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Entry Regulation and Entrepreneurship Empirical Evidence from a German Natural Experiment

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Entry Regulation and Entrepreneurship

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Empirical Evidence from a German Natural Experiment

Davud Rostam-Afschar[a]

September 26, 2010

Abstract

The amendment to the German Trade and Crafts Code in 2004 offers a natural experiment to asses the causal effects of this reform on the probabilities of being self-employed and transition into and out of self-employment, using cross-sections (2002-2006) of German microcensus data. This study applies the difference-in-differences technique in logit models for four occupational groups. Easing the educational entry requirement has fostered self-employment significantly for less qualified craftsmen, almost doubling the entry probability, even as exit rates remained unaffected. Weaker effects occur for other occupational groups. These findings have implications for the design of regulations with educational requirements.

Keywords: Regulation · Entrepreneurship · Educational entry requirement
Natural experiment · Craftsmanship
JEL: L51 · J24 · I28 · M13

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I INTRODUCTION

Invoking an entrepreneurial spirit is widely regarded as a remedy to the disease of high unemployment and a source of economic dynamism. However, entrepreneurship remains one of the scarcest resources in many European countries, leaving unanswered the question of what restrains people from starting a business. In Germany, entrepreneurship measured as either the rate of self-employment or the rate of transition into self-employment is lower than in the United States (cf. Holtz-Eakin and Rosen, 2005). In addition to credit constraints (e.g., Evans and Jovanovic, 1989; Blanchflower and Oswald, 1998; Hurst and Lusardi, 2004; Fossen, forthcoming; Holtz-Eakin and Rosen, 2005), entry regulation appears to hamper entrepreneurship in Europe, particularly in Germany where craftsmanship accounts for approximately one-sixth of all self-employed people and represents a vibrant, well-organized, important economic sector that comprises not only artisans, but also building and construction trades. This group of entrepreneurs is highly regulated by the German Trade and Crafts Code (HwO), which imposes an educational requirement on entry into self-employment.

The mandatory qualification as a master craftsman required to start a business in Germany as such dates back to the late nineteenth century. Then, as today, proponents of the entry requirement (e.g., German Confederation of Skilled Crafts, 2003) cited market failures due to information asymmetries and external effects, while opponents (e.g., German Deregulation Commission, 1991; German Monopolies Commission, 1998, 2002) have objected that these regulations lead to inefficiencies. The government justifies the regulation mainly as a means to prevent danger to health. However, the master craftsman qualification underwent a dramatic change after amendment to the HwO in January 2004, which decreased the number of occupations subject to these regulations from 94 to 41. Moreover, the entry requirements for the remaining 41 occupations were relaxed.

Empirical studies related to this topic use aggregate data from many countries to investigate the effects of regulation on entrepreneurship, as in the influential work by Djankov et al. (2002) and subsequently in research by Klapper et al. (2006), Ciccone and Papaioannou (2007), and Stel et al. (2007). Kaplan et al. (2007) and Bruhn (2008) also present evidence that lower entry costs increase entry into entrepreneurship. Bruhn (2008), Ardagna and Lusardi (2008, 2009), and Prantl and Spitz-Oener (2009) add to the almost unanimous evidence against entry regulation using microdata. The latter discuss regulatory effects in light of the German unification in 1990, explicitly considering the educational entry requirement in craftsmanship. Finally, Müller (2006) provides the

first descriptive insights about outcomes after the reform but no evidence about the reform's causal effects and does not use individual data.

This study, in contrast, contributes to literature on entry regulation and entrepreneurship by providing the initial causal evidence of the effects of this reform, exploiting the policy change in 2004 as a natural experiment. For this analysis, I use repeated cross-sections (2002-2006) of German microcensus data and apply the difference-in-differences approach to estimate the effects of the policy change on the probabilities of being self-employed, as well as of transitioning into and out of self-employment for four distinct occupational groups. This contribution adds to a particular strand in literature on entry regulation and entrepreneurship, in that it explores a special kind of entry regulation, namely, an educational requirement. Furthermore, this work connects to the few studies that use microdata to investigate entry regulation and entrepreneurship, and it is one of the rare studies that focuses explicitly on craftsmanship.

In the next section, I employ a simple theoretical model to demonstrate that two distinct kinds of entry regulation exist and may have different effects on the probability of starting a new business. On the one hand, regulations in the form of fees, bribes, and so on, which solely decrease expected profits (i.e., rip-off regulations), influence this probability negatively. On the other hand, pay-off regulations – such as the educational requirement for German craftsmen – have an ambiguous effect on the probability of starting a business, because their influence moves through two channels: raising human capital and raising the costs of entrepreneurship. Although studies like Praag and Cramer (2001) and Parker (1996) emphasize the strong positive effect of human capital accumulation on entrepreneurial success, it remains unclear which effect dominates a priori. Hence the question arises, Is the entry requirement imposed by the HwO a rip-off regulation or a pay-off regulation? To shed some light on this question, I estimate the direction and magnitude of the causal effects of the policy change in 2004 in an econometric analysis.

The empirical results provide evidence that the probability of being self-employed increased with the amendment to the HwO. The strongest relative increase significantly raised the probability to a level more than 20% higher than a hypothetical situation without the reform for an occupational group with relatively low qualified workers and a relatively low propensity to engage in entrepreneurship. This group has been completely exempted from an entry requirement. The reform also seems to have increased the probability of being self-employed for professions that experienced only a reduction or a partial exemption from the entry requirement, though these effects are weaker. The

analysis shows further that these increases result from increasing the probabilities of entry, while the probabilities of exit from self-employment have remained virtually unaffected by the policy change.

In the remainder of this paper, I illustrate the institutional framework of the natural experiment and outline the empirical approach used to obtain the results with described data.

II THEORY

A simple static model related to the work of Lucas (1978), Evans and Jovanovic (1989), and Ardagna and Lusardi (2009) illustrates the effects of regulation on the individual decision to start a new business. The model incorporates the special nature of an educational entry regulation by introducing regulation-induced effects on human capital. This extension distinguishes two kinds of regulation, which affect the probability of engaging in entrepreneurial activity differently.

Equation (1) describes the decision of a risk-neutral individual i at the beginning of each period t, to remain in his current employment status ($entry = 0$) or to start a new business activity ($entry = 1$). Currently, the person receives an exogenous income $\overline{y}$ that is independent of individual characteristics.

$$entry = \begin{cases} 1 & \text{if } \pi = R(f,X) - (c + f + \nu) + \varepsilon \geq \overline{y} \\ 0 & \text{otherwise.} \end{cases} \tag{1}$$

As an entrepreneur, the individual's gross income, which is subject to an exogenous shock ε with distribution function $\Gamma(\varepsilon)$ and probability density function $\gamma(\varepsilon)$, amounts to π. The entrepreneurial profit is equal to the revenue function $R(f,X)$, less the costs of operating a business $C = (c + f + \nu)$. Here, c denotes the production costs, and two kinds of regulatory costs exist from the individual's perspective:

to any kind of expense or other monetary obstacles, such as fees, bribes, forgone profits due to bureaucratic delay, and so on, that have solely a negative impact on entrepreneurial income π. In contrast, pay-off regulation costs can have a positive effect on entrepreneurial income. Assuming $\frac{\partial R}{\partial f} > 0$ and $\frac{\partial^2 R}{\partial f^2} < 0$, entrepreneurial revenue is increasing in f with diminishing marginal returns. On the one hand, pay-off regulation costs decrease π by f, but on the other hand, f reflects an investment in human capital. Finally, X represents individual characteristics such as age, gender, education, and ability.

Therefore, the probability to enter entrepreneurship is given by

$$P(entry) = P[R(f,X) - (c + f + \nu) + \varepsilon \geq \overline{y}] =$$
$$1 - P[\varepsilon < \overline{y} - R(f,X) + (c + f + \nu)] = 1 - \Gamma[\overline{y} - R(f,X) + (c + f + \nu)] = 1 - \Gamma(G). \quad (2)$$

Using these results, the effects of a change in regulatory costs on the individual probability of starting a new business are as follows: A marginal increase in the rip-off regulation costs hampers entry, because the effect on the probability of starting a new business is

$$\frac{\partial P(entry)}{\partial \nu} = -\frac{\partial \Gamma(G)}{\partial G}\frac{\partial G}{\partial \nu} = -\gamma(G) \leq 0. \quad (3)$$

An increase in the pay-off regulation costs though creates an unknown outcome. We do not know whether it raises or diminishes the probability to enter, because

$$\frac{\partial P(entry)}{\partial f} = -\frac{\partial \Gamma(G)}{\partial G}\frac{\partial G}{\partial f} = -\gamma(G)\left(-\frac{\partial R(f,X)}{\partial f} + 1\right) = \gamma(G)\left(\frac{\partial R(f,X)}{\partial f} - 1\right) \overset{?}{\gtrless} 0. \quad (4)$$

Equation (5) further shows that regulation costs pay off positively when entrepreneurial revenue is increasing over proportionally in f for $\gamma(G) > 0$:

$$\frac{\partial P(entry)}{\partial f} \begin{cases} > 0 & \text{if } \frac{\partial R(f,X)}{\partial f} > 1 \\ \leq 0 & \text{otherwise.} \end{cases} \quad (5)$$

By comparing rip-off and pay-off regulations, we discern three properties regarding regulation and entrepreneurship. At the starting point where $\nu = f = 0$, rip-off and pay-off regulations are indistinguishable. Now suppose a policy change increases the regulatory costs in two identical countries A and B. Country A increases ν from 0 to $\nu_1 > 0$, leaving everything else unchanged, whereas country B increases f from 0 to $f_1 > 0$, ceteris paribus. In country A, the probability of engaging in entrepreneurial activity decreases according to Equation (3), but the situation in country B is different: If the human capital effect outweighs the impact of the costs, regulation can raise entry probability. Thus, the second result is that the pay-off regulation can have a positive

effect on the likelihood to become an entrepreneur. Note further that the human capital effect is stronger when f is smaller in this model.

