Empirical evaluation of selected labour market and education policies

Inaugural Dissertation

zur Erlangung des akademischen Grades

Doktor der Wirtschaftswissenschaft (Dr. rer. pol.)

an der Freien Universität Berlin

vorgelegt von

Dipl.-Vw. Hans J. Baumgartner, B.A., M.Sc.

eingereicht im Juli 2006

Tag der Disputation: 24. November 2006

Erstgutachter:	Prof. Dr. Viktor Steiner Freie Universität Berlin
Zweitgutachter:	Prof. Stephen P. Jenkins, D.Phil. University of Essex

To my son

Erklärung gem. §7(2) der Promotionsordnung des Fachbereichs Wirtschaftswissenschaften vom 27. Januar 1993

Hiermit erkläre ich, dass ich für meine Dissertation folgende Hilfsmittel und Hilfen verwendet habe:

- Stata 8.2, sowie die darin enthaltene ado-Programme,
- Microsoft Excel 2000,
- LaTeX 2_ε,
- sowie die im Abschnitt Bibliography angegebene Literatur.

Auf dieser Grundlage habe ich die Arbeit selbständig verfasst.

Berlin, 11. Juli 2006 _____

Hans J. Baumgartner

Contents

List of Tables

List of Figures

Chapter 1

Main introduction: Empirical policy validation

1.1 Motivation

About five million people are unemployed in Germany, which is a disturbingly high unemployment rate of about twelve percent. Thus, the pressure to bring people into employment is one of the most challenging issues facing the German government. But finding an adequate policy to fight against high unemployment rates and bringing it down to a lower rate is a difficult task. Closely related to labour market policies are education policies, which are equally important since a highly qualified workforce is a necessity for prosperous economies in a globalised world and in turn secures employment. Education policies are thus flanking policies for labour market policies. But education policies appear to be equally unsuccessful in Germany. For instance, attainment in tertiary education is below the OECD average in Germany and Germany's pupils did not perform very well in the PISA[1] tests.

In medical research, it is obligatory to evaluate new treatments empirically. In economics, however, new policies are rarely systematically evaluated, if at all. But empirical research could help to find policies that are likely to be more successful. Ex post it can be evaluated whether a certain policy has achieved

[1] PISA (Programme for International Student Assessment) is a standardised test that was run by the OECD in 2000 and 2003.

its aims or whether it has failed to do so. From such experience, policies could be refined to make them more powerful. However, policies are formed in the political process and are in most instances based on the bargaining power and the persuasive arguments of political parties and lobby organisations. These groups have expectations on policies that are mostly based on theoretical and argumentative considerations only. But whether a policy turns out to achieve its aims is not a question of persuasive arguments. Is an empirical question and should therefore be answered empirically.

This doctoral thesis is devoted to analysing three selected issues in the field of labour market and education policy. I evaluate these policy issues empirically and give an answer to how successful these policies are in achieving their hoped for aims. In the next chapter, I analyse the effectiveness of two similar active labour market policy instruments which support formerly unemployed people if they decide to exit unemployment by becoming self-employed. The likely effect of a reduced class size in secondary schools on early career earning is estimated in the third chapter. In the fourth chapter, I provide an evaluation of two student aid reforms which both aimed to increase enrolment in tertiary education. Chapter five closes this doctoral thesis with a general conclusion and summary. Before turning to discuss the empirical work in more detail in the following chapters, I first give a brief overview of the main body of this doctoral thesis.

1.2 Overview

Self-employment out of unemployment: Exiting unemployment via self-employment has become a major part of active labour market policy in Germany. In 1994 there were only about 37,000 business start-ups of formerly unemployed individuals, while ten years later, in 2004, this number well exceeded 350,000. This almost tenfold increase is partly driven by the introduction of

a new programme – start-up subsidy (Existenzgründungszuschuss, ExGZ) – which was introduced in the course of the 'Hartz-Refoms' in 2003. Until 2003 there was only one single scheme supporting the self-employment of formerly unemployed individuals – namely the bridging allowance (Überbrückungsgeld, ÜG).

The second chapter of this doctoral thesis is titled "The Sustainability of Supported Start-Up Firms of Formerly Unemployed People" and evaluates two active labour market programmes that support business start-ups out of unemployment. One programme (ÜG) supports the first six months in self-employment and aims to secure the living maintenance, while the other programme (ExGZ) aims to secure the social security for the starting phase of up to three years. Both programmes are analysed separately in two dimensions.

The first research question concerns the labour market activity if supported self-employment terminates. Does the experience of self-employment lead to an integration into the primary labour market or do unsuccessful entrepreneurs return to the pool of unemployment? From the perspective of active labour market policy; the unemployment rate is reduced directly if unemployed people become self-employed, since self-employed people are ex definition no longer unemployed. Moreover, an experience in self-employment is just another type of experience relevant to the labour market which may or may not help in finding a new job in the primary labour market. From the perspective of the self-employed individual, however, it is different. They have to decide whether the newly established business should be carried on or whether they should take up dependent employment instead which may or may not be a better opportunity for them. I model the transitions to dependent employment versus unemployment out of self-employment with a discrete time duration model. I find the transition rate to dependent employment, in the month after an un-

successful business was closed down, to be negligibly low for both programmes. This small transition rate can be either due to unavailable outside options or because the labour market does not value skills from self-employment. The transition rate to unemployment, on the other hand, is relatively small but jumps to a higher level at specific points in time, when the support either runs out (ÜG) or is reduced to a lower rate (ExGZ).

The second research question I am analysing in this chapter is the sustainability in supported self-employment and by which characteristics it is determined. Supported self-employment of formerly unemployed people appears to be very sustainable on average. More than 80 percent of supported start-ups are still in business 1.5 years after they have been set up. The ExGZ programme, however, is still ongoing and so the relatively high survival rate might be due to the ongoing support of ExGZ. Nevertheless, the sustainability of supported self-employment of formerly unemployed people is very similar in both programmes. These findings have important policy implications for the adjustment of recent labour market reforms in Germany.

Class size effects: The third chapter of this doctoral thesis analyses an aspect of education policy and how this policy effects an outcome later on in the labour market. This chapter thus bridges thus the labour market policy and education policy and is titled "Class Size Effects on Early Career Earnings". Class-size reduction is probably the most popular policy for improving the quality of secondary education. The reason for this popularity may be twofold: (i) all parties concerned with education – that is, parents, teachers, policy-makers, and voters – have an almost uniform preference for smaller classes; and (ii) class size is rather easy to measure, to implement, and also to communicate politically.

Despite the popularity of class-size reduction, it is still empirically unclear,

whether smaller classes influence labour market outcomes or not. In the early 1990s, there was a vital discussion in the US-American economic literature, kicked off by Card and Krueger (1992a), who found positive and significant effects on earnings. They estimate that a reduction of the pupil-teacher-ratio – which is closely related to class size – by ten students would increase earnings by 3.6 percent. Other researchers, however, could not verify Card and Krueger's findings and rejected the proposition that class size or any other measure of the quality of schooling had an impact on earnings. Whether there are any class-size effects has recently also been discussed in the European economic literature but without finding a clear cut answer either. Dustmann, Rajah, and van Soest (2003), for instance, find wage effects of class size, while Dolton and Vignoles (2000) find no significant relation between class size and earnings.

I contribute to this literature by adding evidence for Germany. I am analysing the potential effect of reduced class size in secondary education on early career earnings drawing on regional data from the Socio-Economic Panel Study (SOEP) enhanced with collected data on class size from nine state offices for statistics. With this enhanced data set, I explore the cross regional and longitudinal variation in class size to identify the effect a reduced class size may have on early career earnings. I do not find this effect to be statistically significant. Moreover, I find this effect to be too low to be economically meaningful, i.e. the point estimate suggests an effect of 0.7 percentage points if the class size in upper secondary schools were reduced by one pupil. This insignificant effect, which is robust to various changes in specification, may have important policy implications for the ongoing debate about how to improve education in Germany and elsewhere.

Student aid: The fourth chapter is titled "Student Aid in Germany". Students from low-income families are eligible for student aid under the federal students' assistance scheme (Bundesausbildungsförderungsgesetz). BAföG, which is the short name for student aid in Germany, aims to guarantee 'equal opportunities'. That is, all young people shall have the opportunity to pursue the path of education that suits their skills the best, independent of the economic and social situation of themselves or their parents. Beside this distributive argument, BAföG aims explicitly to increase the rate of university graduates in society by motivating students from low-income families who have the necessary entrance qualifications for tertiary education to matriculate into a university or similar institution of tertiary education (Ramsauer, 2002).

BAföG is means tested, i.e. it depends on the financial capacity of the students and their parents. Since the cost of living changes over time, the regulations for how BAföG is granted have to be revised on a bi-yearly basis. Two adjustments have reshaped BAföG and can be classified as major reforms.

The first reform I am evaluating took place in 1990 and concerns the repayment obligations. In the 1980s, BAföG had the form of an interest-free loan which had to be repaid after graduation. The reform changed this pure loan system to a mixed grant and loan system. That is, 50 percent of the support has the form of an interest-free loan and the other 50 percent has the form of a grant. This change in the repayment obligations translates into a substantial reduction of the net cost of tertiary education for students eligible for student aid. The second reform I am analysing took place in 2001 and concerns the way BAföG is means tested. The basic allowance on parents income and the pre-defined maintenance needs were lifted in 2001 which resulted in an average increase of the monthly support by about 10 percent. Furthermore, since parents are allowed to earn more under the new regulations, more students have become eligible for student aid.

One common fact of both reforms is that students can be divided into those eligible for student aid and those ineligible for student aid. The reforms had an effect only on eligible students. Observing two groups of which one group experiences a certain treatment while the other does not is a classical textbook example for a difference-in-difference estimation. I apply the difference-in-difference estimator using a discrete time transition rate model to analyse the effectiveness of BAföG in achieving its main aim, which is to motivate students from low-income families to pursue tertiary education. Analysing the influence of student aid on the school-to-university transition, I have to overcome the difficulty that the eligibility status is only observable for enrolled students. However, students not enrolling in a university could be eligible, given the financial capacity of their parents, if they had matriculated themselves. I thus build a simulation model to infer the eligibility status of enrolled and not enrolled students.

To put the main evaluation results of both reforms into a nutshell, I could not find any significant effects on the enrolment probabilities of students eligible for BAföG. Based on my estimations, I find both BAföG reforms to be ineffective in increasing the enrolment rate of eligible students. The findings presented in this chapter contribute to the ongoing debate about how to finance higher education in Germany and elsewhere.

Chapter 2

The sustainability of supported start-up firms of formerly unemployed people

2.1 Introduction

A rising rate of nascent entrepreneurs in an economy may have positive effects on market efficiency and the rate of innovations. Growing levels of entrepreneurial activity may also reduce the unemployment rate, which may decrease if more people establish new firms: (i) because founders themselves may have been unemployed before they started their new firm; and (ii) newly established firms may increase labour demand if successful. Because positive effects on employment are likely, the federal government of Germany supports unemployed people that take the chance to become an entrepreneur.

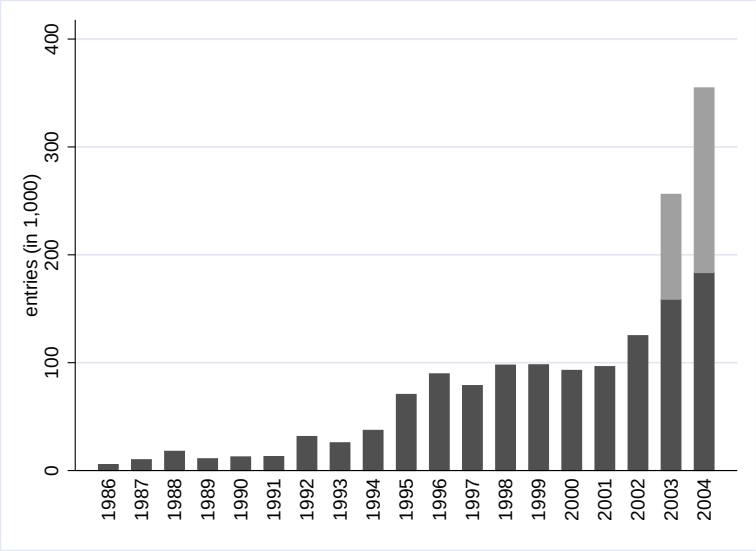
The self-employment decision of unemployed people is supported through two programmes. The first programme – 'Überbrückungsgeld' (ÜG) (bridging allowance) – supports the first six months and aims to secure the living maintenance. The second programme was introduced in January 2003 in the course of the 'Hartz-Reforms'. This programme is called 'Existenzgründungszuschuss' (ExGZ) (start-up subsidy) and aims to secure social security contributions for the starting phase of up to three years. Both programmes are very popular and are attracting a growing number of participants, as Figure 2.1 shows.

Participation in either of these programmes has soared in 2003. The massive increase might be due to a marketing campaign for ExGZ, which has, since then, also been known as 'Ich-AG' (Me plc). In 2004, the last year for which data is available, as many as 354,800 people participated in either of these programmes.

Being unemployed for a too long period may be a stigma and may hence decrease the chance of finding new employment. Furthermore, human capital may be devaluated during an unemployment spell. Exiting unemployment through self-employment secures therefore human capital (Niefert, 2005). However, it is a specific sort of human capital one needs to be an entrepreneur (Blanchflower, 2004), and it is unclear whether the labour market values these specific skills, since people who have shorter experiences in self-employment, that is less than five years, have difficulty returning to dependent employment (Bruce and Schuetze, 2004). Moreover, Brixy and Grotz (2002) find that more than 50 percent of start-ups become bankrupt within the first five years in Germany. Beside the considerable high risk of becoming bankrupt and ending up with liabilities and debt, wage from dependent employment is often substantially higher than the income generated out of self-employment (Hamilton, 2000), i.e. there seems to be no risk premium.

Despite of these findings, the 'Bundesagentur für Arbeit' (BA) (federal employment agency) applies two active labour market policy instruments that motivate the unemployed to become self-employed. In this chapter, I evaluate both programmes in two dimensions. The first is the labour market activity if self-employment is terminated. Are supported self-employed people integrated into the first labour market or do they return to the pool of unemployment? The second question to address is the survival of supported start-ups. How long do formerly unemployed persons remain self-employed if their self-employment was supported through one of these programmes?

Figure 2.1: Development of supported start-ups out of unemployment per year



The dark bars show the amount of start-ups that are supported through ÜG and the grey bars those supported through ExGZ.

Source: Bundesagentur für Arbeit, Arbeitsmarkt, various issues.

These questions have been paid rather scant attention in the empirical literature so far. Oberschachtsiek (2004) tests the skill distribution as put forward by Lazear (2003) and its influence on the exit probability of ÜG participants. He rejects, in a simple probit model, the hypothesis that the skill distribution would influence the exit probability. Reize (2000) applies a hazard rate model to data from the German Socio-Economic Panel Study (SOEP). He finds that the duration in unemployment does not influence the self-employment decision but self-employment is more stable than dependent employment, since the self-employed face a lower risk of becoming unemployed again.

This chapter unfolds as follows: Section 2.2 describes the two active labour market policy instruments. Section 2.3 introduces the econometric approach before Section 2.4.1 describes the data used. The econometric results are discussed in Section 2.5 and Section 2.6 concludes.

2.2 Two alternative subsidies for start-ups of formerly unemployed people

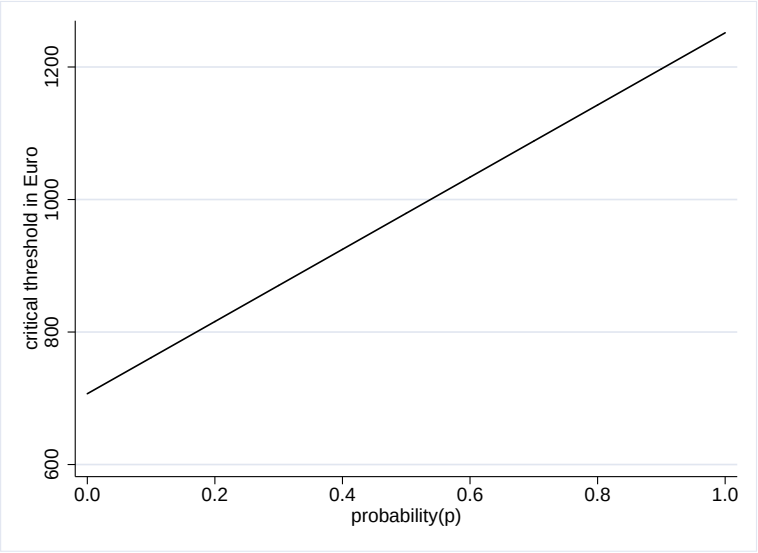
Two alternative active labour market policy instruments support unemployed people who become self-employed. In this section I briefly describe these two instruments.

The 'Überbrückungsgeld' (ÜG) (bridging allowance) was established in 1986. The main goal of this instrument is to secure the cost of living including social security contributions at the beginning of a new period of self-employment.[1] ÜG supports the first six months in self-employment through the unemployment transfer that the participants in ÜG would have received if they were still unemployed. Since the unemployment scheme also covers social security liabilities – that is, health insurance, retirement insurance, etc. – a lump sum for social security is granted that was 68.5 percent of the otherwise received unemployment transfer in 2003.[2]

Since January 2003 there has also been a second scheme to support unemployed people who establish a new firm. The 'Existenzgründungszuschuss' (ExGZ) (start-up subsidy) was introduced together with a wide portfolio of other active labour market policy instruments in the course of the so-called 'Hartz-Reforms'.[3] The goal of ExGZ is to support the starting phase in self-employment. For the first three years, ExGZ secures the social security contributions of the newly self-employed (IAB and GfA, 2005, p. 191). The support is a fixed sum of 600 Euro per month in the first year. A growth barrier – the 25k rule – is implemented in ExGZ, i.e. the support is only granted if the income is not expected to exceed 25,000 Euro per year. The support reduces to 360 Euro in the second and to 240 Euro in the third year.[4] In contrast to

[1] §57(1) SGB III.
[2] This lump sum is annually adjusted. The 2005 rate is 70.8 percent.
[3] Wunderlich (2004) provides an overview of all 'Hartz-Reforms'.
[4] §421 l SGB III.

Figure 2.2: The critical threshold from which ÜG becomes the rational choice



The critical threshold depending on π_j is drawn for $\pi_2 = p$ and $\pi_3 = \pi_2 p$ and an annual discount rate of three percent. See appendix 2.A.1 for further discussion.
Source: Own calculation

ÜG participants, participants in ExGZ are bound to become members of the legal pension insurance scheme and are able to take advantage of a reduced rate for the legal heath insurance (Koch and Wießner, 2003).[5]

Due to the institutional setting – ÜG rises with the unemployment transfers while ExGZ is fixed – it is rational to chose the programme ÜG if the unemployment transfer exceeds the threshold c.

$$c = \frac{\sum_{t=1}^{t=12} 600R + \pi_2 \sum_{t=13}^{t=24} 360R + \pi_3 \sum_{t=25}^{t=36} 240R}{\sum_{t=1}^{t=6} \omega R}, \tag{2.1}$$

where π_j are the expected probabilities to receive ExGZ in the year j (25k rule), ω is the lump sum for social security contributions granted for ÜG, and R is the discount factor $(1+r)^{-(t-1)}$, with r as the monthly discount rate. Equation (2.1) is derived in the appendix 2.A.1.

Given an annual discount rate of 3 percent and an expectation to receive ExGZ for the entire three years ($\pi_2 = \pi_3 = 1$), the unemployment transfer

[5] V.10 Durchführungsanweisung zur Förderung der Aufnahme einer selbständigen Tätigkeit nach §421 l SGB III (Stand 29.11.2004) (internal orders of the federal employment agency).

needs to exceed 1,252 Euro per month to make the programme ÜG to be the rational choice. However, if ExGZ is expected to be received only for one year ($\pi_2 = \pi_3 = 0$), the unemployment transfer needs to exceed 707 Euro to make ÜG to be the rational choice. Figure 2.2 draws the critical threshold for all values of π_j.

2.3 Empirical methods

Analysing the survival in self-employment requires the modelling of time, which is a continuous concept. Estimating time first requires measuring it and every measuring exercise has to split it into discrete intervals. If these discrete intervals are sufficiently small, continuous time survival rate models can be estimated (van den Berg, 2001). However, I observe a monthly event history and a month is a too wide interval to be interpreted as being continuous. I assume therefore that the transitions rates are constant within intervals (Box-Seffensmeir and Bradford, 2004; Jenkins, 2004a). This approach is applied, for instance, by Steiner (2001) and Jenkins (1995; 2004b) provides an intuitive introduction.

The time of an event occurrence, i.e. when an individual terminates self-employment, is described by the discrete non-negative random variable T, which takes on integer values only. Hence, the spell of person i in self-employment is observed from the beginning of month $t = 1$ through to the end of month T_i. The hazard rate $\lambda_i(t)$ is the probability that an event occurs in T_i given that person i survived up to T. Given that no event occurred until the beginning of t, the hazard rate into the labour market activity j is:

$$\lambda_{ij}(t|X_{it}, \varepsilon_i) = P(T_i = t, J_i = j|T_i \geq t, \mathbf{X}_{it}, \varepsilon_i) \tag{2.2}$$

with $\mathbf{X}_{it}$ = vector of covariates of individual i in interval t;

J = transition into activity j;

ε_i = time-invariant individual effect.

The individual effect ε_i that accounts for unobserved population heterogeneity in $\lambda_{ij}(t)$ is assumed to be constant over j, time-invariant, independently distributed of $\mathbf{X}_{it}$, and normally distributed with zero mean, i.e. $\varepsilon_i \sim \mathrm{N}(0, \psi)$, where $\psi \equiv \mathrm{var}(\varepsilon_i)$.

The individual effect controls for unobserved heterogeneity, which is eventually not sufficiently controlled for by the observed background characteristics. An example of such unobserved factors could be, for instance, the ability to be an entrepreneur. According to Lazear (2003), an entrepreneur is a person that combines multiple and transferable skills rather than being specialised in a single skill. The skill portfolio a person may have, however, is mostly unobserved.

Conditioned on $\mathbf{X}_{it}$ and ε_i and assuming that the transition can only occure at the boundaries of the interval, the transitions into activity j are independent and can hence be modelled as competing risks.[6] The hazard rate, i.e. the rate of terminating self-employment, is given by the sum of the state-specific hazard rates:

$$\lambda_i(t|\mathbf{X}_{it}, \varepsilon_i) = \sum_{j=1}^{J} \lambda_{ij}(t|\mathbf{X}_{it}, \varepsilon_i). \tag{2.3}$$

In terms of the hazard rate, the probability of survival up to t, conditional on not having terminated self-employment until the end of month $t-1$ is given by

$$P(T_i > t|T_i \geq t, \mathbf{X}_{it}, \varepsilon_i) = 1 - \lambda_i(t|\mathbf{X}_{it}, \varepsilon_i). \tag{2.4}$$

The survivor function which gives the (unconditional) probability of not having terminated self-employment up to month t, can be written as

$$P(T_i > t|\mathbf{X}_{it}, \varepsilon_i) = S(t|\mathbf{X}_{it}, \varepsilon_i) = \prod_{\tau=1}^{t-1} \big[1 - \lambda_i(\tau|\mathbf{X}_{it}, \varepsilon_i)\big]. \tag{2.5}$$

In terms of the hazard rate (2.4) and the survivor function (2.5), the probability of terminating self-employment and exiting into the labour market activity J

[6] If ε_i is restricted to zero, the independence of irrelevant alternatives (IIA) assumption needs to hold (Cameron and Trivedi, 2005).

at the end of month t is given by

$$P(T_i = t, J = j|\mathbf{X}_{it}, \varepsilon_i) = \lambda_{ij}(t|\mathbf{X}_{it}, \varepsilon_i) \prod_{\tau=1}^{t-1} \big[1 - \lambda_i(\tau|\mathbf{X}_{it}, \varepsilon_i)\big]. \tag{2.6}$$

It remains to specify a functional form of the hazard rate, which I model as a multinomial logit, i.e.:

$$\lambda_j(t|\mathbf{X}_{it}, \varepsilon_i) = \frac{\exp(\gamma_j \mathbf{T}_{it} + \beta_j \mathbf{X}_{it} + \varepsilon_i)}{\sum_{j=0}^{J} \exp(\gamma_j \mathbf{T}_{it} + \beta_j \mathbf{X}_{it} + \varepsilon_i)} \quad \forall\ j \in \{0; 1; \ldots; J\}. \tag{2.7}$$

To identify the model, I restrict the parameters for the base category to zero, which is self-employment. Hence, the activity-specific hazard rates from the multinomial logit model are given by:

$$\lambda_j(t|\mathbf{X}_{it}, \varepsilon_i) = \frac{\exp(\gamma_j \mathbf{T}_{it} + \beta_j \mathbf{X}_{it} + \varepsilon_i)}{1 + \sum_{j=1}^{J} \exp(\gamma_j \mathbf{T}_{it} + \beta_j \mathbf{X}_{it} + \varepsilon_i)} \quad \forall\ j \in \{1, 2, \ldots; J\}, \tag{2.8}$$

where the row vector $\mathbf{T}_{it}$ includes the baseline dummies that account for duration effects on the hazard rate in a flexible way. This allows the baseline hazard to vary completely unrestrictedly and enables it to pick up any spikes that may occur in the hazard rate. The row vector $\mathbf{X}_{it}$ includes observed individual characteristics to control for the socio-demographic characteristics, qualification, and entrepreneurial characteristics. The column vectors γ_j and β_j include the parameters to be estimated.

Assuming that, conditional on $\mathbf{X}_{it}$ and ε_i, all observations are independent, the sample likelihood function is given by

$$L(param.|\mathbf{X}_{it}, \varepsilon_i) = \prod_{i=1}^{N} \int_{-\infty}^{\infty} \Bigg(\prod_{j=1}^{J_i} \big[\lambda_{ij}(\tau|\mathbf{X}_{it}, \varepsilon_i)\big]^{\delta_i} \prod_{\tau=1}^{t_i-1} \big[1 - \lambda_i(\tau|\mathbf{X}_{it}, \varepsilon_i)\big] \Bigg) \psi \mathrm{d}\varepsilon_i, \tag{2.9}$$

$$\text{with } \delta_i = \begin{cases} 1 \text{ if the } t\text{'s spell of individual } i \text{ transits into state } j, \text{ and} \\ 0 \text{ otherwise.} \end{cases}$$

Thus, for a person with an observed transition into activity j the contribution to the likelihood function is given by the respective transition rate in

Equation (2.4), for a censored spell it is given by the survivor function in Equation (2.5), both written in terms of hazard rates. The maximum likelihood estimates of the parameters can be obtained by standard numerical optimisation procedures.[7]

2.4 Data

2.4.1 Sample design

I am using a unique data set, which was collected for a policy report (Caliendo, Steiner, and Baumgartner, 2005). The aim of this report is to analyse the success of two active labour market policy measures of the 'Bundesagentur für Arbeit' (BA) (federal employment agency) to support business start-ups out of unemployment – namely the 'Überbrückungsgeld' (ÜG) (bridging allowance) and the 'Existenzgründungszuschuss' (ExGZ) (start-up subsidy). One part of the data set is IEB[8] data, which contains relevant register data from various sources: the employment history; the transfer recipient's history; active labour market measures; and the unemployment history.[9] One drawback of the IEB data is that the employment history covers only employment that is liable for social security contributions. However, self-employment is not liable for social security contributions and hence register data does not provide any information on self-employment. To analyse self-employment, Caliendo, Steiner, and Baumgartner (2005) conducted together with infas [10] an interview of supported self-employed individuals. The interview focused on unemployed individuals who started one of the two programmes ÜG or ExGZ in the third quarter of

[7]Version 2.3.10 of the gllamm programme as implemented in Stata version 8.2 is used for the estimations. A description of the programme as well as technical details are provided by Rabe-Hesketh and Skrondal (2004) and Rabe-Hesketh, Skrondal, and Pickles (2001).

[8]IEB abbreviates 'Integrierte Erwerbs-Biographien' (integrated labour market biographies).

[9]The four sources of IEB data are: 'Beschäftigtenhistorie' (BeH); 'Leistungsempfängerhistorie' (LeH); 'Maßnahme-Teilnehmer-Gesamtdatenbank' (MTG); and 'Arbeitsuchendenstatus aus dem Bewerberangebot' (ASU/BewA).

[10]Institut für angewandte Sozialwissenschaft GmbH.

2003. The interviews were conducted by infas in January and February 2005. I am thus observing 17 to 20 months of labour market activity depending on the start of the programme and the interview month.[11] That is, I am able to observe up to 19 month-to-month transitions.

The interview should also provide information about the whereabouts of the not-supported unemployed versus the remainder of the supported unemployed. To realise this, Caliendo, Steiner, and Baumgartner (2005) carried out a propensity score-matching based on the IEB register data. They randomly draw $4 \times 3,000$ observations from successfully matched participants and for each of these participants they selected the four closest neighbours, i.e. non-participants. A CATI ('computer-assisted telephone interview') was then conducted on these selected individuals. The aim was to interview 3,000 pairs of participants and matched non-participants for both schemes. The interview realised 3,025 ExGZ participants and 3,098 ÜG participants. The interview information of non-participants is not used any further in this chapter.[12]

Amongst many other background information, the CATI asked all interviewees:

> *"In which of the following activities were you engaged? It is important that you mention all activities as an individual period, even if this activity only had a short duration."*[13]

The questionnaire explicitly allows for parallel or overlapping spells. The interviewees could mention the following twelve activities:

[11] I observe 17 month, if the individual entered the programme in September 2003 and was interviewed in January 2005. 20 months are observed if the measure started in July 2003 and the interview took place in February 2005.

[12] Baumgartner and Caliendo (2006) use the full data, including non-participants, to apply a kernel matching estimator.

[13] Since the question was asked in German, it reads as: *"Welche der folgenden Aktivitäten haben Sie (dann) noch gemacht? Dabei ist es wichtig, dass Sie bitte jede Aktivität, auch wenn sie nur kurz gedauert hat, als eigene Phase angeben."*

1. *self-employment*
2. *employment with social security contributions (without ABM[14] or similar employment)*
3. *mini-job (max. 400 Euro per month)*
4. *midi-job (between 400 and 800 Euro per month)*
5. *helping member of a family business*
6. *unemployed or seeking employment*
7. *ABM or similar measures of the BA*
8. *continuous training or re-qualification (with support of the BA)*
9. *continuous training or re-qualification (without support of the BA)*
10. *vocational training or higher education*
11. *retirement or early retirement*
12. *parental leave*
13. *else (e.g. house work, more than six weeks of illness, rehabilitation, etc.)*

About one percent of the interviewed individuals report a starting month for their self-employment that does not correspond with the information from the BA register. In other words, the BA register shows an earlier month in which the programmes started than the individuals report in the interviews as the month in which they started with their self-employment. This might be due to a memory error or a recording error in the course of the interview. Table 2.a in the appendix shows the starting months for self-employment and for the bridging allowance, respectively. I observe the majority of 'wrongly' reported starting months for self-employment in the first month after the support was granted. One reason for this could be that the programme was granted in, say, end-July and hence the new business actually started in early-August. If this were true, Table 2.a would actually not show 'wrongly' reported starting months. Be that as it may, I am interested in the possible success of the two active labour market policy measures – namely ÜG and ExGZ – measured by

[14] 'Arbeitsbeschaffungsmaßnahmen' (job creation measures).

the survival of supported self-employment and the labour market activities if this self-employment should be terminated. My main interest hence lies in the event history during and beyond the support period. Therefore, I redefine the starting month according to the starting month of the respective programme from the register data.

Table 2.b in the appendix provides information about the competing destinations – that is the activity in the month when supported self-employment is terminated. A majority of more than three quarters remain in self-employment within the observation period. 14 percent do not continue with supported self-employment and enter again the pool of unemployed or those looking for employment. About 5 percent carry on with some sort of dependent employment. Since there are too-few observations to analyse all destinations individually, I have to combine some of the categories.

As already mentioned, I am interested in the success of two active labour market schemes. It is hence a natural question to ask whether the unemployed, whose newly established firm had no success, came back to the pool of unemployment or whether they found their way into some sort of dependent employment. I therefore combine the activities 6, 7, and 8 into one category and label it 'unemployment'. The category 'unemployment' now embraces those being unemployed and those being on some other schemes such as ABM or further training.[15] Another category is built upon the activities 2, 3, and 4 and is labelled 'dependent employment'. This is employment with and without social security contributions. All other activities are combined into one miscellaneous category. Table 2.c in the appendix presents these four competing categories and their month-to-month transitions. The upper panel of Table 2.c depicts the transitions of ExGZ participants and the lower panel for ÜG participants.

[15]Those in ABM or further training are – according to the official definition – not unemployed. Nevertheless, they return to being customers of the BA again.

Since a majority of 77 percent does not terminate self-employment, I observe the most month-to-month transitions within self-employment. That is, I observe 48,568 and 47,698, respectively, intervals within self-employment. Of those who received ExGZ, 357 make a transition into unemployment, 142 a transition into some sort of dependent employment, and 93 make a transition into the category 'miscellaneous'. I observe more transitions into unemployment and dependent employment if the individuals were supported by ÜG; 518 become unemployed again and 176 make a transition into dependent employment.

I am most interested in two alternative labour market activities – namely unemployment and dependent employment. Transitions into the miscellaneous category are hence defined to be right-censored. In other words, as long as individuals stay in supported self-employment they contribute to the likelihood function but their hazard does not. The baseline hazard will be estimated for each interval and will be interpreted relative to the first interval, which is the reference category. In order to observe a sufficient amount of transitions in the reference category, I combine the first five months. I combine also the last five months, i.e. month 15-19, since I observe fewer transitions in the later months.[16] Combining these intervals assumes implicitly a constant hazard rate in these month. Table 2.c in the appendix documents the transitions out of self-employment. I observe relatively more transitions if the support ends (ÜG) or is reduced (ExGZ). That is, the raw data let me expect a non-monotone development of the hazard rate.

[16]Transition 19, for instance, is observed only for participants, who started in July 2003 and are interviewed in February 2004. If participants started later and/or are interviewed earlier, I observe fewer transitions.

Table 2.1: Descriptive statistics: socio-demographic and regional characteristics

	ExGZ	ÜG
gender		
female	0.455	0.258
family status		
married	0.520	0.550
children		
no children	0.623	0.638
one children	0.187	0.174
two or more children	0.190	0.188
Age categories		
18 - 29	0.212	0.186
30 - 39	0.360	0.399
40 - 49	0.298	0.304
50 - 64	0.131	0.111
regional cluster		
regional cluster I	0.223	0.220
regional cluster II	0.199	0.218
regional cluster III	0.282	0.258
regional cluster IV	0.085	0.115
regional cluster V	0.211	0.189
N	3025	3098

Source: IAB data as described in Section 2.4.1.

2.4.2 Descriptive statistics

This section describes the background characteristics of 6,123 interviewed individuals who established a new firm out of unemployment. 3,025 individuals of them were supported through ExGZ and 3,098 received support through ÜG. The background characteristics come from two different sources: the socio-demographic characteristics and qualifications are from the IEB register data and the entrepreneurial characteristics are from the interviews carried out.

Socio-demographic and regional characteristics (Table 2.1): Fewer females decided to establish a new firm out of unemployment. About 54 percent of those supported through ExGZ were males. But this gender gap even more pronounced for participants in ÜG, where only about one-quarter of participants are females. Married people are a little bit more often observed than singles. About 52 percent (ExGZ) and 55 percent (ÜG), respectively, are married. Most of them do not have any children; 62 percent (ExGZ) and

64 percent (ÜG). Participants are not equally distributed over the four age categories. Most individuals are observed in the age category between 30-39 years for both programmes. About 30 percent are between 40-49. The third most observed age category is 18-29 years and the least filled is 50-64 years. The age-distribution seems thus to be hump-shaped with a maximum in the thirties.

The BA combines regional districts in so-called strategy clusters in order to focus the labour market policies according to regional necessities. There are five strategy clusters. How these strategy clusters where constructed is described in more detail by Blien et. al. (2004). The following list describes the five strategy clusters briefly:

strategy cluster I: BA districts in east Germany with dominant labour market deficits;

strategy cluster II: West German BA districts dominated by large cities and high unemployment rate and the BA district of Dresden;

strategy cluster III: BA districts in west Germany dominated by medium-sized cities and rural areas with an average unemployment rate;

strategy cluster IV: centres in west Germany with favourable labour market conditions and high dynamics; and

strategy cluster V: BA districts in west Germany with good labour market conditions and high dynamics.

The regional distribution of supported start-ups seems to be very similar for both programmes. Roughly about one-fifth establishes in clusters I, II, and V. There is less start-up activity observed in cluster IV and more in cluster III. That is, in rural areas the entrepreneurial activity appears to be higher.

Qualifications (Table 2.2): The longer a person is unemployed the less worth is his/her human capital . For that reason, I discuss the unemployment

Table 2.2: Descriptive statistics: qualifications

	ExGZ	ÜG
unemployment career		
$<$ 3 month	0.298	0.307
$\geq$ 3 month - $<$ 1 year	0.500	0.558
$\geq$ 1 year	0.203	0.135
job qualification as categorised by the BA[b]		
top-management & tertiary education	0.130	0.242
skilled workers	0.629	0.621
unskilled workers	0.241	0.137
secondary school degree[a]		
no decree	0.025	0.012
lower secondary schooling	0.320	0.236
middle secondary schooling	0.365	0.339
upper secondary schooling	0.290	0.414
employment status before unemployment		
employment	0.576	0.735
self-employment	0.062	0.028
others but once employed	0.156	0.086
others (incl. illness; schooling; training; etc.)	0.206	0.152
N	3025	3098

[a] lower secondary schooling ('Hauptschulabschluss'), middle secondary schooling ('Mittlere Reife' or 'Fachoberschulreife'), upper secondary schooling ('Abitur' or 'Fachhochschulreife').
[b] top-management & tertiary education ('Spitzenkräfte (TOP-Management) & Kräfte mit Hochschulniveau & Kräfte mit Fachhochschulniveau'), skilled workers ('Kräfte mit Fachschulniveau & Fachkräfte'), unskilled workers ('Kräfte mit und ohne Fachkenntnisse/n').
Source: IAB data as described in Section 2.4.1.

history under the headline qualifications. About 30 percent are less than three months unemployed before they enter either of the two programmes. The majority – i.e. 50 percent (ExGZ) and 56 percent (ÜG) – are unemployed for more than three months but less than one year. Individuals, who have an unemployment career that lasts for more than one year are more frequently observed in the programme ExGZ; about 20 percent of ExGZ participants are in this category, while only 14 percent of ÜG participants have an experience in unemployment of more than one year.

The BA categorises the job qualification level of the unemployed. Since the definitions of these categories are very similar and some categories contain only a few observations, I combine them into three categories: top-management and highly skilled workers on the niveau of tertiary education; skilled workers through vocational training; and unskilled workers with and without on-the-job knowledge. Skilled workers are almost equally distributed in the two

programmes. However, the ÜG programme seems to attract more people with higher on-the-job qualifications. About 24 percent are highly qualified workers, while about 14 percent are categorised as unskilled workers. Interesting to note are reversed shares for ExGZ, where only 13 percent are highly qualified workers and 24 percent have no qualifications at all.

A similar picture is shown with respect to the secondary school degrees. There are more ÜG participants with a higher school degree than participants in ExGZ. Just 29 percent graduated from an upper secondary school and are supported through ExGZ, while more than 41 percent of ÜG participants have a similar school qualification. The same holds for the lower tail of the school qualification distribution. 35 percent of ExGZ participants have either a degree from lower secondary schooling or no degree at all, while only 25 percent of ÜG participants have such a low qualification.

There seems also to be an imbalance between these two programmes with respect to the employment status before the individuals became unemployed. Those with labour market experience from dependent employment seem to be more likely to enrol into ÜG, while the categories 'self-employed', 'others but once employed' and 'others' such as illness, schooling, training, etc. are more frequently observed for ExGZ participants.

All characteristics described so far are known by the BA, since it is register data. The interview data, however, provides not only information about the whereabouts, i.e. the month-by-month labour market status, but also about some relevant background information regarding supported self-employment, which will be described next.

