IZA

DISCUSSION PAPER SERIES

IZA DP No. 2034

Does More Generous Student Aid Increase Enrolment Rates into Higher Education? Evaluating the German Student Aid Reform of 2001

Hans J. Baumgartner
Viktor Steiner

March 2006

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

Does More Generous Student Aid Increase Enrolment Rates into Higher Education? Evaluating the German Student Aid Reform of 2001

Hans J. Baumgartner
DIW Berlin

Viktor Steiner
Free University of Berlin, DIW Berlin and IZA Bonn

Discussion Paper No. 2034
March 2006

IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
Email: iza@iza.org

IZA Discussion Paper No. 2034
March 2006

ABSTRACT

Does More Generous Student Aid Increase Enrolment Rates into Higher Education? Evaluating the German Student Aid Reform of 2001*

Students from low-income families are eligible to student aid under the federal students' financial assistance scheme (BAfoeG) in Germany. We evaluate the effectiveness of a recent reform of student aid that substantially increased the amount received by eligible students to raise enrolment rates into tertiary education. We view this reform as a 'natural experiment' and apply the difference-in-difference methodology using a discrete-time hazard rate model to estimate the causal effect on enrolment rates into higher education. We find that the reform had a small positive but statistically insignificant effect on enrolment rates.

JEL Classification: H31, I28, I22, J24

Keywords: educational transitions, educational finance, natural experiment and difference-indifference estimation

Corresponding author:

Viktor Steiner
DIW Berlin
Königin-Luise-Straße 5
14195 Berlin
Germany
Email: vsteiner@diw.de

* We thank Gerard van den Berg and participants at several workshops and conferences for helpful discussions and comments on an earlier version of this paper. The usual disclaimer applies.

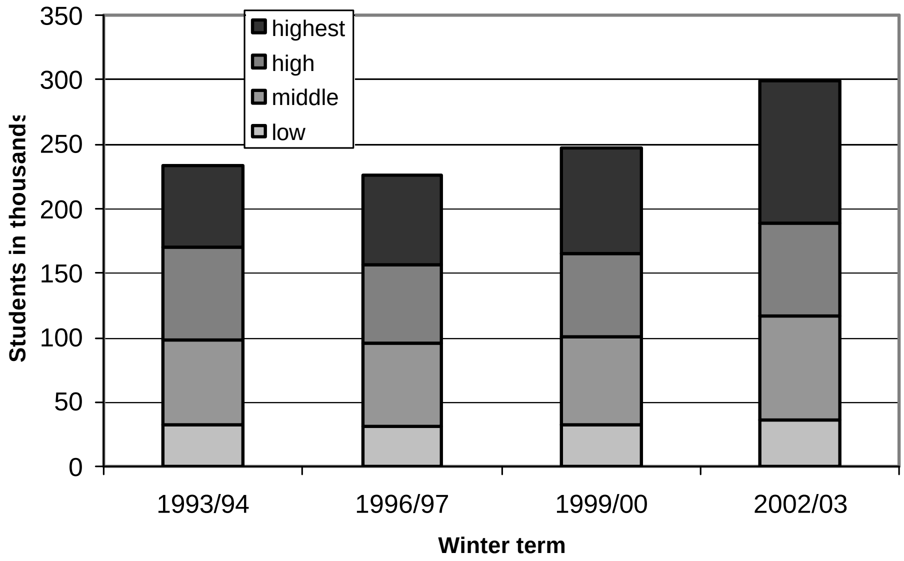
1 Introduction

In Germany, students from low-income families are eligible to student aid under the federal students' financial assistance scheme (*Berufsausbildungsfoerderungsgesetz*, BAfoeG) which covers a substantial share of the monthly living costs of students enrolled in higher education. In the education policy debate BAfoeG is typically rationalized by its supposedly positive effect on enrolment rates into higher education of youth from low income families. The standard argument usually given in support for this belief is the presence of credit constraints to finance higher education preventing potential students from low-income families to enter higher education. Another popular argument is that, even in the absence of such constraints, youth from low-income families would effectively be prevented from enrolling into higher education because they are more reluctant to incur debt than those with a more affluent parental background. Thus, financial assistance provided through student aid is widely considered to be not only necessary for guaranteeing 'equal opportunity' in access to higher education but also to increase the proportion of school leavers who pursue tertiary education.

Entering university education in Germany depends still very much on social origin. Figure 1 shows the development of freshmen students by social origin over the last decade, where a student's social origin is derived by combining information on parents' educational attainment and occupational status. The increase in enrolment over recent years is mainly driven by those students coming from higher and highest social origins, while the number of freshmen students from low social origin remained fairly stable over time. Since parents' educational attainment is a good proxy for parents' income which, in turn, is the major determinant of students' eligibility to financial aid to students, Figure 1 does not indicate that the 2001 BAfoeG reform has achieved the intended effect of increasing enrolment rates into higher education of students from relatively poor families.

Whether student aid is an effective policy instrument to raise enrolment rates into higher education is still a rather unexplored issue, not only in Germany but also internationally. Whereas there exist various studies on the effects of student aid on enrolment rates for the United States (e.g. Dynarski 2002, 2003; Kane 1995; Long 2003; Shea 2000) or Europe (e.g. Winter-Ebmer and Wirz 2002), this topic remains still rather unexplored for Germany. Lauer (2002) finds on the basis of a microeconometric choice model estimated on data from the German Socio Economic Panel (SOEP) that extending the entitlement to BAfoeG seems to be more effective in raising enrolment rates in higher education than increasing the BAfoeG amount received by the individual student entitled to this subsidy. Using a different methodological approach and the same data source Baumgartner and Steiner (2004; 2005) find that the BAfoeG reform of 1990, which changed student aid from a full loan system to a partial loan/grant system, had virtually no influence on enrolment rates into higher education.

Figure 1: Freshmen students by social origin in Germany, 1993 – 2003.



Source: Economic and Social Conditions of Student Life in the Federal Republic of Germany 2003 - 17th Social Survey; Federal Statistical Office Germany; own calculations.

In this paper we evaluate the effect of the change in the BAfoeG eligibility rules introduced in 2001 which increased the basic allowance on parent's income (*Freibetrag*) and the level of the defined maintenance need (*Bedarfssatz*) by roughly 20 percent and 6 percent, respectively. This change increased both the amount of student aid for those already eligible to BAfoeG before the reform and the number of eligible students. We consider this change as a 'natural experiment' to identify the 'causal' effect of this particular policy reform on enrolment rates in Germany. Using data from the SOEP, our estimation results from a microeconometric transition rate model based on the difference-in-difference methodology shows that the BafoeG reform of 2001 seems to have been ineffective in raising enrolment rates into higher education. This somewhat counter-intuitive result may have important implications for the current policy debate on how to finance and to secure access to higher education in Germany and elsewhere.

