction of the year in question and being in the treatment group

*, **, *** correspond to 10, 5, and 1 percent levels of statistical significance

**Table 7 Placebo tests checking whether the common trends assumption holds. Source: Microcensus 2000–2010**

| | Men full-time | | Woman full-time |
|---|---|---|---|
| | (1) | (2) | (3) |
| | Placebo reform all sectors | Placebo reform only construction | Placebo reform all sectors |
| 2001 | 0.03 (0.028) | 0.06** (0.028) | −0.09 (0.057) |
| 2002 | 0.01 (0.030) | 0.01 (0.028) | 0.03 (0.040) |
| 2003 | 0.02 (0.024) | 0.03 (0.021) | 0.09** (0.039) |
| 2004 | −0.00 (0.034) | −0.01 (0.022) | 0.00 (0.050) |
| $R^2$ | 0.25 | 0.24 | 0.27 |
| N | 9682 | 5621 | 1280 |

*, **, *** correspond to 10, 5, and 1 percent levels of statistical significance

insights, given that these estimates are also insignificant (see Table 6).

The placebo test for the full-time male sample does at least not discredit the verification of the parallel trends assumption. Simulated policy changes before 2004 all have insignificant effects on income (see column 1, Table 7). For other samples, the picture is more worrisome: while the same procedure mostly holds for construction crafts trades, it fails in 2001, where a significant placebo reform is found (see column 2, Table 7). Hence, the significant negative income effect found in the construction sector should be interpreted with caution. For the female sample, the parallel trends assumption appears to be violated in 2003 (see column 3, Table 7).

Lergetporer et al. (2018) try to alleviate the problem of non-parallel trends by complementing their analysis with a matching technique. Accordingly, the negative effect on incomes as a result of the deregulation all but disappears. Fredriksen and Runst (2018) use synthetic control estimation, which estimates treatment effects using a weighted counterfactual that best matches the treatment group in the pre-treatment period, and they also find no significant income effects of the 2004 deregulation. Both results speak in favor of the findings in this paper.

In summary, in the presence of conflicting evidence in the literature, the results in this paper align with the studies that find no or weak income effects of occupational licensing. In particular, overall my findings are consistent with existing studies in the German crafts case. I find no evidence that reforming the traditional licensing scheme in the German crafts sector negatively affected the incomes of male self-employed craftsmen overall.

Certain groups may have been affected (men in the construction sector negatively and females positively). However, I am wary about these results, for different reasons. Just like Lergetporer et al. (2018) find that using a matching-like procedure halves their reform estimate, I find worrying signs that the parallel trends assumption in these cases may not be met and the sample size—in particular when using only full-regulation trades as the control group—for these groups is relatively small.

### 3.2 Discussion

The existing literature predominately discusses two channels through which occupational licensing raises incomes (Redbird 2017). The first channel originates from the supply side, whereby restricting market entry creates an artificial scarcity of supply, allowing producers to charge higher prices and hence increase their incomes. The second channel originates from the demand side, whereby occupational licensing schemes are defended on the grounds of quality insurance. Under this assumption, licensing would increase consumers' willingness to pay and thus the prices that producers may charge, and ultimately their incomes.

Nonetheless, this empirical study of a reform removing occupational licensing in certain German crafts trades finds no significant effect on the incomes of the affected professionals. Several counterweighing mechanisms can explain this result.

Starting with the supply channel, it is conceivable that the deregulation in 2004 did not affected supply scarcity. One way of explaining this is to look at the pre-reform market situation in the German crafts market, given that it is conceivable that there were no—or very low—economic rents in licensed occupations to begin with. Several contributions in the literature have challenged the Cournot theorem, according to which a competitive equilibrium only occurs as the number of firms proceeds to infinity (see e.g. Fama and Laffer 1972; Stiglitz 1987 p. 1042; Bresnahan and Reiss 1991). In the case of Germany, by the end of 2003—just before the reform took place—there were roughly 75,000 firms operating in the 53 different trades that make up the deregulated market segment (see Mueller 2006). Hence, competition may have already been (close to) sufficient to ensure prices close to marginal costs.

