IV.2 Entry regulation and entrepreneurship: a natural experiment in German craftsmanship[59] (Davud Rostam-Afschar)

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Abstract

This paper uses the 2004 amendment to the German Trade and Crafts Code as a natural experiment for assessing the causal effects of this reform on the probabilities of being self-employed and of transition into and out of self-employment. This is achieved by using repeated cross-sections (2002–2009) of German microcensus data. I apply the difference-in-differences technique for three groups of craftsmen which were subject to different intensities of treatment. The results show that the complete exemption from the educational entry requirement has fostered self-employment significantly by substantially increasing the entry probabilities, while exit rates have remained unaffected. I find similar, though weaker relative effects for the treatment groups that were subject to a reduction of entry costs or a partial exemption from the entry requirements. Moreover, I consider effect heterogeneity within each of the treatment groups with respect to gender and vocational training, and show that the deregulation of entry requirements has been most effective for untrained workers.

1 Introduction

How does entry regulation influence entrepreneurship? In an attempt to answer this question, many different kinds of regulation, such as the regulation of product and labor markets, have been investigated. The theoretical predictions of the effects of these kinds of regulations are ambiguous. On the one hand, the public choice theory argues that regulations lead to socially inefficient outcomes, while on the other hand the public interest

59 Rostam-Afschar, D. (2014): Entry regulation and entrepreneurship: a natural experiment in German craftsmanship, Empirical Economics, 47(3), 1067–1101.

theory of regulation claims that regulations serve to cure market failures. With a focus on entrepreneurship, Branstetter et al. (2013) predict that a reform which reduces the fixed costs of setting up a business leads to an increase in the number of firms as well as in employment but the additional firms will have entrepreneurs with relatively lower entrepreneurial ability. This study further shows that these firms will be smaller and have a lower probability of survival.

Empirical evidence tends to support the view that various implementations of entry regulation have detrimental effects. Most of these studies rely on aggregate data from many countries, as in the influential work by Djankov et al. (2002) and subsequently in research by Klapper et al. (2006); Ciccone and Papaioannou (2007), and van Stel et al. (2007). In addition, evidence based on microdata (cf. Bruhn 2011; Ardagna and Lusardi 2010, 2009; Branstetter et al. 2013) enforces the conclusion that lower entry costs increase entry into (formal) entrepreneurship.

Moreover, there is a set of studies that, while not directly focusing on entrepreneurship, investigates the effects of entry regulation with microdata, including, e.g., Bertrand and Kramarz (2002); Sadun (2008); Viviano (2008). In an important contribution, Bertrand and Kramarz (2002) evaluate a commercial zoning regulation implemented via regional zoning boards in French retailing. This study finds that greater entry regulation reduced employment growth in the retail sector, while concentration and prices increase. Other work connected to this study investigates the effects of product market deregulation for industry dynamics (Aghion et al. 2009; Cetorelli and Strahan 2006; Kerr and Nanda 2009). From studying a deregulation of the French banking industry in the 1980s, one of the findings in Bertrand et al. (2007) corroborates the notion that less state intervention is associated with increased firm entry and exit rates. Briefly, the empirical literature almost unanimously comes to the conclusion that entry regulation in its various forms restrains entrepreneurship and similarly other economic outcomes.

One particularly interesting implementation of entry regulation is the requirement of the Meister degree in German craftsmanship, as required by the German Trade and Crafts Code (HwO) for registration as an entrepreneur. Prantl and Spitz-Oener (2009) and Prantl (2012) explicitly consider the entry requirement for craftsmanship to discuss regulatory effects in the wake of German reunification in 1990.

The aim of this study is to evaluate a change in the regulatory requirements empirically. It contributes to the literature on entry regulation and

entrepreneurship by providing evidence of the causal effects of entry regulation, exploiting this change to the HwO as a natural experiment. Dating back to the late nineteenth century, this latter entry requirement, called Meister (see Sect. 2), underwent a dramatic change: the amendment to the HwO in January 2004 decreased the number of occupations in which craftsmen were required to hold a Meister degree in order to start a business from 94 to 41. Moreover, the entry requirements for the remaining 41 occupations were relaxed. To the best of my knowledge, this is the first paper to use this setting as a natural experiment.

The reform was the result of a passionate debate in which proponents of the entry requirement (e.g., German Confederation of Skilled Crafts 2003) cited market failures resulting from information asymmetries and external effects, while opponents (e.g., German Deregulation Commission 1991; German Monopolies Commission 1998, 2002) objected, in the spirit of the public choice theory, that these regulations would lead to greater inefficiencies. The government justified the regulation primarily as a means to prevent health related dangers. This argument, in turn, was itself controversial because there was no agreement as to whether the costs of regulation would outweigh the costs incurred by careless craftsmen doing hazardous jobs, for example barbers or chimney sweeps.

Focusing on entrepreneurship, in addition to credit constraints (e.g., Evans and Jovanovic 1989, Blanchflower and Oswald 1998, Hurst and Lusardi 2004, Fossen 2011), the entry requirement is regarded as a key impediment to starting a business. For instance, Holtz-Eakin and Rosen (2005) point to the entry requirement as a disincentive to taking up self-employment in German craftsmanship.

To shed some light on the effects of this regulation on entrepreneurship, I use repeated cross-sections (2002–2009) of German microcensus data on self-employment to proxy for business creation. I apply the difference-in-differences (DID) approach to estimate the effects of the policy change for three distinct occupational groups on the probability of self-employment, as well as the probability of transitioning into and out of self-employment.

The empirical results provide evidence that the probability of being self-employed increased in line with the amendment to the HwO. The strongest relative increase significantly raised the probability of self-employment to a level more than 40 % higher than a hypothetical situation without the reform for an occupational group with a relatively low propensity to engage in entrepreneurship. This group, hereafter referred to as the group of B1-occupations, has been completely exempted from the entry

requirement. The reform also seems to have increased the probability of being self-employed for professions that experienced only a reduction of or a partial exemption from the entry requirement. The effects for these groups are also positive, although weaker. The analysis shows further that these increases resulted from increasing the probability of entry, while the probability of exit from self-employment has remained virtually unaffected by the policy change. The reforms seem to have affected individuals across professional qualifications differently; the deregulation of entry has been most effective for the group of untrained workers who are disadvantaged in the labor market.

Below, in Sects. 2 and 3, respectively, I describe the institutional framework of the natural experiment and outline the empirical approach. In Sects. 4 and 5, I describe the data and discuss the results. Section 6 concludes.

2 The amendment to the German Trade and Crafts Code in 2004 as a natural experiment

Over the course of time, three key institutions of German craftsmanship have emerged: the small proof of competence (Kleiner Befähigungsnachweis), the greater 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 Meister certificate, though such a degree was not required to start a business. However, the greater proof of competence mandated that craftsmen obtain a Meister 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 greater proof of competence, and unrestricted trades similar to crafts (Handwerksähnliche Gewerbe), referred to in this text as A-occupations and B2-occupations, respectively. In this study, the focus is on craftsmen in A-occupations who remained regulated by a form of the greater proof of competence, in contrast to those in B2-occupations.

The Meister title is the highest professional degree in craftsmanship. To attain it, a person must complete several levels of training and pass examinations. Having obtained the qualification level called Geselle, a craftsman could be employed in a business or continue on to a Meister degree.

Full-time courses to prepare for the Meister exam take 1–3 years, and the occupation-specific overall costs range, according to the Chambers of Crafts and Trade, from 4,000 to 10,000 Euros. The Meister exam tests both occupation-specific skills and general education in business and commercial knowledge, as well as law. Moreover, the exam contains a pedagogical component, as holding a Meister degree makes the craftsman eligible to train apprentices. Those who have passed the examination and started a business are recorded in the register; though in rare exceptional cases, some people may be recorded in the register without a Meister degree.

In the situation immediately prior to the amendment to the HwO in 2004, the options available to a crafts-person were to get hired in a business or to set up a business after having obtained the Meister degree. This analysis exploits this reform to assess the causal effects of entry regulation on entrepreneurship. In the next section, I describe how the different components of this reform altered the options available to a craftsman, and define treatment groups and a control group accordingly.

Table 4.2.1: The natural experiment

Before	Requirement	After	Requirement
A	(Meister)	*AC*	(Meister)
A	(Meister)	*A1*	(Altgeselle)
A	(Meister)	*A2*	(Altgeselle, no requirement[a])
A	(Meister)	*B1*	(no requirement)
B2	(no requirement)	*B2*	(no requirement)

Notes: This table describes the requirement before and after the reform in descending order of a priori supposed intensity of entry regulation. The control group comprises pre- and post-reform occupations that turned out to belong to the AC-occupations. Each treatment group includes pre- and post-reform occupations that turned out to belong to the B1-, A1-, and A2-occupations, respectively. The occupational groups B1, A1, A2, and AC are defined to be mutually exclusive. However, non-craft occupations and B2-occupations within these groups are not always discriminable due to data protection, and have been excluded from the analysis where possible. The main results remain unchanged when these occupations are included in the sample[a] For A2-occupations, no requirement is imposed after the reform if a prospective entrepreneur commits to limit the range of the activities of his firm to tasks that can be learned within 3 months

3 Empirical specification

3.1 Definition of the treatment and control groups

How did the reform alter the options available to a craftsman? After the reform, a crafts-person could choose to seek employment in a business, regardless of her obtained professional degree, just as before the reform. The choice to start a business on her own, in contrast, was facilitated by the amendment to the HwO. 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. It defines certain occupational groups which are subject to different degrees of regulation. I matched each reported occupation of an individual in the German microcensus with the respective occupation listed in the law, with examples of these vocations provided below. From this information, I was able to construct four occupational dummies that reflect the different intensities of the treatment, as outlined in Table 4.2.1.

The deregulation of the Meister degree requirement, which is the main element of the policy change, generated a group of 53 B1-occupations by dividing up the former 94 A-occupations. After the reform, craftsmen belonging to the group of B1-occupations were allowed to start businesses without a Meister degree, but still had to demonstrate their ability to train apprentices. These B1-occupations represent the treatment group that was deregulated most, referred to as B1-craftsmen. This category includes tile and mosaic layers, coppersmiths, turners, tailors, millers, and photographers.

The remaining 41 A-occupations comprise three more groups: AC, A1, and A2. The AC group is comprised of strictly regulated occupations that remained subject to virtually the same requirements as before the policy change; they had already needed a mandatory Meister certificate to enter entrepreneurial activities. These vocations serve as the control group. They include chimney sweeps, optometrists, hearing aid audiologists, orthopedic technicians, and dental technicians.

