# Does Regulation Trade-Off Quality against Inequality?
## The Case of German Architects and Construction Engineers

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**Abstract**

*We exploit an exogenous price increase by about 10% for architectural services to answer the question how price regulation affects income inequality and service quality. Using individual-level data from the German microcensus for the years 2006 to 2012, we find a significant reform effect of 8% on personal net income for self-employed architects and construction engineers. This group moved from the second lowest to the highest quintile of the net income distribution. This increase in inequality is associated with a deterioration of service quality. The reform reduced average scores of a peer ranking for architects by 18%.*

**Keywords** Regulation · Inequality · Wages · Service Quality · Entrepreneurship · Natural Experiment

**JEL Classification** L5 · L11 · L74 · J44 · L26

### 1. Introduction

Regulation typically comes in two forms: entry regulation and price regulation. Both represent a severe market intervention (e.g., Koumenta and Pagliero 2018; Kleiner and Krueger 2013, 2010). The main argument in favour of regulation is to provide a minimum level of quality. A layman is often not able to objectively judge the quality of services provided by professionals like architects and engineers (Friedman and Kuznets 1954). This may result in prices that do not reflect the true valuation of consumers. Therefore, prices for experience or credence goods are often fixed by law. Such price regulations are particularly prevalent among the so-called liberal professions (lawyers, physicians, tax advisors, etc.) in many European countries.[1] However, they may have unintended effects on the labour market. In particular, these regulations may result in

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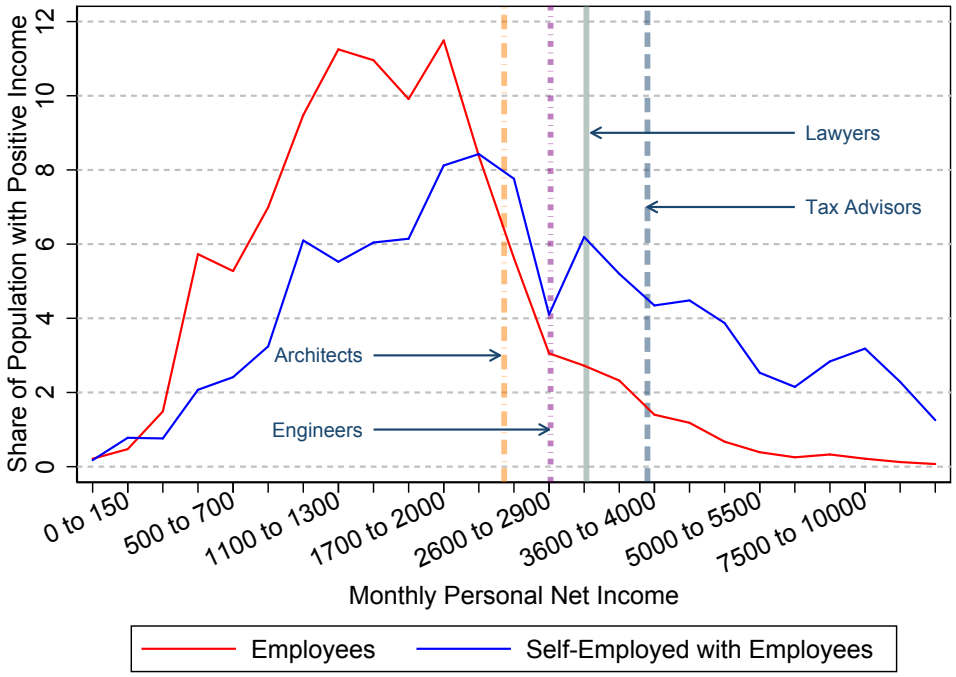
[1] A non-exhaustive list of examples shows that binding price regulation exists for services of accountants in Germany and Greece, lawyers in Austria, Germany, Greece, and Italy, notaries in Austria, Belgium, France, Germany, Italy, Netherlands, and Spain, architects in Belgium, Germany, Italy, and Luxembourg, engineers in Germany, Italy, Luxembourg, and pharmacists in Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, Netherlands, Portugal, Spain, Sweden (Paterson et al. 2007).

FIGURE 1

Distributions of Monthly Net Income of Self-Employed and Employees




*Notes:* This figure plots the income distributions (monthly net income) separately for employees (in red) and self-employed individuals who have their own employees (in blue) for the year 2009. The vertical lines document the average net income for architects, engineers, lawyers and tax advisor.

*Source:* Own calculations based on the scientific use file of the German microcensus (2006-2012).

higher income inequality. Evidence for the US, presented in Smith et al. (2017), showed that firms owned by the top 1-0.1% income earners are single-establishment firms in professional services (e.g., consultants, lawyers, skilled tradespeople) or health services (e.g., physicians, dentists). Figure 1 shows that also in Germany, the self-employed individuals—which are very often exposed to professional regulation—have higher incomes compared to employees. Evidence on how regulation affects income inequality and service quality is of primary importance for our understanding of economic behaviour and for the organisation of markets. This paper contributes first evidence on the association between regulation and income inequality on the one hand and regulation and service quality on the other hand. In particular, we provide an answer to the question whether business owners used extra revenue generated by an exogenous price increase to raise wages of employees, to invest in quality or to increase their own incomes.

To answer our key questions, we use the unique setting of architects and construction engineers in Germany as a natural experiment in which the price level was increased in the year 2009. This price hike was exogenous, since prices were not determined by demand and supply but instead by price regulation (called "Honorarordnung für Architekten und Ingenieure", HOAI), which sets both price ceilings and floors

for all architectural services offered by architects and construction engineers during the entire period of observation. At the same time, the actual market price was unknown and could have lied always below the price stipulated by the legislation. This is clearly the case in the extreme case of very strong information asymmetries, where the market would fail to agree on a price above zero (Akerlof 1970). Therefore, we contrast the income distribution of regulated architects and construction engineers to that of their unregulated peers, i.e. we use "all other engineers" operating in markets where prices are determined by supply and demand as a control group. The HOAI is currently subject to a heated debate. The European Commission claims that this strict price regulation is not compliant with the European Union Services Directive (Directive 2006/123/EC) and therefore opened an infringement case against Germany. In the past, these fixed prices were changed by amendments to the HOAI. In particular, prices increased on average by about 10% from 2009 onwards. This drastic change is the natural experiment that we exploit to study the reform effects on income, income inequality, and measures of service quality.

We estimate the causal effects of the reform using various Difference-in-Differences (DD) models on two datasets. The main analysis is based on individual-level data from the German microcensus for the years 2006 to 2012 that provides information on treatment status, personal net income, weekly hours worked and further relevant individual-level characteristics. In a first step, to investigate how the exogenous price increase affected incomes, we estimate a linear DD model using self-reported data on the individual-level asking for profession and industry to define the treatment group that includes all architects and construction engineers who work either as self-employed or employees in firms that experienced the price increase. The control group comprises all other engineers (self-employed or employees) where prices are determined by market forces. We find that incomes of self-employed architects and construction engineers increased significantly by 8.0% while incomes for employees did not change at all. This implies that the full share of the price hike ended up in the business owners' pockets and that there is no evidence that any part of the extra profits affected employees' incomes.