A third finding reveals that the probability of entry is always higher if a pay-off regulation is imposed instead of a rip-off regulation for all $\nu = f > 0$. Therefore, country B should have more entrepreneurs. Because it is unknown whether the pay-off or rip-off effects of an educational entry regulation prevail, the causal effect of a policy change must be quantified empirically to determine the kind of regulation.

III THE AMENDMENT TO THE GERMAN TRADE AND CRAFTS CODE IN 2004 AS A NATURAL EXPERIMENT

Institutional Background

Over the course of time, three key institutions for German craftsmanship have emerged: the *small proof of competence* (Kleiner Befähigungsnachweis), the *grand proof of competence* (Großer Befähigungsnachweis), and the *register of self-employed craftsmen* (Handwerksrolle). The small proof of competence restricted the training of apprentices to craftsmen who held a master certificate, though such a degree was not required to start a business. However, the grand proof of competence mandated that craftsmen obtain a master certificate for both activities, to train and to have a new business listed in the register.

Since 1965, legislation has distinguished between restricted regular craftsmanship (Vollhandwerke), which requires a grand proof of competence, and unrestricted trades similar to crafts (Handwerksähnliche Gewerbe), referred to in this text as A-occupations and B2-occupations, respectively. Craftsmen in A-occupations, in contrast to those in B2-occupations, must hold a master degree for admission as an entrepreneur.[1]

The qualification as a master craftsman is the highest professional qualification in craftsmanship.[2] To attain it, a person must complete several stages of training and examinations. Full-time courses to prepare for the master exam take one to three years, and the occupation-specific overall costs range, according to Chambers of Crafts and Trade, from 4,000 to 10,000 Euro. The master exam tests both occupation-specific skills and general education in business and commercial knowledge, as well as law. Moreover, the exam contains a pedagogical portion, because holding a master degree makes the craftsman eligible to train apprentices. Those who have passed the examination are

recorded in the *register*; in rarely granted exceptions, some people may be recorded in the register without a master degree.

This setting was the situation just prior to amendment to the HwO in 2004, which this analysis exploits to assess the causal effects of entry regulation on entrepreneurship. The principle element of this reform was a reduction in f, or the costs of entry into entrepreneurship, by exempting some occupations completely and others partially from the requirement of attaining a master degree.

Entrepreneurs and Craftsmen in Germany

The amendment came into effect on January 1, 2004, in the context of a series of reforms aimed at the German social system and labor market called *Agenda 2010*.[3] Figure 1 summarizes self-employment numbers in Germany before and in the aftermath of the reform.

INSERT FIGURE 1 ABOUT HERE

The graph clearly shows that the development of self-employment in Germany is strictly increasing. On close inspection, the graph exhibits a somewhat steeper slope after March 2004, and the share of self-employed among the working population increased at an almost constant rate until 2005.

The number of craftsmen also decreased rather powerfully prior to the amendment to the HwO in 2004, which was reversed to some extent afterward. The rate of self-employed among craftsmen is not decreasing simultaneously with the number of craftsmen though, which shows that the decline before the reform was mostly due to dependently employed craftsmen. In contrast, the period after the reform exhibits a rising number of craftsmen in combination with an increasing rate of self-employment among craftsmen. To what extent is this effect due to the amendment to the HwO?

IV EMPIRICAL SPECIFICATION

Definition of the Treatment and Control Groups

The amendment to the HwO defines certain occupational groups that are subject to different degrees of regulation. I match each reported occupation of an individual in the German microcensus with the respective occupation listed in the law. With this information, I can construct four occupational dummies that reflect the different intensities of the treatment.

The deregulation of the master degree requirement, which is the main element of the policy change, generated a group of 53 B1-occupations by splitting up the former 94 A-occupations.

Craftsmen belonging to the group of B1-occupations were allowed to start businesses without a master degree but still had to demonstrate their ability to train. These B1-occupations represent the main treatment group, referred to as non-mandatory-requirement craftsmen (*nmc*). Some examples are tile and mosaic layers, coppersmiths, turners, tailors, millers, and photographers.

The remaining 41 A-occupations comprise three more groups. First, a group of strictly regulated occupations remained subject to virtually the same requirements as before the policy change; they still needed a mandatory master certificate to enter entrepreneurship. These vocations, referred to as *strict* occupations, serve as the control group. They include chimney sweeps, opticians, hearing aid audiologists, orthopedic technicians, and dental technicians.

Second, according to the reform, no master certificate would be required for jobs that can be learned within three months, which defines the second treatment group that experienced only a small reduction of the entry costs. It resembles the control group, in that an entry requirement is still mandatory. However, these A-occupations that use the easyjobs-rule to be exempted from the master requirement can be grouped separately as *easyjobs* (cf. Müller, 2006): masons and concreters, painters and varnishers, metalworkers, motor vehicle body and vehicle construction mechanics, bike mechanics, information electronics technicians, vehicle technicians, and butchers.

Third, for the remainder of the A-occupations, entry restrictions were loosened by permitting senior journeymen to start a business without a master degree. This senior journeyman rule defines the third treatment group (*sjr* occupations). This group includes professions such as roofers, surgical instrument makers, gunsmiths, plumbers, gas and water fitters, joiners, and pastry cooks. However entry has not been deregulated completely, because this third element of the policy change obligates a senior journeyman to prove six years of work experience, four in a decision-making position, in his or her prospective occupation.

Methodology

The empirical strategy exploits the reform of the regulatory framework of entrepreneurial craftsmen in 2004 as a natural experiment. This approach reveals evidence of the causal effects of the regulatory change, because the policy event exogenously provides a control group that resembles a control group in a randomized laboratory experiment. With such a group, I can contrast the changes in the average outcomes of occupational choices before and after the reform in each of the groups affected by the policy with the changes in the average outcomes of occupational choices before and after the

reform in the control group. The difference in these changes, known as the difference-in-differences (DID) estimator, represents the average treatment effect on the treated (ATT), which equals the average causal effect of the reform (e.g., Blundell and Dias, 2009).

I use data from 2002 to 2006 for three occupational groups ($nmc, sjr, easyjobs$) that were subject to regulation changes of different intensities as the treatment groups (cf. Meyer, 1995) and the group of strictly regulated occupations as a comparison group. To find the ATT with the DID approach in this context means specifically comparing the difference in the average self-employment rates of each of the three treatment groups before and after the reform with the average self-employment rates of the *strict* occupation group before and after the reform.

The policy change could have influenced the self-employment rate positively, negatively, or not at all. However, the direction of the effect depends on how the new policy has caused the entry and exit rates to change. Generally, an increase in the self-employment rate could result from a higher entry rate or a lower exit rate. A decrease could result from a lower entry rate or a higher exit rate or from a higher entry rate, which in turn is exceeded by an even higher exit rate. Another possibility is that the self-employment rate overall remains unchanged if the policy shifts the entry rate as well as the exit rate equally in the same direction. Therefore, with this analysis, I investigate not only the probability of being self-employed but also the probability of entry into self-employment and exit from self-employment.

Furthermore, identifying the ATT using DID requires the assumption that the treatment groups and the control group experience common trends. This implies that macro shocks exert the same effects on both groups. For example, a sudden decrease of the interest rate should influence trades related to health and hygiene, which are common among the strictly regulated group, exactly as it does the building and construction trades, which are part of the *easyjobs* group. If so, a hypothetical trend without a reform in the treated group would parallel the trend of the control group in the post-policy period. Otherwise, it would be unclear whether differences between these groups are caused by the reform or other factors.