The remainder of the A-occupations had their entry restrictions loosened by receiving permission to start a business without a Meister degree after having reached the level of an Altgeselle, i.e., by having proven 6 years of work experience as a Geselle, four of these in a decision-making position, in his or her prospective occupation. This Altgesellen rule defines the third treatment group (A1-occupations) which includes profes-

sions such as roofers, surgical instrument makers, gunsmiths, plumbers, gas and water fitters, joiners, and pastry cooks.

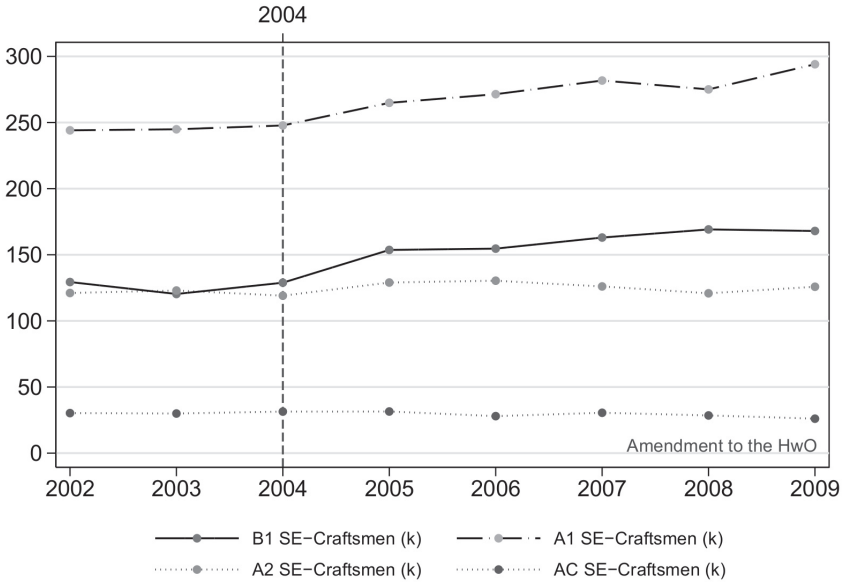
Workers in A1-occupations can start a business without providing proof of any qualification, provided they commit to limiting the range of their activities to tasks that can be learned within 3 months. This partial exemption from the already reduced entry regulation aims particularly at supporting the establishment of small businesses. However, for a prospective entrepreneur who plans to carry out the full range of activities, obtaining vocational training according to the Altgesellen rule is still mandatory. Individuals in occupations that use this so-called easy-job-rule are grouped separately into the A2 group (cf. Müller 2006), including masons and concreters, painters and varnishers, metalworkers, motor vehicle body and vehicle construction mechanics, bike mechanics, information electronics technicians, vehicle technicians, and butchers.

In summary, the three treatment groups are described in descending order of their expected treatment intensity: the B1-, A2-, and A1-occupations, while the AC-occupations are used as the control group. Having defined the three treatments and the control group, I describe in the following the development of the level of self-employment and self-employment rates for these groups.

3.2 Trends in craftsmanship

Between 2002 and 2009, the period relevant for this analysis, the number of self- employed craftsmen remained stable in the control group, while this number increased in the treatment group that has experienced the strongest treatment, i.e., the B1-occupations (see Figure 4.2.1), after the reform in 2004. This growth pattern can also be observed for the A1 and A2 groups, though it is less pronounced. In contrast to the B1 group's almost monotonic increase, the number of A2 craftsmen reverted to its prepolicy level. The number of A1 craftsmen also declined from 2007 to 2008 but nevertheless remained at a substantially higher level than before the reform. These facts may indicate that the reform had a positive impact on the self-employment rate in the treatment groups.

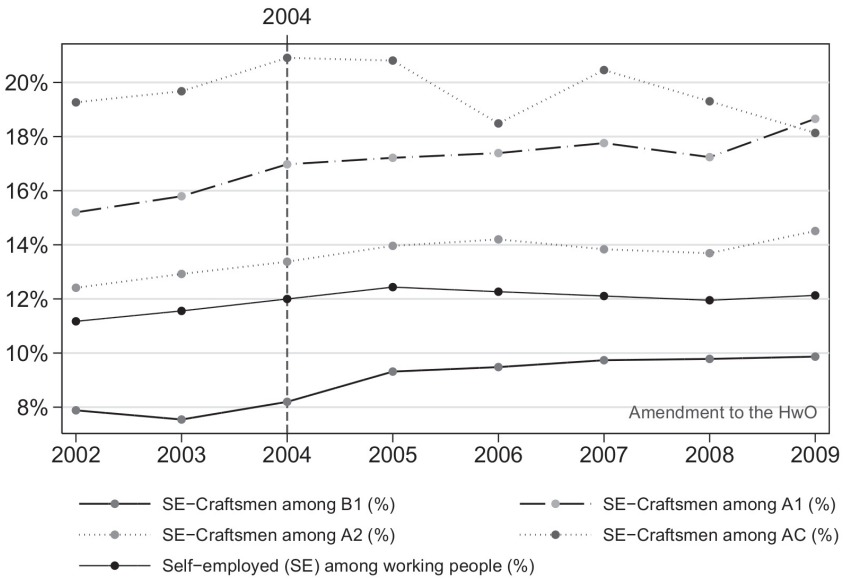
Figure 4.2.1: Self-employment in treatment groups and control group: number of self-employed craftsmen in B1, A1, A2, and AC occupations in thousands



Source: Own calculations based on the scientific use file of the German microcensus (2002–2009)

Figure 4.2.2 depicts the time trends in the self-employment rates, defined as the ratio of the number of self-employed craftsmen to the number of both self-employed and employees in the treatment groups and the control group, respectively.

Figure 4.2.2: Self-employment rates in treatment groups and control group: percentage share of self-employed among B1, A1, A2, and AC occupations



Source: Own calculations based on the scientific use file of the German microcensus (2002–2009)

Before the policy change in 2004, the differences between the time trends of the treatment groups and the control group remained steady. In subsequent years, however, the differences between the self-employment rates of each of the treatment groups and the control group decreased substantially. This may again support the hypothesis that the 2004 reforms increased the probability of self-employment for the treatment groups. Note that the dip in the share of self-employed craftsmen in AC-occupations is due to a temporary increase in the number of employees. See Sect. 5.4 for a robustness check that shows that the results are not driven by this fluctuation.

Interestingly, the trajectory prior to the treatment is very similar for craftsmen compared to non-craftsmen; in my sample, the rate of self-employed among working persons raised from 11.17 % in 2002 gradually to 12.00 % in 2004. In 2005 this figure peaked at 12.44 % and remained thereafter relatively steady around 12 % (see Table 4.2.9). One explanation

for this development might be the substantial increase in the number of self-employed in East-Germany.

3.3 Identification of causal effects

The empirical strategy outlined here exploits the reform of the regulatory framework of entrepreneurial craftsmen in 2004 as a natural experiment. To this end, I calculate the differences in the changes in average outcomes of employment status choices across each treatment group both before and after the reform. I then measure the changes in average outcomes of employment status decisions of the control group before and after the reform. The differences in these changes is known as the DID estimator, and represents the average treatment effects on the treated group (ATT) (e.g., Blundell and Costa Dias 2009).

I use data from 2002 to 2009 for the three occupational groups (B1, A1, A2) subject to different intensities of regulation changes, as detailed in Sect. 3.1. These three groups are used as the treatment groups (cf. Meyer 1995) while the group of AC-occupations is used as the comparison group. To determine the ATT with the DID approach means specifically comparing the difference in the average self-employment probability of each of the three treatment groups before and after the reform with the average self-employment probability of the AC-occupation group before and after the reform.

Therefore, for this 8-year period, I have been able to quantify the effects of the reform on the probability of self-employment. The main hypothesis, based on the theory of public choice, suggests that the policy change could have influenced the self-employment rate 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 either a higher entry rate, a lower exit rate or both. However, an increase could also result from a higher exit rate, which in turn is exceeded by an even higher entry 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 or has no effect at all. Therefore, with this analysis, I investigate not only the probability of being self-employed but also the probability of entry into and exit from self-employment.

Identifying the ATT using the DID approach requires the assumption that the treatment groups and the control group are subject to common trends. This implies that macro shocks exert the same effects on both groups. For example, a sudden decrease in the interest rate should influence trades related to health and hygiene, which are common among the AC group, just as it does the building and construction trades, which are part of the A2 group. If this is true, a hypothetical trend without a reform in the treated group would parallel the trend in the control group in the post-policy period. Otherwise, it would be unclear whether differences between these groups are caused by the reform or by other factors. Section 5.4 provides evidence in favor of the identifying common trend assumption.

Furthermore, this setting does not seem to be susceptible to what is a frequent concern in natural experiments. That is, the problem of self-selection should not exist, because the different treatment groups are distinguished by a law that was proclaimed for the first time in March 2003 (cf. Müller 2006), resulting in a relatively short time for workers to adjust and change occupations.

Work in a specific vocation in craftsmanship, like individual characteristics, changes little over time. In the sample used for the estimation, 73.85 % of the individuals in B1-occupations had been working in their current occupation for 3 years or more in 2004 and 72.80 % in 2008. For the other groups of craftsmen, this figure is larger. Self-employed craftsmen tend to be less likely to change occupations. Again, the B1 group was the most dynamic, though in this group 83.42 % had run their business for 3 years or more in 2004 and 82.18 % in 2008. Therefore, adjusting behavior in expectation of the reform should not challenge the identification of the ATT parameter. Moreover, after the announcement of plans for the amendment to the HwO, a controversy arose with an unpredictable outcome. It was therefore not known what intensity of treatment each occupation would receive 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 complicated new rules.

Regarding changes between groups, the situation after the regulations were eased is somewhat different, as the B2-occupations could be substituted for similar B1 or A-occupations more easily, which means that the compositions of the treatment and control groups might change systematically. For instance, changing from a B2-occupation to engage in self-em-

ployment in a B1-occupation might have been harder for an individual not having obtained the required degree before the reform. Conversely, a craftsman trained in a B1-, or A-occupation might have been more likely to move into an occupation from the B2-vocations because she wants to set up a business before the reform. This would bias the estimate of the treatment if these changes occur in anticipation of the reform.

Moreover, 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. I assume that changes in unobserved factors are the same between the treatment and control groups.

3.4 Other entrepreneurship policies

Some other major policies may also have interfered with the effects of the policy change. These are the enlargements of the European Union (EU) in 2004 and 2007 as well as some subsidies for entrepreneurship.