Second, we estimate multinomial logit DD models with maximum likelihood for the quintiles of the personal net income distribution of all self-employed individuals. The results indicate that the probability to belong to the second lowest 20% and the mid 20% of the personal net income distribution decreased due to the HOAI reform by 5 percentage points, while the probability to stay in the second highest and highest 20% increased correspondingly. This is consistent with a novel alternative estimation strategy: We fit the multinomial logit DD model for all engineers who were not subject to price regulations both before and after the reform and for architects and construction engineers who were subject to the regulation but only before the reform. We then perform out-of-sample predictions with our estimates to simulate what part of the income distribution an individual architect or construction engineer would have belonged to if the reform would not have occurred. The idea is identical to the usual DD approach except for the fact that we do not use post-treatment information for the predictions. The main advantage is that the average

treatment effect on the treated (ATT) is much easier to obtain in non-linear models. A comparison with the actually observed frequencies in the respective income classes shows that the probability to stay in the top two quintiles increased by about 5 percentage points.

Finally, we show how service quality changed due to the reform. Using a well-known office ranking within the architectural profession, we find that the exogenous price increase did not have the intended positive quality effects. In contrast, by using the Synthetic Control Method (SCM) following Abadie and Gardeazabal (2003), the reform had a negative effect on service quality with a significant decrease of two score points within that ranking which corresponds to a decrease by 18%.

This article is organised as follows. In Section 2, we give a brief overview of the literature on regulation and quality. Section 3 provides institutional details on the HOAI reform. We describe the dataset in Section 4. In Section 5, we present the empirical strategy and report our results. Section 6 concludes. Additional information on background information and robustness tests can be found in our Online Appendix.

## 2. Regulation, Quality, and Inequality: Theory and Existing Evidence

Our study lies at the intersection of labour economics, law, and industrial organisation and is thus related to studies on the labour and product market effects of regulation. Although we focus on a particular kind of regulation, namely price regulation, our approach is close to that of studies on occupational licensing (Kleiner 2000).

With respect to product market effects, recent evidence (e.g., Pagliero 2013, 2011) suggests that entry requirements create monopoly rents within an occupation. On the other hand, theoretical models show that regulation can be welfare increasing since it guarantees a minimum level of quality if quality is uncertain (Pigou 1938; Akerlof 1970; Leland 1979). However, there is few empirical evidence for quality or productivity effects of regulation. While Rostam-Afschar (2014) finds that less entry regulation in the German crafts sector mainly resulted in new entries by untrained workers, Kleiner and Kudrle (2000) find few effects of tougher regulation on malpractice insurance rates or complaints to state licensing boards. In this light, the case of the HOAI is particularly interesting to study, since the German Monopolies Commission (2006) claims that the introduction of this price regulation in 1977 was not a response to actual problems with quality in the construction sector. Still, the legislation was justified to shut down price competition such that firms may focus on competition in quality.

There is more evidence on the effects of entry regulation on incomes and wages; and this evidence consistently finds a positive, economically significant effect. For instance, Koumenta and Pagliero (2018) show that entry regulation is associated with 4% higher hourly wages for the European Union. Gittleman and Kleiner (2016) find a somewhat larger effect for the US. We add to this literature by showing how *price regulation* affects incomes, and—since labour supply of architects and engineers is quite inelastic—hourly

wages. Moreover, we are able to show how the price increase is split in wage increases for employees and self-employed individuals.

Finally, our analysis contributes to the literature on income inequality and wealth inequality. Though Fossen et al. (2017) show that entry regulation could be an important determinant of the wealth concentration, we leave it to future research to investigate this for our case because our data does not provide information on wealth. However, the data provides a measure on personal net income, which allows us to study income inequality. It is well known that wage increases in the form of performance pay—intended to boost output quality—increase the income dispersion (Bryan and Bryson 2016). However, the role of regulation with the same objective is still unclear. While Kleiner and Krueger (2013) and Gittleman and Kleiner (2016) do not find significant effects of entry regulation on the wage dispersion in the US, Koumenta and Pagliero (2018) provide evidence that licensing disproportionately benefits those at the top of the income distribution. We add to this literature by focusing on price regulation and applying methods in a novel way to measure the effect on inequality.

## 3. Regulation of Architects and Engineers and the HOAI-Reform 2009

Though we focus on price regulation, this is not the only form of regulation for architects and construction engineers in Germany. Entry as well as carrying the professional title "architect" or "engineer" is conditional on having i) completed at least four years of studies and ii) at least two years of work experience plus iii) on having registered as paid member in the German Chamber of Architects, a public institution. Only listed professionals are entitled to submit their planning documents in order to obtain planning permissions from German construction authorities. Moreover, to attain continued education is mandatory.[2] The fact that these regulations did not change in the period that we focus on, enables us to isolate the effect of the price increase caused by the HOAI reform in 2009.

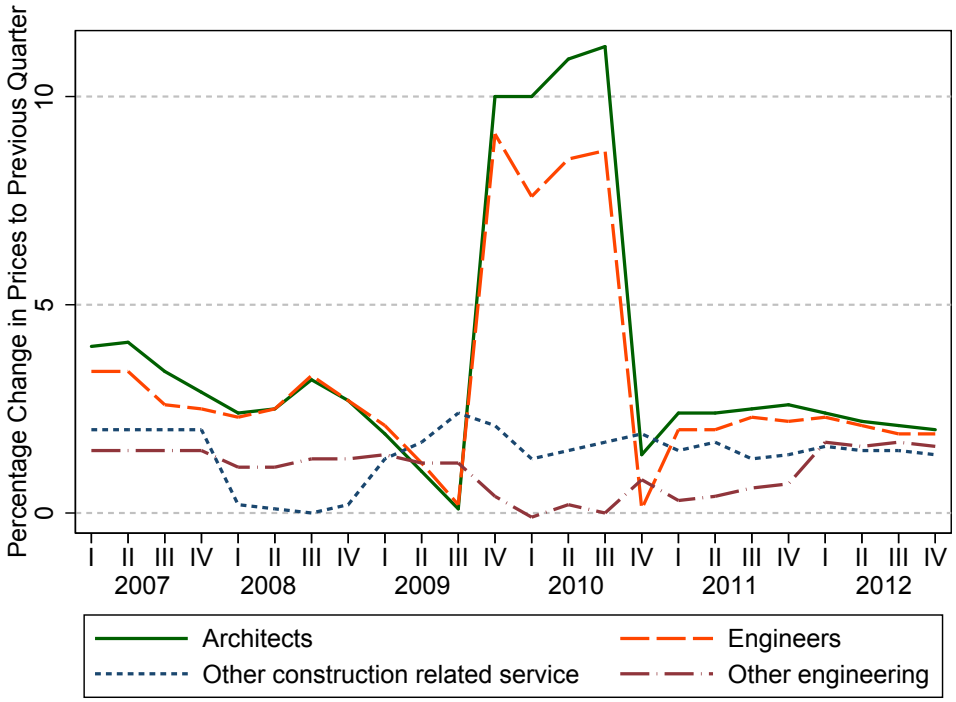
The German Fee Scale for Architects and Engineers ("Verordnung über die Honorare für Leistungen der Architekten und der Ingenieure", HOAI) is a federal ordinance to regulate the remuneration of services rendered by architects and engineers operating in the construction sector.[3] Based on the Law on the Regulation of Services of Engineers and Architects, it is considered a binding price law for all planning and monitoring services offered by architects and engineers. It has its roots already in 1871 when a non-binding recommendation for fees was introduced. The economic policy in the National Socialist period favoured regulation of economic activity and introduced price regulation for architects in 1935 which were adopted as binding price ceilings for architects in 1950, while price recommendations for engineers remained unbinding. When

[2]Requirements to provide evidence of continued education vary across federal states.