Entrepreneurial characteristics (Table 2.3): Interviewees were asked a wide range of questions on how they prepared themselves for the new start-up. Since they could give multiple answers to this question the reported shares do

Table 2.3: Descriptive statistics: entrepreneurial characteristics

	ExGZ	ÜG
preparation[a]		
external support	0.556	0.728
potential customers interviewed	0.464	0.498
no extra preparation	0.124	0.064
experience in the field of self-employment		
from employment	0.707	0.786
from hobby	0.118	0.072
no experience	0.175	0.142
self-employment without any support		
yes, exactly the same	0.332	0.451
yes, but on a smaller scale	0.299	0.269
no	0.339	0.246
missing	0.030	0.034
branch		
manufacturing; building; agriculture	0.198	0.211
retail	0.173	0.158
business related services[b]	0.129	0.193
other services	0.423	0.354
missing	0.077	0.084
trade licence necessary		
yes	0.819	0.784
N	3025	3098

[a] The dummies for different aspects of preparation do not sum up to unity – as the other categories do –, since this questions allowed for multiple answers.
[b] IT-services; banking; insurance; etc.
Source: Interview data as described in Section 2.4.1.

not sum up to unity. One mean to prepare was the take up of external support – that is, start-up centres, coaching, business advisors, tax advisors, etc. – which was used by 56 percent of ExGZ participants. A higher share of ÜG participants, namely about 73 percent took advantage of some sort of external support. 46 percent of ExGZ participants and 50 percent of ÜG participants interviewed their potential customer. And no preparation at all was done by 12 percent of ExGZ participants, while 6 percent of ÜG participants did not prepare themselves.

A further question was whether the individuals had some experience in the field in which they became self-employed. 71 percent (ExGZ) and 79 percent (ÜG) answered, yes, they have some experience in the field from previous employment. A lower share – roughly about 10 percent – said that they have some experience from leisure activities such as hobby. A non-trivial share of 18 percent (ExGZ) and 14 percent (ÜG) answered that they had no experience in

the field in which they made themselves self-employed. When interviewed, the interviewees had at least 17 months experience in supported self-employment. Nevertheless, they were asked to retrospectively answer a counterfactual question – namely, whether they had become self-employed in the absence of any support. The interviewees know about the success of their start-ups and may or may not have this outcome in mind when answering this question. Hence, the information provided needs to be interpreted carefully. A share of about one-third of ExGZ participants said that they would have taken exactly the same decision and the same share of about one-third said no, they would not have chosen to become self-employed at all. In the group of ÜG participants these two shares are not balanced. A large share of more than 45 percent said that they would have chosen exactly the same start-up in the absence of any support. And about one-quarter said, no, they would not have set up their firm without support from the BA. These raw descriptive numbers – even though they need to be interpreted carefully – may indicate that ÜG participants have much more confidence in their success in self-employment. Since ÜG participants are better-qualified, they might also be better-informed about the circumstances of their start-ups and may thus foresee its success, and will hence answer that they would have established a start-up even without any support. But if it is true that such a high share had decided to become self-employed out of unemployment even without any support, the question is whether the resources to finance the ÜG programme were targeted at the right people. Be this as it may, this is an ex-ante interpretation of an ex-post question and will not be pursued any further.

The branch of industry in which the individuals became self-employed was also asked about in the interview. It turns out that most interviewees – 42 percent (ExGZ) and 35 percent (ÜG) – answered that they had established their start-up in the category 'other services'. This category spans quite a wide

range of businesses: cleaning services, health care, and facilities management, but also business advisor, marketing, and architecture offices. A disadvantage of this category is hence that it covers branches of business that require rather little human capital – such as house-keeping services – but also services that require quite a high level of qualification – such as marketing or architecture offices. A much lower share of 13 percent (ExGZ) and 19 percent (ÜG) established their business in the area of business related services.[17] Roughly about 20 percent of participants from both programmes set up their business in manufacturing, building or agriculture.

Almost all start-ups need a trade licence ('Gewerbeschein'). However, there are some lines of business for which no trade licence is necessary. Such lines of business are so-called 'Freie Berufe' (liberal professions), that is journalists, architects, artists, etc., but also most businesses in agriculture. 82 percent of ExGZ participants needed a trade licence for their start-ups. This share is, at 78 percent, a little lower for ÜG participants.

2.5 Results

ÜG participants are described as more qualified on average than ExGZ participants. Before I start to lay out my empirical results, I discuss therefore a potential selection bias that might be introduced through the attractiveness of ÜG to higher qualified people. I then carry on to analyse two alternative labour market activities, namely unemployment and dependent employment, if supported self-employment is terminated. The other side of this coin is the survival in self-employment, which will be discussed before I turn to the marginal effects that background characteristics may have on the sustainability of supported self-employment.

[17] I combined the categories 'traffic and logistic'; 'credit and insurance'; 'IT services' and labelled them business related services.

2.5.1 Selection

The preceding section described the background characteristics of participants in ExGZ and ÜG, respectively, and revealed that the ÜG programme attracts people with a much higher level of qualification and experience. For instance: the share of participants with a job qualification on the level of tertiary education is higher in ÜG; and so is the share of participants having completed upper secondary schooling; ÜG participants have on average also shorter unemployment spells; and were more often in dependent employment before they became unemployed; and ÜG participants are less often found in the youngest age category. The attractiveness of ÜG to better qualified people is obvious if I recall the institutional settings of the two programmes, which were discussed in more detail in section 2.2. People with a higher level of qualification and more labour market experience receive ceteris paribus a higher income (Lauer and Steiner, 1999; Lauer and Steiner, 2001) and thus also a higher support through ÜG. Additionally, if better qualified people are also able to generate a higher income out of self-employment, they risk 'losing' their support due to the 25k rule in ExGZ. However, a selection bias would be introduced if better-qualified people also had lower hazard rates. This would be the case if better-qualified people are more likely to have transferable and/or multiple skills, which are necessities for becoming a successful entrepreneur (Lazear, 2003). However, the transferability and multiplicity of skills are not observable.

Section 2.2 discussed rational programme choice to be determined by the amount of the unemployment transfer. Thus, I can exploit the unemployment transfer as an exclusion restriction in a Heckman (1979) two-stage selection model.[18]

In the first step I estimate programme choice by a probit model using the

[18] A selection model for binary outcomes in the second step was first applied by van de Ven and van Praag (1979).

pooled sample of both ÜG and ExGZ participants.

$$\mathrm{P}(prg = 1|\mathbf{X}_i, transfer_i) = \Phi_{probit}(\beta\mathbf{X}_i + \gamma transfer_i), \tag{2.10}$$

where $\mathbf{X}$ is a vector capturing the regressors as described in section 2.4.2 and $transfer$ is the daily unemployment transfer received before one of the two programmes is joined. $\Phi_{probit}(\cdot)$ is the probit function. Equation (2.10) estimates thus the probability to participate in the respective programme.

Table 2.d in the appendix documents the results from the first stage regression. The left column shows the estimation results for ExGZ versus ÜG and the right column for ÜG versus ExGZ. Since both regressions use the same sample and the same specification, all coefficients have the same size and significance but with reversed signs. The daily unemployment transfer explains programme choice significantly, as expected. The propensity to prefer ÜG over ExGZ increases with the unemployment transfer. Moreover, the unemployment transfer has a very strong effect on programme choice because the propensity to choose ÜG rises by 1.6 percentage points on average if the daily unemployment transfer increases by one Euro.

The unemployment transfer does not only need to have a strong explanatory power on programme choice to be a valid exclusion restriction in a two-stage selection model, it does also need to have no partial effect on the hazard rate (Wooldridge, 2003, p. 589). Table 2.e documents this auxiliary regression and shows on the bottom that the daily unemployment transfer is statistically insignificant, given all the other covariates. I interpret therefore the daily unemployment transfer to be a valid exclusion restriction.

Using the estimates from Equation (2.10), I calculate the inverse Mill's ratio

$$\xi_i = \frac{\phi(\hat{\beta}\mathbf{X}_i + \hat{\gamma} transfer_i)}{\Phi(\hat{\beta}\mathbf{X}_i + \hat{\gamma} transfer_i)}, \tag{2.11}$$

where ϕ is the standard normal probability distribution function (pdf) and Φ

Table 2.4: The estimated coefficient of the inverse Mill's ratio[a,b,c]

	ExGZ		ÜG	
	coeff.	s.e.	coeff.	s.e.
ρ^d	−0.411	(0.767)	0.491	(0.494)
NT	49,016		47,980	

[a] The level of significance is: ** 1 %; * 5 %; + 10 %.
[b] As recommended by Cameron and Trivedi (2005, p. 550), the estimates are bootstrapped using 1,000 replications.
[c] The full estimation is documented in Table 2.f in the appendix.
[d] ρ is the coefficient on the inverse Mills ratio.

the standard normal cumulated distribution function (cdf).

In the second step I estimate the hazard rate to terminate self-employment and include the calculated inverse Mill's ratio as an additional regressor.

$$\lambda_j(t|\mathbf{T}_{it}, \mathbf{X}_{it}, \xi_i) = \frac{\exp(\gamma_j \mathbf{T}_{it} + \beta \mathbf{X}_{it} + \rho\xi_i)}{1 + \exp(\gamma_j \mathbf{T}_{it} + \beta \mathbf{X}_{it} + \rho\xi_i)}. \tag{2.12}$$

Table 2.4 shows the estimated coefficient of the inverse Mill's ratio (ρ) and Table 2.f in the appendix documents the full estimation results of Equation (2.12). The standard errors are bootstrapped using 1,000 replications because the usual standard errors reported from the regression of equation (2.12) are incorrect (Cameron and Trivedi, 2005, p. 550).

If I had no exclusion restriction or if the daily unemployment transfer would not sufficiently explain programme choice, the inverse Mill's ratio in Equation (2.12) would be likely to be collinear with $\mathbf{X}_{it}$, since it were approximately linear over a wide range of its arguments.[19] But multicollinearity can easily be detected by calculating the condition number (Cameron and Trivedi, 2005, p. 551). Using the data matrix consisting of $\mathbf{X}_{it}$ and ξ_i, the condition number is 3.76 for the sample of ExGZ participants and 4.45 for the sample of ÜG participants. Both condition numbers are well below the critical threshold.[20] I conclude therefore multicollinearity to be not present in the second-step and

[19] Nawata (1993), Nawata and Nagase (1996), Leung and Yu (1996), for example, make this point drawing on finding from Monte Carlo experiments and Puhani (1997) surveys this literature.

[20] Greene (2003, p. 58) suggests a threshold of 20 and Cameron and Trivedi (2005, p. 350) suggest a threshold of 100 (sic!). Whichever threshold is used, the condition number of my data remains comfortably below.

standard errors are thus not inflated.

A simple t-test on ρ provides an easy measure whether selection correction is needed or not. I cannot reject the null hypothesis that ρ is equal to zero. Therefore, there appears to be no selection bias that needs to be corrected.

Rational programme choice is driven by unemployment transfer and I speculated that this fact could introduce a selection bias if those with higher transfers have unobservable characteristics that influence their hazard rate. Since I am observing a huge portfolio of different explanatory variables, I am explaining the selection process sufficiently. Therefore, I do not need to correct for selection when estimating the hazard rates.

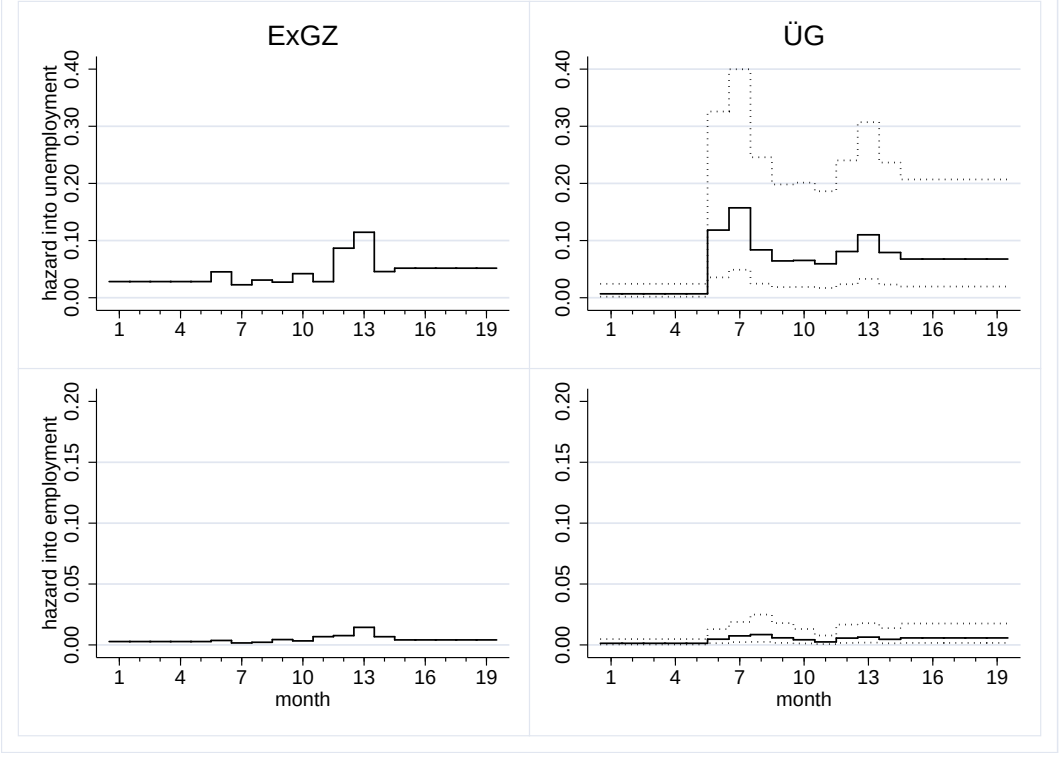
2.5.2 Transition rates into unemployment and employment

Estimation results for the competing transition rates into unemployment and dependent employment – as stated in Equation (2.8) – are reported in Table 2.g and Table 2.h in the appendix. Table 2.h controls for unobserved heterogeneity and in Table 2.g the variance of ε_i is restricted to zero, i.e. for a standard transition model not controlling for unobserved heterogeneity.[21] The left two columns show the coefficient vectors – for the transitions into unemployment and employment – for the sample of ExGZ participants and the right two columns for the sample of ÜG participants. At the bottom the tables, I report the log-likelihood of the respective estimation.

Comparing the two log-likelihoods, I note that unobserved heterogeneity does not seem to be present in the estimation for ExGZ participants. That is, neither the log-likelihood could be increased nor is the estimated variance of ε_i significantly different from zero. The hazard of ÜG participants, on the other hand, seems to be – inter alia – driven by unobserved factors, since the log-

[21] The individual effects (ε_i) are assumed to be normally distributed, with $\psi \equiv \text{var}(\varepsilon_i)$ (c.f. section 2.3 for further discussion).

Figure 2.3: Hazard rates into unemployment and employment for both programmes



ExGZ: The baseline hazard probability is calculated from equation (2.8) with $\mathbf{X}_{it} = 0$ and $\varepsilon_m = 0$.
ÜG: The baseline hazard probabilities are calculated from equation (2.8) with $\mathbf{X}_{it} = 0$. The dashed lines draw the baseline hazard with plus/minus one standard deviation of $\widehat{\text{var}}(\varepsilon_i)$.

likelihood could be significantly increased[22] and the variance of ε_i significantly estimated. I thus present in the following discussion estimation results for ExGZ participants from a restricted model[23] and for ÜG participants from a model controlling for unobserved heterogeneity.

Figure 2.3 displays the reference hazard rates[24] for participants in both programmes. The hazard rates start to increase in month 12 and jump to a higher level in month 13 to 11.5 percent for the transition into unemployment

[22]This is evaluated by the Akaike information criterion ($AIC = -2\ln L + 2q$), where $\ln L$ is the estimated log-likelihood and q the number of estimated parameters. The model with the smaller AIC is preferred (Cameron and Trivedi, 2005, p. 278).

[23]In the restricted model the IIA assumption needs to hold. I cannot reject the null hypothesis with $\chi^2_{(44)} = 10,56$, hence the IIA assumption is not violated (Hausman and McFadden, 1984).

[24]These reference hazard rates are derived from the coefficient vectors reported in Table 2.g and 2.h and evaluated for $\mathbf{X}_{it} = 0$.

and to 1.4 percent for the transition into employment. Although the hazard rates in the months before the jump could not be estimated to be statistically different from each other,[25] the hazard rate into employment is on an economically unimportant low level of about 0.3 percent, while the hazard rate into unemployment is about 3 percent. The jump in the hazard after one year of support coincides with the reduction of the support. That is, it is likely that participants used ExGZ to extend the period in which they could receive transfers. When the ExGZ payment reduces in the second year, they decide to give up their business to become unemployed again and to receive other transfers, which may be higher than the reduced ExGZ payment. However, I observe also an increased hazard rate into employment, although at a much lower level, which may indicate that participants could not generate enough income from their enterprises and that the reduction might have forced them to close their unsuccessful businesses. It may also be the case that registering as unemployed again, may result in a higher income than running an unsuccessful business with the reduced rate of ExGZ.

On the right-hand side of Figure 2.3, the hazard rates are drawn for ÜG participants. The solid line shows the hazard rate for $\text{var}(\varepsilon_i) = 0$ and the dashed lines for plus/minus one standard deviation of the estimated $\widehat{\text{var}}(\varepsilon_i)$. Two spikes describe the hazard rate of ÜG participants, the first after six and the second after twelve months. The first spike coincides with the end of the ÜG support. The second spike, which is statistically not different from the first spike,[26] cannot be explained by the end of an observed support.[27] Nevertheless, a tentative explanation could be that contracts (for franchise, offices, etc.) can be terminated for the first time after twelve months and

[25] That is, the null hypothesis that the coefficients on month t are equal cannot be rejected.

[26] That is, the null hypothesis that the coefficient of month 7 is equal to the coefficient of month 13 could not be rejected for either coefficient vector at the five percent level of significance.

[27] The second spike is observed after 12 months which is the third quarter of 2004.

that ÜG participants keep on running their unsuccessful businesses until they can opt-out of these contracts for the first time. I stress that this may also explain – at least partly – the spike observed for ExGZ participants. The hazard rate into unemployment is about 15.7 percent in month 7 and about 11.0 percent in month 13. Adding less favourable unobserved factors of one standard deviation, however, raises the hazard rate to 40.0 percent in month 7 and 30.7 in month 13 and reduces it to a low level of 4.9 (3.3) percent in month 7 (13) if more favourable unobserved factors are added. The hazard rate into employment rises to an economically unimportant low level of 0.7 percent in month 7 on average.

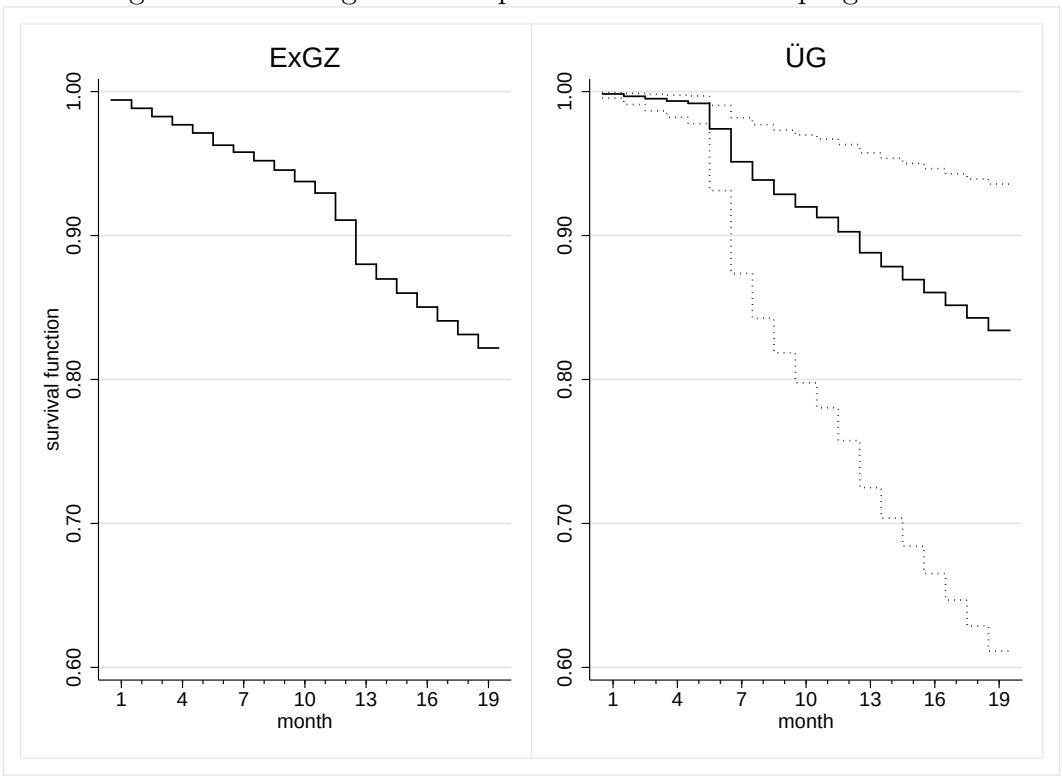
2.5.3 Survival

Having described the two competing hazards into unemployment and dependent employment, I turn now to the determinants of the survival probability in self-employment. That is, I am answering the question, which background characteristics influence the survival in (supported) self-employment.

Analysing the competing hazards in the preceding subsection, I had to treat those individuals, who made a transition into a 'miscellaneous' labour market activity as right-censored (see Section 2.4.1 for further details). However, those individuals, who stay in self-employment until the observation window closes, are also treated as right-censored and included in the calculation of the survival probability. Thus, calculating the predicted survival from the estimates from the competing transition rate model would clearly upward bias the survival probability. I therefore combine all possible transitions out of self-employment and estimate a so-called single risk model that allows me to analyse unbiased probabilities of survival in supported self-employment.[28]

[28]Redefining my data, I gain back one individual (ExGZ), who made a transition into the miscellaneous category in the first month and was therefore not used for the estimation in section 2.5.2.

Figure 2.4: Average survival probabilities for both programmes



ExGZ: The survival function is calculated from equation (2.5) with $\mathbf{X}_{it} = \bar{\mathbf{X}}_{\cdot t}$ and $\varepsilon_i = 0$.
ÜG: The solid line draws the survival function with $\mathbf{X}_{it} = \bar{\mathbf{X}}_{\cdot t}$ and $\varepsilon_i = 0$. The dashed lines draw the survival function with plus/minus one standard deviation of $\widehat{\text{var}}(\varepsilon_i)$.

Table 2.i and 2.j presents the estimation results for the duration model in supported self-employment for ExGZ and ÜG participants.[29] The sign on the coefficients indicates the effect on the duration. That is, a negative coefficient has a positive effect on the duration and a positive coefficient has a negative effect on the duration.[30]

Figure 2.4 displays the survival functions over time for an 'average' participant.[31] The graphs on the left-hand side are for the ExGZ participants and on the right-hand side for the ÜG participants. The survival function for ÜG participants is drawn for $\varepsilon_i = 0$, as well as plus/minus one standard

[29] As in section 2.5.2, I find unobserved heterogeneity for ÜG participants but not for ExGZ participants. See the discussion in section 2.5.2 for the comparison of log-likelihoods using the AIC.

[30] This reversion of signs comes from the specification in Equation (2.8), where $J = 1$, i.e. the coefficients show actually the effect on the hazard rate.

[31] This is at the mean of the covariates, i.e. $\mathbf{X}_{it} = \bar{\mathbf{X}}_{\cdot t}$.

deviation of $\widehat{\text{var}}(\varepsilon_i)$, since I found evidence for unobserved heterogeneity in the sample of ÜG participants. The survival function is monotone decreasing because it is the product over time of the survival probabilities (see also Equation (2.5)). The survival function of ExGZ participants decreases faster around the months 12 and 13, which are the months with the highest hazard rates. The slope in the second year is also steeper than in the first year indicating that the survival probability decreases faster in the second year than in the first year. ÜG participants, on the other hand, have an almost horizontal survival function in the first five months. In month six and seven it falls faster than in the months thereafter. The survival functions for unobserved heterogeneity are impressive. Having favourable unobserved characteristics results in a relatively flat survival function, while having less favourable unobserved characteristics results in survival function that falls relatively fast. This implies that after 19 months as much as 94 percent of ÜG participants with favourable unobserved characteristics still run their businesses. Whereas only 61 percent of ÜG participants with less favourable characteristics are still self-employed after 19 months. The average is 83 percent and thus similar to the survival of ExGZ participants, which is 82 percent. However, I stress that support through ExGZ is still ongoing, although on a smaller scale.

2.5.4 Marginal effects

The shape of the survival function was drawn for an average participant. Most interesting, however, is which background characteristics positively or negatively influence the survival in (supported) self-employment. Table 2.5 therefore presents the implied marginal effects of the background characteristics. Since all covariates are either dummy variables or categorical variables, I calculate the effect if one variable changes from zero to unity, holding all other variables constant at zero. As I have discussed above, the risk of ter-

minating self-employment is the highest in month 13 for ExGZ participants and in month 7 for ÜG participants. Since the marginal effects may change over time, I calculate the effects for the months, when the amount of ExGZ is substantially reduced and ÜG expires, respectively. Table 2.5 thus presents the change of the reference hazard rate for these months and Tables 2.k-2.n in the appendix document the marginal effects for all months. The marginal effects are reported for all background characteristics. Needless to mention, if the underlying coefficient is insignificant, the marginal effect is insignificant too. Thus, the marked significance levels refer to the respective estimates in Table 2.i and 2.j in the Appendix.

The reference hazard rate of terminating self-employment in month 13 is 14.6 percent for ExGZ participants if all explanatory variables are set to zero. Table 2.5 presents three marginal effects for ÜG participants. Column three shows the average effects, i.e. ε_i is set to zero and columns two and four present the marginal effects for plus/minus one standard deviation. That is, column two presents marginal effects for ÜG participants, whose unobserved heterogeneity influences the hazard rate negatively by one standard deviation. And column four calculates the effects for unobserved factors that are less favourable, i.e. increase the hazard rate out of self-employment. On average the reference hazard rate for ÜG participants is 13.0 percent in month 7 and varies between 29.1 and 5.2 percent for plus/minus one standard deviation. Unobserved factors have hence quite a substantial influence on the estimated marginal effects.

Socio-demographic and regional characteristics: Being married reduces the reference hazard rate in month 13 by 2.5 percentage points for ExGZ participants. A more significant effect is observed for the age categories. The older participants in ExGZ, the less likely they are to terminate their start-up

Table 2.5: Marginal effects at the end of a support period in percentage points[a,b,c]

	ExGZ in month 13	ÜG in month 7		
		-1 S.D.[e]	$\varepsilon_i = 0$[e]	+1 S.D.[e]
reference hazard rate[d] (in percent)	14.597	5.167	13.006	29.089
socio-demographic and regional characteristics				
female	−0.951	0.129	0.298	0.540
married	−2.459∗	−0.706	−1.650	−3.080
one children	−1.257	0.049	0.112	0.204
two or more children	2.357	0.682	1.558	2.778
age category (30 - 39)	−5.246 ∗∗	−0.612	−1.427	−2.656
age category (40 - 49)	−5.946 ∗∗	0.087	0.201	0.366
age category (50 - 64)	−6.661 ∗∗	1.800	4.039	6.964
strategy cluster II	−0.153	1.342	3.035	5.304
strategy cluster III	1.005	0.673	1.538	2.744
strategy cluster IV	0.509	−0.612	−1.427	−2.656
strategy cluster V	−1.556	0.643	1.470	2.625
qualification				
unempl. career ($\geq$ 3 month - $<$ 1 year)	−1.329	0.775	1.767	3.142
unemployment career ($\geq$ 1 year)	−1.787	1.740∗	3.908∗	6.750∗
tertiary education	−1.402	−1.783∗	−4.237∗	−8.219∗
skilled workers	−4.370 ∗∗	−1.213∗	−2.855∗	−5.425∗
no degree	−0.879	−1.275	−3.006	−5.725
middle secondary schooling	−3.784 ∗∗	0.056	0.128	0.233
upper secondary schooling	−6.449 ∗∗	0.374	0.858	1.545
self-employed before unemployed	1.995	4.565 ∗∗	9.823 ∗∗	15.714 ∗∗
others but once empl. before unemployed	4.294 ∗∗	0.516	1.182	2.120
others before unemployed	0.669	1.360∗	3.074∗	5.369∗
entrepreneurial characteristics				
external support	−1.801	−1.167∗	−2.746∗	−5.210∗
potential customers interviewed	4.175 ∗∗	1.594 ∗∗	3.589 ∗∗	6.225 ∗∗
no extra preparation	3.725+	−1.318	−3.110	−5.932
experience from employment	−2.217+	−1.835 ∗∗	−4.365 ∗∗	−8.483 ∗∗
experience from hobby	0.009	0.101	0.233	0.423
without support: yes exactly the same	−5.685 ∗∗	−4.044 ∗∗	−9.983 ∗∗	−21.211 ∗∗
without support: yes, but on a smaller scale	−5.993 ∗∗	−2.786 ∗∗	−6.733 ∗∗	−13.575 ∗∗
manufacturing; building; agriculture	−5.722 ∗∗	−3.101 ∗∗	−7.533 ∗∗	−15.381 ∗∗
capital intensive services	−2.462	−0.330	−0.767	−1.414
other services	−1.855	−2.096 ∗∗	−5.007 ∗∗	−9.828 ∗∗
trade licence necessary	3.914+	0.920	2.093	3.705

[a] Since all background characteristics are described by dummies and categorial variables the marginal effect is calculated as the difference between the reference category and a discrete change in the respective variable, holding all other variables constant at zero.

[b] ExGZ is reduced after twelve months but is ongoing on a smaller scale. ÜG, on the other hand, ends after six months. Month 13 and month 7, respectively, are hence the first months with either a reduced support or no support.

[c] Significance level of the underlying estimation: ** 1 percent; * 5 percent; + 10 percent.

[d] The reference hazard rate is calculated for $\mathbf{X}_{it} = 0$.

[e] Unobserved heterogeneity is assumed to be normally distributed. To depict how these unobserved factors alter the hazard probability, I calculate the marginal effects for ± one standard deviation (c.f. Table 2.j).

firm. The reference hazard rate is reduced by as much as 6.7 percentage points if an ExGZ participant is between 55 and 64 years old. That is the hazard rates of ExGZ participants in the age category 55-64 are almost the half of otherwise similar participants in the age category 18-29. Older people may use self-employment to bridge their remaining time till retirement, because they may have fewer 'outside options'. Thus, the lower hazard rate of elderly people does not necessary imply that their start-ups are more successful. Turning to participants in ÜG, I find none of the socio-economic characteristics to influence the reference hazard rate significantly. The strategy clusters describing the labour market dynamics do not appear to have a significant effect on the hazard.

Qualification: The unemployment career has only an impact on the reference hazard rate of ÜG participants and this holds only if they experienced an unemployment duration exceeding one year. If they were more than one year unemployed before they joined the programme, their hazard rate is on average 3.9 percentage points higher than the reference hazard rate. An interesting finding is that ExGZ participants with a job qualification equivalent to tertiary education, have a duration probability that is not significantly different from unskilled workers (reference category); but skilled workers have a significantly lower hazard rate than their unskilled colleagues. The difference between them is 4.4 percentage points. Having a tertiary education reduces the reference hazard rate of ÜG participants between 1.8 and 8.2 percentage points depending on the distribution of the unobserved characteristics. The duration probability also rises with the level of the obtained school degree but only for ExGZ participants. That is, ExGZ participants, who are holding a degree from middle secondary schooling, have a lower hazard rate than participants holding a degree from lower secondary schooling. The hazard rate

of ExGZ participants with a degree from upper secondary schooling reduces by 6.5 percentage points and for those having a degree from middle secondary schooling it reduces by 3.8 percentage points. A further background characteristic is the employment status before unemployment. Having some experience of self-employment before unemployment raises the baseline rate substantially for ÜG participants. That is, ÜG participants, who gave up to be self-employed before they became unemployed have a good chance of failing again with their new attempt at self-employment. Their risk of terminating the new firm is on average 9.8 percentage points higher than the risk for people who have experience of employment before they became unemployed. Having less favourable unobserved factors of one standard deviation and having some experience in self-employment, which has a marginal effect of 15.7, results in an estimated hazard rate of as much as 44.8 percent in month 7, which can be regarded as being dramatically high. This effect, on the other side, is not estimated to be significant for ExGZ participants.

Entrepreneurial characteristics: Having enjoyed some kind of external support – such as seminars, coaching, start-up centres, and/or business or tax advisors – reduces the hazard rate of ÜG participants by 2.7 percentage points on average. Having interviewed potential customers before setting up the new firm may make the new entrepreneurs over confident about the success of their star-ups. That is, answering this question with "yes" indicates a significant increase in the hazard rate by 4.2 (3.6) percentage points for ExGZ (ÜG) participants. Experience in the field of self-employment has only a weakly significant effect on the hazard rate of ExGZ participants. But ÜG participants, who have experience from dependent employment, have a significantly lower hazard rate, which decreases by 4.4 percentage points. I stressed already in section 2.4.2 that the variables on 'the self-employment decision without any

support' need to be interpreted carefully, since this is a retrospectively asked question on a counterfactual outcome. Nevertheless, participants in both programmes, who answered that they would have established a new firm even without any support, have a significantly lower hazard rate. This may indicate a misuse of the programme, since both programmes aim to motivate unemployed people to become self-employed and not to support those who had become self-employed anyway. Finally, I control also for the branch of business and whether a trade licence was necessary for the start-up. Needing a trade licence for ExGZ participants has only a weakly significant effect on the hazard rate of 3.9 percentage points, while it does not influence the hazard rate of ÜG participants.

2.6 Conclusion

This chapter analysed two alternative active labour market policy programmes that support self-employment out of unemployment. One of these programmes (ÜG) depends on transfers the participants would have received if they were still unemployed and supports a shorter period. The other programme (ExGZ) runs for up to three years and does not depend on the unemployment transfers. The difference in the design of the programmes attracts people with different qualification patterns. That is, participants in ÜG are on average better qualified than participants in ExGZ.

Rational programme choice is driven by unemployment transfer and I speculated that this fact could introduce a selection bias if those with higher unemployment transfers have unobservable characteristics that influence their hazard rate. Testing for a potential selection bias by a two-stage selection model, however, suggested selection to be not a problem in my sample.

Modelling the transition rates out of (supported) self-employment into unemployment and into dependent employment, I observe negligible low rates

of entering the first labour market. The risk of becoming unemployed again, on the other hand, is also relatively low but jumps to a higher level at specific points in time. The hazard rate jumps to a higher level for participants in both programmes after one year and for ÜG participants also after six months, when the support ends. That is, when the support is reduced (ExGZ) or when the support ends (ÜG) participants are likely to return to the pool of unemployed. One tentative explanation for this could be that participants extended their received transfers by participating in either programme. However, since a second spike is estimated for ÜG participants for which I do not have any institutionalised explanation, it is possible that ExGZ participants have another motive, when terminating self-employment after one year. It might be that some of the self-employed of both programmes that terminate after one year would have preferred to end self-employment earlier, since their business was not a success but they could not opt-out since they had signed contracts for offices or franchises, which could only opted out of after one year. If this is true, the estimated spike after 12 months is not entirely due to the reduced support.

I am able to observe up to 19 month-to-month transitions. When the observation window closes, as much as 82 percent of ExGZ participants are still self-employed. This high survival might be due to the fact that the support is still ongoing, if the start-up firm does not generate more than 25,000 Euro. However, the survival of ÜG participants is 83 percent on average. Although they run their businesses more than one year without any support. Thus, the survival of ExGZ participants is on a comparably high level. Moreover, ÜG participants can be divided into entrepreneurs with favourable unobserved characteristics and entrepreneurs with characteristics that are not considered advantageous in running a business. As many as 94 percent of those with favourable characteristics survive in self-employment, while only 61 percent

with disadvantageous characteristics are still self-employed at the end of the observation period.

It is too early for a final evaluation of the ExGZ programme, since it is still ongoing. That is, I am observing programme participants in their second year of support but the programme supports them for up to three years. Nevertheless, the sustainability of supported self-employment out of unemployment is very similar in both programmes.

The German parliament is currently resolved to merging ÜG and ExGZ. The new scheme is planned to run for nine months in the first phase of support and the amount of the support is planed to be the unemployment transfer plus a fixed amount of 300 Euro for social security contributions. The second phase lasts for six further months and grants only the fixed amount of 300 Euro for social security contributions.[32] ÜG and ExGZ are attractive for people with different skill pattern and the institutional setting of both programmes seems to fit the necessities of the new entrepreneurs very well, since both programmes are successful in terms of sustainability. Those, who had chosen ÜG are supported under supported the new scheme for nine months longer, although they would not necessarily require a longer support period. Moreover those, who had chosen ExGZ because of their low unemployment transfer may now be offered a support that is too low to secure their starting phase. The new programmes thus risks the success of two successful programmes.

[32]This information is taken from a web page that is maintained by the Federal Ministry of Economics and Technology (www.existenzgruender.de/05/02/bundesweit/01881/index.php accessed: June 26, 2006).

2.A Appendix

2.A.1 Derivation of the threshold from which ÜG becomes the rational choice

If participants are rational, they choose the programme that offers them the highest financial support.

Expressed in net-present value, the support of the programme ExGZ is

$$ExGZ_{NPV} = \sum_{t=1}^{t=12} 600R + \pi_2 \sum_{t=13}^{t=24} 360R + \pi_3 \sum_{t=25}^{t=36} 240R, \tag{2.13}$$

where π_j are the expected probabilities to receive ExGZ in year j, due to the 25k rule,[33] and R is the discount factor $(1+r)^{-(t-1)}$, with r as the discount rate.[34]

The support of the programme ÜG depends on the unemployment transfer ($transfer$), which is topped by a lump sum for social security contributions (ω). Expressed in net-present value, the support is

$$UEG_{NPV} = \sum_{t=1}^{t=6} transfer(\omega)R. \tag{2.14}$$

From Equations (2.13) and (2.14) it follows that participants are indifferent if the net-present value of both supports equates, i.e. if

$$ExGZ_{NPV} = UEG_{NPV}. \tag{2.15}$$

Inserting Equations (2.13) and (2.14) into Equation (2.15) and rearranging yields the critical threshold (c) of the unemployment transfer at which participants are indifferent between both programmes.

$$\begin{aligned} c &= transfer \\ &= \frac{\sum_{t=1}^{t=12} 600R + \pi_2 \sum_{t=13}^{t=24} 360R + \pi_3 \sum_{t=25}^{t=36} 240R}{\sum_{t=1}^{t=6} \omega R}, \end{aligned} \tag{2.16}$$

If the unemployment transfer exceeds this threshold (c), ÜG becomes the rational choice.

[33] Since the ExGZ cannot be granted in the third year if it was not granted in second year, $\pi_3 = \pi_2 \times \tilde{\pi}_3$, where $\tilde{\pi}_3$ is the probability of receiving the support in the third year.

[34] The first month is not discounted, due to $t-1$.

2.A.2 Tables

Table 2.a: Non-corresponding starting months[a]

Start according to the interview	Start according to the BA register			
	July 2003	August 2003	September 2003	Total
August 2003	16	0	0	16
September 2003	5	12	0	17
October 2003	1	2	10	13
November 2003	1	4	3	8
December 2003	0	1	0	1
January 2004	2	0	0	2
February 2004	0	1	1	2
May 2004	1	0	1	2
June 2004	1	1	0	2
July 2004	0	0	1	1
September 2004	0	1	0	1
November 2004	0	1	0	1
Total	27	23	16	66

[a] This table displays those observations, which report a starting month for self-employment that does not correspond with the starting month of their support.
Source: IAB and interview data as described in Section 2.4.1.

Table 2.b: Alternative activities if supported self-employment is terminated[a]

Destination		Frequency	Percent
1.	self-employment	4,756	77.67
2.	employment with social security liabilities	270	4.41
3.	mini-job (max. 400.- Euro per month)	42	0.69
4.	midi-job (max. 800.- Euro per month)	6	0.10
5.	helping member of the family	3	0.05
6.	unemployed or seeking for employment	859	14.03
7.	ABM or similar measures from the BA	8	0.13
8	further or re-education (with support from BA)	8	0.13
9	further or re-education	19	0.31
10.	vocational training or higher education	21	0.34
11.	retirement or pre-retirement	7	0.11
12.	parental leave	15	0.24
13.	else	109	1.78
	Total	6,123	100.00

[a] The destinations are described in more detail in the text.
Source: IAB and interview data as described in Section 2.4.1.