This setting is not subject to a frequent concern in natural experiments. That is, the problem of self-selection does not exist, because the different treatment groups are assigned by law, and the policy change was proclaimed for the first time in March 2003 (cf. Müller, 2006). Consequently there was only a short time for people to adjust and change occupations in expectation of the reform. Moreover, after the announcement of plans for the amendment to the HwO, a controversy arose

with an unpredictable outcome. It was therefore unknown which occupations would receive what intensity of treatment before the reform actually came into being. Considering this unpredictability, it seems unlikely that craftsmen would have changed jobs in anticipation of the effects caused by the complicated new rules. Thus, adjusting behavior cannot challenge the identification of the ATT parameter.

After the regulations were eased B2-occupations could be substitutes for similar B1 or A-occupations, which means that the compositions of the treatment and control groups might change systematically. This change would bias the estimated ATT parameter, because DID requires the treatment and control groups to remain unchanged over time to ensure before-and-after comparability. Although B2-occupations are excluded from the analysis when the data set can distinguish them, some of these professions remain in the sample. Because they remain in the same group (e.g., *nmc*) over the entire period, according to their time-invariant job definition, their presence in the sample does little harm. In addition, the analysis includes a set of observable, time-varying covariates and other characteristics to control for the potential for systematic differences in the populations over the two periods. Moreover, most professions require specific education and work experience, especially the *sjr* occupations, the *easyjobs*, and the control group. Consequently, it is reasonable to assume that changes in unobserved factors are the same between the treatment and control groups.

In estimating the effects of the reform for all treatments with repeated cross-sections from 2002 to 2006 in logit models using the maximum likelihood estimator, all three treatment groups are included jointly in the regression models to yield more precise estimates.

In a linear probability model, the ATT equals the coefficient of the interaction term between the treatment and the post-policy dummy. This interaction effect reflects the comparison of the changes of predicted probabilities before and after the reform for the treatment and control groups. However, predicted probabilities are not bounded by 0 and 1 in the linear probability model, so the preferred specification is the logit model for observation i described by Equation (6):

$$
\begin{aligned}
Prob(Y_i = 1 | dpost_i, dO_i, X_i) &= \frac{1}{1 + \mathrm{e}^{-z_i}} \text{ with} \\
z_i &= \beta_0 + \delta_0 dpost_i + \beta_{nmc} dnmc_i + \beta_{sjr} dsjr_i + \beta_{easyjobs} deasyjobs_i \\
&+ \delta_{nmc} dpost_i \cdot dnmc_i + \delta_{sjr} dpost_i \cdot dsjr_i + \delta_{easyjobs} dpost_i \cdot deasyjobs_i \\
&+ X_i \beta_4. \qquad (6)
\end{aligned}
$$

The outcome variable is assumed to be determined by the logistic function, and thus the model is nonlinear. In turn, the coefficient of the DID interaction cannot be interpreted as the ATT, and the effects of the reform must be computed as differences of predicted probabilities. The corresponding standard errors for the predicted probabilities can be obtained by applying the delta method.[4]

The dependent variable Y_i is a binary variable that indicates self-employment in the stock models and transition into or out of self-employment in the flow models. The conditional expectation of the binary outcome equals the response probability, denoted by $Prob(\cdot)$. The regressors $dpost_i, dO_i$, and X_i are included in z_i, where $dpost_i$ is a dummy variable for individuals observed in the post-policy period;[5] $dO_i = dnmc_i, dsjr_i, deasyjobs_i$ indicates an individual's affiliation to one of the treatment groups; and X is the vector of control variables. The specification includes interaction terms between the respective treatment group indicators and the post-policy dummy. Moreover, δ_0, δ_ω, β_ω and β_4, with $\omega = nmc$,sjr,$easyjobs$, represent the respective coefficients or vector of coefficients, and β_0 is a constant.

In addition to dummies for the years 2003 and 2004, all models include variables for the following individual characteristics: age and its square, dummy variables indicating gender, type of secondary schooling and professional qualification, nationality, region of residence, the size of the respondent's residence city, marital status, the number of children, the branch of craftsmanship, and a constant. An indicator also reveals the citizenship of foreigners in a member state of the European Union (EU) and its interaction with the post-policy period. Controlling for these characteristics is important for two reasons. First, as indicated previously, the determinants of self-employment might have changed over the time. Second, including these control variables allows me to estimate $\hat{\delta}_\omega$ more efficiently.

The estimation sample consists of all craftsmen in a given year in the models for which the dependent variable is the self-employment rate. The same population is used in the entry models. Some unemployed or inactive persons do not report a profession, and thus it is unclear what portion of these groups participates as reserve in the labor market for craftsmen. Because the analysis excludes those who do not report an occupation, the results reflect an approximation of the probability of entering self-employment from dependent employment, unemployment, or inactivity, because not all potentially self-employed persons are included in the estimation sample.

The estimation sample of the exit models comprises self-employed craftsmen in the previous year. Therefore, it is the population that possibly could exit from self-employment within the year. With this sample, it is appropriate to estimate the probability of exit, because the dependent variable

clearly indicates whether a person is not self-employed after 12 months, but instead is dependently employed, unemployed, or inactive. Apart from these differences in the estimation population and the response variables, the econometric framework is identical in the stock models and the flow models.

V DATA AND DESCRIPTIVES

Sample Design

This analysis uses data from the German microcensus (Mikrozensus), which is provided by the Federal Statistical Office. This official representative yearly household survey is comparable to the Current Population Survey in the United States and the Labor Force Survey in the United Kingdom. The German microcensus is a 1% sample of all households in Germany. A subsample of 70% or approximately 500,000 observations per year, is selected at random and provided to researchers as a scientific use file by the Federal Statistical Office. The large sample size is important for this analysis, because less than 10% of the population are craftsmen. Most questions are subject to compulsory response; the German microcensus is a mandatory census, which guarantees a low rate of item non-response and ensures that entrepreneurs are adequately represented.

This analysis uses pooled cross-sections of the German microcensus from 2002 to 2006. Years before 2002 are not considered for several reasons. First, effects of the amendment to the HwO from 1998 were still significant at the beginning of 2001, in that the process of adjusting expectations and changing occupations in response to the reform took some time. Second, training in some traditional occupations, such as blacksmith and turner, ceased to be in force as of August 2002, superseded by more modern training structures with new fields of specialization. To avoid confusion due to these influences, I excluded the year 2001 from the analysis. The results when I include this wave are available on request.

The transition variables reflect questions from the supplementary program that ask for a person's employment status in the year before the interview, retrospectively. The supplementary questions were only posed to a 45% random subsample of the microcensus up until 2004. Section B of the Appendix provides a description of how the key variables are constructed.

Because the focus of this study is on entrepreneurship among German craftsmen, I restrict the sample as follows: I exclude all individuals younger than 18 years or older than 65 years. People whose occupational choice is conjecturally determined by different factors also are dropped from the

sample to avoid distortions. Thus, civil servants (who may report being craftsmen), apprentices, soldiers, conscripts, persons in education, or those drafted in the previous year, as well as all remaining non-craftsmen, are excluded. Moreover, family workers helping in a family business are not included in the sample, because they are not entrepreneurs in the sense that they run their own businesses. This process leaves us with a sample of about 26,000 observations per year, which represent nearly 4 million craftsmen in the German population.

Descriptives

In contrast with the development of the other occupational groups, as the upper left graph of Figure 2 shows, the number of *nmc* entries increased tremendously after 2004, returned to a somewhat lower level in 2006, but still remained higher than in the period before the reform. The exits remained constant for a time before declining in the aftermath of the policy change. Note that the balance (defined as $entry - exit$) exhibits sa similar path as the number of self-employed craftsmen in Figure 3, which implies that most of the variation stems from this particular group.

Although the figures roughly resemble the descriptions provided by Müller (2006), the German microcensus does not allow me to identify precisely the exact professions, because of data protections and the changes in the survey design after 2005.

Insert Figure 2 about here

The upper right graph in Figure 2 illustrates that neither the entries nor the exits of the *strict* occupations exhibit any singularity over time. The path of the growth rate and the balance correspond. Apparently, the numbers of entries and exits are both rather small. Sensitivity tests correcting for rare events are available on request.

In the lower left graph of Figure 2, the transition variables do not exhibit any major oscillation, other than a peak in 2003, in the difference of entries and exits for *easyjobs*. This peak is perceptible in the growth rate as well. In the post-policy period, the growth rate increases substantially, but the balance contrasts this development.

The series of transitional variables for *sjr* occupations depicted in the lower right part of Figure 2 show that the entries modestly increase, whereas the exits remain roughly constant. Here, the balance series and the growth rate similarly show an increase in 2005 and a subsequent decrease in 2006.

I report some characteristics of the four occupational groups in Table 1 as weighted averages from the pooled cross-sections from 2002 to 2006.

INSERT TABLE 1 ABOUT HERE

VI RESULTS

Treatment Effects on Self-Employment Probabilities

The results of the logit estimations in Table 2 refer to the probability of being self-employed and the probabilities of entry into and exit out of self-employment, using pooled cross-sections from 2002 to 2006.