The remainder of this paper proceeds as follows. In the next section, we describe the German system of student aid and the BAfoeG reform of 2001 analyzed in this paper. In section 3 we describe our sample design and data, and present our empirical method to estimate the effects of this reform on enrolment rates into higher education. In section 4 we summarize our empirical findings and present some robustness checks. Section 5 summarises the main results of our study and concludes.

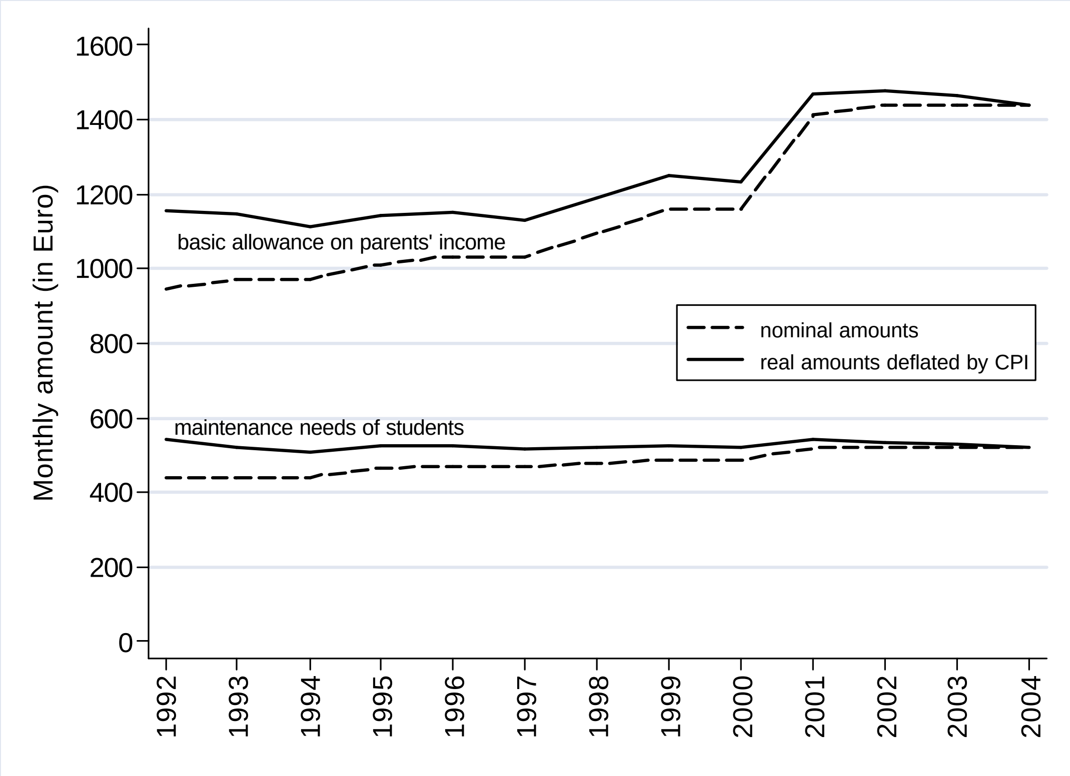
2 The Structure of Student Aid and the 2001 BAfoeG Reform

In order to provide some institutional background for the following empirical analysis, we start with a brief description of student aid in Germany, which is regulated under the German federal financial assistance scheme to promote education (*Bundesausbildungsfoerderungge-setz,* BAfoeG). When introduced in 1971, this law was meant to allow all qualified young people to enter university regardless of their parent's financial capacity. BAfoeG was changed several times since then, and today subsidises also pupils in secondary education, further education, and also those enrolled in vocational training courses. The main political goal of BAfoeG remains, however, to encourage students from low-income families to pursue higher education.

Student aid is means tested, i.e. depends on the financial capacity of parents or of her/his spouse in case of married students. BAfoeG defines a maintenance need (*Bedarfssatz*) that depends on whether the student lives with her parents and possibly also on the costs for health care insurance for the student. To be eligible for student aid this pre-defined maintenance need has to exceed parents' financial capacity which is the sum of household incomes from various sources, reduced by the income tax paid in the previous year, a lump sum for social security contributions, and basic allowances that depend on family status as well as any alimony obligations for other children (for a more detailed description, see the appendix). Currently, a share of about 50 percent of student aid is granted as an interest free loan repayable from future earnings after graduation, the other half is provided as a non-repayable grant.

The BAfoeG eligibility rules were changed on 1st April 2001. The basic allowance on parents' income was increased by about 20 percent and the amount of maintenance needs by about 6 percent, outpacing the increase in living costs. Figure 2 depicts the development of these two main determinants for the eligibility and the amount of student aid. The dashed lines show the nominal amounts as defined by the law, the solid lines show the respective amounts deflated by the consumer price index (in 2004 prices). The figure shows that in the early 1990's the nominal increase of the basic allowance was outpaced by price inflation. The resulting decline in the real amount of the basic allowance and its subsequent stagnation was followed by a modest rise in the late 1990's and a marked increase by about 20 percent in 2001 due to the BAfoeG reform in this year. The amount of maintenance needs remained more or less stable in real terms throughout the 1990's and slightly increased (by some 6 percent) in 2001 due to the BAfoeG reform.

Figure 2: Real and nominal free allowance on parents' income and maintenance needs of students

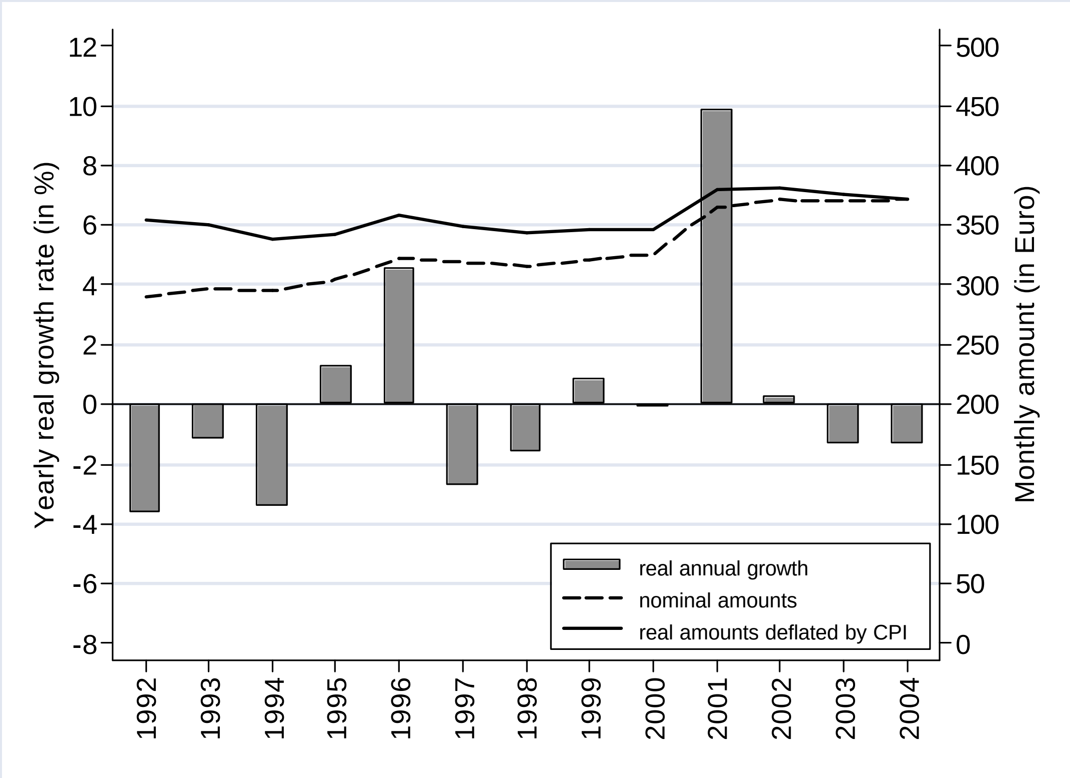


Source: Report of the Student Aid Council (*Bericht des Beirats für Ausbildungsförderung*); various years; own calculation.

