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The Enrollment Effect of Secondary School Fees in Post-War Germany

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ABSTRACT

The Enrollment Effect of Secondary School Fees in Post-War Germany*

This study utilizes the heterogeneity of the fee abolition for West German secondary schools to identify its effect on enrollment and to obtain an estimate of the price elasticity of demand for education. The analysis is based on administrative school enrollment statistics as well as on representative individual-level data from three annual surveys of the German Mikrozensus. Estimates suggest that enrollment in Advanced Schools increased by about six percent due to the fee abolition, where the results are sensitive to the specification choice. The positive enrollment effect of fee abolition for women exceeds that for men. A fifty percent reduction in fees is associated with an overall change in enrollment rates by 3 percent, where the elasticity of the demand for females' education again exceeds that for males'.

JEL Classification: I20, H52, H71, C21

Keywords: school fees, tuition, enrollment, demand for education, natural experiment

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1. Introduction

In industrialized countries secondary education is typically provided free of charge. This is commonly justified by positive externalities, equity, and distributive justice arguments. Historically, school fees were the rule at public schools and they are still commonplace for private schools. Even though economic theory suggests that the demand for schooling responds to its price and the price of schooling has changed over the last decades, we know little about the effects of these price changes. Studies on the development of educational attainment over time often do not account for price changes, and generally, the price elasticity of secondary schooling has found little research attention compared e.g. to the price elasticity of college enrollment.

I take advantage of a natural experiment in post-war Germany to identify and estimate the effect of (the abolition of) school fees on school enrollment and the price elasticity of demand for secondary education. This contributes to several ongoing debates. First, the results are informative for the discussion of school vouchers in the United States (e.g. Ladd 2002, Neal 2002, Epple and Romano 1998, Epple et al. 2004) where the cost of secondary education and its behavioral effect is an important aspect. Second, the debates on the effect of tuition subsidies in the United States and on the introduction of university fees in Europe generally lack reliable measures of the price elasticity of education demand: In Europe, data on prior experiences with academic fees are often unavailable. In the U.S., the measurement of the effect of public aid on college enrollment is hampered by the potential endogeneity of aid receipt.[1] The evidence presented here is relevant to both of these discussions. Third, this study is related to a growing literature that investigates instruments to increase school attendance in developing[2] as well as in

[1] The literature addresses this endogeneity using a variety of natural experiments (e.g. involving the GI Bill, tuition, or subsidy changes). For a survey see Dynarski (2002). Key contributions are Kane (1994, 1995), Ichimura and Taber (2002), or Heckman et al. (1998).

[2] Examples are Vermeersch (2002) on school meals in Kenya, Miguel and Kremer (2004) on deworming pupils in Kenya, Schultz (2000) on cash transfers to Mexican parents, or Kim et al. (1999) on subsidies for girls' education in Pakistan.

industrialized countries.[3] Finally, following up on Goldin (1998) and Goldin and Katz (1997) I provide an analysis of secondary schools in Germany, which did not experience anything similar to the North American increase in graduation rates in the first half of the century.[4] I investigate whether the existence of school fees contributes to explain the international difference in educational enrollment.

Up until the end of World War II, fees had to be paid for advanced secondary education in Germany, typically amounting to about ten percent of an average worker's gross earnings per pupil. After the war, when educational authority was returned to the federal states, fees for advanced secondary education were abolished state by state between 1947 and 1962. The variation in the timing of fee abolition across regions is used here to identify the effect of fees on Advanced School enrollment, and to compare the price responsiveness of demand for the education of male and female youth. The analysis first investigates the responsiveness of education demand to the existence of a school fee, and then it evaluates the sensitivity of enrollment to changes in the price of education.

I find surprisingly small but significant effects of school fee abolition on enrollment. The responsiveness of the demand for female education exceeds that of the demand for male education. Overall, fee abolition seems to be associated with an increase in enrollment rates by about 6 percent. A drop in the fee-to-income ratio by 50 percent or five percentage points increases the aggregate enrollment rate by about 3 percent again with larger effects for females

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[3] See Meghir and Palme (2003) for the Swedish experience in the 1950s and 1960s, Aakvik et al. (2003) on Norway, and Dearden et al. (2003) for a current program in the United Kingdom.

[4] Goldin (1998) points out that in the United States, where secondary education was provided generally free of charge, high school enrollment rates rose from 18 to 73 percent and graduation rates from 9 to 51 percent between 1910 and 1940. Goldin and Katz (2003) investigate the role of compulsory schooling laws in this development. As Advanced School participation until today exceeds the minimum requirements of compulsory schooling laws in Germany, these regulations are not relevant to this analysis.

than for males.

2. Institutional Background, Theoretical Model, and Hypotheses

2.1 School Fees in a Historical Perspective

Traditionally and until today, the German schooling system has been structured not only by years of schooling, but also by parallel tracks with different performance requirements. Since the 19th century the standard education has been provided by Basic Schools (*Volksschule* / *Hauptschule*) which used to last 8 years and prepared pupils for apprenticeships or vocational schools. It was possible to advance from Basic School after 4 years to either Middle School (*Realschule* / *Mittelschule*) or Advanced School (*Gymnasium* / *Oberschule*),[5] where education continued for an additional 6 or 9 years, respectively (cf. Figure 1). The system hardly changed over time, and the Advanced School degree remained the key requirement for university studies.

Through the 19th century there was a fee to be paid for any type of school financing teacher salaries. Starting with Prussia (1888) and ending with Saxony (1919) fees for Basic Schools were abolished state by state by 1920 (Kahlert 1974). The regulations on school fees for Middle and Advanced Schools varied across regions. The fees per pupil at times exceeded 10 percent of an average labor income. Figure 2 depicts the share of school fees in average income for the case of Prussia. It reflects nominally rising earnings during the inflation when fees remained unadjusted. Around the time of WWII the German educational system was centralized and underwent major distortions connected to the manpower needs of the military (for an evaluation see Ichino and Winter-Ebmer 2004): Advanced School education was reduced by one year in 1938, and starting 1941 it was at times shortened by an additional 6 months. Also, for the the birth cohorts 1922-1928 final examination requirements were reduced and frequently dropped

[5] Depending on region and period more or less demanding entrance exams were required to enter Middle or Advanced School (cf. Kuhlmann 1970).

completely to facilitate military service.

After the war, the authority for the administration of the school system was returned to the German federal states. They increased the duration of Advanced School education back to 9 years and - instigated in part by the political ideas of the allied occupation forces (Furck 1998) - re-regulated the fee system: Starting with the city-state of Bremen (1947) and ending with Rhineland-Palatinate (1962) over time all states abolished tuition fees for public secondary schools (Benatzky 2001, Berger and Ehmann 2000). While until 1945 the annual fee level was set uniformly at 240 *Reichsmark,* there was considerable regional variation in the speed and extent of fee abolition afterwards, which we use to identify its effects. Figure 3 describes the fee abolition pattern across the 11 federal states.[6] Frequently, tuition was abolished stepwise, e.g. annually by one seventh over the course of 7 years as in the case of Hamburg, or in steps from 100, to 50, and 25 percent of the original amount as in the case of Bavaria. Figure 4 describes the development of school fees per pupil by state and over time as a share of average earnings for selected states. It shows the heterogeneity of the abolition process between 1947 and 1962.

In our analysis, we distinguish three cohort groups among the youths growing up in a given state, based on the fee system they faced when entering Advanced School: The first and oldest group would have had to pay school fees upon entering Advanced School (pre group). For a second group it depended on the speed at which they completed primary education whether they would have entered Advanced School prior to the abolition of the school fees (transitional group). For these individuals we cannot tell for sure under which regime they attended school. Finally, the third group consists of those birth cohorts who certainly would not have had to pay school fees, because they entered Advanced School after the fee abolition (post group). Since fees were abolished at different dates for different states, Table 1 indicates the relevant cohort groups

[6] We describe only the developments in former West Germany. School fees were abolished for all of East Germany in 1957 (Geissler 2000).

for each of the 11 states, Appendix A provides additional explanations.[7]

In order to convincingly argue that fee abolition helps identify the response of education demand to changes in education prices we need to establish that the abolition is not jointly determined with state school enrollment. It is difficult to obtain historic accounts of the political processes leading to fee abolition.[8] However, we can investigate the correlation between the timing of abolition and state enrollment patterns. Figure 5a depicts Advanced School enrollment rates for each state's last birth cohort that certainly had to pay fees by the year of fee abolition. If we account for the general trend to higher enrollment rates over time, the scatter plot hardly suggests a systematic correlation between the year of abolition and the enrollment rate. A more likely mechanism determining abolition years might be the political orientation of state governments: Figure 5a indicates whether the government passing the legislation abolishing fees was left-wing (SPD) or right-wing (conservative). The evidence is not compelling but suggests that socialdemocratic governments abolished fees earlier. Figures 5b and 5c present absolute and relative changes in enrollment rates for the last three birth cohorts unaffected by fee abolition by year of fee abolition. Again there is no indication of a systematic relationship. Therefore, I consider the timing of the state-wise abolition of fees as exogenous to state enrollment rates.[9]

The abolition of school fees was not the only development in the German educational system during the 1950s and 1960s. Similar to other industrialized countries, the educational system expanded beginning in the early 1960s. This was due to more sizeable birth cohorts, an

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[7] Immediately after the war there was some regional experimentation with e.g. longer duration of primary education of 6-8 years prior to the transition into the differentiated track system. This should not affect our analysis. Details are provided in Appendix A.

[8] An interesting description of the case of Bavaria is provided by Klafki (1976).

[9] Kuhlmann (1970, p.59) provides an illustration of how policy makers thought about school reforms in the 1950s. Reforms were passed based on their alleged educational benefit. Economic cost benefit calculations were not performed. One state secretary of education was in fact publicly scolded for raising economic consideration in the discussion of educational reform.

increased demand for advanced education, as well as a broadening of access to education with increasing public investments in the education system.[10]

2.2 Theoretical Model and Hypotheses

Similar to Card (1999) we model optimal schooling in a framework that abstracts from dynamic processes and describes the schooling decision as a tradeoff between increases in the present discounted value of the utility of derived from future earnings, and of the disutility deriving from education costs. However, we are not interested in the optimal number of years of schooling but in an individual's (latent) propensity to enrol in Advanced School (S*):

$$(1) \qquad S_i^* = Y(S_i, A_i; \mu_i) - H(S_i, A_i, C; \nu_i)$$

Y is the discounted utility of lifetime earnings and H is the discounted disutility deriving from Advanced School participation. Both depend on school enrollment (S_i) where we would assume an increasing concave function for Y and an increasing convex function for H. Both may also vary with a pupil's ability (A), which may yield higher earnings advantages and lower disutility from additional schooling.[11] C represents the direct cost of Advanced School participation affecting the utility loss due to school participation. μ_i and ν_i are person-specific effects.

