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- The German crafts case

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Does occupational licensing impact incomes? - The German crafts case

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Abstract

The empirical literature on occupational licensing finds standard monopoly effects of entry regulations: Less competition and economic rents for professionals. I exploit the natural experimental design of a change in the German crafts regulation in 2004, which removed the traditional licensing requirement only for certain trades, and find no robust effects. I point out that existing studies on the income effects of occupational licensing suffer from methodological weaknesses. Most studies rely on cross-section estimates that are likely to be biased due to unobserved heterogeneity whereas others do not rigorously define the population of interest. Based on my results, I suggest that demand-effects and market saturation have so far been inadequately discussed in the literature.

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[1] The author would like to thank Dr. Petrik Runst for important intellectual input to this study.

1.   Introduction

Why do some occupations pay more than others? Occupational licensing (i.e. requiring minimum levels of human capital investment by professionals) is the strictest form of regulation to access professions that blocks market access for individuals without the necessary credentials. A large body of literature has found that the subsequent reduction in labor supply generates an income premium for market incumbents (see e.g. Friedman and Kuznets, 1954; and Kleiner and Krueger, 2010). This again is an incentive to lobby for perpetuating licensing requirements at ever-increasing standards (see Rottenberg, 1962).

National occupational licensing schemes have been steadily on the rise since the Second World War, in both Europe and the US (see Kleiner, 2006; and Kleiner and Kueger, 2010) replacing in part the decline in labor union membership. This development is at odds with European institutions' efforts to create a common market area with comparable regulatory frameworks to enable and foster the free movement of labor between countries (see COM, 2013) since costly investment in national specific qualifications precludes cross-country worker mobility.

In line with those efforts and in order to increase market competition by facilitating entry, the German regulator – contrary to the trend observed by Kleiner and colleagues – in 2004 liberalized the traditional occupation licensing scheme in the crafts sector. Since then, craftsmen in a number of crafts trades are no longer required to hold the traditional Meister title (an advanced vocational training certificate) to found a company to facilitate and foster entrepreneurship in craft trades.

Free market scepsis motivated by fear of earning erosion due to low-cost entrants are a constant feature in the process of Europen integration, most recently sparked by the EU enlargements of 2004-2013, the Commission's plans of integrating further eastern European

states in the common market as well as the continuous extra-european immigration. Such reservations may lead member states and national interest groups to demand (and obtain) protective measures to avoid a decrease of earnings in groups particularly affected by low-cost competition. An informed policy debate on how measures such as occupational licensing (and its suspension) actually affect incomes is therefore worth striving for.

In recent years, a number of studies have used the 2004 reform for this exact purpose. Conducted primarily from a sociological perspective, they have found significant negative income effects of removing occupational licensing in the crafts, which may fuel the popular concerns noted above (see Bol, 2014; Bol and Weeden, 2014; Damelang et al., 2017; and Lergetporer et al., 2016). I re-examine the effects of this deregulation and contrary to the existing literature, I find no convincing evidence of reduced incomes. In the explanation of these results, I go beyond the common focus on reduced price competition and thus the appearance of economic rent and propose alterantive mechanisms whereby occupational licensing and incomes interact that also include the demand-side. I conclude that in light of my findings viewing occupational licensing as an "active labor market policy" to safeguard living standards is not only be harmful to the common market, but it may also not have the desired effect or – at best – it might affect incomes at random.

The remainder of this paper is organized as follows. Chapter two is a critical review of the related literature, before chapter three describes the data used for the empirical analysis. Chapter four details the methodology used, while chapter five summarizes the results. Finally, chapter six provides possible explanations for the findings, whereas chapter seven concludes.

2.   Related literature

Human capital models in which individual rewards are a function of education and skills have been supplemented with contributions stressing the role of government regulation to explain income differences. Licensing schemes – which require minimum levels of human capital investment by professionals and as such constitute a market entry barrier – have received particular attention in the literature.

Several theoretical contributions (see e.g. Stigler, 1971; and Friedman and Kuznets, 1954) expect a positive link between occupational licensing and incomes, which empirical studies (see e.g. Kleiner and Kudrle, 2000; and Kleiner and Krueger, 2010) – mainly using US data – have confirmed. Recently, several empirical studies concerning licensing and incomes have been conducted using the example of the German crafts sector, reaffirming this positive relationship (see Bol, 2014; Bol and Weeden, 2014; Damelang et al., 2017; and Lergetporer et al., 2016). However, the studies are subject to various methodological and interpretational issues.

