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Entry Regulation and Entrepreneurship

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Entry Regulation and Entrepreneurship

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A Natural Experiment in German Craftsmanship

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 (2002-2008) 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 somewhat 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 the group of untrained workers.

Keywords Entrepreneurship · Regulation · Firm entry · Natural experiment · Craftsmanship

JEL Classification L51 · J24 · I28 · M13

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1 Introduction

How does entry regulation influence entrepreneurship? In an attempt to answer this question, many different types 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.¹ On the one hand, the public choice theory argues that regulations lead to socially inefficient outcomes, while on the other hand the public interest theory of regulation opposes that regulations serve to cure market failures.

Recent 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 & Papaioannou (2007), and van Stel et al. (2007). Apart from this, sparse evidence based on microdata (cf. Bruhn, 2011; Ardagna & Lusardi, 2010, 2009) adds to the almost unanimous finding that lower entry costs increase entry into (formal) entrepreneurship.

A particularly interesting implementation of regulation is the mandatory qualification in German craftsmanship required by the German Trade and Crafts Code (HwO)² for registration as an entrepreneur. Prantl & Spitz-Oener (2009) and Prantl (2012) explicitly consider the educational entry requirement for craftsmanship to discuss regulatory effects in light of German unification in 1990. However,

¹See Djankov et al. (2002) for a discussion of the theory of regulation.

²This regulation was amended in the context of a series of reforms aimed at the German social system and labor market. See Blanchard & Giavazzi (2003) for a consideration of the interactions between product and labor market regulation.

their results remain limited to a comparison between East and West Germany as their data set exhibits no variation in the regulatory setting.

This study, in contrast, contributes to the literature on entry regulation and entrepreneurship by providing initial evidence of the causal effects of entry regulation by exploiting a change to the HwO as a natural experiment. Dating back to the late nineteenth century, this latter mandatory qualification, called *Meister* (see section 2.1), underwent a dramatic change: the amendment to the HwO in January 2004 decreased the number of occupations in which craftsmen are required to hold a Meister qualification to start a business from 94 to 41. Moreover, the entry requirements for the remaining 41 occupations were relaxed.³

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.

With a focus on entrepreneurship, in addition to credit constraints (e.g., Evans & Jovanovic, 1989; Blanchflower & Oswald, 1998; Hurst & Lusardi, 2004; Fossen, 2011), the entry requirement is regarded as a key impediment to starting a business. For instance, Holtz-Eakin & Rosen (2005) point to the entry require-

³The 57 trades similar to crafts referred to as *B2* occupations below, are not subject of this analysis.

ment as a disincentive for taking up self-employment in German craftsmanship.

Abstracting from its effects on human capital, this regulation imposes monetary and time costs and should therefore reduce the incentive to start a business. From another perspective, however, this regulation can be regarded as a compulsory investment, because it imposes a qualification requirement that may increase expected profits. Thus, the regulatory influence has two aspects: it increases the value of human capital while raising the costs of entrepreneurship.

Still, craftsmen would improve their education even without any regulatory incentives, therefore the effect of entry regulation could be either negative or zero. In a world characterised by asymmetric information, however, unregulated craftsmen may not be honest about the quality of their products. According to Akerlof (1970), the costs of misrepresentation of quality consist of the amount by which the purchaser is cheated and most importantly the loss incurred from driving businesses out of existence. This study suggests that licensing practices would solve these pathologies. The HwO restricts entry but does not restrict who works in an occupation. Thus, the effects of this regulation would be more likely to constrain entrepreneurship.

To shed some light on this hypothesis, I use repeated cross-sections (2002-2008) of German microcensus data. I apply the difference-in-differences approach to estimate the effects for three distinct occupational groups of the policy change on the probabilities of being self-employed, as well as the probability of transitioning into and out of self-employment. This work is connected to the few studies that use microdata to investigate entry regulation and entrepreneurship, and it is one of the rare studies that focuses explicitly on craftsmanship.

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 32%

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 but weaker. The analysis shows further that these increases result 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 affect 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, I describe the institutional framework of the natural experiment and outline the empirical approach used to obtain the results with described data.

2 The Amendment to the German Trade and Crafts Code in 2004 as a Natural Experiment

2.1 Institutional Background

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 compe-

tence 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 that remained regulated by a form of the greater proof of competence, in contrast to those in *B2*-occupations.⁴

The qualification as a Meister craftsman is the highest professional qualification in craftsmanship. To attain it, a person must complete several levels of training and examinations. Having obtained the qualification level, called *Geselle*, a craftsman can continue to a Meister degree. Full-time courses to prepare for the Meister exam take one to three 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 portion, as holding a Meister degree makes the craftsman eligible to train apprentices. Those who have passed the examination are recorded in the *register*; though in rare exceptions, some people may be recorded in the register without a Meister degree.

This was the situation just prior to the amendment to the HwO in 2004. This analysis exploits this reform to assess the causal effects of entry regulation on entrepreneurship. In the next section, I describe the components of this reform, and define treatment groups and a control group accordingly.

⁴When a major amendment to the HwO reduced the number of regular craftsmanship occupations from 127 to 94 in 1998, the entry requirement for *A*-occupations remained untouched.

3 Empirical Specification

3.1 Definition of the Treatment and Control Groups

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 that are subject to different degrees of regulation.⁵ I match 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 can construct four occupational dummies that reflect the different intensities of the treatment as outlined in Table 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 splitting 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. These *B1*-occupations represent the treatment group were 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 remained subject to virtually the same requirements as before the policy change; they had already

⁵This amendment is based on two laws, *the greater amendment to HwO* (Drittes Gesetz zur Änderung der Handwerksordnung und anderer handwerksrechtlicher Vorschriften) and *the small amendment to HwO* (Gesetz zur Änderung der Handwerksordnung und zur Förderung von Kleinunternehmen).