A simple linear specification of the two factors could be:

$$(2) \qquad Y_i = \mu_i + b_1 S_i + b_2 A_i \qquad \text{with } b_1, b_2 > 0$$

$$(3) \qquad H_i = \nu_i + c_1 S_i + c_2 A_i + C \qquad \text{with } c_1 > 0 \text{ and } c_2 < 0$$

such that

$$(4) \qquad S_i^* = (\mu_i - \nu_i) + (b_1 - c_1) S_i + (b_2 - c_2) A_i - C$$

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[10] The standing conference of state ministers of education agreed in the 1960s to raise the level of education and to increase Advanced School enrollment. In consequence, education expenditures went up significantly (Fränz and Schulz-Hardt 1998).

[11] For the more able it may be possible to earn the highest returns given their education, and they may learn quicker, incurring lower cost of education, e.g. by earning an income at the side, whereas the less able may need additional time and tutoring to meet requirements.

Clearly, a reduction in C will increase the probability of Advanced School enrollment. Participation probabilities are higher for the more able students. Also, if e.g. individual effects μ_i or returns to schooling and ability (b_1, b_2) vary systematically across population groups g (with g=0,1), such as the two sexes, with $\mu^{g=1} > \mu^{g=0}$, $b_1^{g=1} > b_1^{g=0}$, or $b_2^{g=1} > b_2^{g=0}$, then it follows that

(5) $$S_i^*{}_{|\,g=1} > S_i^*{}_{|\,g=0}.$$

Figure 6 depicts a situation, where pupils are sorted by ability on the abscissa. We expect that those for whom the expected lifetime benefit of schooling (Y_i) exceeds the discounted disutility (H_i) will attend Advanced School. In Figure 6 everybody to the right of point A will enrol in Advanced School, here amounting to the 10 percent most able pupils. If fees are abolished, direct costs (C) decline and the share of pupils in Advanced Schools may increase to e.g. the 20 percent most able individuals (see point B in Figure 6).

If we hypothesize further, that for parts of the population such as females the expected benefits at all ability levels are below the average[12] - e.g. due to a smaller value of parameter b_2 - then this group's participation share should be below average, both before and after the abolition of fees. In that case females' Y'_i schedule is flatter than males' and females' response to the abolition of school fees in terms of the relative enrollment increase may exceed that of males (for details on the theoretical analysis see Appendix B).

Within this framework, the abolition of tuition should cause a decline in the direct cost of education and yield an overall increase in the participation rate. Given the variation across federal states and time, we hypothesize:

H1: Advanced School participation increases after the abolition of school fees.
H2: Advanced School participation in states without fees exceeds that of states with fees.
H3: Advanced School participation for males exceeds that of females.
H4: The increase in Advanced School participation may be more pronounced for females than for males.

[12] Cawley et al. (1999) provide evidence of higher returns to ability for men compared to women among whites and hispanics.

The next sections describe the procedure applied to test these hypotheses.

3. Data Description and Empirical Strategy

3.1 Aggregate Evidence

Our first approach at evaluating the enrollment effects of school fee abolition takes advantage of historic enrollment data at the state level. Ideally, we would measure the size of the annual school entry cohorts, but unfortunately only the total number of pupils per year cumulatively over all 9 grades in Advanced School is available from the state statistical offices. A disadvantage of this aggregate measure is that changes in school entry can only be measured to the extent that they change *total* school enrollment.[13] Because of this imprecision in the data we present average figures across the years before and after the fee abolition. As there were significant changes in birth cohort sizes in this period we generated demography-corrected cohort-specific Advanced School enrollment rates by state.[14]

Since not all of the 11 state statistical offices could provide the necessary figures we are restricted to evaluate the 6 states described in Table 2. The numbers indicate sizeable increases in aggregate enrollment rates around the time of the fee abolition. On average the cohort share attending Advanced School increased by 22.9 percent between the five year periods before and after the abolition of school fees. This supports Hypothesis 1 (H1), Advanced School participation increased after the abolition of school fees.

However, this evidence blurs the fee effect by looking at the total number of pupils in Advanced School and by disregarding the general education expansion over time. For more

[13] If, e.g. fees are abolished in Hamburg as of 1957, we would expect higher entry rates (at grade 5) in 1958. However, in 1958 we only know the total number of pupils attending Advanced School (grades 5-13).

[14] Enrollment rates are calculated as the ratio of the number of pupils in Advanced School in a given state over the total population aged 10-19 in that same state and year.

precise measures and to control for time trends we now turn to individual level data.

3.2 Individual Level Evidence

3.2.1 Data Source and Sample

The individual level data are taken from the Mikrozensus, which is an annual survey of a one percent random sample of German households. Public use files of 70 percent of the original data are available for the years 1989, 1991, 1993, 1995, 1996, and 1997. The Mikrozensus uses a rotating scheme in that the inhabitants of a given dwelling are re-interviewed up to four times. Unfortunately, households or individuals cannot be identified across survey waves. To avoid a duplication of records we restrict the analysis to the surveys of 1989, 1993, and 1997 for which the sets of respondents do not overlap.

Our sample considers the birth cohorts of 1930 through 1959 if they are German nationals and live in West German states. We drop observations with missing values on key variables such as age, sex, schooling, or state. This yields a total of 433,315 observations, with about one third from each of the 3 surveys, and between 10'000 and 18'000 per birth cohort. The key advantage of the dataset is its size, which allows to compare state-level differences by birth cohort. The main disadvantage is the lack of social background variables. It would be useful to control for parental human capital as a determinant of child educational attainment. Given that such measures are not available, the findings presented below cannot separate the impact of social background from the measured state and cohort effects. This limitation is addressed in the discussion below.

3.2.2 Descriptives

Figure 7 describes the development of the enrollment rate in Advanced Schools over subsequent cohorts. As suggested by Hypothesis 3 (H3) the enrollment rates differ between the

sexes. They were at about 10 percent for men and 5 percent for women up until the birth cohorts of the 1930s for which the rates started to increase. For more recent birth cohorts females reach and even exceed males' educational attainment.

Hypothesis 2 (H2) suggested that enrollment rates in states without fees exceed those in states with fees for any given cohort. Confirmative evidence is presented in Figure 8, which depicts average enrollment rates for cohorts in states with and without school fees.

To obtain preliminary evidence on the fee effect, we calculated state-specific Advanced School participation rates for cohorts entering Advanced School before and after the abolition of fees. The results in Table 3 yield that on average participation rates increased vastly from 8 percent among the "pre-fee" cohorts to 18 percent for the "post-fee" cohorts. These developments are similar when calculated for males and females separately. The figures show that not only *levels* of education attainment vary across states but also the developments over time are heterogeneous. We find particularly large increases in Advanced School participation in Hamburg, and Hesse and the smallest increases in Schleswig-Holstein, and Rhineland-Palatinate. As these results do not control for time trends we use a regression based strategy next.

3.2.3 Empirical Strategy

The objective of the analysis is to reliably identify the effect of the abolition of school fees on Advanced School enrollment. Our dependent variable describes whether an individual obtained an Advanced School degree ("*Abitur*") and we use a simple logit estimator.[15] We apply four flexible approaches to control for state and period effects.

In Table 1 we categorized the residents of every state in three groups depending on their year of birth: Those who would have had to pay fees upon entering Advanced School ("pre" fee

[15] The models were also run as linear probability models. While the coefficient signs and significances sometimes differed, predictions were in the same direction and at similar orders of magnitude to those obtained using the logit specification.

abolition cohorts), those for whom we cannot be sure ("trans"(itional) birth cohorts), and those who would not have had to pay fees ("post" fee abolition cohorts). Our four estimation approaches differ in the flexibility with which they control for time trends and state-specific effects:

(i) A first approach controls for the cohort groups (pre, trans, post), for state fixed effects, and a linear cohort effect:

$$\Pr(S_i = 1) = \Lambda(\alpha + \beta_1 \text{trans}_i + \beta_2 \text{post}_i + \gamma\, c_i + \delta\, \text{State FE}_i),$$

where S indicates Advanced School attendance, Λ represents the logistic cumulative distribution function, c represents the birth year, trans and post represent cohort group indicators, State FE stands for a vector of state fixed effects, and α, β, γ, and δ are coefficients to be determined.

(ii) In order to allow for the possibility that the speed of educational expansion varies depending on the fee regime in place, a second approach allows for group-specific linear cohort effects as opposed to one overall linear cohort trend:

$$\Pr(S_i = 1) = \Lambda(\alpha + \beta_1 \text{trans}_i + \beta_2 \text{post}_i + \gamma_1 (\text{pre}_i \cdot c_i) + \gamma_2 (\text{trans}_i \cdot c_i) + \gamma_3 (\text{post}_i \cdot c_i) + \delta\, \text{State FE}_i).$$

(iii) The third approach instead considers state-specific linear cohort trends:

$$\Pr(S_i = 1) = \Lambda(\alpha + \beta_1 \text{trans}_i + \beta_2 \text{post}_i + \gamma^o (\text{State FE}_i \cdot c_i) + \delta\, \text{State FE}_i).$$

(iv) Combining ii and iii, the final approach controls for cohort effects by state and by group:

$$\Pr(S_i = 1) = \Lambda(\alpha + \beta_1 \text{trans}_i + \beta_2 \text{post}_i + \gamma_1^o (\text{State FE}_i \cdot \text{pre}_i \cdot c_i) + \gamma_2^o (\text{State FE}_i \cdot \text{trans}_i \cdot c_i) + \gamma_3^o (\text{State FE}_i \cdot \text{post}_i \cdot c_i) + \delta\, \text{State FE}_i).$$

The coefficients of the post cohort group indicators (β_2) and simulation results inform on the significance and magnitude of changes in Advanced School attendance after the fee abolition. Following the recommendations of Bertrand et al. (2004), standard errors are adjusted for clusters at the state and cohort level.

4. Results and Discussion: The Enrollment Effect of Fee Abolition

4.1 Estimation and Simulation Results

The results of the four estimation approaches discussed above are presented in Panel A of Table 4. The coefficient estimates for β_2 are significant and positive in all specifications suggesting that the probability of attending Advanced School is higher for individuals who are not subject to school fees compared to those in the "pre" cohorts.[16] The state-specific heterogeneity in Advanced School enrollment rates is reflected in highly significant state fixed effects (δ).[17] In contrast to those in Tables 2 and 3, these results control for aggregate and state-specific trends reflecting e.g. the educational expansion over time.

Panel B of Table 4 presents the average predicted effect of the abolition of school fees on Advanced School enrollment probabilities. The hypothetical enrollment probability of an individual born in the midst of the transition cohort group is compared for the case that she were to follow the enrollment pattern of the pre-abolition group to the enrollment probability that would result if she behaved like individuals born in the post-abolition group.[18]

About representative for subsequent specifications, model (1) yields that the average enrollment probability increased significantly after the abolition of fees by 6.3 percent for the full sample.[19] Based on separate estimations of model (1) for the two sexes we find a more sizeable effect of the abolition of fees for females with 6.7 compared to 6.0 percent for males.

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[16] In models 2 and 4 with interaction terms of the cohort group indicators the main effects (β_1, β_2) can of course not be interpreted independently.

[17] The results regarding β_2 and δ hold similarly for estimations that were performed separately for the two sexes.