Most empirical studies on the effects of occupational licensing use a cross-section study design. Some studies of the German crafts sector also use this methodology and find very strong and significant effects on incomes; for instance, Bol (2014) explores survey data for 2006 and finds that self-employed craftsmen subject to occupational licensing receive 13% higher income. However, 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. It is difficult to perfectly control for individual human-level characteristics and differences in occupation-level productivity. The estimates of income differences between licensed und unlicensed professions are therefore likely to be biased due to this unobserved heterogeneity.

The German craft sector however actually presents researchers with a golden opportunity to improve on the cross-section design when examining the effects of occupational licensing, since the scheme was recently reformed in a way that resembles a natural experiment. The German craft sector is regulated by the so-called Trade and Crafts Code (*Handwerksordnung*) and comprises about 5 million professionals in 93 trades[2], which make up about 12% of the working population (Federal Statistical Office, 2016). From 1953 to 2004, all German craft trades were subject to a licensing scheme. Only craftsmen with a Meister title (internationally recognized as tertiary education) could found a company. Since 2004, market entry in certain trades[3] has been open, although craftsmen in these trades can still choose to acquire the Meister title. In the remaining trades, the licensing requirement remains fully[4] or partially[5] intact.[6]

It is therefore in theory possible to assess the casual effect of removing occupational licensing using a difference-in-differences estimation, which some studies have already done. This has the great advantage of eliminating the unobserved heterogeneity bias given certain key assumptions. With this approach, Damelang et al. (2017) find an average treatment effect of minus 13 euros per month in the deregulated trades. Lergetporer et al. (2016) find a significant negative reform effect on the incomes of employed craftsmen of 0.7% on average each year since the reform, with effect sizes only setting in 5 years after the deregulation. Using a second dataset and looking at total household incomes, they find stronger effect sizes

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[2] The term "trade" pertains directly to the craft sector, whereas the term "occupation" is more general since a given occupation can contain both craft and non-crafts workers, as well as different craft trades. When referring to the German context, I will use the term "trade" to remain true to the German regulation. However, for insights on regulation, I will use the term "occupation" to align with the relevant literature.

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

[4] Examples of fully regulated trades are roofers, gunsmiths and plumbers.

[5] Examples of partially regulated trades are optometrists, orthopedic and dental technicians.

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

(-8% to -12%), with employed craftsmen being more affected by the reform compared with self-employed craftsmen.

While the authors of these papers conclude that workers in deregulated occupations have been affected negatively by the policy reform, it is worth highlighting the small effect sizes. Also, it is not clear that the observed income reduction from 2009 can actually be attributed to the policy change in 2004. Lergetporer et al. (2016) themselves note that they are planning future work which also includes more recent years in order to see beyond the financial crisis. It is also not clear why the authors use household income to investigate the effects of a reform that affects individual incomes. Again, Lergetporer et al. (2016) themselves point out that in this case, intra-household adjustments may bias results.

Furthermore, while the unbiasedness of the difference-in-difference estimator does not require controlling for every difference (apart from the treatment itself) in the treatment and comparison groups, it does require that in the absence of treatment, the unobserved differences between treatment and comparison groups are the same over time. Since it is not possible for an individual to be simultaneously treated and non-treated, the identifying assumption of the same trend in the absence of treatment cannot be tested. However, researchers can examine whether the assumption of a common trend appears justified in the pre-treatment period, an aspect left open by Damelang et al. (2017), while Lergetporer et al (2016) conclude that the common trends assumption holds after some procedure of observation matching (by which most of the effect size disappears).

Additionally, the allocation of observations into regulated and deregulated craft trades is problematic. The considered datasets do not contain explicit information about craft membership, which thus must be defined from the occupation variable. However, the line between craft and industry activity in the occupation classification, which does not reflect the legal definition of the crafts, is blurred. This opens room for strong discretion. To illustrate

this Runst et al. (2016, p. 4) conduct a thorough re-examination of the crafts classification in Rostam-Afschar (2014) and find that: "the demarcation chosen […] is most probably too broad: While it certainly includes many of the occupations that German craftsmen would practice, it also very likely contains a large proportion of non-crafts individuals who are unaffected by the policy reform".

Finally, in the interpretation of the results, the existing literature fails to recognize that there may be other channels besides economic rents tying together occupational licensing and incomes. In particular, since the average qualification level of craftsmen may have been affected by the reform, any income reduction may also just be a result of lower productivity.

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. I contribute to the existing literature by addressing the aforementioned problems of previous studies.

3.   Descriptive data analysis

3.1   Dataset

I use the microcensus dataset for 2000-2010, which is a representative official sample survey of the German population.[7] 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 last two features contribute to a very low unit non-response rate of 3%.

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

The usual problems with data collection for income studies still hold for the microcensus. Notably, the very top or the 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.