Table 1: The natural experiment

Before	Qualification	After	Qualification
<i>A</i>	(Meister)	<i>AC</i>	(Meister)
<i>A</i>	(Meister)	<i>A1</i>	(Altgeselle)
<i>A</i>	(Meister)	<i>A2</i>	(Altgeselle, no requirement*)
<i>A</i>	(Meister)	<i>B1</i>	(no requirement)
<i>B2</i>	(no requirement)	<i>B2</i>	(no requirement)

Notes: This table describes the minimum level of qualification required 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.

* 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 three months.

needed a mandatory Meister certificate to enter entrepreneurial activities. These vocations serve as the control group⁶. They include chimney sweeps, opticians, 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 six 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 professions 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 three months. This partial exemption of 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 *easyjob*-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

⁶*B2*-occupations are not used as control group because these occupations are not always discriminable in the dataset due to data protection.

and the control group, I describe in the following the development of the level of self-employment and self-employment rates for these groups.

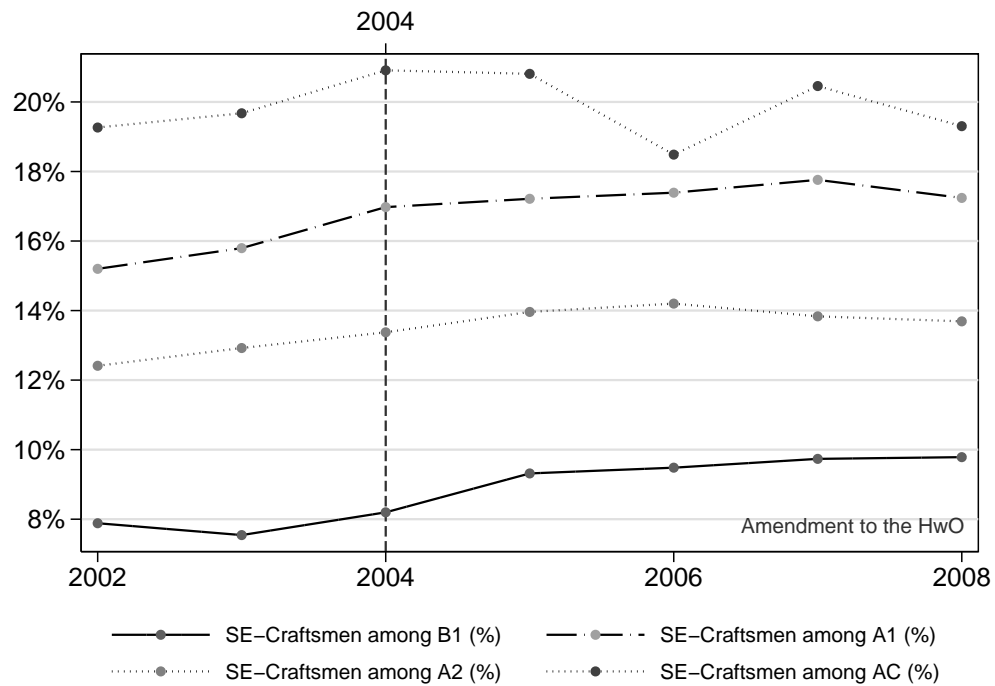
3.2 Trends in Craftsmanship

Between 2002 and 2008, 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 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 monotonic increase, the number of *A2* craftsmen reverts to its pre-policy level. The number of *A1* craftsmen also declines from 2007 to 2008 but nevertheless remains 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 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. 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 section 5.4 for a robustness check that shows that the results are not driven by this

Figure 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 micro-census (2002-2008).



outlier. Below, I describe the empirical methodology used to identify the effects of the reform on self-employment rates.

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 sta-

tus choices across each treatment group both before and after the reform. Then, I 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 difference-in-differences (DID) estimator, and represents the average treatment effects on the treated group (ATT) (e.g. Blundell & Costa Dias, 2009).

I use data from 2002 to 2008 for the three occupational groups (*B1*, *A1*, *A2*) subject to different intensities of regulation changes, as detailed in section 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 rates of each of the three treatment groups before and after the reform with the average self-employment rates of the *AC*-occupation group before and after the reform.

Therefore, during this seven-year period, I have been able to quantify the effects of the reform on the rate 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 self-employment 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 favour 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, hardly changes 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 three years or more in 2004 and 72.80% in 2008. For the other groups of craftsmanship, 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% ran their business for three 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*-occupations⁷ 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-employment in a *B1*-occupation might have been harder for an individual not having obtained the required qualification before the reform. The other way round, a craftsman trained in a *B1*-, or *A*-occupation might have been more likely to change to an occupation from the *B2*-vocations because if he wants to set up a business before the reform.⁸ 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

⁷Although *B2*-occupations are excluded from the analysis when the data set can distinguish them, some of these professions remain in the sample. Because they remain in the same group (e.g., *B1*) over the entire period, according to their time-invariant job definition, their presence in the sample does little harm. In fact, the results do not change if the *B2*-occupations are kept in the sample.

⁸I assume that non-anticipating substitution across the groups is negligible and thus most of the treatment effect is caused by a higher rate of self-employment across groups. This assumption receives support from the observation that the stock of businesses increased from 2003 to 2004 in all three groups reported in Table 8.

and other characteristics to control for the potential for systematic differences in the populations over the two periods. Therefore, it is reasonable to 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 entrepreneurship subsidies.