[3]Our Online Appendix provides a more detailed description of the HOAI in Section A, including an example for the fee scale in Table A2.

FIGURE 2

The HOAI Reform 2009 as Natural Experiment




*Notes:* This figure plots the percentage change in prices relative to the previous quarter against time for the years 2007 to 2012.

*Source:* Own calculations based the consumer price index provided by the German Federal Statistical Office.

the HOAI that today exists came into force in 1977, binding price floors were added and coverage was extended to construction engineers. In practice, if an architect or construction engineer posts a price below (or above) the statutory price span on her website, this is a violation of the price regulation and simultaneously an act of unfair competition ("German Act against Unfair Competition") which is usually settled after warning letters with a declaration to cease and desist, but can lead to fines up to 250.000 € or up to two years of incarceration.[4]

Since 1977 prices have been raised several times with the last revisions of the HOAI price schedule in 1996, 2009 and 2013[5]. While the reform of 2009 had been initiated already in 1995 (see the "Bundesratsbeschluss vom 06.06.1997" combined with its resolution five weeks later), it was not clear that it would indeed result in a price increase, since even 10 years later when the grand coalition came into power in 2005, it was discussed whether to abolish the price regulation altogether (see Jochem and Kaufhold 2016). A final draft of the reform had been proposed only in the End of April 2009 (see "Bundesrat Drucksache

[4]For instance, see verdicts of German regional high court Oldenburg in 2007 (1 W 39/07) and of German regional high court Celle in 2009 (13 U 86/09).

[5]We do not focus on the 2013 price change, since our data is available only until 2012.

395/09") after a first draft from February 2008 was discussed. The amendment to the HOAI was announced in June 2009 to be effective as of August 2009. Since there was only a very short time between the announcement and the date of effectiveness, it is highly unlikely that behaviour has been adjusted in anticipation of the concrete content of the reform. In the Online Appendix, we show that internet searches for keywords related to the HOAI increased very shortly before the reform came into effect.

The reform in 2009 changed both price floors and ceilings of specific services provided by architects and construction engineers for the 133,000 architects and 144,000 construction engineers in Germany (according to official figures of the Federal Chamber of Engineers). The effect of the reform is clearly visible in Figure 2, which shows the percentage change in the producer price index provided by the German Statistical Office (based on the HOAI, construction statistics, national accounts and a survey of 143 offices) from 2007 to 2012. Strikingly, the binding price increase resulting from the HOAI reform immediately translated into practice. The producer price index sharply increased in the third quarter of 2009 with an average price increase of about 10% when the HOAI 2009 came into effect. For other engineers or providers of other construction services, price growth fluctuated around a small positive value which shows that the reform clearly did not influence the prices of our control group during the sample period.

## 4. Data and Descriptive Statistics

We employ data from the German microcensus, which is an official, representative annual household survey comparable to the Current Population Survey in the United States. It covers approximately 830,000 individuals in 370,000 households, which is 1% of all private households in Germany. Due to its mandatory nature, the data guarantee a low rate of item non-response, which is a major advantage compared to other surveys. This also ensures that our groups of interest, architects and engineers who comprise 3.2% of the working population in Germany in 2011, are adequately represented.

This empirical analysis uses pooled cross-sectional data of the German microcensus for the years 2006 to 2012 covering the major HOAI reform in 2009. For our DD analyses, we restrict the sample to architects, engineers in the construction sector and "all other engineers" with the latter forming our control group. We further exclude individuals younger than 23 years, or older than 67 years as well as family workers supporting a family business. After these minor sample adjustments, we are left with around 35,000 observations in total. See Table 1 for descriptive statistics broken down by employment and treatment status.

Table 2 provides summary statistics for all of our outcome variables. It shows the weighted averages for each time period with "Pre" referring to the years 2006 to 2009 (before the HOAI 2009), and "Post" to the years 2010 to 2012 (subject to HOAI 2009). The reported averages are weighted using survey weights provided by the German microcensus.

TABLE 1
Outcomes and Controls by Treatment and Employment Status

| | Treatment group | | | Control group | | |
|---|---|---|---|---|---|---|
| | All | Self-employed | Employed | All | Self-employed | Employed |
| Female | 0.29 | 0.22 | 0.36 | 0.10 | 0.06 | 0.11 |
| Married | 0.86 | 0.78 | 0.92 | 0.84 | 0.69 | 0.86 |
| German citizen | 0.95 | 0.96 | 0.95 | 0.93 | 0.95 | 0.93 |
| Age (in years) | 43.82 | 47.71 | 39.81 | 43.25 | 49.20 | 42.74 |
| Number of children | 0.85 | 0.90 | 0.81 | 0.87 | 0.84 | 0.87 |
| Lowest school degree | 0.01 | 0.01 | 0.01 | 0.03 | 0.03 | 0.03 |
| Middle school degree | 0.02 | 0.02 | 0.02 | 0.07 | 0.07 | 0.07 |
| Highest school degree | 0.97 | 0.97 | 0.98 | 0.90 | 0.90 | 0.90 |
| Hours Worked | 42.60 | 45.99 | 39.12 | 41.18 | 47.41 | 40.66 |
| Net income very low | 0.13 | 0.16 | 0.11 | 0.03 | 0.11 | 0.03 |
| Net income low | 0.31 | 0.20 | 0.42 | 0.17 | 0.16 | 0.17 |
| Net income mid | 0.24 | 0.20 | 0.28 | 0.31 | 0.17 | 0.32 |
| Net income high | 0.13 | 0.16 | 0.10 | 0.26 | 0.18 | 0.27 |
| Net income very high | 0.12 | 0.20 | 0.05 | 0.17 | 0.28 | 0.16 |
| Self-Employed (in %) | 50.79 | 100.00 | 0.00 | 7.77 | 100.00 | 0.00 |
| Firmsize | 12.91 | 3.40 | 22.71 | 42.45 | 6.23 | 45.43 |
| Cont. Education (in %) | 32.69 | 33.43 | 31.93 | 33.37 | 24.78 | 34.09 |
| Cont. Education (in hours) | 16.70 | 17.35 | 16.03 | 19.88 | 15.76 | 20.23 |
| Tenure (in years) | 10.01 | 12.74 | 7.08 | 11.41 | 11.29 | 11.42 |
| Observations | 4,834 | 2,487 | 2,347 | 34,523 | 2,733 | 31,790 |

*Notes:* This table reports weighted averages for all of our outcome and control variables split by treatment and employment status. The treatment group consists of architects and construction engineers while the control group comprises "all other engineers". The numbers are weighted by survey weights provided by the microcensus.
*Source:* Own calculations based on the scientific use file of the German microcensus (2006-2012)

A striking fact from Table 2 is that the share of self-employed architects is exceptionally high. In the pre-reform period, 52.3% of all registered architects and construction engineers were self-employed. This share decreases by 3 percentage points to 49.1% after the HOAI reform. In comparison, the self-employment rate for other engineers—which form our control group—stays quite constant at a much lower level of around 8%. Considering hours worked, architects and construction engineers apparently work 43 hours on average per week in the pre-treatment period, while this figure is a bit lower for the control group (41 hours). On average, architectural and construction engineering firms are smaller as measured by the number of employees than other firms. While the average firm counts around 42 employees, architects firms consist of only 13 employees. Over the sample period, average firm size slightly increased for architects and HOAI engineers, while it stayed almost constant for the control group. Finally, around one third of all architects and engineers participated in continuing education programs and on-the-job training. Within a year, architects received 18 hours of training in the time before the reform was introduced. This number decreased to 15 hours after the HOAI reform.