Furthermore, individual income in the microcensus is reported in 24 intervals and hence not as specific as could be wished. In order to achieve a metric scale for the variable, I use a method described by the Leibniz Institute for Social Sciences[8] 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 incomes are normally distributed within the intervals, the more accurate that this transformation will be. Although this feature of the microcensus might prevent picking up small changes in individual incomes over time, it also prevents overemphasizing effects of very little economic consequence (see my previous critique of Damelang, 2017).

If one is 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. Like most other datasets, the microcensus does not contain a separate crafts variable, which is a potential pitfall for researchers unfamiliar with the specific legal definition of the German craft sector. Several occupations cover both craft and industry activities, hence solely relying on this variable, which all published studies on the effect of the deregulation on incomes to date do, is unlikely to capture the true population of interest. Indeed, Runst et al.

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[8] In: GESIS (Hg.): Mikrodaten-Informationssystem MISSY. URL:
    http://www.gesis.org/missy/files/documents/MZ/Auswertungsbeispiel_BerechnungDes%C3%84quivalenze
    inkommens.pdf (downloaded am 24.07.2017)

(forthcoming) show that identifying the crafts solely on the basis of 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 use the method proposed in Runst et al. (forthcoming) for identifying the crafts, which combines the occupation codes in the microcensus with data from the Federal Institute for Vocational Education and Training on the share of crafts apprentices within each occupational code. Only occupations where this share exceeds 60% are considered as crafts here, which is an effective procedure for deriving a clean definition for the microcensus data, which excludes individuals in the agricultural or industrial or service sectors of the economy, which are unaffected by changes in crafts legislation.[9]

I restrict my analysis to the self-employed, in line with most studies on the effects of the reform when self-employment can be identified in the data. 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. It has however been argued that employees may have been affected my the reform through intra-firm bargaining effects (see notably Damelang et al., 2017). In theory, it is even imaginable that through such mechanisms self-employed are left fully unaffected by removing occupational licensing. In order to make sure that all theoretical effects of the reform are accounted for, I estimate an alternative specification (not presented in the paper) where the population of interest also include all employees in the German crafts sector. The findings do not change, hence I conclude that restricting the analysis to the self-employed is appropriate.

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[9] The classification scheme in the microcensus (KldB1992) merges about 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. As in Runst et al. (forthcoming), I therefore do not include cleaners in the analysis.

Also, 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).

The dataset 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 as public transfers will partly compensate for the reduction. Another reason to regret the absence of an earnings variable is that unobserved factors influencing other income sources may bias the results if they affect craft trades differently. Therefore, I only include in the sample respondents who reported that labor earnings are the primary source of their net individual income. 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. According to official German craft statistics (*"Handwerkszählung"*), my sample make up 7% of the total German craft population where the share of workers in a deregulated trade amounts to 20%. Table 1 provides a descriptive summary of the sample in this study.

Table 1: Descriptive statistics of the variables by treatment and control group

	Treatment group		Control group	
	mean	St.d.	mean	St.d.
Individual net total income	1858.93	1608.98	2112.85	1925.82
Age	45	10	45	10
Female	0.23	0.42	0.13	0.34
Migrant	0.10	0.31	0.06	0.23
Hours worked	47	14	51	14
Part-time	0.10	0.30	0.04	0.19
Time-in-current-job	12	10	12	10
Number of employees	4	15	5	14
Last labor market status				
Unemployed	0.01	0.12	0.01	0.10
Employed	0.71	0.46	0.71	0.46
Student	0.00	0.06	0.00	0.04
Other	0.01	0.09	0.01	0.07
Marital status				
Single	0.25	0.43	0.20	0.40
Married	0.64	0.48	0.71	0.45
Widow(er)	0.02	0.13	0.01	0.11
Divorced	0.10	0.30	0.08	0.27
Number of children				
None	0.63	0.48	0.58	0.49
One child	0.18	0.39	0.20	0.40
Two children	0.15	0.36	0.17	0.38
Three+ children	0.04	0.20	0.05	0.22
General education				
Lower secondary school	0.51	0.50	0.62	0.49
Intermediate secondary school	0.25	0.44	0.26	0.44
University entrance qualification	0.23	0.42	0.12	0.32
Vocational qualification				
None	0.03	0.17	0.02	0.13
Vocational training	0.89	0.31	0.96	0.19
Advanced vocational training	0.08	0.27	0.02	0.15
University	0.00	0.03	0.00	0.02
N (max-min)	*5819-5235*		*27079-25456*	

Source: Microcensus 2000-2010

3.2 Incomes in the German crafts sector

Over the period considered, the average craftsman in Germany received 2066 euros net per month. As figure 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.

Futhermore, a visual interpretation of figure 1 may suggest that deregulated occupations have been 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. No such effect can be observed for the female- and part-time sample.