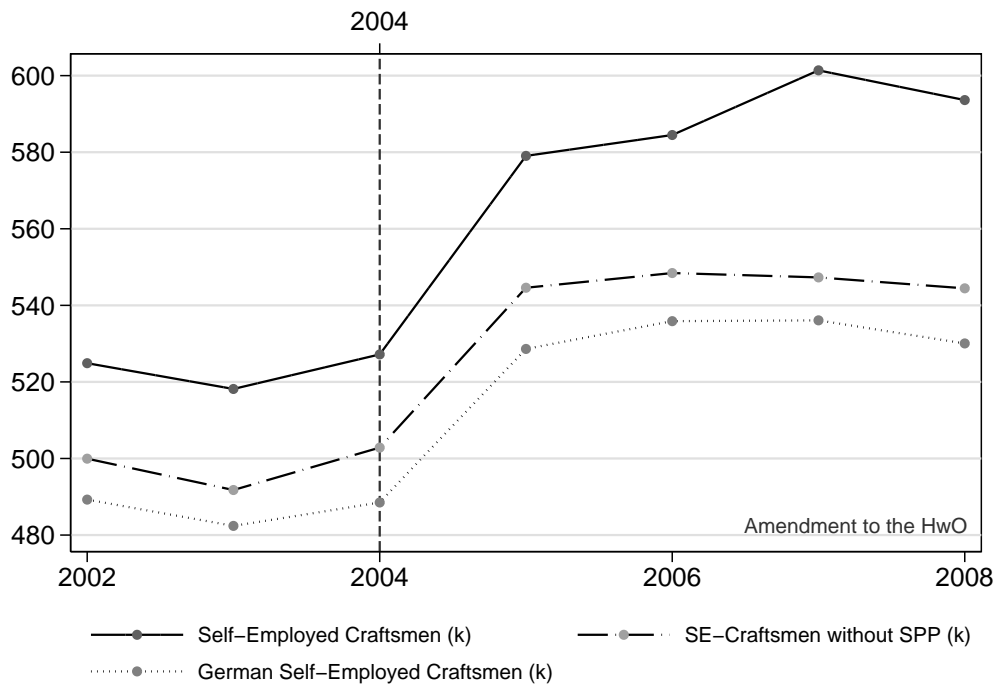
The first relevant enlargement of the EU based on the 2003 Treaty of Accession took place in 2004, when ten countries became new member states. Moreover, the 2007 enlargement of the EU which is based on the 2005 Treaty of Accession saw Bulgaria and Romania join the EU. Although Germany restricted its labor market from workers from these twelve 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.⁹

Other important policy instruments are 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 & Steiner, 2005; Caliendo & Künn, 2011). The years in which each of the programs was adopted and the year of its abolishment is given in the parentheses. According

⁹See (Müller, 2008) for first empirical assessments on how the 2004 enlargement of the EU influenced German craftsmanship.

Figure 3: Craftsmanship and entrepreneurship policies. Total, unsubsidized, and German self-employed craftsmanship in thousands.

Source: Own calculations based on the scientific use file of the German micro-census (2002-2008).



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 startups 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 3 shows three graphs from 2002 to 2008: 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: They equal, for each year, approximately 90% of the stock of businesses reported in Table 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, although 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 section 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 2008, 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 in the linear probability model (LPM). However, I also employed LPMs for all of these specifications (available on request) and the results remain essentially the same.

In a 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 Y_i 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 $Prob(\cdot)$. In the main specification, given as equation (1) below, the regressors $dPost_i, dO_i$, and X_i are included in z_i , where $dPost_i$ is a dummy variable for individuals observed in the post-policy period;¹⁰ $dO_i = dB1_i, dA1_i, dA2_i$ indicates an individual's affiliation to one of the treatment

¹⁰The post-policy period could be defined as the period from 2004 to 2008. 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 2008. Results from a specification where the post-policy period is defined from 2004 to 2008 or 2004 is dropped are

groups; and X is the vector of control variables. The specification includes interaction terms between the respective treatment group indicators and the post-policy dummy. Moreover, δ_0 , δ_ω , β_ω and β_4 , along with $\omega = B1, A1, A2$, represent the respective coefficients or vector of coefficients, and β_0 is a constant.

$$\begin{aligned}
\text{Prob}(Y_i = 1 | dPost_i, dO_i, X_i) &= \frac{1}{1 + e^{-z_i}} \text{ with} \\
z_i &= \beta_0 + \delta_0 dPost_i + \beta_{B1} dB1_i + \beta_{A1} dA1_i + \beta_{A2} dA2_i \\
&+ \delta_{B1} dB1_i \cdot dPost_i + \delta_{A1} dA1_i \cdot dPost_i + \delta_{A2} dA2_i \cdot dPost_i \\
&+ X_i \beta_4.
\end{aligned} \tag{1}$$

In addition to dummies for the years 2003, 2004, 2006, 2007 and 2008, 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 craftsmanship¹¹, and a constant. The included indicator dEU shows the citizenship of foreigners in an EU member state, and is included as well as 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 section 3.4. Controlling for these characteristics is important for two reasons. First,

shown in Table 9. 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 10 and discussed in section 5.4.

¹¹Craftsmanship has seven branches: i) building and construction trades, ii) electrical and metal-working trades, iii) woodwork trades, iv) clothing, textile and leather trades, v) foodstuffs trades, vi) trades related to health and hygiene, including chemical and cleaning trades, and vii) glass, paper, ceramic and other trades.

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 rate. The same population is used in the entry models. Note that the employment status in the previous year, used for the construction of the transition variables, is asked 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. The German microcensus is a mandatory census, which guarantees a low rate of item non-response and ensures that entrepreneurs are adequately represented.

This analysis uses pooled cross-sections of the German microcensus from 2002 to 2008. 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 9 shows that the estimates from the main specification using the years 2001 to 2008 remain similar if 2001 is included. These results and others

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 of 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.

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 (177,434). People whose employment status choice is determined by different factors are also omitted from the sample to avoid distortions. Thus, civil servants (11,989), apprentices (7,846), soldiers (960), conscripts (722), persons in education, or those drafted in the previous year (13,515 and 343, respectively), as well as all remaining non-craftsmen (254,233), are excluded. Moreover, family workers (2,048) 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 subsection shows how the transition variables used in the estimation evolved over time, and describes the characteristics of the

occupational groups.