As shown in Figure 3, the changes in the eligibility rules due to the BAfoeG reform resulted in an increase in the average amount of student aid, measured in real terms, by more than 10 percent in 2001. Between 2000 and 2002 the average monthly amount of student aid increased form 326 to 371 Euro, and the number of students receiving BAfoeG increased substantially by about 100 thousand to roughly 450 thousand people. Due to this reform, the fiscal costs have increased by about 50 percent to 1.35 billion Euro in this period.[1] To some extent, this strong increase compensated for the decline of the real value of student aid during the second half of the 1990s, as shown by the solid line in Figure 3. Starting in 2002, in real terms the amount of student aid has been declining in three consecutive years.

[1] German Federal Statistical Office (2003); Fachserie 11 Reihe 7.

Figure 3: Nominal and real average amounts of student aid (Euro per month) and changes over time (yearly real growth rate, in percent)



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

3 Empirical Methodology

3.1 Identification

There are two potential endogeneity issues encountered in the estimation of the effect of the BAfoeG reform on enrolment rates into higher education. First, in our data we only observe eligibility to BAfoeG for those individuals who actually decided to enrol into higher education. That is, there is no direct information whether an individual with the formal entrance requirement for university education would have been eligible for BAfoeG if she had decided to study. Obviously, we therefore cannot directly use the information whether a student receives BAfoeG or not to construct an indicator variable for inclusion in a regression model explaining enrolment into higher education. Second, even if we could observe BAfoeG eligibility for all potential students including such an indicator as an explanatory variable in a regression model could still cause the usual endogeneity problem related to the correlation of both this variable and the decision to enrol into higher education with some unobserved individual effects. One way to deal with this problem would be to estimate a selection model in the spirit of Heckman (1979). Another possibility, is to instrument the BAfoeG eligibility variable by some exogenous variable. Such variables are typically hard to find, but in certain circumstances may be provided by a 'natural experiment', i.e. an exogenous policy change affecting individual participation in some program but, by its nature being outside the control of the individual, having no direct effect on the outcome variable of interest (see, e.g., Angrist and Krueger 1999, Blundell and Costa Dias 2000).

In this paper, we interpret the BAfoeG reform of 2001 described in the previous section as a 'natural experiment' which introduces an exogenous variation in the entitlement to student aid which we use to identify the 'causal' effect of this particular reform on enrolment rates into higher education. This exogenous variation derives from the change in entitlement rules due to the BAfoeG reform in 2001. This reform only affected potential students whose parents' income remained below certain thresholds (the treatment group), as described above, and not those above these thresholds (the control group). In order to identify the causal effect of the reform on those affected by it, the following conditions have to be fulfilled[2]:

First, parents of potential students must not adjust their behavior in expectation of the reform in such a way that their children become entitled to BAfoeG. This could occur, for example, if parents reduced their labor supply so that their income would fall below the threshold which defines the treatment group. Although such behavior cannot be excluded a priori, especially not for those on the margin, to us it seems rather unlikely for the great majority of the relevant population.

Second, the reform must not have been anticipated by students when deciding on enrolment into higher education. For the following reasons, we believe this to be the case. In Germany, the winter term starts for all universities on 1st October. Due to the central study place allocation agency (*Zentrale Studienplatzvergabe*, *ZVS*) and internal regulations of the universities, students have to apply in almost all circumstances not later than 15th July to commence studying the coming winter term. Plans to use the revenues from the sold UMTS-licences to finance the BAfoeG reform were first publicly revealed in October 2000, followed by the passing of the law in the German parliament in February 2001 which came into effect on 1st April 2001, when the new summer term started. Since the political debate to reform BAfoeG started in the same month (October) when the winter term began, it seems very

2 Assume the regression model $y_{it} = a_0 + b'X_{it} + c_1GD_{it} + c_2TD_{it} + d(GD \times TD)_{it} + u_{it}$, with GD=1: individual i belongs to the treatment group, GD=0: control group, TD=1: observation period t belongs to the post-reform period, TD=0: pre-reform period, GD×TD := interaction term; X is a vector of exogenous variables, a_0, b', c_1, and d are (vectors of) regression parameters, and u is an error term. In an OLS regression the parameter d identifies the causal effect of the reform on those affected by it ("treatment effect on the treated") if: $E(u_{it} \mid X, GD = 1, TD=1) - E(u_{it} \mid X, GD = 1, TD=0) := \Delta E(u_{it} \mid X, GD=1) = \Delta E(u_{it} \mid X, GD=0)$. That is, conditional on X, the expected change of error terms between the pre-reform and post-reform periods in the two groups must be balanced. This assumption would be violated if either of the three conditions discussed in the text were not fulfilled.

unlikely that students could postpone enrolment to the summer term in order to take advantage of the more generous student aid regulations right from their first semester.

The third condition is that, conditional on observable variables, enrolment rates of the treatment and the control group would not have developed differently in the absence of the BAfoeG reform. That is, we have to assume that time trends do not differ between the two groups (common trend assumption). This assumption cannot really be tested under the alternative hypothesis of a non-zero causal effect of the reform on enrolment rates but, as we show below, informal checks suggest that this assumption may hold empirically.

Given these conditions are met, the causal effect of the BAfoeG reform on enrolment into higher education could, in principle, be estimated by applying the simple difference-in-difference (DD) estimator to grouped data. The DD-estimator identifies the average effect of the reform on the enrolment rate of people affected by the reform, i.e. the "average treatment effect on the treated" (see, e.g., Angrist and Krueger 1999, Blundell and Costa Dias 2000). The simplest way to estimate this effect would be to take the differences in mean enrolment rates of students eligible to BAfoeG (treatment group) and of students not eligible to BAfoeG and thus not affected by the reform (control group), both before and after the reform, and then take the difference of these two differences. By aggregating the data this procedure would, however, yield inefficient estimates of the average treatment effect. Furthermore, this simple estimator cannot account for changes in the composition of the treatment and control groups due to sample attrition and right-censoring of observations related to our sampling design. As described in the next section, given our sample design it is essential to account for this in the econometric model.