Table 2.c: Transitions per month into four competing categories

transition[a]	self-employment	unemployment[b]	dependent employment[c]	miscellaneous
		ExGZ (start-up subsidy)		
1	3,011	10	3	1
2	2,993	10	8	0
3	2,971	12	8	2
4	2,938	26	3	4
5	2,913	16	5	3
6	2,880	23	7	2
7	2,857	11	3	4
8	2,834	15	4	3
9	2,808	13	8	3
10	2,778	20	6	3
11	2,748	13	12	4
12	2,681	41	14	12
13	2,572	53	26	27
14	2,528	19	11	5
15	2,498	14	5	6
16	2,460	23	6	5
17	2,195	21	5	4
18	1,335	15	4	5
19	568	2	4	0
total	48,568	357	142	93
		ÜG (bridging allowance)		
1	3,094	2	2	0
2	3,083	6	4	1
3	3,074	3	5	1
4	3,056	8	9	1
5	3,030	14	6	4
6	2,911	91	16	10
7	2,777	99	22	9
8	2,705	43	21	3
9	2,654	30	13	7
10	2,609	28	9	5
11	2,571	24	5	5
12	2,526	31	11	2
13	2,462	40	12	9
14	2,422	26	8	5
15	2,388	17	9	6
16[d]	2,355	16	12	5
17[d]	2,129	24	4	2
18[d]	1,302	15	8	5
19[d]	550	1	0	1
total	47698	518	176	81

[a] The first transition is from month_1 to month_2. Hence there are 19 transitions for 20 months.
[b] Including other measures such as ABM or supported further education.
[c] Employment with social security contributions, mini-job, and midi-job.
[d] Transition 19, for instance, is observed only for participants, who started in July 2003 and are interviewed in February 2004. If participants started later and/or are interviewed earlier, I observe fewer transitions.
Source: IAB and interview data as described in Section 2.4.1.

Table 2.d: Programme choice: Probit estimates for the first stage of the two-stage selection model[a,b]

	ExGZ vs. ÜG	ÜG vs. ExGZ
constant	1.327***	−1.327***
	(0.143)	(0.143)
socio-demographic and regional characteristics		
gender (ref. male)		
female	0.262***	−0.262***
	(0.047)	(0.047)
family status (ref. non-married)		
married	−0.012	0.012
	(0.043)	(0.043)
children (ref. none)		
one children	0.053	−0.053
	(0.053)	(0.053)
two children	0.098*	−0.098*
	(0.054)	(0.054)
age category (ref. 18-29)		
30 - 39	0.038	−0.038
	(0.052)	(0.052)
40 - 49	0.153***	−0.153***
	(0.058)	(0.058)
50 - 64	0.321***	−0.321***
	(0.075)	(0.075)
strategy clusters (ref. typ I)		
typ II	0.104*	−0.104*
	(0.059)	(0.059)
typ III	0.221***	−0.221***
	(0.054)	(0.054)
typ IV	0.138*	−0.138*
	(0.074)	(0.074)
typ V	0.246***	−0.246***
	(0.060)	(0.060)
qualification		
unemployment career (ref. $>$ 3 months)		
3 months - $<$ 1 year	−0.106***	0.106***
	(0.041)	(0.041)
$\leq$ 1 year	0.042	−0.042
	(0.062)	(0.062)
job qualification as categorised by the BA (ref. unskilled workers)		
tertiary education	−0.330***	0.330***
	(0.079)	(0.079)
skilled workers	−0.143***	0.143***
	(0.052)	(0.052)
secondary school degree (ref. lower secondary schooling)		
no degree	0.136	−0.136
	(0.138)	(0.138)
middle secondary schooling	−0.089*	0.089*
	(0.048)	(0.048)
upper secondary schooling	−0.114 * *	0.114 * *
	(0.057)	(0.057)
employment status before unemployment (ref. employment)		
self-employment	0.657***	−0.657***
	(0.090)	(0.090)
others but once employed	0.248***	−0.248***
	(0.057)	(0.057)
others	0.122***	−0.122***
	(0.047)	(0.047)

Continued

Table 2.d continued.

	ExGZ vs. ÜG	ÜG vs. ExGZ
entrepreneurial characteristics		
preparation		
external support	−0.446***	0.446***
	(0.041)	(0.041)
customers interviewed	0.007	−0.007
	(0.038)	(0.038)
no extra preparation	0.118	−0.118
	(0.076)	(0.076)
experience in the field of self-employment (ref. non)		
from employment	−0.032	0.032
	(0.051)	(0.051)
from hobby	0.136*	−0.136*
	(0.073)	(0.073)
self-employment without any support (ref. no)		
yes, exactly the same	−0.271***	0.271***
	(0.044)	(0.044)
yes, but on a smaller scale	−0.044	0.044
	(0.047)	(0.047)
missings	−0.203*	0.203*
	(0.105)	(0.105)
branch (ref. retail)		
manuf.; building; agricult.	0.106*	−0.106*
	(0.060)	(0.060)
capital intensive services	−0.118*	0.118*
	(0.063)	(0.063)
other services	0.084	−0.084
	(0.056)	(0.056)
missings	−0.051	0.051
	(0.078)	(0.078)
trade licence necessary (ref. no)		
yes	−0.128 * *	0.128 * *
	(0.057)	(0.057)
exclusion restriction in the second stage		
daily unemployment transfer	−0.040***	0.040***
	(0.004)	(0.004)
log-likelihood	-3304.757	-3304.757
N	6087	6087

[a] The level of significance is: *** 1 %; ** 5 %; * 10 %

[b] Due to missing information on the daily unemployment transfer, 36 observations are not used for estimation.

Source: IAB and interview data as described in Section 2.4.1.

Table 2.e: Auxiliary regression to test the partial effect of the exclusion restriction on the hazard rate[a]

	ExGZ vs. ÜG	ÜG vs. ExGZ
constant	−3.560***	−4.308***
	(0.310)	(0.305)
baseline		
month 6	0.412 **	2.273***
	(0.202)	(0.155)
month 7	−0.147	2.462***
	(0.255)	(0.153)
month 8	0.064	1.828***
	(0.235)	(0.175)
month 9	0.165	1.580***
	(0.226)	(0.189)
month 10	0.373*	1.437***
	(0.210)	(0.199)
month 11	0.388*	1.247***
	(0.210)	(0.212)
month 12	1.262***	1.535***
	(0.156)	(0.196)
month 13	1.787***	1.901***
	(0.138)	(0.179)
month 14	0.699***	1.479***
	(0.195)	(0.204)
month 15-19	0.663***	1.377***
	(0.133)	(0.153)
socio-demographic and regional characteristics		
gender (ref. male)		
female	−0.085	0.016
	(0.098)	(0.093)
family status (ref. non-married)		
married	−0.217 **	−0.118
	(0.102)	(0.092)
children (ref. none)		
one children	−0.102	0.007
	(0.135)	(0.111)
two or more children	0.180	0.109
	(0.129)	(0.113)
age category (ref. 18-29)		
30 -39	−0.513***	−0.100
	(0.113)	(0.116)
40 -49	−0.599***	0.059
	(0.128)	(0.123)
50 -64	−0.692***	0.274*
	(0.162)	(0.154)
strategy clusters (ref. typ I)		
typ II	−0.007	0.196
	(0.139)	(0.120)
typ III	0.087	0.117
	(0.129)	(0.120)
typ IV	0.045	−0.121
	(0.183)	(0.156)
typ V	−0.117	0.118
	(0.141)	(0.129)

Continued

Table 2.e continued.

	ExGZ vs. ÜG	ÜG vs. ExGZ
qualification		
unemployment career (ref. > 3 month)		
unemployment career (3 months - < 1 year)	−0.122	0.099
	(0.096)	(0.089)
unemployment career (≥ 1 year)	−0.161	0.233∗
	(0.126)	(0.122)
job qualification as categorised by the BA (ref. unskilled workers)		
tertiary education	−0.123	−0.341 ∗ ∗
	(0.210)	(0.156)
skilled workers	−0.412∗∗∗	−0.203∗
	(0.096)	(0.113)
secondary school degree (ref. lower secondary schooling)		
no degree	−0.073	−0.308
	(0.226)	(0.411)
middle secondary schooling	−0.330∗∗∗	0.021
	(0.107)	(0.103)
upper secondary schooling	−0.648∗∗∗	0.053
	(0.141)	(0.123)
employment status before unemployment (ref. employment)		
self-employment	0.149	0.552∗∗∗
	(0.186)	(0.200)
others but once employed	0.308∗∗∗	0.099
	(0.113)	(0.131)
others	0.054	0.203 ∗ ∗
	(0.111)	(0.101)
entrepreneurial characteristics		
preparation		
external support	−0.146	−0.207 ∗ ∗
	(0.096)	(0.092)
customers interviewed	0.302∗∗∗	0.249∗∗∗
	(0.094)	(0.080)
no extra preparation	0.266∗	−0.258
	(0.145)	(0.183)
experience in the field of self-employment (ref. non)		
from employment	−0.184∗	−0.339∗∗∗
	(0.109)	(0.101)
from hobby	0.011	0.039
	(0.145)	(0.151)
self-employment without any support (ref. no)		
yes, exactly the same	−0.553∗∗∗	−1.304∗∗∗
	(0.105)	(0.093)
yes, but on a smaller scale	−0.598∗∗∗	−0.641∗∗∗
	(0.105)	(0.092)
missings	0.309	−1.028∗∗∗
	(0.217)	(0.249)
branch (ref. retail)		
manuf.; building; agricult.	−0.562∗∗∗	−0.812∗∗∗
	(0.148)	(0.132)
capital intensive services	−0.213	−0.058
	(0.151)	(0.115)
other services	−0.155	−0.448∗∗∗
	(0.112)	(0.109)
missings	0.157	−0.513∗∗∗
	(0.175)	(0.166)
trade licence necessary (ref. no)		
yes	0.294∗	0.147
	(0.158)	(0.117)
exclusion restriction in the second stage		
daily unemployment transfer $\times 10^2$	−0.003	−0.140
	(0.479)	(0.277)
log-likelihood	-2980.685	-3565.864
N	49016	47980

[a] The level of significance is: *** 1 %; ** 5 %; * 10 %
Source: IAB and interview data as described in Section 2.4.1.

Table 2.f: Hazard rate to terminate self-employment with a Heckman two-step selection correction[a,b]

	ExGZ		ÜG	
	coeff.	s.e.	coeff.	s.e.
constant	−3.434 ∗ ∗	(0.422)	−4.669 ∗ ∗	(0.403)
Baseline (ref. month 1-5)				
month 6	0.406+	(0.213)	2.283 ∗ ∗	(0.162)
month 7	−0.164	(0.275)	2.475 ∗ ∗	(0.159)
month 8	0.055	(0.244)	1.836 ∗ ∗	(0.183)
month 9	0.145	(0.245)	1.592 ∗ ∗	(0.195)
month 10	0.365+	(0.210)	1.445 ∗ ∗	(0.196)
month 11	0.379+	(0.215)	1.255 ∗ ∗	(0.219)
month 12	1.262 ∗ ∗	(0.164)	1.541 ∗ ∗	(0.196)
month 13	1.794 ∗ ∗	(0.138)	1.914 ∗ ∗	(0.181)
month 14	0.689 ∗ ∗	(0.211)	1.488 ∗ ∗	(0.203)
month 15-19	0.666 ∗ ∗	(0.139)	1.392 ∗ ∗	(0.156)
socio-demographic and regional characteristics				
gender (ref. male)				
female	−0.117	(0.108)	−0.023	(0.100)
family status (ref. non-married)				
married	−0.219∗	(0.103)	−0.119	(0.090)
children (ref. none)				
one children	−0.107	(0.136)	0.004	(0.113)
two children	0.174	(0.138)	0.107	(0.112)
age category (ref. 18-29)				
30 - 39	−0.505 ∗ ∗	(0.115)	−0.090	(0.117)
40 - 49	−0.599 ∗ ∗	(0.127)	0.065	(0.122)
50 - 64	−0.698 ∗ ∗	(0.164)	0.271+	(0.152)
strategy clusters (ref. typ I)				
typ II	−0.007	(0.139)	0.199+	(0.121)
typ III	0.080	(0.130)	0.115	(0.120)
typ IV	0.042	(0.190)	−0.115	(0.158)
typ V	−0.127	(0.151)	0.115	(0.123)
qualification				
unemployment career (ref. $>$ 3 months)				
3 months - $<$ 1 year	−0.116	(0.096)	0.101	(0.087)
$\leq$ 1 year	−0.169	(0.127)	0.212+	(0.124)
job qualification as categorised by the BA (ref. unskilled workers)				
tertiary education	−0.096	(0.221)	−0.302∗	(0.149)
skilled workers	−0.399 ∗ ∗	(0.100)	−0.182+	(0.110)
secondary school degree (ref. lower secondary schooling)				
no degree	−0.108	(0.252)	−0.401	(0.455)
middle secondary schooling	−0.326 ∗ ∗	(0.108)	0.037	(0.107)
upper secondary schooling	−0.649 ∗ ∗	(0.140)	0.075	(0.124)
employment status before unemployment (ref. employment)				
self-employment	0.106	(0.194)	0.481∗	(0.211)
others but once employed	0.287∗	(0.123)	0.067	(0.139)
others	0.043	(0.113)	0.184+	(0.105)

Continued

Table 2.f continued.

	ExGZ		ÜG	
	coeff.	s.e.	coeff.	s.e.
entrepreneurial characteristics				
preparation				
external support	−0.117	(0.106)	−0.170+	(0.093)
customers interviewed	0.302 ∗ ∗	(0.100)	0.248 ∗ ∗	(0.079)
no extra preparation	0.261+	(0.146)	−0.272	(0.181)
experience in the field of self-employment (ref. none)				
from employment	−0.180+	(0.108)	−0.334 ∗ ∗	(0.100)
from hobby	0.002	(0.146)	0.033	(0.147)
self-employment without any support (ref. no)				
yes, exactly the same	−0.536 ∗ ∗	(0.111)	−1.286 ∗ ∗	(0.094)
yes, but on a smaller scale	−0.598 ∗ ∗	(0.108)	−0.635 ∗ ∗	(0.095)
missings	0.315	(0.220)	−1.036 ∗ ∗	(0.250)
branch (ref. retail)				
manuf.; building; agricult.	−0.576 ∗ ∗	(0.148)	−0.829 ∗ ∗	(0.132)
capital intensive services	−0.203	(0.152)	−0.051	(0.115)
other services	−0.164	(0.114)	−0.466 ∗ ∗	(0.111)
missings	0.155	(0.177)	−0.518 ∗ ∗	(0.164)
trade licence necessary (ref. no)				
yes	0.308+	(0.163)	0.154	(0.117)
inverse Mill's ratio				
ρ^c	−0.411	(0.767)	0.491	(0.494)
NT	49,016		47,980	

[a] The level of significance is: ** 1 %; * 5 %; + 10 %.
[b] As recommended by Cameron and Trivedi (2005, p. 550), the estimates are bootstrapped using 1,000 replications.
[c] ρ is the coefficient on the inverse Mills ratio.
Source: IAB and interview data as described in Section 2.4.1.

Table 2.g: Transition rates into unemployment and employment from self-employment without controlling for unobserved heterogeneity[a]

	ExGZ		ÜG	
	unemployment	employment	unemployment	employment
constant	−3.536	−5.859	−4.631	−6.247
	(0.372)**	(0.739)**	(0.378)**	(0.642)**
baseline (ref. month 1-5)				
month 6	0.492	0.299	2.728	1.196
	(0.240)*	(0.425)	(0.203)**	(0.319)**
month 7	−0.228	−0.537	2.906	1.580
	(0.324)	(0.609)	(0.201)**	(0.290)**
month 8	0.093	−0.240	2.120	1.570
	(0.285)	(0.536)	(0.232)**	(0.294)**
month 9	−0.035	0.466	1.789	1.121
	(0.302)	(0.403)	(0.253)**	(0.340)**
month 10	0.415	0.195	1.758	0.774
	(0.253)	(0.451)	(0.257)**	(0.387)*
month 11	0.002	0.899	1.626	0.204
	(0.302)	(0.348)**	(0.268)**	(0.490)
month 12	1.190	1.086	1.918	1.017
	(0.197)**	(0.330)**	(0.250)**	(0.361)**
month 13	1.507	1.758	2.213	1.137
	(0.182)**	(0.276)**	(0.235)**	(0.350)**
month 14	0.506	0.914	1.812	0.752
	(0.259)+	(0.360)*	(0.263)**	(0.406)+
month 15-19	0.632	0.428	1.589	0.908
	(0.165)**	(0.281)	(0.208)**	(0.263)**
socio-demographic and regional characteristics				
gender (ref. male)				
female	−0.220	−0.129	0.023	−0.157
	(0.123)+	(0.197)	(0.112)	(0.185)
family status (ref. non-married)				
married	−0.274	−0.381	−0.160	−0.179
	(0.135)*	(0.203)+	(0.112)	(0.189)
children (ref. none)				
one children	−0.188	0.193	0.034	0.147
	(0.176)	(0.260)	(0.135)	(0.230)
two children	0.105	0.271	0.021	0.392
	(0.172)	(0.271)	(0.143)	(0.220)+
age category (ref. 18 - 29)				
30 - 39	−0.447	−0.535	−0.233	0.254
	(0.149)**	(0.199)**	(0.142)	(0.253)
40 - 49	−0.444	−0.668	0.018	0.440
	(0.165)**	(0.233)**	(0.147)	(0.268)
50 - 64	−0.401	−2.218	0.380	0.075
	(0.201)*	(0.594)**	(0.177)*	(0.373)
strategy clusters (ref. typ I)				
typ II	−0.348	0.721	0.142	0.361
	(0.179)+	(0.320)*	(0.139)	(0.261)
typ III	−0.099	0.422	0.019	0.202
	(0.159)	(0.320)	(0.139)	(0.271)
typ IV	−0.352	1.026	−0.308	0.311
	(0.244)	(0.361)**	(0.197)	(0.303)
typ V	−0.474	0.583	−0.156	0.602
	(0.182)**	(0.328)+	(0.156)	(0.263)*

Continued

Table 2.g continued.

	ExGZ		ÜG	
	unemployment	employment	unemployment	employment
qualification				
unemployment career (ref. < 3 months)				
3 months ≥ 1 year	0.035	−0.334	0.106	0.056
	(0.128)	(0.182)+	(0.108)	(0.176)
≥ 1 year	0.078	−0.514	0.223	−0.114
	(0.161)	(0.277)+	(0.147)	(0.276)
job qualification as categorised by the BA (ref. unskilled workers)				
tertiary education	−0.805	0.449	−0.673	0.408
	(0.333)∗	(0.377)	(0.197)∗∗	(0.355)
skilled workers	−0.536	0.054	−0.303	0.268
	(0.115)∗∗	(0.218)	(0.129)∗	(0.295)
secondary school degree (ref. lower secondary schooling)				
no degree	−0.515	0.068	−0.974	0.493
	(0.318)	(0.521)	(0.634)	(0.685)
middle secondary schooling	−0.432	−0.261	−0.012	−0.142
	(0.134)∗∗	(0.232)	(0.123)	(0.230)
upper secondary schooling	−0.900	−0.249	−0.042	0.076
	(0.189)∗∗	(0.269)	(0.149)	(0.252)
employment status before unemployment (ref. employment)				
self-employment	0.484	−0.478	0.628	0.612
	(0.210)∗	(0.461)	(0.265)∗	(0.371)+
others but once employed	0.301	0.158	−0.011	0.265
	(0.151)∗	(0.229)	(0.166)	(0.268)
others	0.085	−0.043	0.119	0.411
	(0.141)	(0.237)	(0.125)	(0.203)∗
entrepreneurial characteristics				
preparation				
external support	−0.194	−0.200	−0.281	−0.215
	(0.123)	(0.193)	(0.114)∗	(0.178)
customers interviewed	0.303	0.190	0.245	0.415
	(0.121)∗	(0.193)	(0.098)∗	(0.166)∗
no extra preparation	0.219	0.402	−0.487	−0.025
	(0.184)	(0.277)	(0.230)∗	(0.361)
experience in the field of self-employment (ref. non)				
from employment	−0.255	0.118	−0.407	−0.251
	(0.136)+	(0.249)	(0.120)∗∗	(0.220)
from hobby	−0.077	0.314	−0.060	−0.058
	(0.183)	(0.309)	(0.181)	(0.340)
self-employment without any support (ref. no)				
yes, exactly the same	−0.678	−0.322	−1.617	−0.817
	(0.136)∗∗	(0.207)	(0.119)∗∗	(0.185)∗∗
yes, but on a smaller scale	−0.671	−0.557	−0.703	−0.514
	(0.135)∗∗	(0.218)∗	(0.110)∗∗	(0.195)∗∗
missings	0.251	−0.412	−1.299	−0.721
	(0.260)	(0.633)	(0.319)∗∗	(0.483)
branch (ref. retail)				
manuf.; building; agricult.	−0.580	−0.469	−0.961	−0.690
	(0.183)∗∗	(0.328)	(0.161)∗∗	(0.291)∗
capital intensive services	−0.167	−0.308	−0.208	0.121
	(0.186)	(0.339)	(0.141)	(0.253)
other services	−0.231	0.174	−0.546	−0.362
	(0.145)	(0.243)	(0.131)∗∗	(0.242)
missings	0.185	−0.023	−0.453	−0.912
	(0.217)	(0.385)	(0.196)∗	(0.399)∗
trade licence necessary (ref. no)				
yes	0.286	0.153	0.413	−0.285
	(0.216)	(0.304)	(0.170)∗	(0.200)
log-likelihood	-2856.580		-3577.327	
NT	49,067		48,392	
N	3,024		3,098	

[a] Cluster robust standard errors are reported in parenthesis. The level of significance is: ** 1%; * 5%; + 10%.
[b] $\text{var}(\varepsilon_i)$ is estimated using 20 approximation points.
Source: IAB and interview data as described in Section 2.4.1.

Table 2.h: Transition rates into unemployment and employment from self-employment controlling for unobserved heterogeneity[a]

	ExGZ		ÜG	
	unemployment	employment	unemployment	employment
constant	−3.582	−5.909	−4.982	−6.603
	(0.450)**	(0.679)**	(0.474)**	(0.670)**
baseline (ref. month 1-5)				
month 6	0.500	0.305	2.979	1.376
	(0.245)*	(0.427)	(0.250)**	(0.338)**
month 7	−0.218	−0.530	3.312	1.880
	(0.330)	(0.611)	(0.292)**	(0.336)**
month 8	0.104	−0.231	2.599	1.931
	(0.292)	(0.539)	(0.336)**	(0.355)**
month 9	−0.021	0.476	2.312	1.529
	(0.312)	(0.408)	(0.364)**	(0.405)**
month 10	0.432	0.207	2.325	1.211
	(0.271)	(0.458)	(0.379)**	(0.451)**
month 11	0.020	0.913	2.222	0.664
	(0.321)	(0.358)*	(0.396)**	(0.545)
month 12	1.213	1.104	2.558	1.512
	(0.239)**	(0.347)**	(0.396)**	(0.442)**
month 13	1.537	1.782	2.899	1.673
	(0.252)**	(0.309)**	(0.402)**	(0.444)**
month 14	0.538	0.939	2.533	1.311
	(0.320)+	(0.389)*	(0.428)**	(0.494)**
month 15-19	0.670	0.459	2.366	1.522
	(0.275)*	(0.333)	(0.415)**	(0.401)**
socio-demographic and regional characteristics				
gender (ref. male)				
female	−0.224	−0.135	−0.019	−0.191
	(0.126)+	(0.197)	(0.137)	(0.207)
family status (ref. non-married)				
married	−0.281	−0.387	−0.222	−0.248
	(0.139)*	(0.218)+	(0.141)	(0.213)
children (ref. none)				
one children	−0.185	0.196	0.046	0.179
	(0.176)	(0.264)	(0.165)	(0.250)
two children	0.112	0.273	0.058	0.446
	(0.173)	(0.276)	(0.175)	(0.240)+
age category (ref. 18 - 29)				
30 - 39	−0.459	−0.549	−0.256	0.197
	(0.166)**	(0.233)*	(0.167)	(0.273)
40 - 49	−0.453	−0.680	0.012	0.425
	(0.168)**	(0.255)**	(0.176)	(0.286)
50 - 64	−0.412	−2.240	0.526	0.160
	(0.209)*	(0.624)**	(0.225)*	(0.389)
strategy clusters (ref. typ I)				
typ II	−0.346	0.725	0.211	0.393
	(0.179)+	(0.324)*	(0.175)	(0.285)
typ III	−0.098	0.427	0.019	0.204
	(0.156)	(0.323)	(0.170)	(0.284)
typ IV	−0.352	1.033	−0.311	0.290
	(0.240)	(0.363)**	(0.228)	(0.327)
typ V	−0.476	0.583	−0.162	0.616
	(0.180)**	(0.325)+	(0.191)	(0.285)*

Continued

Table 2.h continued.

	ExGZ		ÜG	
	unemployment	employment	unemployment	employment
qualification				
unemployment career (ref. < 3 months)				
3 months ≥ 1 year	0.034	−0.337	0.138	0.087
	(0.131)	(0.186)+	(0.132)	(0.190)
≥ 1 year	0.080	−0.516	0.270	−0.059
	(0.161)	(0.276)+	(0.183)	(0.294)
job qualification as categorised by the BA (ref. unskilled workers)				
tertiary education	−0.806	0.452	−0.875	0.270
	(0.333)∗	(0.360)	(0.241)∗∗	(0.358)
skilled workers	−0.544	0.050	−0.418	0.171
	(0.129)∗∗	(0.225)	(0.161)∗∗	(0.288)
secondary school degree (ref. lower secondary schooling)				
no degree	−0.529	0.059	−1.013	0.449
	(0.338)	(0.543)	(0.677)	(0.693)
middle secondary schooling	−0.441	−0.269	−0.044	−0.174
	(0.144)∗∗	(0.232)	(0.154)	(0.253)
upper secondary schooling	−0.916	−0.261	−0.008	0.093
	(0.215)∗∗	(0.279)	(0.174)	(0.266)
employment status before unemployment (ref. employment)				
self-employment	0.493	−0.476	0.830	0.853
	(0.219)∗	(0.466)	(0.324)∗	(0.412)∗
others but once employed	0.306	0.162	−0.038	0.286
	(0.153)∗	(0.230)	(0.206)	(0.285)
others	0.087	−0.041	0.168	0.463
	(0.141)	(0.231)	(0.154)	(0.230)∗
entrepreneurial characteristics				
preparation				
external support	−0.199	−0.207	−0.392	−0.291
	(0.127)	(0.196)	(0.142)∗∗	(0.199)
customers interviewed	0.303	0.195	0.306	0.458
	(0.123)∗	(0.194)	(0.121)∗	(0.178)∗
no extra preparation	0.222	0.406	−0.644	−0.118
	(0.186)	(0.276)	(0.283)∗	(0.391)
experience in the field of self-employment (ref. non)				
from employment	−0.263	0.110	−0.571	−0.400
	(0.144)+	(0.256)	(0.160)∗∗	(0.242)+
from hobby	−0.086	0.312	−0.083	−0.089
	(0.193)	(0.314)	(0.224)	(0.369)
self-employment without any support (ref. no)				
yes, exactly the same	−0.692	−0.329	−2.058	−1.175
	(0.161)∗∗	(0.209)	(0.233)∗∗	(0.256)∗∗
yes, but on a smaller scale	−0.688	−0.568	−0.959	−0.729
	(0.166)∗∗	(0.228)∗	(0.171)∗∗	(0.234)∗∗
missings	0.258	−0.389	−1.628	−0.965
	(0.265)	(0.615)	(0.389)∗∗	(0.521)+
branch (ref. retail)				
manuf.; building; agricult.	−0.585	−0.472	−1.219	−0.917
	(0.189)∗∗	(0.324)	(0.221)∗∗	(0.323)∗∗
capital intensive services	−0.163	−0.307	−0.258	0.138
	(0.188)	(0.341)	(0.172)	(0.265)
other services	−0.230	0.177	−0.727	−0.467
	(0.147)	(0.252)	(0.175)∗∗	(0.264)+
missings	0.191	−0.015	−0.678	−1.028
	(0.223)	(0.384)	(0.256)∗∗	(0.419)∗
trade licence necessary (ref. no)				
yes	0.292	0.157	0.451	−0.249
	(0.216)	(0.271)	(0.190)∗	(0.229)
unobserved heterogeneity				
standard deviation of ε_i	0.393		1.294	
	(1.095)		(0.307)∗∗	
log-likelihood	-2856.562		-3572.594	
NT	49,067		48,392	
N	3,024		3,098	

[a] Cluster robust standard errors are reported in parenthesis. The level of significance is: ** 1%; * 5%; + 10%.
[b] $\text{var}(\varepsilon_i)$ is estimated using 20 approximation points.
Source: IAB and interview data as described in Section 2.4.1.

Table 2.i: Hazard rate to terminate self-employment without controlling for unobserved heterogeneity[a]

	ExGZ	ÜG
constant	−3.552	−4.343
	(0.304)**	(0.301)**
baseline (ref. month 1-5)		
month 6	0.412	2.272
	(0.202)*	(0.155)**
month 7	−0.148	2.462
	(0.255)	(0.153)**
month 8	0.063	1.843
	(0.235)	(0.175)**
month 9	0.165	1.580
	(0.226)	(0.189)**
month 10	0.373	1.437
	(0.210)+	(0.199)**
month 11	0.387	1.247
	(0.210)+	(0.212)**
month 12	1.261	1.535
	(0.156)**	(0.196)**
month 13	1.785	1.901
	(0.138)**	(0.179)**
month 14	0.698	1.479
	(0.195)**	(0.204)**
month 15-19	0.668	1.384
	(0.133)**	(0.152)**
socio-demographic and regional characteristics		
gender (ref. male)		
female	−0.078	0.034
	(0.095)	(0.090)
family status (ref. non-married)		
married	−0.213	−0.122
	(0.102)*	(0.091)
children (ref. none)		
one children	−0.105	0.003
	(0.134)	(0.111)
two children	0.178	0.103
	(0.129)	(0.112)
age category (ref. 18 - 29)		
30 - 39	−0.505	−0.114
	(0.112)**	(0.115)
40 - 49	−0.590	0.043
	(0.126)**	(0.121)
50 - 64	−0.685	0.255
	(0.161)**	(0.150)+
strategy clusters (ref. typ I)		
typ II	−0.012	0.197
	(0.138)	(0.116)+
typ III	0.078	0.113
	(0.129)	(0.116)
typ IV	0.040	−0.130
	(0.182)	(0.153)
typ V	−0.131	0.103
	(0.142)	(0.124)

Continued

Table 2.i continued.

	ExGZ	ÜG
qualification		
unemployment career (ref. $<$ 3 months)		
3 months $\geq$ 1 year	−0.111	0.117
	(0.096)	(0.088)
$\geq$ 1 year	−0.151	0.257
	(0.125)	(0.120)∗
job qualification as categorised by the BA (ref. unskilled workers)		
tertiary education	−0.117	−0.335
	(0.206)	(0.156)∗
skilled workers	−0.406	−0.204
	(0.094)∗∗	(0.112)+
secondary school degree (ref. lower secondary schooling)		
no degree	−0.072	−0.295
	(0.226)	(0.410)
middle secondary schooling	−0.343	0.019
	(0.107)∗∗	(0.103)
upper secondary schooling	−0.656	0.039
	(0.141)∗∗	(0.121)
employment status before unemployment (ref. employment)		
self-employment	0.152	0.564
	(0.185)	(0.200)∗∗
others but once employed	0.309	0.099
	(0.113)∗∗	(0.131)
others	0.053	0.206
	(0.111)	(0.101)∗
entrepreneurial characteristics		
preparation		
external support	−0.153	−0.207
	(0.096)	(0.092)∗
customers interviewed	0.302	0.251
	(0.094)∗∗	(0.080)∗∗
no extra preparation	0.272	−0.251
	(0.144)+	(0.183)
experience in the field of self-employment (ref. non)		
from employment	−0.190	−0.353
	(0.108)+	(0.101)∗∗
from hobby	0.001	0.023
	(0.145)	(0.151)
self-employment without any support (ref. no)		
yes, exactly the same	−0.558	−1.314
	(0.105)∗∗	(0.093)∗∗
yes, but on a smaller scale	−0.596	−0.645
	(0.105)∗∗	(0.092)∗∗
missings	0.266	−1.075
	(0.218)	(0.249)∗∗
branch (ref. retail)		
manuf.; building; agricult.	−0.562	−0.805
	(0.148)∗∗	(0.132)∗∗
capital intensive services	−0.213	−0.056
	(0.150)	(0.115)
other services	−0.157	−0.448
	(0.112)	(0.109)∗∗
missings	0.129	−0.518
	(0.175)	(0.166)∗∗
trade licence necessary (ref. no)		
yes	0.284	0.153
	(0.158)+	(0.117)
log-likelihood	-2987.231	-3578.350
NT	49,160	48,473
N	3,025	3,098

[a] Cluster robust standard errors are reported in parenthesis. The level of significance is: ** 1%; * 5%; + 10%.
[b] var(ε_i) is estimated using 20 approximation points.
Source: IAB and interview data as described in Section 2.4.1.

Table 2.j: Hazard rate to terminate self-employment controlling for unobserved heterogeneity[a]

	ExGZ	ÜG
constant	−3.563	−4.581
	(0.331)**	(0.371)**
baseline (ref. month 1-5)		
month 6	0.414	2.401
	(0.204)*	(0.183)**
month 7	−0.146	2.680
	(0.258)	(0.213)**
month 8	0.066	2.104
	(0.238)	(0.246)**
month 9	0.168	1.872
	(0.232)	(0.268)**
month 10	0.376	1.755
	(0.218)+	(0.285)**
month 11	0.392	1.586
	(0.220)+	(0.302)**
month 12	1.267	1.900
	(0.178)**	(0.301)**
month 13	1.793	2.298
	(0.179)**	(0.304)**
month 14	0.706	1.898
	(0.232)**	(0.328)**
month 15-19	0.678	1.844
	(0.198)**	(0.317)**
socio-demographic and regional characteristics		
gender (ref. male)		
female	−0.079	0.026
	(0.095)	(0.106)
family status (ref. non-married)		
married	−0.215	−0.154
	(0.106)*	(0.111)
children (ref. none)		
one children	−0.103	0.010
	(0.133)	(0.131)
two children	0.180	0.131
	(0.133)	(0.134)
age category (ref. 18 - 29)		
30 - 39	−0.509	−0.133
	(0.127)**	(0.133)
40 - 49	−0.594	0.018
	(0.132)**	(0.141)
50 - 64	−0.689	0.318
	(0.178)**	(0.180)+
strategy clusters (ref. typ I)		
typ II	−0.011	0.245
	(0.141)	(0.141)+
typ III	0.080	0.130
	(0.130)	(0.137)
typ IV	0.042	−0.133
	(0.180)	(0.176)
typ V	−0.130	0.124
	(0.140)	(0.149)

Continued

Table 2.j continued.

	ExGZ	ÜG
qualification		
unemployment career (ref. $<$ 3 months)		
3 months $\geq$ 1 year	−0.111	0.148
	(0.097)	(0.105)
$\geq$ 1 year	−0.151	0.309
	(0.125)	(0.147)∗
job qualification as categorised by the BA (ref. unskilled workers)		
tertiary education	−0.117	−0.442
	(0.205)	(0.186)∗
skilled workers	−0.408	−0.280
	(0.101)∗∗	(0.133)∗
secondary school degree (ref. lower secondary schooling)		
no degree	−0.073	−0.297
	(0.231)	(0.438)
middle secondary schooling	−0.345	0.011
	(0.109)∗∗	(0.125)
upper secondary schooling	−0.659	0.074
	(0.153)∗∗	(0.139)
employment status before unemployment (ref. employment)		
self-employment	0.153	0.682
	(0.184)	(0.252)∗∗
others but once employed	0.311	0.101
	(0.114)∗∗	(0.156)
others	0.053	0.248
	(0.110)	(0.122)∗
entrepreneurial characteristics		
preparation		
external support	−0.154	−0.268
	(0.097)	(0.111)∗
customers interviewed	0.302	0.286
	(0.095)∗∗	(0.095)∗∗
no extra preparation	0.272	−0.308
	(0.143)+	(0.212)
experience in the field of self-employment (ref. non)		
from employment	−0.192	−0.458
	(0.112)+	(0.130)∗∗
from hobby	−0.000	0.020
	(0.145)	(0.178)
self-employment without any support (ref. no)		
yes, exactly the same	−0.561	−1.568
	(0.116)∗∗	(0.178)∗∗
yes, but on a smaller scale	−0.600	−0.804
	(0.121)∗∗	(0.139)∗∗
missings	0.271	−1.253
	(0.218)	(0.292)∗∗
branch (ref. retail)		
manuf.; building; agricult.	−0.564	−0.949
	(0.150)∗∗	(0.174)∗∗
capital intensive services	−0.213	−0.070
	(0.150)	(0.137)
other services	−0.158	−0.542
	(0.114)	(0.139)∗∗
missings	0.130	−0.623
	(0.175)	(0.201)∗∗
trade licence necessary (ref. no)		
yes	0.286	0.174
	(0.154)+	(0.138)
unobserved heterogeneity		
standard deviation of ε_i	0.193	1.009
	(1.396)	(0.302)∗∗
log-likelihood	-2987.229	-3575.873
NT	49,160	48,473
N	3,025	3,098

[a] Cluster robust standard errors are reported in parenthesis. The level of significance is: ** 1%; * 5%; + 10%.
[b] var(ε_i) is estimated using 20 approximation points.
Source: IAB and interview data as described in Section 2.4.1.

Table 2.k: Marginal effects: ExGZ participants[a]

	sig.[b]	month 5	month 6	month 7	month 8	month 9	month 10	month 11	month 12	month 13	month 14	month 15
reference hazard rate[c] (in percent)		2.79	4.15	2.41	2.96	3.27	4.00	4.05	9.19	14.60	5.45	5.30
socio-demographic and regional characteristics												
female		−0.20	−0.30	−0.18	−0.22	−0.24	−0.29	−0.29	−0.63	−0.95	−0.39	−0.38
married	*	−0.52	−0.77	−0.45	−0.55	−0.61	−0.74	−0.75	−1.63	−2.46	−1.00	−0.97
one children		−0.27	−0.40	−0.23	−0.29	−0.32	−0.38	−0.39	−0.84	−1.26	−0.51	−0.50
two or more children		0.52	0.77	0.46	0.56	0.61	0.74	0.75	1.59	2.36	0.99	0.97
age category (30 - 39)	**	−1.09	−1.60	−0.94	−1.15	−1.27	−1.55	−1.57	−3.43	−5.25	−2.09	−2.03
age category (40 - 49)	**	−1.22	−1.81	−1.06	−1.30	−1.43	−1.74	−1.77	−3.88	−5.95	−2.35	−2.29
age category (50 - 64)	**	−1.36	−2.01	−1.18	−1.45	−1.59	−1.94	−1.97	−4.33	−6.66	−2.62	−2.55
strategy cluster II		−0.03	−0.05	−0.03	−0.04	−0.04	−0.05	−0.05	−0.10	−0.15	−0.06	−0.06
strategy cluster III		0.22	0.32	0.19	0.23	0.26	0.31	0.32	0.68	1.00	0.42	0.41
strategy cluster IV		0.11	0.16	0.10	0.12	0.13	0.16	0.16	0.34	0.51	0.21	0.21
strategy cluster V		−0.33	−0.49	−0.29	−0.35	−0.39	−0.47	−0.48	−1.03	−1.56	−0.64	−0.62
qualification												
unempl. career ($\geq$ 3 month - $<$ 1 year)		−0.29	−0.42	−0.25	−0.30	−0.33	−0.40	−0.41	−0.88	−1.33	−0.54	−0.53
unemployment career ($\geq$ 1 year)		−0.38	−0.56	−0.33	−0.41	−0.45	−0.54	−0.55	−1.19	−1.79	−0.73	−0.71
tertiary education		−0.30	−0.44	−0.26	−0.32	−0.35	−0.43	−0.43	−0.93	−1.40	−0.57	−0.56
skilled workers	**	−0.91	−1.35	−0.79	−0.97	−1.07	−1.30	−1.31	−2.87	−4.37	−1.75	−1.70
no degree		−0.19	−0.28	−0.16	−0.20	−0.22	−0.27	−0.27	−0.59	−0.88	−0.36	−0.35
middle secondary schooling	**	−0.79	−1.17	−0.69	−0.84	−0.93	−1.13	−1.14	−2.49	−3.78	−1.52	−1.48
upper secondary schooling	**	−1.32	−1.95	−1.15	−1.40	−1.55	−1.88	−1.91	−4.20	−6.45	−2.54	−2.48
self-employed before unemployed		0.44	0.65	0.38	0.47	0.52	0.62	0.63	1.35	2.00	0.84	0.82
others but once empl. before unemployed	**	0.97	1.42	0.85	1.03	1.13	1.37	1.39	2.93	4.29	1.83	1.79
others before unemployed		0.15	0.21	0.13	0.16	0.17	0.21	0.21	0.45	0.67	0.28	0.27
entrepreneurial characteristics												
external support		−0.38	−0.57	−0.33	−0.41	−0.45	−0.55	−0.55	−1.20	−1.80	−0.73	−0.72
potential customers interviewed	**	0.95	1.38	0.82	1.00	1.10	1.33	1.35	2.85	4.18	1.78	1.73
no extra preparation	+	0.84	1.23	0.73	0.89	0.98	1.18	1.20	2.53	3.72	1.58	1.54
experience from employment	+	−0.47	−0.69	−0.41	−0.50	−0.55	−0.67	−0.68	−1.47	−2.22	−0.90	−0.88
experience from hobby		0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.01	0.00	0.00
without support: yes, exactly the same	**	−1.17	−1.73	−1.02	−1.25	−1.37	−1.67	−1.69	−3.71	−5.69	−2.25	−2.20
without support: yes, but on a smaller scale	**	−1.23	−1.82	−1.07	−1.31	−1.44	−1.75	−1.78	−3.91	−5.99	−2.37	−2.31
manufacturing; building; agriculture	**	−1.18	−1.74	−1.02	−1.25	−1.38	−1.68	−1.70	−3.74	−5.72	−2.27	−2.21
capital intensive services		−0.52	−0.77	−0.45	−0.56	−0.61	−0.74	−0.75	−1.63	−2.46	−1.00	−0.97
other services		−0.40	−0.58	−0.34	−0.42	−0.46	−0.56	−0.57	−1.23	−1.85	−0.76	−0.74
trade licence necessary	+	0.88	1.29	0.77	0.94	1.03	1.25	1.26	2.67	3.91	1.66	1.62

[a] Since all background characteristics are described by dummies and categorial variables the marginal effect is calculated as the difference between the reference category and a discrete change in the respective variable, holding all other variables constant at zero.
[b] Significance level of the underlying estimation: ** 1 percent; * 5 percent; + 10 percent.
[c] The reference hazard rate is calculated for $\mathbf{X}_{it} = 0$.
Source: IAB and interview data as described in Section 2.4.1.