4.2 Descriptives

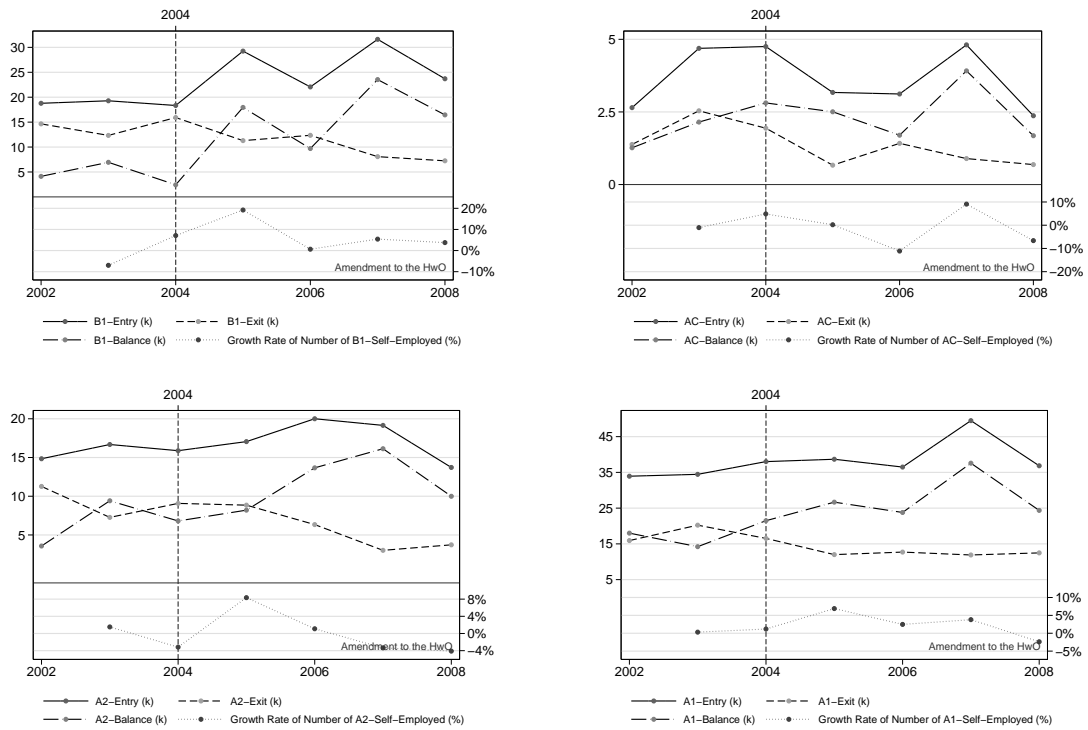
The upper left graph of Figure 4 shows how the number of *B1* entries increased tremendously after 2004, returned to a somewhat lower level in 2006, peaked in 2007 and reverted in 2008, but still remained higher than in the period before the reform. The exits remained constant for a time, before declining in the aftermath of the policy change. Note that the balance (defined as *entry-exit*) exhibits a similar, though less pronounced, path than the number of self-employed craftsmen in Figure 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 a percentage, and the balance shows how large the non-response bias in the transition variables is because both variables should contain the same information. Indeed, in almost all of the graphs in figure 4 the growth rate roughly resembles the pattern of the balance.

The upper right graph in Figure 4 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 to 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 refer to Rostam-Afschar (2010).

In the lower left graph of Figure 4, 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: Entries into and exits from self-employment and their difference among B1 (upper left), AC (upper right), A2 (lower left), and A1 (lower right). Left ordinates: Number in thousands, Right: Growth rate in percent.

Source: Own calculations based on the scientific use file of the German micro-census (2002-2008).



The series of transitional variables for *A1* occupations, depicted in the lower right part of Figure 4, 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.

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 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 2 as weighted averages from the pooled cross-sections from both the pre-policy period (2002-2004) and from the post-policy period (2005-2008). 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.

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 dO_i . 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 largest increase is documented for the *A1*-occupations, one should be cautious about attributing this to the effects of

Table 2: Weighted averages by treatment and control groups in pre- and post-reform (2002-2004;2005-2008) samples

	B1		A1		A2		AC	
	Pre	Post	Pre	Post	Pre	Post	Pre	Post
Self-employed (%)	7.87	9.62	15.96	17.42	12.89	13.93	19.94	19.78
Female (%)	58.81	58.79	16.90	17.56	3.08	2.83	41.24	43.78
Age (a)	42.77	43.56	38.97	39.67	39.46	39.95	38.99	39.90
East (%)	16.47	17.59	21.50	21.62	23.44	23.34	17.71	16.97
Nationality								
German (%)	80.89	80.79	90.17	90.72	90.84	90.48	95.95	95.82
EU (%)	4.64	6.23	3.57	3.94	3.04	3.94	1.80	2.08
Non-EU (%)	14.47	12.98	6.26	5.34	6.12	5.58	2.25	2.10
Professional qualification								
University (%)	1.09	1.33	0.77	1.00	0.25	0.42	1.04	0.72
UAS ^a (%)	0.94	1.09	1.23	1.30	0.53	0.54	1.52	1.69
Meister ^b (%)	5.64	5.18	17.99	17.49	16.37	16.97	27.23	28.56
Geselle ^c (%)	54.32	58.99	65.67	70.19	69.96	72.91	62.46	65.15
None (%)	31.09	32.93	8.50	9.51	7.04	8.67	2.38	3.71
Non-response (%)	6.92	0.48	5.84	0.51	5.85	0.49	5.37	0.17
Secondary School								
Abitur ^d (%)	4.88	5.71	4.74	5.31	2.65	3.16	13.98	18.72
Other ^e (%)	84.00	85.99	89.57	91.96	91.47	94.06	82.02	80.65
None (%)	5.76	7.36	1.54	2.03	1.52	2.12	0.22	0.24
Non-response (%)	5.36	0.94	4.15	0.70	4.36	0.66	3.78	0.39
Children under 16 (#)	0.72	0.65	0.68	0.62	0.65	0.62	0.59	0.55
Married (%)	70.50	68.64	60.01	57.71	60.14	57.95	57.68	58.21
City size								
> 500,000 (%)	14.30	14.79	10.89	12.15	10.16	11.37	11.78	13.79
20,000 – 500,000 (%)	44.93	47.26	38.80	42.95	37.93	41.89	43.09	45.33
≤ 20,000 (%)	40.77	37.95	50.31	44.90	51.91	46.74	45.13	40.88
% of all self-employed	24.11	27.16	46.92	46.35	23.13	21.46	5.84	5.03
Observations	28,188	37,442	27,424	35,678	16,733	20,615	2,792	3,456

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 (2002-2008).