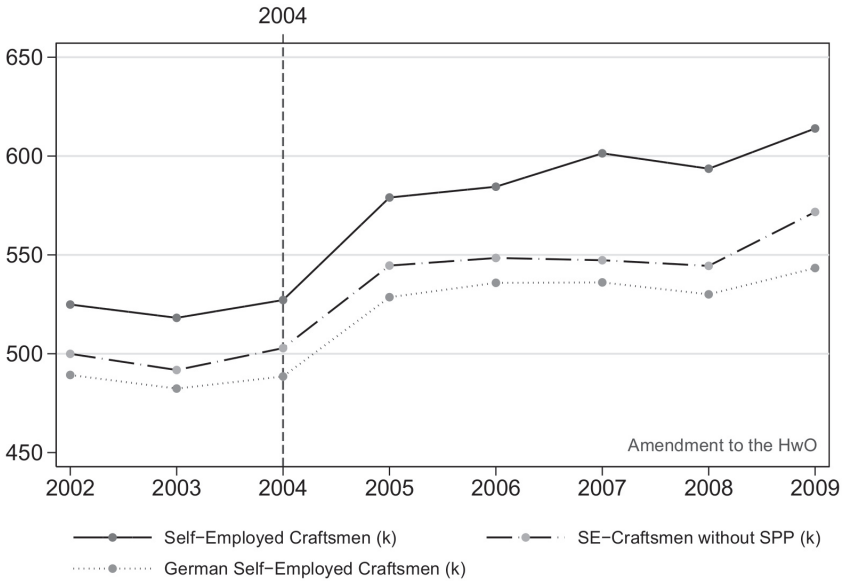
The first relevant enlargement of the EU based on the 2003 Treaty of Accession took place in 2004, when ten countries became new member states. Moreover, the 2007 enlargement of the EU based on the 2005 Treaty of Accession saw Bulgaria and Romania join the EU. Although Germany restricted its labor market from workers from these 12 new member states, exceptions were granted to specific groups. Most importantly, a person was permitted to engage in entrepreneurship immediately after her state of origin became member of the EU.

Other important policy instruments include subsidies to entrepreneurs, such as the transitional allowance (Überbrückungsgeld, 1986–2006), the start-up subsidy (Existenzgründungszuschuss [EXGZ], 2003–2006), the entrance grant for entrepreneurs (Einstiegsgeld für selbständige Tätigkeit, since 2005), and another start-up subsidy (Gründungszuschuss, since 2006) (cf. Caliendo and Steiner 2005, Caliendo and Künn 2011). The years in which each of the programs was adopted and the year of its abolition is given in parentheses. According to Baumgartner et al. (2006), the EXGZ in particular had significant effects on entrepreneurship, and thus could confound the main analysis.

Although there are no reliable numbers, a surmise based on Müller (2006) would imply that just 2.93 % of the A-businesses established in 2004 received the EXGZ, and 2.13 % in 2005. For B1-businesses, less

than 5.79 % of the start-ups in 2004 and 3.58 % in 2005 were subsidized by the EXGZ. This suggests that we should not be too concerned about the effects of these subsidies.

Figure 4.2.3: 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 (2002–2009)

Figure 4.2.3 shows three graphs from 2002 to 2009: the development of total self- employed craftsmanship, the number of craftsmen who did not report receiving SPP payments (a dummy for public payments for self-employed), and the number of German self-employed craftsmen. 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, with data taken from the register of craftsmen. Disregarding the B2-occupations, these figures equal, in each year, approximately 90 % of the stock of businesses reported in Table 4.2.8 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.

Together with the number of self-employed craftsmen, the graphs for unsubsidized, self-employed craftsmen and for German self-employed craftsmen evolve almost uniformly over time, though the effects of the 2007 enlargement of the EU is clearly visible. This suggests again at least that the subsidies did not affect the number of self-employed craftsmen systematically. However, to identify the effect of the amendment to the HwO separately from these policies, I include a dummy indicating EU citizenship and its interaction with the post-policy period in most of the specifications. Moreover, in Sect. 5.3, I discuss the results, first by excluding all non-German craftsmen and then by excluding all craftsmen that receive any subsidy.

3.5 Estimation procedure

In estimating the effects of the reform for all treatments with repeated cross-sections from 2002 to 2009, all three treatment groups are included jointly in the regression models to yield more precise estimates.

I present estimates of logit models using the maximum likelihood estimator in much of the rest of the paper, because predicted probabilities are not bounded by 0 and 1 and the partial effects of independent variables are constant in the linear probability model (LPM). However, I also employed LPMs for all of these specifications and the results remain essentially the same.

In an LPM, 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 in predicted probabilities before and after the reform for the treatment and control groups.

In a logit model, 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.

The dependent variable Yi for observation 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 probability *Prob(·)*. In the main specification, given as Eq. (1) below, the regressors *dPosti, d Oi,* and *Xi* are included in *zi*, where *dPosti* is a dummy variable for individuals observed in the post-policy period;[60] *d Oi = d B1i, d A1i, d A2i* 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, 0, δω, βω, and β4, along with ω = B1, A1, A2, represent the respective coefficients or vector of coefficients, and β0 is a constant.

$$\text{Prob(Yi} = 1|\text{dPosti, dOi, Xi)} = \frac{1}{1+e^{-z_i}} \textit{ with}$$

$$z_i = \beta_0 + \delta_0 dPost_i + \beta_{B1} dB1_i + \beta_{A1} dA1_i + \beta_{A2} dA2_i + \delta_{B1} dB1_i \cdot dPost_i + \delta_{A1} dA1_i \cdot dPost_i + \delta_{A2} dA2_i \cdot dPost_i + X_i \beta_4 . \quad (1)$$

In addition to dummies for the years 2003, 2004, 2006, 2007, 2008 and 2009, all models include in X variables for the following individual characteristics: age and its square, and dummy variables indicating gender, type of secondary schooling and professional qualification, nationality, region of residence, the size of the respondent's city of residence, marital status, number of dependent children, the branch of craftsmanship, the occupation, and a constant. The included indicator *d* EU shows the citizenship of foreigners in an EU member state, and is included along with its interaction with the post-policy period, to separate the effects of the enlargements of the EU from the effects of the amendment to the HwO, as discussed in Sect. 3.4. Controlling for these characteristics is important for two reasons. First, the determinants of self-employment may have changed over the time. Second, including these control variables allows to obtain the 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 probabili-

60 The post-policy period could be defined as the period from 2004 to 2009. 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 the main specifications includes only the years 2005 and 2009. Results from a specification where the post-policy period is defined from 2004 to 2009 or 2004 is dropped are shown in Table 4.2.11. The post-policy dummy equals 1 for both years, which prevents the interaction effect from differing in the post-policy periods. A more flexible specification is presented in Table 4.2.12 and discussed in Sect. 5.4.

ty. The same population is used in the entry models. Note that employment status in the previous year, used for the construction of the transition variables, is queried retrospectively, and it is not mandatory to respond. In contrast, the indication of the current employment status, which is used for the transition variables and the stock variable, is found in the mandatory section of the questionnaire.

Moreover, some unemployed or inactive persons do not report a profession, and it is thus unclear what proportion of these groups participates as a 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.

In contrast, 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 given 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 an employee, unemployed, or inactive. Apart from these differences in the estimation population and the dependent variables, the econometric framework is identical in the stock models and the flow models.

4 Data and descriptives

4.1 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 Labour Force Survey in the United Kingdom. The German microcensus is a 1 % sample of all households in Germany. A subsample of 70 % or approximately 494,000 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 crucial to this analysis, because less than 10 % of the population are craftsmen. Most questions are compulsory; therefore, the German microcensus, a mandato-

ry census, 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 2009. The years before 2002 are not considered for several reasons. First, effects of other policy changes, e.g., the amendment to the HwO from 1998, could still be significant at the beginning of 2001, insofar as the process of adjusting expectations and changing occupations in response to the reform took some time. Second, training in some traditional occupations, such as blacksmiths and turners, ceased as of August 2002, superseded by more modern training structures with new fields of specialization. However, Müller (2006) shows empirical evidence that suggests that these changes had no substantial effect on the transition rates. To avoid confusion due to these influences, I excluded the year 2001 from the analysis. Table 4.2.11 shows that the estimates from the main specification using the years 2001 to 2009 remain similar if 2001 is included. Other results including 2001 are available on request.

The transition variables reflect questions from the supplementary program that ask retrospectively for a person's employment status in the year before the interview. Note that the supplementary questions were only posed to a 45 % random subsample of the microcensus up until 2004. Since the number of observations is still quite large, this does not influence further analysis. However, this program is non-mandatory and therefore non-response is higher.

Indication of status as self-employed is used to measure entrepreneurship in German craftsmanship, because the HwO refers explicitly to self-employment. While the majority of self-employed craftsmen run non-incorporated businesses, the term self-employment can cover also incorporated businesses. The Appendix provides a description of how the key variables are constructed. The reader should keep in mind that indication of status as self-employed is based on self-assessment and thus is rather a proxy for the true number of self-employed.

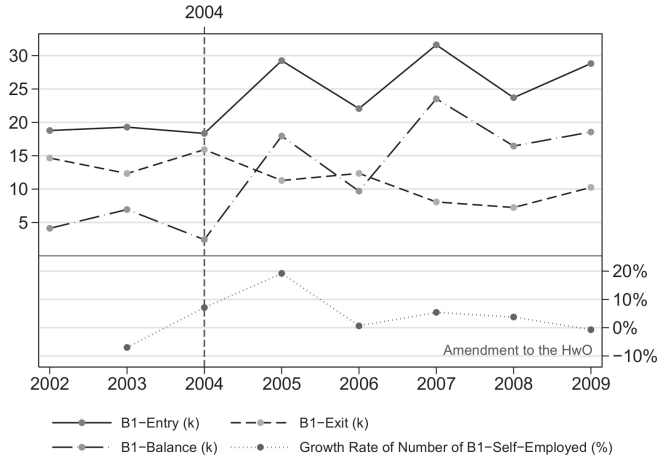
Because the focus of this study is on entrepreneurship among German craftsmen, I restrict the sample as follows, reporting the average number of dropped observations per year in parentheses: I exclude all individuals younger than 18 years, or older than 65 years (177,740). People whose employment status choice is determined by different factors are also omitted from the sample to avoid distortions. Thus, civil servants (11,978), apprentices (7,885), soldiers (968), conscripts (730), persons in education, or those drafted in the previous year (13,531 and 336, respectively), as well

as all remaining non-craftsmen (254,571), are excluded. Moreover, family workers (1,981) 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 me with a sample of about 25,000 observations per year, which represent about 4 million craftsmen in the German population. To complete the picture, the following section shows how the transition variables used in the estimation evolved over time, and describes the characteristics of the occupational groups.