Table 2.l: Marginal effects: ÜG participants, with minus one standard deviation of var(ε_i)[a]

	sig.[b]	month 5	month 6	month 7	month 8	month 9	month 10	month 11	month 12	month 13	month 14	month 15
reference hazard rate[c] (in percent)		0.37	3.96	5.17	2.97	2.37	2.11	1.79	2.44	3.58	2.43	2.31
socio-demographic and regional characteristics												
female		0.01	0.10	0.13	0.08	0.06	0.05	0.05	0.06	0.09	0.06	0.06
married		−0.05	−0.55	−0.71	−0.41	−0.33	−0.30	−0.25	−0.34	−0.50	−0.34	−0.32
one children		0.00	0.04	0.05	0.03	0.02	0.02	0.02	0.02	0.03	0.02	0.02
two or more children		0.05	0.53	0.68	0.40	0.32	0.29	0.25	0.33	0.48	0.33	0.31
age category (30 - 39)		−0.05	−0.47	−0.61	−0.36	−0.29	−0.26	−0.22	−0.30	−0.43	−0.30	−0.28
age category (40 - 49)		0.01	0.07	0.09	0.05	0.04	0.04	0.03	0.04	0.06	0.04	0.04
age category (50 - 64)		0.14	1.40	1.80	1.07	0.86	0.77	0.65	0.88	1.28	0.88	0.84
strategy cluster II		0.10	1.04	1.34	0.79	0.64	0.57	0.49	0.66	0.95	0.65	0.62
strategy cluster III		0.05	0.52	0.67	0.40	0.32	0.29	0.24	0.33	0.48	0.33	0.31
strategy cluster IV		−0.05	−0.47	−0.61	−0.36	−0.29	−0.26	−0.22	−0.30	−0.43	−0.30	−0.28
strategy cluster V		0.05	0.50	0.64	0.38	0.30	0.27	0.23	0.31	0.45	0.31	0.30
qualification												
unempl. career ($\geq$ 3 month - $<$ 1 year)		0.06	0.60	0.77	0.46	0.37	0.33	0.28	0.38	0.55	0.38	0.36
unemployment career ($\geq$ 1 year)	*	0.13	1.36	1.74	1.03	0.83	0.74	0.63	0.85	1.23	0.85	0.81
tertiary education	*	−0.13	−1.38	−1.78	−1.04	−0.83	−0.74	−0.63	−0.86	−1.25	−0.85	−0.81
skilled workers	*	−0.09	−0.94	−1.21	−0.71	−0.57	−0.51	−0.43	−0.58	−0.85	−0.58	−0.55
no degree		−0.10	−0.99	−1.28	−0.75	−0.60	−0.53	−0.45	−0.61	−0.90	−0.61	−0.58
middle secondary schooling		0.00	0.04	0.06	0.03	0.03	0.02	0.02	0.03	0.04	0.03	0.03
upper secondary schooling		0.03	0.29	0.37	0.22	0.18	0.16	0.13	0.18	0.26	0.18	0.17
self-employed before unemployed	**	0.36	3.58	4.56	2.74	2.21	1.98	1.69	2.27	3.27	2.27	2.16
others but once empl. before unemployed		0.04	0.40	0.52	0.30	0.24	0.22	0.19	0.25	0.36	0.25	0.24
others before unemployed	*	0.10	1.06	1.36	0.81	0.65	0.58	0.49	0.66	0.96	0.66	0.63
entrepreneurial characteristics												
external support	*	−0.09	−0.90	−1.17	−0.68	−0.55	−0.49	−0.42	−0.56	−0.82	−0.56	−0.53
potential customers interviewed	**	0.12	1.24	1.59	0.94	0.76	0.68	0.58	0.78	1.13	0.78	0.74
no extra preparation		−0.10	−1.02	−1.32	−0.77	−0.62	−0.55	−0.47	−0.63	−0.93	−0.63	−0.60
experience from employment	**	−0.14	−1.42	−1.83	−1.07	−0.86	−0.77	−0.65	−0.88	−1.29	−0.88	−0.83
experience from hobby		0.01	0.08	0.10	0.06	0.05	0.04	0.04	0.05	0.07	0.05	0.05
without support: yes, exactly the same	**	−0.29	−3.11	−4.04	−2.34	−1.87	−1.67	−1.41	−1.92	−2.81	−1.91	−1.82
without support: yes, but on a smaller scale	**	−0.21	−2.15	−2.79	−1.62	−1.30	−1.16	−0.98	−1.33	−1.95	−1.33	−1.26
manufacturing; building; agriculture	**	−0.23	−2.39	−3.10	−1.80	−1.44	−1.29	−1.09	−1.48	−2.16	−1.47	−1.40
capital intensive services		−0.02	−0.26	−0.33	−0.19	−0.16	−0.14	−0.12	−0.16	−0.23	−0.16	−0.15
other services	**	−0.16	−1.62	−2.10	−1.22	−0.98	−0.87	−0.74	−1.00	−1.47	−1.00	−0.95
trade licence necessary		0.07	0.72	0.92	0.54	0.44	0.39	0.33	0.45	0.65	0.45	0.43

[a] Since all background characteristics are described by dummies and categorial variables the marginal effect is calculated as the difference between the reference category and a discrete change in the respective variable, holding all other variables constant at zero.
[b] Significance level of the underlying estimation: ** 1 percent; * 5 percent; + 10 percent.
[c] The reference hazard rate is calculated for $\mathbf{X}_{it} = 0$.
Source: IAB and interview data as described in Section 2.4.1.

Table 2.m: Marginal effects: ÜG participants, with $\text{var}(\varepsilon_i) = 0^a$

	sig.[b]	month 5	month 6	month 7	month 8	month 9	month 10	month 11	month 12	month 13	month 14	month 15
reference hazard rate[c] (in percent)		1.01	10.16	13.01	7.75	6.25	5.60	4.76	6.41	9.25	6.40	6.08
socio-demographic and regional characteristics												
female		0.03	0.24	0.30	0.19	0.15	0.14	0.12	0.16	0.22	0.16	0.15
married		−0.14	−1.33	−1.65	−1.03	−0.85	−0.76	−0.65	−0.87	−1.22	−0.86	−0.82
one children		0.01	0.09	0.11	0.07	0.06	0.05	0.04	0.06	0.08	0.06	0.06
two or more children		0.14	1.26	1.56	0.99	0.81	0.73	0.63	0.83	1.16	0.83	0.79
age category (30 - 39)		−0.12	−1.15	−1.43	−0.90	−0.73	−0.66	−0.57	−0.75	−1.05	−0.75	−0.71
age category (40 - 49)		0.02	0.16	0.20	0.13	0.10	0.09	0.08	0.11	0.15	0.11	0.10
age category (50 - 64)		0.37	3.29	4.04	2.60	2.14	1.94	1.67	2.19	3.04	2.19	2.09
strategy cluster II		0.28	2.47	3.03	1.95	1.60	1.45	1.24	1.64	2.28	1.63	1.56
strategy cluster III		0.14	1.25	1.54	0.98	0.80	0.73	0.62	0.82	1.15	0.82	0.78
strategy cluster IV		−0.12	−1.15	−1.43	−0.90	−0.73	−0.66	−0.57	−0.75	−1.05	−0.75	−0.71
strategy cluster V		0.13	1.19	1.47	0.94	0.77	0.69	0.60	0.79	1.10	0.78	0.75
qualification												
unempl. career ($\geq$ 3 month - $<$ 1 year)		0.16	1.43	1.77	1.13	0.92	0.83	0.72	0.95	1.32	0.94	0.90
unemployment career ($\geq$ 1 year)	*	0.36	3.18	3.91	2.52	2.07	1.87	1.61	2.12	2.94	2.12	2.02
tertiary education	*	−0.36	−3.38	−4.24	−2.63	−2.14	−1.93	−1.65	−2.19	−3.10	−2.19	−2.09
skilled workers	*	−0.25	−2.29	−2.86	−1.78	−1.45	−1.31	−1.12	−1.49	−2.10	−1.49	−1.42
no degree		−0.26	−2.41	−3.01	−1.87	−1.53	−1.38	−1.18	−1.57	−2.21	−1.56	−1.49
middle secondary schooling		0.01	0.10	0.13	0.08	0.07	0.06	0.05	0.07	0.10	0.07	0.06
upper secondary schooling		0.08	0.69	0.86	0.54	0.45	0.40	0.35	0.46	0.64	0.46	0.44
self-employed before unemployed	**	0.97	8.13	9.82	6.50	5.40	4.90	4.24	5.52	7.54	5.51	5.28
others but once empl. before unemployed		0.11	0.96	1.18	0.75	0.62	0.56	0.48	0.63	0.88	0.63	0.60
others before unemployed	*	0.28	2.50	3.07	1.97	1.62	1.46	1.26	1.66	2.30	1.66	1.58
entrepreneurial characteristics												
external support	*	−0.24	−2.20	−2.75	−1.71	−1.40	−1.26	−1.08	−1.43	−2.02	−1.43	−1.36
potential customers interviewed	**	0.33	2.92	3.59	2.31	1.90	1.72	1.48	1.94	2.70	1.94	1.85
no extra preparation		−0.27	−2.49	−3.11	−1.94	−1.58	−1.42	−1.22	−1.62	−2.28	−1.62	−1.54
experience from employment	**	−0.37	−3.48	−4.36	−2.70	−2.20	−1.98	−1.70	−2.26	−3.19	−2.25	−2.15
experience from hobby		0.02	0.19	0.23	0.15	0.12	0.11	0.09	0.12	0.17	0.12	0.12
without support: yes, exactly the same	**	−0.80	−7.86	−9.98	−6.03	−4.88	−4.38	−3.73	−5.00	−7.17	−4.99	−4.75
without support: yes, but on a smaller scale	**	−0.56	−5.34	−6.73	−4.13	−3.35	−3.01	−2.57	−3.44	−4.89	−3.43	−3.27
manufacturing; building; agriculture	**	−0.62	−5.96	−7.53	−4.60	−3.73	−3.35	−2.86	−3.83	−5.45	−3.82	−3.64
capital intensive services		−0.07	−0.62	−0.77	−0.48	−0.40	−0.36	−0.31	−0.40	−0.57	−0.40	−0.39
other services	**	−0.42	−3.99	−5.01	−3.09	−2.52	−2.26	−1.94	−2.58	−3.66	−2.57	−2.45
trade licence necessary		0.19	1.70	2.09	1.34	1.10	0.99	0.85	1.12	1.56	1.12	1.07

[a] Since all background characteristics are described by dummies and categorial variables the marginal effect is calculated as the difference between the reference category and a discrete change in the respective variable, holding all other variables constant at zero.
[b] Significance level of the underlying estimation: ** 1 percent; * 5 percent; + 10 percent.
[c] The reference hazard rate is calculated for $\mathbf{X}_{it} = 0$.
Source: IAB and interview data as described in Section 2.4.1.

Table 2.n: Marginal effects: ÜG participants, with plus one standard deviation of var(ε_i)[a]

	sig.[b]	month 5	month 6	month 7	month 8	month 9	month 10	month 11	month 12	month 13	month 14	month 15
reference hazard rate[c] (in percent)		2.73	23.68	29.09	18.73	15.45	13.99	12.07	15.82	21.86	15.79	15.09
socio-demographic and regional characteristics												
female		0.07	0.47	0.54	0.40	0.34	0.32	0.28	0.35	0.45	0.35	0.34
married		−0.38	−2.68	−3.08	−2.24	−1.91	−1.76	−1.55	−1.95	−2.52	−1.95	−1.87
one children		0.03	0.18	0.20	0.15	0.13	0.12	0.11	0.13	0.17	0.13	0.13
two or more children		0.37	2.45	2.78	2.08	1.79	1.65	1.46	1.83	2.32	1.82	1.76
age category (30 - 39)		−0.33	−2.31	−2.66	−1.93	−1.65	−1.52	−1.34	−1.69	−2.18	−1.68	−1.62
age category (40 - 49)		0.05	0.32	0.37	0.27	0.23	0.21	0.19	0.24	0.30	0.24	0.23
age category (50 - 64)		0.99	6.21	6.96	5.33	4.62	4.28	3.80	4.71	5.91	4.70	4.54
strategy cluster II		0.73	4.71	5.30	4.02	3.48	3.22	2.85	3.55	4.48	3.54	3.42
strategy cluster III		0.37	2.42	2.74	2.05	1.77	1.63	1.44	1.80	2.29	1.80	1.74
strategy cluster IV		−0.33	−2.31	−2.66	−1.93	−1.65	−1.52	−1.34	−1.69	−2.18	−1.68	−1.62
strategy cluster V		0.35	2.32	2.63	1.96	1.69	1.56	1.38	1.72	2.19	1.72	1.66
qualification												
unempl. career ($\geq$ 3 month - $<$ 1 year)		0.42	2.78	3.14	2.36	2.03	1.88	1.66	2.07	2.63	2.07	1.99
unemployment career ($\geq$ 1 year)	*	0.95	6.02	6.75	5.16	4.47	4.14	3.68	4.56	5.73	4.55	4.39
tertiary education	*	−0.96	−7.05	−8.22	−5.83	−4.94	−4.52	−3.96	−5.04	−6.62	−5.03	−4.84
skilled workers	*	−0.65	−4.69	−5.43	−3.90	−3.32	−3.04	−2.67	−3.38	−4.41	−3.38	−3.25
no degree		−0.69	−4.94	−5.72	−4.11	−3.49	−3.21	−2.81	−3.56	−4.65	−3.56	−3.42
middle secondary schooling		0.03	0.20	0.23	0.17	0.15	0.14	0.12	0.15	0.19	0.15	0.15
upper secondary schooling		0.20	1.36	1.54	1.15	0.99	0.91	0.81	1.01	1.29	1.01	0.97
self-employed before unemployed	**	2.54	14.36	15.71	12.59	11.11	10.36	9.29	11.29	13.77	11.27	10.93
others but once empl. before unemployed		0.28	1.87	2.12	1.58	1.36	1.26	1.11	1.39	1.77	1.39	1.34
others before unemployed	*	0.74	4.77	5.37	4.07	3.53	3.26	2.89	3.59	4.53	3.59	3.46
entrepreneurial characteristics												
external support	*	−0.63	−4.50	−5.21	−3.75	−3.19	−2.93	−2.57	−3.25	−4.24	−3.25	−3.13
potential customers interviewed	**	0.87	5.54	6.23	4.74	4.11	3.80	3.38	4.19	5.27	4.18	4.04
no extra preparation		−0.71	−5.12	−5.93	−4.25	−3.62	−3.32	−2.91	−3.69	−4.81	−3.68	−3.54
experience from employment	**	−0.99	−7.27	−8.48	−6.01	−5.09	−4.66	−4.08	−5.19	−6.82	−5.19	−4.98
experience from hobby		0.05	0.37	0.42	0.31	0.27	0.25	0.22	0.27	0.35	0.27	0.26
without support: yes, exactly the same	**	−2.15	−17.61	−21.21	−14.15	−11.78	−10.71	−9.29	−12.05	−16.35	−12.03	−11.52
without support: yes, but on a smaller scale	**	−1.49	−11.49	−13.58	−9.38	−7.89	−7.20	−6.28	−8.06	−10.73	−8.05	−7.72
manufacturing; building; agriculture	**	−1.66	−12.95	−15.38	−10.54	−8.84	−8.06	−7.02	−9.04	−12.09	−9.02	−8.65
capital intensive services		−0.18	−1.23	−1.41	−1.04	−0.89	−0.82	−0.72	−0.90	−1.17	−0.90	−0.87
other services	**	−1.13	−8.40	−9.83	−6.91	−5.85	−5.35	−4.68	−5.97	−7.87	−5.96	−5.72
trade licence necessary		0.50	3.28	3.71	2.79	2.41	2.22	1.97	2.45	3.11	2.45	2.36

[a] Since all background characteristics are described by dummies and categorial variables the marginal effect is calculated as the difference between the reference category and a discrete change in the respective variable, holding all other variables constant at zero.
[b] Significance level of the underlying estimation: ** 1 percent; * 5 percent; + 10 percent.
[c] The reference hazard rate is calculated for $\mathbf{X}_{it} = 0$.
Source: IAB and interview data as described in Section 2.4.1.

Chapter 3

Class size effects on early career earnings

3.1 Introduction

Improving the quality of education is a policy goal that is on the top of the policy makers' agenda. And almost all parties concerned with education – that is parents, teachers, policy makers and voters – believe the most straight forward way to improve the quality of education would be to reduce the size of classes. The rationales behind this idea are manifold: (i) Teachers are enabled to apply different teaching methods; (ii) students may receive more individual attention; (iii) disruptive behaviour may be reduced; and (iv) teachers may have closer contact with pupils and parents. All these factors may lead to a deeper understanding of the learnt content. The self-esteem and the responsibility of pupils may also be higher if tuitions are more individualised. Smaller classes may thus improve 'hard-skills' and 'soft-skills', which both are valued by the labour market as an early career outcome.

The quality of education could also be improved by other factors, such as better educated teachers, alternative teaching methods, field specialisation or generalisation, to name just a few. But opinions of these methods vary. Some parents may favour alternative teaching methods, such as Montessori or Waldorf, while others firmly dislike such approaches and favour rather more

traditional teaching methods. There may also be disagreement over whether the teaching plans should aim to specialise in one specific field or not. Despite of these different opinions, there is some agreement that pupils should be educated in smaller classes. Above this uniform preference for smaller classes, class size is fairly easy to measure, to implement, and also to communicate politically, which makes it additionally favourable. Class size reduction is hence "probably the most popular and most funded school improvement policy" (Hoxby, 2000, p. 1).

Despite of the popularity of class size reduction, it is still empirically unclear whether the labour market values smaller classes through higher wages or not. Dustmann, Rajah, and van Soest (2003), for instance, find wage effects of class size, while Dolton and Vignoles (2000) find no significant relation between class size and earnings.

The effect on earnings that reduced classes may have in Germany has not yet been addressed in the economic literature. I analyse this issue, drawing on data from the German Socio-Economic Panel Study (SOEP). This data, however, does not provide any direct information on the size of attended school classes, nor any other indicator of the quality of schooling. I therefore collect regional information on class size and match it to the SOEP data. Using this extended data, I estimate wage regressions that control also for class size. However, I cannot confirm that class size would influence early career income.

This chapter unfolds as follows: The next Section reviews the related literature. Section 3.3 introduces the different modeling strategies that I found to be most often used in the economic literature. Section 3.4 describes the combined data before Section 3.5 presents the empirical results. Section 3.6 concludes.

3.2 Related literature

The literature on the quality of schooling assumes in most instances an educational production function, where quality as one input factor transforms through an unknown technology into some sort of output. Depending on how output is measured, the literature divides into two groups. One group focuses on in-school performance, measured by test scores or attainment, while the other group measures outcomes in the labour market – measured by earnings or employment probabilities.

Financial resources spent on schooling – as one rough measure for the quality of schooling – is mostly found to have a positive impact on attainment. That is, the more money is spent on education, the longer pupils stay in education. The positive relation between attainment – as measured by acquiring higher degrees and better test scores – and financial resources are found by Card and Krueger (1998), Dolton and Vignoles (2000), Sander (1993), Barro and Lee (2001), Wilson (2000), and West, Pennell, Travers, and West (2001). However, Wößmann (2000; 2001), analysing TIMMS[1] data, finds that it is important how the financial resources are utilised. In-school performance – as measured by test scores – is not associated with available resources. It is rather the institutional setting of schooling which is of much more importance. This is confirmed by Angrist and Lavy (2002), who find that computer-aided instructions, which require more financial resources, are not associated with higher test scores. Hanushek (2002) surveys this literature.

Even if better schooling is unable to increase the in-school performance, the labour market may value the different 'hard-skills' and 'soft-skills' learnt by pupils during their school career. Therefore, the effectiveness of schooling is often measured by individual earnings in the empirical literature.

The literature on the quality-earning link – that is the return on the qual-

[1]TIMMS: Third International Mathematics and Science Study.

ity of schooling – appears to be much less voluminous than the literature on the quantity-earning relations – which is the return to an extra year of schooling. The quantity-earning relation is reviewed by Ashenfelter, Harmon, and Oosterbeek (1999), Blundell, Card (1995), Card (1999), and Psacharopoulos (1994). In a recent publication, Harmon, Walker, and Westergaard-Nielsen (2001) collect evidence on the quantity-earning relation for a series of European countries, which also includes a section on Germany (Lauer and Steiner, 2001).

The return that an extra year of schooling yields may vary across schools, if they supply different qualities of education. An "objective 'market test'" – as Card and Krueger (1996b) name it – is met by schools that increase their pupils' subsequent earnings. Applying such a test, Card and Krueger find in a series of papers (1992a; 1992b; 1996a; 1996b; 1996c) supporting evidence for a positive quality-earning relation. Applying different measures for the quality of schooling – such as pupil-teacher ratio, average term length and relative teacher pay – they find that the return to an additional year of schooling is higher for those men who are educated in states that provide a higher quality of schooling.

Their result, however, is challenged by Betts (1995) who finds that traditional measures of school quality fail to explain subsequent earnings. He speculates that structural changes may have weakened a quality-earning relation in the United States. Card and Krueger (1992b; 1992a) analyse a cohort that enjoyed schooling before the 1960s, while Betts' (1995) cohort went to school later. The variation in US school quality, however, has converged in recent years, which may explain why studies focusing on a younger cohort are not able to find significant relations between quality and earnings. If school quality has diminishing effects, and if school quality improved over time, then current studies may be on the flatter part of the production function, where the variation in quality induces less variation in earnings compared to the steeper

part. Note that Card and Krueger observe rages from an earlier period when quality may have had stronger effects.[2]

Another explanation for the different findings is shown by Betts (1996, Figure 6.1). Surveying 24 articles he finds that studies which measure quality at the school level find insignificant effects of quality on earnings, while those studies that measure schooling on a more aggregate level are more likely to find a significant relation. A reason for this aggregation bias could be the omission of important state differences in school policy (Hanushek, Rivkin, and Taylor, 1996).[3]

Heckman, Layne-Farrar, and Todd (1995; 1996) directly examine Card and Krueger's conclusions and find that it is sensitive to the crucial assumption that migration is random and not based on different earning opportunities. Card and Krueger's strategy in estimating the effect of a particular school input is to compare the earnings of men who received their schooling in a different state than the one they currently live in. If migration is self-selecting, their results might be biased. It is unclear, Burtless (1996) notes, whether non-random migration significantly biases Card and Krueger's results.

The focus of the quality-earning literature is clearly on the US (Hanushek, 2002) but recent studies draw conclusions also from British data. These studies all use the same data source, the National Child Development Study (NCDS), which is a longitudinal survey of all British citizens born in one particular week in 1958. One of the advantages of this data is that it also provides standardised test scores at the age of seven and eleven that can stand proxy for ability, which is commonly unobserved in other data. Dolton and Vignoles

[2]This argument is based on the assumption that on higher levels of quality the marginal productivity of an improved quality may be significantly less than at lower levels of quality. This hypothesis, however, is challenged since developing countries, which may be rather on the steeper part of such a production function, lack significantly stronger quality effects (Hanushek, 1995)

[3]Fertig and Wright (2004) find such an aggregation bias also for class size effects on PISA test scores.

(2000) find that although the quality of schooling has a small positive impact on student attainment, there is no measurable relation between the quality of schooling and subsequent earnings. This finding is generally confirmed by Dearden, Ferri, and Meghir (2000), who find that the quality of schooling only has an impact on wages of women with low ability and by Harmon and Walker (2000), who also cannot find a significant quality-earning relation. In contrast, Dustmann, Rajah, and van Soest (2003) do find class size effects using the most recent NCDS wave, where the cohort is 42 years old. They argue that they find class size effects due to their structural approach. However, class size effects turn out to be significant for both genders only at the age of 42. At the age of 33, there appears to be no class size effect for females and at the age of 23 the class size effect disappears completely (Dustmann, Rajah, and van Soest, 2003, Table 10).

Recently, class size effects are also analysed using continental European data. Heinesen and Schindler Rangvid (2005) find that reducing the size of classes would not improve the employment probability of Danish pupils.

3.3 Econometric specification

A standard earning function regresses the log of hourly earnings $\log(y)$ on years of schooling (S) and a vector of other variables ($\mathbf{X}$) that may have an impact on earnings.

$$\log(y) = \alpha + \beta_1 S + \beta' \mathbf{X} + \varepsilon \tag{3.1}$$

The coefficient on schooling estimates the increase in earnings resulting from one additional year of schooling and is typically interpreted as the rate of return to schooling (Polachek and Siebert, 1993; Borjas, 2000).

This specification, however, does not account for differences in school quality. It is implicitly assumed in Specification 3.1 that a given level of schooling would yield a constant return at all levels of quality. To allow the quality of

schooling to have an impact on earnings, I assume β_1 to be a function of school quality (Q).

$$\log(y) = \alpha + \beta_1(Q)S + \beta'\mathbf{X} + \varepsilon$$

Since I do not know the functional form of $\beta_1(Q)$, I assume a quadratic approximation, as proposed for instance by Card and Krueger (1992a):

$$\beta_1(Q) = \gamma_1 + \gamma_2 Q + \gamma_3 Q^2$$

Lazear (2001) provides a theoretical model for optimal class size. He emphasises the public good aspect of classroom education. If one pupil disrupts the class, learning is reduced for all other pupils. The probability that one pupil disrupts rises with class size. On the other hand, pupils do learn from their peers. This suggests that class size is strictly concave, i.e. there are diminishing returns to quality for a given level of schooling. Substitution yields

$$\log(y) = \alpha + (\gamma_1 + \gamma_2 Q + \gamma_3 Q^2)S + \beta'\mathbf{X} + \varepsilon \tag{3.2}$$

I explore two further approaches to model the quality-earning relation. Both alternatives assume that the quality of schooling has an impact on effective schooling (S^*). That is, human capital depends on both quality and quantity of schooling:

$$\log(y) = \alpha + \beta_1 S^*(S; Q) + \beta'\mathbf{X} + \varepsilon.$$

Again, I do not know the functional form of $S^*(S; Q)$ and therefore first assume an additive relation between schooling and quality.

$$S^*(S; Q) = \varphi_1 + \varphi_2 S + \varphi_3 Q,$$

which yields by substitution

$$\log(y) = (\alpha + \phi_1) + \phi_1 S + \phi_3 Q + \beta'\mathbf{X} + \varepsilon \tag{3.3}$$

$$\text{with} \quad \phi_i = \beta_1 \varphi_i.$$

Or, allowing S and Q to interact

$$\log(y) = (\alpha + \phi_1) + \phi_1 S + \phi_3 Q + \phi_4 SQ + \beta' \mathbf{X} + \varepsilon \qquad (3.4)$$
$$\text{with} \quad \phi_i = \beta_1 \varphi_i.$$

Equation (3.3) is in fact the most often used specification if school quality is allowed for in British studies (c.f. Dearden, Ferri, and Meghir, 2000; Dolton and Vignoles, 2000; Harmon and Walker, 2000). In this setting, however, the quality of schooling leads to a parallel shift of the earnings function. In other words, the quality of schooling has the same effect at every year of schooling. Equation (3.4), on the other hand, allows the quality of schooling to have a potentially larger effect on early career income if the pupil stays in school longer. This is the setting used, for instance, by Betts (1995).

3.4 Data and variable definition

Card and Krueger (1996c; 1998) suggest that pupils who are educated at better schools should benefit more per year of schooling than those pupils attending lower quality schools. Probably the most popular method of improving the quality of schooling is to reduce the size of classes. There are also other ways to improve the quality of schooling. However, there seems to be a uniform preference for smaller classes, while other improvements – such as alternative teaching methods, content of teaching plans, etc. – may be disputed. I therefore measure the quality of schooling by class size.

There are, however, problems regarding how to measure class size properly, since more able pupils may be sorted into larger classes (Hanushek, 2002). More able pupils might better learn with and from their peers, whereas less able pupils might require more interaction with their teachers and do not perform well in small working groups. Hence, school directors may put pupils with learning difficulties into smaller classes if they have more classes per grade

in their schools. Moreover, the schooling regulations in the state of Berlin, for instance, reduce class size by 30 percent if pupils with explicit learning difficulties are integrated in ordinary classes (Baumgartner and Steiner, 2005a).

Measuring the quality of schooling at the school level raises a related problem. It might be that parents, who care more about their offspring's education, tend to move into catchment areas of higher quality schools (Leech and Campos, 2001). Moreover, if schools are in socially difficult areas, some school authorities provide more teaching resources for these schools in order to enable them to form smaller classes. The states of Bremen and Berlin are examples for applying such a policy (Baumgartner and Steiner, 2005a).

Averaging class size cancels out such sorting effects. Appendix 3.A.1 shows why individual class size is biased and how aggregation ma solve this problem. It is worth to mention the findings of Hanushek, Rivkin, and Taylor (1996) in this context, who find that studies which use less aggregated data produce more reliable estimates. They argue that determinants of school policy are likely to be omitted and that aggregation to the level of the omitted variable inflates the coefficient on class size. For these reasons, I aggregate class size only to the district level, where a district is defined to be either a 'Landkreis' (county council) or a 'Stadtkreis' (city council). School policy is determined on a much higher level of aggregation, namely on the 'Länder', i.e. state level.

I collected regional information on the number of pupils and classes from nine state offices for statistics in west Germany. By collecting this data, I had to exclude the states Berlin, Bremen and Hamburg, since these city states do not collect data on subsidiary political levels. With the collected information, I constructed a panel on class size from 1984 to 2000 across 324 west German districts. Table 3.a and 3.b in the appendix describe the cross sectional and the longitudinal context of my panel on class size. Schleswig-Holstein provide the smallest classes for pupils from lower and middle secondary schooling, while it

is Baden-Württemberg that provides the smallest classes for pupils attending upper secondary schooling. The standard error of the mean of class size is 6.9 for Hesse. This large variation is partly due to two outlying districts[4] that report an unreasonable large class size exceeding 100 (sic!).[5] Without these outlyers the variation remains fairly high and ranges from 23 to 50 pupils per class in the state of Hesse. The longitudinal variation is described in Table 3.b. The mean of class size is declining in all three types of schooling until 1989/1990, when the two parts of Germany united. The mean for classes of lower secondary schools increased till 1996 and has been declining again since then. For middle secondary schools, on the other hand, class size has increased continuously up to 2000. There is no clear pattern for the mean of class size in upper secondary schools in the first half of the 1990s. Since then the mean of class size is also increasing for upper secondary schools.

My empirical analysis is based on the German Socio-Economic Panel Study (SOEP).[6] This is a longitudinal survey of individuals living in private households in Germany covering each year since 1984. Although the SOEP is a rich data source on individuals and the households they live in, it provides rather less information on attended education.[7] I therefore match my constructed panel on class size on the SOEP data. I focus thereby on interviewees from which I observe the year in which they graduated from secondary school.[8] I have to assume that they graduated from a school that is in the same district as the one they life in. This year and regional information is used to merge the two data sets.

People can pursue further educational activities – such as vocational train-

[4]These two districts are Hersfeld-Rotenburg and Main-Taunus.

[5]These districts are not used for my estimation, since no individual graduated in one of these districts.

[6]Haisken-DeNew and Frick (2004) provide further information on the SOEP data.

[7]Since 2001, the SOEP has a new questionnaire (bioyouth) that provides more information on education but no measure for class size whatsoever.

[8]There is a tracking system in Germany. Hence, secondary schooling is divided into lower secondary, middle secondary, and upper secondary schooling.

ing and/or tertiary education – before they enter their first employment. To allow for these activities I take the last graduation year from either schooling, vocational training or tertiary education and add two further years before I measure labour income. Measuring income immediately after graduation would be likely to underestimate income when people have lower earnings during the transition into the labour market. Furthermore, I exclude professions which have few if any earnings from dependent employment; that is self-employed, freelance professionals, or working in a family business.

I can successfully add class size information to 1,074 individuals. My focus is on first employment. However, there is a so-called 'second chance education' ('zweiter Bildungsweg') in Germany.[9] The SOEP, however, does not provide any information on how the individuals obtained their schooling degree and so I assume those individuals that I consider to be too old to have gained their degree directly as graduates from 'second chance education'.[10] 80 graduates are considered to be graduates from 'second chance education' and hence not used in my estimations. I do not observe labour market incomes of 201 individuals two years after schooling, vocational training and/or tertiary education. Of the remaining 793 individuals, five individuals were not used since their household income is missing in the year of graduation from secondary schooling.

The SOEP provides information on actual and contractual hours worked per week. I use the maximum of these two to calculate hourly earnings. This avoids under counting the nominal 40 hours of salaried jobs or if a person employed full time actually only worked, say, 10 hours that interview week due to sickness but would normally work 40 hours. The OECD-MEI consumer price index deflates earnings, since data spans more than a decade. Bauer

[9]Employees can attend evening schools or pause their employment to attend a full time school to obtain a higher secondary schooling degree.

[10]Individuals are considered to be graduates from 'second chance education' if they are 20 years or older when they obtain their degree from lower or middle secondary education and 23 or older when they complete upper secondary education.

and Haisken-DeNew (2001), for instance, use the same approach to calculate hourly earnings.

The SOEP provides only information on the awarded degree of secondary education. It is unknown, however, if the interviewees attended school straight through or if they had to repeat grades.[11] Schooling is thus defined as the minimum years to reach the awarded degree. That is nine years for lower secondary schooling, ten years for middle secondary schooling and 13 years for having completed upper secondary schooling. The reason for this definition is that an additional year of schooling does yield a return but a repeated grade is assumed to yield a zero return. As mentioned above, the quality of schooling is measured by class size at the district level.

Table 3.1 describes the 788 individuals used for estimations. About four percent have mothers that have an upper secondary schooling degree and up to twelve percent have fathers with such a degree. Roughly 40 percent have Catholic parents and monthly household income is about 2,500 Euro in the year of graduation from secondary school. 17 percent are employed in the public sector and most individuals are employed by firms with more than 200 employees. Firm size is missing for about 6 percent; these individuals would have been lost if I had not included a dummy for this missing information. Years of schooling is 10.7 on average, where the types of schooling are equally distributed with about one third for each type. 60 percent pursued vocational training and 15 percent have also a degree from tertiary education. Class size is about 22 in lower secondary schooling and about 35 in upper secondary schooling.[12] In the estimations presented in the next section, I normalise class

[11]The SOEP does provide this information since 2001 but could not used in my analysis since I focus on graduates leaving school between 1984 and 2000.

[12]The large standard deviation of about 6.4 in upper secondary schooling reflects the fact that the smallest classes have 23 pupils in districts in Lower Saxony and 52 in districts in Hesse. Although this seems to be a very large figure these classes are not outliers, since I am observing class size in all ranges between 23 and 52. The large figure in Hesse is also not due to the reporting state office for statistics, since I also observe a class size of 25 in Hesse.

Table 3.1: Descriptive Statistics

dependend variable	
log(hourly earnings)	2.111
	(0.474)
socio-economic background	
female	0.486
mother completed upper sec. school	0.038
father completed upper sec. school	0.121
mother is Catholic	0.402
father is Catholic	0.381
houshold income in 1,000 Euro	2.486
	(1.983)
employer's characteristics	
public sector	0.170
firm size (20-199 employees)	0.320
firm size ($\geq$200 employees)	0.406
firm size (missing)	0.058
education	
years of schooling	10.706
	(1.688)
vocational training	0.607
tertiary education	0.150
class size lower sec. schooling	21.993
	(1.473)
class size middle sec. schooling	25.650
	(1.818)
class size upper sec. schooling	34.888
	(6.379)
state	
Schleswig-Holstein	0.019
Lower Saxony	0.128
North Rhine-Westphalia	0.299
Hesse	0.098
Rhinel.-Palatinate, Saarland	0.060
Baden-Wurttemberg	0.245
Bavaria	0.151
N	788

Standard errors are in parenthesis, where applicable.
Source: SOEP 1984-2004 and collected data on class size from nine state offices for statistics.

size and measure the deviation from the type specific mean of class size. In other words, the effect of a class size reduction by, say, one student is assumed to have the same effect in all three types of schooling.

3.5 Results

I combine micro data with aggregated data at the district level. Moulton (1990), however, raises the issue that with such an approach standard errors from ordinary least square regressions are downward biased and inferences are spurious if the disturbance terms are correlated within groups. Cohort members who share an observable characteristic – district of residence – may also

share unobservable characteristics that could lead the disturbance terms to be correlated across individuals within the same distinct. Roger (1993) provides an estimation method – clustered regressions – that controls for correlated disturbance terms. I apply this method and cluster the observations by the district where schooling was completed. That is, I specify the observations to be independent across clusters (districts) but not necessarily independent within clusters.

Table 3.2 presents in the first column a Mincer-type clustered ordinary least square regressions for early career earnings that control for the socio-economic background, for employer's characteristics, and for different levels of education. Columns two, three and four control also for the quality of education measured by class size using different specification. The federal states ('Bundesländer') are sovereign in setting their educational policy and hence there is institutional variation across states. An example of such an institutional variety is the duration of primary education, which is in some states four years, while in others it lasts up to six years (Baumgartner and Steiner, 2005a). School state dummies account for such differences. Lauer and Steiner (2001) as well as Bookmann and Steiner (2000) find the returns to education to be declining over time in Germany. I control therefore also for cohort effects by including graduation year dummies in the estimations.[13] Estimation results, which do not control for cohort nor school state effects are presented for illustrative reasons in Table 3.c and 3.d in the appendix.

Socio-economic background: Everything else being constant, the gender gap in early career earnings between males and females is -15.0 percent.[14] Nei-

[13]It were desirable to include also year dummies to account for calendar time effects. In the empirical analysis, however, it turned out that these dummies are collinear with graduation year dummies.

[14]Coefficients of dichotomous variables have to be transformed before they can be interpreted as percentage effect of a change from zero to unity (Halvorsen and Palmquist, 1980; Kennedy, 1981). The transformation for the estimated coefficient on the dummy variable

Table 3.2: Estimation results from pooled OLS controlling for fixed effects across graduation cohorts as well as state of graduation.