^a University of applied sciences.

^b The degree Meister-craftsman certifies the highest professional qualification in craftsmanship.

^c The degree Geselle 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.

Table 3: Estimation results of self-employment state and transition probabilities

	LPM Entry	LPM Entry	LPM Entry	Logit Entry	Logit Exit	Logit Self-employed
dB1-dPost	0.0090* (0.0052)	0.0090* (0.0052)	0.0095* (0.0051)	0.5319*** (0.1790)	-0.0113 (0.3631)	0.3024*** (0.1008)
dA1-dPost			0.0086* (0.0049)	0.3575*** (0.1767)	-0.0212 (0.3895)	0.1879** (0.0905)
dA2-dPost			0.0076 (0.0048)	0.3436* (0.1807)	0.1210 (0.4137)	0.1626 (0.1152)
dEU-dPost		0.0159** (0.0067)	0.0150*** (0.0057)	0.5299* (0.2828)	-0.4243 (0.5105)	0.5207** (0.2337)
dB1	-0.0162* (0.0081)	-0.0097* (0.0049)	-0.0077 (0.0056)	-0.5672* (0.3165)	0.7230 (0.4856)	-0.6163 (0.4177)
dA1			-0.0011 (0.0105)	-0.0253 (0.4700)	0.1337 (0.5382)	0.1275 (0.6622)
dA2			-0.0066 (0.0102)	-0.3276 (0.4529)	0.0539 (0.5081)	-0.1159 (0.6453)
dPost	-0.0048 (0.0048)	-0.0042 (0.0048)	-0.0033 (0.0046)	-0.0941 (0.1725)	-0.7661** (0.3686)	-0.0356 (0.0855)
dEU		0.0001 (0.0039)	0.0033 (0.0037)	0.2045 (0.2245)	-0.3482 (0.5146)	0.4895*** (0.1500)
Constant	0.0280*** (0.0066)	0.0167 (0.0134)	0.0120 (0.0129)	-4.2875*** (0.7038)	1.3675 (1.1304)	-5.4426*** (0.8104)
Year dummies		✓	✓	✓	✓	✓
Branch dummies		✓	✓	✓	✓	✓
Controls		✓	✓	✓	✓	✓
Wald χ^2				3,135.56	1,578.11	11,525.63
Log likelihood				-11,891.53	-2,019.11	-52,147.28
Adj- R^2 /Pseudo- R^2	0.00	0.02	0.01	0.06	0.15	0.23
Observations	55,009	55,009	131,554	131,554	14,860	172,328

Notes: Robust standard errors, clustered by occupation, are given in parentheses below the coefficients of the linear probability model (LPM) and 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 and 2008, and indicators for the branch of craftsmanship, 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 (2002-2008).

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 2008, shown in the second column of Table 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 0.90 percentage points. This result does not change when year and branch dummies and further control variables are added (third column of Table 3).

Moreover, including the *A1*- and *A2*-occupations in the sample shows that the treatment for *A1*-occupations increased the probability of entry significantly, by 0.79 percentage points, while the *A2*-occupations seem not to be significantly affected (fourth column). Note the large significant coefficient of the interaction of the EU dummy and the post-policy period, underlining 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 1.59 (1.50) percentage points according to column three (four). In section 5.3, I demonstrate that the principle results remain unchanged after all Non-German craftsmen are excluded from the sample.

The fifth column of Table 3 presents the same full specification as used in column four, employing a logit model. The estimates tell a consistent story: the

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

	<i>B1</i>	<i>A1</i>	<i>A2</i>	<i>AC</i>	Δ_{B1}	Δ_{A1}	Δ_{A2}
Before reform 2004	0.90 (0.29)	1.53 (0.36)	1.14 (0.29)	1.57 (0.45)	-0.67 (0.40)	-0.04 (0.72)	-0.43 (0.62)
After reform 2004	1.41 (0.48)	2.02 (0.47)	1.48 (0.34)	1.46 (0.36)	-0.05 (0.43)	0.56 (0.77)	0.02 (0.63)
Δ between after and before reform 2004	0.51 (0.22)	0.49 (0.21)	0.34 (0.14)	-0.11 (0.27)	0.62 (0.33)	0.60 (0.29)	0.46 (0.28)
Before reform 2004	0.90 (0.29)	1.53 (0.36)	1.14 (0.29)	1.57 (0.45)	-0.67 (0.40)	-0.04 (0.72)	-0.43 (0.62)
After reform 2004	0.83 (0.30)	1.42 (0.40)	1.05 (0.30)	1.46 (0.36)	-0.62 (0.32)	-0.04 (0.67)	-0.40 (0.56)
Δ between after and before reform 2004	-0.07 (0.14)	-0.11 (0.24)	-0.08 (0.18)	-0.11 (0.27)	0.05 (0.12)	0.00 (0.05)	0.03 (0.09)
Difference in differences					0.58 (0.26)	0.60 (0.29)	0.43 (0.22)
Relative difference in differences					69.88	42.25	40.95

Notes: The upper panel 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 lower panel 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.

Source: Own calculations based on the scientific use file of the German microcensus (2002-2008).

signs are the same across models, and, apart from the coefficient of the interaction between the post-policy dummy and the indicator for *A2*-vocations, the same variables are statistically significant. Obviously, the functional form helps to identify the coefficients of the treatment interactions more precisely, whereas the coefficient of the post-policy period's interaction with the EU dummy only becomes significant at the 10% level.

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 fraction 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 six 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*- and *A1*-occupations point to an interpretation that exit probabilities remained constant or may even be reduced in the post-policy period. For the *A2* group, the coefficient is positive but also insignificant. Recall here that a modest decline of the exit rates is also shown in Figure 4.

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 3 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.