4.2 Descriptives

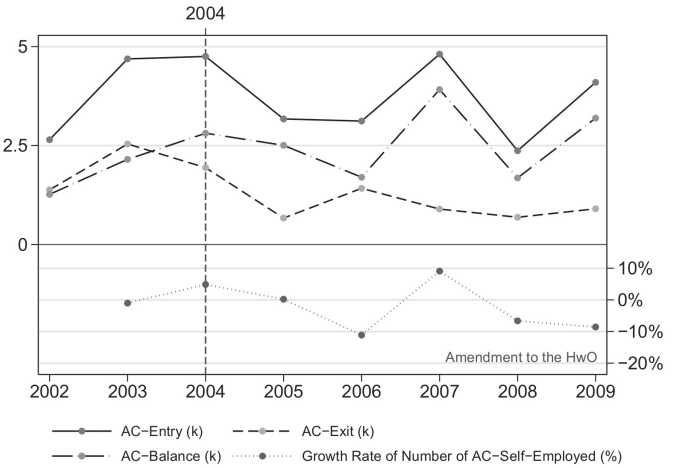
Figure 4.2.4 shows how the number of B1 entries increased tremendously after 2004, returned to a somewhat lower level in 2006, peaked in 2007 and reverted in 2008, but still remained higher than in the period before the reform. The exits remained constant for a time, before declining in the aftermath of the policy change. Note that the balance (defined as entry-exit) exhibits a similar, though less wiggly, path than the number of self-employed craftsmen in Figure 4.2.3, which implies that most of the variation stems from this particular group. The two peaks in 2005 and 2007 might reflect the effects of the enlargements of the EU on the entry rate on top of the effects of the reform to the HwO. A comparison of the path of the growth rate, measured as the annual change in the number of self-employed craftsmen in percentage, and the balance shows how large the non-response bias in the transition variables is, since both variables should contain the same information. Indeed, in almost all of the graphs in Figs. 4.2.4, 4.2.5, 4.2.6, and 4.2.7, the growth rate seems to resemble the pattern of the balance, though only very roughly. Figure 4.2.5 illustrates that neither the entries into nor the exits from the AC-occupations exhibit any singularity until 2006. The subsequent peak might again stem from the enlargement of the EU. The path of the growth rate and balance correspond. Apparently, the numbers of entries and exits are both rather small. For sensitivity tests correcting for rare events see Sect. 5.4.

Figure 4.2.4: Entries into and exits from self-employment and their difference among B1-occupations, left ordinate number in thousands, right growth rate in percent



Source: Own calculations based on the scientific use file of the German microcensus (2002–2009)

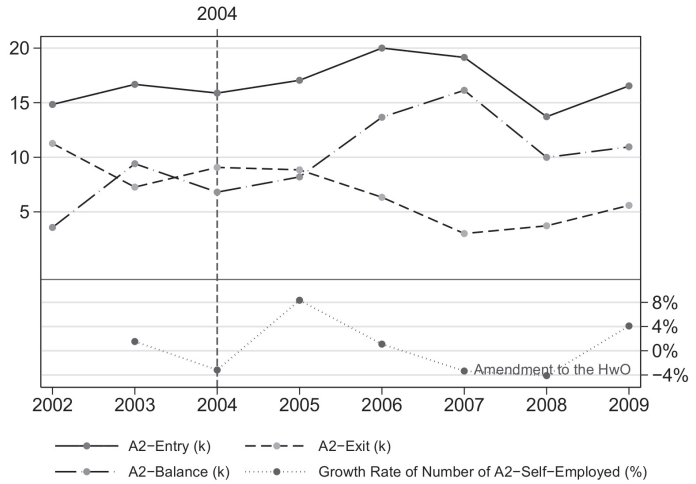
Figure 4.2.5: Entries into and exits from self-employment and their difference among AC-occupations, left ordinate number in thousands, right growth rate in percent



Source: Own calculations based on the scientific use file of the German microcensus (2002–2009)

In Figure 4.2.6, the transition variables do not exhibit any major oscillation. In the post-policy period, the growth rate increases substantially and then slows down, but the balance contrasts with this development.

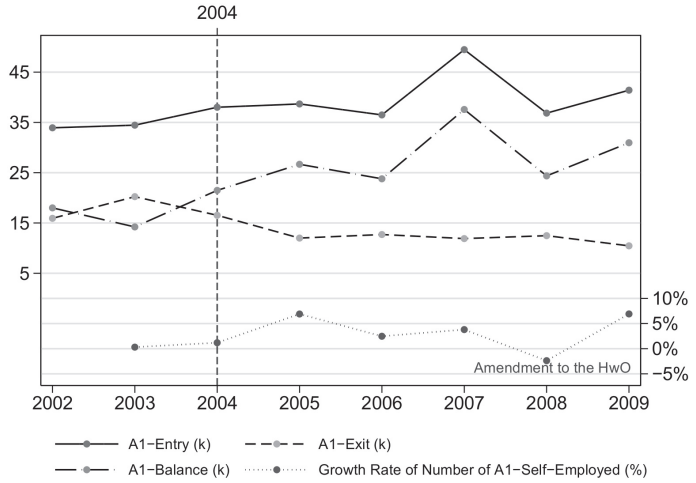
Figure 4.2.6: Entries into and exits from self-employment and their difference among A2-occupations, left ordinate number in thousands, right growth rate in percent



Source: Own calculations based on the scientific use file of the German microcensus (2002–2009)

The series of transitional variables for A1-occupations, depicted in Figure 4.2.7, show that the entries increase modestly, whereas the exits remain roughly constant. Here, the balance series and the growth rate also show an increase in 2005 and a subsequent decrease in 2006. Again, entries peak in 2007.

Figure 4.2.7: Entries into and exits from self-employment and their difference among A1-occupations, left ordinate number in thousands, right growth rate in percent



Source: Own calculations based on the scientific use file of the German microcensus (2002–2009)

Now that we know how the dependent variables developed, I will describe some of the characteristics of the four occupational groups included in the vector of control variables. Furthermore, I will show the share of self-employed craftsmen among all craftsmen in each group, and the share of self-employed craftsmen in each group among all self-employed craftsmen in Table 4.2.2 as weighted averages from the pooled cross-sections from both the pre-policy period (2002–2004) and from the post-policy period (2005–2009). In all three treatment groups, the share of self-employed is higher after the reform than before, while this figure seems to remain constant in the control group. Again, this points to a positive effect of the reform.

Table 4.2.2: Weighted averages by treatment and control groups in pre- and post-reform (2002–2004; 2005–2009) samples

	B1		*A1*		*A2*		*AC*	
	Pre	Post	Pre	Post	Pre	Post	Pre	Post
Self-employed (%)	7.87	9.69	15.96	17.67	12.89	14.05	19.94	19.47
Female (%)	58.81	58.97	16.90	17.59	3.08	2.82	41.24	43.80
Age (a)	42.77	43.66	38.97	39.82	39.46	40.14	38.99	40.13
East (%)	16.47	17.77	21.50	21.70	23.44	23.28	17.71	17.41
Nationality								
German (%)	80.89	80.60	90.17	90.66	90.84	90.50	95.95	96.14
EU (%)	4.64	6.36	3.57	3.98	3.04	3.98	1.80	1.90
Non-EU (%)	14.46	13.03	6.26	5.36	6.12	5.52	2.25	1.96
Professional qualification								
University (%)	1.08	1.31	0.78	0.99	0.25	0.41	1.05	0.71
UAS[a] (%)	0.94	1.13	1.23	1.34	0.53	0.55	1.52	1.54
Meister[b] (%)	5.64	5.24	17.99	17.64	16.37	17.41	27.23	28.85
Geselle[c] (%)	54.32	59.21	65.67	70.15	69.96	72.70	62.46	65.11
None (%)	31.09	32.60	8.50	9.41	7.04	8.45	2.38	3.51
Non-response (%)	6.92	0.52	5.84	0.47	5.85	0.49	5.37	0.28
Secondary school								
Abitur[d] (%)	4.88	5.78	4.74	5.44	2.65	3.23	13.98	18.06
Other[e] (%)	84.00	85.73	89.58	91.78	91.47	93.92	82.02	81.39
None (%)	5.76	7.33	1.54	2.01	1.52	2.12	0.22	0.21
Non-response (%)	5.36	1.16	4.14	0.76	4.36	0.73	3.78	0.34
Children under 16 (#)	0.72	0.64	0.68	0.61	0.65	0.61	0.59	0.55
Married (%)	70.50	68.26	60.01	57.36	60.14	57.81	57.68	57.99
City size								
>500,000 (%)	14.30	14.81	10.89	11.96	10.16	11.25	11.78	13.83
20,000–500,000 (%)	44.93	46.77	38.80	42.05	37.93	41.10	43.09	44.70
≤20,000 (%)	40.77	38.42	50.31	45.99	51.91	47.65	45.13	41.47
% of all self-employed craftsmen	24.11	27.20	46.92	46.67	23.13	21.27	5.84	4.86
Observations	28,188	47,002	27,424	44,675	16,733	25,553	2,792	4,302

Notes: All numbers are weighted by survey weights provided by the microcensus.

Source: Own calculations based on the scientific use file of the German microcensus (2002–2009)

[a] University of Applied Sciences
[b] The Meister-craftsman degree certifies the highest professional qualification in craftsmanship.
[c] The Geselle degree can be obtained by completing an apprenticeship.

[d] Abitur refers to the higher secondary school degree that qualifies a student for university admission in Germany.
[e] Other secondary school refers to a secondary school degree that does not qualify a student for university admission in Germany, typically obtained at a Realschule or a Hauptschule.

A remarkable difference between the treatment groups is that the A2 group has almost no female workers, while the majority of B1 jobs are done by women. Another interesting point is that individuals working in a B1 vocation rarely engage in self- employment compared to the other groups. This is accounted for in the estimation by including the binary variables *dOi*. Moreover, it is noteworthy that persons working in a B1-occupation are on average less qualified, as around 1/3 reports no professional qualification.