A second explanation emphasized in Lergetporer et al. (2018) is that the new market entrants after the barrier fell did not constitute a real threat to the incumbent firms. This argument is supported by the findings in Runst et al. (2018b) of increased market exit after the reform, which—assuming that a majority of firms exiting entered the market after 2004—is a sign that new entrants are not competitive. Furthermore, Fredriksen et al. (2019) find that Meister companies are rated higher by consumers in the deregulated market segment. Assuming that a sufficient number individuals have a clear preference for high (Meister) quality, firms that have entered after the deregulation are not considered as substitutes to incumbents by crafts consumers.

Other regulations could also impede changes in relative scarcity from affecting economic outcomes. Lergetporer et al. (2018) also emphasize the role of German labor market institutions (minimum wages and collective bargaining agreements) that can reduce or delay the effects of the deregulation on incomes. However, since my results also show no income effects among the self-employed—whose earnings should not be influenced by the mentioned labor market institutions—they are unlikely to be the main channel through which occupational licensing and incomes interact in the German crafts.

Furthermore, in the case of the German crafts, one must also consider that the deregulated trades are still subject to a voluntary certification scheme. According to White (1980, p. 48), "institutionally oriented economists have sometimes questioned whether introducing mandatory licensure makes much difference in labor markets were voluntary licensure is already well-established". Since consumers rate Meister companies higher (Fredriksen et al. 2019)—suggesting that they perceive a quality difference between Meisters and non-Meisters—one can deduce that the voluntary certification scheme reduced consumer search costs for information about quality.

Turning more specifically to the demand channel, Damelang et al. (2018, p. 37) assume that "the total volume of labour remained constant, as there is no evidence that the reform led to changes in the demand for craft work". While no facts are available to support this (or the opposite) claim, it is still useful to conduct the discussion on a theoretical level.

The empirical literature on the link between occupational licensing and quality is inconclusive (see e.g. Carroll and Gaston 1983; Shilling and Sirmans 1988; Angrist and Guryan 2007). While licensing could in theory lead to more demand through higher service quality, the opposite may also occur if producers in non-competitive markets over time reduce their efforts to produce quality and invest in innovative activities, which in turn suppresses consumers' willingness to pay for the service. Furthermore, higher prices under licensing can lead consumers to other means of acquiring the service or simply resigning to an inadequate status quo and purchasing no service at all. In both cases, competitive forces from free entry could have sparked new earning potentials.

It is finally worth highlighting the problematic nature of the parallel trends assumption. While authors in the literature on the German crafts reform have used techniques like matching and synthetic control to address this, these methods also rely on untestable assumptions and are therefore not "be-all end-all" approaches to estimating casual treatment effects. They notably require the unconfoundness assumption, stating that "adjusting treatment and control groups for differences in observed covariates, or pretreatment variables, remove all biases in comparisons between treatment and control units" (Imbens and Wooldridge 2009, p. 2). As is most often the case in observational studies, casual interpretations of results remain an issue.

## 4 Conclusion

By investigating the link between occupational licensing and incomes in the German crafts, this paper contributes to the scientific literature on occupational licensing by exploiting a natural experiment. Furthermore, since the German crafts sector accounts for 14% of total employment in Germany, the reform in 2004 is also a policy decision worth evaluating in itself with respect to the national economy. Finally, the insights from this study are relevant for the current policy discussions of harmonizing national labor markets in Europe, where proponents stress the importance of achieving a unified European labor market, whereas opponents worry about the potential adverse effects of a race to the bottom in terms of government regulation.

I found no income effects of removing occupational licensing and while previous studies have found significant effects of the reform on the incomes of employees, such effects were small in size. This prompts the question whether occupational licensing schemes similar to the German one truly generate economic rents. Broadly, three different stories may explain these results: if competition in a regulated market is high despite the existence of a market entry barrier, there is no room to charge higher prices; if competition in a regulated market is low, productivity and incentives may be similarly low, which again may reduce quality and hence, the demand side will put a stop on the scope to raise prices. Finally, competition in a deregulated market may also be low if entrants to not pose a threat to incumbents. In none of these three cases would licensing lead to higher incomes. In the case of the German crafts, there may be some truth to them all.