The positive coefficient of the interaction between the dummy for *nmc* professions and the dummy indicating the post-policy period is highly significant in the model of the probability of being self-employed, as well as in the model of the entry probability. The term is negative but insignificant in the exit model. The coefficients for the interaction between the dummy for the *easyjobs* group and the dummy for the post-policy period in the entry probability model are significant and positive. In the self-employment probability model and the exit model, positive but insignificant coefficients appear for this group. In contrast, all three models return insignificant coefficients on the interaction term for the *sjr* professions.

As the first two columns reveal, the interaction effects between the indicator for citizenship in an EU member country and the post-policy period dummy are positive, large, and significant. However, the probability of being self-employed is higher for EU citizens before the policy change compared with German craftsmen in strictly regulated occupations, which suggests that the relative effect is not that large. Nevertheless, this result demonstrates the importance of controlling for this confounding factor.

INSERT TABLE 2 ABOUT HERE

To find the quantitative effect of the amendment on the probability of being self-employed, I first predict probabilities using the estimates reported in Table 2. However, the correct means to compute the treatment effect is subject to debate (e.g., Ai and Norton, 2003; Puhani, 2008). Therefore, I present two alternative interpretations of the treatment effect for each model. These alternatives rely on different counter-factual treatment effects, which can lead to dissimilar results.

Having obtained the predicted probabilities in a first step, I calculate their differences, as well as the difference of these differences, which represents the first interpretation of the treatment effect (see Ai and Norton, 2003). Next, I predict the counter-factual probabilities for the post-policy period and calculate the corresponding differences, as well as the counter-factual difference in these differences. The treatment effect compliant with the second interpretation is that obtained in the first step, subtracted from the counter-factual treatment effect (see Puhani, 2008). Table 3 summarizes the outcomes of this procedure and reports in bold the treatment effects, with those according to the first interpretation in the upper part and those according to the alternative at the bottom of the table.

Insert Table 3 about here

A person with average characteristics in a *nmc* occupation has the lowest probability of being an entrepreneur, both before and still after the policy change, in comparison with the other occupational groups, though the amendment to the HwO caused this probability to increase significantly by 1.27 percentage points with a standard error of 0.78 (p-value: 0.10), as reported in the upper part of Table 3. The probability of being self-employed would have been 6.69-1.27 = 5.42 in the hypothetical situation without a reform, and thus, the relative effect of the policy change on the *nmc* occupations equals 23.43%.

The absolute effect of the reform on this group in the bottom of this table amounts to 1.43 percentage points; with a standard error of 0.50 (p-value: 0.00), it is not much different from its counterpart in the upper part of the table. In this interpretation, a counter-factual probability of being self-employed is calculated for each treatment group in the post-policy period, which is reported in the second row of the lower part of the table. The relative effect in turn can be calculated with this value. In the case of the *nmc* professions, the counter-factual probability in the first column is 6.69-1.43 = 5.26%, such that the relative effect of the reform equals 27.19%.

Analogously, the effects of the reform on the *sjr* and *easyjobs* occupations increased the probability of being self-employed by 1.27 and 1.25 percentage points when I interpret each single cross-difference as the respective treatment effect. These effects just fail to achieve statistical significance at the 10% level with standard errors of 0.80 and 0.82 (p-values: 0.11 and 0.13), respectively. In relative terms, the effects amount to 12.03% for the *sjr* vocations and 14.50% for the *easyjobs* professions. Similar relative effects of 11.60% and 14.90%, respectively, result from the alternative definition of

the treatment effect, and the absolute effects of the reform in this interpretation of 1.23% and 1.28%, with standard errors of 0.89 and 0.78 (p-values: 0.17 and 0.10), resemble those reported previously.

Treatment Effects on Transition Probabilities

INSERT TABLE 4 ABOUT HERE

I also examine the quantitative effects of the reform on the probability of entry into self-employment, as reported in Table 4. Again it is obvious that the *nmc* group is special, in the sense that the probability of engaging in entrepreneurship for a person with average characteristics in this group is substantially lower than that of the other occupational groups, especially before the reform. Consistent with my prior findings, entry probability increased due to the reform by 0.75 (0.68)[6] percentage points. This economically relevant effect is also statistically significant with a standard error of 0.35 (0.24) (p-value: 0.03 (0.00)). Entry probability thus has been almost doubled with the reform, because its relative effect amounts to 96.15% (80.00%).

For the *sjr* and *easyjobs* professions, a smaller, marginally significant, positive effect emerges. The former group experienced an increase in the probability of entry of 0.58 (0.59) percentage points, with a standard error of 0.36 (0.35) (p-value: 0.11 (0.09)), whereas for the latter, I find an increase of 0.61 (0.56) percentage points with a standard error of 0.36 (0.28) (p-value: 0.09 (0.04)). In relative terms, the reform increased the entry probability of the *sjr* to a level 42.03% (43.07%) higher. For *easyjobs*, the entry probability is 62.89% (54.90%) higher than the hypothetical situation without the reform.

These results imply that the increase in the probabilities of being self-employed is largely a result of the positive effects of the reform on the entry probabilities. However, these increases could be accompanied by increased exit probabilities. As we found, both the probabilities of entering *nmc* occupations and being self-employed in these professions showed significant increases. Thus, the exit rates cannot be high enough to offset the effects of the entry probabilities completely, but they still could have increased. This finding would be consistent with the view that a fraction of the new entrepreneurs in the post-policy period use fly-by-night tactics. Moreover, constant or lower exit probabilities are also possible.

INSERT TABLE 5 ABOUT HERE

To clarify these possibilities, in Table 5 I provide the effects of the amendment of the HwO on the probability of exit from self-employment. The reform's effect for the *nmc* occupations is -1.27 (-0.19) percentage points with a standard error of 1.31 (1.52). This negative effect is insignificant (p-value: 0.33 (0.90)). Consequently, the hypothesis that the reform had no impact on the probability of exit cannot be rejected, although the negative point estimate gives the idea that the exit probability could have been decreased. A similar conclusion applies to the *sjr* and *easyjobs* vocations, because the point estimates of -0.63 (-0.45) and 0.24 (0.29), respectively, are highly insignificant with standard errors of 1.18 (0.93) and 1.24 (0.90) (p-values: 0.59 (0.63) and 0.85 (0.75)), respectively.

Specification and Sensitivity Tests

To assess the validity of the assumptions on which the DID approach is based, and to gauge the robustness of the findings in this analysis, the logit models of the probability of being self-employed and of the transition probabilities are reestimated, varying the specification, the definition of variables, and the estimation sample.

As a first test, I scrutinized whether other influences than the actual treatment on the treatment groups were present but did not influence the comparison group. Such influences would have confounded the analysis. In most settings, there is no way to test for these influences directly, so "placebo tests" based on the idea of reestimating the models while pretending that the policy event took place in a year prior to the actual policy change, are employed. First, the post-policy period indicator is redefined to represent the period from 2003 to 2004, as if the policy change had taken place in late 2002. Second, the logit model for the probability of being self-employed is reestimated without the actual post-policy period to avoid measuring the true effect of the reform. These steps are repeated for a placebo policy reform in late 2003.

As columns II and IV in Table A.1 in the Appendix show, the interaction coefficients in both placebo tests are insignificant, which would not be the case if confounding factors existed before the policy change. Therefore, assuming this result extends to the post-policy period, the validity of the identifying assumption of the DID analysis receives support. In contrast, columns I and III show significant coefficients for the interaction terms, in particular for the *sjr* occupations, when the estimation sample includes the year 2001 – which is why the main analysis was based on the sample from 2002 to 2006.

Furthermore, the assumption of common trends can be examined more explicitly, using the full

sample from 2001 to 2004 and the restricted sample from 2002 to 2004. After including interaction terms between occupational groups and a time trend, I estimate the logit models for the probability of being self-employed. Columns I and II in Table A.2 in the Appendix again show significant effects when 2001 is included; excluding this year demonstrates that no significant differences in the trends of the occupational groups exist in the period before the policy change. The assumption that this result is also true for the post-policy period is necessary for the identification of the ATT.

Although these two tests provide evidence of the validity of the identifying assumption, they rely on a key assumption for the post-policy period. However, the treatment groups and the control group comprise occupations of different branches, so it is possible to control for branch-specific trends. A time trend interacted with the dummy variables indicates branch affiliation in the model of the self-employment probability. This term can capture not just differential trends between the branches of craftsmanship but also between the treatment and control groups. To illustrate, suppose that a macro variable influences the building and construction sector proportionally more severely than it does the health and hygiene trades during the sample period. This differential trend would be captured by the time trend interactions with the respective branch dummies and thus guarantee that the branches in each treatment group and the control group have common trends.

Column III in Table A.2 contains the results of this test for the entire period from 2002 to 2006. The interaction term is only significant for the building and construction trades, and the foodstuffs trades (not reported). Evidently, the general results are robust to this variation of the specification. The comparison of the coefficients of the interaction of the treatment groups and the policy dummy with those obtained in the main analysis reveals quite small differences. Thus, the necessary condition of common trends is met.