Respondents are explicitly asked for their net income which includes salaries and wages, bonus payments like 13th month pay, business income, transfers like child allowances and unemployment benefits net of taxes (that is wage tax, payroll tax and social security contributions) in the microcensus.

TABLE 2
Outcome by Treatment Status and Reform Time

| | Treatment group | | Control group | |
|---|---|---|---|---|
| | Pre | Post | Pre | Post |
| Professionals (per 1,000 working individuals) | 3.23 | 3.54 | 24.14 | 24.52 |
| Hours worked (in hours) | 43.05 | 42.07 | 41.05 | 41.35 |
| Net income very low | 0.14 | 0.12 | 0.04 | 0.03 |
| Net income low | 0.32 | 0.30 | 0.19 | 0.15 |
| Net income mid | 0.23 | 0.25 | 0.31 | 0.31 |
| Net income high | 0.12 | 0.14 | 0.25 | 0.27 |
| Net income very high | 0.12 | 0.13 | 0.15 | 0.20 |
| Self-employed (in %) | 52.26 | 49.05 | 7.93 | 7.58 |
| Firmsize | 12.33 | 13.60 | 42.37 | 42.56 |
| Cont. Education (in %) | 32.83 | 32.53 | 33.42 | 33.30 |
| Cont. Education (in hours) | 18.12 | 15.01 | 19.55 | 20.30 |
| Observations | 2,625 | 2,209 | 19,392 | 15,131 |

*Notes:* This table reports weighted averages for all of our outcome variables split by treatment status and reform time. The treatment group consists of architects and construction engineers while the control group comprises all other engineers. "Pre" includes the years 2006 to 2009 and "Post" refers to the years 2010 to 2012. All numbers are weighted by survey weights provided by the microcensus.
*Source:* Own calculations based on the scientific use file of the German microcensus (2006-2012)

This information is provided in income categories going from "below 150€" to "more than 18,000€". In total, there are 24 income bins in between such that the income information is sufficiently detailed. Except for the descriptive statistics, we treat the income measure as a continuous variable (logarithmised). For the former, we build the following five categories: very low income: income $<$ 1300; low income: 1300 $\geq$ income $<$ 2000; middle income: 2000 $\geq$ income $<$ 2900; high income: 2900 $\geq$ income $<$ 4000; very high income: income $>$ 4000. Unfortunately, gross income is not available in the dataset.

Our individual-level control variables include gender, marital status, an indicator of German citizenship, number of children, education dummies, tenure and tenure squared (see Table 1). Except for gender, architects and engineers do not differ significantly in their baseline characteristics compared to control engineers (columns "All"). On average, they are 44 years old. 86% are married and they have on average 0.9 children. However, in both professions, women are strongly underrepresented. While the share of women among all architects and construction engineers amounts to 30%, this share is 10% for all other engineers.

Figure B2 in the Online Appendix shows the personal net income distribution for architects and construction engineers for the pre- and post-reform period for employees and for self-employed individuals, respectively. The red (blue) vertical lines indicate the median, which is just below 2,500 Euro for both before and after and for not self-employed and for self-employed. These figures show strikingly that the distribution of income is much more equal for self-employed than for none-self-employed: The highest bars of the distribution of self-employed just exceed 20% of the population for self-employed, while the second bar of low income earners make up well above 30% of the population for none-self-employed. Focusing on the pre and post differences of the height of the bars, it appears that the lowest parts of the population decreased and the fourth bar increased in both figures. This suggests that the reform could have moved individuals in

higher income classes. However, Figure B3 demonstrates that at least some of the upward mobility was a general effect, possibly due to the Great Recession of 2008/2009, since it shows that this can be observed in the distribution of all working persons (except for architects and construction engineers) as well. A second striking observation from these figures is that the distribution of income is much more unequal even for self-employed for persons who are not architects or construction engineers. Moreover, the movements to higher income classes seems to be not as concentrated as for the treatment group. This points to a positive effect of the price regulation both on income and on the position in the income distribution.

But what does this mean for wages? The distribution of usual weekly hours worked (available on request) shows that some of the inequality in incomes is due to 10 extra hours that the median self-employed works per week compared to her employee peers. This is a typical pattern that can be explained by precautionary labour supply (cf. Jessen et al. 2018). The reform effect on income, which is estimated below, however, seems to translate directly to hourly wages, since there is hardly any change in labour supply before and after the reform.

## 5. Empirical Methods and Results

We specify DD models of the following general form

$$E[y_{igt}|\mathbb{1}_g^{\text{Treated}}, \mathbb{1}_t^{\text{Post}}, X_{igt}] = T(\gamma \mathbb{1}_g^{\text{Treated}} + \vartheta \mathbb{1}_t^{\text{Post}} + \omega \mathbb{1}_g^{\text{Treated}} \mathbb{1}_t^{\text{Post}} + X_{igt}\xi), \tag{1}$$

where $T(z_{igt})$ is a transformation function, $y_{igt}$ is a measure of income, income inequality, or service quality that varies across units $i$, group status $g \in \{\text{Treated}, \text{Untreated}\}$, and time $t \in \{\text{Pre}, \text{Post}\}$. $\mathbb{1}_g^{\text{Treated}}$ is an indicator equal to 1 if a unit was treated, $\mathbb{1}_t^{\text{Post}}$ is an indicator equal to 1 if an observation belongs to the post reform period. Control variables indicating year, federal state, nationality, children, gender, marital status, educational and vocational qualification, tenure and its square are included in the vector $X_{igt}$. $\gamma$, $\vartheta$, and $\omega$ are coefficients to be estimated.

We define the time span from 2006 to 2009 as pre-treatment period and the time span from 2010 to 2012 as post-treatment period. In the main analysis with the German microcensus, we use individual-level data and assign each individual to the treatment or control group based on the self-reported occupation and industry branch of activity. For the analysis of the peer ranking data, we use country-level data with Germany as treatment group and determine a synthetic control group with the method developed by Abadie and Gardeazabal (2003). In the investigation of income and the peer ranking score, we use a linear transformation function where $T(z_{it}) = z_{it}$. For the analysis of the income distribution, we use a DD multinomial logit model where $T(z_{it})$ is a non-linear function.

Identification of the ATT depends on the absence of pre-treatment effects. If income, inequality or quality measures are adjusted in anticipation of the reform, estimation of the ATT could be biased. In our

application, this is unlikely to be the case, since although the amendment to the HOAI was announced in 2005, violations of the price regulation were prosecuted until the amendment came into effect. In the Online Appendix gives an overview of the timing of events and shows that internet searches for keywords related to the HOAI remained low until shortly before the reform came into effect and dramatically increased thereafter.

In addition, the causal effect for each individual must be stable in order to get an unbiased estimate. This means in the case of architects that first, the amendment has the same effect on all architects, and second, that the effect of the amendment to an individual is independent of the exposure of other individuals. While the first requirement is met because the price regulation applies uniformly to all architects, the second requirement could be violated if, e.g., an architect and an engineer who is not subject to the price regulation contribute to the same household income as a married couple. Although we use information on individual incomes, joint filing for married couples could result in a bias of the estimator.