However, this provides no conclusive evidence on a possible reform effect for the sub-groups concerned, in particular since 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. Furthermore, one would have expected the effects of such a reform to be long-lasting however incomes in the deregulated sector pick up after 2005 and follow the same up-ward sloping trend as income in the still-regulated sector.

Another important insight from figure 1 relates to the years before the reform (2000-2004). As previously underlined, the treatment and control groups must follow identical trends in absence of the policy intervention for the treatment effect to be identifiable, although it is unclear whether this is the case here. 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. The rest of the paper is dedicated to investigating these intuitions using regression analysis.

Figure 1: Evolution of incomes of self-employed (male, female, full-time and part-time) in the treatment- and control group
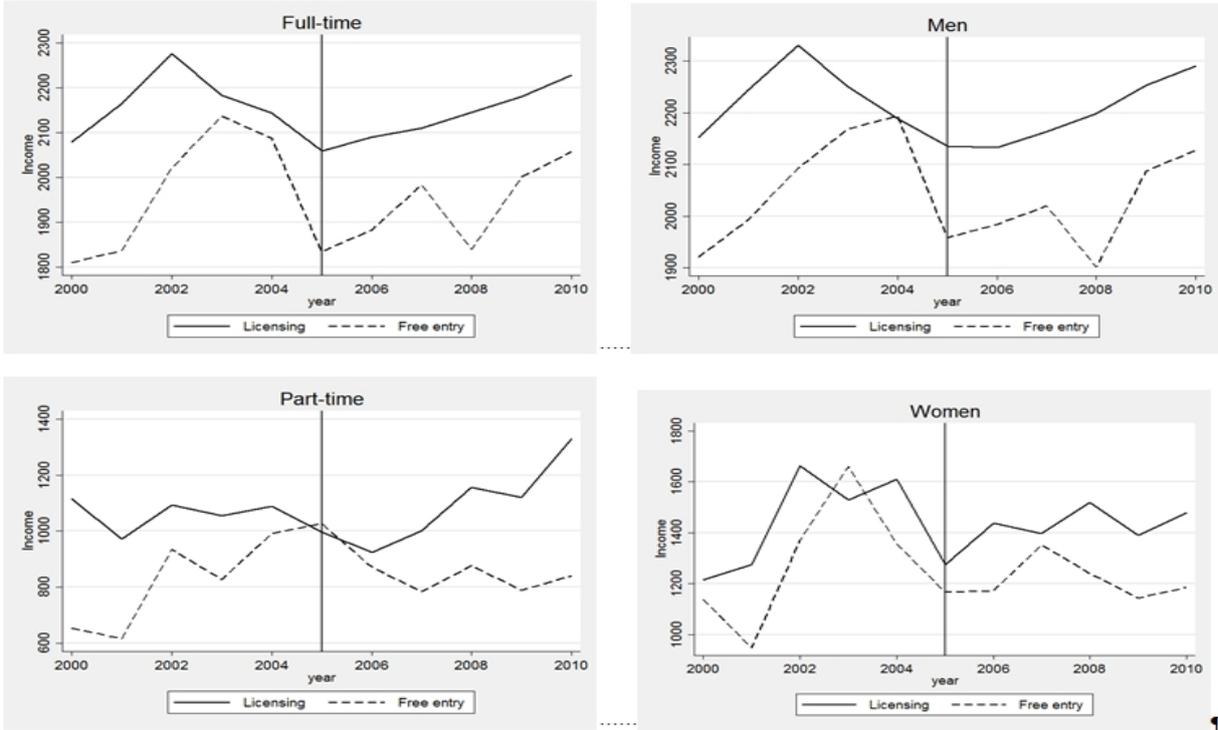
Source: Microcensus 2000-2010

4.   Empirical approach

4.1   Estimation

By using difference-in-differences (DID), 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.

This methodological approach has been chosen in several existing studies on the effects of the reform in 2004 on market entry and exit effects and it is the approach that I

favor to examine the income effects of the German crafts reform. I therefore estimate the following regression:

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

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 are 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 is year.

A concern in difference-in-differences analysis is that the policy may have been anticipated by the concerned individuals and as a consequence, they would have adjusted their behavior in the pre-treatment years. If the researcher is not aware of this, the estimation will not pick up existing treatment effects. Anticipation effects are very unlikely in the case of the German crafts deregulation. Very long there was great discussion and uncertainty regarding which crafts trades were to be deregulated. The governing coalition presented a reform proposal in in 2003 which it was criticized and rejected by the Federal Assembly, the upper house of the German parliament and as a result the final discussion in the mediation committee occurred on December 10 in 2003 after which many of the trades intended for deregulation would remain regulated or would be only partially deregulated (Runst, 2018). Still, to be on the safe side, I estimate an alternative specification (not presented in the paper) where 2004 is defined as the treatment year. The findings do not change, hence I deduce that choosing 2005 as the treatment year is not erroneous.