In the first case, the deregulation appears unnecessary. In the second case, it will cause a shift of producer surplus to consumers and avoid a deadweight loss associated with the regulation. In the third case, the deregulation appears insufficient and can only ensure competitive markets through supplementary policies. While none of these cases a priori imply that the deregulation was a poor policy choice, for a final verdict on the 2004 deregulation reform it is important to keep in mind that it generated a cascade of significant effects in other areas, such as market entry and exit, training, the quality of services produced as well as migrant self-employment in the crafts sector (see e.g. Runst et al. 2018a, b; Runst and Thomä 2019), which must also be taken into consideration.

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**Authors' contributions**
KF is the sole author and only contributor to the paper. The author read and approved the final manuscript.

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**Availability of data and materials**
The dataset analyzed during the current study is not publicly available for free but can be purchased from Statistics Germany.

**Competing interests**
The author declares no competing interests.

## Appendix

See Table 8.

**Table 8 Selected recent studies on the effects of occupational licensing on income. Source: Original studies**

| Study | Country | Occupation | Methodology | Main finding |
|---|---|---|---|---|
| White (1980) | USA | Registered nurses | Three-stage least-squares | Licensing has no impact on pay |
| Kleiner and Petree (1988) | USA | Teachers | Fixed effects analysis | Licensing has no impact on teacher pay |
| Kleiner (2000) | USA | Dentists, lawyers, barbers and cosmetologists | Residual wage gap analysis | Earnings are higher for licensed occupations that require more education and training relative to comparable unlicensed occupations |
| Kleiner and Kudrle (2000) | USA | Dentists | Tobit specification, OLS, reduced form equations using sample of Air Force recruits | Practitioners in the most regulated states earn 12% more than those in the least regulated states |
| Angrist and Guryan (2007) | USA | Teachers | Estimation of wage equations | State-mandated teacher testing slightly increases teacher salaries (2.4–3.4%) |
| Timmons and Thornton (2008) | USA | Radiologic technologists | OLS, IV | Radiologic technologists working in states with licensing statutes earn 3.3–6.9% more |
| Timmons and Thornton (2013) | USA | Massage therapists | OLS, with duration effects | Massage therapists working in states with licensing receive an earnings premium 16.2% |
| Bol (2014) | Germany | Crafts | Two-level multilevel regression models | Self-employed craftsmen subject to occupational licensing receive 13% higher income |
| Bol and Weeden (2015) | Germany and UK | National level | Multilevel random intercept models | Positive wage returns to occupational licensing in both countries (9% in Germany, 8% in the UK) |
| Kleiner et al. (2016) | USA | Medical services | Two-way fixed effects estimation of wage equation | Removing prescription authority regulations increased wages of the deregulated by 5% and decreased wages of the regulated by 3% |
| Zapletal (2017) | USA | Cosmetologists | Pooled OLS with year fixed effects | Occupational licensing does not affect market prices |
| Redbird (2017) | USA | National level | Multilevel fixed effects model and longitudinal model | No aggregate wage effect of licensure |
| Pizzola and Tabarrok (2017) | USA | Funeral services | Difference-(in-differences)-in-differences, synthetic control | Occupational licensing causes wage premium of 11–12% |
| Damelang et al. (2018) | Germany | Crafts | Difference-in-difference approach | Removing occupational licensing has reduced earnings of craft employees by 13 Euros per month |
| Lergetporer et al. (2018) | Germany | Crafts | Difference-in-difference estimation, entropy balancing | Removing occupational licensing has decreased wages of workers by 2.3%. No effect on the self-employed |
| Ingram (2018) | USA | National level | Matching estimator, simple border state framework | Licensed workers have an income premium of 4–6% |
| Sonntag and Lutter (2018) | Germany | Crafts | Difference-in-differences estimation | Very small (hardly detectable) income effects of removing occupational licensing found |

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