Finally, we need to assume that the proportionality of trends of the outcome measures for treated and untreated that we observe pre-treatment would have been the same after the reform in absence of the treatment. As a first check, Figure B1 in the Online Appendix plots the average of log income against time. The lines move quite parallel up to 2008 where income seems to be quite constant. Then, there is a small increase in the net income for both self-employed groups in 2009 (same parallel trend), but then the two dashed lines, which represent the net income for self-employed, clearly go in different directions. While the time series for self-employed other engineers slightly falls after 2009, the income for self-employed architects and construction engineers keeps increasing a bit and then stays constant at a higher level (which is what we would expect). As a second check, we also conducted an event study, which will be discussed in the next section.

### 5.1 Effects of Price Regulation on Income

The first question we answer is concerned with the enforcement of the price regulation and the use of the price increases. If there is full enforcement, revenues must have increased by on average 10% all else equal. Part of this extra revenue could have been used to increase the capital stock or hire more employees (see Section 5.3). Since architects and engineers work with little non-human capital and few employees, profits would increase by 10% if no extra costs were incurred. The business owner then could decide to split the extra profits among her employees and take the residual as extra income. Whether the increased revenue is shifted to the firms' employees depends on the structure of the labour market. While wages (and thereby net incomes given inelastic labour supply in the short run) would increase in a competitive labour market, this would not happen if firm owners have some (local) monopsony power. In the latter case, we would expect to see an increase in the firm owners' income.

Table 3 shows estimates of the ATT obtained with a linear OLS model. The first column excludes all employees from the sample and shows estimates for a model that includes only three variables and a

constant: an indicator for the treatment group, the post policy period, and its interaction. The coefficient on the interaction term $\omega$ is 9.7% which is statistically not different from the hypothesized 10% extra profits. The coefficient on the treatment indicator shows that architects and construction engineers earn on average about 22% less than engineers in the control group. This reduces to 14.5% with the inclusion of control variables.

TABLE 3
Effects of Regulation on Income for Architects and Engineers

| | I | II | III | IV | V | VI | VII |
|---|---|---|---|---|---|---|---|
| Sample | Self-Employed | Self-Employed | Employees | All | Self-Employed | Employees | All |
| Treated × Post | 0.097** | 0.080* | 0.005 | 0.010 | | | |
| | (0.048) | (0.044) | (0.021) | (0.019) | | | |
| Treated | -0.219*** | -0.145*** | -0.218*** | -0.175*** | -0.150** | -0.182*** | -0.167*** |
| | (0.031) | (0.029) | (0.015) | (0.013) | (0.060) | (0.029) | (0.028) |
| Post | 0.007 | -0.075 | 0.064*** | 0.073*** | | | |
| | (0.033) | (0.046) | (0.008) | (0.010) | | | |
| Regulated × Placebo Post | | | | | -0.003 | -0.080* | -0.040 |
| | | | | | (0.081) | (0.042) | (0.039) |
| Placebo Post | | | | | 0.007 | 0.002 | 0.003 |
| | | | | | (0.055) | (0.009) | (0.009) |
| Year Indicators | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| State Indicators | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Other Controls | | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 4,092 | 4,092 | 29,275 | 33,367 | 1,180 | 7,897 | 9,077 |
| Adjusted $R^2$ (%) | 1.5 | 18.3 | 33.1 | 28.6 | 22.4 | 31.5 | 27.7 |

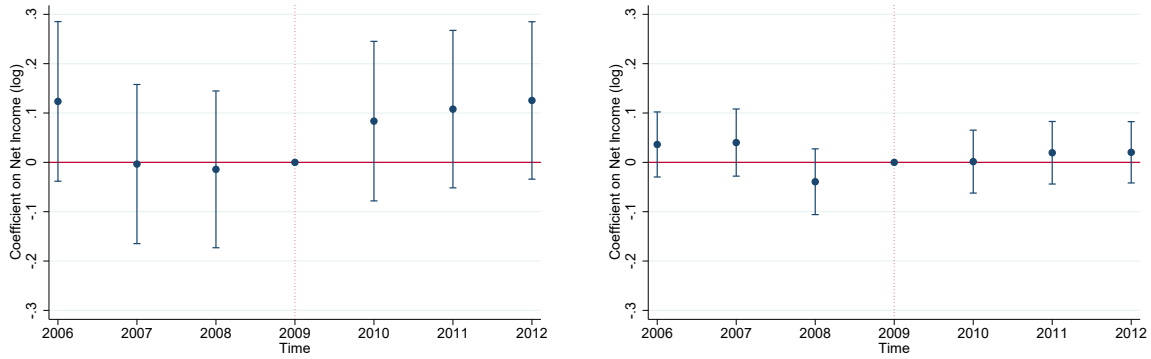
*Estimation Equation:* DD estimated using OLS.
*Control variables:* Indicators of year, federal state, nationality, children, gender, marital status, educational and vocational qualification, tenure and its square.
*Inference:* Robust standard errors are in parentheses, significance levels are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
*Source:* Own calculations based on the scientific use file of the German microcensus (2006-2012).

In column II, when we include our set of control variables, the estimate for the ATT is more precise (since the standard error decreased somewhat), but smaller. The point estimate indicates that of the 10.0% potential extra profits only 8.0% arrived in the business owners pockets. This means that 2.0% of the extra profits could have ended up somewhere else, though an effect of 10.0% is within the 95%-confidence bands. Therefore, we reestimate the specification from column II for employees only. The results are presented in column III. The quite precise estimate of the ATT is insignificantly different from zero, which indicates that, on average, employees did not receive any share of the extra profits. Please note that since we have to use net income as dependent variable, it is likely that the 2% reflect tax liabilities or contributions. Column IV shows that the overall effect is dominated by employees. This is no surprise since their share in the population is seven times higher than the share of self-employed.

In column V, we conduct a placebo experiment where we pretend that the reform occurred in 2008 using only data from 2007 and 2008 to make sure that the pre and post policy periods have similar numbers of observations. Since the reform was to some extent designed to catch-up with the general price development,

FIGURE 3

Event Studies for Net Income




*Notes:* The two figures present event studies for net income. The left graph plots the coefficient and the 96%-confidence band of the interaction terms of the treated dummy and the respective year dummy. The left graph shows the results for self-employed individuals and the right graph plots the effects for employees only. In both graphs, the coefficient on the year 2009 serves as a baseline effect.

*Source:* Own calculations based on the scientific use file of the German microcensus (2006-2012).

persons working under regulated prices could have responded to this pressure before 2009. These kind of anticipation effects could lead to violation of the assumption that the 2009 fee revision resembles an exogenous experiment. The estimate on the interaction term shows that this is not the case. In line with our assumption it is small and insignificant.

The results from the event studies are shown in Figure 3. The left graph plots the coefficients and the 96% confidence bands for the self-employed while the right graphs does the same for employees only. Given that the sample of self-employed within our time frame is only about 5,000 individuals, the confidence bands are quite large when compared to the analogous regression for the employed individuals. This also renders the single coefficients insignificant, but nevertheless, it is nice to see that all coefficients after 2009 are positive suggesting that the reform had the positive effect on the business' owners incomes. Although the coefficient in 2006 is also positive, we think that it is important to note that for 2007 and 2008, the point estimates are almost exactly at zero which speaks in favour of the common-trends assumption.