[18] This simulation procedure involves slight out of sample predictions as the cohort effects of the pre and the post groups are applied to a person born in the trans group (cf. Table 1). The predictions for the full sample are based on the regression results in Table 4. Those for the gender subsamples are based on separate regressions that are not presented.

[19] The standard errors are bootstrapped using 100 repeated draws from the original data.

Adding flexibility to the representation of the time trend, model (2) allows for different slopes by cohort group: Instead of one time coefficient three interacted effects are estimated. Their positive coefficients agree with the secular increase in Advanced School enrollment over time with slower growth for the last cohort group. The predicted fee effects remain within the range of simulations obtained based on model (1).

Columns (3) and (4) allow for more state-specific flexibility, where the group-cohort interactions are replaced with eleven state-cohort interaction terms. The predicted overall fee effect in column (3) of Panel B hardly differs from that in column (1). In model (4) we control for a separate set of three time trend effects for each of the eleven states yielding 33 parameters to represent the cohort effects. The predictions still yield an aggregate enrollment increase of 6.6 percent after the abolition of fees.[20]

The individual level data provides robust evidence for a significant, yet small increase in the probability of attending Advanced School after the abolition of fees: Starting from an overall enrollment rate of ten percent (cf. Figure 8) the estimated effect of six percent causes a change in enrollment rates from about 0.1 to 0.106.[21] The effects are small but highly significant.

The outcomes by sex yield mixed evidence: Specifications (1) and (2) suggest a larger fee effect for females, specifications (3) and (4) yield the reverse. In more restrictive estimations that impose identical cohort and state effects for both sexes and include a sex main effect and interactions for the cohort group indicators, we find significantly larger fee abolition effects for women with a difference of more than 20 percentage points.

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[20] A test of the specifications in columns (1) and (3) against their more flexible counterparts in columns (2) and (4) yields in both cases that the parameter restrictions implied in specifications (1) and (3) must be rejected at the 1 percent level.

[21] For females who start out with an average enrollment rate of about 9 percent before fee abolition the average absolute increase in enrollment rates amounts to 0.6 percentage points, for males starting out with an average enrollment rate of 14.5 percent before fee abolition it amounts to 0.9 percentage points (based on the predictions in specification 1).

4.2 Robustness Tests and Discussion

Next we investigate whether the above results hold up to robustness tests, and discuss how data limitations might affect the findings.

Sample: One objection to the above analysis may concern the selection of the sample which includes all observations born between 1930 and 1959. This might cause misleading estimates of cohort and cohort group effects, as at times irrelevant cohorts are considered, and in some instances the number of cohorts available to support the estimates is limited.[22]

To evaluate the effect of such sampling problems the analysis was redone, this time considering for each state only those 15 cohorts entering Advanced School before and after fee abolition. The results obtained with this sample are presented in Table 5. We find again confirmation for increasing enrollment probabilities over time (see the β_2 and γ coefficients). Panel B confirms that the abolition of fees yielded increases in enrollment probabilities: The simulated effects in specifications (1) and (3) are significantly different from zero at the one percent level and slightly larger than those in Table 4. On the other hand the predicted effects for the full sample in specifications (2) and (4) are now reduced in magnitude and statistically insignificant. Given the significant coefficient estimates and overall positive effects of fee abolition, our main conclusions are confirmed: There are significant enrollment responses to the abolition of fees varying between 3 and 7 percent with larger effects for females than for males.

Income Effects: One may argue that the estimation suffers from omitted controls for the increasing income of the population in post-war Germany. As the speed of economic growth differed across federal states this may be responsible for the heterogeneity in responses to fee

[22] If, e.g., fees were abolished in Rhineland-Palatinate in 1962, trends for those born in the 1930s may cause spurious results.

abolition which we noticed in Table 3: inhabitants of poorer states may respond stronger to the price changes of secondary education.[23] To address this problem one would ideally control for state-specific annual incomes. As an approximation we consider real state-specific per capita gross domestic product (GDP). Such figures are available annually for West-German states since 1950.[24] The GDP information is not available for years prior to 1950, and for Berlin and Saarland only after 1960. The above estimations were repeated with controls for annual state-level per capita GDP, where a 'missing-value indicator' was added to the specification for observations with missing GDP information.[25]

The estimations with GDP controls yield results that are quite similar to those presented in Tables 4 and 5.[26] The GDP indicators are statistically significant with negative coefficients in the estimations for the full sample and the male subsample, and insignificant in the female subsample. This suggests that ceteris paribus the enrollment rate was higher in states with low GDP per capita (or missing values). Table 6a summarizes the simulation results obtained for both samples when GDP controls are added to the models. The fee effects for the original sample are within the range observed in Table 4 and generally statistically significant. Those for the modified sample are slightly reduced in size (cf. Table 5). Again, the effects for females are typically larger than those for men and overall the results seem to be robust to income controls.

Growth Effects: Even though controls for average per capita income do not affect the results,

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[23] Goldin (1998) finds that higher per capita income at the state level had a strong positive effect on secondary schooling in the United States in the early twentieth century.

[24] The figures confirm the heterogeneity in state-specific growth processes. While Bavaria or Schleswig-Holstein quadrupled their real per capita GDP between 1950 and 1980, Bremen or Northrhine-Westphalia merely tripled theirs.

[25] We use the value of state-level per capita GDP which was measured in the year when the individual turned 11, the typical age of transition to Advanced School.

[26] The estimates are not presented to save space and are available upon request.

controls for changes in per capita incomes may well do so. Three arguments support this presumption: first, when parents consider their children's future earnings potential their expectations may vary with current and expected future growth rates. Second, parents' liquidity - as a function of past savings - may be determined by growth rates of the regional economy in contrast to current income levels. Finally, high growth may also reduce educational investments because by causing wage raises they increase the opportunity cost of education. To investigate whether such mechanisms affect our estimates of the fee effect we reestimated our models this time adding controls for the growth rate of regional per capita GDP instead of its levels. Simulation results are presented in Table 6b. They yield no important differences compared to prior results.[27]

Functional Form Effects: So far we used four linear specifications to control for overall state and cohort effects. Next, we investigate whether the results are robust to changes in the functional form of the cohort effects. First, we add quadratic cohort effects (c^2) to models (i)-(iii):

(i') $\Pr(S_i = 1) = \Lambda(\alpha + \beta_1 trans_i + \beta_2 post_i + \gamma_1 c_i + \gamma_2 c_i^2 + \delta \text{ State } FE_i)$,

(ii') $\Pr(S_i = 1) = \Lambda(\alpha + \beta_1 trans_i + \beta_2 post_i + \gamma_1 (pre_i \cdot c_i) + \gamma_2 (trans_i \cdot c_i) + \gamma_3 (post_i \cdot c_i) + \gamma_4 (pre_i \cdot c_i^2) + \gamma_5 (trans_i \cdot c_i^2) + \gamma_6 (post_i \cdot c_i^2) + \delta \text{ State } FE_i)$.

(iii') $\Pr(S_i = 1) = \Lambda(\alpha + \beta_1 trans_i + \beta_2 post_i + \gamma^o_1 (\text{State } FE_i \cdot c_i) + \gamma^o_2 (\text{State } FE_i \cdot c_i^2) + \delta \text{ State } FE_i)$

This generates one additional parameter in specification one, three in specification two, and eleven in specification three.[28] The coefficients of the quadratic cohort effects turn out to be

[27] In another experiment we controlled for both income growth and income levels, which did not affect the estimated fee abolition effects, either.

[28] As model (iv) already considered 33 coefficients, the addition of 33 squared terms caused an overspecification with some unplausibly large coefficient estimates. The simulations did not provide informative estimates of the fee effects and are not presented.

statistically significant. Panel A of Table 7 describes the simulated fee abolition effects: The fee effect is still positive and significant in the first and third specifications but negative and always insignificant in specification (2).

As a second modification of the functional form we add a full vector of birth cohort fixed effects to the four original specifications.[29] These fixed effects are always jointly highly significant. After adding these rich cohort effects to the previous linear models the predicted fee abolition effects vanish almost completely. We still find a systematic difference in the effects obtained for males and females but any remaining fee effect is of small magnitude. We cannot reject the hypothesis that the fee effects presented before are due to insufficient controls for overall time trends and year-specific effects in the data. However, the small size of the measured effects may in part be due to a downward bias that follows from data limitations which we discuss next.

Enrollment vs. Completion: So far we have not paid much attention to the definition of our dependent variable, which does not measure Advanced School enrollment but completion of the Advanced School degree. This could systematically bias the results if the group of individuals starting Advanced School and the group completing it differ in a way that is correlated with the effect of school fees. Such a correlation is indeed likely, as the children of better off parents would be less restricted by school fees *and* are more likely to receive extra support in completing their school work compared to children of poor parents. After the abolition of fees more poor kids might have started Advanced School than show up in our data, which describes only those successfully completing Advanced School. Therefore, this measurement problem can cause an

[29] For the original (modified) sample we estimated an additional 29 (38) parameters, which yielded a total of 30, 32, 40, and 62 cohort effect parameters for the four specifications for the original sample, and 39, 41,49, and 71 cohort parameters for the modified sample.

underestimate of the true enrollment effect of fee abolition, rendering our figures lower bounds.[30]

Regional Mobility: Since we observe only the state of residence at the time of the survey, we do not know in which state an individual actually lived when attending school. As long as individuals moved between states in a random fashion, this would cause an attenuation bias in the measured effects, again rendering our results lower bounds of the true effect.

An upward bias could result only if migration were correlated with state-specific cohort group effects, e.g. if individuals with (without) Advanced School degrees moved to states where they belong to the post (pre) group. We have no evidence confirming such migration patterns.

The German Socioeconomic Panel (GSOEP), an annual household panel survey, provides evidence on the regional mobility of Germans: every respondent is asked whether he still lives in the town where he was raised. Living in the same town is obviously more restrictive than residing in the same state. Nevertheless, in 1985 (2001) 58 (55) percent of the respondents of the GSOEP sample still lived in the town where they were raised.

As the GSOEP has been in the field for more than 18 years now we can also use the panel nature of the data to find out that of the (non-representative) sample of 6,284 individuals who were surveyed both in 1984 and in 2001 only about 5 percent changed their federal state of residence in between. These figures are indicative of an immobile population. Similar evidence is provided by Pischke (2003): He shows that about 80 percent of all adult respondents to a large German social survey still live in their state of birth. Therefore, the effect of neglecting residential mobility is not likely to be large in magnitude and - unless mobility followed very specific patterns - it is likely to be uncorrelated with the abolition of school fees. The

[30] This underestimation of the true effect is limited by the extent to which children of poor parents may be able to balance disadvantages related to their parental background by higher effort or ability. However, Kuhlmann (1970, p.78) shows evidence that the share of pupils with highly educated parents increases over subsequent grades in Advanced School. This suggests that pupils from disadvantaged backgrounds indeed dropped out at faster rates.

measurement error may downward bias our estimates.

Anticipation Effects: The abolition of secondary school fees took time: Some states abolished them early on in their constitutions. Others gave in to the pressure of the occupation forces after WWII (Furck 1998), while others followed the recommendations of the National Education Advisory Board, which recommended the abolition of school fees in 1954 (Bohnenkamp et al. 1966). It is possible that we measure only small fee abolition effects because parents changed their behavior not only when fees were abolished - as we have assumed so far - but much earlier.