3.2 Data and Sample Design

Our empirical analysis is based on the German Socio-Economic Panel Study (SOEP). This is a longitudinal survey of individuals living in private households in Germany covering each year since 1984.[3] We restrict the sample to people with completed upper secondary schooling, since only those are entitled to enrol into higher education in Germany. Our focus is on the transition from upper secondary schooling into higher education. Since, for various reasons[4], a large share of all school leavers who eventually enrol into higher education do so only after some time has elapsed, we allow for a transition period of up to three years after completion

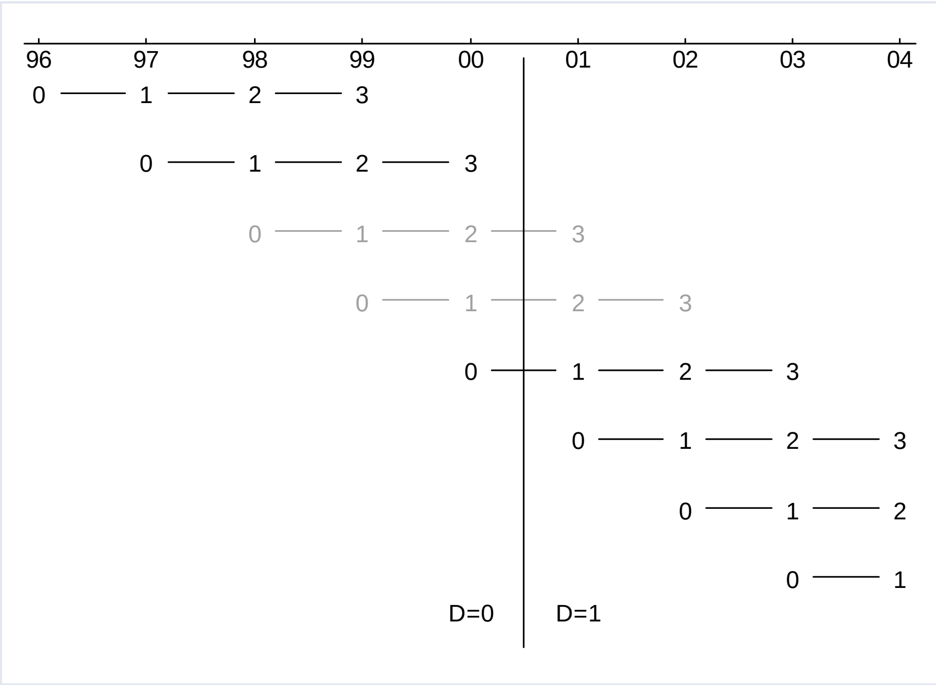
---

[3] We use the waves between 1992 and 2004 for our analysis. Haisken-DeNew and Frick (2001) provide detailed information on the SOEP data.

[4] These reasons include, among others, military service between 10 and 18 month depending on the recruitment year, vocational training of up to three years, and the existence of so called *'Wartesemester'* (waiting terms) for obtaining a place of study in some subjects (like business studies, law, and medicine) allocated by the *Zentrale Studienplatzvergabestelle* (central study place allocation agency).

of upper secondary school. For the post-reform period, the three-years' transition period ends in 2004, the year for which the most recent wave of the SOEP is available. Since we want to define the pre-reform period as closely as possible before the policy change took place and, at the same time, avoid a school-leaver cohort's transition period to overlap with the post-reform period, we only include the school leaving cohorts 1996 and 1997 in the pre-reform sample for our basic estimation (see Figure 4). The post-reform sample includes the school-leaving cohorts 2000, 2001, 2002, and 2003, where among the latter two cohorts a substantial share of all observations is right-censored at the time the observations window ends. In alternative specifications of our transition model estimated below, we also include additional cohorts by, respectively, extending the pre-reform period and including the two 'ambiguous' school-leaving cohorts of 1998 and 1999 whose transition period overlaps both the pre-reform and post-reform period (see Figure 4).

Figure 4: Sample design



Note: "0" indicates the school leaving year; "1", "2", and "3" indicates, respectively, the first, second, and third year after schooling. Light numbers indicate 'ambiguous' school leaving cohorts, as defined in the text. If a school leaver has not registered with an university within three years, she is counted as a non-student.

The derivation of individual school-to-university transitions from information contained in the SOEP is described in the appendix. Our sample contains 775 individuals with the required entrance qualification for tertiary education who completed upper secondary school between 1996 and 2003. We had to discard 243 observations because of information on school leavers' parents required for the simulation of BAfoeG eligibility was completely missing in the

SOEP.[5] 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 termination of the observation period in 2004. As mentioned above, for our basic specification we exclude the two 'ambiguous' school-leaving cohorts 1998 and 1999 (142 school leavers), which leaves us with a sample of 456 school leavers, of whom 396 are uncensored, and a total number of panel observations (number of individuals times average number of years observed in the panel) of 798 for our basic estimation. Descriptive sample statistics and information on the variables included in the transition rate model are given in Table 1 below, where we distinguish between school leavers eligible (treatment group) and those not eligible to BAfoeG (control group).

Table 1 shows that roughly 50% of all school leavers enrolled into higher education within 3 years after having completed upper secondary schooling, about 22% made a transition to some other state (e.g., employment, apprenticeship training, unemployment), and 28% are right-censored due to either sample attrition or the termination of the observation period in 2004. Enrolment amongst ineligible school leavers is about 8 percentage points higher than amongst those eligible for BAfoeG. Excluding censored observations from the sample shows a much larger increase in the average enrolment rate after the BAfoeG reform for school leavers entitled to student aid – from 47% to about 83% – than for those not entitled to BafoeG (from roughly 60 to 80%). However, note that the share of censored observations among school leavers entitled to BAfoeG is considerably larger than among those not entitled to student aid.

As shown in the table, the treatment of right-censored observations strongly affects the calculation of the enrolment rate for the two groups. Treating all right-censored observations as if school leavers did not enrol in university education within the three years' observation window would lead to an estimated increase of the enrolment rate by about 5 percentage points for the treatment group and a decrease by about 10 percent for the control group. On the other hand, assuming all right-censored observations eventually enrolled in tertiary education would almost double the enrolment rate for the treatment group between the two periods, and also lead to a very strong increase for the control group. Of course, there is no reason to believe that the BAfoeG reform has either reduced the enrolment rate for the control group or that it has almost doubled the rate for the treatment group, nor can such changes be observed in empirical enrolment rates (see Figure 1). For the consistent estimation of the

5 We do not expect this to lead to any selection problem because it seems unlikely that missing information on parents is related to the students' decision to enrol into higher education.

effect of the BAfoeG reform on the enrolment rate into higher education it seems therefore essential to account for censoring in a coherent way, as described in the next section.