Further, the share of craftsmen that served as apprentices, and thus held the vocational degree Geselle, is substantially higher for the post-policy period across all four groups. However, even though the documented increases are large, in particular for the (B1- and) A1-occupations, one should be cautious about attributing this to the effects of the Altgesellen rule as the changes might simply reflect the fact that the survey response probability increased after the reform. Section 5.3 picks up on this in a detailed discussion of heterogeneous effects with respect to gender and different levels of vocational training.

5 Results

Did the 2004 amendment to the HwO have the intended effects? According to the plain DID results from an LPM using pooled cross-sections from 2002 to 2009, shown in the second column of Table 4.2.3, the answer for the B1-occupations is yes. A glance at the coefficient of the interaction term reveals that the reform increased the probability of entering self-employment significantly, by 0.79 percentage points. This result does not change significantly when year and branch dummies and further control variables are added (third column of Table 4.2.3).

Moreover, including the A1- and A2-occupations in the sample shows that while the A2-occupations seem not to be significantly affected (fourth column), the probability of entering self-employment is 0.69 percentage points higher for the A1-occupations. Note the large significant coefficient of the interaction of the EU dummy and the post-policy period, underlin-

ing the importance of controlling for the enlargements of the EU. This coefficient shows that the 2004 enlargement of the EU raised the probability of entry by 1.52 (1.22) percentage points according to column three (four). In Sect. 5.3, I demonstrate that the principal results remain unchanged after all non- German craftsmen are excluded from the sample.

Table 4.2.10 presents the same specifications as used in Table 4.2.3, employing logit models. The estimates tell a consistent story: the signs of the interaction terms are the same across models, and, apart from the coefficient of the interaction between the post-policy dummy and the indicator for A2-vocations and between the post-policy and the EU dummies, the same interactions are statistically significant. Here, the functional form might help to identify the coefficients of the treatment interactions more precisely, whereas the coefficient of the post-policy period's interaction with the EU dummy becomes insignificant at the 10 % level in column four of Table 4.2.10.

While entry probabilities increased, the reform may have raised exit probabilities in the same way. This finding would be consistent with the view that a major portion of new entrepreneurs in the post-policy period use fly-by-night tactics, i.e., they set up a company, do business for a short period and then disappear suddenly. However, the results reported in column five of Table 4.2.10 suggest rather that the policy change generated quite a sustainable number of start-ups. The negative, though highly insignificant point estimates for the interaction terms of the B1-occupations point to an interpretation that exit probabilities remained constant or may even have fallen in the post-policy period. For the A1 and the A2 group, the coefficients are negative and insignificant as well. Similarly, the corresponding LPM estimates shown in column five of Table 4.2.3 are insignificant throughout.

Higher entry probabilities and roughly steady exit probabilities would imply that the stock of self-employed craftsmen should be higher after the reform. And indeed, the last column of Table 4.2.10 presents estimates that are in line with the earlier findings. The interaction term of being self-employed has a significant positive coefficient for both the B1- and A1-vocations, the coefficient for the A2 group is also positive, though insignificant. The results of the LPM presented in column six of Table 4.2.3 are again consistent with the logit estimates.

Table 4.2.3: Estimation results of self-employment state and transition probabilities

	LPM Entry	LPM entry	LPM entry	LPM exit	LPM self-employed
*dB*1 × *dPost*	0.0079*	0.0070*	0.0077*	−0.0285	0.0227*
	(0.0043)	(0.0042)	(0.0041)	(0.0240)	(0.0118)
*dA*1 × *dPost*			0.0069*	−0.0103	0.0266**
			(0.0039)	(0.0145)	(0.0101)
*dA*2 × *dPost*			0.0055	−0.0067	0.0161
			(0.0038)	(0.0136)	(0.0110)
dEU × *dPost*		0.0152**	0.0122**	−0.0585	0.0539***
		(0.0060)	(0.0052)	(0.0395)	(0.0164)
*dB*1	−0.0161*	−0.0012	−0.0019	0.0330***	0.0094
	(0.0081)	(0.0058)	(0.0050)	(0.0116)	(0.0167)
*dA*1			0.0060	−0.2727***	0.0407***
			(0.0038)	(0.0137)	(0.0063)
*d A*2			−0.0303***	0.0758***	0.0133
			(0.0038)	(0.0106)	(0.0105)
dPost	−0.0036	−0.0005	−0.0018	−0.0182	−0.0083
	(0.0037)	(0.0038)	(0.0036)	(0.0123)	(0.0088)
dEU		0.0007	0.0034	0.0039	0.0200*
		(0.0038)	(0.0036)	(0.0449)	(0.0106)
Constant	0.0280***	0.0128	0.0337***	0.7161***	−0.2571***
	(0.0066)	(0.0091)	(0.0083)	(0.0420)	(0.0470)
Year dummies		yes	yes	yes	yes
Occ. Dummies		yes	yes	yes	yes
Branch dummies		yes	yes	yes	yes
Controls		yes	yes	yes	yes
Adj-R^2	<0.01	0.03	0.02	0.08	0.26
Observations	64,842	64,842	154,940	17,211	196,669

Notes: Robust standard errors, clustered by occupation, are given in parentheses below the coefficients of the linear probability model (LPM). Controls included are age and its square, and dummy variables indicating gender, type of secondary schooling and professional qualification, nationality, region of residence, the size of the respondent's city of residence, marital status, number of dependent children, citizenship of foreigners in an EU member state and its interaction with the post-policy period. Moreover, year dummies for 2003, 2004, 2006, 2007, 2008, and 2009, and indicators for the branch of craftsmanship, for the occupation, as well as a constant are included. 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 (2002–2009)

5.1 Treatment effects on transition probabilities

To ascertain the quantitative effect of the amendment on the probability of entry and exit, I first predict the respective probabilities of a person with average characteristics before and after the reform, using the estimates from the preferred logit models reported in Table 4.2.10. Having obtained these, in a second step I calculate their differences. For the entry probabilities the results are reported in Table 4.2.4. The expected probabilities for each of the three treatment groups and for the control group before the reform are shown in columns two to five of the first row, with their standard errors below. The same figure for the period after the reform is shown in columns two to five of the third row. The last three columns of row one and three report the differences in the expected probabilities of each of the treatment groups and the control group before and after the reform. Columns two to five of the last two rows in the upper panel present the differences in the same occupational group before and after the reform along with their standard errors. Finally, the last three columns show the difference in these differences, i.e., the cross differences.

The lower panel shows how the counter-factual cross differences are obtained (see Puhani 2012). While the row displaying the expected probabilities before the reform is identical to the corresponding row in the upper panel, the expected probabilities for the post-reform period are predicted to constrain the reform's effect to zero. Then, at the bottom of the table, the average treatment effects on the treated, i.e., the differences in the actual cross differences from the upper panel and the counter-factual cross differences from the lower panel, are reported, in both absolute and relative terms.

The first thing that leaps out is that the probability of engaging in entrepreneurship for individuals of the B1 group is substantially lower than that of the other occupational groups before the reform. From this comparably lower level, the entry probability resulting from the reform increased by 0.15 % points. This economically relevant effect is also statistically significant, with a standard error of 0.04 (p value <0.01). The probability of entering self-employment would have been 0.40−0.15 = 0.25 in the hypothetical situation without a reform. This shows that the entry probability has been increased dramatically with the reform; its relative effect amounts to 60.00 %.

Effects of this kind are found in the A1- and A2-professions, too. The former group experienced an increase in the probability of entry of 0.56

Table 4.2.4: Probabilities of entry into self-employment (in %): difference-in-differences

	*B*1	*A*1	*A*2	*AC*	*ΔB*1	*ΔA*1	*ΔA*2
Panel A							
Before reform 2004	0.25***	1.86***	2.38***	3.73****	−3.48***	−1.88***	−1.35***
	(0.02)	(0.16)	(0.20)	(0.49)	(0.50)	(0.45)	(0.42)
After reform 2004	0.40***	2.39***	3.01***	3.68***	−3.28***	−1.29***	−0.67***
	(0.02)	(0.12)	(0.16)	(0.30)	(0.31)	(0.22)	(0.19)
Δ Between after and before reform 2004	0.14***	0.53***	0.63***	−0.06	0.20	0.59	0.69
	(0.02)	(0.18)	(0.21)	(0.48)	(0.48)	(0.47)	(0.49)
Panel B							
Before reform 2004	0.25***	1.86***	2.38***	3.73***	−3.48***	−1.88***	−1.35***
	(0.02)	(0.16)	(0.20)	(0.49)	(0.50)	(0.45)	(0.42)
After reform 2004	0.25***	1.83***	2.35***	3.68***	−3.43***	−1.85***	−1.33***
	(0.04)	(0.25)	(0.33)	(0.30)	(0.30)	(0.30)	(0.29)
Δ Between after and before reform 2004	0.00	−0.03	−0.04	−0.06	0.05	0.03	0.02
	(0.03)	(0.24)	(0.31)	(0.48)	(0.45)	(0.24)	(0.17)
Panel C							
Difference-in-differences					0.15***	0.56**	0.67**
					(0.04)	(0.26)	(0.34)
Relative difference-in-differences					60.00	30.60	28.63

Notes: Panel A shows the expected probabilities for the treatment groups (B1, A1, A2) and for the control group (AC) of a person with average characteristics before and after the reform, rounded to two decimal places. Moreover, it depicts the differences in the expected probabilities and the difference in these differences, i.e., the cross differences. The next part of the table shows how the counter-factual cross differences are obtained using the expected probabilities for the post-reform period, which result when the reform's effects are restricted to zero. Panel C reports the ATT, i.e., the differences in these cross differences. The relative differences in differences are computed, respectively, as the fraction of the treatment effect and the expected probability in the post-policy period subtracted by the treatment effect. The same calculation, based on the averages of the respective probabilities among actual persons in the data instead of the expected probabilities of a person with average characteristics, yields similar results and is available upon request Cluster (occupation) robust standard errors calculated by the delta method are in parentheses.

Asterisks (*/**/***) denote significance at the 10 %/5 %/1 % level

Source: Own calculations based on the scientific use file of the German microcensus (2002–2009)

percentage points. This increase is significantly different from zero, with a standard error of 0.26 (p value 0.03). Consequently, this suggests that the opportunity to start a business without the Meister certificate provided by

the Altgesellen rule has been used extensively in this group. The results further show that the craftsmen in A2-occupations responded to the reduction and partial exemption of the entry barrier with an increase of 0.67 percentage points, which is significant with a standard error of 0.34 (p value 0.05). In relative terms, the reform increased the entry probability of the A1 group by 30.60 %, while for A2-occupations, the entry probability was 28.63 % higher than the hypothetical situation without the reform.