To provide an alternative "benchmark" indicator of the fee effect on enrollment we investigate whether enrollment probabilities vary with the true number of years that pupils would have had to pay fees upon enrolling in Advanced School. We modified specifications (1) and (3) of our model and - in addition to state and cohort effects - controlled for the actual number of years of fee payment ("feeyears") following Advanced School enrollment.[31] This represents a fee effect under the assumption that parents had perfect foresight. Table 8 presents estimation and simulation results.

Generally, the number of years during which parents would have had to pay fees yield a significant negative effect on the probability of child enrollment in Advanced School. The bottom panel suggests that switching from full fee payment to no fee payment is associated with increases in the enrollment probabilities at the order of 7 to 8 percent based on the specifications with cohort fixed effects. Most effects are statistically significant and they are larger for females than males in specifications controlling for cohort fixed effects. The results support the conclusion that the abolition of school fees had small but measurable effects on enrollment probabilities that were larger for females than for males.

[31] Specifications 2 and 4 are omitted because the number of "feeyears" is highly correlated with the cohort interactions in these models.

Omitted Parental Characteristics: The data do not provide information on important covariates that may influence school attendance decisions, such as parental human capital. This causes a systematic bias if the omitted parental measures are correlated with the cohort group indicators (pre, trans, post). Such a correlation is plausible if either the distribution of parental characteristics or the relevance of the intergenerational transmission of education changed over time.[32] The existing evidence for Germany (see e.g. Blossfeld 1993, Müller and Haun 1993) suggests that in spite of the educational expansion the intergenerational correlation of educational success has not changed over time. In addition, there is no reason to expect a large difference in the level of parental education for those entering Advanced School before and after the fee abolition, as the educational expansion took place only for cohorts born later than the parents of the youths considered here. Thus, the omission of parental controls should not substantially affect the nature of our estimates.

Admission Requirements: Changing admission requirements to Advanced School education over time may cause shifts in enrollment patterns that interfere with our measures of fee abolition effects. Unfortunately, there is no source of information on this aspect. Until today admission standards vary significantly across federal states, with strict grade requirements in southern Germany and less restrictive systems in other states. Only if these requirements changed exactly at the time of fee abolition would our estimates be biased.[33] However, there does not seem to be evidence for such developments.

5. Estimation of the Enrollment Sensitivity to Fee Changes

[32] Kane (1994) shows that a large fraction of the increase in black high school graduation rates in the early 1980s was due to substantial improvements in parental educational background.

[33] In that case we would not be able to distinguish between the effects of fee abolition and changed admission requirements.

After the first part of our analyses was devoted to the question of whether the abolition of fees as such caused behavioral responses in Advanced School enrollment, we now turn to the price sensitivity of demand for higher secondary education. We regress individual school enrollment on the fee amounts.[34] As the fee amount was set nominally and its real value changed over time, we deflate the fee measure by calculating the share of the fee per pupil in average incomes. National average incomes are available from the records of the retirement insurance. The income share variable represents the state-specific fee-to-income ratio in the year when the pupils were 11 years of age.

The analysis uses both, the original and the modified sample. The specification of the school attendance model follows the models described above, only now adding controls for state- and period-specific income effects (GDP per capita) in all estimations. Since we are no longer focusing on fee abolition, the cohort group indicators used above are not relevant here. The models control instead for linear, quadratic, and cubic cohort effects either as main effects or interacted with state indicators. The estimation results on the original sample are summarized in Panel A of Table 9.

The estimates yield a clear and generally highly significant negative correlation of the fee-to-income ratio with individual enrollment probabilities. While the estimated coefficients vary across specifications, the predicted effect of changes in the fee-to-income ratio on enrollment rates appears to be rather stable. Panels B and C of Table 9 present the simulated effects of a decline in the fee-to-income ratio from 10 to 5 percent for the two samples. The simulated effects are higher when only general cohort controls are considered in the estimations (models 1-3) compared to the flexible state-specific controls (models 4-6). The predicted enrollment responses

[34] Since the full set of relevant state-specific fees is not available, some of the missing information on fee amounts was replaced by plausible assumptions. For the state of Saarland we only know that fees were abolished in 1959. We make no assumptions regarding the developments before 1958 and instead disregard observations from Saarland in this analysis. The evidence on fee developments is discussed in Benatzky (2001).

are generally highly significant. A reduction in the fee-to-income ratio by fifty percent is correlated with enrollment increases at the order of about 3 percent for the full sample with somewhat larger effects for the female subsample.[35] This result matches the 6 percent average enrollment effect predicted for fee abolition (i.e. reduction by 100 percent) surprisingly well.

6. Discussion and Conclusions

In times of tight public budgets changes in school fees and tuition are intensely debated. Informed political decisions must be based on evidence regarding the price sensitivity of education demand which is difficult to obtain. This study takes advantage of a natural experiment to measure the effect of school fees on enrollment: In post-WWII West Germany, fees for advanced secondary schools were abolished at different points in time between 1947 and 1962 across eleven federal states. The variation across time and state is used to identify the fee effect.

Based on a variant of Card's (1999) optimal schooling model, we derive four hypotheses on the enrollment effect of school fees. Overall, our evidence is consistent with the hypotheses that Advanced School participation (1) increases after the abolition of fees, (2) is higher in states without fees than in states with fees, (3) among males exceeds that among females, and (4) increases more for females than for males following the fee abolition.

Aggregate data suggest sizeable enrollment increases around the period of fee abolition. When we control for overall time trends using individual level data we find significant increases in Advanced School enrollment at the time of the abolition of fees at the order of 6 percent with larger effects for females than for males. Similarly, a reduction in the fee-to-income ratio by 50 percent is associated with an increase in the enrollment rates of about 3 percent, with a higher

[35] In estimations with sex interactions the simulated differences where much larger. The results presented in Table 9 are based on separate estimations for the two sexes and are preferred since they more flexibly capture the sex-specific trend effects.

price elasticity for girls' than for boys' education.[36]

There are two important limitations to these results: On the one hand they may underestimate the true effect of fee abolition because our data are subject to measurement error relating to the exact definition of school enrollment and to individuals' state of residence in their youth. On the other hand the results are not completely robust to changes in specification. While they hold up to different definitions of the relevant sample, to four increasingly flexible models which are linear in cohort effects, and some models which allow for quadratic cohort effects, the predicted effects diminish in magnitude or vanish when rich birth cohort fixed effects are added to the specifications. Even though some of these specifications yield significant small positive effects of the abolition of school fees on enrollment, the most flexible specifications do not allow us to reject the null hypothesis of zero enrollment effects of fee abolition.

This result might be affected by the attenuation bias following from measurement error in our explanatory variables or from systematic noise in the dependent variable. Substantively, the small effect may have a number of reasons: It might reflect that demand for education was indeed price inelastic or that the fee amount was too small to yield more sizeable responses. Küchenhoff (1952) and Bergmann (1955) point out that even after tuition fees were abolished certain other education related payments (examination fees, certification fees, accident insurance premia) were still collected and Advanced School fees only reflected a small part of the total expenses related to this type of schooling.

Alternatively, the small magnitude of the measured effect might be due to the protracted abolition of fees, where parents expected that fees were to be abolished and thus adjusted their behavior over longer periods then assumed here. Also, the change in fees may have followed a change in parental tastes for education such that the price elasticity of demand had changed (e.g.

[36] In terms of current incomes the simulated fee change (five percent of annual incomes) would be equivalent to a nominal change in fees by € 1400 (or $ 1700) per year.

due to social developments) even before fees were abolished. This would be a variant of an anticipation effect.

Capacity constraints may be responsible for the small demand response to fee abolition, as well: immediately after the war there was a major shortage of buildings for all types of schools. Kuhlmann (1970 p.27) suggests that through the mid 1950s insufficient school capacity was an acknowledged limitation to educational expansion: the National Education Advisory Board recommended in 1954 that school fees ought to be abolished but not at the expense of the solution of more urgent problems such as the provision school buildings (Bohnenkamp et al. 1966). If schools could not accommodate pupils we may not be able to measure demand effects.

Finally, it may be that those able pupils for whom school fees would have been prohibitive had always been supported on the basis of individual scholarships. If such systems were successful in promoting the brightest poor this would clearly reduce the measurable effect of the fee abolition. While such support systems existed, their overriding effectiveness is questionable. Küchenhoff (1955) discusses that only between one and two percent of the pupils in Advanced Schools were supported by public funds and that scholarships were available only for university students.

In sum, our results suggest that the existence of school fees for Advanced Schools set a small but measurable barrier to higher education which affected females more than males. The differential price sensitivity for male and female students may provide part of the answer to the question why even today one finds significant enrollment differences between the sexes for higher education: Parental demand for girls' education seems to be more price elastic. Finally, given the limited size of the measured effects, the existence of school fees hardly explains the differential developments of secondary school graduation rates during the early decades of the twentieth century in Germany and the United States.

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Table 1 Abolition Phases and Affected Birth Cohorts

	Year of abolition	Cohorts with fee	Cohorts in transition	Cohorts without fee
		"Pre"	"Trans"	"Post"
Baden-Württemberg	1957	-1944	1945 - 1947	1948 +
Bavaria	1951	-1938	1939 - 1941	1942 +
Berlin	1948	-1934	1935 - 1938	1939 +
Bremen	1947	-1933	1934 - 1937	1938 +
Hamburg	1957	-1944	1945 - 1947	1948 +
Hesse	1947	-1933	1934 - 1937	1938 +
Lower- Saxony	1959	-1946	1947 - 1949	1950 +
NRW	1959	-1946	1947 - 1949	1950 +
Rhineland-Palatinate	1962	-1949	1950 - 1952	1953 +
Saarland	1959	-1946	1947 - 1949	1950 +
S.-Holstein	1952	-1939	1940 - 1942	1943 +

Source: Own calculations based on abolition dates summarized by Benatzky (2001), see Appendix A.

Table 2 Average Demography-Corrected Advanced School Enrollment Rates Before and After the Abolition of School Fees

	Rhineland-Palatinate	Hamburg	Lower Saxony	Baden Württemberg	NRW	Saarland	All
5 years prior	0.109	0.093	0.094	0.111	0.092	0.088	0.092
5 years after	0.145	0.108	0.102	0.125	0.119	0.112	0.113
Abs. Diff.	0.036	0.015	0.008	0.014	0.026	0.024	0.021
Rel. Diff. (%)	33.2	16.5	8.3	12.2	28.4	26.7	22.9

Note: The figures present average enrollment rates in the 5 years preceding and following the abolition of school fees in each of the states. Since abolition happened at different points in time, different calendar years are covered by each column (for Rhineland-Palatinate years 1958-68, for Hamburg and Baden Württemberg years 1953-63, and for the remaining years 1955-1665). The last column provides an average over the 6 states.
The absolute difference is the difference between the two entries, the relative difference calculates the percent increase in enrollment rates after the fee abolition.