	(1)	(2)	(3)	(4)
constant	1.253	1.265	1.252	1.261
	(0.157)**	(0.166)**	(0.157)**	(0.157)**
socio-economc background				
female	−0.162	−0.162	−0.163	−0.162
	(0.030)**	(0.031)**	(0.030)**	(0.030)**
mother completed upper sec. school	−0.026	−0.024	−0.024	−0.024
	(0.082)	(0.083)	(0.083)	(0.084)
father completed upper sec. school	−0.107	−0.113	−0.112	−0.110
	(0.084)	(0.086)	(0.085)	(0.085)
mother is Catholic	0.014	0.011	0.011	0.009
	(0.040)	(0.040)	(0.040)	(0.039)
father is Catholic	0.039	0.043	0.043	0.044
	(0.042)	(0.042)	(0.042)	(0.041)
houshold income	0.020	0.021	0.021	0.021
	(0.011)+	(0.011)+	(0.011)+	(0.011)+
employer's characteristics				
public sector	−0.125	−0.123	−0.123	−0.126
	(0.036)**	(0.036)**	(0.036)**	(0.036)**
firm size (20-199 employees)	0.068	0.069	0.068	0.067
	(0.035)+	(0.035)+	(0.035)+	(0.035)+
firm size ($\geq$ 200 employees)	0.227	0.226	0.225	0.225
	(0.041)**	(0.041)**	(0.041)**	(0.041)**
firm size (missing)	0.111	0.107	0.107	0.104
	(0.084)	(0.083)	(0.083)	(0.082)
education				
years of schooling	0.036	0.035	0.037	0.038
	(0.012)**	(0.013)**	(0.012)**	(0.012)**
vocational training	0.280	0.276	0.276	0.277
	(0.034)**	(0.033)**	(0.033)**	(0.033)**
university	0.424	0.419	0.419	0.414
	(0.054)**	(0.056)**	(0.057)**	(0.057)**
quality of education				
years of schooling × class size $\times 10^{-3}$	–	0.518	–	−5.165
		(0.422)		(4.095)
years of schooling × (class size)^2	–	0.012	–	–
		(0.034)		
class size	–	–	0.008	0.074
			(0.005)	(0.053)
N	788	788	788	788
adjusted R^2	0.303	0.303	0.304	0.305
Wald	–	1.094	2.135	1.838
prob > Wald	–	0.337	0.145	0.162

- White (1980) heteroscedasticity corrected standard errors of clustered regression (Roger, 1993) across 217 clusters are shown in parenthesis.
- Significance levels: + 10 percent; * 5 percent; ** 1 percent.
- The Wald test tests whether the model can be restricted to model (1).
- *Source*: SOEP 1984-2004 and collected data on class size from nine state offices for statistics.

ther parents education nor their religious affiliation is estimated to be statistically significant. It is interesting to note that early career income is persistent. In other words, pupils from wealthier households receive ceteris paribus also a higher early career income when graduating from secondary schooling. If a household's income rises by one unit, which is 1,000 Euro in my data, the expected early career income raises by 2.0 percent. However, this is estimated to be only weakly significant, i.e. at the ten percent level.

Employer's characteristics: The expected early career income of people who start in the public sector is lower than that in the private sector, and early career earnings rise with firm size. The early career income gap between the private and the public sector is -11.7 percent. In firms with 20-199 employees, the expected early career income is 7.0 percent higher and in firms with 200 or more employees it is as much as 25.5 percent higher than in the reference category, which is firms with 1-19 employees.

Education: The more education is acquired, the higher the expected early career earnings. Being vocationally trained raises early career earnings by 32.3 percent, while having completed tertiary education raises early career earnings by as much as 52.8 percent. In other words, people who graduated from a university, or similar institution, have an expected early career earning that is 52.8 percent higher than that in the reference category. One extra year of secondary schooling increases early career earning by 3.7 percent, ceteris paribus. As mentioned in Section 3.4, there are three school tracks in Germany. Hence pupils who graduated from middle secondary schools earn between 1.3 and 6.0 percent more than their peers from lower secondary school. And graduates from upper secondary schools earn between 4.0 and 18.0 percent

$female$ is: $\exp(\beta_{female}) - 1 = \exp(-0.162) - 1 = 15.0$.

more than their peers from middle secondary schools.[15] These comparisons are valid only under ceteris paribus conditions. That is, I compare two pupils, who differ in their years of schooling but are, for instance, vocationally trained, have no university degrees, and both have the same socio-economic backgrounds and employer's characteristics.

Quality of education: The quality of education is measured by average class size at the district level, as discussed in Section 3.4. Column two, three, and four regress class size in the form of the specifications set out in Section 3.3. That is, specification (2) assumes the return to education to be a function of class size, specification (3) assumes quality to be additive and specification (4) allows for an interaction between the quantity and the quality of schooling. The adjusted R^2 measure – as reported at the bottom of Table 3.2 – indicates the explanatory power of the model. If I were to choose the specification that maximises the adjusted R^2, I would have opted for specification (4). However, the increase is rather minor and explains the data only 0.2 percentage points better than the base model. I add class size in alternative specifications to the base model as shown in the first column of Table 3.2. The base model is hence nested in specifications two to four which allows me to apply a Wald test. A Wald test measures if the model can be restricted. The restrictions are to set all respective class size coefficients on the quality of education equal to zero. The F-statistics for this Wald test as well as the corresponding p-values are also reported at the bottom of Table 3.2. The null hypothesis for this test is that the coefficients on the quality of education are jointly equal to zero. I cannot reject this null hypothesis neither at the five nor at the ten percent level of significance. It is hence suggested by the test to restrict the

[15]Pupils in middle secondary schools stay one year longer than their peers in lower secondary schools, while pupils in upper secondary schools stay three years longer than their peers in middle secondary schools. The rages are calculated confidence intervals, i.e. $CI = \beta_S \pm 1.96 \times s.e.(\beta_S)$.

specifications two, three and four, since controlling for class size effects adds no explanatory power to the base model. The effect class size might have on early career earnings is not only estimated to be statistically insignificant, the point estimates also imply these effects to be economically rather unimportant. The point estimate of specification four, for instance, implies that the expected early career earnings increase by 0.7 percent if class size at upper secondary schools was reduced by one student.[16] The implied effect is estimated to be higher for lower degrees, i.e. it is 2.2 percent for middle secondary schooling.[17] But since the point estimates are not significantly different from zero, I should not give too much emphasis on these point estimates.

3.6 Conclusion

Research on the relation between school quality and earning has focused mainly on the US and recently also on the UK. The main finding of this literature is that there appears to be no significant effect of class size on early career earnings. It was hypothesised in this chapter that a quality-earning relation for the German example could still exist, since the schooling system in Britain and the USA differ in many aspects from the schooling system in Germany. The findings presented in this chapter, however, reject this hypothesis. I could not find discernible effects of the quality of schooling measured by class size on early career earnings in Germany.

The cohorts of my data are too young to observe their complete age-earning profile. The variable to be explained is actually defined as earnings in the early career. But Burtless (1996) postulates that beneficial effects of school quality may not begin to emerge until the cohort has reached its peak earnings years,

[16]This percentage change is given by the first derivative of equation (3.4) with respect to class size, i.e. $\frac{\delta \log(y)}{\delta Q} = \phi_3 + \phi_4 S$. Since it takes 13 years to complete upper secondary schooling, I insert $S = 13$.

[17]Estimated with the same derivative as in footnote 16 but with $S = 10$.

namely around the age of forty. And indeed the two often cited studies by Card and Krueger (1992a; 1992b) do find a positive and significant quality-earning relation with a cohort that spans an entire working age. Dustmann, Rajah, and van Soest (2003) also find a positive relation between class size and earnings using the most recent NCDS wave where individuals are 42 years old, i.e. at an age where the age-earning profile peaks. All other studies[18], including my analysis for Germany, reject the quality-earning relation but analyse data that is limited by fairly young respondents. To combine SOEP data with my panel data on class size, I had to observe the year of graduation. Using this combined data prevented me from observing an age-earning profile at the peak for a sufficiently large sample today. In some years time, when data is available that spans a complete age-earning profile, further research will reveal whether there are beneficial class size effects on earnings later on in the labour market.

The policy conclusion suggested by my findings is thus that reducing class size would not improve early career earnings. No doubt there exist schools in impoverished areas where more spending is needed to raise school quality but on average school quality approached by class size has no significant effect on early career earnings.

However, earnings are just one indicator of labour market outcomes. An issue that is not addressed in this chapter is whether class size would have other economic gains. I observe earnings only if individuals are employed. But employability itself could be influenced by the quality of schooling in Germany, although Heinesen and Schindler Rangvid (2005) using Danish data do not find early career employment to be related to the size of classes. Furthermore, for a given educational attainment there appears to be no class size effect on earnings. Smaller classes, however, may encourage pupils to acquire higher levels

[18]Betts (1995; 1996), Dolton and Vignoles (2000), Dearden, Ferri, and Meghir (2000), Harmon and Walker (2000), Hanushek (2002), and Heckman, Layne-Farrar, and Todd (1995; 1996)

of schooling. Hence, it might be that an indirect link exists between quality and earnings, since more schooling returns higher earnings. More research on these issues promises to yield further interesting insights.

3.A Appendix

3.A.1 Aggregated vs. individual class size

Individual class size is likely to be biased in a standard wage regression, if more able pupils are sorted into larger classes.[19] In this appendix section, I argue that the bias, although present at the individual level, cancels out measuring class size on an aggregated level.

The natural logarithm of wages y_i is regressed on class size cs_i and a vector of other relevant variables captured in the vector $\mathbf{X}_i$.

$$y_i = \beta_0 + \beta_1 cs_i + \beta' \mathbf{X}_i + \epsilon_i$$

Since ability A_i is unobservable and hence not captured in the vector $\mathbf{X}_i$, an estimate of $\hat{\beta}_1$ is likely to be upward biased. This is, because

$$cs_i = \alpha_0 + \alpha_1 A_i + \upsilon_i$$

with $\hat{\alpha}_1 > 0$ and

$$y_i = \gamma_0 + \gamma_1 A_i + \nu_i$$

with $\hat{\gamma}_1 > 0$ results in

$$E(\hat{\beta}_1) = \beta_1 + \hat{\alpha}_1 \hat{\gamma}_1.$$

Hence $E(\hat{\beta}_2)$ is upward biased, since $\hat{\alpha}_1$ and $\hat{\gamma}_1$ are both likely to be positive (Wooldridge, 2003, p. 91). The positive correlation between class size and ability ($\hat{\alpha}_1 > 0$) may be due to a sorting effect as discussed in Section 3.4. School directors may put pupils with learning difficulties into smaller classes and school authorities may provide more teaching resources to schools in socially difficult areas to enable them to form smaller classes. Earnings and ability are also positively correlated ($\hat{\gamma}_1 > 0$), since ability raises marginal productivity and hence earnings.

[19] Hanushek (1996) and Hanushek, Rivkin, and Taylor (1996) motivate this point and Fertig and Wright (2004) provide a recent empirical validation.

However, I do not measure individual class size but class size aggregated to the district level. That is,

$$\overline{cs}_i = N^{-1} \sum_{n=1}^{N} cs_d,$$

where $\overline{cs}_i$ is the aggregated, i.e. average, class size of the district in which pupil i completed schooling, N is the number of classes over all schools within one district and cs_d is the size of each class within the district. cs_d, however, is actually

$$cs_d = bs + \upsilon + \tau,$$

where bs is the base size a class should have according to the state specific schooling regulations, i.e. bs varies across states. The fact that realised or actual class size deviates from the base size due to a different ability distribution within each class is captured by the scalar υ. If υ is normal distributed within districts with $E(\upsilon) = 0$ and var $= \sigma^2$, the effect ability may have on class size ($\overline{cs}_i$) cancels out due to aggregation, given N is sufficiently large. Exogenous demographic factors (τ) introduce an extra variation in cs_d and thus in $\overline{cs}_i$.[20] That is, τ has a effect on all classes within an district.

Therefore, the wage regression on aggregated class size

$$y_i = \beta_0 + \beta_1^* \overline{cs}_i + \beta' \mathbf{X}_i + \varepsilon_i$$

yields an unbiased estimate of $\hat{\beta}_1^*$, since $\hat{\alpha}_1^* = 0$ in

$$\overline{cs}_i = \alpha_0 + \alpha_1^* A_i + \epsilon_i,$$

which results in

$$E(\hat{\beta}_1^*) = \beta_1^*.$$[21]

[20] An example for such a demographic factor is the development of class size in and around the city of Leipzig. Families tend to move into rural areas surrounding Leipzig creating an upward pressure through τ_d in these rural areas and a downward pressure in the urban areas of Leipzig. (Baumgartner and Steiner, 2005a)

[21] $\hat{\alpha}_1^* \hat{\gamma}_1 = 0$ if $\hat{\alpha}_1^* = 0$.

3.A.2 Tables

Table 3.a: Descriptive statistics of average class size from 1984-2000 by state

	districts	lower sec. schooling	middle sec. schooling	upper sec. schooling
Baden-Württemberg	45	21.500	25.301	30.850
		(0.894)	(1.426)	(1.564)
Bavaria	96	23.336	26.690	32.263
		(0.946)	(1.604)	(1.671)
Hesse	26	20.946	25.305	43.213
		(2.921)	(3.374)	(6.889)
Lower Saxony	47	21.362	24.017	26.624
		(1.236)	(1.332)	(3.960)
Rhineland-Paatinate	35	22.212	26.214	37.983
		(1.004)	(1.641)	(3.065)
North Rhine-Westphalia	54	22.804	27.212	39.250
		(1.011)	(1.187)	(2.307)
Saarland	6	20.542	25.633	31.014
		(0.795)	(2.188)	(2.749)
Schleswig-Holstein	15	19.362	22.055	34.642
		(1.540)	(1.552)	(2.330)

Standard errors are in parenthesis.
Source: Collected data on class size from nine state offices for statistics.

Table 3.b: Descriptive statistics of average class size by year

	lower sec. schooling	middle sec. schooling	upper sec. schooling
1984	23.070	27.688	36.500
	(1.638)	(2.044)	(7.089)
1985	22.331	26.565	35.837
	(1.149)	(1.958)	(7.045)
1986	21.862	25.630	35.102
	(1.169)	(1.869)	(6.767)
1987	21.520	24.979	34.505
	(1.154)	(1.788)	(6.744)
1988	21.480	24.591	33.877
	(1.234)	(1.858)	(6.532)
1989	21.536	24.443	33.119
	(1.322)	(1.818)	(6.009)
1990	21.655	24.494	32.680
	(1.855)	(1.889)	(6.030)
1991	21.884	24.754	32.507
	(1.778)	(1.903)	(5.533)
1992	22.069	25.218	32.560
	(1.832)	(2.010)	(5.131)
1993	22.278	25.579	32.633
	(2.111)	(2.079)	(4.935)
1994	22.331	25.788	32.472
	(1.795)	(1.981)	(4.992)
1995	22.595	26.101	32.701
	(2.224)	(2.057)	(5.181)
1996	22.584	26.325	33.158
	(1.714)	(2.023)	(5.378)
1997	22.550	26.599	34.578
	(1.633)	(1.986)	(4.188)
1998	22.421	26.578	34.781
	(1.611)	(1.936)	(3.638)
1999	22.327	26.553	35.043
	(1.414)	(1.973)	(3.769)
2000	22.276	26.745	34.924
	(1.412)	(2.055)	(3.616)

Standard errors are in parenthesis.
Source: Collected data on class size from nine state offices for statistics.

Table 3.c: Estimation results from pooled OLS without fixed effects

	(1)	(2)	(3)	(4)
constant	1.377	1.352	1.375	1.375
	(0.118)**	(0.122)**	(0.119)**	(0.119)**
socio-economc background				
female	−0.160	−0.162	−0.161	−0.161
	(0.030)**	(0.031)**	(0.030)**	(0.031)**
mother completed upper sec. school	−0.021	−0.025	−0.023	−0.024
	(0.081)	(0.080)	(0.079)	(0.080)
father completed upper sec. school	−0.107	−0.108	−0.109	−0.109
	(0.083)	(0.086)	(0.085)	(0.085)
mother is Catholic	0.032	0.028	0.029	0.029
	(0.040)	(0.040)	(0.039)	(0.039)
father is Catholic	0.045	0.043	0.045	0.045
	(0.043)	(0.042)	(0.043)	(0.043)
houshold income	0.022	0.023	0.023	0.023
	(0.011)+	(0.012)+	(0.012)+	(0.012)+
employer's characteristics				
public sector	−0.114	−0.113	−0.113	−0.114
	(0.036)**	(0.036)**	(0.036)**	(0.036)**
firm size (20-199 employees)	−0.010	−0.012	−0.012	−0.012
	(0.036)	(0.036)	(0.036)	(0.035)
firm size ($\geq$ 200 employees)	0.177	0.175	0.175	0.175
	(0.041)**	(0.041)**	(0.041)**	(0.041)**
firm size (missing)	0.025	0.024	0.021	0.021
	(0.089)	(0.087)	(0.088)	(0.088)
education				
years of schooling	0.044	0.047	0.044	0.045
	(0.011)**	(0.012)**	(0.012)**	(0.012)**
vocational training	0.263	0.263	0.262	0.262
	(0.034)**	(0.034)**	(0.034)**	(0.034)**
university	0.393	0.395	0.392	0.392
	(0.056)**	(0.058)**	(0.057)**	(0.057)**
quality of education				
years of schooling × class size $\times 10^{-3}$	–	0.228	–	−0.639
		(0.351)		(3.377)
years of schooling × (class size)^2	–	−0.022	–	–
		(0.035)		
class size	–	–	0.003	0.011
			(0.004)	(0.043)
N	788	788	788	788
adjusted R^2	0.263	0.262	0.262	0.262
Wald	–	0.350	0.382	0.216
prob > Wald	–	0.705	0.537	0.806

- White (1980) heteroscedasticity corrected standard errors of clustered regression (Roger, 1993) across 217 clusters are shown in parenthesis.
- Significance levels: + 10 percent; * 5 percent; ** 1 percent.
- The Wald test tests whether the model can be restricted to model (1).
- *Source*: SOEP 1984-2004 and collected data on class size from nine state offices for statistics.

Table 3.d: Estimation results from pooled OLS controlling for fixed effects across states of graduation.

	(1)	(2)	(3)	(4)
constant	1.435	1.435	1.438	1.441
	(0.145)**	(0.148)**	(0.143)**	(0.145)**
socio-economc background				
female	−0.160	−0.161	−0.160	−0.161
	(0.031)**	(0.031)**	(0.031)**	(0.031)**
mother completed upper sec. school	−0.018	−0.018	−0.018	−0.018
	(0.082)	(0.082)	(0.082)	(0.082)
father completed upper sec. school	−0.116	−0.117	−0.117	−0.117
	(0.084)	(0.086)	(0.085)	(0.086)
mother is Catholic	0.025	0.024	0.024	0.024
	(0.041)	(0.041)	(0.041)	(0.041)
father is Catholic	0.039	0.040	0.040	0.040
	(0.043)	(0.043)	(0.043)	(0.043)
houshold income	0.023	0.023	0.023	0.023
	(0.012)+	(0.012)+	(0.012)+	(0.012)+
employer's characteristics				
public sector	−0.117	−0.116	−0.116	−0.116
	(0.037)**	(0.036)**	(0.037)**	(0.037)**
firm size (20-199 employees)	−0.016	−0.017	−0.017	−0.018
	(0.035)	(0.035)	(0.035)	(0.035)
firm size ($\geq$ 200 employees)	0.173	0.172	0.172	0.172
	(0.041)**	(0.041)**	(0.041)**	(0.041)**
firm size (missing)	0.030	0.029	0.029	0.028
	(0.087)	(0.086)	(0.086)	(0.087)
education				
years of schooling	0.045	0.046	0.046	0.046
	(0.011)**	(0.012)**	(0.011)**	(0.011)**
vocational training	0.264	0.263	0.263	0.263
	(0.034)**	(0.033)**	(0.033)**	(0.033)**
university	0.397	0.397	0.397	0.397
	(0.058)**	(0.059)**	(0.059)**	(0.059)**
quality of education				
years of schooling × class size $\times 10^{-3}$	–	0.126	–	−0.548
		(0.393)		(3.667)
years of schooling × (class size)^2	–	−0.002	–	–
		(0.034)		
class size	–	–	0.002	0.009
			(0.005)	(0.047)
N	788	788	788	788
adjusted R^2	0.265	0.264	0.265	0.264
Wald	–	0.053	0.116	0.068
prob > Wald	–	0.949	0.734	0.934

- White (1980) heteroscedasticity corrected standard errors of clustered regression (Roger, 1993) across 217 clusters are shown in parenthesis.
- Significance levels: + 10 percent; * 5 percent; ** 1 percent.
- The Wald test tests whether the model can be restricted to model (1).
- *Source*: SOEP 1984-2004 and collected data on class size from nine state offices for statistics.

Chapter 4

Student aid in Germany

4.1 Introduction

In Germany, students from low-income families are eligible for financial aid under the federal students' financial assistance scheme (Berufsausbildungsfoerderungsgesetz, BAfoeG). This student aid aims to allow all qualified young people to pursue higher education regardless of their parents' financial capacity. BAfoeG covers a substantial share of the monthly living costs of students enrolled at universities.

Efficiency as well as income-distribution arguments justify financial support for students pursuing higher education (see for instance Poterba, 1996; Barr, 2003; Barr, 2004). First, there may be positive external effects in the sense that social returns may exceed private returns to higher education. These may arise from progressive taxation and reduced welfare dependency of highly educated people, or spill-over effects from a highly educated and trained workforce, such as innovation and economic growth. Second, students from low-income families may invest too little in education due to potential credit constraints. Governments may therefore want to provide subsidised loans or grants to students to foster 'equal opportunities' for otherwise disadvantaged pupils. These arguments also dominate current discussions about on the financing of higher education in Germany.

The effectiveness of BAfoeG, however, is rather unexplored in the economic

literature. Only few evaluations exist of whether BAfoeG is able to effectively increase enrolment of the targeted students as it aims to.[1] BAfoeG has been reformed many times since it was introduced in 1971. In this chapter I evaluate two major BAfoeG reforms – the 2001 student aid reform and the 1990 student aid reform. Both reforms were explicitly targeted at students from low-income families, since it is believed that these students are kept out of tertiary education due to credit constraints. In 2001, the underlying regulations as to how the means-tested student aid is calculated were changed. This change resulted in an increase of the monthly student aid by more than ten percent on average. The reform of 1990 left the monthly support unchanged but switched BAfoeG from a full loan system to a 50 percent loan, 50 percent grant regime. This implied that the debt burden of a fully supported student was on average reduced by some 23,500 DEM (12,000 EUR) from 47,000 DEM (24,000 EUR). Both reforms decreased substantially the net cost of study and were expected to increase the enrolment of eligible students.

I consider both reforms as 'natural experiments' to identify the effect of student aid on enrolment rates in Germany. I evaluate the effectiveness of these reforms by applying a microeconometric transition rate model based on the difference-in-difference methodology. Drawing on data from the German Socio-Economic Panel Study (SOEP), I find that neither reform has had any effect on enrolment. This somewhat surprising result may have important implications for the current policy debate on how to finance and secure access to higher education in Germany, and elsewhere.

The remainder of this chapter develops as follows: In the next section I summarise empirical studies about the effect of student aid on enrolment decisions. Section 4.3 provides a thorough overview of BAfoeG and the two evaluated reforms. Sample design and the simulation model for BAfoeG eligi-

[1] Lauer (2002) and Baumgartner and Steiner (2005b; 2006) evaluate BAfoeG and are reviewed in Section 4.2.

bility are discussed in Section 4.4. The empirical strategy is set out in Section 4.5 and Sections 4.6 and 4.7 present the empirical results for the student aid reforms of 2001 and 1990, respectively. Section 4.8 concludes.

4.2 Literature

Following Becker (1962), economists usually analyse the decision to enrol in higher education in the framework of human capital theory. According to standard human capital theory, an individual's education decision depends on the comparison between the discounted costs and future returns of an additional year of education. The higher the private costs of an additional year in education, the higher its private return in terms of future wages has to be to induce the individual to invest in education. In this simple model, higher direct costs of educational investment, such as tuition fees, would be associated with a lower optimal level of education and a higher private return to education. Likewise, lower financial educational net costs, by subsidised loans or grants to finance tuition fees and living expenses, would increase the optimal level of education and reduce its private return. Under the assumption of perfect capital markets, the private return to education just equates its private marginal costs in a present value sense. Given time preferences, the optimum level of education is then defined by individual abilities or learning productivities. For a given ability level, individuals with a higher rate of time preference (individual discount rate) will choose a lower level of education, other things being equal, and realise a higher return to education. Likewise, individuals who have to pay a higher rate of interest to finance their educational investment will choose a lower investment level. Given imperfect capital markets and/or a higher time preference prevailing among low-income households, the optimum level of investment in education would negatively depend on household income even if 'ability' between students was controlled for. Of course, if imperfec-

tions in capital markets did not only result in different market interest rates but in binding credit constraints, this effect would be even stronger (Kodde and Ritzen, 1985).

There are various distribution and allocation arguments for state intervention in the market for higher education as summarised by Hanushek (2002) and Poterba (1996). One of the most important concerns the supposed existence of binding credit constraints for students from low-income families. Not surprisingly, in this case state intervention can be rationalised not only by a distribution argument but also for reasons of economic efficiency. The efficiency argument is even stronger in the case of the external effects of higher education, i.e. if the social return of higher education exceeds its private return. In such case, higher enrolment rates in tertiary education may in fact lead to higher economic growth and social welfare. Another important issue in education policy concerns the relative efficiency of loans and grants subsidised and/or guaranteed by the State, and its financing of increasing enrolment rates in higher education (Barr and Crawford, 1998; Barr, 2003). The positive correlation between enrolment rates and the level of parents' income found in several empirical studies for various countries has typically been interpreted as evidence for the existence of credit constraints in the market for student loans. This empirical correlation has been used to rationalise the provision of loans or grants along the lines mentioned above.

This causal interpretation, however, is criticised on the basis of recent empirical research. Cameron and Heckman (1998) and Carneiro and Heckman (2002) show that the observed correlation between students' educational achievement and parental income can also be explained by a positive correlation between the latter and parental ability, which itself is correlated with their children's ability. In another line of recent empirical research, it has been shown that the potential effects of credit constraints may be very much mit-

igated if there is the option of paid employment for students while enrolled at a college, and that the mentioned positive correlation can be explained by parental financial transfers which act the same way as a reduction of tuition fees (Keane and Wolpin, 2001; Keane, 2002).

In contrast to this more structural estimation approach, recent empirical research on the effects of loans and grants on decisions to enrol in higher education simply relate this decision to variables suggested by basic human capital theory. These variables typically include financial indicators, such as parents' income, tuition fees, and student loans or grants. Most of these studies have been undertaken for the United States, while it seems that this topic has remained rather unexplored for Germany so far. As summarised by McPherson and Schapiro (1991), most US studies up to the beginning of the 1990s tend to find statistically and economically significant positive effects of financial factors on enrolment decisions. These studies also tend to find that the responsiveness of enrolment decisions is higher for students from low-income families. McPherson and Schapiro's (1991) empirical investigation based on time series data on enrolment rates for three income groups between 1974 and 1984 finds that reducing the net costs by 1,000 US-dollars, which is defined as the difference between tuition fees and the subsidy value of the student aid, would increase the enrolment of low-income students on average by about 6.8 percentage points, but would have an insignificant effect on the enrolment rates of students from higher-income groups. This is usually interpreted as evidence supporting the hypothesis of credit constraints in education choices.

In a couple of recent related papers, Dynarski analyses the effect of various policy changes related to financial aid on students' college enrolment decisions in the US. She interprets these policy changes as 'natural experiments', since they affect not all students. This allows comparison between two groups of students over time – students who experienced a policy change with students who

did not experience such a change. This difference-in-difference methodology is thoroughly introduced in Section 4.5.1. Dynarski (2002a) uses a difference-in-difference methodology on data from the Current Population Survey. The exogenous variation used to analyse college entry is the introduction of the Georgia HOPE scholarship which allows free attendance at the state's public colleges for residents with a certain minimum scholarly attainment in high school. The control group is composed of college freshmen in other south-eastern states. She finds that the introduction of the scholarship increased college attendance by 7.9 percentage points. Using the same difference-in-difference methodology, Dynarski (2003) analyses the impact of the elimination of the US Social Security Student Benefit Program in 1982. The removal of this programme affected young people entitled to student aid awarded to those who had experienced the death of a parent during their childhood. Dynarski (2003) finds that this policy change has increased enrolment rates by about 18 percentage points on average. This relatively large effect is, however, not comparable to the effect for the HOPE scholarship programme because these programmes affected different groups of people. In another paper also based on a difference-in-difference methodology, Dynarski (2002b) uses as a 'natural experiment' the removal of home equity from the set of assets that are taken into account for assessment in the federal financial aid formula by the US Higher Education Amendment in 1992. Since home equity is a large proportion of US household net worth, this change was expected to have a strong impact on students' eligibility for financial aid and, hence, on enrolment decisions. Although the effect of this policy shift on enrolment rates is insignificant in the full sample, she detects a significant positive effect on a sub-sample of students whose parents have some home equity, arguably the group of people most affected by the policy change.

In another frequently cited paper, Kane (1995) also applies a difference-in-

difference methodology to evaluate the introduction of the Pell Grant Program in the US, which is similar to the German BAfoeG by providing means-tested financial support for students from low-income families. He compares the years around the introduction in 1973 and defines eligible students as the treatment group. According to his estimates, the introduction of the Pell Grant Program had no effect on enrolment rates in higher education.

It appears that the effect student aid may have on enrolment in higher education in Germany is rather unexplored in the economic literature.[2] Lauer (2002) includes some indicators for the provision of BAfoeG, derived from the German Socio-Economic Panel Study (SOEP), as explanatory variables in a discrete choice model. Her empirical results suggest the enrolment to be positively related to BAfoeG entitlement, the amount of student aid as well as the loan share of BAfoeG. Baumgartner and Steiner (2005b) control for potential endogeneity of student aid on enrolment by applying a difference-in-difference estimator. The exogenous variation they are exploring comes from a discrete policy change on the repayment obligations, i.e. a change from a pure loan regime to a regime where half of student aid is distributed as a grant. They do not find student aid to influence the enrolment decision of the targeted students. In this chapter, I extend the approach of Baumgartner and Steiner (2005b) and apply the difference-in-difference estimator using a discrete time hazard rate model to analyse two student aid reforms in Germany.

4.3 Student aid in Germany

In order to provide some institutional background, I start with a brief description of student aid in Germany, which is regulated under the German federal financial assistance scheme to promote education (*Bundesausbildungsfoerderunggesetz, BAfoeG*). When introduced in 1971, this law was meant to

[2]There seems to be only very few empirical studies on this topic for other European countries (Winter-Ebmer and Wirz, 2002).

allow all qualified young people to pursue higher education regardless of their parents' financial capacity. BAfoeG has been changed many times since then and today also subsidises pupils in secondary education, further education, and also those enrolled in vocational training courses. The main political goal of BAfoeG remains, nonetheless, to encourage students from low-income families to pursue higher education.

Student aid is means tested, i.e. it depends on the financial capacity of parents or, in the rare circumstances when students are married, on the financial capacity of their spouse. BAfoeG defines a maintenance need (*Bedarfssatz*) that depends on whether the students live with their parents, and how they are insured for health care. This maintenance need is compared to parents' financial capacity. The definition of the financial capacity takes various sources of income into account, i.e. it is reduced by actual paid taxes, a lump sum for social security contributions and considers also if an obligation exists to pay alimony for other people. And finally, there are absolute and relative basic allowances, which also reduce the defined financial capacity. Students are eligible for BAfoeG if the calculated maintenance need exceeds the calculated financial capacity of the parents. I discuss the calculation of student aid in more detail in Section 4.4.2 where I introduce a model to simulate BAfoeG for all the individuals in my data.

Initially, BAfoeG was provided in the form of a non-repayable grant to most eligible students. Only in a few instances was it granted as a loan. This was the case if the study subject was changed, or if the student had already completed a university degree and was continuing to study for another degree (*Zweitstudium*). The amounts for maintenance needs and basic allowances are adjusted on a bi-yearly basis in order to keep pace with the cost of living.

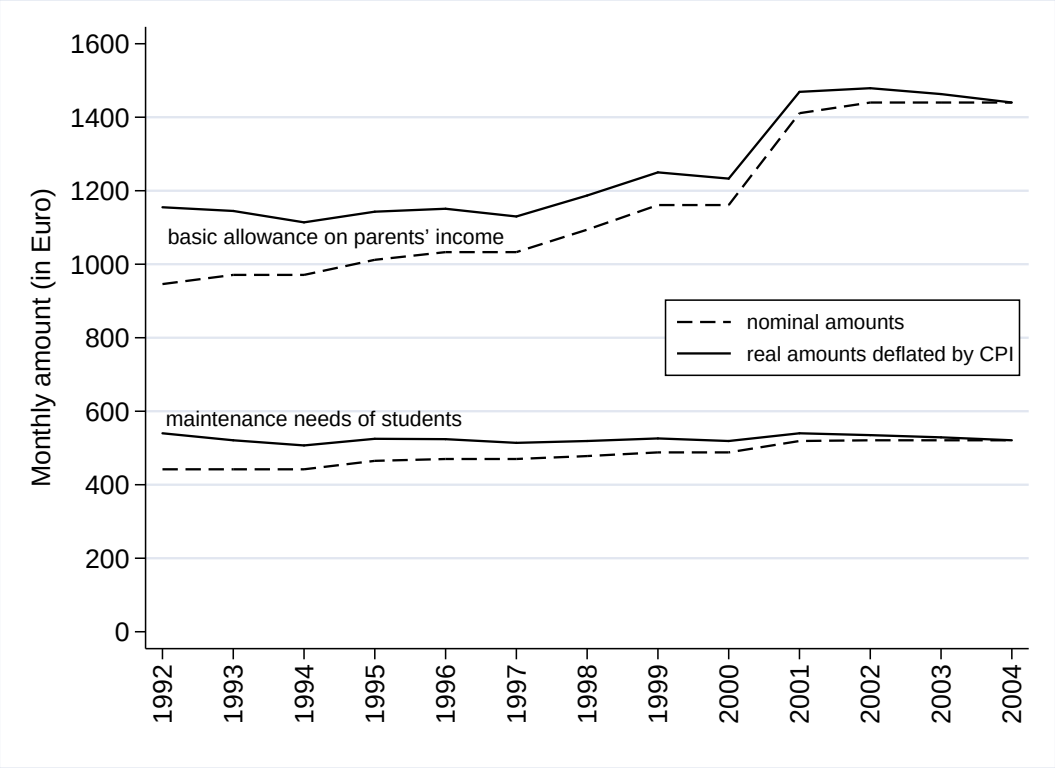
In winter 1983/84, the first major BAfoeG reform changed the repayment regulations from a non-repayable grant to an interest-free loan that had to be

paid back from future earnings. This change was associated with a marked reduction in the enrolment rates of graduates from low-income families, which dropped by some 18 percentage points from 81 percent to 63 percent between 1976 and 1986 (Beirat für Ausbildungsförderung, 1988, Table 26). On the basis of this observation the Advisory Board on BAfoeG (*Beirat für Ausbildungsförderung*) suggested reforming the repayment obligations and splitting the financial aid into a 50 percent grant and a 50 percent loan. It was expected by the board that such a substantial reduction of the BAfoeG debt burden would motivate students from low-income families to increase their enrolment in tertiary education (Beirat für Ausbildungsförderung, 1988, p. 111).

Following the suggestions of the Advisory Board, the 12th revision of student aid reformed the repayment regulation in July 1990. From then onwards, all supported students have had to pay back only 50 percent of the support from their future earnings. The eligibility criteria for BAfoeG remained by and large unchanged. Hence, this discrete change in the BAfoeG repayment regulations affected the net present value of the financial aid. For a fully supported student, this changed repayment regulation meant, on average, a reduction of the debt burden from about 47,000 DEM (24,000 EUR) to about 23,500 DEM (12,000 EUR) (HIS, 1987, p. 10).

The most recent student aid reform took effect on 1st April 2001. The eligibility rules were changed, i.e. the regulation for how student aid is calculated. The basic allowance on parents' income was increased by some 20 percent and the maintenance needs were also lifted by some 6 percent and outpaced the increase of living costs. Figure 4.1 depicts the development of the basic allowance on parents' income and the maintenance needs of students, arguably the two most important parameters in calculating the monthly support. The dashed lines show the parameters as they appear in the law and the solid lines indicate the parameters deflated by the consumer price index (CPI), i.e. in

Figure 4.1: Basic allowance on parents' income and maintenance needs

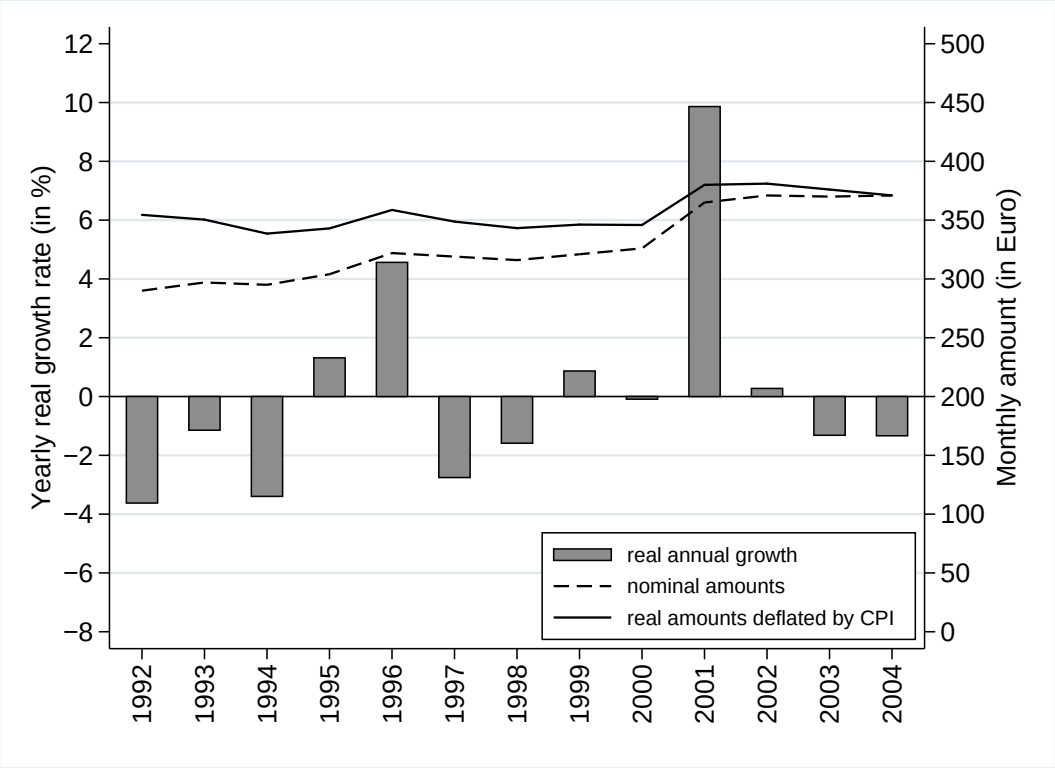


Source: Reports of the student aid council (Bericht des Beirats für Ausbildungsförderung); various years; own calculation.

2004 prices. In the early 1990s, the adjustment of the federal students' financial assistance scheme did not keep pace with the price development. This decline was neutralised in the late 1990s and resulted in a gentle U-shape of the basic allowance. In 2001, the basic allowance jumped upwards by some 20 percent and maintenance needs by some 6 percent. This discrete change in the eligibility rules resulted in an average growth rate of student aid by more than ten percent, as shown in Figure 4.2.

This discrete policy change was arguably not forseeable by students. The winter term starts at all German universities by the 1st October. Due to the central course allocation agency (*Zentrale Studienplatzvergabe, ZVS*) and internal regulations of the universities, students have to apply in almost all circumstances no later than 15th July to commence studying in the coming winter term. Ms. Edelgard Bulmahn – the Federal Minister for Education

Figure 4.2: Real and nominal average student aid and yearly growth



Source: Federal Statistical Office Germany; Fachserie 11 Reihe 7; various years; own calculation.

and Research in charge at this time – announced at a press conference in October 2000 that she was planning to use the revenues generated from selling the UMTS licences to reform BAfoeG. The German parliament resolved this proposal in February 2001, which came into effect on 1st April 2001, as the new summer term started. It was hence not possible for the students to defer their enrolment to the summer term in order to take advantage of the increased student aid right from their first semesters, since the political debate about reforming BAfoeG started in the very same month when the winter term was starting – namely October 2000.

4.4 Data

I discuss the sample design and the simulation model for BAfoeG eligibility in the next two subsections.

4.4.1 Sample design

My empirical evaluation of student aid is based on the German Socio-Economic Panel Study (SOEP), which is a longitudinal survey of individuals living in private households in Germany.[3] I restrict the sample to people who have completed upper secondary schooling, since only those are entitled to enrol in higher education in Germany. I allow for a transition window of up to three years. That is, I follow up school leavers from upper secondary schooling for as much as three years. If they do not enrol within this transition window, I count them as non-students. Appendix 4.A.1 explains the construction of the relevant information in more detail.