Another robustness check investigates the potential influence of less than clear-cut definitions of the treatment and control groups. To determine if the estimates are influenced by the fuzziness of the job definitions, I use a narrower definition of craftsmanship to reestimate the models from the main analysis. This narrower definition excludes those occupations that cannot be unequivocally found in the data. The results in Table A.3 in the Appendix imply that the estimates in the main analysis are robust to this change of definitions, because the coefficients remain similar in size and significance.

Sensitivity tests correcting for rare events and varying the definition of the treatments and the control group are available on request.

Other Entrepreneurship Policies

Two major policies that could have interfered with the effects of the policy change include the enlargement of the European Union in 2004 and entrepreneurship subsidies. The enlargement of the EU based on the Treaty of Accession of 2003 took place in 2004, when ten countries became new member states. This event eased access for foreign entrepreneurs and thus was of relevance for craftsmanship in Germany (cf. Müller, 2008). Other important policy instruments are subsidies to entrepreneurs, such as *transitional allowance* (Überbrückungsgeld), *start-up subsidy* (Existenzgründungszuschuss, EXGZ), *entrance grant for entrepreneurs* (Einstiegsgeld für selbständige Tätigkeit), and another *start-up subsidy* (Gründungszuschuss) (cf. Caliendo and Steiner, 2005). According to Baumgartner et al. (2006), the EXGZ in particular has significant effects on entrepreneurship.

Insert Figure 3 about here

Figure 3 shows three graphs from 2002 to 2006: the development of total self-employed craftsmanship, the number of craftsmen who did not report receiving PPI payments (a dummy for public payments), and the number of German self-employed craftsmen. In Appendix B, I explain how this dummy for public payments is constructed. It captures only substantial subsidies and is designed according to the qualities of the EXGZ. The graphs evolve almost uniformly over time, which suggests that the enlargement of the EU and the subsidies did not affect the number of self-employed craftsmen systematically.

All three series experienced a substantial increase after the amendment to the HwO came into effect. The number of self-employed craftsmen jumped from 518,163, measured a year before the reform, to 579,036 in 2005 and then to 584,494 in 2006. This enormous change is also documented for the stock of businesses by Müller (2006), who uses data from the register of craftsmen. These numbers equal, for each year, approximately 90% of the stock of businesses reported in Müller (2006) which confirms how well these occupations are represented in the data. Note that this result holds after accounting for the actual stock of businesses, which is approximately 15% lower than the reported stock.

To check, whether other entrepreneurship policies could have biased the estimation results, I reestimated the models of the main analysis for different samples. Although the effects of the enlargement of the EU are controlled for by the dummy variable indicating citizenship of non-Germans in an EU member country and its interaction with the dummy for the post-policy period

in the main analysis, this test restricts the sample to German individuals only and excludes both EU and non-EU foreigners. According to Table A.4 in the Appendix, the estimated coefficients are somewhat smaller in the model of the self-employment probability compared with the main results. In the entry model, the coefficient of the *nmc* interaction term is slightly greater, whereas the other interaction terms have almost identical coefficients. In the exit model, all three coefficients again are insignificant. Therefore, the estimations are not distorted because of the enlargement of the EU.

To examine the influence of subsidies for entrepreneurs, I exclude all individuals from the estimation sample who indicated having received substantial public payments, that is, if the binary variable PPI defined in the Appendix B equals 1. This shift has little effect on the previous findings, though the coefficients in Table A.5 in the Appendix are slightly smaller. Excluding all individuals who reported having received any public payment (PPII) other than pensions, social benefits like housing or unemployment benefits, and scholarships, I again find coefficients similar in size and significance (results are available on request). Thus, the results are not distorted by public payments.

VII SUMMARY AND CONCLUSIONS

In the pursuit of an answer to the question of what keeps entrepreneurs from starting a business, this paper begins with a simple model of occupational choice to illustrate the qualities of two kinds of regulations. The paragon of entry regulation is the rip-off regulation that appears in most studies concerned with entry regulation and imposes costs without affecting the expected income of an entrepreneur positively. In contrast, pay-off regulations can have a positive effect on expected entrepreneurial income, in addition to that on the costs. From this point of view, the pay-off regulation represents an investment in human capital. However, the effects of such a regulation on the probability of starting a business depend on specific parameters and variable values.

Thus, to understand the nature of this kind of regulation, this study has examined the various features of the amendment to the HwO in 2004, which intended to foster self-employment in the highly regulated sector of craftsmanship. Among other modifications, these legislative changes exempted one group of craftsmen completely and another partially from the requirement of passing a master examination for admission as entrepreneur. Moreover, for a third occupational group, the entry requirement has been reduced. The reform also provides a natural comparison group, because for some professions, the entry requirement remained mandatory. Therefore, four distinct

occupational groups can be identified in data from the German microcensus from 2002 to 2006 and be exploited within this setting in a natural experiment.

The results from a DID analysis provide evidence that the probability of being self-employed increased with the amendment to the HwO among the *nmc* group. For both alternative definitions of the treatment effects, the strongest relative increase amounts to more than 20%, which occurred in the group of craftsmen completely exempted from the entry requirement. This finding is consistent with the notion that the level of entry regulation imposed by the HwO before the reform hampered entry into entrepreneurship instead of providing necessary human capital. Consequently, the regulation exhibited the properties of a rip-off regulation. In professions that experienced only a reduction or a partial exemption from the entry requirement, the results indicated only weak, positive effects. The analysis shows further that these increases are caused by increases to the probabilities of entry, whereas the probabilities of exit from self-employment remained virtually unaffected by the policy change. Various robustness checks support the findings of this study.

Two key results have important policy implications. First, the strongest relative effect applies to an occupational group with a relatively low probability of entrepreneurship and entry. The effects of a future reform to reduce the costs of entry regulation in the regulated professions with a comparatively high probability of being self-employed therefore might result in rather meager changes in the self-employment rate. Figure 1 shows that the self-employment rate is already higher among craftsmen than among the total working population. Although there might be some potential to increase entrepreneurship among craftsmen further, implementing an entrepreneurial culture in other sectors first could be more beneficial.

Second, persons who work in jobs that require only a relatively low level of qualification have a lower probability of starting a business. When an educational requirement is imposed, the probability falls even lower, as is true for workers in *nmc* professions who achieved a lower average qualification and benefited in the post-policy period from the entry requirement exemption by engaging more in entrepreneurship. In this case, the required educational level was inadequate for the educational requirements of the jobs and hindered transitions into self-employment. The same conclusion applies to unemployed and inactive workers who were restrained from entry into self-employed by this inadequate educational requirement, because a large fraction of this group had low qualifications.

Consequently, there are two ways to help an individual jump the hurdle to self-employment. First, reducing the size of the hurdle can help. Second, promoting professional education could lead to

greater jumping power. As the results have demonstrated, occupational groups in craftsmanship are heterogeneous. Therefore, a general entry requirement is suboptimal if it does not consider the heterogeneous propensities for education.

One of the caveats to be taken into consideration is that this analysis is based on limited observations for the post-policy period, which includes only two years after the amendment to the HwO. The effects of this policy change might still be significant in subsequent periods, so the results of this analysis should be regarded with appropriate caution.

For policy makers, the results suggest that reducing entry regulation may be considered a suitable policy instrument to promote entrepreneurship; reforming a regulation with rip-off characteristics could release some beneficial energies by invoking an entrepreneurial spirit.

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Notes

[1]When a major amendment to the HwO reduced the number of regular craftsmanship occupations from 127 to 94 in 1998, the entry requirement for A-occupations remained untouched.

[2]There are non-craft occupations for which a person can obtain a master degree. However, according to Müller (2006), approximately 70% of all master examinations involve craftsmanship.

[3]This amendment is based on two laws, *the grand amendment to HwO* (Drittes Gesetz zur Änderung der Handwerksordnung und anderer handwerksrechtlicher Vorschriften) and *the small amendment to HwO* (Gesetz zur Änderung der Handwerksordnung und zur Förderung von Kleinunternehmen).

[4]In case the optimal step size search did not converge, bootstrapped standard errors were computed.

[5]The post-policy period is defined as the period from 2004 to 2006. However, the data from 2004 refer to the beginning of this year, which basically represents the status quo ante, so the post-policy period in this analysis includes only the years 2005 and 2006. The post-policy dummy equals 1 for both years, which prevents the interaction effect from differing in the post-policy periods.

[6]The effects, standard errors, and p-values calculated according to Ai and Norton (2003) are reported, followed in parentheses by the respective values calculated according to Puhani (2008).