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 of the highest obtained general education and vocational qualification.[10] I also control for a number of labor market attributes, namely working hours, 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 Breusch-Pagan/Cook-Weisberg test for heteroscedasticity rejects the null-hypothesis of homoscedasticity. Therefore, I calculate robust standard errors allowing for intra-group correlation between different occupations.

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). Lastly, I look at a sample consisting only of Meister-companies, as means of looking at the effects of the reform on market incumbents. The existing literature on this reform that investigates heterogeneous effects mostly find significant income reductions only for male crafts (see Lergetporer et al., 2016). Hence, these regressions focus on the full-time male craftsmen sample.

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 where reform effects take time to

materialize. For instance, Lergetporer et al. (2016) find that significant income effects of the 2004 reform only appear with a five year lag.

The DID approach is only valid as long as unobserved factors are constant over time or change in a similar way for the treatment and control groups. Therefore, as is standard for DID analyses, I test the likelihood of common trends 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.

I also estimate the baseline regression with an alternative control group. One obvious choice here is individuals outside the crafts sector. Additionally, I estimate the baseline DID regression with the fully deregulated trades as well as and partially deregulated trades as treatment groups and only the still fully regulated trades as the control group. Despite still being subject to occupational licensing, some trades were nevertheless affected by the reform in 2004 as subsequently, experienced employees without a Meister title in these trades have been permitted to start a business. 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.

5.   Results

5.1   Main findings

Contrary to previous studies, I find no immediate indication of a negative effect on incomes for male craftsmen as a result of the deregulation in 2004. Surprisingly, a rather sizeable positive income effect for female craftsmen emerges, which is significant at the 10% level (see table 2, 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).

Table 2: Main results

	(1) Men full-time	(2) Men part-time	(3) Women full-time	(4) Women part-time
Reformeffect	-0.0071	0.42	0.096*	0.032
Deregulated trade	-0.13***	-0.18	-0.41***	-0.048
Post 2004	0.12***	-0.18	0.15***	0.051
Age	0.027***	-0.015	0.039***	-0.025
Age square	-0.00032***	0.00019	-0.00050***	0.00027
Migrant	-0.071***	-0.019	-0.070	-0.0091
Time-in-current-job	0.0069***	0.012*	0.0084***	0.0065***
Number of employees	0.045***	0.028	0.0031**	0.0020
Last labor market status				
Unemployed	Comparison	Comparison	Comparison	Comparison
Employed	0.088***	-0.16	0.18***	0.16
Student	-0.18***	-0.19	-0.20	0.31*
Other	0.0029	-0.21	0.21	-0.20*
Marital status				
Single	Comparison	Comparison	Comparison	Comparison
Married	0.12***	-0.058	-0.016	-0.23***
Widow(er)	0.048	0.13	0.21***	0.24
Divorced	0.038*	-0.11	0.11***	0.045
Number of children				
None	Comparison	Comparison	Comparison	Comparison
One child	0.048***	0.078	-0.026	-0.043
Two children	0.11***	-0.26*	-0.041	0.0034
Three+ children	0.13***	-0.17	0.13	0.14*
General education				
Lower secondary school	Comparison	Comparison	Comparison	Comparison
Intermediate secondary school	0.063***	0.34***	0.058***	0.084
University entrance qualification	0.0061	0.076	0.050	0.24*
Vocational qualification				
None	Comparison	Comparison	Comparison	Comparison
Vocational training	0.010	0.27**	-0.064	0.36***
Advanced vocational training	0.028	0.39**	-0.030	0.32***
University	0.090**	0.29	0.057	0.27
	0.25	0.34	0.23	0.31
N	*17807*	*452*	*2506*	*547*

Source: Microcensus 2000-2010

When looking more specifically at different sub-groups of the male, full-time working crafts population, it becomes apparent that distinguishing between construction and non-

construction occupations is important (see table 3, column 1 and 2). The deregulation appears to have reduced incomes by 6% in the construction crafts, a result which has high statistical significance. Symmetrically, when construction trades are excluded, there is no income impact of the deregulation for male full-time workers. Excluding the occupation 'floor tilers'changes nothing from the baseline results (see table 3, column 3).

Furthermore, it is interesting to note that in the sample consisting only of Meister-companies, the reform-effect is far from significant (see table 3, column 4). This implies that those companies in the previously regulated market overall have not seen their income drop as a result of the deregulation. This speaks against the predominant view of economic rents in the licensed occupations and favor the explanation that any drop in incomes are a result of changes in the average human capital of supply.