How sustainable are these entries? In Table 4.2.5, I present results that support the hypothesis that the amendment of the HwO did not significantly alter the probability of exit from self-employment. The reform's effect for the B1-occupations is −1.77 percentage points, with a standard error of 3.19. This negative effect is insignificant (p value 0.58). Similarly, the effect of −0.22 percentage points for A1-vocations is highly insignificant, with a standard error of 0.38 (p value 0.57). Thus, more sustainable business entries could be established after the deregulation. The results suggest that this is due to the reform, and support the findings in Prantl (2012) that this entry regulation suppressed long-living entrants. For A2-occupations, the treatment effect of −0.11, though insignificant with a standard error of 0.37 (p value 0.76), points to a rather small decrease in the exit rate caused by the amendment. In fact, the point estimate is even positive once 2009 is excluded from the sample. One reason for this could be that in this group fly-by-night strategies might be more common. These, in turn, could be encouraged by the combination of the Altgesellen rule and the partial exemption for small businesses. For instance, splitting a firm up into one that runs the main business and another that serves as an ancillary business makes it easy to once more absorb the smaller one when it becomes convenient. However, on top of the fact that none of the effects on the exit probabilities is significant, the relative effects are rather small using the sample up to 2006 or 2008. Including 2009, they are −17.00, 20.37, and −11.83 % for the B1-, A1-, and A2-vocations, respectively.

5.2 Treatment effects on self-employment probabilities

As discussed above, the higher entry rates, together with constant exit rates, should raise the stock of self-employed persons. In fact, Table 4.2.6 shows that after accounting for the counter-factual situation without the reform, a person with average characteristics in a B1-occupation is 0.41 percentage points more likely to engage in entrepreneurship. This effect is

Table 4.2.5: Probabilities of exit from self-employment (in %): difference-in-differences

	*B*1	*A*1	*A*2	*AC*	*ΔB*1	*ΔA*1	*ΔA*2
Panel A							
Before reform 2004	19.69*** (3.23)	2.24*** (0.30)	1.94*** (0.29)	0.67*** (0.16)	19.02*** (3.21)	1.57*** (0.31)	1.27*** (0.31)
After reform 2004	8.64*** (0.92)	0.86*** (0.07)	0.82*** (0.10)	0.32*** (0.04)	8.33*** (0.93)	0.54*** (0.08)	0.50*** (0.10)
Δ Between after and before reform 2004	−11.05*** (2.92)	−1.38*** (0.36)	−1.13*** (0.34)	−0.35* (0.19)	−10.70*** (2.88)	−1.03*** (0.37)	−0.78** (0.38)
Panel B							
Before reform 2004	19.69*** (3.23)	2.24*** (0.30)	1.94*** (0.29)	0.67*** (0.16)	19.02*** (3.21)	1.57*** (0.31)	1.27*** (0.31)
After reform 2004	10.41*** (3.31)	1.07*** (0.36)	0.93*** (0.35)	0.32*** (0.04)	10.09*** (3.28)	0.75** (0.33)	0.61** (0.31)
Δ Between after and before reform 2004	−9.28** (3.81)	−1.16*** (0.43)	−1.01*** (0.36)	−0.35* (0.19)	−8.93** (3.64)	−0.81*** (0.26)	−0.66*** (0.19)
Panel C							
Difference-in-differences					−1.77 (3.19)	−0.22 (0.38)	−0.11 (0.37)
Relative difference-in-differences					−17.00	−20.37	−11.83

Notes: Panel A shows the expected probabilities for the treatment groups (B1, A1, A2) and for the control group (AC) of a person with average characteristics before and after the reform rounded to two decimal places. Moreover, it depicts the differences in the expected probabilities and the difference in these differences, i.e., the cross differences. The next part of the table shows how the counter-factual cross differences are obtained using the expected probabilities for the post-reform period, which result when the reform's effects are restricted to zero. The Panel C reports the ATT, i.e., the differences in these cross differences. The relative differences in differences are computed, respectively, as the fraction of the treatment effect and the expected probability in the post-policy period subtracted by the treatment effect. The same calculation, based on the averages of the respective probabilities among actual persons in the data instead of the expected probabilities of a person with average characteristics, yields similar results and is available upon request Cluster (occupation) robust standard errors calculated by the delta method are in parentheses.

Asterisks (*/**/***) denote significance at the 10 %/5 %/1 % level

Source: Own calculations based on the scientific use file of the German microcensus (2002–2009)

significant, with a standard error of 0.11 (p value <0.01). The effect on the A1-occupations is larger. The probability of being self-employed in-

creased significantly by 2.80 percentage points, with a standard error of 1.08 (p value 0.01). A more flexible specification reported in Table 4.2.12 shows that this large effect is driven mainly by the years 2007, 2008 and 2009. Given that these years are relatively far from the date of the amendment to the HwO, one should be careful to attribute this effect to the reform. Still, a marginally insignificant effect of 1.23 percentage points (p value 0.89) is observed when the years 2007, 2008 and 2009 are excluded (cf. Rostam-Afschar 2010). Further, the treatment effect for the A2-vocations including all years is 2.10 percentage points. This effect does not achieve statistical significance at the 10 % level with a standard error of 1.42 (p value 0.14).

Note that the probability of being self-employed is substantially smaller for the B1-vocations in the first place. Therefore, the relative effect of 41.41 % is the largest compared to the other groups of craftsmen. For the A1-vocations the relative effect amounts to 20.59 % and to 14.21 % for the A2-professions.

5.3 Heterogeneous treatment effects

Who are these new entrepreneurs in craftsmanship? In this section, I take a closer look at the heterogeneity of treatment effects. This helps to determine individual subgroups within the treatment groups on which the reform had the greatest impact. Individuals, who are disadvantaged in terms of labor market opportunities, such as craftsmen with- out any professional qualification and female craftsmen, might see self-employment as a way out of unemployment (cf. Caliendo and Künn 2011). From Table 4.2.2, we know that treatment group B1, which ultimately showed the strongest relative increase in the post-policy period, comprises more craftsmen without qualification, as well as more female craftsmen, compared with the other treatment groups. Thus, I expect the effects of the policy change to be highest for craftsmen with the above-mentioned characteristics in the B1 group.

If the higher entries documented previously for the A1- and the A2-occupations reflect the effects of the Altgesellen rule, this would be the result of more Geselle- qualified craftsmen engaging in entrepreneurship. Thus, I expect that the largest effect for the groups of A1- and the A2-vocations will be observed for the subsample with this level of professional qualification.

Table 4.2.6: Probabilities of being self-employed (in %): difference-in-differences

	B1	A1	A2	AC	$\Delta B1$	$\Delta A1$	$\Delta A2$
Panel A							
Before reform 2004	1.04***	14.21***	15.44***	22.62***	−21.58***	−8.41***	−7.18***
	(0.07)	(0.71)	(0.93)	(1.99)	(1.98)	(1.59)	(1.47)
After reform 2004	1.40***	16.40***	16.88***	21.73***	−20.33***	−5.34***	−4.85***
	(0.06)	(0.70)	(0.95)	(1.99)	(1.98)	(1.43)	(1.41)
Δ Between after and before reform 2004	0.36***	2.19***	1.44	−0.89	1.25	3.08**	2.33
	(0.09)	(0.76)	(1.22)	(1.47)	(1.46)	(1.48)	(1.69)
Panel B							
Before reform 2004	1.04***	14.21***	15.44***	22.62***	−21.58***	−8.41***	−7.18***
	(0.07)	(0.71)	(0.93)	(1.99)	(1.98)	(1.59)	(1.47)
After reform 2004	0.99***	13.59***	14.78***	21.73***	−20.74***	−8.14***	−6.95***
	(0.10)	(1.15)	(1.35)	(1.99)	(1.95)	(1.44)	(1.32)
Δ Between after and before reform 2004	−0.05	−0.62	−0.66	−0.89	0.84	0.27	0.23
	(0.08)	(1.00)	(1.07)	(1.47)	(1.38)	(0.46)	(0.39)
Panel C							
Difference-in-differences					0.41***	2.80***	2.10
					(0.11)	(1.08)	(1.42)
Relative difference-in-differences					41.41	20.59	14.21

Notes: Panel A shows the expected probabilities for the treatment groups (B1, A1, A2) and for the control group (AC) of a person with average characteristics before and after the reform rounded to two digits after the decimal point. Moreover, it depicts the differences in the expected probabilities and the difference in these differences, i.e., the cross differences. The next part of the table shows how the counter-factual cross differences are obtained using the expected probabilities for the post-reform period, which result when the reform's effects are restricted to zero. The Panel C reports the ATT, i.e., the differences in these cross differences. The relative differences in differences are computed, respectively, as the fraction of the treatment effect and the expected probability in the post-policy period subtracted by the treatment effect. The same calculation, based on the averages of the respective probabilities among actual persons in the data instead of the expected probabilities of a person with average characteristics, yields similar results and is available upon request.

Cluster (occupation) robust standard errors calculated by the delta method are in parentheses.

Asterisks (*/**/***) denote significance at the 10 %/5 %/1 % level

Source: Own calculations based on the scientific use file of the German microcensus (2002–2009)

Moreover, I split the sample by nationality and by indication of having received public payments to show that the effects of the amendment to the

HwO are not distorted by the effects of other policies that potentially affect entrepreneurs.

Table 4.2.7 shows the results of repeating the logit estimations from the main analysis for different subsamples, and then obtaining the absolute and the relative treatment effects. The first two columns present findings when the sample is restricted to German craftsmen and to craftsmen who indicated having not received substantial public payments (SPP). Apparently, for both subsamples the estimated coefficients are almost always slightly smaller compared with the overall results. This is true for the probabilities of entering self-employment and the probabilities of being self-employed. The probabilities of exit from self-employment are again insignificant (not reported, available on request). As the magnitudes and the significance of the effects are roughly the same, I conclude that the main results are not confounded by the enlargements of the EU or by subsidies for entrepreneurs.

The next two columns display the treatment effects for female and male craftsmen. Surprisingly, the reform turns out not to have been effective for the entry probability of female craftsmen. Instead, the effects on the probabilities of entering self-employment are all positive and significant for male craftsmen.

Moreover, the probabilities of exit are again highly insignificant for female and male craftsmen of all vocational groups apart from females working in B1-occupations; their treatment effect does not fail significance at the 10 % level (p value 0.07). Here, the results indicate that the reform decreased the probabilities of exit from self-employment by 7.24 percentage points with a standard error of 3.98 implying a substantial relative reduction, namely of 58.45 %. This means that a reduction of exits, together with a constant entry rate, increased the stock of female craftsmen. Indeed, the probability of being self-employed seems to be higher for female B1-craftsmen after the reform. This fact is intriguing, since the reform should affect entries as it deregulates entry barriers but not exits. However, this result could stem from indirect effects of the deregulation, as the reform changed the competitive environment. To investigate this further is left to future research.