Whether an individual is eligible for student aid depends, as mentioned above, mainly on the financial capacity of the parents relative to the maintenance need of the student. Whether an individual receives BAfoeG is only observed for students and not for those who decided not to enrol in tertiary education, even though these individuals might be eligible to student aid due to the relatively low income of their parents. Potential eligibility has thus to be inferred from parents' income and other relevant information contained in the SOEP data. I built hence a model to simulate BAfoeG eligibility for all individuals and for each year within the observation period. The simulation approach is described in the next section.

4.4.2 Simulated BAföG

Whether an individual receives financial support through BAfoeG or not is only observed for students enrolled in higher education. But since BAfoeG is means tested, some individuals would be eligible if they enrolled in higher education. Since this counterfactual BAfoeG eligibility is not observable, I apply a small micro simulation model to infer who is eligible for the financial

[3]Haisken-DeNew and Frick (2004) provide detailed information on the SOEP data.

study support and who is not. In this section, I briefly describe the simulation model.

The BAfoeG regulations define the maintenance need for students conditional on the student's living situation, i.e. whether she lives with her parents or on her own, and whether the student is covered as a family member in the public health insurance or whether she is insured on her own. This maintenance need is reduced by the financial capacities of the student, her husband, and her parents. However, it is parents' income that predominantly determines BAfoeG eligibility. Only 3.2 percent of all BAfoeG recipients at universities are actually married in 2004 (Federal Statistical Office Germany; Fachserie 11 Reihe 7) and nobody is married in my sample, since I am focusing on school leavers and university freshmen. I abstract from own income in the simulation model since not enrolled students have naturally a much higher income from labour market activities than enrolled students.

The relevant income for defining parents' financial capacities is post-tax income, asset and pension earnings from the father and the mother. Income tax and social security liabilities are only available for the household and not for the individuals. I depart thus from the individual level and calculate the relevant income on the household level. If parents live together, this is straightforward. If they, however, do not share the same household, I take net-tax income from the father's and the mother's household separately. If the father or the mother shares the household with another spouse, I take half the net-tax household income. An allowance is granted on parents' income conditional on the family status and for each child covered by alimony. A share of the remaining income – that again depends on the amount of children covered by alimony – is then subtracted from the maintenance needs. If the difference is positive, the individual is eligible ($BE_t = 1$) for the study support and vice versa.

These two equations may clarify the simulation idea further.

$$
\begin{aligned}
BAfoeG_t \;=\; & (maintenance\ needs|living\ status)_t \\
& -\Big\{\Big[(parents'\ income)_t - (allowance_1|family\ status)_t \\
& -(allowance_2|family\ status)_t\Big] \\
& \times\Big[1-(allowance_3+allowance_4\times children)_t\Big]\Big\}
\end{aligned} \tag{4.1}
$$

$$
BE_t = \begin{cases} 1 \text{ if } BAfoeG_t > 0 \\ 0 \text{ if } BAfoeG_t \leq 0 \end{cases} \tag{4.2}
$$

The simulations routine runs for each individual over all observed periods.

4.5 Empirical strategy

In the next three subsections, I develop the empirical strategy applied in this chapter to evaluate the two student aid reforms. In the first subsection, I introduce the simple difference-in-difference estimator and the key identifying assumption necessary for estimating the average treatment effect on the treated. Since student aid is not binary but fades out as parents' financial capacity increases, I extend the simple difference-in-difference estimator and differentiate the treatment effects. As has been discussed in the literature review in Section 4.2, most evaluations of student aid apply the difference-in-difference estimator comparing enrolment probabilities in a static way. However, BAfoeG aims to increase enrolment, i.e. effects the transition from upper secondary schooling into tertiary education, I estimate thus a difference-in-difference within a discrete hazard rate model, which is specified in Subsection 4.5.3.

4.5.1 Simple difference-in-difference

A simple difference-in-difference estimation compares the mean enrolment decision of two groups (first difference): a treatment group – eligible students – and a comparison group that is not affected by the policy shift – ineligible

students. This difference is then compared between the two time periods: before and after the discrete policy shift (second difference). Thus, the simple difference-in-difference estimator is:

$$\begin{aligned} \alpha_{ATT} &= [S(EB=1, D=1) - S(EB=1, D=0)] \\ &- [S(EB=0, D=1) - S(EB=0, D=0)], \end{aligned} \tag{4.3}$$

where $S(EB, D)$ is the share of people enrolled at university with EB as an indicator variable showing the eligibility status for BAfoeG, i.e. $EB = 1$ if the student is eligible for BAfoeG and with D as an indicator variable that takes on the value of unity after the policy change. α_{ATT} hence measures the growth in enrolment of eligible students, which is 'extra' to the growth of the control group.

The coefficient α_{ATT} measures the average effect of the reform on the enrolment share in the group of people affected by the reform, which is also known as the "average treatment effect on the treated" in the empirical policy evaluation literature (see for instance Angrist and Krueger, 1999; Blundell and Costa Dias, 2000; Meyer, 1995). The key identifying assumption is that the causal effect would be zero in the absence of the policy shift, i.e. any shift in the probability of enrolment of eligible students is attributable to the policy change.

The simple difference-in-difference estimator from Equation (4.3) is equivalent to the coefficient on the interaction term in the following pooled linear regression:

$$S_{it} = \alpha_0 + \alpha_1 EB_{it} + \alpha_2 D_{it} + \alpha_3 (EB \times D)_{it} + \upsilon_{it}, \tag{4.4}$$

where S_{it} is the schooling variable for person i in period t, EB and D are dichotomous variables as defined above, υ is an error term and α_j are parameters to be estimated. In order to yield unbiased estimates of the parameters in regression (4.4), the key identifying assumption mentioned above has to hold.

This implies that the expectation of the difference of the error terms before and after the policy change is the same for both groups, i.e.:

$$E(\upsilon_{i1} - \upsilon_{i0}|EB = 1) = E(\upsilon_{i1} - \upsilon_{i0}|EB = 0). \tag{4.5}$$

The coefficient α_3 identifies the average treatment effect on the treated (α_{ATT}), since taking the difference of Equation (4.4) with respect to EB and D yields

$$\frac{\Delta^2 S}{\Delta EB \Delta D} = \alpha_3 = \alpha_{ATT}. \tag{4.6}$$

This intuition, however, does not extend to non-linear model specifications, for instance, if S_{it} is estimated by a logit model, i.e.:

$$S_{it} = \Lambda\big(\alpha_0 + \alpha_1 EB_{it} + \alpha_2 D_{it} + \alpha_3 (EB \times D)_{it}\big), \tag{4.7}$$

where $\Lambda(z)$ is the logit function ($\frac{\exp(z)}{1+\exp(z)}$). Taking the cross difference of equation (4.7) with respect to EB and D yields[4]

$$\alpha_{ATT} \equiv \frac{\Delta^2 S}{\Delta EB \Delta D} = \alpha_3 \frac{\Delta \Lambda(z)}{\Delta z} + (\alpha_1 + \alpha_3 D)(\alpha_2 + \alpha_3 EB)\frac{\Delta^2 \Lambda(z)}{\Delta z^2}. \tag{4.8}$$

The average treatment effect on the treated estimated by a non-linear model depends therefore not only on the coefficient of the interaction term (α_3) but also on all other coefficients as well as the value of the other regressors. Due to this interdependence, the average treatment effect on the treated may have another sign than the coefficient α_3 or may be non-zero although α_3 is estimated to be insignificant. Ai and Norton (2003) highlight this issue and discuss its implication in more detail.

4.5.2 Heterogeneous versus average treatment

So far, I have discussed BafoeG eligibility as a binary variable. Students are either eligible for student aid ($EB = 1$) or they are not eligible for student aid ($EB = 0$). However, the monthly support a student may receive is not

[4]Equation (4.8) is derived in Appendix 4.A.2.

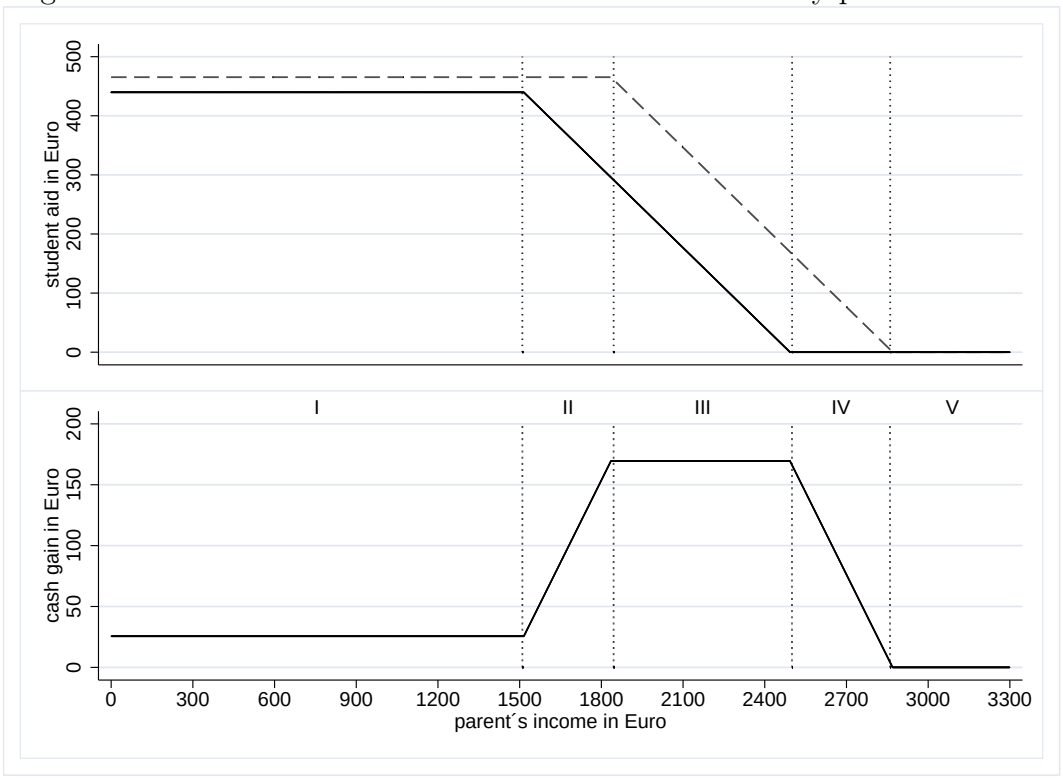
binary. That is, students may receive the full student aid (*Vollförderung*) if their parents' financial capacity is less than or equal to zero. With increasing parents' financial capacity, student aid fades out (*Teilförderung*) till the financial capacity exceeds the pre-defined maintenance needs. In other words, if the subtrahend in Equation (4.1) is less than or equal to zero, the student receives the full student aid and respectively less as the subtrahend increases.

To differentiate between students who receive the full support and those whose student aid is deducted is unimportant when I evaluate the student aid reform of 1990. The change in repayment obligations had an equal effect on both groups of students cutting the interest-free loan to a 50 percent grant. Evaluating the student aid reform of 2001, however, it might be more important to distinguish between these two groups of supported students, since the increased basic allowance and maintenance need increases student aid in a different way for fully supported students and for partly supported students.

I stress that the 2001 student aid reform introduces heterogeneity in the treatment itself which may reflect different outcomes. The point I am making should hence not be confounded with the literature on heterogeneous responses when the treatment is binary (see for instance Moffitt, 1999).

The eligibility status is simulated by the microsimulation model as presented in Section 4.4.2. The simulation model allows not only defining BAfoeG eligibility regardless of the student status, it also allows distinguishing between fully and partly supported students. Figure 4.3 depicts the relation between student aid (on the vertical axis) and parents' income (on the horizontal axis). The flat part of the curves shows the full support for students coming from low-income families. The graph shows further how student aid fades out as parents' income increases until student aid becomes zero. Figure 4.3 visualises also how the reform of 2001 increased student aid and how this changed the relation between the amount of student aid and parents' income. The solid

Figure 4.3: Student aid before and after the 2001 reform by parents' income



Upper panel draws student aid before (solid line) and after (dashed line) the reform.
Lower panel draws the cash gain as the vertical difference.
This figure is drawn for parents, who have one further alimony entitled child. The curves are shifted to the right if parents have more alimony entitled children.
Source: Own calculation.

curve shows this relation before the reform while the dashed curve depicts the relation between the amount of student aid and parents' income after the reform.

The vertical difference between these two curves is the cash gain due to the reform. Figure 4.3 draws this gain which varies with parents' income. The cash gain with respect to parents' income can be divided into five categories:

Category I: Students who received full student aid before the reform also receive the full support after the reform;

Category II: Students who received reduced student aid before the reform and receive full student aid after the reform. The gain for these students increases with parents' income;

Category III: Students who received reduced student aid before the reform and also after the reform;

Category IV: Students who were not eligible for student aid before the reform and became eligible for reduced support due to the reform. Their capital gain decreases with parents' income;

Category V: Students who are not eligible for student aid. Neither before nor after the reform.

BAfoeG eligibility equals unity ($BE = 1$) in Equation (4.2) if students belongs to category I-III, BAfoeG eligibility varies with D if they belong to category IV, and BAfoeG eligibility is zero ($BE = 0$) if they belong to category V. I am thus able to differentiate between four different treatments which are averaged together into one single treatment in Section 4.5.1. Equation (4.4) expands therefore to one difference-in-difference estimators for each category.[5]

$$\begin{aligned} S_{it} &= \alpha_0 + \alpha_1 EB^I_{it} + \alpha_2 EB^{II}_{it} + \alpha_3 EB^{III}_{it} + \alpha_4 EB^{IV}_{it} + \alpha_2 D_{it} \\ &\quad + \alpha_{ATT_1}(EB^I \times D)_{it} + \alpha_{ATT_2}(EB^{II} \times D)_{it} + \alpha_{ATT_3}(EB^{III} \times D)_{it} \\ &\quad + \alpha_{ATT_4}(EB^{IV} \times D)_{it} + \upsilon_{it}, \end{aligned} \tag{4.9}$$

where $EB^j \ \forall \ j \in \{I, II, III, IV\}$ are dichotomous variables that take on the value of unity if the student is in categories I, II, III or IV, respectively, and zero otherwise. The coefficients α_{ATT_j} on the interaction between these dichotomous variables and the post-reform indicator D identify the average treatment effects for students in the respective categories, if estimated by a pooled linear regression. All other variables and coefficients are defined as in Equation (4.4).

[5]There is no difference-in-difference estimator for the fifth category, since this is the reference category.

4.5.3 Difference-in-difference using a discrete time transition rate model

Econometric analyses of the decision to enrol in higher education using micro data are typically based on discrete-choice models ranging from simple static binary logit models to dynamic structural models based on explicit intertemporal optimisation (see for instance Manski and Wise, 1983; Cameron and Heckman, 1998; Keane and Wolpin, 2001). The basic idea underlying these models is that educational decisions are based on the comparison of utility levels associated with alternative choices, with the chosen education level determined by the highest obtainable utility level. In most empirical applications estimation is based on relatively simple static models, which relate the rate of enrolment in higher education to various financial variables, such as parental income, tuition fees, loans, grants, and a set of control variables.

Instead of explaining the probability of enrolment in higher education at a particular point in time I model transition rates between education levels. As stressed by Cameron and Heckman (1998), one important advantage of this alternative model specification is the possibility to account for dynamic selection bias in education decisions over time. Given that I only look at enrolments into higher education over a relatively short time period of 3 years, this potential modelling advantage may seem somewhat limited at first sight. However, taking into account the relatively large amount of right-censoring in my data due to sample design and attrition, it turns out that accounting for this potential selection bias by modelling transitions between states is important.

The school to university transition can easily be restated in terms of a discrete-time hazard rate model (Steiner, 2001). This allows me to account for sample attrition in a straightforward way, and also provides a more appealing interpretation of enrolment decisions in terms of transition rates in higher education. The observed variable statistically related to these transition rates

is the duration between graduation from upper secondary school and enrolment in higher education. This duration is described by a non-negative random variable, T, which takes on integer values only. If an observation ends in the interval $(I_{t-1}, I_t]$, which is one calendar year in my empirical analysis, this variable takes on the value of $T = t$. The hazard rate for individual i, $\lambda_i(t)$, is the conditional rate of a transition into higher education in year t. Given that no transition occurs until the beginning of t, the hazard rate is:

$$\lambda_i(t|X_{it}, \varepsilon_m) = P(T_i = t|T_i \geq t, X_{it}, \varepsilon_m), \tag{4.10}$$

with i = 1,2,... n;

$\mathbf{X}_{it}$ = vector of covariates of individual i in interval t;

ε_m = time-invariant individual effect.

Following Heckman and Singer (1984), the time-invariant individual effects are assumed to have the following properties:

$$E(\varepsilon) = \sum_{m=1}^{M} P(\varepsilon_m)\varepsilon_m = 0 \tag{4.11}$$

$$\sum_{m=1}^{M} Pr(\varepsilon_m) = 1 \tag{4.12}$$

$$E(\varepsilon_m, \mathbf{X}_{it}) = 0 \tag{4.13}$$

I estimate $M - 1$ individual effects and choose the M^{th} individual effect such that the properties, as in Equations (4.11) and (4.12), are satisfied. Appendix 4.A.3 derives the M^{th} individual effect.

I assume a logit specification for the functional form of the hazard rate, i.e.:

$$\lambda(t|\mathbf{T}_{it}, \mathbf{DD}_{it}, \mathbf{Z}_{it}, \varepsilon_m) = \Lambda(\gamma\mathbf{T}_{it} + \alpha\mathbf{DD}_{it} + \delta\mathbf{Z}_{it} + \varepsilon_m), \tag{4.14}$$

where $\Lambda(\cdot)$ is the logistic cumulated distribution function ($\frac{\exp(\cdot)}{1+\exp(\cdot)}$), the column vector $\mathbf{T}_{it}$ includes the baseline dummies to account for duration effects on the hazard rate,[6] the column vector $\mathbf{DD}_{it}$ includes the group dummies and their

[6]The baseline hazard gives the shape of the hazard for any given individual. The hazard rate is duration-dependent if the baseline hazard changes over time.

interaction as in Equation (4.4) or (4.9) to identify the average treatment effect on the treated, and the column vector $\mathbf{Z}_{it}$ includes further covariates to control for the socio-economic background. The row vectors γ, α, and δ include the parameters to be estimated. The random effect is assumed to have the properties stated in the equations (4.11) - (4.13). Note that the explanatory variables in equation (4.14) are allowed to vary over time, which provides an additional source of information for estimation of the model.

Assuming that, conditional on $\mathbf{X}_{it}$ and ε_m, all observations are independent, the sample likelihood function is given by[7]

$$L(param.|\mathbf{X}_{it}, \varepsilon_m) = \prod_{i=1}^{N} \left\{ \sum_{m=1}^{M} \left[Pr(\varepsilon_m)\right] \left[\lambda_i(t|\mathbf{X}_{it}, \varepsilon_m)\right]^{\delta_i} \prod_{\tau=1}^{t-1} \left[1-\lambda_i(t|\mathbf{X}_{it}, \varepsilon_m)\right] \right\}, \tag{4.15}$$

$$\text{with } \delta_i = \begin{cases} 1 \text{ if individual } i \text{ enrols,} \\ 0 \text{ otherwise.} \end{cases}$$

Plugging the hazard rate (4.14) into the likelihood function (4.15), ML estimates of the parameters, the mass points and their probabilities, taking into account the above-mentioned restrictions on the individual effects, can be obtained by standard numerical optimisation procedures.[8]

Calculating the average treatment effect on the treated using a discrete time transition rate model is straight forward and follows the same intuition as calculating the marginal effect of a binary variable. If x_1 is a binary variable, its marginal effect in a discrete time transition rate model – as specified in Equation (4.14) – is the difference between the predicted hazard rate, given $x_1 = 1$, and the predicted hazard rate, given $x_1 = 0$.[9] The average treatment effect on the treated is therefore analogous to the simple difference-in-difference

[7] The survivor function as well as the probability of a transition in higher education in year t are equivalent to the Equations (2.5) and (2.6) in Chapter 2.

[8] The programme gllamm version 2.3.10 as implemented in Stata version 8.2 is used for the estimations. A description of the programme as well as technical details are provided by Rabe-Hesketh and Skrondal (2004) and Rabe-Hesketh, Skrondal, and Pickles (2001).

[9] For further reference see for instance Wooldridge (2002).

estimator of Equation (4.3), i.e. it is the difference-in-difference of estimated hazard rates.

$$\hat{\alpha}_{ATT}(t|\cdot) = [\hat{\lambda}(t|EB=1, D=1, \mathbf{Z}=\bar{z}) - \hat{\lambda}(t|EB=1, D=0, \mathbf{Z}=\bar{z})] \quad (4.16)$$
$$-[\hat{\lambda}(t|EB=0, D=1, \mathbf{Z}=\bar{z}) - \hat{\lambda}(t|EB=0, D=0, \mathbf{Z}=\bar{z})],$$

where the individual effects ($\hat{\varepsilon}_m$) are also averaged using the estimated probabilities as weights and included in $\mathbf{Z}$. The standard error for $\hat{\alpha}_{ATT}$ is obtained using the delta method (Cameron and Trivedi, 2005; Ai and Norton, 2003).

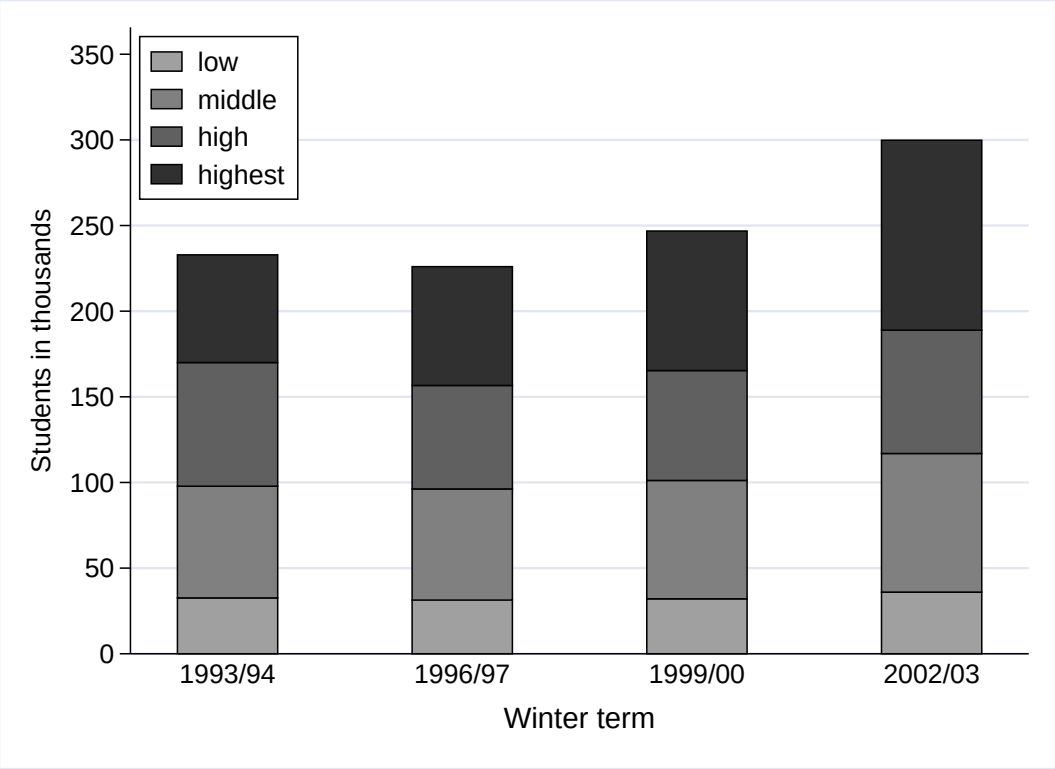
4.6 Evaluation of the student aid reform of 2001

In this section, I discuss the estimation results a discrete time hazard rate model based on the difference-in-difference methodology. I evaluate the 2001 student aid reform and discuss several robustness tests. Due to the institutional setting of this reform, I can also identify groups for which the reform may have had different effects – as discussed in Section 4.5.2.

Figure 4.4 shows the aggregated development of freshmen students by social origin, which is an index that combines parents' educational attainment and occupational status in selected years since the early 1990s.[10] The rise in enrolment in recent years is mainly driven by those coming from higher and highest social origins, while the number of freshmen students of low social origin has remained stable over time. In other words, the share of freshmen students of low social origin has declined. Since parents' educational attainment is a good proxy for parents' income, which is the major determinant of students' eligibility for financial aid, Figure 4.4 does not indicate that the 2001 BAfoeG reform has achieved the intended aim, which was to increase enrolment of students from low-income families.

[10] Table 4.a in the Appendix describes the index of social origin in more detail.

Figure 4.4: Development of the social composition of freshmen students by groups of origin



Source: Economic and Social Conditions of Student Life in the Federal Republic of Germany 2003 - 17th Social Survey; Federal Statistical Office Germany, Fachserie 11 Reihe 4.1, various years; own calculations.

Since Figure 4.4 is based on aggregated data, it provides only a rough indication of the potential effect the 2001 student aid reform may have had. Using micro data and applying micro-econometric evaluation techniques provides further insights into the mechanism of a reform and may suggest a different conclusion.

4.6.1 Sample development and descriptive statistics

I observe 775 pupils who left school between 1996 and 2003 with the necessary entrance qualification for tertiary education. I have to drop 243 of them, since I cannot simulate BafoeG eligibility if one of their parents does not participate in the SOEP. Of the remaining 532 school leavers, 259 entered tertiary education within three years, 137 decided not to do so within three years, 43 are right-censored due to sample attrition, and 93 are right-censored due to the end

of the SOEP survey period in 2004. For the default estimation, I exclude the two 'ambiguous' school-leaving cohorts 1998 and 1999 (142 school leavers) that make their transition around 2001, when student aid was reformed. The construction of the sample and the exact coding of the schooling and enrolment variables is described in more detail in Appendix 4.A.1.

Descriptive statistics for the 396 uncensored observations are shown in Table 4.1: 65.4 percent enrolled within three years of completing upper secondary schooling. The simulation model shows that 37.6 percent are eligible for student aid. 10.9 percent receive the full student aid before and after the reform (category I) and 19.9 percent are simulated to receive a reduced rate before and after the reform (category III). The simulation indicates further that 8.3 percent become eligible due to the 2001 student aid reform (category IV) and 4.0 percent change from the reduced rate to the full student aid (category II). Some of these categories appear to be rarely observed in the post-reform periods. Table 4.1 describes uncensored observations but most right-censored spells are in the post-reform period. Nevertheless, category II and category IV are the fewest observed categories.

Table 4.1 also presents descriptive statistics for the treatment and the control group, respectively. The two groups differ markedly in their parents' educational backgrounds and father's occupational status. 5.4 percent of eligible students have a self-employed father, while as many as 19.8 percent of ineligible students have a self-employed father. The share of students whose father is a civil servant or a white-collar worker is also higher amongst ineligible students. In contrast to this, the share of students with a father out of the labour force (including unemployment) is higher amongst eligible students. I also find more students with parents who completed upper secondary schooling among the ineligible students. A similar pattern with regard to education can also be found in average enrolment rates. The share of students enrolled in

Table 4.1: Descriptive statistics: Student aid reform of 2001

Variable	Full sample	Eligible for BAfoeG (treatment group)	Ineligible for BAfoeG (control group)
higher education	0.654	0.564	0.709
after	0.374	0.362	0.381
eligible to BAfoeG	0.376	1.000	0.000
category I	0.109	0.289	0.000
category II	0.040	0.107	0.000
category III	0.199	0.530	0.000
category IV	0.083	0.074	0.089
after × eligible to BAfoeG	0.136	0.362	0.000
after × category I	0.035	0.094	0.000
after × category II	0.005	0.013	0.000
after × category III	0.068	0.181	0.000
after × category IV	0.028	0.074	0.000
father self-employed	0.144	0.054	0.198
father white collar	0.354	0.289	0.393
father civil servant	0.159	0.094	0.198
father out of labour force	0.071	0.114	0.045
male	0.477	0.477	0.478
abitur	0.846	0.792	0.879
school leaving age	19.429	19.423	19.433
	(1.071)	(1.274)	(0.930)
father completed upper secondary schooling	0.359	0.174	0.470
mother completed upper secondary schooling	0.260	0.154	0.324
German nationality	0.904	0.846	0.939
East Germany	0.311	0.389	0.263
N	396	149	247

Standard deviation, where applicable, is in parenthesis.
Dummy base for father's occupational status is blue collar.
Source: SOEP 2004.

higher education is 14.5 percentage points higher in the control group.

4.6.2 Estimation results

Estimation results for the discrete-time hazard rate model outlined in Section 4.5.3 are presented in Table 4.2. The first column of Table 4.2 shows results for the default model estimated on the sample described in Section 4.6.1. Columns two, three, and four refer to alternative estimations, which are reported in order to analyse potential sensitivity to changes in variable and sample definitions.

Main results

Before I turn to the estimated effect the 2001 BAfoeG reform may have on the enrolment rate into higher education, I summarise the estimation results in general. Two mass points account for unobserved heterogeneity in transition. I conclude that unobserved heterogeneity is present in my data, i.e. the first

mass point and its probability are both estimated to be statistically significant and the log-likelihood improves significantly versus a transition rate model not controlling for unobserved heterogeneity evaluated by the Akaike information criterion (AIC).[11]

The baseline dummies account for potential duration-dependence of the transition to higher education. The baseline transition rate rises in the second year after schooling to 0.36 on average and falls back again to the level of the first year.[12] This is indicated by the positive and significant coefficient on the dummy of the second year and the insignificant coefficient on the dummy of the third year. Both are estimated relative to the first year as the reference category.

I also control for other covariates that may have an effect on the transition between upper secondary schooling and higher education. Fathers' occupation, parents' education as well as having a German nationality do not seem to be influential on the enrolment decision, since all these variables are estimated to be insignificant. These results seem to be surprising at the first glance, since attainment is known to be positively correlated with parent's education (Schneider, 2004) and negatively with having a foreign nationality (Riphahn, 2003). However, I would like to stress that I am analysing the last stage of an educational career. That is, parents' education may have an effect on attainment in earlier stages but it does not influence the transition from upper secondary schooling into tertiary education. A similar argument applies to foreigners. Having a non-German nationality might decrease attainment. But those foreigners who complete upper secondary education, i.e. those who are

[11] $AIC = -2\ln L + 2q$, where $\ln L$ is the estimated log-likelihood and q the number of estimated parameters. The model with the smaller AIC is preferred (Cameron and Trivedi, 2005, p. 278). The AIC of the estimation reported in column (1) of Table 4.2 controlling for unobserved heterogeneity is 789.63 and is hence smaller than the AIC not controlling for unobserved heterogeneity, which is 818.00.

[12] The baseline transition rate of 0.36 is a weighted average over both probability groups calculated for a person who is 19 years old and otherwise in the respective base categories.

Table 4.2: Transition probability into tertiary education: student aid reform of 2001[a]

	(1)	(2)	(3)	(4)
	Transition rates into tertiary education	Robustness tests: Alternative post-reform definition	Robustness tests: Including 'ambiguous' cohorts	Robustness tests: Longer post-reform period
constant	−26.709***	−27.809***	−24.026***	−26.191***
	(4.285)	(4.499)	(3.907)	(3.218)
baseline hazard:				
year 2	1.430***	1.591***	1.156***	1.108***
	(0.375)	(0.552)	(0.317)	(0.274)
year 3	0.472	0.718	0.402	0.105
	(0.671)	(0.912)	(0.573)	(0.459)
difference-in-difference:				
after	−0.121	0.353	0.066	0.076
	(0.388)	(0.522)	(0.328)	(0.328)
eligible for BAfoeG	−0.556	−0.504	−0.081	−0.229
	(0.541)	(0.510)	(0.390)	(0.325)
after × eligible for BAfoeG	0.096	−0.193	−0.479	−0.204
	(0.642)	(0.720)	(0.507)	(0.462)
covariates:				
father self-employed	0.371	0.413	0.199	0.677
	(0.502)	(0.525)	(0.437)	(0.441)
father white collar	0.590	0.620	0.350	0.499
	(0.488)	(0.530)	(0.406)	(0.346)
father civil servant	0.955*	1.016	0.969**	1.049**
	(0.571)	(0.623)	(0.480)	(0.456)
father out of labour force	0.339	0.191	−0.327	0.657
	(0.686)	(0.732)	(0.637)	(0.551)
male	−2.091***	−2.370***	−2.000***	−2.038***
	(0.472)	(0.758)	(0.418)	(0.369)
abitur	1.973***	1.572	1.862***	2.030***
	(0.633)	(0.966)	(0.495)	(0.434)
school leaving age	1.211***	1.272***	1.106***	1.151***
	(0.205)	(0.222)	(0.191)	(0.157)
father completed up. sec. schooling	−0.522	−0.468	−0.520	−0.136
	(0.406)	(0.429)	(0.341)	(0.321)
mother completed up. sec. schooling	0.689*	0.716*	0.933***	0.414
	(0.367)	(0.391)	(0.328)	(0.303)
German nationality	−1.116*	−1.149*	−1.668**	−0.731*
	(0.656)	(0.692)	(0.753)	(0.428)
east	0.862**	0.937**	0.912***	1.026***
	(0.389)	(0.427)	(0.341)	(0.308)
mass points:				
ε_1	−4.179***	−4.141***	−3.905***	−3.837***
	(0.551)	(0.568)	(0.481)	(0.405)
$P(\varepsilon_1)^b$	0.388	0.410	0.406	0.409
${\pi_1}^b$	−0.456***	−0.364*	−0.381***	−0.367***
	(0.157)	(0.195)	(0.135)	(0.118)
${\varepsilon_2}^b$	2.649	2.878	2.668	2.658
$P(\varepsilon_2)^b$	0.612	0.590	0.594	0.591
N	456	456	598	691
NT	798	798	1083	1309
log-likelihood[c]	-413.814	-413.623	-563.940	-662.461
log-likelihood[d]	-425.999	-424.195	-575.224	-681.755

[a] Robust standard errors are reported in parenthesis. The level of significance is: *** 1%; ** 5%; * 10%.
[b] $P(\varepsilon_m)$ and ε_2 are calculated as described in appendix 4.A.3. π_1 is the log-odd of $P(\varepsilon_1)$.
[c] Log-likelihood controlling for mass points.
[d] Log-likelihood not controlling for mass points.
Source: Baumgartner and Steiner (2006).

structurally positively selected, have a relatively high probability of pursuing higher education (Gang and Zimmermann, 2000). The regional context is controlled for by a dichotomous variable for east Germany that is significant and positive. That is, enrolment is estimated to be higher in east German universities.

The effect the 2001 BAfoeG reform may have on enrolment rates into higher education is estimated by the difference-in-difference methodology. The estimated discrete time transition rate models, however, is non-linear and therefore the effect has to be calculated taking into account all estimated parameters and values of regressors. The 2001 BAfoeG reform increased enrolment of eligible students by 0.8 percentage points. This point estimate is too low to be economically meaningful. Moreover, the standard error calculated by the delta method suggests this estimate to be insignificant.[13]

Robustness tests

The insignificance of the estimated treatment effect may derive from the estimation procedure and/or the relatively small number of observations. To validate the robustness of the base estimation, I discuss several robustness tests.

Alternative post-reform definition: The post-reform indicator ($after$) is defined according to calendar time, since the 2001 BAfoeG reform applies to every eligible student from 2001 onwards. However, it might be that pupils make their enrolment decision when graduating from upper secondary schooling and this decision might be fixed regardless of an eventual reform. Column two of Table 4.2 shows estimation results for the same sample as in column one but redefines the post-reform indicator $after$ according to the graduation

[13]The discussed estimate is the average over time. The effect is insignificant in each year and the point estimates are 0.9 percentage points in the first year, 0.7 percentage points in the second year, and 0.8 percentage points in the third year.

year. The re-definition of the indicator variable $after$ has no effect on the overall conclusion, since the effect is estimated to be insignificant. However, the sign changes and the average point estimated of $\hat{\alpha}_{ATT}$ is -1.4 percentage points.[14]

'Ambiguous' cohorts: There are two school-leaving cohorts, those who completed schooling just before the 2001 student aid reform and whose observed transition window closes in the post-reform period. These are the school-leaving cohorts of 1998 and 1999, which I label 'ambiguous' school-leaving cohorts. Due to their ambiguity I excluded them in my base estimation. Including these cohorts, on the other hand, does not change the overall conclusion, which is the ineffectiveness of the 2001 BAfoeG reform. However, the average point estimate of $\hat{\alpha}_{ATT}$ is -4.1 percentage points and thus unreasonable large and negative though insignificant.[15] This result indicates that the smaller sample of the base estimation is preferable to also including 'ambiguous' cohorts.

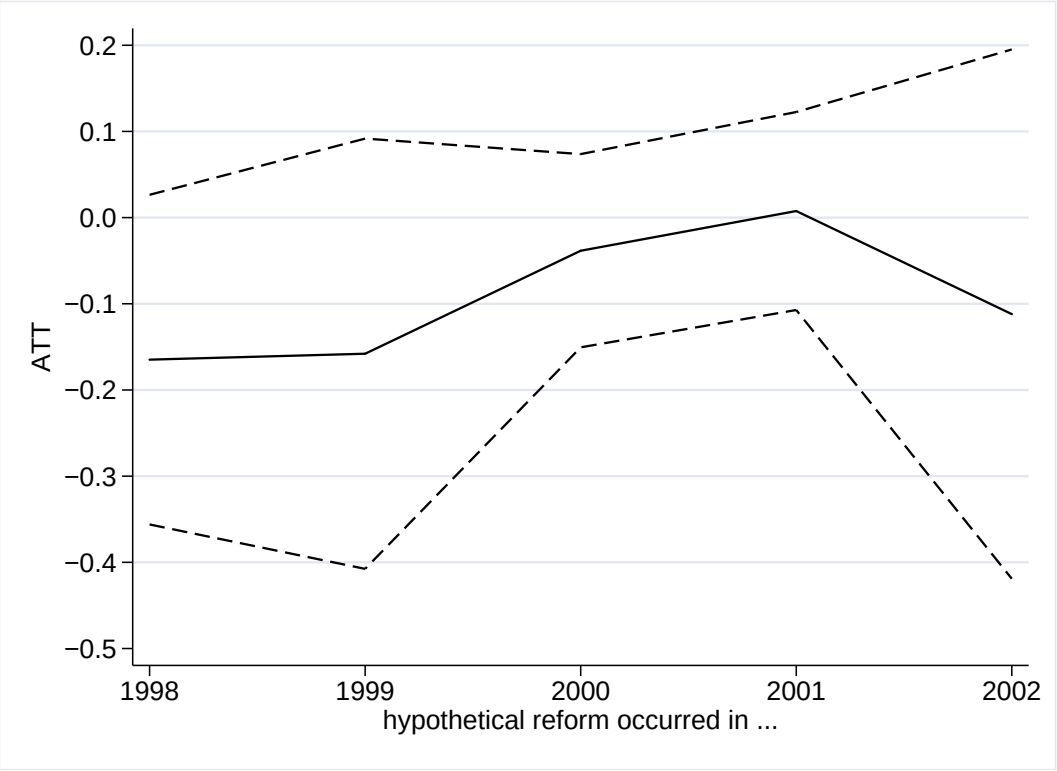
Longer pre-reform period: I have also tested whether the insignificance of the treatment effect on the probability of entering tertiary education is in fact related to the relatively small number of observations in my sample by extending the pre-reform period using all school-leaving cohorts since 1992. It turns out, however, that the average point estimate of $\hat{\alpha}_{ATT}$ actually becomes negative (-1.7 percentage points), although it remains insignificantly different from zero.[16] This result could be explained by the fact that the decline in student aid in real terms was set back in 1996, as discussed in Section 4.3. I

[14]The effect is -1.6 in the first year, -1.2 in the second year, and -1.4 in the third year and insignificant in all years.

[15]The effect is -4.5 in the first year, -3.9 in the second year, and -4.3 in the third year and insignificant in all years.

[16]The effect is -1.8 in the first year, -1.6 in the second year, and -1.8 in the third year and insignificant in all years.