5.1 Treatment Effects on Transition Probabilities

To find the quantitative effect of the amendment on the probability of entry and exit, I first predict the probabilities of a person with average characteristics before and after the reform, using the estimates from the preferred logit models reported in Table 3. Having obtained these, in a second step I calculate their differences, both reported in Table 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 and 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, 2008). 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

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

	<i>B1</i>	<i>A1</i>	<i>A2</i>	<i>AC</i>	Δ_{B1}	Δ_{A1}	Δ_{A2}
Before reform 2004	5.53	3.15	2.91	2.76	2.77	0.38	0.15
	(1.63)	(0.86)	(0.66)	(1.12)	(1.85)	(1.51)	(1.38)
After reform 2004	2.58	1.44	1.53	1.28	1.30	0.15	0.24
	(0.63)	(0.31)	(0.32)	(0.46)	(0.78)	(0.59)	(0.64)
Δ between after and before reform 2004	-2.95	-1.71	-1.38	-1.48	-1.47	-0.23	0.09
	(1.15)	(0.70)	(0.57)	(0.94)	(1.32)	(1.17)	(1.09)
Before reform 2004	5.53	3.15	2.91	2.76	2.77	0.38	0.15
	(1.63)	(0.86)	(0.66)	(1.12)	(1.85)	(1.51)	(1.38)
After reform 2004	2.61	1.47	1.35	1.28	1.33	0.18	0.07
	(1.11)	(0.63)	(0.54)	(0.46)	(1.08)	(0.74)	(0.66)
Δ between after and before reform 2004	-2.92	-1.68	-1.56	-1.48	-1.44	-0.20	-0.08
	(1.37)	(0.74)	(0.64)	(0.94)	(0.98)	(0.78)	(0.72)
Difference in differences					-0.03	-0.03	0.17
					(0.92)	(0.56)	(0.56)
Relative difference in differences					-1.15	-2.04	12.50

Notes: The upper panel 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 lower panel 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.

Source: Own calculations based on the scientific use file of the German microcensus (2002-2008).

Table 6: Probabilities of being self-employed (in %): Difference in differences

	<i>B1</i>	<i>A1</i>	<i>A2</i>	<i>AC</i>	Δ_{B1}	Δ_{A1}	Δ_{A2}
Before reform 2004	5.13 (1.94)	10.22 (2.72)	8.19 (2.40)	9.11 (3.48)	-3.98 (3.06)	1.11 (5.72)	-0.91 (5.14)
After reform 2004	6.70 (2.59)	11.88 (3.06)	9.34 (2.42)	8.96 (3.30)	-2.25 (3.11)	2.93 (5.95)	0.39 (5.13)
Δ between after and before reform 2004	1.57 (0.72)	1.66 (0.60)	1.15 (0.69)	-0.15 (0.72)	1.72 (1.06)	1.81 (0.87)	1.30 (0.91)
Before reform 2004	5.13 (1.91)	10.22 (2.69)	8.19 (2.39)	9.11 (3.30)	-3.98 (2.96)	1.11 (5.64)	-0.91 (5.06)
After reform 2004	5.04 (1.91)	10.05 (2.70)	8.05 (2.39)	8.96 (3.29)	-3.91 (2.95)	1.09 (5.64)	-0.90 (5.05)
Δ between after and before reform 2004	-0.09 (0.42)	-0.17 (0.79)	-0.14 (0.65)	-0.15 (0.72)	0.06 (0.31)	-0.02 (0.11)	0.01 (0.11)
Difference in differences					1.66 (0.87)	1.83 (0.95)	1.29 (0.86)
Relative difference in differences					32.94	18.21	16.02

Notes: The upper panel 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 lower panel 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.

Source: Own calculations based on the scientific use file of the German microcensus (2002-2008).

entry probability resulting from the reform increased by 0.58 percentage points. This economically relevant effect is also statistically significant, with a standard error of 0.26 (p-value: 0.03). The probability of entering self-employment would have been $1.41 - 0.58 = 0.83\%$ 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 69.88%.

Remarkable 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 a similar magnitude as the *B1*-occupations, namely 0.60 percentage points. This rise is significantly different from zero, with a standard error of 0.29 (p-value: 0.04). 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. Surprisingly, the results show that the craftsmen in *A2*-occupations responded to the reduction and partial exemption of the entry barrier with only an increase of 0.43 percentage points, which is significant with a standard error of 0.22 (p-value: 0.05). In relative terms, the reform increased the entry probability of the *A1* group by 42.25%, while for *A2*-occupations, the entry probability was 40.95% higher than the hypothetical situation without the reform.

How sustainable are these entries? In Table 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 -0.03 percentage points, with a standard error of 0.92. This negative effect is insignificant (p-value: 0.98). Similarly, the effect of -0.03 percentage points for *A1*-vocations is highly insignificant, with a standard error of 0.56 (p-values: 0.96). Thus, more sustainable business entries could be established after the deregulation. The results suggest that this is due to the reform. However, for

A2-occupations, the treatment effect of 0.17, though insignificant with a standard error of 0.56 (p-value: 0.76), points to an increase in the exit rate caused by the amendment. 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 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, namely -1.15%, -2.04%, and 12.50% 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 6 shows also that after accounting for the counter-factual situation without the reform, a person with average characteristics in a *B1*-occupation is 1.66 percentage points more likely to engage in entrepreneurship. This effect is significant, with a standard error of 0.87 (p-value: 0.06). The effect on the *A1*-occupations is even larger. The probability of being self-employed increased significantly by 1.83 percentage points, with a standard error of 0.95 (p-value: 0.04). A more flexible specification reported in Table 10 shows that this large effect is driven by an outlier in 2007. Still, a marginally insignificant effect of 1.27 percentage points (p-value: 0.11) is observed when the years 2007 and 2008 are excluded (cf. Rostam-Afschar, 2010). Further, the treatment effect for the *A2*-vocations including all years is 1.29 percentage points. This effect just fails to achieve statistical significance at the 10% level with a standard error of 0.86 (p-value: 0.13).

Note, that the probability of being self-employed is substantially smaller for the *B1*-vocations in the first place. Therefore, the relative effect of 32.94% is the largest compared to the other groups of craftsmen. For the *A1*-vocations the relative effect amounts to 18.21% and to 16.02% for the *A2*-professions.