Turning to the effects on the share of self-employed in the bottom part of Table 4.2.7, a different pattern is apparent for both sexes. For male craftsmen from the A2 group, the increases in the entry probabilities are not accompanied by a significant rise in the probabilities of being self-employed. For females in this group, the entry probability seems to have been

Table 4.2.7: Treatment effects on entry into self-employment and on the share of self-employed for subgroups (in %): difference-in-differences

Sample	German	Unsubsi-dized	Female	Male	No quali-fication	Geselle	Meister
Treatment effects on entry into self-employment							
DID*B*1	0.13***	0.13***	−0.01	0.36***	0.23***	0.16***	0.07
	(0.03)	(0.03)	(0.06)	(0.12)	(0.06)	(0.05)	(0.46)
Relative DID*B*1	57.90	68.18	−8.16	55.92	806.97	78.09	4.61
DID*A*1	0.57**	0.64***	−1.34	0.62***	1.43*	0.81***	−2.33
	(0.26)	(0.21)	(1.08)	(0.20)	(0.79)	(0.21)	(1.51)
Relative DID*A*1	32.25	43.27	−52.10	57.46	354.18	93.87	−30.87
DID*A*2	0.66**	0.60**	0.00	0.57**	6.40***	0.89***	−1.87
	(0.33)	(0.25)	(0.01)	(0.26)	(2.44)	(0.33)	(1.49)
Relative DID*A*2	30.01	32.38	0.00	39.23	573.94	71.37	−26.56
Treatment effects on the share of self-employed							
DID*B*1	0.34***	0.34***	0.56***	0.51**	0.50***	0.32	0.88
	(0.10)	(0.10)	(0.16)	(0.24)	(0.12)	(0.20)	(0.55)
Relative DID*B*1	35.70	37.71	114.38	24.59	77.55	29.79	18.39
DID*A*1	2.64**	2.48**	7.33**	1.63**	2.29	2.20**	2.61
	(1.05)	(1.14)	(3.55)	(0.65)	(1.77)	(1.12)	(2.33)
Relative DID*A*1	18.71	19.55	53.66	13.84	29.52	28.20	4.49
DID*A*2	1.73	1.82	11.33	0.87	3.22	1.86	3.05
	(1.37)	(1.41)	(9.42)	(0.86)	(2.58)	(1.37)	(2.62)
Relative DID*A*2	11.58	13.87	44.43	7.00	32.22	21.14	6.04

Notes: The treatment effects are based on the expected probabilities for a person with average characteristics. The relative differences in differences are computed as the fraction of the treatment effect and the expected probability in the post-policy period, subtracted by the treatment effect, respectively.

Cluster (occupation) robust standard errors calculated by the delta method are in parentheses.

Asterisks (*/**/***) denote that a difference-in-differences is significantly different from zero at the 10 %/5 %/1 % level

Source: Own calculations based on the scientific use file of the German microcensus (2002–2009)

unaffected, while the self-employment probability seems to have been increased, though not significantly. This could be due to increased exit rates although the positive point estimates are insignificant as well and very small. However, for females as well as for males in the B1- and A1-occupations, the treatment effects on the share of self-employed achieve significance at the 10 % level (p values <0.01, 0.04, 0.04 and 0.01). Thus, while the evidence is not strong that both female in all and male craftsmen in the

A2 group experienced the intended effects of the reform, the results suggest that the increases reported in Tables 4.2.4 and 4.2.6 for the B1- and A1-occupations stem largely from male craftsmen engaging more often in entrepreneurship.

The last three columns show the results obtained by splitting up the sample by professional qualification. The results show a clear picture: The amendment to the HwO had a positive effect on the entry probabilities of untrained craftsmen across all three treatment groups. These increases in entries also raised the probability of being self-employed for each of the groups. This implies that these businesses survived for some time. While the latter effects are insignificant for the A1- and A2-vocations, they are highly significant for the B1-occupations.

Therefore, the expectation that craftsmen without professional qualification entered entrepreneurship more often in the B1-occupations is supported by the results.

Furthermore, the reform encouraged craftsmen who hold a Geselle degree to enter entrepreneurship. This was the purpose of the Altgesellen rule, and the objectives of this policy seem to have been accomplished. However, the fact that these entries could not increase the probability of being self-employed significantly, except for A1-vocations, favors the view that some of these new entrepreneurs used fly-by-night strategies. For example, a Meister could ask one of his Altgesellen to set up an ancillary business to drive a rival out of the market. The last column shows that for Meister craftsmen the reform had, as expected, no significant effect at all.

5.4 Specification and sensitivity tests

To assess the validity of the assumptions on which the DID approach is based, and to gage 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 estimation sample, the definition of variables, and the specification.

Column one in Table 4.2.11 shows the results of estimating the same specification as in the main analysis, in which the year 2001 is included. Obviously, the size and significance of the estimates are similar to those reported in Table 4.2.10. Hence, using this sample does not distort the main results. However, I decided to exclude 2001 from the sample be-

cause a "placebo test" discussed below indicates significant coefficients of the interaction between a placebo reform dummy and the A1- and A2-vocation dummies, respectively.

In columns two and three, I display the results when the year 2004 is omitted from the sample and when it is defined as belonging to the post-policy period, respectively. Recall that the post-policy period was defined as being from 2005 to 2009 in the main analysis. I do this because up until 2005 the last week of April was usually the reference week of the survey, and the amendment to the HwO came into effect at the beginning of 2004. Apparently, dropping the year 2004 does not change the results a great deal, while defining 2004 as part of the post-policy period reduces the estimates somewhat. This shows that individuals needed some time to adjust to the new policy, as argued above.

Next, in columns four to six, I scrutinized whether influences other than the actual treatment of 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 are 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. 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.

In column four, the coefficients for the interaction terms turn out to be significant for the B1- and A1-occupations when the estimation sample includes the year 2001— which is why the main analysis was based on the sample from 2002 to 2009. The interaction coefficients in columns five and six 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.

Furthermore, I examine the assumption of common trends more explicitly by replacing the post-policy period dummies in the interactions with time dummies and all interactions involving the post-policy period dummy with interactions using time dummies instead. Correspondingly, I included interactions between the branch dummies and time dummies. The results (see Table 4.2.12) are in line with the prior findings shown in Table

4.2.10 that provided evidence that the probability of being self-employed increased significantly for B1-occupations; the coefficients of the interactions of the B1-dummy with year dummies from the post-policy period are individually positive, significant and of similar magnitude throughout. Interestingly, the coefficients for the interactions in the A1-occupations are increasing in size and statistical significance over time. This suggests that the effects of the reform presented in Table 4.2.6 emerged only after some time for the A1-occupations. This is consistent with the picture in Figs. 4.2.1 and 4.2.2 where the lines representing the A1-occupations change slower than those of the B1-occupations.

The fact that only very few entries are observed compared to non-entries could lead to a different problem highlighted in King and Zeng (2001). This study shows that applying the standard logistic regression potentially leads to significant distortion of the results as the finite sample bias is amplified by the rare occurrence of events. This should not be a problem because the sample size used should be sufficiently large— for the B1-occupations more than 700 entries are observed. Indeed, the differences between the standard logit and a rare events logit model are very small (available on request); the general results of the main analysis remain unchanged.

In a further robustness check which is available on request, I collapse the sample by occupation and year. Then, I calculate the differences in differences as in the analysis based on individual data. The coefficient of the interaction between the B1-dummy and the post-policy period dummy of the same specification as in column four of Table 4.2.3 changes from 0.0077 (0.0041) to 0.0073 (0.0054). Running the specification of column six where the rate of self-employment is the dependent variable, the coefficient of the same interaction has a standard error of 0.0118 on the individual and of 0.0146 on the occupational level. The point estimate is 0.0179 on the occupational level.

Moreover, I run regressions restricting the sample to individuals above various ages to see whether conditioning on vocational training could bias the results. The idea is that craftsmen who achieved their qualification objectives long ago do not condition their decision to obtain a Meister degree on the desire to be an entrepreneur. Otherwise they would have had enough time to obtain the Meister degree if they had wanted to. On average, since 2002 craftsmen have obtained the Meister degree at an age below 30, according to the Chambers of Crafts and Trade. Less than 7 % of the craftsmen that passed the Meister exam were above 40 years old in

2006/7. Restricting the sample to craftsmen older than 30, 35, and 40, I obtained treatment effects that are significant but slightly larger than those obtained from the entire sample. Complete estimation results are available on request.

6 Summary and conclusions

In pursuit of an answer to how the amendment to the HwO in 2004 influenced entrepreneurs in German craftsmanship, this paper evaluates the effect of this reform on the probability of entering self-employment and of exiting from self-employment. Evidence is provided concerning how the probability of being self-employed changed as a result of the reform for three treatment groups that experienced different degrees of deregulation. Among other modifications, these legislative changes exempted the group of B1-craftsmen altogether from the requirement of passing a Meister examination for admission to entrepreneurship, while for the A1- and A2-occupations the entry requirement has been reduced; a lower level of vocational training has been required since the reform. This is known as the Altgesellen rule. Moreover, the amendment exempted a portion of the A1-occupations from the Altgesellen rule under the condition of limiting business to simple activities that frequently take the opportunity to establish small businesses. This defines the A2 group. Apart from these deregulations, the HwO also provides a natural comparison group, because for some professions, the entry requirement remained mandatory. According to the legislation, four distinct occupational groups can be identified in the data from the German microcensus from 2002 to 2009. These groups are exploited within this setting in a natural experiment. The results of a DID analysis provide evidence that the probability of being self- employed increased significantly with the amendment to the HwO among B1- and A1-occupations, while the positive effect fails to achieve significance for the A2- vocations. The strongest relative increase amounts to more than 40 %. This occurred in the group of B1-craftsmen that have received the strongest treatment. In A1- and A2-occupations, the results indicate weaker, but still positive relative effects. The analysis shows further that these increases are caused by significant increases to the probabilities of entry across all three groups, whereas the probabilities of exit from self-employment remained virtually unaffected by the policy change.