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Table 1: Weighted averages by treatment and control groups in pooled sample

	Unit	nmc	sjr	easyjobs	strictmaster
Female	(%)	58.95	17.11	3.00	42.04
Age	(*a*)	42.97	39.15	39.50	39.17
Residency in the East	(%)	16.70	21.39	23.37	17.27
Self-employed	(%)	8.49	16.50	13.36	19.82
Job tenure	(*a*)	8.81	10.36	10.63	9.96
Nationality					
German	(%)	80.92	90.39	90.72	95.62
EU	(%)	5.08	3.69	3.41	2.00
Non-EU	(%)	13.99	5.92	5.87	2.39
Professional qualification					
University	(%)	1.18	0.81	0.30	0.95
UAS*	(%)	0.97	1.27	0.54	1.56
Master of crafts	(%)	5.56	17.92	16.44	27.36
Apprenticeship	(%)	55.87	67.16	70.95	63.64
No qualification	(%)	32.17	9.18	8.05	3.18
Non-response	(%)	4.26	3.67	3.72	3.31
Secondary School					
Qualification for university entrance	(%)	5.24	4.93	2.78	16.16
Other secondary school	(%)	84.78	90.58	92.57	81.12
No qualification	(%)	6.54	1.80	1.81	0.26
Non-response	(%)	3.44	2.70	2.84	2.45
Children under 16	(#)	0.59	0.58	0.55	0.50
Married	(%)	70.02	59.33	59.45	57.67
City size					
$>$ 500,000 inhabitants	(%)	13.97	10.75	9.84	12.07
20,000 $-$ 500,000	(%)	45.12	38.71	37.88	43.94
$\leq$ 20,000	(%)	40.91	50.54	52.28	43.98
Incomes					
Public payment I	(%)	0.51	0.87	0.69	0.78
Personal net income	(*k*)	1.0051	1.4289	1.4609	1.5663
HH net income	(*k*)	2.1486	2.3667	2.3389	2.7541
Capital income	(%)	1.10	1.63	1.44	2.80
Rent income	(%)	2.53	4.79	5.06	6.51
Percentage of all self-employed craftsmen		25.13	46.57	22.77	5.53
Number of observations		8,090	7,715	4,660	763

* University of applied sciences.

Notes: All numbers weighted. Source: Own calculations based on the scientific use file of the German microcensus (2002-2006).

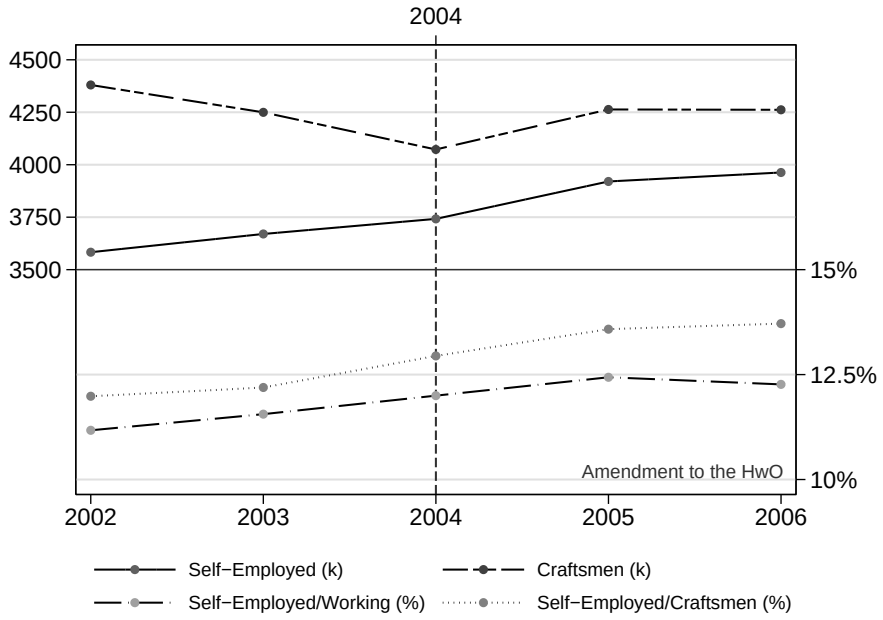

Figure 1: Self-employment and craftsmanship in Germany. Left ordinate: Number of self-employed and craftsmen in thousands. Right ordinate: Percentage share of self-employed among working population and among craftsmen. Source: Own calculations based on the scientific use file of the German microcensus (2002-2006).

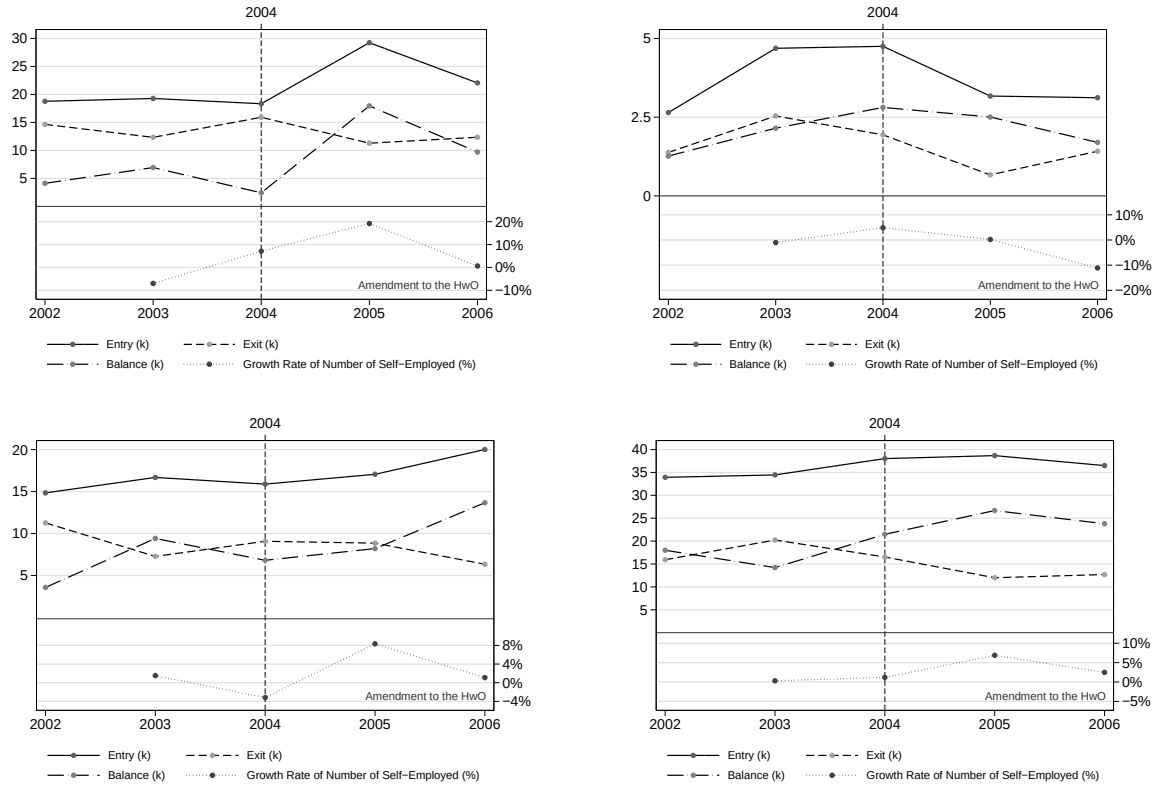

Figure 2: Entries into and exits from self-employment and their difference among *nmc* (upper left), *strict* (upper right), *easyjobs* (lower left), and *sjr* (lower right). Left ordinates: Number in thousands, Right: Growth rate in percent. Source: Own calculations based on the scientific use file of the German microcensus (2002-2006).

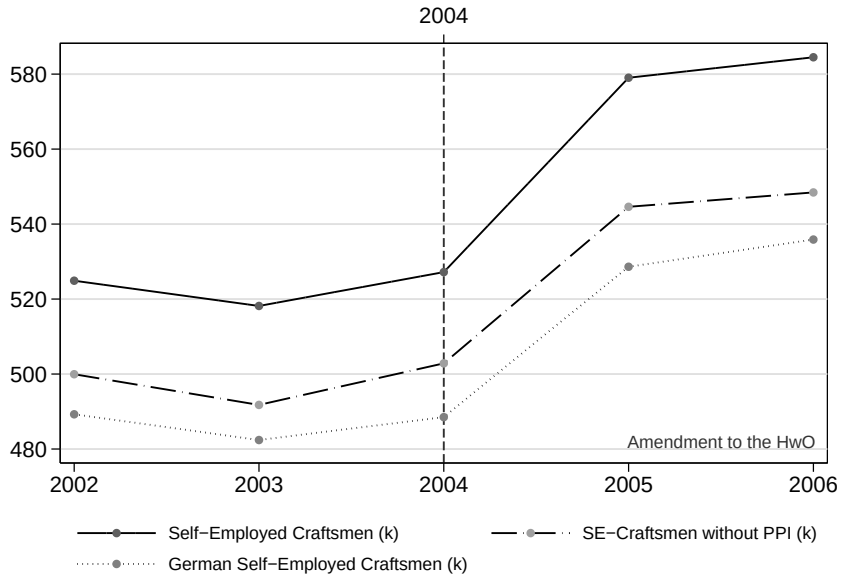

Figure 3: Craftsmanship and entrepreneurship policies. Total, unsubsidized, and German self-employed craftsmanship in thousands. Source: Own calculations based on the scientific use file of the German microcensus (2002-2006).