Sources:

(i) Pupils in Advanced School by state and year: Federal Statistical Office, *Zeitreihen zur Fachserie 11, Reihe 1*.

(ii) Population aged 10-19 by state and year: State Statistical Offices.

Table 3 Average Advanced School Enrollment Rates Before and After the Abolition of School Fees by State

Cohort	All	S. Hol.	Ham-burg	L. Sax.	Bre-men	NRW	Hesse	R.Pa-latin.	B. Württ.	Bava-ria	Saar-land	Berlin
Fee	0.08	0.08	0.12	0.08	0.07	0.08	0.07	0.08	0.09	0.07	0.07	0.15
No fee	0.18	0.15	0.28	0.17	0.16	0.18	0.18	0.15	0.19	0.15	0.14	0.26
Abs. Diff.	0.10	0.07	0.16	0.09	0.09	0.10	0.11	0.07	0.10	0.08	0.07	0.11
Rel. Diff.	1.25	0.88	1.33	1.13	1.29	1.25	1.57	0.88	0.90	1.14	1.00	0.73

Notes: 1. The cohort group "fee" describes the average Advanced School participation of a state's population born between 1930 and the last cohort that had to pay the fee (the "pre" group of Table 1). The cohort group "no fee" describes the average Advanced School participation of a state's population born as the first cohort not having to pay a fee up until 1959 (the "post" group of Table 1).

2. Abs.Diff. describes the percentage point difference between the two averages, Rel.Diff describes the percent increase in average participation rates: (Abs.Diff / Fee) - 1.

Source: Own calculations based on Mikrozensus data 1989, 1993, 1997.

Table 4 Estimation and Simulation Results - Logit on Advanced School Enrollment Original Sample

	1		2		3		4	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
A: Estimation Results								
pre (reference)	-	-	-	-	-	-	-	-
β_1: trans	0.040	0.028	0.005	0.209	0.036	0.028	-0.478	0.746
β_2: post	0.070	0.034*	0.455	0.153**	0.069	0.036○	0.513	0.233*
γ: cohort	0.050	0.002**	-	-	-	-	-	-
γ_1: pre * cohort	-	-	0.055	0.003**	-	-	-	-
γ_2: trans * cohort	-	-	0.055	0.004**	-	-	-	-
γ_3: post * cohort	-	-	0.046	0.002**	-	-	-	-
δ: state FE (10)	yes	**	yes	**	yes	**	yes	**
γ^o: state * cohort (11)	-	-	-	-	yes	**	-	
γ_j^o: state * cohort * (pre/trans/post) (33)	-	-	-	-	-	-	yes	**
α: constant	yes	**	yes	**	yes	**	yes	**
Log Likelihood	-162,356.4		-162,345.5		-162,311.8		-162,278.7	
B: Simulation of Enrollment Change from Pre to Post Cohorts								
Fee effect - Full sample	6.3%**		5.2%**		6.2%**		6.6%**	
	(0.0193)		(0.0195)		(0.0205)		(0.0217)	
Fee effect - Females	6.7%*		6.5%*		5.5%		6.4%○	
	(0.0311)		(0.0320)		(0.0350)		(0.0384)	
Fee effect - Males	6.0%**		4.4%		6.5%*		6.9%*	
	(0.0268)		(0.0264)		(0.0276)		(0.0297)	

Notes:

1. In Panel A, **, *, and ○ indicate statistical significance of the coefficients at the 1, 5, and 10 percent level. Standard errors are corrected for clusters at the state-birth cohort level. In Panel B, **, * and ○ refer to the statistical significance of the predicted enrollment changes. Here standard errors (in parentheses) are bootstrapped using 100 repeated draws from the original data.
2. The figures in parentheses in the very first column represent the number of coefficients estimated for the particular group of covariates.
3. The full sample fee effect is based on the above represented coefficient estimates. The model was reestimated for the 2 sexes to generate the predictions for males and females. Coefficient estimates for these are not presented to save space.
4. The simulations provide the percent increase in the predicted probability of Advanced School attendance for youth born in the middle of the trans-group ("index cohort"). For this birth cohort we predicted the hypothetical probability of Advanced School enrollment if they were part of the post and of the pre group. The fee effect is the average of the following expression over all observations:

   [Pr (S=1 | index cohort, post =1) / Pr (S=1 | index cohort, pre =1)] - 1
5. The estimation on the full sample used 433,315 observations, the regressions for females and males 217,248 and 216,067 observations, respectively.

Source: Own calculations based on Mikrozensus data 1989, 1993, 1997.

Table 5 Estimation and Simulation Results - Logit on Advanced School Enrollment Modified Sample

	1		2		3		4	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
A: Estimation Results								
pre (reference)	-	-	-	-	-	-	-	-
β_1: trans	0.044	0.028	0.073	0.208	0.054	0.028*	-0.105	0.767
β_2: post	0.072	0.036*	0.557	0.135**	0.080	0.034*	0.786	0.159**
γ: cohort	0.049	0.002**	-	-	-	-	-	-
γ_1: pre * cohort	-	-	0.059	0.003**	-	-	-	-
γ_2: trans * cohort	-	-	0.057	0.004**	-	-	-	-
γ_3: post * cohort	-	-	0.047	0.002**	-	-	-	-
δ: state FE (10)	yes	**	yes	**	yes	**	yes	**
γ^o: state * cohort (11)	-	-	-	-	yes	**	-	
γ_j^o: state * cohort * (pre/trans/post) (33)	-	-	-	-	-	-	yes	**
α: constant	yes	**	yes	**	yes	**	yes	**
Log Likelihood	-182,581.3		-182,561.94		-182,516.78		-182.471.52	
B: Simulation of Enrollment Change from Pre to Post Cohorts								
Fee effect - Full sample	6.5%**		2.7%		7.3%**		2.7%	
	(0.0161)		(0.0195)		(0.0205)		(0.0217)	
Fee effect - Females	8.4%*		5.8%		8.8%*		3.4%	
	(0.0377)		(0.0392)		(0.0385)		(0.0404)	
Fee effect - Males	6.4%*		1.0%		7.3%**		2.3%	
	(0.0266)		(0.0275)		(0.0276)		(0.0301)	

Notes:

1. In Panel A, **, *, and ∘ indicate statistical significance of the coefficients at the 1, 5, and 10 percent level. Standard errors are corrected for clusters at the state-birth cohort level. In Panel B, **, * and ∘ refer to the statistical significance of the predicted enrollment changes. Here standard errors (in parentheses) are bootstrapped using 100 repeated draws from the original data.
2. The figures in parentheses in the very first column represent the number of coefficients estimated for the particular group of covariates.
3. The full sample fee effect is based on the above represented coefficient estimates. The model was reestimated for the 2 sexes to generate the predictions for males and females. Coefficient estimates for these are not presented to save space.
4. The simulations provide the percent increase in the predicted probability of Advanced School attendance for youth born in the middle of the trans-group ("index cohort"). For this birth cohort we predicted the hypothetical probability of Advanced School enrollment if they were part of the post and of the pre group. The fee effect is the average of the following expression over all observations:

   [Pr (S=1 | index cohort, post =1) / Pr (S=1 | index cohort, pre =1)] - 1
5. The estimation on the full sample used 462,120 observations, the regressions for females and males 231,247 and 230,873 observations, respectively.

Source: Own calculations based on Mikrozensus data 1989, 1993, 1997.

Table 6a Simulation Results of Enrollment Change from Pre to Post Cohort Based on Estimations with Income Controls
(Predicted Fee Effect in Percent)

Specification:	1	2	3	4
Original Sample (N = 433,315)				
Full sample	6.2% **	6.2% **	5.3% *	6.8% **
	(0.0193)	(0.0203)	(0.0202)	(0.0217)
Females	6.8% *	6.2% ○	5.4%	6.5% ○
	(0.0318)	(0.0322)	(0.0356)	(0.0388)
Males	5.7% *	5.8% *	5.2% *	6.9% *
	(0.0243)	(0.0247)	(0.0250)	(0.0271)
Modified Sample (N = 462,120)				
Full sample	4.6% *	3.7%	4.5% *	3.4%
	(0.0188)	(0.0220)	(0.0202)	(0.0240)
Females	8.0% **	5.7% ○	7.8% *	3.2%
	(0.0307)	(0.0301)	(0.0314)	(0.0336)
Males	3.1%	2.7%	3.3%	3.3%
	(0.0252)	(0.0257)	(0.0249)	(0.0292)

Table 6b Simulation Results of Enrollment Change from Pre to Post Cohort Based on Estimations with Controls for Income Growth
(Predicted Fee Effect in Percent)

Specification:	1	2	3	4
Original Sample (N = 433,315)				
Full sample	6.1% **	6.6% **	5.3% *	6.8% **
	(0.0218)	(0.0223)	(0.0230)	(0.0242)
Females	6.7% *	7.5% *	5.1% ○	7.2%
	(0.0311)	(0.0329)	(0.0305)	(0.0459)
Males	5.7% *	5.8% *	5.3% *	6.6% *
	(0.0225)	(0.0233)	(0.0244)	(0.0258)
Modified Sample (N = 462,120)				
Full sample	4.4% *	1.0%	4.9% *	-
	(0.0215)	(0.0229)	(0.0227)	
Females	8.2% *	5.1%	9.6% **	-
	(0.0324)	(0.0365)	(0.0368)	
Males	2.8%	-1,3%	3.7%	-
	(0.0242)	(0.0240)	(0.0275)	

Notes: See Tables 4 and 5.
The estimation in column (4) for the modified sample yielded unreasonably large predicted fee effects which are most likely due to collinearity among the regressors.

Source: Own calculations based on Mikrozensus data 1989, 1993, 1997.

Table 7 Simulation Results of Enrollment Change from Pre to Post Cohort Based on Estimations with Different Specifications (Predicted Fee Effect in Percent)

Specification:	1	2	3	4
A. Quadratic Instead of Linear Cohort Controls				
Original full sample	4.9% *	< 0%	5.5% *	-
	(0.021)	(0.083)	(0.028)	
Original female sample	6.5% *	< 0%	5.2%	-
	(0.031)	(0.069)	(0.033)	
Original male sample	3.9% ○	< 0%	5.9% *	-
	(0.021)	(0.145)	(0.029)	
Modified full sample	1.9%	< 0%	3.5%	-
	(0.020)	(0.108)	(0.022)	
Modified female sample	4.5%	< 0%	5.3%	-
	(0.034)	(0.068)	(0.038)	
Modified male sample	0.2%	< 0%	2.4%	-
	(0.025)	(0.145)	(0.029)	
B. Adding a Full Set of T-1 Birth Cohort Fixed Effects				
Original full sample	1.3%	0.4%	< 0%	1.1%
	(0.021)	(0.040)	(0.023)	(0.140)
Original female sample	4.2%	3.1%	2.3%	3.3%
	(0.034)	(0.072)	(0.036)	(0.230)
Original male sample	< 0%	< 0%	< 0%	< 0%
	(0.021)	(0.043)	(0.022)	(0.172)
Modified full sample	1.0%	< 0%	0.02%	0.1%
	(0.021)	(0.048)	(0.023)	(1.452)
Modified female sample	3.5%	2.2%	1.1%	2.2%
	(0.035)	(0.068)	(0.037)	(0.163)
Modified male sample	< 0%	< 0%	< 0%	< 0%
	(0.027)	(0.052)	(0.029)	(0.163)

Notes:
1. See Tables 4 and 5.
2. Each cell entry represents the prediction from a separate regression.
3. In Panel B, T-1=29 fixed effects were added with the original sample specification, and T-1 = 38 fixed effects were added with the modified sample specification.