Turning to the explanatory variables included in the model, Table 1 shows that the simulated share of students eligible to student aid (would they choose to study) among all potential students amounts to 45.6 percent in the sample. Of course, whether an individual actually receives BAfoeG is only observed for students and not for those who decided not to enrol into tertiary education, even though these individuals might have been eligible to student aid had they decided to enrol into higher education. We use a simple microsimulation model, briefly described in the appendix, to determine potential BAfoeG eligibility for each individual in our sample and for each year within the observation period. As mentioned in section 2 above, eligibility to student aid mainly depends on parents' financial capacity relative to the student's maintenance needs. In the calculation of potential BAfoeG eligibility we take all relevant income information of the student and her/his parents available in the SOEP as well as the relevant living circumstances of the parents and the student into account.

Table 1 also reveals some marked differences between the treatment and control group regarding parental educational background, father's occupational status, and nationality. For example, the share of eligible students with a self-employed father is only 7.7 percent, whereas for ineligible students the respective share amounts to 21.8 percent. We also observe more students with parents who completed upper secondary schooling among ineligible students. Of course, these differences mainly reflect the positive correlation of household income, which determines BAfoeG eligibility to a large extent, and parents' educational background or occupational status.

Table 1: Descriptive statistics

	Sample[a)]	Treatment	Control
Dependent variable			
observed in both periods (before and after)			
higher education = 0	0.224	0.202	0.242
higher education = 1	0.491	0.447	0.528
right-censored [b)]	0.285	0.351	0.230
before (D=0)			
higher education = 0	0.151	0.130	0.169
higher education = 1	0.200	0.115	0.270
right-censored [b)]	0.031	0.034	0.028
after (D=1)			
higher education = 0	0.072	0.072	0.073
higher education = 1	0.292	0.332	0.258
right-censored [b)]	0.254	0.317	0.202
Covariates			
after	0.618	0.721	0.532
eligible to BafoeG [c)]	0.456	1.000	0.000
after $\times$ eligible to BAfoeG	0.329	0.721	0.000
father self employed	0.154	0.077	0.218
father white collar	0.404	0.389	0.415
father civil servant	0.134	0.115	0.149
father out of labour force	0.070	0.101	0.044
male	0.458	0.433	0.480
abitur	0.825	0.798	0.847
school leaving age	19.421	19.351	19.480
	(1.109)	(1.234)	(0.990)
father completed upper secondary schooling	0.346	0.245	0.431
mother completed upper secondary schooling	0.285	0.240	0.323
German nationality	0.923	0.880	0.960
East Germany	0.292	0.341	0.250
number of school leavers (basic sample)	456	208	248
number of observations	798	347	451
average number of observations/person	1.75	1.67	1.82

Notes:

a) refers to the sample used for our basic specification (see text)

b) right-censored due to incomplete observation window or sample attrition

c) simulation of BafoeG eligibility as described in the text

3.3 Econometric Specification

We implement the DD estimator in a discrete-time hazard rate model which is the natural framework to analyze transition rates into higher education and also allows us to take into account the relatively large number of right-censoring in our data due to sample design and attrition. The observed variable statistically related to these transition rates is the duration between graduation from upper secondary school and enrolment into higher education. Since this duration is measured in years, we model the transition from completed upper secondary education into tertiary education in terms of a discrete-time hazard rate model. The duration of this process 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 will be one calendar year in our empirical analysis, this variable takes on a value of $T = t$. The hazard rate for individual *i*, $l_i(t)$, is the conditional probability of a transition into higher education in year *t*, given that no transition has occurred until the beginning of *t* :

(1) $$l_i\left(t \mid \mathbf{X}_{it}, e^m\right) = P\left[T_i = t \mid T_i \geq t, \mathbf{X}_{it}, e^m\right]$$

with i = 1,2,...n

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

e^m = time-invariant individual effect

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

(2) $$E(e\ ) = \sum_{m=1}^{M} \Pr\left(e^m\right) e^m = 0$$
$$\sum_{m=1}^{M} \Pr\left(e^m\right) = 1$$
$$E\left(e^m \mathbf{X}_{it}\right) = 0$$

with *M*-1 individual effects ("mass points") and their respective probabilities to be estimated, and the M^{th} individual effect and its corresponding probability derived from the equalities given in equation (2).

The survivor function, which gives the (unconditional) probability of not having enrolled into higher education up to period t, can be written as

(3) $$P\left(T_i > t \mid \mathbf{X}_{it}, e^m\right) = S\left(t \mid \mathbf{X}_{it}, e^m\right) = \prod_{t=1}^{t-1}\left[1 - l\left(t \mid \mathbf{X}_{it}, e^m\right)\right]$$

In terms of the hazard rate and the survivor function, the probability of a transition into higher education in period *t* is given by

$$(4) \qquad P\left(T_i = t \mid \mathbf{X}_{it}, e^m\right) = l\ \left(t \mid \mathbf{X}_{it}, e^m\right) \prod_{t=1}^{t-1} \left[1 - l\ \left(t \mid \mathbf{X}_{it}, e^m\right)\right]$$

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

$$(5) \qquad L = \prod_{i=1}^{n} \left\{ \sum_{m=1}^{M} \left[\Pr\left(e^m\right)\right] \left[l_i\left(t_i \mid \mathbf{X}_{it}, e^m\right)\right]^{d_i} \prod_{t=1}^{t_i - 1} \left[1 - l_i\left(t \mid \mathbf{X}_{it}, e^m\right)\right] \right\}$$

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

Hence, for a person with an observed transition into higher education the contribution to the likelihood function is given by the respective transition probability in equation (4), for a censored spell it is given by the survivor function in equation (3), both written in terms of the hazard rate. Note that the survivor function not only provides information on individuals right-censored at the end of the observation period, but also for those who left the panel due to sample attrition.

It remains to specify a functional form for the hazard rate, for which we assume the logit specification, i.e.:

$$(6) \qquad l\ \left(t \mid \mathbf{T}_{it}, \mathbf{DD}_{it}, \mathbf{Z}_{it}, e^m\right) = \frac{\exp\left(g\,\mathbf{T}_{it} + a\,\mathbf{DD}_{it} + d\,\mathbf{Z}_{it} + e^m\right)}{1 + \exp\left(g\,\mathbf{T}_{it} + a\,\mathbf{DD}_{it} + d\,\mathbf{Z}_{it} + e^m\right)},$$

where the vector **T** includes the baseline dummies to account for duration effects on the hazard rate, the vector **Z** includes possibly time-varying exogenous variables to control for the parental background and students individual characteristics, such as nationality, and region of residence. The vector **DD** includes a time dummy for the post-reform period (equal to one after 2001, and zero in earlier periods), a group dummy for potential students affected by the reform (equal to one for those affected, and zero otherwise) and an interaction term between this group dummy and the post-reform dummy. The coefficient of this interaction dummy thus identifies the effect of the BAfoeG reform on the enrolment rate into higher education of those affected by the reform (the ‘treatment effect on the treated’) under the assumptions stated above.