Two key findings that result from an investigation of heterogeneous treatment effects have important policy implications. First, the findings suggest that the increases in the entry probabilities result from male craftsmen who are significantly more likely to start businesses after the reform in all occupation groups. There is weaker evidence that, for these groups, the probabilities of being self-employed also increased after the reform. The results for female craftsmen are less clear: the entry probabilities seem not to have been affected at all, while the results for the self-employment probabilities point to a positive effect.

Second, untrained workers, mainly among the B1- and A2-vocations, have a significantly higher probability of starting a business after the reform. Consequently, the probability of being self-employed is higher for this group, which is disadvantaged in the labor market. Craftsmen of all occupations that completed an apprenticeship also have engaged more in entrepreneurship since the reform, which was the intended effect of the Altgesellen rule. The increase in entries seems to have led to a greater probability of being self-employed for craftsmen trained in an apprenticeship, though the evidence is weak.

Interpreting these results, it is important to bear in mind that these results focus only on engagement in entrepreneurship, and do not replace an evaluation of the reform in terms of its welfare effects on the German economy.

Appendix

Table 4.2.8: Stock of businesses at the end of the year

	A	*B*1	*B*2	Total
2002	590,146	76,044	177,471	843,661
2003	587,762	74,940	183,886	846,588
2004	595,309	102,568	189,216	887,093
2005	600,287	129,591	192,805	922,683
2006	603,443	149,981	193,474	946,898
2007	603,757	166,015	191,434	961,206
2008	602,605	175,557	188,526	966,688
2009	Data not available heretofore			

Notes: Müller (2006) argues that the actual stock of businesses is about 15 % lower than the reported stock due to registered but non-active businesses.

Source: Own calculations based on Müller (2006) and data provided by the German Confederation of Skilled Crafts

Table 4.2.9: Self-employment rates in treatment groups and control group by year

	*B*1	*A*1	*A*2	*AC*	WP
2002	7.88	15.20	12.41	19.26	11.17
2003	7.54	15.80	12.92	19.68	11.56
2004	8.20	16.98	13.38	20.91	12.00
2005	9.32	17.22	13.96	20.81	12.44
2006	9.48	17.39	14.20	18.49	12.27
2007	9.73	17.76	13.83	20.46	12.11
2008	9.78	17.24	13.69	19.30	11.95
2009	9.87	18.66	14.51	18.13	12.13

Percentage share of self-employed among B1, A1, A2, and AC occupations and percentage share of self- employed among working persons (WP)

Source: Own calculations based on the scientific use file of the German microcensus (2002–2009)

Table 4.2.10: Estimation results of self-employment state and transition probabilities

	Logit entry	Logit entry	Logit entry	Logit exit	Logit self-employed
*dB*1 × *dPost*	0.4556***	0.4152***	0.4630***	−0.2057	0.3528***
	(0.1466)	(0.1446)	(0.1440)	(0.3426)	(0.1071)
*d A*1 × *dPost*			0.2719*	−0.2270	0.2205**
			(0.1427)	(0.3657)	(0.0907)
*d A*2 × *dPost*			0.2571*	−0.1331	0.1579
			(0.1440)	(0.4098)	(0.1099)
dEU × *dPost*		1.1145**	0.4527	−0.8180	0.5809**
		(0.5305)	(0.2855)	(0.5496)	(0.2667)
*dB*1	−0.8704*	0.1464	−2.7259***	3.5941***	−3.3255***
	(0.4738)	(0.1935)	(0.1749)	(0.2985)	(0.1234)
*d A*1			−0.7175***	1.2221***	−0.5682***
			(0.1290)	(0.2482)	(0.0852)
*d A*2			−0.4634***	1.0784***	−0.4706***
			(0.1157)	(0.2652)	(0.0795)
dPost	−0.1400	0.1150	−0.0309	−0.7154**	−0.0709
	(0.1310)	(0.1951)	(0.1346)	(0.3469)	(0.0831)
dEU		−0.1224	0.1646	−0.2910	0.4252***
		(0.5657)	(0.2094)	(0.5718)	(0.1453)
Constant	−3.5482***	−3.7692***	−1.4658***	−1.4618	−2.2017***
	(0.2416)	(0.7454)	(0.4525)	(0.8993)	(0.5096)
Year dummies		yes	yes	yes	yes
Occ. dummies		yes	yes	yes	yes
Branch dummies		yes	yes	yes	yes
Controls		yes	yes	yes	yes
Log likelihood	−5,346.12	−4,577.49	−13,667.83	−2,159.01	−54,391.46
Pseudo-R^2	<0.01	0.15	0.10	0.20	0.30
Observations	64,842	64,842	154,940	17,211	196,669

Notes: Robust standard errors, clustered by occupation, are given in parentheses below the coefficients of the logit models. Controls included are age and its square, and dummy variables indicating gender, type of secondary schooling and professional qualification, nationality, region of residence, the size of the respondent's city of residence, marital status, number of dependent children, citizenship of foreigners in an EU member state and its interaction with the post-policy period. Moreover, year dummies for 2003, 2004, 2006, 2007, 2008, and 2009, and indicators for the branch of craftsmanship, for the occupation, as well as a constant are included.

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 (2002–2009)

Table 4.2.11: Timing sensitivity: Logit estimation results of self-employment state probabilities

	Self-employed: timing (2001–2009) I	Self-employed: timing 2004 ropped (2002–2009) II	Self-employed: timing 2004 as post (2002–2009) III	Self-employed: Placebo reform in 2002 (2001–2004) IV	Self-employed: Placebo reform in 2002 (2002–2004) V	Self-employed: Placebo reform in 2003 (2002–2004) VI
*dB*1 × *dPost*	0.4505*** (0.1029)	0.3853*** (0.1055)	0.3173*** (0.0839)	0.2432* (0.1427)	0.0829 (0.1373)	0.1087 (0.1163)
*dA*1 × *dPost*	0.3183*** (0.0895)	0.2533*** (0.0850)	0.2295*** (0.0667)	0.2878** (0.1401)	0.1433 (0.1309)	0.1133 (0.0938)
*dA*2 × *dPost*	0.2139** (0.1079)	0.1818* (0.1105)	0.1476* (0.0879)	0.2032 (0.1414)	0.1370 (0.1397)	0.0788 (0.0924)
dEU × *dPost*	0.6124** (0.2497)	0.6695*** (0.2529)	0.5106** (0.2485)	0.2622 (0.1617)	0.3116* (0.1813)	0.2640 (0.1900)
*dB*1	−1.0689*** (0.2088)	−1.0644*** (0.2093)	−0.9379*** (0.2159)	−1.1287*** (0.2103)	−1.0182*** (0.2296)	−1.0017*** (0.2265)
*dA*1	1.7002*** (0.2000)	1.6872*** (0.2046)	1.7793*** (0.2013)	1.6867*** (0.2032)	1.7667*** (0.2283)	1.8234*** (0.2108)
*dA*2	−0.5579*** (0.0709)	−0.5403*** (0.0841)	−0.4517*** (0.0665)	−0.6524*** (0.0891)	−0.4838*** (0.1110)	−0.4168*** (0.0645)
dPost	−0.0953 (0.0778)	−0.0996 (0.0730)	−0.1750*** (0.0593)	−0.1052 (0.1364)	−0.0492 (0.1261)	−0.0258 (0.0898)
dEU	0.4142*** (0.1370)	0.3323*** (0.1216)	0.5428*** (0.1302)	0.3276** (0.1317)	0.2344 (0.1482)	0.3574*** (0.1243)
Constant	−4.5595*** (0.4784)	−4.4227*** (0.4945)	−4.4666*** (0.4845)	−4.8397*** (0.4515)	−4.8152*** (0.4603)	−4.8564*** (0.4536)
Year dummies	yes	Yes	yes	Yes	yes	yes
Occ. dummies	yes	Yes	yes	Yes	yes	yes
Branch dummies	yes	Yes	yes	Yes	yes	yes
Controls	yes	Yes	yes	Yes	yes	yes
Log likelihood	−60,898.75	−47,945.16	−54,411.65	−25,907.46	−19,434.86	−19,435.57
Pseudo-R^2	0.30	0.30	0.30	0.32	0.31	0.31
Observations	223,241	172,664	196,669	101,709	75,137	75,137

Notes: Robust standard errors clustered by occupation are given in parentheses below logit coefficients. Controls included are age and its square, and 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, citizenship of foreigners in a member state of the European Union and

its interaction with the post-policy period. Moreover, year dummies and indicators for the branch of craftsmanship, for the occupation, and a constant are included.

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 (2001–2009)

Table 4.2.12: Robustness: Logit estimation results of self-employment state probabilities

	Self-employed: *B*1	Self-employed: *A*1	Self-employed: *A*2
dO × *d*2003	−0.0010	0.0875	0.0747
	(0.1596)	(0.1567)	(0.1773)
dO × *d*2004	0.2407	0.1582	0.1383
	(0.1664)	(0.1746)	(0.1942)
dO × *d*2005	0.4233***	0.2287	0.1826
	(0.1608)	(0.1626)	(0.1912)
dO × *d*2006	0.4283**	0.2445	0.1815
	(0.1824)	(0.1762)	(0.1968)
dO × *d*2007	0.4755***	0.3556***	0.2027
	(0.1356)	(0.1324)	(0.1667)
dO × *d*2008	0.4566***	0.3337**	0.1501
	(0.1638)	(0.1401)	(0.1960)
dO × *d*2009	0.5520***	0.4807***	0.3297*
	(0.1957)	(0.1541)	(0.1781)

Notes: Robust standard errors, clustered by occupation, are given in parentheses below logit coefficients. Controls included are age and its square, and dummy variables indicating gender, type of secondary schooling and professional qualification, nationality, region of residence, the size of the respondent's city of residence, marital status, the number of dependent children, citizenship of foreigners in an EU member state and its interaction with the post-policy period. Moreover, year dummies and indicators for the branch of craftsmanship, for the occupation, and a constant are included. The log-likelihood value is −54,322.07, the pseudo-R2 0.3, and the number of observations 196,669.

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 (2002–2009)

Description of key variables

– Entrepreneur: Are you working as self-employed (with or without employees)? This definition includes non-incorporated self-employed as well as incorporated self- employed.

- B1, A1, A2, AC: 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 2009.
- 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 2009.
- SPP: Indicates receiving subsidies for self-employed. After excluding individuals eligible for child benefit, the dummy variable PP is restricted to all recently (assuming start-ups are subsidized for at most three years) self-employed individuals, who earn below 26,076 Euros (close to the 25,000 Euro threshold of the EXGZ) per year and receive public payments.
- EU: Indicates citizenship of foreigners in a member state of the European Union (EU).

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