Figure 4.5: Pre/post-programme test for the 2001 reform



The solid line shows the average treatment effect on the treated estimated by equation (4.16). The dashed lines show the confidence interval estimated by the delta method.

thus conclude that restricting the sample to school-leaving cohorts that left school after and including 1996 is justified and consider the smaller sample, as used in the first column, as more reliable despite the smaller number of observations.

pre/post-programme test: One of the key identifying assumptions of my estimation is that the enrolment of eligible students and ineligible students would not have developed differently in the absence of the 2001 BAfoeG reform. This, however, is a non-testable assumption. Nevertheless, a weaker assumption can be tested, as Angrist and Krueger (1999) propose. That is, comparing the two groups around an arbitrary year in which no reform occurred, should result in an insignificant effect. Figure 4.5 draws the estimated average treatment effect on the treated (ATT) and its corresponding confi-

dence interval for arbitrary years. The specification and sample is as in the first column of Table 4.2. The only change made is a re-definition of the post-reform indicator $after$, which takes on the value of unity in the years shown in the figure and thereafter. The effect drawn for 2001 is thus the same as discussed above. It becomes apparent from Figure 4.5 that the estimated effects are not statistically significant, since their confidence intervals include zero throughout the observed years.

Common time trend: Another key identifying assumption is that both groups – eligible and ineligible students – follow a common time trend over the entire period observed. Since the discrete policy change affects one group's trend, namely the trend of eligible students, this key identifying assumption can also not be tested directly. Nevertheless, it is possible to test a weaker assumption, which is that both groups follow a common time trend after the student aid reform. Table 4.b in the appendix shows transition rate models for the school-leaving cohorts of 1992 till 1997. Since I do not know the parametric form of an eventual time trend, I model a linear, a quadratic, and a non-parametric time trend.[17] These time trends are allowed to differ between both groups of students. The bottom of Table 4.b shows the χ^2 statistic for the null hypothesis that both groups follow the same time trend. I cannot reject the null hypothesis and conclude therefore that both groups follow a common time trend in the pre-reform period. Of course, this result does not prove that the key identifying assumption holds. However, the fact that enrolment rates for the two groups developed in a similar way in the pre-reform period lends some credibility to the key identifying assumption.

[17] A non-parametric time trend is simply a dummy-variable for each year.

Table 4.3: Heterogeneous treatment: student aid reform 2001[a,b]

	(1)	(2)	(3)	(4)
after	−0.187	−0.315	−0.226	−0.340
	(0.362)	(0.373)	(0.375)	(0.373)
category I	−0.140	−0.195	0.104	–
	(0.871)	(0.925)	(0.977)	
category II	3.187∗	–	–	–
	(1.656)			
category III	−0.703	−0.895	–	−1.057
	(0.670)	(0.722)		(0.727)
category IV	−0.021	–	–	–
	(0.795)			
after × category I	0.061	0.372	−0.343	–
	(1.062)	(1.143)	(1.170)	
after × category II	−1.893	–	–	–
	(1.916)			
after × category III	−0.104	0.081	–	0.197
	(0.843)	(0.894)		(0.893)
after × category IV	−1.096	–	–	–
	(1.100)			
N	456	437	370	396
NT	798	705	575	631
log-likelihood[c]	-410.122	-360.263	-313.785	-327.613
log-likelihood[d]	-420.081	-371.897	-318.431	-336.403

[a] Robust standard errors are reported in parenthesis. The level of significance is: *** 1%; ** 5%; * 10%.
[b] The full estimation is documented in Table 4.c in the Appendix.
[c] Log-likelihood controlling for mass points.
[d] Log-likelihood not controlling for mass points.
Source: SOEP 2004.

4.6.3 Effects of heterogeneous treatments

The estimation results, discussed so far, interpreted the 2001 student aid reform as a binary treatment and estimated the average treatment effect on the treated. However, as I was arguing in Section 4.5.2, the 2001 student aid reform resulted in different cash gains which introduces a heterogenous treatment. Table 4.3 displays the coefficients necessary to estimate the difference-in-difference for each category.[18] Category V is the reference category. That is, the effects are estimated relative to students who are not eligible for student aid, neither before not after the reform.

Table 4.3 summarises the estimation results using the same sample as the base model in Section 4.6.2 and Table 4.4 summarises the implied point estimates using Equation (4.16). The point estimate averaged over three years is 0.5 percentage points for category I, -12.1 percentage points for category II,

[18]The full estimation is documented in Table 4.c in the appendix.

Table 4.4: Average treatment effects: student aid reform of 2001

	(1)	(2)	(3)	(4)
category I	0.0049	0.0283	−0.0298	
	(0.0871)	(0.0876)	(0.1021)	
category II	−0.1208			
	(0.1437)			
category III	−0.0099	0.0047		0.0143
	(0.0727)	(0.0731)		(0.0729)
category IV	−0.0936			
	(0.1014)			

The average treatment effects are calculated by equation (4.16) using the estimates of Table 4.c. The documented effects are the averages over time.
Standard errors estimated by the delta method are in parenthesis.

-0.9 percentage points for category III, and -9.4 percentage points for category IV. However, none of these estimates is statistically significant.

Since I do not observe many students in category II and IV, which may drive the negative signs, I drop these observations and estimate the effect for category I and III only. The point estimate for those students with the largest cash gain (category III) becomes positive in this estimation (0.5 percentage points) but remains insignificant. The average treatment effect for category I increases in size (2.8 percentage points) and remains insignificant, too. Due to this finding, I estimate the effects for category I and category IV separately, which reduces the sample size. The sign for the effect for category I turns out to be negative, indicating a lower enrolment rate after the reform than the reference category V, although this effect is insignificant. The effect for category III remains positive and increases in size (1.4 percentage points) but remains also insignificant.

The statistical insignificance of the estimated average treatment effects may derive from the relatively small number of observations in my sample. Estimating all effects in one regression suggests an negative effect for category III. However, focusing on the average treatment effect for category III (column two and four) indicates a positive treatment effects. Students in category III enjoyed the largest cash gain due to the 2001 reform. Their enrolment rate

should therefore respond the most. It remains ambiguous, however, if the insignificant treatment effect comes from the relatively small sample size or whether it indicates the ineffectiveness of the student aid reform to increase enrolment rates even for those students who gained the most.

4.7 Evaluation of the student aid reform of 1990

Having evaluated the 2001 BAfoeG reform, I discuss estimation results of the evaluation of the 1990 student aid reform in this section. By doing so, I apply the same methods and robustness tests as before in Section 4.6.

4.7.1 Sample development and descriptive statistics

I observe 457 pupils, who left schooling between 1985 and 1993 with the necessary entrance qualification for tertiary education. I have to drop 67 of them, since I cannot simulate BafoeG eligibility if one of their parents does not participate in the SOEP. Of the remaining 390 school leavers, 223 entered tertiary education within three years, 134 decided not to do so within three years, and 33 are right-censored due to sample attrition. For my default estimation I exclude – as in the evaluation of the 2001 reform – the school-leaving cohorts that make their transition around 1990, when student aid was reformed. That is, 107 school-leavers who left upper secondary schooling in 1987 or 1988.

Descriptive statistics for the 357 uncensored observations are shown in Table 4.5: 62.5 percent enrolled within three years of completing upper secondary schooling. My simulation model (c.f. Section 4.4.2) shows that 23.5 percent of my sample are eligible for student aid. Table 4.5 also presents descriptive statistics for the treatment and the control group, respectively. The two groups differ markedly in their parents' educational background and father's occupational status – as has already been shown in Section 4.6.1. 23.8 percent of

Table 4.5: Descriptive statistics: 1990 BAfoeG reform

Variable	Full sample	Eligible for BAfoeG (treatment group)	Ineligible for BAfoeG (control group)
higher education	0.625	0.643	0.619
after	0.417	0.393	0.425
eligible for BAfoeG	0.235	1.000	0.000
after x eligible for BAfoeG	0.092	0.393	0.000
father self employed	0.104	0.119	0.099
father white collar	0.361	0.238	0.399
father civil servant	0.182	0.071	0.216
father out of labour force	0.045	0.119	0.022
male	0.585	0.524	0.604
abitur	0.776	0.762	0.780
school leaving age	19.675	19.417	19.755
	(1.171)	(1.078)	(1.189)
German nationality	0.787	0.595	0.846
father completed upper secondary schooling	0.252	0.060	0.311
mother completed upper secondary schooling	0.070	0.060	0.073
N	357	84	273

Standard deviation, where applicable, are in parenthesis.
Dummy base for father's occupational status is blue collar.
Source: SOEP 2004, sample A and B.

eligible students have a father employed as a white-collar worker, while as many as 39.9 percent of ineligible students have a father with a white-collar occupation. The share of students whose father is a civil servant or self-employed is also higher amongst ineligible students. In contrast to this, the share of students with a father out of the labour force (including unemployment) is higher amongst eligible students. I also find more students with parents who completed upper secondary schooling among the ineligible students.

4.7.2 Estimation results

Estimation results for the discrete-time hazard rate model outlined in Section 4.5.3 are presented in Table 4.6. The first column of Table 4.6 shows results for the model estimated on the sample described in Section 4.7.1, while columns two, three, and four refer to alternative estimations that are reported in order to analyse potential sensitivity to changes in variable and sample definitions. The empirical strategy, (i) to evaluate the reform and (ii) to test for potential sensitivity, is hence the same as for the evaluation of the 2001 student aid reform.

Main results

The default estimation as presented in column one in Table 4.6 accounts for unobserved heterogeneity in the transition rates by two mass points. The first mass point and its corresponding probability are both estimated to be significantly different from zero and the log-likelihood – as displayed at the bottom of Table 4.6 – does improve and results in a larger AIC.[19] I conclude therefore that unobserved heterogeneity is present in my data.

The baseline dummies account for the duration-dependence of the transition to higher education. Both baseline dummies are to be interpreted relative to the reference category, which is the first year after graduating from upper secondary schooling. Both baseline dummies are estimated to be negative while only the baseline dummy for the third year is statistically significant. In other words, the enrolment rate declines over time, while the enrolment rate does not differ in the first two years.

I also control for further covariates that may have an effect on the transition between upper secondary schooling and tertiary education. There appears to be no difference in the transition rate of pupils whose father is a white-collar worker, civil servant or out of the labour force interpreted relative to the reference category, which is blue collar. Only pupils whose father is self-employed have a significantly higher enrolment rate than their peers in the reference category. This differs from my estimation for the 2001 reform, where only fathers as civil servants have a significant positive effect. Parents' educational background does not seem to influence enrolment. As discussed in Section 4.6.2, parents' education may have an effect on attainment in earlier stages but it does not influence the transition from upper secondary schooling to higher education. And pupils with a German nationality are estimated to have a lower

[19]The calculation of the AIC is explained in Footnote 11 on page 125. The AIC of the estimation reported in column (1) of Table 4.6 is 719.82 and is hence smaller than the AIC not controlling for unobserved heterogeneity, which is 728.51.

Table 4.6: Transition probability into tertiary education: BAfoeG reform 1990[a]

	(1)	(2)	(3)	(4)
	Transition rates into tertiary education	Robustness tests: Alternative post-reform definition	Robustness tests: Including 'ambiguous' cohorts	Robustness tests: Longer post-reform period
constant	−21.892***	−23.014***	−20.453	−21.051***
	(3.796)	(4.569)	(150.846)	(2.916)
baseline hazard:				
year 2	−0.228	−0.158	−0.035	0.020
	(0.307)	(0.314)	(0.237)	(0.253)
year 3	−0.858*	−0.849**	−0.823**	−0.642
	(0.439)	(0.432)	(0.369)	(0.402)
difference-in-difference:				
after	0.403	0.627	0.118	0.282
	(0.345)	(0.407)	(0.257)	(0.298)
eligible for BAfoeG	0.389	0.646	−0.071	0.501
	(0.545)	(0.492)	(0.380)	(0.535)
after × eligible for BAfoeG	−0.038	−0.602	0.395	0.066
	(0.709)	(0.747)	(0.523)	(0.666)
covariates:				
father self-employed	1.650**	1.779**	0.427	0.696
	(0.776)	(0.885)	(0.428)	(0.548)
father white collar	−0.148	−0.104	−0.437	−0.135
	(0.408)	(0.414)	(0.320)	(0.348)
father civil servant	0.497	0.591	0.060	0.749
	(0.596)	(0.616)	(0.400)	(0.460)
father out of labour force	0.388	0.439	0.426	0.233
	(0.803)	(0.777)	(0.675)	(0.749)
male	−0.521	−0.547	−0.099	−0.535*
	(0.351)	(0.350)	(0.256)	(0.302)
abitur	1.727***	1.723***	1.523***	1.532***
	(0.401)	(0.422)	(0.329)	(0.327)
school leaving age	0.957***	1.012***	0.788***	0.908***
	(0.184)	(0.214)	(0.124)	(0.139)
father completed up. sec. schooling	0.423	0.471	0.690**	0.322
	(0.479)	(0.529)	(0.316)	(0.332)
mother completed up. sec. schooling	0.862	1.213	0.016	0.558
	(1.230)	(1.675)	(0.449)	(0.446)
German nationality	−0.816*	−0.994*	−0.811**	−0.899**
	(0.496)	(0.541)	(0.375)	(0.382)
mass points:				
ε_1	−3.070***	−2.997***	−12.756	−3.769***
	(0.808)	(0.602)	(553.848)	(0.750)
$P(\varepsilon_1)^b$	0.313	0.326	0.215	0.322
${\pi_1}^b$	−0.788**	−0.727**	−1.294***	−0.744***
	(0.321)	(0.322)	(0.308)	(0.189)
${\varepsilon_2}^b$	1.396	1.449	3.497	1.791
$P(\varepsilon_2)^b$	0.687	0.674	0.785	0.678
N	315	315	406	465
NT	671	671	857	979
log-likelihood[c]	-341.912	-341.332	-450.017	-501.995
log-likelihood[d]	-348.254	-347.447	-455.853	-512.758

[a] Robust standard errors are reported in parenthesis. The level of significance is: *** 1%; ** 5%; * 10%.
[b] $P(\varepsilon_m)$ and ε_2 are calculated as described in appendix 4.A.3. π_1 is the log-odd of $P(\varepsilon_1)$.
[c] Log-likelihood controlling for mass points.
[d] Log-likelihood not controlling for mass points.
Source: SOEP 2004, sample A and B.

enrolment rate than their peers of a different nationality.

From the estimates in Table 4.6 I can derive the average treatment effect on the treated of the 1990 BAfoeG reform. Using Equation (4.16), I cannot confirm that the student aid reform had an effect on enrolment rates of eligible students. The point estimate, moreover, indicates a slightly lower enrolment rate (-0.5 percentage points) of eligible students after the reform than for ineligible students. But this average effect is not estimated to be significant.[20] That is, the change of student aid from a loan to a loan/grant regime did not change the enrolment of pupils from economically disadvantaged families.

Robustness tests

The insignificance of the estimated effect of the 1990 BAfoeG reform may derive from the estimation procedure and/or the relatively small number of observations. In the following discussion, I hence test whether the insignificance is somehow related to the sample definition, sample size or estimation procedure. By doing so, I apply the same sensitivity tests as above, where I tested the robustness of the evaluation of the 2001 reform.

Alternative post-reform definition: The post-reform indicator ($after$) is defined according to calendar time, since the 1990 BAfoeG reform applies to every eligible student from 1990 onwards. However, it might be that pupils make their enrolment decision when graduating from upper secondary schooling and that this decision is fixed regardless of an eventual reform, as discussed before when I evaluated the 2001 reform. Column two of Table 4.6 shows estimation results for the same sample as in column one but redefines the post-reform indicator ($after$) according to the graduation year. The re-definition of the indicator variable $after$ has no effect on the overall conclusion. The

[20] The effect is -0.5 percentage points in the first and the second year, and -0.3 percentage points in the third year and insignificant in all years.

average point estimate of $\hat{\alpha}_{ATT}$ is -7.4 and insignificant.[21] Although the point estimate increases in size, it remains insignificant. The result presented in column one is thus robust to an alternative post-reform definition.

'Ambiguous' cohorts: There are two school-leaving cohorts that completed schooling just before the discrete policy change and whose observed transition window closes in the post-reform period. This is, the school-leaving cohorts of 1987 and 1988. Due to their ambiguity I excluded them in my default estimation. Including these cohorts does not change the overall conclusion, which is the ineffectiveness of the BAfoeG reform. However, the insignificant average point estimate becomes positive (5.8 percentage points).

Longer post-reform period: Applying a difference-in-difference estimation strategy, one has to be careful to compare similar individuals. Therefore, I constructed a sample that allows me to observe school-leaving cohorts as close as possible around the policy change. The cost for doing so is a relatively small sample size of 315 individuals observed in 671 intervals. Increasing the sample by including the cohorts up to 1997, I am able to observe 465 individuals on 979 intervals. The estimation result for the increased sample is shown in the fourth column of Table 4.6. The overall conclusion also does not alter when a larger sample is used. That is the estimated effect of the reform remains statistically insignificant. I note, however, that the sign of the implied average treatment effect becomes positive (0.9 percentage points) although still insignificant if the sample is increased and a priori a positive effect has been expected.

Pre/post-reform test: In order to test the key identifying assumption that enrolment of eligible students and ineligible students would not have developed

[21] The effect is -7.3 percentage points in the first and the second year, and -7.0 percentage points in the third year and insignificant in all years.

Table 4.7: Pre/post-programme test for the 1990 reform

hypothetical reform occurred in ...	ATT	
1987	-0.122	(0.499)
1988	0.127	(0.959)
1989	-0.060	(0.092)
1990	-0.005	(0.089)
1991	0.057	(1.498)
1992	0.044	(0.450)

- The average treatment effects are calculated by equation (4.16) using the estimates of Table 4.6. The documented effects are the averages over time.
- The specification is as in the first column of Table 4.6 with an altered definition for the post-reform dummy.
- Standard error as calculated by the delta method are in parenthesis.

differently in the absence of the 1990 BAfoeG reform, I apply the a pre/post-reform test (Angrist and Krueger, 1999). Table 4.7 shows the estimated effect around various hypothetical reform years. The specification and the sample are as in the first column of Table 4.6. The only change made is a re-definition of the post-reform indicator $after$, which takes on the value of unity in the years of the hypothetical reform. The effect for the year 1990 is thus the $\hat{\alpha}_{ATT}$ of the 1990 student aid reform. It becomes apparent from Table 4.7 that the estimated effects are not statistically significant throughout the observed years. I note that the estimated effect becomes positive after the reform (1991 and 1992) but with inflated standard errors. The standard errors increase because there are fewer observations in the hypothetical post-reform period.[22] Adding more observations to the hypothetical post-reform period does not change the overall conclusion.

Common time trend: Eligible and ineligible students are assumed to follow the same time trend, which cannot be tested for the full period. Nevertheless, it is possible to test the weaker assumption, which is that both groups follow

[22]This was also depicted in Figure 4.5, where the confidence interval for the year 2002 became wider.

a common time trend after the student aid reform. Table 4.d in the appendix shows transition rate models for the school-leaving cohorts of 1991 till 1997. Since I do not know the parametric form of an eventual time trend, I model a linear and a quadratic trend.[23] These time trends are allowed to differ between both groups of students. The bottom of the table shows the χ^2 statistic for the null hypothesis that both groups follow the same time trend. I cannot reject the null hypothesis at the commonly used level of significance, i.e. five percent, and conclude thus that both groups follow a common time trend in the pre-reform period.[24] As mentioned above, when I tested for a common time trend as I evaluated the 2001 reform, the fact that both groups follow a common time trend in the post-reform period does not prove whether the key identifying assumption is violated or not. Nevertheless, since the weaker assumption holds it is likely that the non-testable key identifying assumption may hold as well.

4.8 Conclusion

I evaluated two BAfoeG reforms in this chapter. The common aspect of both reforms is that they aimed to increase enrolment rates in higher education of students from low-income households. I have used the supposedly exogenous variation in student aid induced by this 'natural experiment' to test whether the political aim has been achieved and to identify the causal effect more generous student aid might have on enrolment in higher education in Germany.

Both reforms substantially reduced the net costs of pursuing higher education. In 2001 the monthly student aid rate rose by as much as ten percent on average and in 1990 the repayment obligations were cut by 50 percent and a

[23] In Section 4.6.2, I tested also for a non-parametric time trend. Such a specification, however, does not converge on the sample used to evaluate the 1990 student aid reform.

[24] I note that the linear time trend has a p-value of 0.103, which is just above the 10 percent level. Relaxing the linearity assumption and assuming the time trends to be quadratic, however, results in a χ^2 statistic that is also insignificant at higher levels of significance.

grant/loan system was introduced. The introduction of both reforms had an effect on eligible students only. I explore this 'natural experiment' and apply a difference-in-difference estimator in a discrete time hazard rate model. The estimation results show that neither reform has achieved its aim and that enrolment from the targeted students did not increase. Moreover, the insignificant point estimate for the 1990 student aid reform suggest a declining enrolment rate for eligible students.

An explanation for the inefficient estimation of the treatment effect could be the relatively small sample. However, a larger informative data set for Germany, which would allow identification of potential BAfoeG eligibility for both students and school leavers, who choose not to study, is not available. Therefore, I cannot really judge to which extent the insignificance of the estimated treatment effect is due to the small sample size.

An alternative explanation to these somewhat surprising results could be that the basic identifying assumption might be violated. To identify the difference-in-difference estimator I have to assume that both groups – eligible and ineligible students – follow a common time trend. This assumption cannot be tested. However, since the weaker assumption of a common time trend either before or after the reform does not appear to be violated, I have no indication of a violation of the basic identifying assumption. Nevertheless, it might be that the decline in the private returns to education – documented for Germany by Boockmann and Steiner (2000) and Lauer and Steiner (2001) – has had different effects on the enrolment decisions of students from low-income households and those not eligible to receive BAfoeG. This may seem theoretically plausible because young people from low-income households may, due to credit constraints and/or a higher rate of time preference, require a higher private return to education to pursue tertiary education. I have, however, no conclusive evidence supporting this view.

Henceforth, I interpret the empirical results as being indicative for the ineffectiveness of more generous student aid in raising enrolment rates in higher education in Germany.

Nevertheless, it may be that student aid has other beneficial effects. The duration of study, for instance, might be positively influenced by student aid, since students are less likely to be forced to work alongside their studies and may thus be more likely to concentrate on their courses. But this was not the political aim of the analysed reform and was therefore not evaluated in this chapter.

4.A Appendix

4.A.1 Construction of Sample used for Estimation from the SOEP

My analysis is based on the German Socio-Economic Panel Study (SOEP), which is a longitudinal survey of individuals living in private households. Since SOEP targets households and does not directly focus on the transition between secondary and tertiary education, I have to derive this information from the following questions. The second wave of SOEP asks in question 53:

> *"Since the beginning of 1984 have you finished school, vocational training, or university?"*,

and proceeds with the question:

> *"What type of qualification did you get?"*

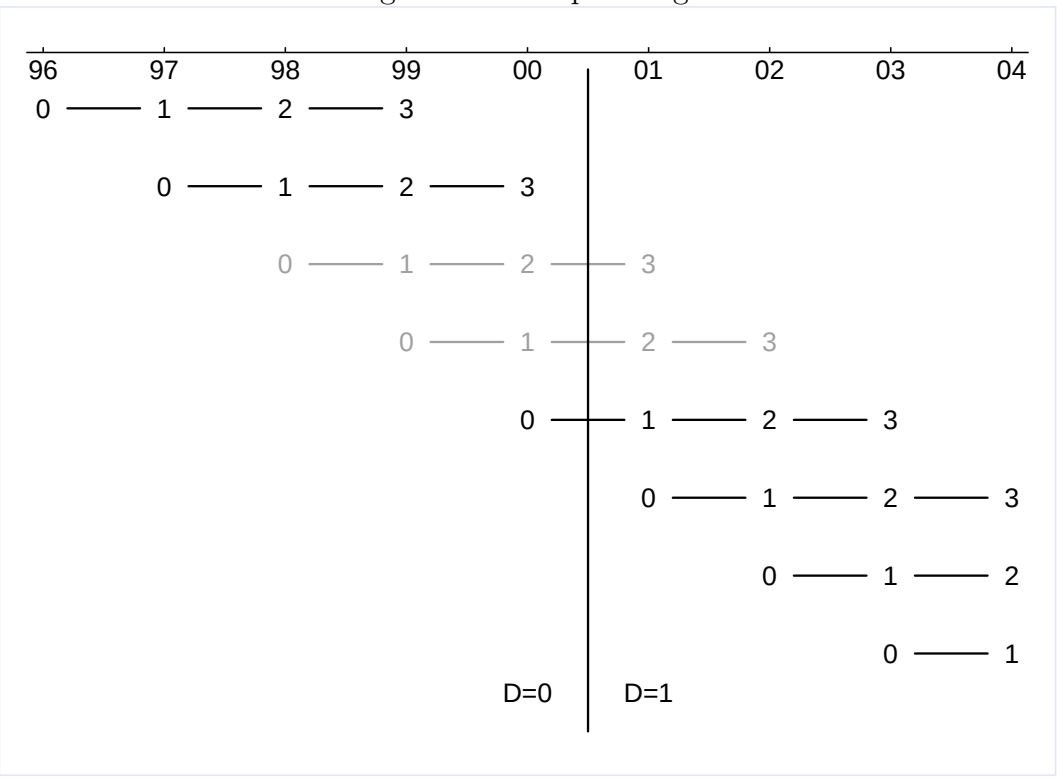
This information defines who has obtained a specialised upper secondary school degree (*Fachhochschulreife*) or an upper secondary school degree (*Abitur*) and is thus qualified to enrol in higher education. This question is also asked in the proceeding waves of the SOEP from which each person's school leaving year can be derived. Interviewees are also asked what they are currently doing in each year. For students, I can thus derive the enrolment year from the questions:

> *"Are you currently in training, attending school, undergoing vocational training or attending a further training course?"*,

and if yes:

> *"What sort of training is it?"*.

Figure 4.6: Sample design



'0' indicates the school leaving year; '1', '2', and '3' indicate, respectively, the first, second, and third year after schooling. Light numbers indicate 'ambiguous' school leaving cohorts, as defined in the text. If school leavers have not registered with a university within three years, they are counted as non-students.
Source: Baumgartner and Steiner (2006).

If it is either a technical college (*Fachhochschule*) or university, I know the year and institution of enrolment.

I allow the student to defer the enrolment decision for up to three years. If students do not attend higher education within three years, they are classified as non-students. The deference period is justified because males have to do their military service which takes between 10 and 18 months conditional on the recruitment year; some students decide to engage in vocational training before they enrol; and finally, enrolment is conditioned on waiting terms (*Wartesemester*) for some courses, due to the central course allocation agency (*Zentrale Studienplatzvergabe, ZVS*). Graph 4.6 depicts this idea in more detail and also visualises those cohorts that I label as being 'ambiguous' because their transition window overlaps with the discrete policy reform.

4.A.2 Derivation of the ATT in non-linear models

The interaction term of the group dummy (EB) and the time dummy (D) identifies the average treatment effect on the treated (α_{ATT}) in linear models. However, this is not the case in non-linear models as it was discussed in subsection 4.5.1. In this appendix I briefly describe the derivation of Equation (4.8).

Using the chain rule, the first difference of Equation (4.7) with respect to EB is

$$\frac{\Delta S}{\Delta EB} = (\alpha_1 + \alpha_3 D)\frac{\Delta \Lambda(z)}{\Delta z}. \tag{4.17}$$

Using the product rule taking the cross difference of Equation (4.17) with respect to D yields

$$\frac{\Delta^2 S}{\Delta EB \Delta D} = \alpha_3 \frac{\Delta \Lambda(z)}{\Delta z} + (\alpha_1 + \alpha_3 D)(\alpha_2 + \alpha_3 EB)\frac{\Delta^2 \Lambda(z)}{\Delta z^2}, \tag{4.18}$$

which is the α_{ATT} estimated by a nonlinear model.

4.A.3 Calculation of the Mth mass point

Unobserved heterogeneity can be modelled with a small number of mass points. That is each observation is assumed to belong to one heterogeneity group ε_m with probability $P(\varepsilon_m)$. To estimate the mass points, it is sufficient to estimate $M-1$ mass points, since the M^{th} mass point and its corresponding probability can be estimated from the properties (4.11) and (4.12), which are restated in this appendix for simplicity.

$$E(\varepsilon) = \sum_{m=1}^{M} P(\varepsilon_m)\varepsilon_m = 0 \tag{4.19}$$

$$\sum_{m=1}^{M} P(\varepsilon_m) = 1 \tag{4.20}$$

To receive the probabilities of the mass points, the estimated log-odds of the probabilities need to be transformed.

$$P(\varepsilon_m) = \frac{\exp(\hat{\pi}_m)}{1+\exp(\hat{\pi}_m)}, \tag{4.21}$$

where $\hat{\pi}_m$ is the estimated log odd of probability m.

Equation (4.20) states that the probabilities sum up to unity. The M^{th} probability is thus

$$P(\varepsilon_M) = 1 - \sum_{m=1}^{M-1} P(\varepsilon_m). \tag{4.22}$$

Knowing $P(\varepsilon_M)$ and rearranging (4.19) yields

$$\varepsilon_M = (-1)\left\{\frac{\sum_{m=1}^{M-1} P(\varepsilon_m)\varepsilon_m}{P(\varepsilon_M)}\right\}, \tag{4.23}$$

which is the locus of the M^{th} mass point. Given the properties (4.19) and (4.20) it is sufficient to estimate $M-1$ mass points, since the M^{th} mass point can be inferred from the estimated $M-1$ mass points.

4.A.4 Tables

Table 4.a: Combining parents' educational attainment and occupational status to an index of social origin of students

parents' occupational status	parents' educational attainment	
	with university degree	without university degree
Self-employed/free-lancers (large scale) e.g. Entrepreneurs with large firms/high income	highest	highest
Officers in the higher grade of civil service e.g. senior executive officers, lecturers at a secondary schools	highest	high
Employees in higher positions e.g. teachers, research assistants, authorised officers	highest	high
Self-employed/free-lancers (medium scale) e.g. retailers with large a shop, large farmers	highest	high
Officers in the upper grade of the civil service e.g. inspectors, chief inspectors, bailiffs	high	middle
Employees in middle positions e.g. officers in charge, bookkeepers, foremen, nurses	high	middle
Self-employed/free-lancers (small scale), masters e.g. retailers with a small shop, craftsmen, small farmers	high	middle
Officers in the middle/lower grade of the civil service e.g. conductors, secretaries	low	low
Employees in lower service e.g. stenotypists, salesmen	low	low
Skilled workers, employed craftsmen	low	low
Unskilled workers	low	low

The index considers that parent with the highest occupational status and highest educational attainment, respectively.
Source: Bundesministerium für Bildung und Forschung (2003, p. 472)

Table 4.b: Test for a common time trend of eligible and ineligible students: BAfoeG reform of 2001[c]

	(1) linear trend	(2) quadratic trend	(3) non-parametric trend
constant	−26.210	−24.892***	−27.173
	(188.338)	(6.130)	(212.408)
baseline hazard:			
year 2	0.546	0.922 **	0.765 **
	(0.345)	(0.438)	(0.346)
year 3	−0.340	0.154	0.162
	(0.571)	(0.727)	(0.595)
trend:			
eligible for BAfoeG	−0.094	0.833	0.190
	(0.986)	(2.979)	(0.872)
trend	−0.020	−0.605	–
	(0.093)	(0.852)	
trend × eligible for BAfoeG	0.084	−0.513	–
	(0.198)	(1.562)	
trend^2	–	0.066	–
		(0.094)	
trend^2 × eligible for BAfoeG	–	0.064	–
		(0.174)	
1993	–	–	−1.212 **
			(0.574)
1994	–	–	0.404
			(0.641)
1995	–	–	−1.226*
			(0.654)
1996	–	–	−0.332
			(0.555)
1997	–	–	−0.433
			(0.551)
1993 × eligible for BAfoeG	–	–	0.870
			(1.135)
1994 × eligible for BAfoeG	–	–	−0.926
			(1.229)
1995 × eligible for BAfoeG	–	–	0.721
			(1.131)
1996 × eligible for BAfoeG	–	–	−0.298
			(1.304)
1997 × eligible for BAfoeG	–	–	0.766
			(1.175)
mass points:			
ε_1	−11.641	−3.893***	−11.706
	(329.772)	(0.555)	(355.113)
$\text{P}(\varepsilon_1)^b$	0.364	0.420	0.374
${\pi_1}^b$	−0.559***	−0.324*	−0.513***
	(0.158)	(0.197)	(0.143)
${\varepsilon_2}^b$	6.656	2.816	7.008
$\text{P}(\varepsilon_2)^b$	0.636	0.580	0.626
N	339	339	339
NT	727	727	727
log-likelihood	-364.666	-361.116	-359.344
equality of trends (χ^2)	0.178	1.544	3.632
equality of trends (P< χ^2)	0.673	0.462	0.604

[a] Robust standard errors are reported in parenthesis. The level of significance is: *** 1%; ** 5%; * 10%.
[b] $P(\varepsilon_m)$ and ε_2 are calculated as described in appendix 4.A.3. π_1 is the log-odd of $P(\varepsilon_1)$.
[c] It is also controlled for the covariates as in Table 4.2.
Source: Baumgartner and Steiner (2006).

Table 4.c: Transition probability into tertiary education: student aid reform of 2001 as a heterogeneous treatment[a]

	(1)	(2)	(3)	(4)
constant	−28.622***	−33.176***	−27.382***	−34.488***
	(4.432)	(5.219)	(6.097)	(5.778)
baseline hazard:				
year 2	1.288***	1.363***	1.393***	1.336***
	(0.351)	(0.382)	(0.450)	(0.406)
year 3	0.193	−0.187	−0.157	−0.306
	(0.614)	(0.664)	(0.814)	(0.708)
difference-in-difference:				
after	−0.187	−0.315	−0.226	−0.340
	(0.362)	(0.373)	(0.375)	(0.373)
category I	−0.140	−0.195	0.104	–
	(0.871)	(0.925)	(0.977)	
category II	3.187*	–	–	–
	(1.656)			
category III	−0.703	−0.895	–	−1.057
	(0.670)	(0.722)		(0.727)
category IV	−0.021	–	–	–
	(0.795)			
after × category I	0.061	0.372	−0.343	–
	(1.062)	(1.143)	(1.170)	
after × category II	−1.893	–	–	–
	(1.916)			
after × category III	−0.104	0.081	–	0.197
	(0.843)	(0.894)		(0.893)
after × category IV	−1.096	–	–	–
	(1.100)			
covariates:				
father self-employed	0.360	0.371	0.287	0.474
	(0.523)	(0.575)	(0.645)	(0.595)
father white collar	0.638	0.691	0.289	0.740
	(0.489)	(0.543)	(0.629)	(0.577)
father civil servant	0.904	0.998	0.825	1.062
	(0.573)	(0.640)	(0.717)	(0.669)
father out of labour force	0.220	0.127	−0.278	0.854
	(0.754)	(0.790)	(0.878)	(1.015)
male	−2.068***	−2.249***	−2.025***	−2.138***
	(0.422)	(0.452)	(0.593)	(0.488)
abitur	2.196***	2.656***	2.409***	2.839***
	(0.556)	(0.601)	(0.644)	(0.676)
school leaving age	1.287***	1.462***	1.214***	1.498***
	(0.207)	(0.238)	(0.278)	(0.259)
father completed up. sec. schooling	−0.542	−0.715*	−0.445	−0.735
	(0.407)	(0.430)	(0.497)	(0.454)
mother completed up. sec. schooling	0.639*	0.739**	0.706*	0.704*
	(0.358)	(0.375)	(0.415)	(0.385)
German nationality	−0.891	−0.224	−0.656	0.207
	(0.643)	(0.708)	(0.802)	(0.799)
East Germany	0.807**	1.125***	0.788*	1.221***
	(0.386)	(0.411)	(0.476)	(0.430)
mass points:				
ε_1	−4.377***	−4.645***	−4.409***	−4.664***
	(0.584)	(0.651)	(0.775)	(0.681)
$P(\varepsilon_1)^b$	0.381	0.380	0.359	0.369
${\pi_1}^b$	−0.487***	−0.490***	−0.580***	−0.535***
	(0.142)	(0.146)	(0.192)	(0.157)
${\varepsilon_2}^b$	2.690	2.846	2.469	2.732
$P(\varepsilon_2)^b$	0.619	0.620	0.641	0.631
N	456	437	370	396
NT	798	705	575	631
log-likelihood[c]	-410.122	-360.263	-313.785	-327.613
log-likelihood[d]	-420.081	-371.897	-318.431	-336.403

All footnotes as in Table 4.2
Source: SOEP 2004.

Table 4.d: Test for a common time trend of eligible and ineligible students: BAfoeG reform of 1990

	(1) linear trend	(2) quadratic trend
constant	−22.096***	−17.621***
	(3.761)	(6.639)
baseline hazard:		
year 2	0.522	0.915*
	(0.359)	(0.477)
year 3	0.013	−0.022
	(0.587)	(0.626)
trend:		
trend	−0.046	−1.202 * *
	(0.084)	(0.613)
eligible for BAfoeG	−0.976	1.189
	(1.128)	(1.932)
trend × eligible for BAfoeG	0.800	−0.417
	(0.491)	(1.066)
trend^2	–	0.118
		(0.072)
trend^2 × eligible for Bafoeg	–	0.081
		(0.135)
covariates:		
father self-employed	0.585	0.152
	(0.644)	(0.775)
father white collar	0.117	−0.551
	(0.495)	(1.039)
father civil servant	0.689	0.803
	(0.628)	(0.794)
father out of labour force	2.177*	−0.343
	(1.313)	(1.047)
male	−0.530	−0.489
	(0.352)	(0.497)
abitur	1.572***	1.302 * *
	(0.455)	(0.619)
school leaving age	0.904***	0.848 * *
	(0.175)	(0.339)
father completed up. sec. schooling	0.654	2.047
	(0.432)	(1.356)
mother completed up. sec. schooling	−0.021	0.410
	(0.516)	(0.772)
German nationality	−0.803	−0.283
	(0.536)	(0.534)
mass points:		
ε_1	−6.134***	−2.218***
	(1.513)	(0.720)
P(ε_1)^b	0.325	0.499
$\pi_1{}^b$	−0.730***	−0.004
	(0.192)	(0.253)
$\varepsilon_2{}^b$	2.956	2.209
P(ε_2)^b	0.675	0.501
N	250	250
NT	530	530
log-likelihood	-273.835	-276.416
equality of trends (χ^2)	2.653	0.871
equality of trends (prob< χ^2)	0.103	0.647

[a] Robust standard errors are reported in parenthesis. The level of significance is: *** 1%; ** 5%; * 10%.

[b] $P(\varepsilon_m)$ and ε_2 are calculated as described in appendix 4.A.3. π_1 is the log-odd of $P(\varepsilon_1)$.

Source: SOEP 2004, sample A and B.

Chapter 5

General conclusion and summary: The importance of empirical analysis

This doctoral thesis evaluates three selected issues in the field of labour market policy and educational policy. The first chapter analyses two active labour market programmes that support the decision of unemployed people to become self-employed. The second chapter analyses the effect of reduced class size on early career earnings, and the third chapter evaluates the effect of student aid on enrolment. Before I turn to the general conclusion that can be drawn from these policy evaluations, I first present a brief concluding summary of the main findings.

Self-employment out of unemployment: In Germany, there are two active labour market policy programmes that support the self-employment decision of unemployed people – namely ÜG (bridging allowance) and ExGZ (start-up subsidy). ÜG depends on the transfers participants would have received if they were still unemployed and supports the first six months in self-employment. ExGZ, on the other hand, provides support for up to three years and is a fixed but annually decreasing amount.

Due to the institutional setting of these programmes, rational programme

choice is mainly driven by unemployment transfers. I speculate that this fact could introduce a selection bias if those with higher unemployment transfers have unobservable characteristics that influence their hazard rate. Testing for a potential selection bias using a two-stage selection model, however, suggests that selection is not a problem in my sample.

Modelling the transition rates out of (supported) self-employment into unemployment and into dependent employment, I observe negligible low rates for entering the first labour market. However, the risk of becoming unemployed again is also relatively low but jumps to a higher level at specific points in time. When the support is reduced (ExGZ) or when the support ends (ÜG), participants are more likely to return to the pool of unemployed. One tentative explanation for this could be that participants extended their received transfers by participating in either programme. An alternative explanation for these findings could be that the new firm did not generate enough income and therefore it was more attractive to become unemployed again once the support ended or was reduced.

About 1.5 years after the programmes were joined, as much as 82 percent of ExGZ participants are still self-employed. This high survival might be due to the fact that the support is still ongoing. However, the survival of ÜG participants is 83 percent on average, although they run their businesses for more than one year without any support.

It is still too early for a final evaluation of the ExGZ programme, since it is still ongoing. That is, I am observing programme participants in their second year of support but ExGZ supports them for up to three years. Nevertheless, the sustainability of supported self-employment is very similar and on a relatively high level in both programmes.