Plugging the hazard rate (6) into the likelihood function (5), 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 optimization procedures.[6]

4 Estimation Results

Column (1) of Table 2 shows results for the sample of our basic specification as described in section 3.2, columns (2) – (4) summarize the estimation results for alternative sample definitions, which serves to check for potential sensitivity of estimation results for our basic specification.

Before we turn to the estimated effect of the BAfoeG reform on the hazard rate into higher education, we briefly summarize our estimation results in general. Two mass points account for unobserved heterogeneity in transitions. We conclude that unobserved heterogeneity is present in our data, since the maximum log-likelihood in the model including two mass points improves significantly versus a transition rate model not controlling for unobserved heterogeneity.[7]

Estimated coefficients of the baseline dummies, which are to be interpreted relative to the baseline rate in the first year after having completed upper-secondary schooling as the reference category, show that the transition rate into tertiary education increases in the second year and returns to its initial level in the third year.

As to the other control variables, we find that fathers' occupation, parents' education and having a German nationality do not significantly affect transition rates into tertiary education. At a first glance, this may seem surprising since school attainment is known to be positively correlated with parents' education and negatively with foreign nationality in Germany (see, e.g., Gang and Zimmermann 2000). However, since we are modelling transition rates conditional on having completed upper-secondary education rather than just the probability of enrolment in tertiary education, we are effectively analysing the last stage in an individual's educational attainment process. That is, parents' education may affect their children's attainment in earlier stages but does not seem to affect transition from upper-secondary schooling into tertiary education. Similarly, foreign nationality seems to negatively

6 The *gllamm* programme as implemented in Stata 8 is used for the estimation. For a description and technical details see Skrondal and Rabe-Hesketh (2004).

7 Evaluated by the Akaike Information Crterion (AIC) which simply compares the improvement of the maximum log-likelihood in the restricted and unrestricted model, adjusted for degrees of freedom. The restricted model yielded a log-likelihood of -425.99, compared to a value of –413.81 for the unrestricted model with one estimated mass point (see the first column of Table 2).

affect educational attainment only at lower levels of the educational attainment process.

As mentioned above, the effect of the 2001 BAfoeG reform on enrolment rates into higher education is estimated by the coefficient on the interaction term between the group and the post-reform dummy. Although this effect is estimated to be positive, as expected, it is neither statistically significant nor is the point estimate economically very important. If it were significant, this point estimate would imply that the average transition rate of those affected by the reform increased by 1.5 percentage points. This would mean that, due to the BAfoeG reform, the average transition rate into tertiary education of those eligible for BAfoeG increased from about 64 percent (see Table A1) to about 65.5 percent. At best, the 2001 BAfoeG reform seems to have contributed relatively little to the strong overall increase of enrolment rates into higher education.

The statistical insignificance of the estimated treatment effect may derive from the inefficiency of the DD estimator in conjunction with the relatively small number of observations in our sample. To check whether the estimated treatment effect would become more significant if we increased the number of observations, we have extended the pre-reform period and included all school leaving cohorts since 1992 available in the SOEP data in our sample. Estimation results for this alternative specification of the transition model are reported in column (2) of Table 2. Although the estimated coefficient of the treatment variable turns negative in sign and, in absolute value, becomes quite large, it remains insignificantly different from zero. This indicates that enrolment into higher education of older school leaving cohorts differs from that of the more recent pre-reform cohorts, which could be related to the development of student aid in real terms as described in section 2. We thus conclude that restricting our sample to the school leaving cohorts 1996 and 1997 is preferable to including older school leaving cohort as well, even if this leaves us with a considerably smaller number of observations.

Table 2: Transition rates into tertiary education – Random-effects logit model

	(1)	(2)	(3)	(4)
		Robustness tests		
	Base model	Longer pre-reform period	Alternative post-reform definition	Including 'ambiguous' cohorts
constant	-26.709	-26.191	-27.809	-24.026
	(4.285)***	(3.218)***	(4.499)***	(3.907)***
baseline hazard:				
year 2	1.430	1.108	1.591	1.156
	(0.375)***	(0.274)***	(0.552)***	(0.317)***
year 3	0.472	0.105	0.718	0.402
	(0.671)	(0.459)	(0.912)	(0.573)
difference-in-difference:				
after	-0.121	0.076	0.353	0.066
	(0.388)	(0.328)	(0.522)	(0.328)
eligible for BAfoeG	-0.556	-0.229	-0.504	-0.081
	(0.541)	(0.325)	(0.510)	(0.390)
after x eligible for BAfoeG	0.096	-0.204	-0.193	-0.479
	(0.642)	(0.462)	(0.720)	(0.507)
covariates:				
father self-employed	0.371	0.677	0.413	0.199
	(0.502)	(0.441)	(0.525)	(0.437)
father white collar	0.590	0.499	0.620	0.350
	(0.488)	(0.346)	(0.530)	(0.406)
father civil servant	0.955	1.049	1.016	0.969
	(0.571)*	(0.456)**	(0.623)	(0.480)**
father out of labour force	0.339	0.657	0.191	-0.327
	(0.686)	(0.551)	(0.732)	(0.637)
male	-2.091	-2.038	-2.370	-2.000
	(0.472)***	(0.369)***	(0.758)***	(0.418)***
abitur	1.973	2.030	1.572	1.862
	(0.633)***	(0.434)***	(0.966)	(0.495)***
school leaving age	1.211	1.151	1.272	1.106
	(0.205)***	(0.157)***	(0.222)***	(0.191)***
father completed upper secondary schooling	-0.522	-0.136	-0.468	-0.520
	(0.406)	(0.321)	(0.429)	(0.341)
mother completed upper secondary schooling	0.689	0.414	0.716	0.933
	(0.367)*	(0.303)	(0.391)*	(0.328)***
German nationality	-1.116	-0.731	-1.149	-1.668
	(0.656)*	(0.428)*	(0.692)*	(0.753)**
East Germany	0.862	1.026	0.937	0.912
	(0.389)**	(0.308)***	(0.427)**	(0.341)***
mass points:				
ε_1	-4.179	-3.837	-4.141	-3.905
	(0.551)***	(0.405)***	(0.568)***	(0.481)***
ε_2	*2,649*	*2,658*	*2,878*	*2,668*
prob(ε_1)	-0.456	-0.367	-0.364	-0.381
	(0.157)***	(0.118)***	(0.195)*	(0.135)***
	[0,388]	[0,409]	[0,410]	[0,406]
prob(ε_2)	*0,612*	*0,591*	*0,590*	*0,594*
N	456	691	456	598
NT	798	1309	798	1083
log-likelihood	-413.814	-662.461	-413.623	-563.940

Notes:

a) Robust standard errors are in parentheses

b) * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

c) The table shows the log odds of prob(ε_1) and the corresponding standard error. The probability is reported in square brackets and calculated by exp(prob(ε_1))/(1+exp(prob(ε_1))). ε_2 and prob(ε_2) is calculated to satisfy the properties of equation (2) given above.

d) Data source: SOEP 1992-2004.