Table 3: Results by sub-group

	(1) Only construction sector	(2) Excluding construction sector	(3) Excluding tilers	(4) Only Meister craftsmen
Reformeffect	-0.058***	0.036	0.010	0.0089
Deregulated trade	0.088***	-0.067**	-0.14***	-0.19***
Post 2004	0.12***	0.13***	0.12***	0.12***
Age	0.033***	0.020***	0.028***	0.026***
Age square	-0.00040***	-0.00026***	-0.00033***	-0.00030***
Migrant	-0.064**	-0.12***	-0.065***	-0.032
Time-in-current-job	0.0088***	0.0067***	0.0069***	0.0056***
Number of employees	0.045***	N/A	0.046***	0.049***
Last labor market status				
Unemployed	Comparison	Comparison	Comparison	Comparison
Employed	0.053*	0.21**	0.086**	0.13**
Student	-0.27***	-0.15*	-0.18***	-0.11
Other	-0.031	0.17	0.0036	-0.11
Marital status				
Single	Comparison	Comparison	Comparison	Comparison
Married	0.10***	0.16***	0.12***	0.10***
Widow(er)	0.11	-0.0076	0.021	0.030
Divorced	0.045*	0.044	0.034	0.042*
Number of children				
None	Comparison	Comparison	Comparison	Comparison
One child	0.042**	0.055**	0.048***	0.046***
Two children	0.10***	0.13***	0.11***	0.092***
Three+ children	0.13***	0.15***	0.13***	0.14***
General education				
Lower secondary school	Comparison	Comparison	Comparison	Comparison
Intermediate secondary school	0.086***	0.073***	0.065***	0.056***
University entrance qualification	0.013	0.050*	0.0069	0.033*
Vocational qualification				
None	Comparison	Comparison	Comparison	Comparison
Vocational training	0.0012	0.0074	0.0075	N/A
Advanced vocational training	0.0079	0.076**	0.021	N/A
University	0.079	0.10*	0.087**	N/A
	0.25	0.22	0.25	0.25
N	10624	8709	17052	10379

Turning to the control variables, they mostly have the expected significance and sign.

The estimate for working in one of the deregulated trades is mostly negative and highly

significant and the estimate size almost identical to the existing literature (-10% in column 1, table 2 and -12% in Bol, 2014).

This is the same finding that Bol (2014) uses to argue the existence of economic rents as a result of licensing in the German crafts sector. However, due to the likely presence of unobserved heterogeneity between trades, these estimates should certainly not be interpreted as a reform effect, nor is it a perfect causal effect of working in a deregulated trade. One important source of heterogeneity is human capital differences, which are inherently very difficult to measure. Beyond individual differences, another important source of heterogeneity is differences in productivity between firms, for which there are no variables in the microcensus dataset.

In full-time samples, age and migrant status both influence earnings, as could be expected from the economic literature. Incomes rise for each additional year of age, whereas migrants face an income penalty. Unsurprisingly, the number of hours worked significantly increases income, as does an additional year spent in the current position.

Household composition also matters for the income, although men and women are somewhat differently affected, not surprisingly. Full-time males as these craftsmen with children have significantly higher incomes than their childless counterparts. This result could in theory be explained by both the public subsidies given to families (as the income variable is net income and not labor income) as well as previous findings in the literature suggesting that high earners are more likely to find a mate (implying reverse causality). Given that this effect can only be found for males and not for the female sample and that for that same sample being married has a significant positive effect on incomes, the latter explanation seems most probable. However, the former could be behind the significant effect of having more than three children on the incomes of part-time working women.

The human capital variables have the expected sign and are sometimes significant. The effect of the intermediate secondary school degree *(Realschule)* is the most robust, which is also the most prevalent general education among individuals starting a vocational education in the German crafts.[11] The effect of the Meister-qualification itself in terms of qualification, the effect of having completed advanced vocational education – which includes Meister craftsmen – is surprisingly not significant.

5.2   Validity

The specifications with alternative control groups corroborate the findings above. No significant income effects for male craftsmen are detected regardless of whether non-craftsmen form the control group or if the partially deregulated trades are switched from the control group to the treatment group (see table 4, coloumn 1 and 2). A significant positive income effect for women is found using both alternative control specifications (see table 4, column 3 and 4). The effect size when only fully deregulated trades form the control group is particularly strong. It should, however,be noted that the number of fully deregulated trades is very small, meaning that this specification should be interpreted with caution.

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[11] Among individuals being educated in the crafts in 2005, 40% had a so-called 'Realabschluss', the intermediate school form in the German educational system, see Mueller (2017).