## 5.2 Effects of Price Regulation on Income Inequality

For the investigation of the effect of price regulation on income inequality, we follow novel strategies by applying the DD model in a multinomial framework: First, we divide the income distribution of self-employed with positive personal net income into quintiles $J$ and estimate multinomial logit models with maximum likelihood, where

$$T(z_{ijt}) = P_{ijt}(z_{ijt}) = \exp(z_{ijt}) / \left(1 + \sum_{j=1}^{J} z_{ijt}\right).$$

We normalize the middle income class to serve as base category. The log-likelihood function is

$$\log \mathscr{L} = \sum_{i=1}^{N} \sum_{j=1}^{J} \log P_{ijt}(z_{ijt}).$$

From this we calculate the treatment effect and obtain standard errors using the delta method.

The ATT at the time of treatment conditional on $X_{i,1,1}$ is

$$\begin{aligned} & E_j[y_{i11}^{\text{factual}} | 1,1,X_{i11}] - E_j[y_{i,1,1}^{\text{counterfactual}} | 1,1,X_{i11}] \\ & = P_{ijt}(\gamma_j + \vartheta_j + \omega_j + X_{i11}\xi_j) - P_{ijt}(\gamma_j + \vartheta_j + X_{i11}\xi_j). \end{aligned} \tag{2}$$

The five estimates for the ATTs are reported in Table 4 for three standard DD models. The first model is based on the sample of self-employed architects and engineers but includes no control variables, while the second model does. The third model uses the sample of all architects and engineers.

The findings are consistent with the results of the previous subsection: In all three models the reform effect for the second and third quintile is negative and economically important. For instance, model I indicates that the probability of belonging to the second quintile of the income distribution is about 4 percentage points smaller. For the other models, the reform effect is of similar size but often imprecisely measured.

Where do these self-employed go? The first two models suggest that the probability of belonging to the upper 20% and upper 40% of the income distribution increased due to the reform. Although statistical significance cannot be achieved at the 10% level, the changes in probabilities are sizable: the probability to have moved to the highest quintile increased by again about 4 percentage points. The results from model III, where all architects and engineers are considered, suggests that the reform was effective mainly for a shift from the second lowest or middle to the second highest quintile. A reason for this could be that there may be high income architects or engineers working as employees who earn more than the average self-employed.

In a next step, we estimate the model coefficients using only pre-treatment information on the treatment group but the full time span for the control group. Then, we predict outcomes out of sample, i.e. the post-treatment period, for the treated group. We contrast the actual income class probabilities with the predicted to obtain the ATT using t-tests. This method is equivalent to the standard DD approach with a large number of cross-sectional observations but much easier to calculate in nonlinear models. To improve prediction, we include a quadratic time trend instead of year indicators.

The results are similar to the previous ones. Self-employed architects and engineers move from the second lowest quintile straight up to the highest quintile. These effects are economically and statistically significant (see last two rows in Table 4). This shows that the reform had a substantial effect on the income distribution. In fact, since architects and engineers earn on average a higher income than the rest of the working population, and self-employed move up the income distribution due to the reform, we conclude that price regulation is a determinant of income inequality in general.

TABLE 4
Effects of Regulation on Income Inequality for Architects and Engineers

| Quintiles | Lowest 20% | Second Lowest 20% | Mid 20% | Second Highest 20% | Highest 20% |
|---|---|---|---|---|---|
| | Model I: No Controls, Sample: Self-Employed | | | | |
| Average Effect of Reform (%) | 0.8 | -4.1* | -1.9 | 1.1 | 4.1 |
| | (1.4) | (2.3) | (1.8) | (2.1) | (2.8) |
| | Model II: Full Set of Controls, Sample: Self-Employed | | | | |
| Average Effect of Reform (%) | -0.1 | -5.1 | -0.7 | 1.5 | 4.4 |
| | (1.2) | (3.0) | (1.7) | (2.1) | (3.2) |
| | Model III: Full Set of Controls, Sample: All | | | | |
| Average Effect of Reform (%) | 0.7 | -1.1 | -0.8 | 3.1*** | -2.0 |
| | (0.6) | (1.1) | (1.0) | (1.4) | (1.6) |
| | Model IV: Full Set of Controls, Sample: Self-Employed, Fitted with Treated Before Only | | | | |
| Observed Probabilities (%) | 8.5 | 13.3 | 8.7 | 18.1 | 51.4 |
| After Reform | (27.9) | (34.0) | (28.2) | (38.5) | (50.0) |
| Predicted Probabilities (%) | 6.7 | 17.4 | 9.4 | 16.6 | 50.0 |
| Without Reform | (5.9) | (12.7) | (6.1) | (6.9) | (17.9) |
| Difference (%) | -0.1 | -4.5*** | -0.8 | 1.6 | 3.8*** |

*Estimation:* Results from multinomial logit models estimated with maximum likelihood.
*Sample:* (Self-employed) architects, construction engineers and unregulated engineers.
*Control variables:* Indicators of year or linear time trend, federal state, nationality, children, gender, marital status, educational and vocational qualification, tenure and its square.
*Inference:* (Delta-method) standard errors or standard deviations are in parentheses, significance levels are * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
*Source:* Own calculations based on the scientific use file of the German microcensus (2006-2012).

## 5.3 Effects of Price Regulation on Further Labour Market Outcomes

Table 5 reports the results for our linear DD models for further labour market outcomes including the number of hours worked (columns I and II), the propensity to be self-employed (III), firm size measured by the number of employees (IV), the propensity to engage in continued education programs (V), and the number of training hours (VI). Since the number of employees is censored at 10, we report results from a tobit regression in column IV with self-employment as additional control variable.

The results confirm the descriptive evidence that hours worked did not change in response to the reform. In addition, we find a negative and slightly statistically significant effect on the propensity to be self-employed. However, the reform led to a significant increase in the number of employees per firm. The point estimate of 1.1 (using a Tobit model for this regression due to censoring) suggests that average firm size has increased by 1.1 employees, which is an increase by around 8%. Thus, the price increase induced by the HOAI had a clear market structure effect resulting in fewer, but larger firms.

In columns V and VI, we investigate whether the reform had a causal impact on on-the-job training, which might serve as a first indication for service quality. While there is no effect on the propensity to enrol

in continued education, we find a negative effect at the intensive margin. Those engaging in these programs reduce their training by around five hours per year. However, it would be misleading to attribute this effect entirely to the 2009 amendment to the HOAI. The reason is that the so-called "Engineering Card", which increased the value of education for our control group, was introduced in 2010 in Germany. This makes it very difficult to distinguish the reform effects. Thus, the large decrease in hours of continued training in Table 5 cannot be interpreted as evidence for a decrease in service quality.

TABLE 5
Effects of Regulation on Labour Market and Quality Outcomes

| | I | II | III | IV | V | VI |
|---|---|---|---|---|---|---|
| Dep. Variable | Hours Worked | Hours Worked | Self-Employed | Firm Size | Cont. Educ (y/n) | Cont. Educ (h) |
| Sample | Self-employed | Employees | All | All | All | All |
| Treated × Post | 0.018 | -0.510 | -0.028* | 1.095** | -0.003 | -5.005** |
| | (0.774) | (0.341) | (0.015) | (0.544) | (0.016) | (2.279) |
| Treated | 0.667 | 1.075*** | 0.457*** | -28.738*** | 0.007 | -0.437 |
| | (0.515) | (0.243) | (0.011) | (0.371) | (0.011) | (1.786) |
| Post | -1.166 | 0.036 | -0.003 | -0.878** | 0.009 | 1.706 |
| | (0.840) | (0.154) | (0.007) | (0.357) | (0.011) | (1.861) |
| Year Indicators | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| State Indicators | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Other Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Observations | 4,521 | 30,615 | 35,157 | 34,933 | 35,154 | 35,006 |

*Estimation:* Results from DD models estimated with OLS.
*Sample:* Architects, construction engineers and unregulated engineers.
*Control variables:* Indicators of year, federal state, nationality, children, gender, marital status, hours worked, educational and vocational qualification, tenure and its square. Column IV: indicator for self-employment.
*Inference:* Robust standard errors are in parentheses, significance levels are $^{*}$ $p < 0.10$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.
*Source:* Own calculations based on the scientific use file of the German microcensus (2006-2012).