In specification (3) we have enlarged our sample by including ‘ambiguous’ cohorts, i.e. the school leaving cohorts of 1998 and 1999, which completed schooling just before the policy change and whose observed transition window overlaps with the post-reform period. In this specification, the estimated coefficient of the treatment variable again becomes negative in sign and more than doubles in size compared to specification (2), but remains statistically insignificant. This result also indicates that the smaller sample used in our basic estimation is preferable to including also ‘ambiguous’ cohorts in the estimation.

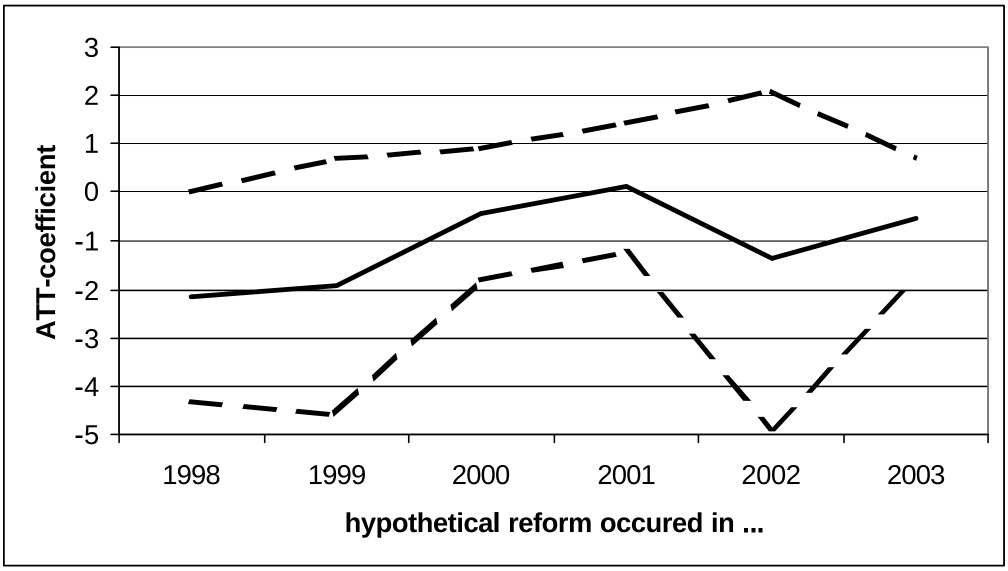
Another specification check we have performed is to estimate the transition model on a sample with the post-reform period defined according to the school leaving year instead of the enrolment year. This assumes that the policy change has an impact on school leaving cohorts instead of enrolment cohorts. The average treatment effect on the sample with an alternative post-reform definition becomes negative again, but remains insignificant. This might be due to the partially false definition, i.e. the school leaving cohort of 2000, for instance, is now defined to belong to the pre-reform period, despite the fact that most of them enrol after the policy change.

Overall, these tests suggest that our preferred specification – as presented in the first column of Table 2 – produces the most reliable results, because the sample used in the estimation includes only people who left school as close as possible around the policy change, excludes ‘ambiguous’ cohorts and avoids confounding school leaving and enrolment cohorts.

Finally, we have tried to check the plausibility of the key identifying assumption of our model, namely that enrolment rates of the treatment and the control group would not have developed differently in the absence of the BAfoeG reform. One way to check this assumption is to test for group differences in enrolment rates around arbitrary years, where we know that the policy has not changed. Figure 4 depicts the coefficient on the interaction term and the corresponding confidence interval. The specification is as in the first column of Table 2 with the exception that the post-reform dummy variable is redefined. In 1998, For instance, in 1998 the dummy variable *after* takes on the value of unity in that year and thereafter, while it is zero in the years before 1998. This way, we have shifted the years of the hypothetical policy change between the years 1998 and 2003. The estimate for 2001, the year when the policy actually changed, depicts the coefficient and the corresponding confidence interval as given in the first column of table 2. Figure 4 shows that, except for the year 2001, estimated coefficients of this interaction term are all negative, although statistically insignificant in all cases.

Another way to check the plausibility of the key identifying assumption is to test the weaker assumption that both groups have been following a common time trend in the pre-reform period. Allowing for a transition window of three years after completing upper secondary schooling, we can use the pre-reform period 1992 – 1997 to test whether time trends differ between the two groups. Estimating alternative specifications of our transition model with linear, quadratic, and non-parametric (with a dummy-variable for each year) time trends we could in no case reject the null hypothesis that enrolment rates of both groups follow a common time trend in the pre-reform period (see Table A1 in the appendix). Of course, this result does not prove that our key identifying assumption holds. However, the fact that enrolment rates of the two groups developed in a similar way in the pre-reform period lends, in our opinion, some credibility to this assumption.

Figure 4: Pre-program test



Note: The graph depicts the estimated coefficient of the interaction term between the treatment group dummy and a time dummy for alternative hypothetical reform years together with 95% confidence bands.

5 Summary and Conclusion

We have analyzed the effect of a change in student aid introduced by the BAfoeG reform of 2001 on enrolment rates into higher education in Germany. This reform extended means-tested student aid to households previously not eligible to BAfoeG and increased the average amount of student aid by about ten percent. An important aim of this reform was to encourage more students from low-income households to pursue higher education by providing more generous student aid. We have used the supposedly exogenous variation in student aid induced by this 'natural experiment' to identify the causal effect of this policy reform on enrolment rates into higher education.

Our estimation results show that this reform was ineffective in increasing enrolment rates of eligible students. One reason for this result may be the inefficiency of the DD method applied to identify the effect of the reform in conjunction with the relatively small number of observations in the SOEP. However, the point estimate of the treatment effect in our preferred specification is relatively small in size, implying that the BAfoeG reform, even if the estimated effect were statistically significant, increased the enrolment rate into higher education by only about 1.5 percentage points. In the absence of larger informative data sets for Germany, which would allow us to identify potential BAfoeG eligibility for both students and school leavers who choose not to study, we cannot really tell to what extent the insignificance of the estimated treatment effect is due to small sample size. In future research, more efficient estimates of the treatment effect could perhaps be obtained on the basis of a more structural microeconometric selection model using the same data set as in this study, although at the cost of arguably somewhat more restrictive identifying assumptions. This would also allow us to explore differences in the effects of the reform across the population, i.e. heterogeneous treatment effects, and possibly identify groups of school leavers for whom the reform might have been effective.

An alternative explanation for the insignificance of the estimated treatment effect is that our basic identifying assumption of a common time trend for the treatment and control groups might be invalid. Although we did not find any evidence indicating violation of this assumption for the school leaving cohorts of 1992 – 1997 on the basis of our specification checks, we cannot directly test the validity of this assumption. For the moment, we therefore interpret our empirical results as being indicative for the ineffectiveness of the recent BafoeG reform in raising enrolment rates into higher education in Germany.

Nevertheless, the BAfoeG reform could have had other beneficial effects on students. For one thing, the reform halted the long-term decline in the real value of student aid and the share of eligible people due to the failure to index basic allowances and maintenance needs to the increase in the cost of living during the 1990's. The reform may also have contributed to reducing the duration to study, since students can more easily avoid working beside 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 the analysis of this topic remains for further research.