Table 2: Logit estimation results of self-employment state and transition probabilities

	Self-employed	Entry	Exit
nmc·policy	0.2554***	0.5949**	−0.0747
	(0.0968)	(0.2593)	(0.5628)
sjr·policy	0.1235	0.3630	−0.3119
	(0.0937)	(0.2499)	(0.5661)
easyjobs·policy	0.1531	0.4447*	0.1745
	(0.0977)	(0.2632)	(0.5837)
eu·policy	0.3767***	0.4967*	−0.4452
	(0.1147)	(0.2963)	(0.5734)
nmc	−0.6210***	−0.4210**	0.6394*
	(0.0635)	(0.1967)	(0.3718)
sjr	0.1378**	0.0614	0.1384
	(0.0646)	(0.1972)	(0.3704)
easyjobs	−0.0947	−0.2369	0.0389
	(0.0690)	(0.2129)	(0.3924)
policy	0.0333	−0.1661	−0.6316
	(0.0908)	(0.2434)	(0.5448)
eu	0.5453***	0.2023	−0.5876
	(0.0971)	(0.2821)	(0.4344)
Constant	−5.5861***	−4.6591***	1.8972*
	(0.1753)	(0.4301)	(0.9914)
Year dummies	✓	✓	✓
Branch dummies	✓	✓	✓
Controls	✓	✓	✓
Wald χ^2	17879.69	1160.73	436.03
Log likelihood	-36825.30	-7347.11	-1383.53
Number of observations	123539	83040	9267

Notes: Heteroscedasticity-robust standard errors in parentheses below logit coefficients.
Significance of the logit coefficients is indicated at the 10%/5%/1% level by asterisks (*/**/***).
Source: Own calculations based on the scientific use file of the German microcensus (2002-2006).

Table 3: Probabilities of being self-employed (in %): Difference in differences

	nmc	sjr	easyjobs	**strict**	Δ_{nmc}	Δ_{sjr}	$\Delta_{easyjobs}$
Before reform 2004	5.04	10.18	8.24	8.99	−3.95	1.19	−0.74
	(0.15)	(0.21)	(0.24)	(0.49)	(0.49)	(0.54)	(0.56)
After reform 2004	6.69	11.83	9.87	9.36	−2.68	2.46	0.51
	(0.23)	(0.29)	(0.33)	(0.64)	(0.65)	(0.69)	(0.72)
Δ between after and	1.65	1.65	1.63	0.38	**1.27**	**1.27**	**1.25**
before reform 2004	(0.24)	(0.34)	(0.37)	(0.76)	(0.78)	(0.80)	(0.82)
Before reform 2004	5.04	10.18	8.24	8.99	−3.95	1.19	−0.74
	(0.15)	(0.21)	(0.24)	(0.49)	(0.49)	(0.54)	(0.56)
After reform 2004	5.26	10.60	8.59	9.36	−4.10	1.24	−0.77
	(0.47)	(0.88)	(0.75)	(0.64)	(0.43)	(0.61)	(0.55)
Δ between after and	0.22	0.42	0.35	0.38	−0.16	0.04	−0.03
before reform 2004	(0.45)	(0.86)	(0.71)	(0.76)	(0.31)	(0.10)	(0.05)
Δ_{Δ}					**1.43**	**1.23**	**1.28**
					(0.50)	(0.89)	(0.78)

Notes: Expected probability of a person with average characteristics. The average of the probability among actual persons in the data yields similar results and is available upon request.
Standard errors calculated by the delta method are in parentheses. Source: Own calculations based on the scientific use file of the German microcensus (2002-2006).

Table 4: Probabilities of entry into self-employment (in %): Difference in differences

	nmc	sjr	easyjobs	**strict**	Δ_{nmc}	Δ_{sjr}	$\Delta_{easyjobs}$
Before reform 2004	0.99	1.59	1.18	1.50	−0.51	0.09	−0.31
	(0.10)	(0.12)	(0.13)	(0.27)	(0.27)	(0.29)	(0.30)
After reform 2004	1.53	1.96	1.58	1.29	0.24	0.67	0.29
	(0.13)	(0.12)	(0.14)	(0.22)	(0.24)	(0.26)	(0.26)
Δ between after and	0.54	0.37	0.40	−0.21	**0.75**	**0.58**	**0.61**
before reform 2004	(0.15)	(0.17)	(0.17)	(0.34)	(0.35)	(0.36)	(0.36)
Before reform 2004	0.99	1.59	1.18	1.50	−0.51	0.09	−0.31
	(0.10)	(0.12)	(0.13)	(0.27)	(0.27)	(0.29)	(0.30)
After reform 2004	0.85	1.37	1.02	1.29	−0.44	0.08	−0.27
	(0.22)	(0.33)	(0.26)	(0.22)	(0.19)	(0.27)	(0.23)
Δ between after and	−0.14	−0.22	−0.16	−0.21	0.07	−0.01	0.04
before reform 2004	(0.21)	(0.33)	(0.25)	(0.34)	(0.13)	(0.03)	(0.09)
Δ_{Δ}					**0.68**	**0.59**	**0.56**
					(0.24)	(0.35)	(0.28)

Notes: Expected probability of a person with average characteristics. The average of the probability among actual persons in the data yields similar results and is available upon request.
Standard errors calculated by the delta method are in parentheses. Source: Own calculations based on the scientific use file of the German microcensus (2002-2006).

Table 5: (Logit) Probabilities of exit from self-employment (in %): Difference in differences

	nmc	sjr	easyjobs	**strict**	Δ_{nmc}	Δ_{sjr}	$\Delta_{easyjobs}$
Before reform 2004	5.16	3.19	2.90	2.79	2.37	0.40	0.11
	(0.76)	(0.47)	(0.55)	(0.97)	(1.16)	(1.02)	(1.07)
After reform 2004	2.58	1.25	1.83	1.49	1.10	−0.23	0.35
	(0.39)	(0.20)	(0.34)	(0.63)	(0.72)	(0.65)	(0.70)
Δ between after and	−2.58	−1.94	−1.07	−1.31	**-1.27**	**-0.63**	**0.24**
before reform 2004	(0.80)	(0.49)	(0.63)	(1.13)	(1.31)	(1.18)	(1.24)
Before reform 2004	5.16	3.19	2.90	2.79	2.37	0.40	0.11
	(0.76)	(0.47)	(0.55)	(0.97)	(1.16)	(1.02)	(1.07)
After reform 2004	2.78	1.70	1.54	1.49	1.29	0.22	0.06
	(1.50)	(0.92)	(0.86)	(0.63)	(1.11)	(0.62)	(0.60)
Δ between after and	−2.38	−1.49	−1.36	−1.31	−1.08	−0.18	−0.05
before reform 2004	(1.54)	(0.96)	(0.88)	(1.13)	(0.64)	(0.42)	(0.48)
Δ_{Δ}					**-0.19**	**-0.45**	**0.29**
					(1.52)	(0.93)	(0.90)

Notes: Expected probability of a person with average characteristics. The average of the probability among actual persons in the data yields similar results and is available upon request.
Standard errors calculated by the delta method are in parentheses. Source: Own calculations based on the scientific use file of the German microcensus (2002-2006).

Appendix

A SPECIFICATION AND SENSITIVITY TESTS

Table A.1: Placebo estimation results of the logit model

	Self-employed: Placebo reform in 2002 (2001-2004)	Self-employed: Placebo reform in 2002 (2002-2004)	Self-employed: Placebo reform in 2003 (2001-2004)	Self-employed: Placebo reform in 2003 (2002-2004)
	I	II	III	IV
nmc·policy	0.1823*	0.0322	0.1630	0.0606
	(0.1042)	(0.1283)	(0.1228)	(0.1299)
sjr·policy	0.2655***	0.1346	0.2222*	0.1161
	(0.1002)	(0.1237)	(0.1183)	(0.1252)
easyjobs·policy	0.1979*	0.1389	0.1526	0.0903
	(0.1048)	(0.1293)	(0.1239)	(0.1311)
eu·policy	0.2770*	0.3177*	0.2823*	0.2627
	(0.1443)	(0.1797)	(0.1620)	(0.1717)
nmc	−0.7684***	−0.6510***	−0.7188***	−0.6494***
	(0.0764)	(0.1081)	(0.0644)	(0.0791)
sjr	−0.0440	0.0653	0.0328	0.1167
	(0.0780)	(0.1083)	(0.0669)	(0.0810)
easyjobs	−0.1922**	−0.1572	−0.1319*	−0.0945
	(0.0830)	(0.1145)	(0.0715)	(0.0864)
policy	−0.1270	−0.0672	−0.0635	−0.0250
	(0.0960)	(0.1171)	(0.1135)	(0.1197)
eu	0.4217***	0.3352**	0.4904***	0.4618***
	(0.1222)	(0.1658)	(0.1042)	(0.1230)
Constant	−5.6950***	−5.6484***	−5.7564***	−5.6847***
	(0.2017)	(0.2378)	(0.1984)	(0.2283)
Year dummies	✓	✓	✓	✓
Branch dummies	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Wald χ^2	14886.09	11028.52	14898.34	11035.04
Log likelihood	-28593.64	-21454.22	-28594.27	-21454.11
Number of observations	101709	75137	101709	75137

Notes: Heteroscedasticity-robust standard errors in parentheses below logit coefficients.
Significance of the logit coefficients is indicated at the 10%/5%/1% level by asterisks (*/**/***).
Source: Own calculations based on the scientific use file of the German microcensus (2002-2006).