Finally, we conducted an extensive set of robustness tests for all of our regressions. First, to tackle the concern that 2009 might be confounded as this year is half treated and half untreated, we dropped 2009 as a pre-reform year. As can be also seen in the Online Appendix (see Tables B1 and B2), our results remain virtually the same both qualitatively and quantitatively after the exclusion of 2009 though the fewer observations leave us with less precise estimates. Second, in Tables B3 and B4, we dropped all individuals younger than 30 to exclude the possibility that our results are confounded by the entry of new and young professionals due to the higher income (given the full pass-through that we observe). Again, this does not change our results at all. Third, we tested whether the results are driven by one of our two treatment occupations. For most of our outcomes, we do not find any evidence that one occupation is driving the results. The only exceptions are self-employment status (significant at the 10% level for architects) and firmsize (significant at the 5% level for construction engineers), see Tables B5 and B6. Finally, we used two other occupations, namely i) tax consultants and ii) lawyers and notaries, as control groups which have a similar high prevalence of self-employment. While for most outcomes the point coefficients go into the same direction, there seems to be a difference, e.g., in hours worked and firm size. The exact results can be found in Tables B7 and B8.

### 5.4 Effects of Price Regulation on Service Quality

To analyse the effect of price regulation on service quality, we employ a novel, country-level data set, which is able to measure architectural quality from a comprehensive perspective, including aesthetic quality. More precisely, we use the score of an office ranking from 2006 to 2012 published every second month by BauNetz Media GmbH on its website. The ranking is based on the number, length and level of detail of publications made within the previous 24 months in professional journals. The number of pages of a report is converted into a score, where a one page illustrated note equals one point, a two page short presentation two points, a three to four pages small report three points, a regular report of five to seven pages four points, and a large report of eight or more pages gives five points. The final score per office is the weighted sum of points, where the weighting factor depends on the information density of the respective journals. An even more detailed description of the calculation method (which is conducted by the BauNetz Media GmbH itself) is given in the Online Appendix. Germany has an average peer ranking score of 8 and scores much lower than, for example, the EU average of about 14. However, if we account for outlier offices by dropping the lowest and highest 1% of the score distribution for each country, this differences shrinks to around 3 score points. For our analysis, we aggregate the office data to the national level, which gives us a country-level balanced panel.

To estimate the causal reform effect, we employ the SCM, first proposed in Abadie and Gardeazabal (2003). The SCM is an extension to the standard DD estimator. Instead of relying on one distinct control group, this method constructs a synthetic counterfactual group, which is computed as a weighted average of all possible control units, denoted by $C$. Assuming a linear factor model, the estimator for the ATT for each post-treatment year $t$ is given by

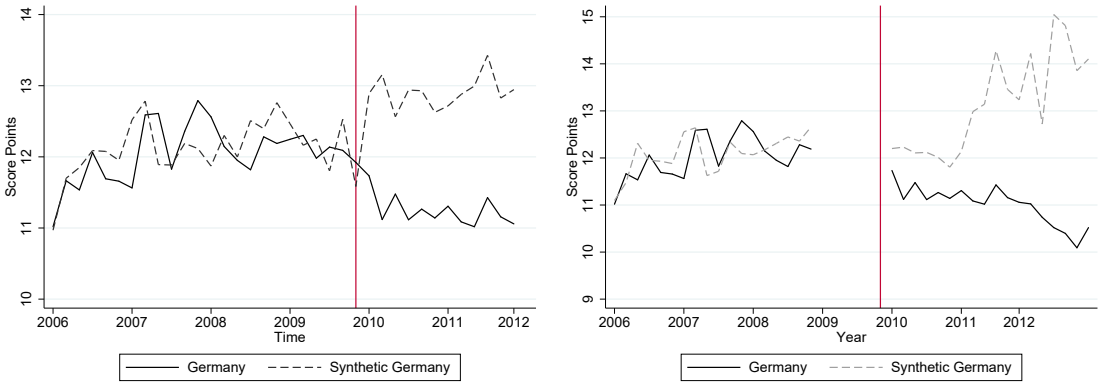
$$\hat{\omega}_t^* = y_{Germany,t} - \sum_{c=1}^{C} w_c^* y_{ct} \quad \text{for} \quad t > 2009, \tag{3}$$

which is, under certain conditions, an unbiased estimator of the treatment effect of the reform (see Abadie et al., 2010). $y_{Germany,t}$ denotes the observed outcome for Germany. The counterfactual outcome is given by a weighted average of the control units $C$, serving as the counterfactual outcome in absence of treatment. The vector $W^* \equiv (w_1, \ldots, w_C)'$ is chosen to minimize the distance $\|X_{Germany} - X_0 W\|$, where $X$ is a matrix containing the covariates and (linear combinations) of pre-intervention outcomes. Intuitively, by matching on observed covariates as well as pre-intervention outcomes, this method aims at controlling for time-varying unobservables. For the analysis below, $X$ includes the following covariates: architects to population ratio, population, GDP per capita, market size and government expenditure. In addition, yearly averages for the years 2006-2009 enter the estimation as 4 distinct linear combinations of our pre-intervention outcomes.

Figure 4a presents the results of the SCM. It depicts the time series of the score points for Germany (solid line) and its synthetic twin (dashed line). The latter results from a double maximisation procedure

FIGURE 4

Effects of Regulation on Quality




(a) 2006-2012 (b) 2009 excluded

*Notes:* The left graph plots the time series from January 2006 (time 1) to January 2012 of the quality score for Germany and the synthetic control group ("Synthetic Germany"). It consists of 6.9% of Croatia, 34.4% of France, 3.1% of Italy, 23.7% of Spain, 4.2% of Switzerland and 27.7% of Turkey. In the right graph, the SCM analysis is conducted without the "potentially polluted" year 2009. Here, the synthetic unit is made up of 51% France, 27.6% Turkey and 21.4% UK.

*Source:* BauNetz Media GmbH: Office-Ranking (2006-2012). Own Calculations.