Source: Own calculations based on Mikrozensus data 1989, 1993, 1997.

Table 8 Estimation and Simulation Results - Logit on Advanced School Enrollment Controlling for the Actual Number of Years of Fee Payment (feeyears)

	1		1		3		3	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
A: Estimation results (Full sample)								
Original Sample								
feeyears	-0.015	0.004**	-0.008	0.003*	-0.020	0.005**	-0.006	0.004
birth cohort FE	no		yes		no		yes	
Modified Sample								
feeyears	-0.017	0.007**	-0.009	0.007	-0.020	0.005**	-0.009	0.007
birth cohort FE	no		yes		no		yes	
B: Simulation of enrollment difference with 0 vs. 10 years of fee payment (Original sample)								
Fee effect - Full sample	14.2%**		7.2%**		18.9%**		5.2%○	
	(0.026)		(0.025)		(0.037)		(0.039)	
Fee effect - Females	9.5%**		9.8%*		8.5%○		7.4%	
	(0.034)		(0.045)		(0.047)		(0.068)	
Fee effect - Males	18.6%**		5.6%○		28.0%**		3.5%	
	(0.032)		(0.029)		(0.050)		(0.044)	
C: Simulation of enrollment difference with 0 vs. 10 years of fee payment (Modified sample)								
Fee effect - Full sample	15.7%**		7.6%**		18.4%**		8.3%	
	(0.028)		(0.028)		(0.032)		(0.050)	
Fee effect - Females	13.4%**		10.0%○		14.3%**		3.2%	
	(0.044)		(0.054)		(0.046)		(0.081)	
Fee effect - Males	19.8%**		6.1%*		24.8%**		10.1%○	
	(0.031)		(0.031)		(0.035)		(0.063)	

Notes:

1. In Panel A, **, *, and ○ indicate statistical significance of the coefficients at the 1, 5, and 10 percent level. Standard errors are corrected for clusters at the state-birth cohort level. In Panel B, **, * and ○ refer to the statistical significance of the predicted enrollment changes. Here standard errors (in parentheses) are bootstrapped using 100 repeated draws from the original data.
2. All estimated models control for a set of state fixed effects. In column 1 a linear birth cohort indicator was controlled, in column 1' it was replaced by indicator variables for each separate birth cohort year. Column 3 controls for state specific linear cohort trends and column 3' adds indicator variables for each separate birth cohort year to this specification.
3. The full sample effect is predicted based on the above represented coefficient estimates. The model was reestimated for the 2 sexes to generate the predictions for males and females. Coefficient estimates for these are not presented to save space.
4. The simulations provide the percent increase in the predicted probability of Advanced School attendance in a situation with zero compared to ten years of fee payment The fee effect is the average of the following expression over all sample observations (and states):
   [Pr (S=1 | feeyears =0) / Pr (S=1 | feeyears =10)] - 1
5. For the original sample we use 433,315 observations, for the modified sample 462,120.

Source: Own calculations based on Mikrozensus data 1989, 1993, 1997.

Table 9 Estimation and Simulation Results - Logit on Advanced School Enrollment

	1		2		3		4		5		6	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
A: Estimation results (Original sample)												
income ratio	-0.77	0.26**	-0.83	0.27**	-0.64	0.25*	-0.62	0.23**	-0.69	0.26**	-0.43	0.23○
state gdp p.c.	yes	○	yes		yes	*	yes	*	yes	○	yes	○
cohort	yes	**	yes	*	yes	**	-		-		-	
cohort^2	-		yes		yes	**	-		-		-	
cohort^3	-		-		yes	**	-		-		-	
state FE * cohort (10)	-		-		-		yes	**	yes	**	yes	**
state FE * cohort^2 (10)	-		-		-		-		yes	*	yes	**
state FE * cohort^3 (10)	-		-		-		-		-		yes	**
state FE (9)	yes	**	yes	**	yes	**	yes	**	yes	**	yes	*
α: constant	yes	**	yes	**	yes	**	yes	**	yes	**	yes	
Log Likelihood		-159,802.2		-159,801.9		-159,767.4		-159,756.4		-159,741,0		159,700.6
B: Simulation of change in income ratio from 10 to 5 percent (Original sample)												
Fee effect - Full sample	3.3%	0.01**	3.6%	0.01**	2.7%	0.01**	2.7%	0.01**	3.0%	0.01**	1.8%	0.01○
Fee effect - Females	4.0%	0.01**	4.3%	0.02*	3.6%	0.01**	3.3%	0.02*	3.3%	0.02○	2.2%	0.02
Fee effect - Males	3.0%	0.01**	3.0%	0.01**	2.0%	0.01○	2.4%	0.01*	2.7%	0.01*	1.4%	0.01
C: Simulation of change in income ratio from 10 to 5 percent (Modified sample)												
Fee effect - Full sample	2.9%	0.01**	3.1%	0.01**	3.6%	0.01**	2.3%	0.01**	3.5%	0.01**	3.1%	0.01*
Fee effect - Females	4.2%	0.02**	2.9%	0.02○	3.8%	0.02*	3.8%	0.02*	2.9%	0.02	2.7%	0.02
Fee effect - Males	2.3%	0.01*	2.8%	0.01*	3.2%	0.01**	1.9%	0.01	3.6%	0.01*	3.2%	0.01*

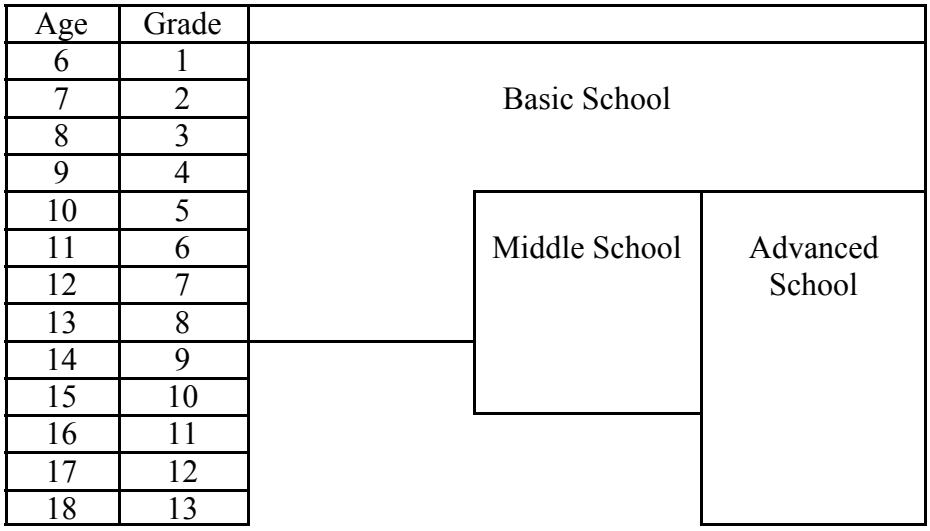
Notes:

1. In Panel A **, *, and ○ indicate statistical significance of the coefficients at the 1, 5, and 10 percent level. Standard errors are corrected for clusters at the state-birth cohort level. In Panels B and C, **, *, and ○ refer to the statistical significance of the predicted enrollment changes, which was calculated using bootstrapped standard errors based on 100 replications.
2. The figures in parentheses in the first column represent the number of coefficients estimated for the particular group of covariates.
3. The estimation on the original full sample used 425,323 observations, the regressions for females and males 213,215 and 212,108 observations, respectively. In the modified sample there are 450,594 observations in total of which 225,453 are female and 225,141 are male.

4. Each fee effect is estimated using sample-specific coefficient estimates.
5. The simulations provide the percent increase in the predicted probability of Advanced School attendance for youth comparing a scenario where 10 and 5 percent of incomes are to be paid for school fees. The predicted individual enrollment rates in both scenarios are averaged over the sample yielding in both panels: [Pr (S=1 | income ratio = 0.05) / Pr (S=1 | income ratio = 0.10)] - 1
6.

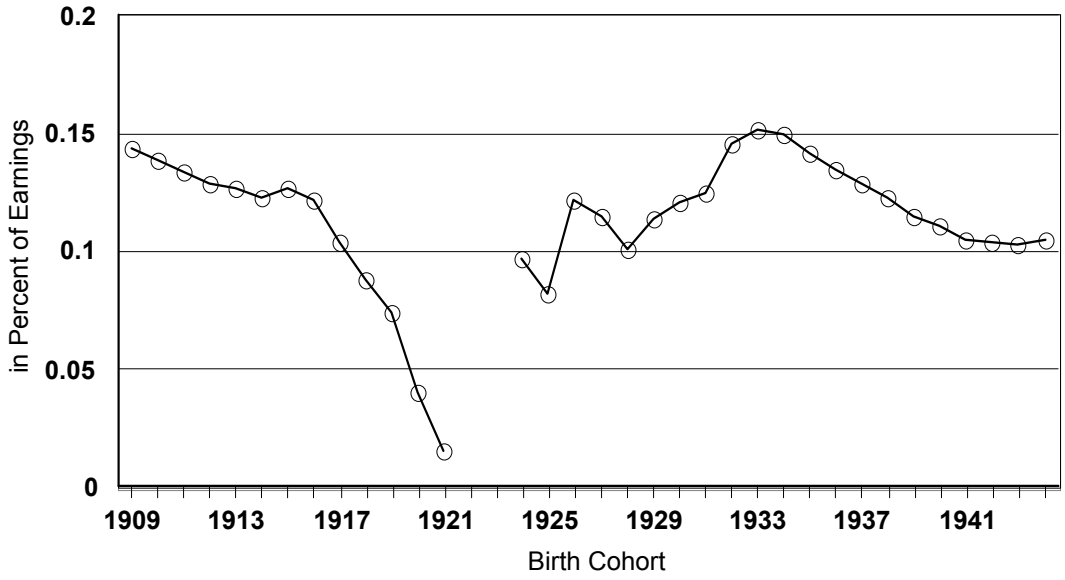
Source: Own calculations based on Mikrozensus data 1989, 1993, 1997.