5.3 Heterogeneous Treatment Effects

Who are these new entrepreneurs in craftsmanship? In this subsection, 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, disadvantaged in terms of labor market opportunities, such as craftsmen without any professional qualification and female craftsmen, might see self-employment as a way out of unemployment (cf. Caliendo & Künn, 2011). From Table 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 of this level of professional qualification.

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 7: Treatment effects on entry into self-employment and on the share of self-employed for sub-groups (in %): Difference in differences

Sample	German	Unsubsidized	Female	Male	No qualification	Geselle	Meister
Treatment effects on entry into self-employment							
DID _{B1}	0.57** (0.25)	0.49** (0.21)	0.02 (0.28)	0.89** (0.44)	0.63*** (0.22)	0.58* (0.31)	0.48 (1.32)
Relative DID _{B1}	69.57	73.21	2.47	63.26	762.70	76.59	12.01
DID _{A1}	0.57** (0.28)	0.57** (0.24)	-2.04 (1.88)	0.77** (0.32)	0.52 (0.37)	0.79** (0.31)	-1.60 (1.86)
Relative DID _{A1}	41.55	50.16	-45.85	64.62	415.45	99.22	-23.29
DID _{A2}	0.43** (0.21)	0.34** (0.17)	1.34* (0.74)	0.52** (0.26)	0.41 (0.26)	0.45** (0.18)	-1.39 (1.77)
Relative DID _{A2}	41.22	39.04	290.08	49.12	697.80	80.27	-22.83
Treatment effects on the share of self-employed							
DID _{B1}	1.54* (0.86)	1.34* (0.74)	1.07* (0.58)	1.92 (1.23)	1.24** (0.50)	0.73 (1.08)	3.88 (2.80)
Relative DID _{B1}	28.92	28.96	94.21	18.50	72.31	13.43	12.03
DID _{A1}	1.62* (0.90)	1.53 (0.94)	5.12* (2.76)	0.90 (0.58)	0.80 (0.81)	1.31 (1.23)	2.63 (2.07)
Relative DID _{A1}	15.35	16.41	55.05	8.09	32.74	18.59	5.32
DID _{A2}	1.09 (0.87)	1.11 (0.88)	4.57 (4.14)	0.58 (0.71)	0.59 (0.70)	0.84 (0.89)	3.99 (2.55)
Relative DID _{A2}	12.51	14.78	58.65	5.85	33.81	16.77	8.58

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 (2002-2008).

Table 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 somewhat 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 except for those working in *A2*-occupations. 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 insignificant for female and male craftsmen of all vocational groups apart from females working in *B1*-occupations. Here, the results indicate that the reform decreased the probabilities of exit from self-employment by 6.78 percentage points with a standard error of 3.68 implying a substantial relative reduction, namely of 58.84%. 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 as well, 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.

Focusing on female *A1* workers, there is neither a significant effect on the entry probability nor on the exit probability. However, both entries and exits dropped according to the point estimates, whereas the exit probability decreased to a lower level than the entry probability. This is consistent with the significant positive effect on the probability of being self-employed.

For female craftsmen from the *A2* group and for male craftsmen across all three groups, the increases in the entry probabilities do not accompany a significant raise of the probabilities of being self-employed. This could be due to the increased exit rates although none of positive point estimates is significant. However, for males in the *B1*- and *A1*-occupations, the treatment effects on the share of self-employed barely fails significance at the 10% level (p -values: 0.12 and 0.12). Thus, while the evidence is not strong that female and male craftsmen in the *A2* group experienced the intended effects of the reform, the results suggest that the increases reported in Tables 4 and 6 for the *B1*- and *A1*-occupations stem 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 both 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 ob-

jectives of this policy seem to have been accomplished. However, the fact that these entries could not increase the probability of being self-employed favours 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 ancilliary 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 gauge the robustness of the findings in this analysis, the logit models of the probability of being self-employed and of the transition probabilities are reestimated, varying the estimation sample, the definition of variables, and the specification.

Column I in Table 9 shows the results of estimating the same specification as in the main analysis, in which the year 2001 is included. Obviously, size and significance of the estimates are similar to those reported in Table 3. Hence, using this sample does not distort the main results. However, I decided to exclude 2001 from the sample because 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 II and III, 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 2008 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 IV to VI, 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 IV, the coefficients for the interaction terms turn out to be significant for the A1- and A2-occupations, when the estimation sample includes the year 2001 –which is why the main analysis was based on the sample from 2002 to 2008. The interaction coefficients in columns V and VI 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 a time trend, and rerun the estimations using the logit models for the probability of being self-employed for the pre-policy period. Column I in Table 10 shows that before the reform, no differential trends are detected. To show that this is true in a more

flexible specification, I replace the time trend with time dummies. Columns II and III display the results for 2001 to 2004 and for 2002 to 2004, respectively. Obviously, no significant differences existed in the period before the reform apart from those in the the year 2001, which I exclude in the main analysis. This corroborates the plausibility of the assumption that in absence of the policy change, common trends would also be observed for the post-policy period.

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

Column IV in Table 10 contains the results of this test for the entire period from 2002 to 2008. Of the coefficients on the interaction terms (not reported) only the coefficient for the foodstuffs trades is significant. Evidently, the general results are robust to this variation of the specification, although the coefficient of the interaction between the post-policy period and the A1-occupations reduced to insignificance.

The last column presents the same specification as in column IV, apart from

the fact that I replaced the post-policy period dummy with time dummies and all interactions involving the post-policy period dummy with interactions using time dummies instead. Correspondingly, I replaced the interactions between the branch dummies and the time trend with the interactions between the branch dummies and time dummies. The results are in line with the prior findings shown in Table 3 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 *A1*-occupations are of similar size to the coefficient of the interaction between the *A1* group and the post-policy dummy in Table 3 and slightly larger than that reported in column IV of Table 10 except for the interaction with the dummy for 2007. Individually these coefficients are insignificant, in a joint test they are significant at the 10% level only if the dummy for 2007 is included.