Table A.2: Logit estimation results of self-employment probabilities

	Self-employed: Groups · time trend (2001-2004)	Self-employed: Groups · time trend (2002-2004)	Self-employed: Branches · time trend (2002-2006)
	I	II	III
nmc·time trend	0.1067**	0.0309	
	(0.0467)	(0.0751)	
sjr·time trend	0.1368***	0.0837	
	(0.0450)	(0.0723)	
easyjobs·time	0.0912*	0.0767	
trend	(0.0471)	(0.0756)	
nmc·policy			0.2846***
			(0.0987)
sjr·policy			0.0828
			(0.1018)
easyjobs·policy			0.0753
			(0.1080)
nmc	−0.8364***	−0.6911***	−0.6329***
	(0.0889)	(0.1632)	(0.0641)
sjr	−0.1155	−0.0123	0.1543**
	(0.0894)	(0.1600)	(0.0666)
easyjobs	−0.2294**	−0.2180	−0.0614
	(0.0947)	(0.1679)	(0.0715)
policy			−0.0609
			(0.0996)
Constant	−5.6276***	−5.6223***	−5.6271***
	(0.2052)	(0.2621)	(0.1924)
Year dummies	✓	✓	✓
Branch dummies	✓	✓	✓
Branch·trend			✓
Controls	✓	✓	✓
Wald χ^2	14894.23	11033.50	17903.66
Log likelihood	-28591.01	-21452.94	-36806.28
Number of observations	101709	75137	123539

Notes: Heteroscedasticity-robust standard errors in parentheses below logit coefficients. Significance of the logit coefficients is indicated at the 10%/5%/1% level by asterisks (*/**/***).
Source: Own calculations based on the scientific use file of the German microcensus (2002-2006).

Table A.3: Logit estimation results using a narrow definition of craftsmanship

	Self-employed	Entry	Exit
nmc·policy	0.2688***	0.5504**	0.0521
	(0.0997)	(0.2655)	(0.5766)
sjr·policy	0.1074	0.3585	−0.2692
	(0.0958)	(0.2536)	(0.5849)
easyjobs·policy	0.0965	0.4513	−0.0379
	(0.1082)	(0.2911)	(0.6414)
eu·policy	0.3351**	0.8005*	0.1846
	(0.1484)	(0.4303)	(0.8762)
nmc	−0.3855***	−0.2430	0.6875*
	(0.0704)	(0.2097)	(0.3835)
sjr	0.4009***	0.1562	−0.0357
	(0.0689)	(0.2053)	(0.3842)
easyjobs	0.3993***	0.0274	0.3578
	(0.0825)	(0.2500)	(0.4635)
policy	0.0457	−0.1918	−0.7558
	(0.0924)	(0.2460)	(0.5528)
eu	0.6641***	−0.0806	−1.3580**
	(0.1269)	(0.4133)	(0.6816)
Constant	−6.2669***	−4.8881***	3.1339***
	(0.2115)	(0.5092)	(1.1339)
Year dummies	✓	✓	✓
Branch dummies	✓	✓	✓
Controls	✓	✓	✓
Wald χ^2	13504.06	917.55	342.58
Log likelihood	-26165.54	-5308.22	-945.67
Number of observations	88999	59865	6583

Notes: Heteroscedasticity-robust standard errors in parentheses below logit coefficients. Significance of the logit coefficients is indicated at the 10%/5%/1% level by asterisks (*/**/***).

Source: Own calculations based on the scientific use file of the German microcensus (2002-2006).

Table A.4: Logit estimation results on German sample

	Self-employed	Entry	Exit
nmc·policy	0.2345**	0.6301**	−0.1961
	(0.0988)	(0.2631)	(0.5825)
sjr·policy	0.0936	0.3639	−0.3023
	(0.0955)	(0.2530)	(0.5821)
easyjobs·policy	0.1211	0.4421*	0.0384
	(0.0996)	(0.2674)	(0.6027)
nmc	−0.5741***	−0.4415**	0.6618*
	(0.0647)	(0.1992)	(0.3944)
sjr	0.1649**	0.0157	0.1578
	(0.0656)	(0.1988)	(0.3913)
easyjobs	−0.0361	−0.2626	0.0834
	(0.0702)	(0.2159)	(0.4146)
policy	0.0536	−0.1685	−0.6426
	(0.0923)	(0.2456)	(0.5596)
Constant	−5.2614***	−4.7247***	1.6213
	(0.1731)	(0.4308)	(1.0262)
Year dummies	✓	✓	✓
Branch dummies	✓	✓	✓
Controls	✓	✓	✓
Wald χ^2	16665.72	1068.66	358.11
Log likelihood	-34065.47	-6626.63	-1245.90
Number of observations	111216	74903	8630

Notes: Heteroscedasticity-robust standard errors in parentheses below logit coefficients.
Significance of the logit coefficients is indicated at the 10%/5%/1% level by asterisks (*/**/***).
Source: Own calculations based on the scientific use file of the German microcensus (2002-2006).

Table A.5: Logit estimation results on unsubsidized craftsmen sample (PPI)

	Self-employed	Entry	Exit
nmc·policy	0.2175**	0.5274*	−0.0637
	(0.0987)	(0.2754)	(0.5631)
sjr·policy	0.1106	0.3486	−0.2971
	(0.0954)	(0.2648)	(0.5660)
easyjobs·policy	0.1381	0.3683	0.2070
	(0.0995)	(0.2788)	(0.5840)
eu·policy	0.3680***	0.4930	−0.4265
	(0.1187)	(0.3189)	(0.5802)
nmc	−0.6161***	−0.4327**	0.6370*
	(0.0648)	(0.2084)	(0.3699)
sjr	0.1381**	0.0475	0.1229
	(0.0660)	(0.2089)	(0.3678)
easyjobs	−0.0895	−0.2011	−0.0014
	(0.0704)	(0.2250)	(0.3904)
policy	0.0325	−0.1413	−0.6332
	(0.0924)	(0.2572)	(0.5445)
eu	0.6486***	0.2979	−0.6654
	(0.1020)	(0.3065)	(0.4415)
Constant	−5.7886***	−4.5528***	2.0798**
	(0.1818)	(0.4555)	(0.9928)
Year dummies	✓	✓	✓
Branch dummies	✓	✓	✓
Controls	✓	✓	✓
Wald χ^2	17538.58	1076.52	443.60
Log likelihood	-35239.31	-6458.17	-1363.51
Number of observations	122697	82435	8929

Notes: Heteroscedasticity-robust standard errors in parentheses below logit coefficients. Significance of the logit coefficients is indicated at the 10%/5%/1% level by asterisks (*/**/***).
Source: Own calculations based on the scientific use file of the German microcensus (2002-2006).

B DESCRIPTION OF KEY VARIABLES

Entrepreneur: Are you working as self-employed (with or without employees)?

NMC, SJR, Easyjobs, Strict: Job title of most recent occupation. Occupational groups are constructed according to job titles in HwO.

Policy: Dummy indicating the post policy period from 2005 to 2006.

Entry, Exit: Employment status in previous year. This non-mandatory question was included before 2005 for 0.45% of the German population and for 1% of the German population in 2005 and 2006.

Public Payment I: Indicates receiving significant subsidies. The questionnaire asks: How many and which public payments or subsidies do you receive? No response option for entrepreneurship subsidies, but for *other public payments*. Accounting for child benefit, other subsidies like first-home buyer allowance are not distinguishable from entrepreneurship subsidies. Nevertheless, after excluding individuals eligible for child benefit, a dummy variable includes all recently (assuming start-ups are subsidized at most for three years) self-employed individuals, who earn below 26,076 Euro (close to the 25,000 Euro threshold of the EXGZ) per year and receive public payments. Although this approach fits the figures of the Federal Employment Agency quite well, the reader should be aware of how it was constructed. This variable is used in Tables 1 and A.5.

Public Payment II: Indicates receiving any subsidies. The questionnaire asks: How many other public payments or subsidies do you receive? A dummy is created for individuals who responded that they receive a nonzero number of other public payments.