References

Angrist, Joshua D. and Alan B. Krueger. 1999. "Empirical Strategies in Labor Economics," in *Handbook of Labor Economics*. Orley Ashenfelter and David Card eds. North-Holland: Elsevier, pp. 1277-366.

Baumgartner, Hans J. and Viktor Steiner. 2004. "Enrolment into Higher Education and Changes in Repayment Obligations of Student Aid: Microeconometric Evidence for Germany." *DIW Discussion Papers Series*:444.

Baumgartner, Hans J. and Viktor Steiner. 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, pp. 29-38.

Blundell, Richard and M. Costa Dias. 2000. "Evaluation Methods for Non-Experimental Data." *Fiscal Studies*, 21:4, pp. 427-68.

Dynarski, Susan M. 2002. "The Behavioral and Distributional Implications of Aid for College." *American Economic Review*, 92:2, pp. 279-85.

Dynarski, Susan M. 2003. "Does Aid Matter? Measuring the Effect of Student Aid on College Attendance and Completion." *American Economic Review*, 93:1, pp. 279-88.

Gang, Ira N. and Klaus F. Zimmermann. 2000. "Is Child like Parent? Educational Attainment and Ethnic Origin." *Journal of Human Resources*, 35, pp. 550-69.

Haisken-DeNew, John P. and Joachim R. Frick eds. 2001. *Desktop Companion to the German Socio-Economic Panel*. Berlin: DIW-SOEP.

Heckman, James J. 1979. "Sample Selection Bias as a Specification Error." *Econometrica*, 47:1, pp. 153-61.

Heckman, James J. and Burton Singer (1984), "Econometric Duration Analysis." Journal of Econometrics, 24, pp. 63-132.

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

Long, Bridget Terry. 2003. *The Impact of Federal Tax Credits for Higher Education Expenses*: National Bureau of Economic Research Inc NBER Working Papers: 9553.

Meyer, Bruce D. 1995. "Natural and Quasi-experiments in Economics." *Journal of Business and Economic Statistics*, 13:2, pp. 151-61.

Scrondal, A. and S. Rabe-Hesketh. 2004. Generalized Latent Variable Modeling. Chapman and Hall, London.

Shea, John. 2000. "Does Parents' Money Matter?" *Journal of Public Economics*, 77:2, pp. 155-84.

Winter-Ebmer, Rudolf and Aniela Wirz. 2002. "Public Funding and Enrolment into Higher Education in Europe." *IZA Discussion Paper Series*:503.

Appendix

Derivation of School-to-University Transitions in the German Socio-Economic Panel Study (SOEP)

Individual transitions between secondary and tertiary education are derived by combining information from various questions contained in each wave of the SOEP. For example, in wave J (1993) we start with question 72:

"*Since the beginning of 1992 have you finished school, vocational training, or university?*",

and proceed with the next question:

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

to define who has obtained a specialized upper secondary school degree (*Fachhochschulreife*) or a upper secondary school degree (*Abitur*) and is thus qualified to enrol into higher education. This information is also contained in each successive wave in 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, we 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 the answer is "*yes*":

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

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

For the reasons mentioned in section 3.2, we allow the student to defer the enrolment decision up to three years. If a student does not attend higher education within three years, she/he is classified as a non-student.

Simulation of BAfoeG Eligibility

Whether an individual receives financial support through BAfoeG or not is only observed for students enrolled into higher education, not for school leavers who do not pursue university education. Since this counterfactual BAfoeG eligibility is not observable, we apply a small microsimulation model to infer who would be eligible for the financial study support and who would not be, irrespective of an individual's actual student status.

The BAfoeG regulations define the maintenance needs for students conditional on the student's living situation, i.e. whether she lives with her parents or on her own, from which the financial capacities of the student, her husband, and her parents are subtracted. A student's maintenance needs may also depend on the cost of health insurance. However, in case at least one of the parents is covered by the public health insurance system the student usually will also be covered by public health insurance free of charge provided the student's own income is below a certain threshold.

Parents' financial capacity is the major factor determining BAfoeG eligibility where relevant income definition includes post-tax labour, asset and pension income. Income tax and social security liabilities are only available for the household and not at the individual level. In case parents live together we take household income, if they live in different households we take net income from the father's and the mother's household separately. If the father or the mother shares the household with another spouse, we take the half of net-tax household income. An allowance is granted on parents' income conditional on the family status and for each child that is entitled to alimony. A share of the remaining income – that again depends on the amount of alimony children in the respective household are entitled to – is then subtracted from the maintenance needs. If the difference is positive the individual is eligible ($BE_t=1$) for the study support.

The two following equations summarize the components of the BAfoeG simulation procedure:

(A1)

$$\begin{aligned} BAfoeG_t = & \left(\text{maintenance needs} \middle| \text{living status}\right)_t \\ & -\left[\left(\text{parents' income}\right)_t - \left\{\left(\text{allowance}_1 \middle| \text{family status}\right)_t + \left(\text{allowance}_2 \times \text{children}\right)_t\right\}\right] \\ & \times\left(1-\left[\text{allowance}_3 + \text{allowance}_4 \times \text{children}\right]_t\right) \end{aligned}$$

(A2) $$BE_t = \begin{cases} 1 \text{ if } BafoeG_t > 0 \\ 0 \text{ if } BafoeG_t \leq 0 \end{cases}$$

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

Table A1: Tests of the common time trend assumption in the transition rate model, school leaving cohort 1992 – 1997.

	(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:			
eligilbe for BAfoeG	-0.094	0.833	0.190
	(0.986)	(2.979)	(0.872)
trend	-0.020	-0.605	--
	(0.093)	(0.852)	
trend x eligible for BAfoeG	0.084	-0.513	--
	(0.198)	(1.562)	
$trend^2$	--	0.066	--
		(0.094)	
$trend^2$ x 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 x eligible for BAfoeG	--	--	0.870
			(1.135)
1994 x eligible for BAfoeG	--	--	-0.926
			(1.229)
1995 x eligible for BAfoeG	--	--	0.721
			(1.131)
1996 x eligible for BAfoeG	--	--	-0.298
			(1.304)
1997 x eligible for BAfoeG	--	--	0.766
			(1.175)
mass point:			
ε_1	-11.641	-3.893	-11.706
	(329.772)	(0.555)***	(355.113)
ε_2	*6,656*	*2,816*	*7,008*
prob(ε_1)	-0.559	-0.324	-0.513
	(0.158)***	(0.197)*	(0.143)***
	[0,364]	[0,420]	[0,374]
prob(ε_2)	*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 (chi2)	0.178	1.544	3.632
equality of trends (prob > chi2)	0.673	0.462	0.604

Notes:

a) Notes as in table2.
b) The school leaving cohort of 1992 is the reference category for the non-parametric trends in column (3).
c) This specifications control also for covariates as well as group dummies for the difference-in-difference as in Table 2.