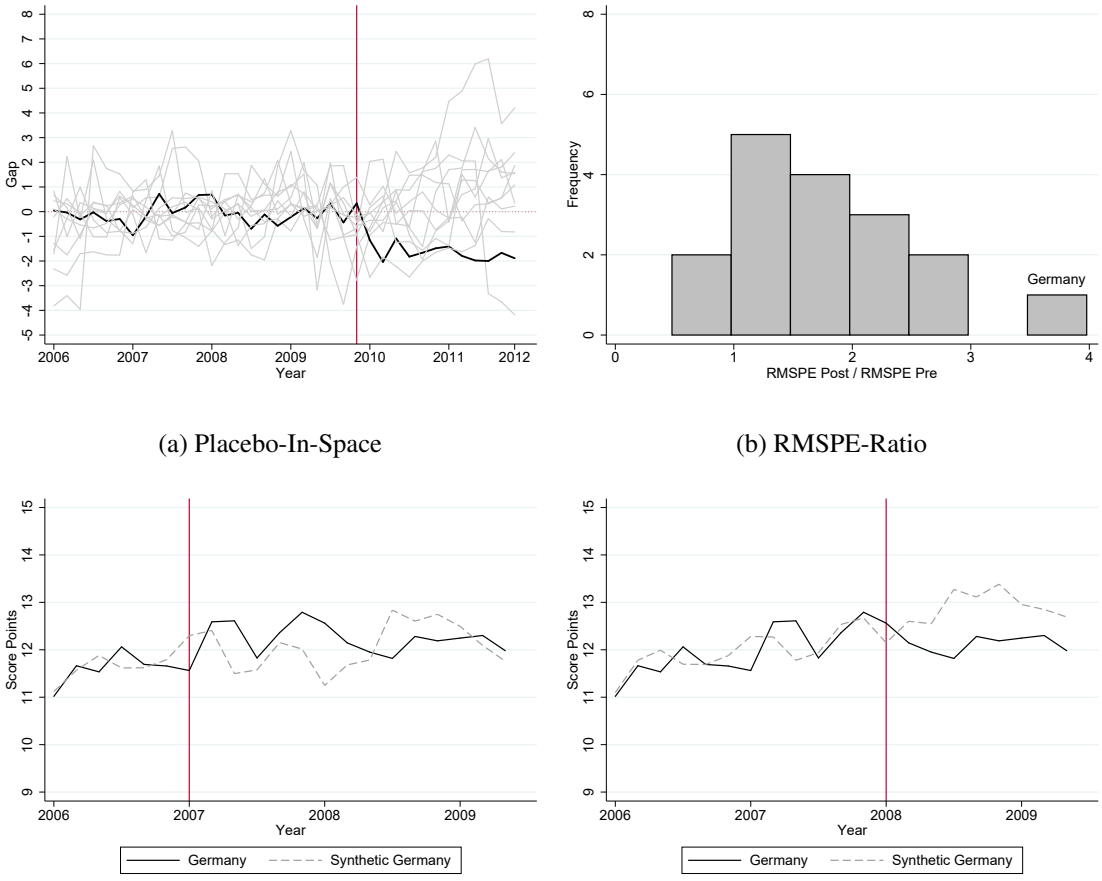
and consists of 6.9% of Croatia, 34.4% of France, 3.1% of Italy, 23.7% of Spain, 4.2% of Switzerland and 27.7% of Turkey. Thus, France, Spain and Turkey are the main ingredients of the synthetic control group.

Figure 4a clearly shows two things. First, the SCM has produced a control group which is able to reproduce the trajectory of Germany quite well, as can be seen from the almost perfect fit in the pre-intervention period indicated by the vertical red line. Second, after the exogenous price increase, the two time series diverge. At least after around one year after the reform, there is a clear negative treatment effect: while the score for the synthetic control group is increasing over time indicating a positive time trend in scores, Germany experiences a decrease. Pinned down in quantitative terms, the average point estimate amounts to -2 score points, which would translate into a percentage effect of -18%. Figure 4b shows the results when we leave out 2009 in the analysis as this year might be polluted due to the specific lagged calculation of the score. Although we thereby loose one year of pre-treatment fit, there is a clear widening of the gap visible after the introduction of the HOAI. Given the 24 months lag of the score, the results seem to be even more plausible as the reform fully kicks in after 24 months.

To assess the statistical significance of the effect, we rely on three standard techniques in the SCM literature, whose results are illustrated in Figure 5. The left upper graph shows the result of placebo-in-space regressions where the treatment status is reassigned to each country $J$ in the pool of control units. Germany, indicated by the thick black line, lies at the outer region of the placebo funnel, which hints at

FIGURE 5

Effects of Regulation on Quality: Inference




(a) Placebo-In-Space

(b) RMSPE-Ratio

(c) Placebo-In-Time 2007

(d) Placebo-In-Time 2008

*Notes:* Figure 5a shows the results of the placebo-in-space studies, where treatment is reassigned to all units in the donor pool. The thick black line denotes Germany. Figure 5b plots the frequency distribution of the RMSPE-ratio. The two bottom graphs show the results of the placebos-in-time where we pretend the treatment to have taken place in 2007 and 2008.

*Source:* BauNetz Media GmbH: Office-Ranking (2006-2012). Own Calculations.

a statistically significant reform effect. To account for the fact that the pre-intervention fit of the placebo effects highly differs, Figure 5b plots the distribution of the root mean squared prediction error (RMSPE) ratio which relates the post-intervention RMSPE to the pre-intervention RMSPE. Compared to the placebos-in-space, this ratio accounts for bad pre-intervention fits. Germany has the largest ratio, which would again suggest that the estimated treatment effect is statistically significant. Finally, we conduct two placebo-in-time tests (shown in Figures 5c and 5d) where we pretend the treatment to have taken place in 2007 and 2008 respectively. Reassuringly, there is no divergence of the lines up to the year 2009 in both cases, suggesting that there is no effect in the pre-intervention period.

Concluding this part, our analysis delivers some first evidence that the HOAI with its sharp price increase did not lead to an increase of quality of architectural services—rather the opposite. Of course, one has to account for the fact, that this ranking, while it serves as a flagship signalling device for architectural quality, is selective, since only slightly more than 1% of all offices in Germany are included. In addition, due to the inherent nature of the ranking, there might be a two-year transition phase after the reform, as publications count in the score for exactly two years. Moreover, the quality response could have evolved non-linearly over time with an immediate response to restore profit margins and later output adjustments. Even with these caveats, our evidence from the office ranking is an important step for the analysis of regulation, since it shows how to make use of a measure that goes beyond "objective" quality measures (like, for example, the number and sum of building damages) and is actually used by the professionals themselves in practice to asses service quality.

## 6. Conclusion

In addition to occupational licensing, prices in professions that provide experience or credence goods are often regulated by law to guarantee a minimum standard of quality. At the same time, such product market regulation may have unintended effects on labour market outcomes with the degree of income inequality being of special interest to the general public. We provide first evidence on the association between income inequality and service quality from a natural experiment to shed light on the question whether price regulation introduces an additional trade-off between quality and inequality.

We study the case of exogenous increases of fixed prices for architects and construction engineers in Germany. We find that incomes of self-employed architects and construction engineers increased significantly by 8% and for employees by 0%. This implies that the full share of the price hike ended up in the business owners' pockets and that there is no evidence that any part of the extra profits affected employees' incomes. In addition, our results indicate that the probability to belong to the second lowest 20% and the mid 20% of the personal net income distribution decreased due to the reform by about 5 percentage points, while the probability to stay in the second highest and highest 20% increased correspondingly. Finally, we

show how service quality changed due to the reform. Using a well-known office ranking, we find that the exogenous price increase did not have the expected positive quality effects. In contrast, the reform seems to have a slightly negative effect on service quality with a significant decrease of two score points within that ranking.

This suggests that there is no additional trade-off between quality and inequality. There results rather show that price regulation may lead to a deterioration of service quality and at the same time be beneficial for those at the higher end of the income distribution. Although we use indicators for quality from the best two data sources available, these measures are not perfect. Therefore, we encourage researchers and practitioners to make better measures of quality available for future research.

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