The German parliament is currently resolved to merging ÜG and ExGZ. The new scheme is planned to run for nine months in the first phase of support

and the amount of the support is planned to be the unemployment transfer plus a fixed amount of 300 Euro for social security contributions. The second phase lasts for six further months and grants only the fixed amount of 300 Euro for social security contributions. ÜG and ExGZ are attractive for people with different skills patterns and the institutional setting of both programmes seems to fit the necessities of the new entrepreneurs very well, since both programmes are successful in terms of sustainability. Those who had chosen ÜG are supported under the new scheme for nine months longer, although they would not necessarily require a longer support period. Moreover, those who had chosen ExGZ because of their low unemployment transfer may now be offered a level of support that is too low to secure their starting phase. The new programme thus risks the success of two successful programmes.

Class size effects: Almost all parties concerned with education – that is, parents, teachers, policy makers and voters – have a uniform preference for smaller classes to improve the quality of secondary education. However, it is not empirically clear whether the labour market values smaller classes through higher wages or not.

Research into the relation between school quality and earning has focused mainly on the US and recently also on the UK. The main finding of this literature is that there appears to be no significant effect of class size on early career earnings. Neither could I find discernible effects of the quality of schooling measured by class size on early career earnings in Germany, thus further confirming this literature.

The cohorts of my data are too young to observe their complete age-earning profile. The dependent variable is actually defined as earnings in the early career. But beneficial effects of school quality may not begin to emerge until the cohort has reached its peak earnings years, namely around the age of forty.

To combine SOEP data with my panel data on class size, I had to observe the year of graduation. Using this combined data prevented me from observing an age-earning profile at the peak for a sufficiently large sample today. In some years time, when data is available that spans a complete age-earning profile, further research will reveal whether there are beneficial class size effects on earnings later on in the labour market career.

The policy conclusion suggested by my findings is that reducing class size would not improve early career earnings. No doubt there exist schools in impoverished areas where more spending is needed to raise school quality but on average school quality as measured by class size has no significant effect on early career earnings.

Student aid: I evaluated two major BAfoeG reforms, which were implemented in 1990 and 2001. The common aspect of both reforms is that they aimed to increase enrolment rates in higher education of students from low-income households. In 1990 the repayment obligations were changed and a grant/loan system was introduced and in 2001 the eligibility rules were changed, resulting in an average increase of ten percent. I have used the supposedly exogenous variation in student aid induced by these 'natural experiments' to test whether the political aim has been achieved and to identify the causal effect more generous student aid might have on enrolment in higher education in Germany.

The introduction of both reforms had an effect on eligible students only and reduced their net costs of pursuing higher education. I explore this 'natural experiment' and apply a difference-in-difference estimator in a discrete time hazard rate model. The estimation results show that neither reform has achieved its aim and that enrolment from the targeted students did not increase.

One explanation for the inefficient estimation of the treatment effect could be the relatively small sample. However, a larger informative data set for Germany, which would allow identification of potential BAfoeG eligibility for both students and school leavers, who choose not to study, is not available. Therefore, I cannot really judge to which extent the insignificance of the estimated treatment effect is due to the small sample size.

An alternative explanation for these somewhat surprising results could be that the basic identifying assumption might be violated. To identify the difference-in-difference estimator, I have to assume that both groups of students follow a common time trend. This assumption cannot be tested. However, since the weaker assumption of a common time trend either before or after the reform does not appear to be violated, I have no indication of a violation of the basic identifying assumption. Nevertheless, it might be that the decline in the private returns to education has had different effects on the enrolment decisions of students from low-income households and those not eligible to receive BAfoeG. Although this may seem theoretically plausible, I have no conclusive evidence supporting this view.

Therefore, I interpret the empirical results as being indicative of the ineffectiveness of more generous student aid in raising enrolment rates in higher education in Germany.

General conclusion: In this doctoral thesis, I evaluated three selected issues in the field of labour market and education policy and showed how successful these policies have actually been. Policy makers have a priori expectations, when implementing a new policy. In almost all instances, however, they are guided by theoretical considerations and/or the bargaining power of those parties engaged in forming new policies. Whether an implemented policy has achieved its hoped-for outcome, however, is not a question of the most

persuasive arguments, it is an empirical question which should be answered empirically.

Most scholars would agree, guided by theory, that if the net cost of study is reduced, with a changed student-aid system, more people would enrol in higher education. However, it turns out that this behaviour could not be found in the data. Hence, it is questionable whether the money spent on BAfoeG is invested efficiently enough. The same likely inefficiency was found if more fiscal money was spent on reducing class size in secondary schools. Although there is a uniform preference towards smaller classes, I could not establish a statistically significant link between class size and early career earnings. And finally, some people argue that unemployed people are pushed into self-employment due to unavailable employment options, which would be an unfavourable motivation for setting up a new firm. However, analysing the sustainability of self-employment of formerly unemployed people, it turned out that these firms are very successful in terms of sustainability. These selected examples showed that it is difficult to gauge a policy outcome on theoretical considerations only and that empirical work can help in measuring the actual effect of policy changes.

In a nutshell, all policies – not only in the field of the labour market and education – should be empirically evaluated, since the outcome of an adjusted and/or reformed policy is far too often ambiguous. And the challenges we as a society are faced with are too severe to base the decisions about new policy reforms on theoretical considerations only. Empirical work can help to gauge the likely effects of the policies that society decides to implement.

Bibliography

Ai, C., and E. C. Norton (2003): "Interaction terms in logit and probit models," *economic letters*, 80, 123–129.

Angrist, J., and V. Lavy (2002): "New Evidence on Classroom Computers and Pupil Learning," *The Economic Journal*, 112(October), 735–765.

Angrist, J. D., and A. B. Krueger (1999): "Empirical strategies in labor economics," in *Handbook of Labor Economics*, ed. by O. Ashenfelter, and D. Card, no. 3A, chap. 23, pp. 1277–1366. Elsevier.

Ashenfelter, O., C. Harmon, and H. Oosterbeek (1999): "Economic Returns to Schooling: New Evidence," *Labour Economics*, 6(4), vii–viii.

Barr, N. (2003): "Financing higher education - comparing the options," mimeo, London School of Economics.

——— (2004): "Higher Education Funding," *Oxford Review of Economic Policy*, 20(2), 264–283.

Barr, N., and I. Crawford (1998): "Funding Higher Education in an Age of Expansion," *Education Economics*, 6(1), 45–70.

Barro, R. J., and J.-W. Lee (2001): "International Data on Education Attainment Updates and Implications," *Oxford Economic Paper*, 53(3), 541–563.

Bauer, T. K., and J. P. Haisken-DeNew (2001): "Employer learning and the returns to schooling," *Labour Economics*, 8, 161–180.

Baumgartner, H. J., and M. Caliendo (2006): "Turning unemployment into self-employment: The relative effectiveness of two programmes in west Germany," DIW Berlin, mimeo.

Baumgartner, H. J., and V. Steiner (2005a): "Lehrerausstattungsvergleich der allgemeinbildenden Schulen in ausgewählten Großstädten," project report to the Senatsverwaltung für Finanzen Berlin (unpublished).

Baumgartner, H. J., and V. Steiner (2005b): "Student aid, repayment obligations and enrolment in higher education in Germany: Evidence from a "natural experiment"," *Journal of Applied Social Science Studies*, 125(1), 29–38, Proceedings of the "6th International Conference of German Socio-Economic Panel Study Users (SOEP 2004)".

——— (2006): "Does more generous student aid increase enrolment rates into higher education? Evaluating the German student aid reform of 2001," Discussion Paper 563, DIW Berlin.

Becker, G. S. (1962): "Investment in human capital: A theoretical analysis," *Journal of Political Economy*, (70), 9–49.

Beirat für Ausbildungsförderung (1988): "Vorschläge zur Reform des Bundesausbildungsförderungsgesetzes (BAföG)," in *Schriftreihe Grundlagen und Perspektiven für Bildung und Wissenschaft*, ed. by Bundesministerium für Bildung und Wissenschaft, vol. 21, pp. 1–155. Koelblindruck.

Betts, J. R. (1995): "Does school quality matter? Evidence from the National Longitudinal Survey of Youth," *The Review of Economics and Statistics*, 77, 231–250.

——— (1996): "Is there a link between school inputs and earnings?," in *Does Money Matter?*, ed. by G. Burtless, pp. 141–191. Brookings Institution, Washington, D.C.

Blanchflower, D. G. (2004): "Self-employment: More may not be better," Discussion Paper 10286, National Bureau of Economic Research.

Blien, U., F. Hirschenauer, M. Arendt, H. J. Braun, D.-M. Gunst, S. Kilcioglu, H. Kleinschmidt, M. Musati, H. Ross, D. Vollkommer, and J. Wein (2004): "Typisierung von Bezirken der Agenturen für Arbeit," *Zeitschrift für Arbeitsmarktforschung*, (2), 146–161.

Blundell, R., and M. Costa Dias (2000): "Evaluation Methods for Non-Experimental Data," *Fiscal Studies*, 21(4), 427–468.

Boockmann, B., and V. Steiner (2000): "Cohort effects and the returns to education in West Germany," Discussion Paper 00-05, ZEW.

Bookmann, B., and V. Steiner (2000): "Cohort effects and the returns to education in West Germany," Discussion Paper 05, ZEW.

Borjas, G. J. (2000): *Labor Economics*. McGraw-Hill, USA, 2 edn.

Box-Seffensmeir, J. M., and J. S. Bradford (2004): *Event History Modeling: A gude for social scientists*. Cambridge University Press.

Brixy, U., and R. Grotz (2002): "Räumliche differenzierungen von Betriebsgründungsintensität und Überlebenschancen in Westdeutschland 1983 bis 1997," *Raumforschung und Raumordnung*, 60(2), 100–122.

Bruce, D., and H. J. Schuetze (2004): "The labor market consequences of experience in self-employment," *Labour Economics*, 11(5), 575–598.

Bundesministerium für Bildung und Forschung (2003): *Die wirtschaftliche Lage der Studierenden in der Bundesrepublik Deutschland 2003*.

Burtless, G. (1996): "Introduction and Summary," in *Does money matter?*, ed. by G. Burtless, pp. 1–42. The Brookings Institute, Washington D.C.

Caliendo, M., V. Steiner, and H. J. Baumgartner (2005): "Teil IV: Mikroökonometrische Analysen," in *Evaluation der Maßnahmen zur Umsetzung der Vorschläge der Hartz-Kommission - Existenzgründungen (Modul 1e)*, ed. by Forschungsverbund IAB, DIW, SINUS, GfA, infas, chap. IV, pp. 338–447. BMWA, Berlin.

Cameron, A. C., and P. K. Trivedi (2005): *Microeconometrics*. Cambridge University Press.

Cameron, S. V., and J. J. Heckman (1998): "Life Cycle Schooling and Dynamic Selection Bias: Models and Evidence for Five Cohorts of American Males," *Journal of Political Economy*, 106(2), 262–333.

Card, D. (1995): "Earnings, Schooling, and Ability Revisited," *Research in Labor Economics*, 14, 23–48.

——— (1999): "The causal effect of education on earnings," in *Handbook of Labor Economics*, ed. by O. Ashenfelter, and D. Card, vol. 3a, pp. 1801–1863. Elsevier, Amsterdam.

Card, D., and A. B. Krueger (1992a): "Does School Quality Matter? Returns to Education and the Characteristics of Public Schools in the United States," *Journal of Political Economy*, 100(1), 1–40.

——— (1992b): "School Quality and Black-White Relative Earnings: A Direct Assessment," *Quarterly Journal of Economics*, 107, 151–200.

——— (1996a): "Labor market effects of school quality: theory and evidence," *NBER Working Paper Series*, 5450.

——— (1996b): "Labour Market Effects of School Quality: Theory and Evidence," in *Does Money Matter? The Link Between Schools, Student Achievement and Adult Success*, ed. by G. Burtless, pp. 97–140. Brookings Institution, Washington D.C.

——— (1996c): "School Resources and Student Outcomes: An Overview of the Literature and New Evidence from North and South Carolina," *Journal of Economic Perspectives*, 10(5), 31–50.

——— (1998): "School Resources and Student Outcomes," *Annals, AAPSS*, (559), 39–53.

Carneiro, P., and J. J. Heckman (2002): "The Evidence on Credit Constraints in Post-secondary Schooling," *Economic Journal*, 112(482), 705–34.

Dearden, L., J. Ferri, and C. Meghir (2000): "The Effect of School Quality on Educational Attainment and Wages," Discussion Paper 22, Institute for Fiscal Studies.

Dolton, P., and A. Vignoles (2000): "The Impact of School Quality on Labour Market Success in the United Kingdom," mimeo.

Dustmann, C., N. Rajah, and A. van Soest (2003): "Class size, education, and wages," *Economic Journal*, 113, 99–120.

Dynarski, S. M. (2002a): "The Behavioral and Distributional Implications of Aid for College," *American Economic Review*, 92(2), 279–85.

——— (2002b): "Loans, Liquidity, and Schooling Decisions," mimeo, University of Harvard.

——— (2003): "Does Aid Matter? Measuring the Effect of Student Aid on College Attendance and Completion," *American Economic Review*, 93(1), 279–288.

Fertig, M., and R. E. Wright (2004): "School Quality, Educational Attainment and Aggregation Bias," Discussion Paper 994, IZA.

Gang, I. N., and K. F. Zimmermann (2000): "Is Child Like Parent? Educational Attainment and Ethnic Origin," *Journal of Human Resources*, 35(3), 550–69.

Greene, W. H. (2003): *Econometric analysis*. Prentice Hall, 5 edn.

Haisken-DeNew, J. P., and J. R. Frick (eds.) (2004): *Desktop Companion to the German Socio-Economic Panel*. DIW-SOEP, Berlin.

Halvorsen, R., and R. Palmquist (1980): "The interpretation of dummy variables in semilogarithmic equations," *American Economic Review*, 70(3), 474–475.

Hamilton, B. H. (2000): "Does entrepreneurship pay? An empirical analysis of the returns to self-employment," *Journal of Political Economy*, 108(3), 604–631.

Hanushek, E. A. (1995): "Interpreting Recent Research on Schooling in Developing Countries," *The World Bank Research Observer*, 10(2), 227–264.

——— (1996): "School resources and student performance," in *Does Money Matter*, ed. by G. Burtless, pp. 43–73. The Brookings Institute, Washington.

——— (2002): "Publicly provided education," Discussion Paper 8799, National Bureau of Economic Research.

Hanushek, E. A., S. G. Rivkin, and L. L. Taylor (1996): "Aggregation and the estimated effects of school resources," *The Economic and Social Review*, 78(4), 611–627.

Harmon, C., and I. Walker (2000): "The Returns to the Quality and Quantity of Education: Evidence for Men in England and Wales," *Economica*, 67, 19–35.

Harmon, C., I. Walker, and N. Westergaard-Nielsen (eds.) (2001): *Education and Earnings in Europe*. Edward Elgar Publishing, Northampton.

Hausman, J., and D. McFadden (1984): "Specification tests for the multinomial logit model," *Econometrica*, 52(5), 1219–1240.

Heckman, J., A. Layne-Farrar, and P. Todd (1995): "The Schooling Quality-Earnings Relationship: Using Economic Theory to Interpret Functional Forms Consistent with the Evidence," *NBER Working Paper Series*, (5288).

——— (1996): "Does Measured School Quality Really Matter? An Examination of the Earnings-Quality Relationship," in *Does Money Matter? The Effect of School Resources on Student Achievement and Adult Success*, ed. by G. Burtless, pp. 192–289. Brookings Institution Press, Washington, D.C.

Heckman, J. J. (1979): "Sample selection bias as a specification error," *Economica*, 47, 153–162.

Heckman, J. J., and B. Singer (1984): "A method for minimising the impact of distributional assumptions in econometric models of duration data," *Econometrica*, 52(2), 271–320.

Heinesen, E., and B. Schindler Rangvid (2005): "Class size effects on early career employment," presented at the annual ESPE conference 2005 in Paris.

HIS (1987): *Studienförderung und Ausbildungsentscheidungen bzw. Studienverhalten*. HIS, Hanover.

Hoxby, C. M. (2000): "The effects of class size on student achievement: New evidence from population variation," *Quarterly Journal of Economcs*, pp. 1239–1285.

IAB, and GfA (2005): "Teil II: Bestandsaufnahme des Gründungsgeschehens," in *Evaluation der Maßnahmen zur Umsetzung der Vorschläge der Hartz-Kommission - Existenzgründungen (Modul 1e)*, ed. by Forschungsverbund IAB, DIW, SINUS, GfA, infas, chap. IV, pp. 338–447. BMWA, Berlin.

Jenkins, S. P. (1995): "Easy estimation methods for discrete-time duration models," *Oxford Bulletin of Economics and Statistics*, 57(1), 129–138.

——— (2004a): "Independent competing risks models for interval-censored survival time data," Research seminar at DIW Berlin, April 2, 2004.

——— (2004b): *Survival Analysis*. Essex.

Kane, T. J. (1995): "Rising Public College Tuition and College Entry: How Well Do Public Subsidies Promote Access to College?," Discussion Paper 5164, National Bureau of Economic Research.

Keane, M. P. (2002): "Financial Aid, Borrowing Constraints, and College Attendance: Evidence from Structural Estimates," *American Economic Review*, 92(2), 293–97.

Keane, M. P., and K. I. Wolpin (2001): "The effect of parental transfers and borrowing constraints on educational attainment," *International Economic Review*, 42(4), 1051–1103.

Kennedy, P. (1981): "Estimation with correctly interpreted dummy variables in semilogarithmic equations," *American Economic Review*, 71(4), 801.

Koch, S., and F. Wiessner (2003): "Wer die Wahl hat, hat die Qual," *IAB Kurzbericht*, (2).

Kodde, D. A., and J. M. M. Ritzen (1985): "The Demand for Education under Capital Market Imperfections," *European Economic Review*, 28(3), 347–62.

Lauer, C. (2002): "Enrolments in higher education: do economic incentives matter?," *Education + Training*, 44(4/5), 179–184.

Lauer, C., and V. Steiner (1999): "Returns to Human Capital in Germany: Review of the Empirical Literature," in *Returns to Human Capital in Europe: A Literature Review*, ed. by R. Asplun, and P. T. Pereira, pp. 125–144. Taloustierto Oy, Helsinki.

——— (2001): "Germany," in *Education and Earnings in Europe*, ed. by C. Harmon, I. Walker, and N. Westergaard-Nielsen, pp. 102–128. Edward Elgar Publishing, Northampton.

Lazear, E. P. (2001): "Educational Production," *Quarterly Journal of Economics*, 116(3), 777–803.

Lazear, E. P. (2003): "Entrepreneurship," Discussion Paper 760, IZA.

Leech, D., and E. Campos (2001): "Is comprehensive education really free? A case study of the effects of secondary school admissions policies on house prices in one local area," Discussion Paper 581, University of Warwick.

Leung, S. F., and S. T. Yu (1996): "On the choice between sample selection and two-part models," *Journal of Econometrics*, 72(1-2), 197–229.

Manski, C. F., and D. A. Wise (1983): *College Choice in America*. Harvard University Press, Cambridge, MA.

McPherson, M. S., and M. O. Schapiro (1991): "Does Student Aid Affect College Enrollment? New Evidence on a Persistent Controversy," *American Economic Review*, 81(1), 309–18.

Meyer, B. D. (1995): "Natural and Quasi-experiments in Economics," *Journal of Business and Economic Statistics*, 13(2), 151–161.

Moffitt, R. A. (1999): "Models of treatment effects when responses are heterogeneous," *Proceedings of the National Academy of Sciences of the United States of America*, 96, 6575–6576.

Moulton, B. R. (1990): "An illustration of a pitfall in estimating the effects of aggregate variables on micro units," *The Review of Economics and Statistics*, pp. 334–338.

Nawata, K. (1993): "A note on the estimation of models with sample-selection bias," *Economics Letters*, 42, 15–24.

Nawata, K., and N. Nagase (1996): "Estimation of sample selection bias models," *Economic Reviews*, 15(4), 387–400.

Niefert, M. (2005): "The impact of self-employment experience on wages and the risk of unemployment," presented at Vereinstagung 2005 in Bonn.

Oberschachtsiek, D. (2004): "Exit-Entscheidung neur Selbständiger auf Basis human-kapitaltheoretischer Überlegungen – Eine Anwendung des Lazear-Konzepts auf Unternehmensgründer aus der Arbeitslosigkeit," *Perspektiven der Mittelstandsforschung*, pp. 191–201.

Polachek, S. W., and S. Siebert (1993): *The Economics of Earnings*. Cambridge University Press, Cambridge.

Poterba, J. M. (1996): "Government Intervention in the Markets for Education and Health Care: How and Why?," in *Individual and social responsibility: Child care, education, medical care, and long-term care in America*, ed. by V. R. Fuchs, pp. 277–304. University of Chicago Press, Chicago.

Psacharopoulos, G. (1994): "Returns to Investment in Education: A global Update," *World Development*, 22(9), 1325–1343.

Puhani, P. A. (1997): "Foul or fair? The Heckman correction for sample selection and its critique: A short survey," Discussion Paper 97-07, ZEW.

Rabe-Hesketh, S., and A. Skrondal (2004): *Generalized latent variable modeling: Multilevel, longitudinal, and structural equation models*. Chapman & Hall, London.

Rabe-Hesketh, S., A. Skrondal, and A. Pickles (2001): *GLLAMM Manual - Technical Report 2001/01*. Department of Biostatistics and Computing, Institute of Psychiatry, King's College, London.

Ramsauer, U. (2002): "Einführung," in *BAföG*. Beck-Texte im dtv.

Reize, F. (2000): "Leaving unemployment for self-employment," Discussion Paper 26, ZEW.

Riphahn, R. T. (2003): "Cohort Effects in the Educational Attainment of Second Generation Immigrants in Germany: An Analysis of Census Data," *Journal of Population Economics*, 16(4), 711–737.

Roger, W. (1993): "Regression standard errors in clustered samples," *Stata Technical Bulletin Reprints*, 3, 88–94.

Sander, W. (1993): "Expenditures and student achievement in Illinois," *Journal of Public Economics*, 52, 403–416.

Schneider, T. (2004): "Der Einfluss des Einkommens der Eltern auf frühe Entscheidungen in der Bildungskarriere.," *Zeitschrift für Soziologie*, 33(6), 471–492.

Steiner, V. (2001): "Unemployment Persistence in the West German Labour Market: Negative Duration Dependence or Sorting?," *Oxford Bulletin of Economics and Statistics*, 63(1), 91–113.

van de Ven, W. P. M. M., and B. M. S. van Praag (1979): "The demand for deductibles in private health insurance – A probit model with sample selection," *Journal of Econometrics*, 17, 229–252.

van den Berg, G. J. (2001): "Duration models: Specification, identification, and mulitple durations," in *Handbook of Econometrics*, ed. by J. J. Heckman, and E. Leamer, vol. V. North-Holland, Amsterdam.

West, A., H. Pennell, T. Travers, and R. West (2001): "Financing school-based education in England: poverty, examination results, and expenditure," *Environment and Planning C: Government and Policy*, 19, 461–471.

White, H. (1980): "A Heteroskedasticity-consistent Covariance Matrix Estimator and a Direct Test for Heteroskedasticity," *Econometrica*, 48, 817–838.

Wilson, K. (2000): "Using the PSID to study the effects of school spending," *Public Finance Review*, 28(5), 428–451.

Winter-Ebmer, R., and A. Wirz (2002): "Public Funding and Enrolment into Higher Education in Europe," Discussion Paper 503, IZA.

Wössmann, L. (2000): "Schooling Resources, Educational Institutions, and Student Performance: The International Evidence," Discussion Paper 983, Institut für Weltwirtschaft Kiel.

——— (2001): "Schulsystem und Schülerleistung im internationalen Vergleich: Was Institutionen ausmachen," *Die Weltwirtschaft*, (3), 283–304.

Wooldridge, J. M. (2002): *Econometric analysis of cross section and panel data*. MIT Press.

——— (2003): *Introductory Econometrics: A Modern Approach*. Thomson South-West.

Wunderlich, G. (2004): "Die Hartz-Gesetze I-IV," in *Hartz-Gesetze – Methodische Ansätze zu einer Evaluierung*, ed. by T. Hagen, and A. Spermann, vol. 74 of *ZEW Wirtschaftsanalysen*. Nomos.

Acknowledgement

I would like to thank Viktor Steiner, my supervisor, for his many suggestions and guidance through my research. From him I learnt how to base thorough policy advise upon empirical research.

I am grateful to Thorsten Schneider, who became never tired of explaining all the details of SOEP data to me. I should also thank all the other colleagues at DIW Berlin and especially in the department of public economics with whom I had vital and eye opening discussions at many different points of my research. Above all Marco Caliendo with whom I worked closely together evaluating start-up subsidies for the project report to the Federal Ministry of Economics and Labour.

My research profited very much from discussion with and comments from participants at a list of international conferences (ESWC, EEA, VfS, ESPE, EALE), summer schools (EEA, IZA), and seminars (DIW Berlin, BeNA). I thank especially Gerald van den Berg, Felix Büchel, Stephen Jenkins, and Jeffrey Smith who commented my work at these instances.

I am also thankful for the assistance of Steffen Künn, who was an aiding hand in editing the graphs of this thesis.

I express my gratefulness to two persons, who I met in a trade union context many years ago – Alexander Rager and Luitpod Rampeldshammer. They motivated me to think more about politics generally and economic policy especially. If I had not met them many years ago, I had not quitted my job as a telecommunication electrician to start with a second chance education and to

study economics.

To close this list, I thank my sister and my parents as well as my best friend Bettina Langer for their emotional support throughout my work on this thesis.

German Abstracts

In diesem Abschnitt werden gem. §7(4) der Promotionsordnung des Fachbereichs Wirtschaftswissenschaften vom 27. Januar 1993 die drei Hauptkapitel meiner Promotionsschrift kurz in deutscher Sprache zusammengefasst.

Start-ups: Zwei Programme der aktiven Arbeitsmarktpolitik fördern die Existenzgründung von Arbeitslosen in Deutschland. Überbrückungsgeld (ÜG) fördert die ersten sechs Monate in selbständiger Tätigkeit durch einen Förderbetrag, der sich aus dem Arbeitslosentransfer beim Eintritt in die Fördermaßnahme plus einer Pauschale für Sozialabgaben zusammensetzt. Das andere Programm – Existenzgründungszuschuss – wurde im Zuge der 'Hartz-Reformen' eingeführt und förderte die ersten drei Jahre in Selbständigkeit durch einen fixen, jedoch jährlich abnehmenden Förderbetrag. Ich untersuche die Nachhaltigkeit von geförderten Existenzgründungen unter Verwendung eines einzigartigen Datensatzes, der die Prozessdaten der Bundesagentur für Arbeit durch Interviewdaten der geförderten Existenzgründer ergänzt. Die Schätzergebnisse eines diskreten Übergangsratenmodells unter Berücksichtigung von unbeobachteten Heterogenitäten zeigen, dass nach Aufgabe der selbständigen Tätigkeit die Übergangsrate in eine abhängige Beschäftigung vernachlässigbar gering ist, wohingegen die Übergangsrate in Arbeitslosigkeit sprunghaft ansteigt. Diese sprunghaften Anstiege der Übergangsraten werden nach sechs Monaten für durch ÜG Geförderte beobachtet, also nach dem Auslaufen der Förderphase, sowie nach zwölf Monaten für durch ExGZ

Geförderte, also wenn die Förderung im zweiten Förderjahr reduziert wird. Hinsichtlich der Nachhaltigkeit, also der Fortführung der geförderten Neugründung, können beide Programme als sehr erfolgreich eingestuft werden, da ungeachtet des Förderinstruments über 80 Prozent der Geförderten 1,5 Jahre nach Förderbeginn immer noch selbständig sind.

Class size effects: Es ist eine weit verbreitete Ansicht, dass sich die Schulqualität der Sekundarstufe am leichtesten durch eine Reduktion der Klassengröße verbessern lässt. Durch eine individuellere Betreuung der Schüler, so die Hoffnung, wird der vermittelte Lehrstoff besser verstanden und es kann aufgrund eines selteneren Auftretens von Unterrichtsstörungen auch mehr Lehrstoff vermittelt werden. Darüber hinaus besteht die Möglichkeit, dass die Schüler durch mehr Interaktion mit den Lehrkräften und ihren Mitschülern mehr Selbstvertrauen und Verantwortungsbewusstsein erlangen. Kleinere Klassen verbessern somit sowohl die 'Hardskills' als auch die 'Softskills' und beides wird positiv auf dem Arbeitsmarkt bewertet. In diesem Kapitel untersuche ich den Effekt einer Klassengrößenreduktion auf die Einstiegsgehälter. Für diese Untersuchung ergänze ich Daten des Sozio-ökonomische Panels (SOEP) um Klassengrößeninformationen der westdeutschen Land- und Stadtkreise und nutze somit die regionale Querschnitts- als auch die Längsschnittsvariation der Klassengrößen um diesen Effekt zu schätzen. Der Einfluss ist allerdings weder statistisch signifikant, noch hinsichtlich der Punktschätzung ökonomisch bedeutsam. Meine Schätzergebnisse bestätigen somit die internationale Literatur, die sich bislang hauptsächlich auf Daten der Vereinigten Staaten als auch Großbritanniens stützt.

Student aid in Germany: Studenten aus einkommensschwachen Haushalten erhalten in Deutschland eine finanzielle Unterstützung durch das Bundesausbildungsförderungsgesetz (BAföG). Hierdurch soll qualifizierten Abiturien-

ten ungeachtet ihrer finanziellen und sozialen Situation ein Studium ermöglicht werden. Neben diesem verteilungspolitischen Aspekt der Studienförderung soll BAföG eine optimale Ausnutzung der vorhandenen Bildungsreserven erreichen. Ob BAföG die Studierentscheidung von Abiturienten beeinflusst, wird am Beispiel von zwei BAföG-Reformen evaluiert, die beide zu einer substantiellen Reduktion der Studienkosten führten. 1990 wurde die Rückzahlungsverpflichtung umgestellt, d.h. bis 1990 wurde BAföG als zinsloses Darlehen gewährt und nach 1990 zu 50 Prozent als nichtzurückzuzahlendes Stipendium. In 2001 wurde BAföG erneut reformiert indem die Freibeträge und Bedarfssätze angehoben wurden, sodass sich der monatliche Förderbetrag im Durchschnitt um zehn Prozent erhöhte. Beide Reformen können als 'natürliche Experimente' angesehen werden, da sie nur einen Teil der Studierenden betreffen. Ich verwende einen Differenzen-von-Differenzen Ansatz in einem diskreten Übergangsratenmodell um den kausalen Effekt der Studienförderung auf die Studierneigung der BAföG-berechtigten Studenten zu schätzen. Unter Verwendung der Daten des Sozio-ökonomische Panels (SOEP) kann kein Einfluss des BAföGs auf die Studierneigung nachgewiesen werden.

Curriculum Vitae*

	Personal Details:
	Nationality: German, Austrian
	Date of birth: 1st February 1972
	Place of birth: Passau (Germany)
Areas of interest	Applied micro-econometrics, labour economics, and economics of education.
	Employment:
since Dec 06	Desc Officer at the German Ministry of Finance
Nov 01 - Nov 06	German Institute for Economic Research (DIW Berlin): Research Associate I moved to the department of public economics in order to start with my PhD research. Since June 2002 I am a PhD candidate at the Free University Berlin under the supervision of Prof. Viktor Steiner.
Jan 01 - Oct 01	German Institute for Economic Research (DIW Berlin): Research Associate I co-ordinated the research network Berlin-Brandenburg and assisted the research of Prof. Klaus F. Zimmermann.
	Teaching experience (Tutor to the courses of Prof. Viktor Steiner):
winter term 06/07	Free University of Berlin: Introduction to empirical economic research.
summer terms 03-05	Free University of Berlin: Introduction to empirical economic research.
winter term 02/03	Free University of Berlin: Introduction to micro-econometrics.
	Higher education:
10.-14. May 04	DIW-PhD-Seminar on Panel Econometrics by Badi Baltagi.
19.-25. Apr 04	7th IZA European Summer School in Labor Economics by Armin Falk and Christian Belzil.
8.-12. Sep 03	XIVth Euro Summer School: "Microeconometrics" at the IFS.
7.-13. Apr 03	6th IZA European Summer School in Labor Economics by Christopher Pissarides and Jeffrey Smith.
Since June 02	PhD candidate at the Free University Berlin. My PhD thesis is titled "Empirical evaluation of selected labour market and education policies".
Oct 99 - Sep 00	Postgraduate studies at the University of Warwick - Master of Science in Economics. Walker/Wooders (Microeconomics), Miller/Rankin (Macroeconomics), Arulampalam/Leech (Econometrics), Cowling (Industrial Structure), Naylor/Oswald (The Market for Labour). MSc dissertation: "Wage Share, Unionism, and Market Structure in UK Manufacturing Industries".
Aug 00 - Sep 00	Ferrara Graduate School in Industrial Development Policy by Keith Cowling and Roger Sugden
Apr 95 - Oct 98	Undergraduate studies at the Hochschule für Wirtschaft und Politik Hamburg - Dipl. Volkswirt.
Sep 97 - May 98	Exchange student at the University of Abertay Dundee - Bachelor of Arts with Distinction in Economics.
	Scholarships and Prices:
	Award for Presentations at International Conferences (2005) granted by Verein für Socialpolitik and financed by the Deutsche Bundesbank.
	Bursary place to present at the World Congress of the Econometric Society 2005.
	Fee waver to present at the ESPE 2005 Conference.
	Award for Presentations at International Conferences (2004) granted by Verein für Socialpolitik and financed by the Deutsche Bundesbank.
	Hans Böckler Stiftung (1995-1998 & 1999-2000).
	ESRC Research Studentship (2000 declined).

*Lebenslauf gem. §7(4) der Promotionsordnung des Fachbereichs Wirtschaftswissenschaften vom 27. Januar 1993

Referee activity:
German Economic Review
Journal of Applied Social Science Studies

Memberships in academic association:
Econometric Society (since 2005).
European Economic Association (since 2004).
Berlin Network of Labour Market Research (BeNA) (since 2004).
Royal Economic Society (since 2003).
European Association of Labour Economists (2003-2004).
Verein für Socialpolitik (since 2002).

Publications:

Refereed Journal:
(2005) Student Aid, Repayment Obligations and Enrolment into Higher Education in Germany - Evidence from a 'Natural Experiment'. Journal of Applied Social Science Studies, 125/1 (together with Viktor Steiner).
(2003): Beiträge zur Bildungsdiskusssion (Editorial), Vierteljahreshefte zur Wirtschaftsforschung (2), (together with Viktor Steiner).

Discussion Papers:
(2006): Does more generous student aid increase enrolment rates into higher education? Evaluating the German student aid reform of 2001, DIW-DP no. 563 and IZA-DP no. 2034 (together with Viktor Steiner).
(2004): Are there any class size effects on early career earnings in West Germany?, DIW-DP no. 417, Berlin.
(2004): Enrolment into Higher Education and Changes in Repayment Obligations of Student Aid - Microeconometric Evidence for Germany, DIW-DP no. 444 (together with Viktor Steiner).

Policy Reports:
(2006 forthcoming) Evaluation der Maßnahmen zur Umsetzung der Vorschläge der Hart-Kommission: Existenzgründungen, second project report to the Federal Ministry of Economics and Labour (together with Marco Caliendo and Viktor Steiner).
(2005) Evaluation der Maßnahmen zur Umsetzung der Vorschläge der Hart-Kommission: Existenzgründungen, first project report to the Federal Ministry of Economics and Labour (together with Marco Caliendo and Viktor Steiner).
(2005) Vergleich der Privatschulfinanzierung für den Bereich der allgemeinbildenden Schulen mit ausgewählten Bundesländern, project report to the Senatsverwaltung für Finanzen Berlin (together with Daniela Glocker, Christoph Muhle, Viktor Steiner).
(2005) Lehrerausstattung der allgemeinbildenden Schulen in ausgewählten Großstädten, project report to the Senatsverwaltung für Finanzen Berlin (together with Daniela Glocker, Christoph Muhle, Viktor Steiner).
(2003): Untersuchungen zu den Wirkungen der gegenwärtigen Ehegattenbesteuererung auf Grundlage von fortgeschriebenen Einzeldaten der Einkommensteuerstatistik, DIW Berlin Research Notes Nr. 27 (together with S. Bach, H. Buslei, D. Svindland, and J. Flach)

Other Publications:
(2006): Existenzgründungsförderung für Arbeitslose - Neuer Ergebnisse für Deutschland, DIW-Wochenbericht 7/2006, (together with Marco Caliendo and Viktor Steiner).
(2003): Kleinere Schulklassen: Wirklich nützlich? Für Westdeutschland zeigt sich kein Einfluss auf Einstiegsgehälter, DIW-Wochenbericht 42/2003.
(2001): Berliner Ausgaben für Wissenschaft und Forschung: Kräftige Impulse für die Stadt, DIW-Wochenbericht, no. 39 (together with Bernhard Seidel).
(2000): The Social Dimension to Globalisation: The Trade Union Response to the Global Economic and Financial Crises, TUAC-OECD (together with Peter Gaskell).

Conference Presentations:

3. - 5. Jul 06	GSOEP: Annual GSOEP User Conference: Berlin/Germany
2. - 4. Feb 06	SOEP/GSSS: Second International SOEP Young Scholars Symposium: Bremen / Germany
17. - 24. Aug 05	WCES (Econometric Society): London/UK - World Congress 2005
15. - 18. Jun 05	ESPE (European Society for Population Economics): Paris/France - XIX Annual Conference
28. Aug - 1. Oct 04	VfS (Verein für Socialpolitik): Dresden/Germany- Jahrestagung 2004
20.-24. Aug 04	EEA (Europen Economic Association): Madrid/Spain - 19th Annual Congress of the EEA
24.-26. Jun 04	GSOEP: Berlin/Germany - 6th International German Socio-Economic Panel User Conference
5.-7. Apr 04	RES (Royal Economic Society): Swansea/UK - Annual Conference 2004
18.-21. Sep 03	EALE (European Association of Labour Economists): Sevilla/Spain - 15. annual EALE Conference
8.-12. Sep 03	EEA (European Economic Association): London/UK (IFS) - XIVth Euro Summer School
7.-13. Apr 03	IZA: Buch(Ammersee)/Germany - 6th IZA European Summer School in Labor Economics

Conference Participations:

22.-23. Nov 02	Max Planck Institute for Human Development: Berlin - Overeducation in Europe: What do we know?
3.-4. Jul 02	GSOEP: Berlin - Annual GSOEP User Conference
26.-27. Apr 01	ARGE Berlin - Tagung: "Migration in Europa"
19.-20. Feb 01	Handelsblatt: Berlin - Standortforum Berlin: "Industriestandort und Standort für die New Economy?!"
14. Apr 00	ESRC & The Bank of England: London - Macro Conference: "The Future of Macroeconomics'"
10. Mai 99	FES : Bonn - G7/8 Summit Conference: "The Social Dimension to Globalisation"
30.-31. Okt 98	WSI Düsseldorf - "Perspektiven der Makropolitik zwischen Nationalstaat und Europäischer Union"
30. Okt 97	iea (The Institute of Economic Affiars): London - "Ninth Annual State of the International Economy Conference"

Relevant work experience:

Feb 99 - Jun 99	TUAC (OECD) in Paris: Trainee.
Nov 98 - Dec 98	The Deutscher Bundestag (German Parliament) in Bonn: Trainee at the Social Democrats' working group on economics and technology.
Jun 98 - Oct 98	Institute for Economic and Social Research (WSI) in Düsseldorf: Internship
Winter term 96/97	Tutor to the introductory course in economics by Prof. Dr. Harald Mattfeldt.
96 - 97	Research assistant (Studentische Hilfskraft) to Prof. Dr. Wolfgang Bessler.

Further work experience:

93 - 95	At the Deutsche Telekom I chaired the youth works council and was thus responsible for a fluctuating amount of trainees (350 - 450) in our office. Twice a year I gave the business report on the general meeting.
89 - 93	Apprenticeship as a telecommunication electrician at the Deutsche Telekom.

Skills and abilities:

Computer: Stata, Eviews, Limdep, LaTeX 2_ε, MS-Office, UNIX.
German: Mother tongue.
English: Professional level.
Through my studies in Great Britain.
French: Intermediate level.
Alliance Française (Certificat d'Etudes de Français Pratique 1er Degré).

Sep 04	Berlin Marathon 2004: Finisher! (4:07:44)
Sep 03	Berlin Marathon 2003: Finisher! (4:30:15)
Oct 97 - May 98	Vice President of the Golf Society Dundee.
96 - 97	Chairman of the student-finance-controlling-bureau.
96 - 97	Member of the student parliament.
Dec 95	I co-organised a one week conference at my university in Hamburg. The conference was on reform options in higher education.