Table 4: Results using alternative control groups

	Men full-time		Women full-time	
	(1)	(2)	(3)	(4)
	Non-craft	Only fully regul.	Non-craft	Only fully regul.
Reformeffect full deregulation	-0.017	-0.00046	0.085**	0.28***
Reformeffect partial deregulation	N/A	0.01	N/A	0.19***
Fully deregulated	-0.034**	-0.24***	-0.048**	-0.83***
Partial deregulated	N/A	-0.11***	N/A	-0.39***
Post 2004	0.11***	0.12*	0.090***	-0.025
Age	0.020***	0.024***	0.027***	0.035***
Age square	-0.00021***	-0.00029***	-0.00031***	-0.00045***
Migrant	-0.060***	-0.062***	-0.040**	-0.082*
Time-in-current-job	0.0094***	0.0067***	0.0098***	0.0080***
Number of employees	0.045***	0.043***	0.039***	0.043***
Last labor market status				
Unemployed	Comparison	Comparison	Comparison	Comparison
Employed	0.24***	0.076**	0.17***	0.17***
Student	-0.022	-0.19***	-0.097*	-0.23
Other	0.16***	-0.0065	-0.0017	0.21*
Marital status				
Single	Comparison	Comparison	Comparison	Comparison
Married	0.14***	0.11***	0.020	-0.0053
Widow(er)	0.073**	0.045	0.12***	0.22***
Divorced	0.048***	0.033	0.067***	0.11***
Number of children				
None	Comparison	Comparison	Comparison	Comparison
One child	0.042***	0.049***	0.022	-0.013
Two children	0.12***	0.11***	0.044***	-0.028
Three+ children	0.18***	0.13***	0.090***	0.13
General education				
Lower secondary school	Comparison	Comparison	Comparison	Comparison
Intermediate secondary school	0.11***	0.058***	0.14***	0.059***
University entrance qualification	0.14***	0.00029	0.14***	0.050
Vocational qualification				
None	Comparison	Comparison	Comparison	Comparison
Vocational training	0.067***	0.017	-0.031*	-0.038
Advanced vocational training	0.087***	0.025	-0.021	-0.014
University	0.16***	0.094**	0.070***	0.071
	0.35	*0.26*	*0.33*	*0.26*
N	74968	17807	26941	17807

Source: Microcensus 2000-2010

Looking at lagged effects over a period of three years does not yield any new substance to the interpretation (see table 5). I would also caution against putting much weight on the interpretation of lagged effects as Lergetporer et al. (2016) do, since the more the analysis moves away from the reform implementation in time, the more one should be

concerned that estimates may be picking up irrelevant developments affecting the outcome variable.

Table 5: Results with yearly interaction terms

| | Men full-time | | Women full-time |
	(1) Yearly interaction terms all sectors	(2) Yearly interaction terms only construction	(3) Yearly interaction terms all sectors
formeffect 2000			
Reformeffect 2001	0.025	0.084***	-0.16**
Reformeffect 2002	0.0094	0.022	-0.10
Reformeffect 2003	0.039	0.073*	0.05
Reformeffect 2004	0.027	0.032	-0.07
Reformeffect 2005	0.011	-0.0051	0.09
Reformeffect 2006	0.011	-0.016	0.05
Reformeffect 2007	0.049	0.020	0.02
Reformeffect 2008	-0.023	-0.074***	0.05
Reformeffect 2009	N/A	N/A	N/A
Reformeffect 2010	N/A	N/A	N/A
Deregulated trade	-0.16***	0.052**	-0.39***
Age	0.024***	0.031***	0.035***
Age square	-0.00029***	-0.00037***	-0.00045***
Migrant	-0.062***	-0.051*	-0.078***
Time-in-current-job	0.0066***	0.0084***	0.0080***
Number of employees	0.043***	0.042***	0.043***
Last labor market status			
Unemployed	Comparison	Comparison	Comparison
Employed	0.076**	0.041	0.18***
Student	-0.19***	-0.27***	-0.22
Other	-0.0049	-0.032	0.20*
Marital status			
Single	Comparison	Comparison	Comparison
Married	0.11***	0.094***	-0.0090
Widow(er)	0.045	0.11	0.21***
Divorced	0.034*	0.040	0.12***
Number of children			
None	Comparison	Comparison	Comparison
One child	0.049***	0.043**	-0.014
Two children	0.11***	0.10***	-0.029
Three+ children	0.13***	0.13***	0.14
General education			
Lower secondary school	Comparison	Comparison	Comparison
Intermediate secondary school	0.058***	0.062***	0.059***
University entrance qualification	0.00036	-0.010	0.045
Vocational qualification			
None	Comparison	Comparison	Comparison
Vocational training	0.017	0.011	-0.044
Advanced vocational training	0.024	0.0058	-0.019
University	0.093**	0.085	0.075
	0.26	0.26	0.24
N	17807	10624	2506

Source: Microcensus 2000-2010

The placebo test lends further credibility to the absence of income effects for male craftsmen. Simulated policy changes before 2004 all have insignificant effects on income (see column 1, table 6). The same procedure mostly holds for construction crafts trades , but fails in 2001, where a significant placebo-reform is found (see column 2, table 6). Hence, the significant negative income-effect found in the construction sector should be taken with a grain of salt. As for the female sample, the common trends assumption appears clearly to be violated since the placebo-reform is significant in the two years proceeding the deregulation (see column 3, table 6). This problem is consistent with concerns in Lergetporer et al. (2016) that differences between treatment and control groups may in some cases be too heterogeneous to identify a causal difference-in-difference estimator.