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 completely 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 is required after 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 2008. 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 just fails to achieve significance at the 10% level for the *A2*-vocations. The strongest relative increase amounts to more than 30%. 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 that are significantly more likely to start businesses after the reform in *B1*- and *A1*-occupations. There is weaker evidence that for these groups the probabilities of being self-employed also increased after the reform.

Second, untrained workers among the *B1*-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 among *A1*- and *A2*-occupations that completed an apprenticeship also engage more in entrepreneurship after the reform, which was the intended effect of the *Altgesellen* rule. However, the increase in entries seems not to lead to a higher probability of being self-employed for the *A1*- and *A2*-craftsmen trained in an apprenticeship.

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.

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7 Appendix

Tables

Table 8: Stock of businesses at the end of the year

	<i>A</i>	<i>B1</i>	<i>B2</i>	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

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

	Self-employed: Timing (2001-2008)	Self-employed: Timing 2004 dropped (2002-2008)	Self-employed: Timing 2004 as post (2002-2008)	Self-employed: Placebo reform in 2002 (2001-2004)	Self-employed: Placebo reform in 2002 (2002-2004)	Self-employed: Placebo reform in 2003 (2002-2004)
	I	II	III	IV	V	VI
dB1-dPost	0.3853*** (0.1000)	0.3224*** (0.0989)	0.2557*** (0.0810)	0.1828 (0.1195)	0.0326 (0.1176)	0.0606 (0.1089)
dA1-dPost	0.2781*** (0.0931)	0.2258*** (0.0862)	0.1882*** (0.0664)	0.2666** (0.1183)	0.1356 (0.1149)	0.1161 (0.0956)
dA2-dPost	0.2175* (0.1156)	0.1934 (0.1179)	0.1331 (0.0922)	0.1991* (0.1186)	0.1401 (0.1258)	0.0903 (0.0974)
dEU-dPost	0.5564** (0.2227)	0.6120*** (0.2267)	0.4722** (0.2146)	0.2773* (0.1519)	0.3179* (0.1653)	0.2627 (0.1738)
dB1	-0.6833 (0.4294)	-0.6512 (0.4206)	-0.5521 (0.4155)	-0.7686* (0.4638)	-0.6512 (0.4509)	-0.6494 (0.4283)
dA1	0.0528 (0.6745)	0.0746 (0.6632)	0.1528 (0.6599)	-0.0446 (0.6996)	0.0646 (0.6859)	0.1167 (0.6573)
dA2	-0.1519 (0.6563)	-0.1673 (0.6486)	-0.0806 (0.6388)	-0.1929 (0.6741)	-0.1581 (0.6587)	-0.0945 (0.6380)
dPost	-0.0639 (0.0864)	-0.0664 (0.0788)	-0.1748*** (0.0574)	-0.0988 (0.1123)	-0.0391 (0.1095)	-0.0250 (0.0934)
dEU	0.4825*** (0.1465)	0.3868*** (0.1344)	0.5972*** (0.1366)	0.4216*** (0.1536)	0.3350** (0.1573)	0.4618*** (0.1466)
Constant	-5.5051*** (0.8160)	-5.3740*** (0.8243)	-5.4124*** (0.8119)	-5.6918*** (0.8095)	-5.6441*** (0.8006)	-5.6847*** (0.7864)
Year dummies	✓	✓	✓	✓	✓	✓
Branch dummies	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Wald χ^2	14,247.18	9,809.69	9,546.92	6,181.82	6,922.08	8,686.93
Log likelihood	-59,307.67	-45,037.83	-52,169.23	-28,591.93	-21,452.52	-21,454.11
Pseudo- R^2	0.23	0.23	0.23	0.25	0.24	0.24
Observations	198,900	148,323	172,328	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, 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 (2001-2008).

Table 10: Robustness: Logit estimation results of self-employment state probabilities

	I	II	III	IV	V
	Self-employed: Groups · trend (2002-2004)	Self-employed: Groups · time dummies (2001-2004)	Self-employed: Groups · time dummies (2002-2004)	Self-employed: Branches · trend (2002-2008)	Self-employed: Branches/Groups · time dummies (2002-2008)
dB1 · Trend	0.0309 (0.0727)				
dB1 · d2001		-0.2851*** (0.0697)		dB1 · dPost 0.3176*** (0.1039)	0.3266** (0.1553)
dB1 · d2003		0.0046 (0.1062)	0.0016 (0.1043)		0.3216* (0.1830)
dB1 · d2004		0.0655 (0.1455)	0.0628 (0.1448)		0.4057*** (0.1405)
dA1 · Trend	0.0837 (0.0675)			dA1 · dPost 0.1591 (0.1033)	0.4102*** (0.1553)
dA1 · d2001		-0.2464*** (0.0342)		dA1 · d2005	0.1600 (0.1701)
dA1 · d2003		0.1057 (0.1078)	0.1026 (0.1060)	dA1 · d2006	0.1621 (0.1899)
dA1 · d2004		0.1711 (0.1357)	0.1680 (0.1349)	dA1 · d2007	0.3178** (0.1603)
dA2 · Trend	0.0767 (0.0724)			dA2 · dPost 0.0861 (0.1181)	0.2418 (0.1683)
dA2 · d2001		-0.1018* (0.0578)		dA2 · d2005	0.1343 (0.2006)
dA2 · d2003		0.1285 (0.1170)	0.1252 (0.1153)	dA2 · d2006	0.1251 (0.2172)
dA2 · d2004		0.1575 (0.1455)	0.1537 (0.1445)	dA2 · d2007	0.1641 (0.1955)
Constant	✓	✓	✓		0.0731 (0.2163)
Year dummies	✓	✓	✓	✓	✓
Branch dummies	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Log likelihood	-21,452.94	-28,587.60	-21,452.04	-52,108.30	-52,088.48
Pseudo-R ²	0.24	0.25	0.24	0.23	0.23
Observations	75,137	101,709	75,137	172,328	172,328

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, 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 (2002-2008).

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 2008.

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 2008.

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).