Figure 1 Sketch of the traditional German schooling system



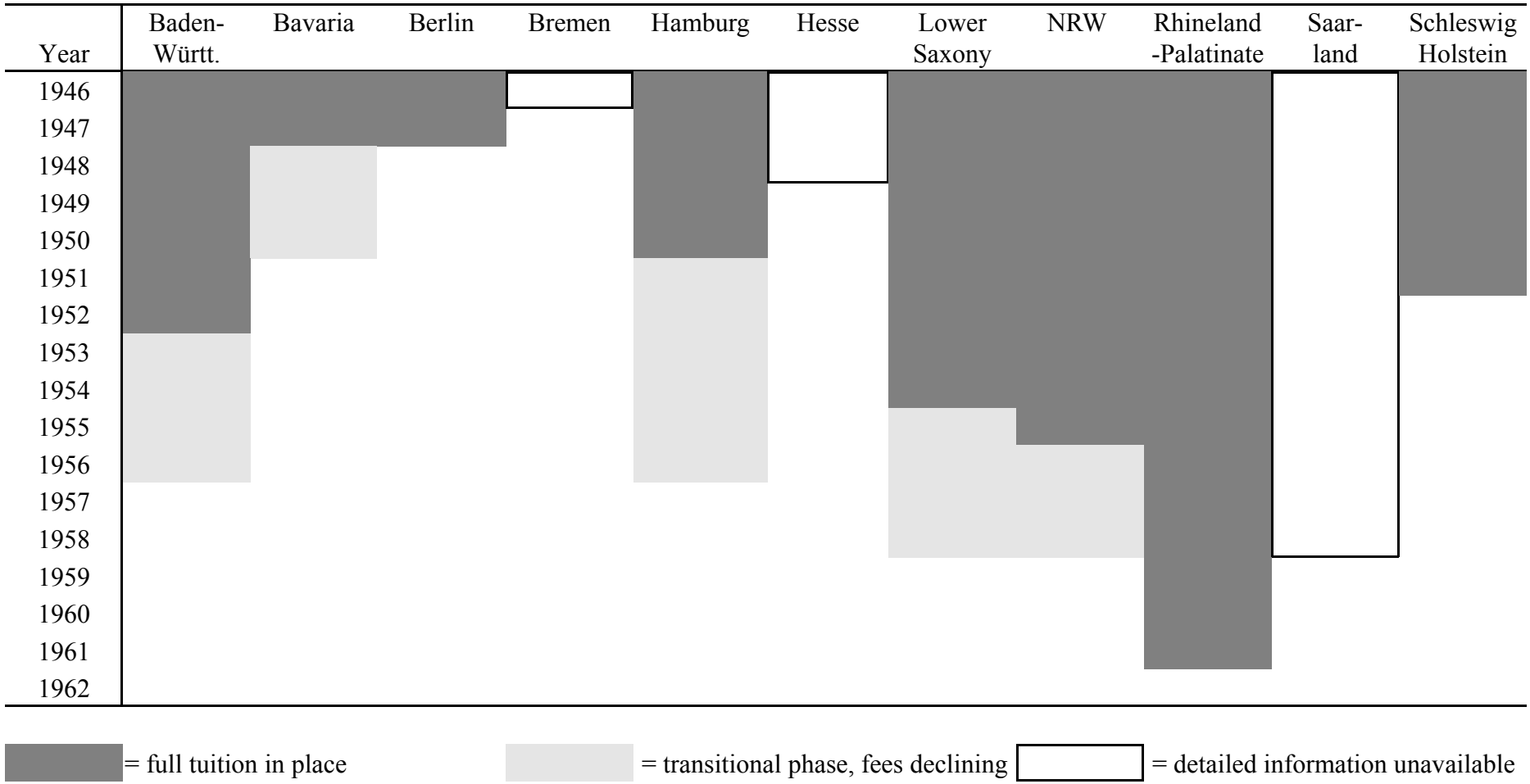
Source: Own presentation

Figure 2 Prussian School Fees for Advanced Schools as Share of Average Earnings



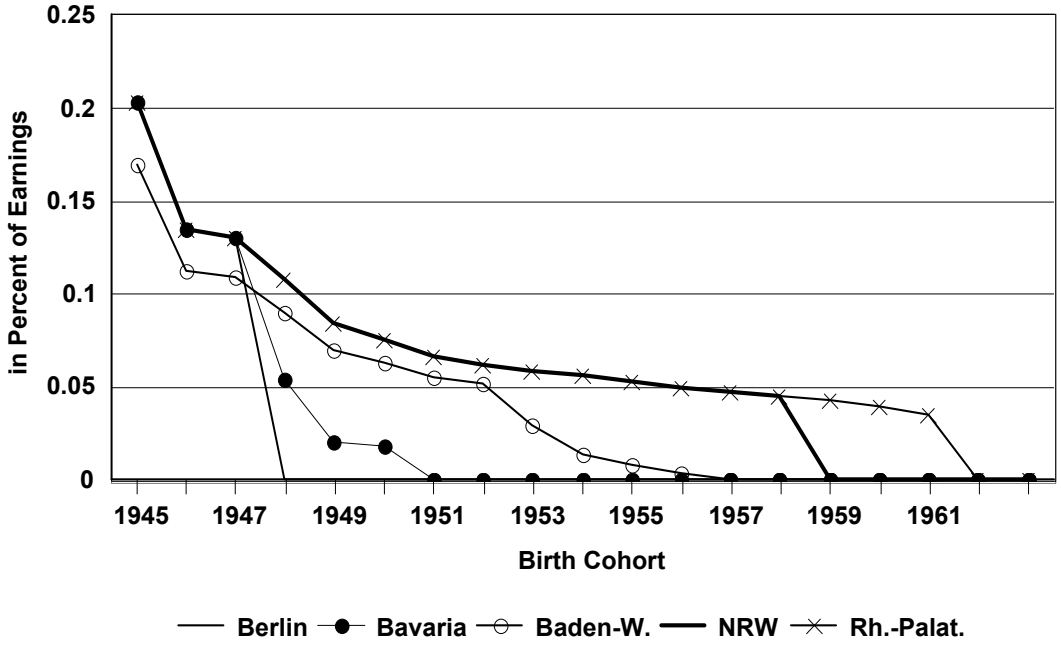
Source: (i) Earnings data from retirement insurance statistics (annual average gross earnings: www.bfa.de 2001). Figures not available during high inflation years 1922 and 1923.
(ii) Tuition fees from various sources (Benatzky 2001).

Figure 3 State-Specific Patterns of Tuition Abolition



Source: Own graphical representation of fee information in Benatzky (2001).

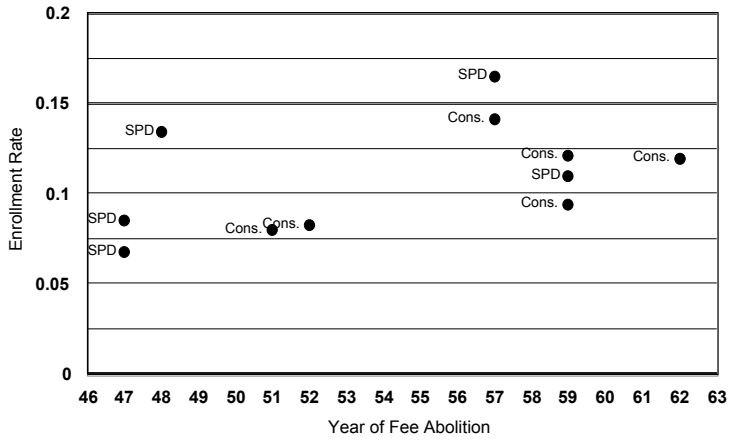
Figure 4 School Fees for Advanced School as Share of Average Earnings (Selected States)



Source: Earnings data from retirement insurance statistics
Tuition fees from various sources (Benatzky, 2001).

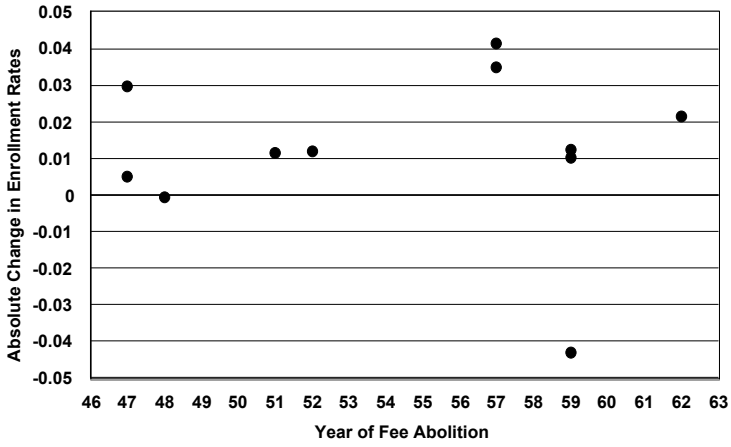
Figure 5 Enrollment Rates and Timing of Fee Abolition

(a) Enrollment rate in the last birth cohort certainly paying a fee

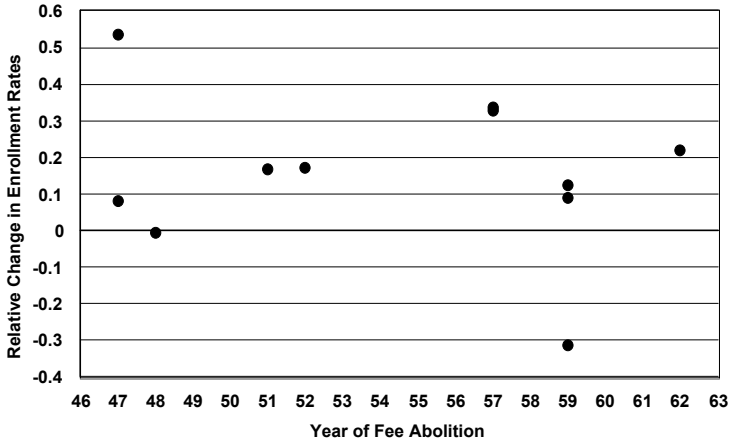


Note: The data labels indicate whether state government passing the fee abolition was run by socialdemocrats (SPD) or conservatives (Cons.).

(b) Absolute change in enrollment rates for the last three birth cohorts certainly paying a fee



(c) Relative change in enrollment rates for the last three birth cohorts certainly paying a fee



Source: Own calculations based on Mikrozensus data 1989, 1993, 1997.