Table 6: Placebo-tests checking whether the common trends assumption holds

| | Men full-time | | Woman full-time |
| | (1) | (2) | (3) |
	Placebo-reform all sectors	Placebo-reform only construction	Placebo-reform all sectors
2001	0.03	0.06**	-0.09
2002	0.01	0.01	0.03
2003	0.02	0.03	0.09**
2004	0.02	0.03	0.09**
	0.25	0.24	0.27
N	9682	5621	1280

Source: Microcensus 2000-2010

Lergetporer et al. (2016) address this in one of their estimations using propensity score matching. In doing so, the negative effect on incomes as a result of the deregulation all but disappears. This speaks for the results obtained in this study, nevertheless optimizing the chances of casual interpretations remain an important field for future policy research also in the case of the German crafts sector.

6.   Discussion

This study is one of the first to exploit a natural experiment to examine the income premiums resulting from occupational licensing. This study challenges the mainstream view that there is a significant income premium benefiting professionals in licensed occupations. I find no evidence that reforming the traditional licensing scheme in the German crafts sector negatively affected the incomes of male craftsmen overall. Certain groups may have been affected (men in the construction sector negatively and females positively), however in both cases, I find worrying signs that the common trends assumption may not be met, reflecting a potential problem that is for the moment left unaddressed.

The economic reasoning in previous studies focus on the mechanism whereby market entry barriers lead to higher prices and hence increase producers' incomes. However, other mechanisms that counterweigh this may be at work, which can help explain my contradictory findings. First, it is possible that there are 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; and Bresnahan and Riess, 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 already have been (close to) sufficient to ensure prices close to marginal costs.

Moreover, the demand-side should also be considered. Quality may be lower in non-competitive markets, which over time could have reduced consumers' willingness to pay for the service. The empirical literature on the link between occupational licensing and quality is inconclusive (see e.g. Carroll and Gaston, 1983; Shilling and Sirmans, 1988; and Angrist and

Guryan, 2007). Higher prices under licensing could also have led 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, demand for crafts services would be lower under occupational licensing putting a downward pressure on the incomes of licensed professionals and free entry could have sparked new earning potentials.

A licensing requirement may also increase incomes simply due to increased human capital and/or as a result of signaling to consumers that the occupation provides services of a particular quality (see Weeden, 2002). In this case, licensing would indeed lead to higher incomes and, vice versa, deregulation would lead to lower incomes. However, this developments are then a result of changes in productivity of professionals and not economic rents. The results in this study lends some credibility to this thesis as the removal of occupational licensing in the German craft sector did not affect Meister-companies, i.e. incumbents in the market.

Also, other (and less restrictive) education policies and means to signal quality such as screening, reputation mechanisms or voluntary certification could achieve the same outcome as licensing. If consumers have the means to find the quality that they seek, market entrants that propose a lower price/quality bundle should not pose a threat to incumbents. In the German case, the Meister-certificate still exist in the deregulated segment and indeed consumers of crafts services perceive that Meister craftsmen in deregulated trades offer higher quality than their non-Meister counterparts (see Fredriksen et al., forthcoming). Furthermore, while the reform in 2004 increased market entry rates, it also increased market exit rates, particularly among newly-founded companies (see Runst et al., forthcoming).

7.   Conclusion

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. This study criticizes the methodology and interpretations in existing studies on the income effects of the German deregulation on incomes and criticize established findings in the literature on occupational licensing and incomes.

The standard explanation for why occupational licensing increases incomes of licensed professional is reduced competition, which allows them to charge higher prices. Two different stories may however be told depending on the degree of competition in the regulated market: If competition is a regulated market is nonetheless high, there is no room to charge higher prices. If competition is low, so may productivity and incentives to innovate be. If the quality of products suffer, the demand-side will put a stop on the scope to raise prices. In either of these two cases, licensing would not lead to higher incomes.

The main implication that can be derived from my results is that public regulation is likely to affect economic outcomes through a variety of channels where some are more present in the policy debate than others. On top of this, effects may well vary across population groups. For these reasons, regulatory interventions in markets should not be taken lightly, but enacted with care and continually evaluated as the consequences unfold.

8.   Literature

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