Figure 6 Representation of Fee Effects

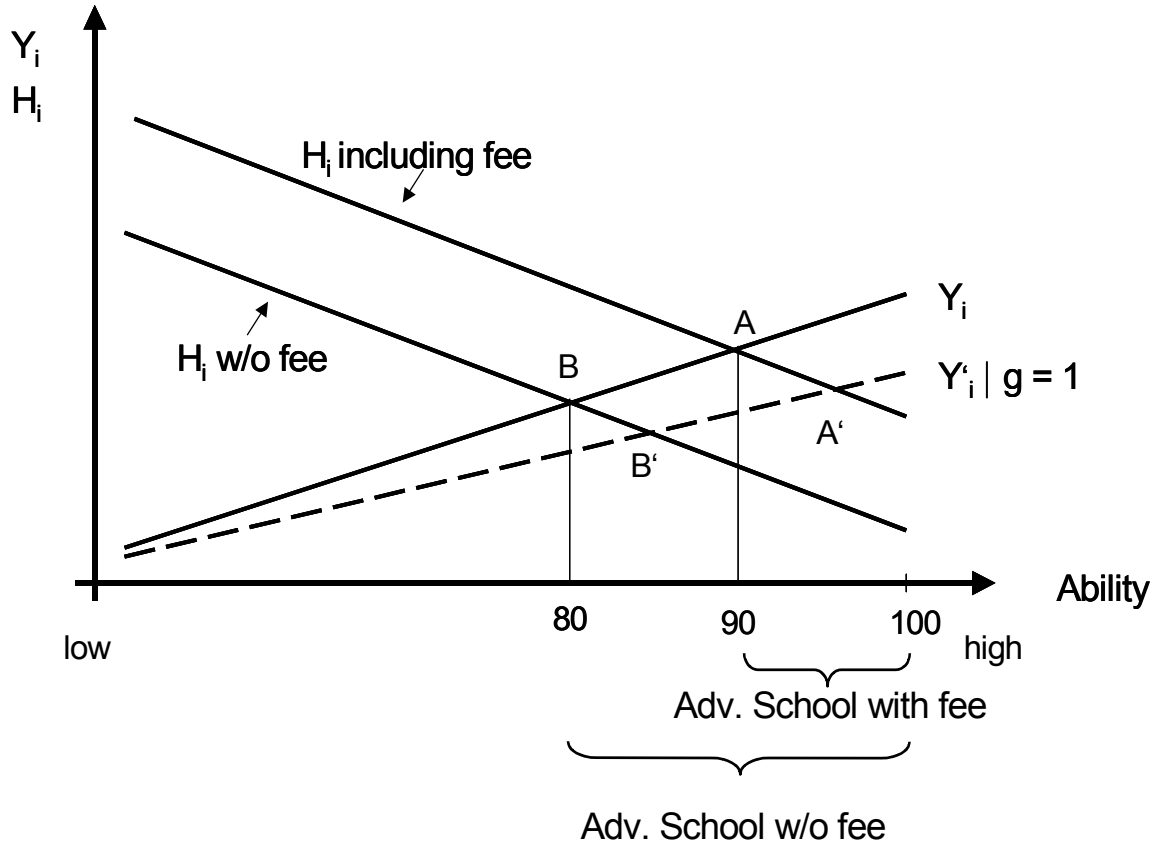
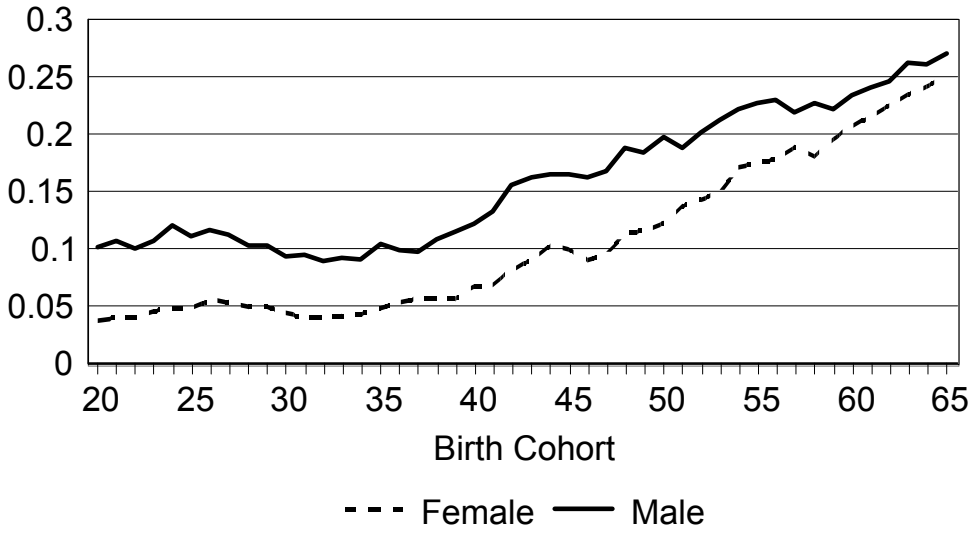
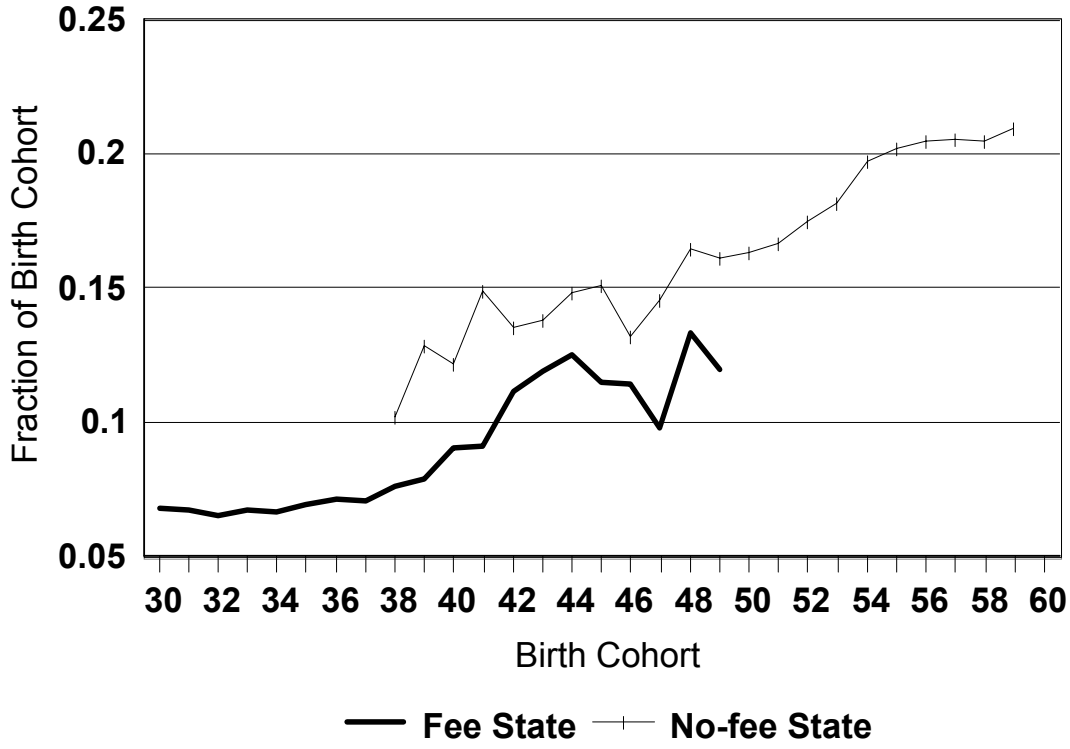


Figure 7 Population Shares with Advanced School Degree by Birth Cohort



Source: Own calculations based on Mikrozensus data 1989, 1993, 1997.

Figure 8 Advanced School Enrollment by Fee Status of State
(Reflects enrollment of pre and post cohort groups)



Note: The depicted fraction of birth cohorts enrolled in Advanced Schools in fee states is the average enrollment rate among individuals of a given birth cohort that reside in a state where they certainly would have had to pay a fee upon entering Advanced School (the "pre abolition" cohorts). The depicted fraction of birth cohorts enrolled in no-fee states presents the average enrollment rate among individuals of a given birth cohort that reside in a state where they certainly would not have had to pay a fee upon entering Advanced School (the "post abolition" cohorts).

Source: Own calculations based on Mikrozensus data 1989, 1993, 1997.

Appendix A: Assignment of State-Specific Cohort Groups

(1) For every state we distinguish three groups of cohorts based on whether they had to pay fees when entering Advanced School (fifth grade):
- Cohorts, which certainly did not pay a fee ("pre" group)
- Cohorts, for which we cannot be sure ("trans" group)
- Cohorts, which certainly had to pay a fee ("post" group)

(2) Cohorts were coded as "pre" cohorts if they were born at least 13 years prior to the abolition year. Cohorts were coded as "post" cohorts if they were born at most 10 years prior to the abolition year.

(3) Exceptions are made for three states, which abolished fees early (Berlin 1948, Bremen 1947, and Hesse 1947). Because educational biographies were strongly affected by the war (e.g. no schools operating, refugees transferred between *available* school types, additional repeat classes due to general stress) I increased the range of uncertainty for those cohorts who were supposed to pass primary schools during the later years of the war. Because the war may have delayed entry to the Gymnasium for these cohorts I shift one additional cohort from the "pre" to the "trans" group for these states. So for these states cohorts were coded as "pre" cohorts if they were born at least 14 years prior to the abolition year.

(4) Example:
In general the age of entry at the Basic School is between 5 and 7. The transfer to Advanced School (after 4 years) takes place between ages 9 and 11. As pupils may have to repeat a grade we consider ages 9-12 for possible transitions.

For the case of Baden Württemberg, where fees were abolished in 1957 we obtain:
Cohorts 1944 and prior certainly had to pay fee ("pre").
Cohorts 1948 and later certainly did not have to pay a fee ("post").
Cohorts 1945 - 1947 are in a transition period ("trans").

(5) Experimentation with differently structured school systems:
In four federal states educational policies after the war experimented with the structure of the schooling system by postponing the transition from primary education to Middle and Advanced Schools (cf. Klafki 1976, p.266): In Schleswig-Holstein the transition was postponed to grade 7 during the years 1948-1950. In Hamburg and Bremen this was the case between 1949 and 1957. In Berlin the transition to Advanced or Middle School was postponed to grade 9 between 1948 and 1950, and starting in 1951 only to grade 7.
Given our definition of the cohort groups and the respective years of fee abolition, the postponement of the transition to Advanced School has no effect on the cohort group definition for all states but Hamburg. For the case of Hamburg the postponement would require us to revise the definition of the pre and post groups. However, it is not altogether clear how strictly this postponement was executed: Kuhlmann (1970, p.31) points out that in many cases Advanced School type education continued to commence after four years of basic school. Therefore, we maintain the overall definition for the case of Hamburg as well. The results are robust to this choice.

Appendix B: Simple Theoretical Framework

Here we derive the effect of changes in C (the direct cost of schooling) as well as of different returns to education and ability that may exist for the two sexes on the propensity to attend Advanced School (S*) as well as on the critical ability level (A*) beyond which school enrollment is the preferred choice (see Figure 6).

Assume

(1) $S_i^* = Y(S_i, A_i; \mu_i) - H(S_i, A_i, C; \nu_i)$

with Y the discounted utility of lifetime earnings and H the discounted disutility deriving from Advanced School participation. Let Y and H depend on school enrollment (S_i) where we assume an increasing concave function for Y and an increasing convex function for H. Y and H both may also vary with a pupil's ability (A), which may yield higher earnings advantages and lower disutility from additional schooling. C represents the cost of Advanced School participation which is positively correlated with the utility loss due to school participation. μ_i and ν_i are person-specific effects.

If we chose the simplest linear specification:

(2) $Y_i = \mu_i + b_1 S_i + b_2 A_i$ with $b_1 , b_2 > 0$

(3) $H_i = \nu_i + c_1 S_i + c_2 A_i + C$ with $c_1 > 0$ and $c_2 < 0$

we obtain

(4) $S_i^* = (\mu_i - \nu_i) + (b_1 - c_1) S_i + (b_2 - c_2) A_i - C$

$$+ \quad + \quad - \quad +$$

and it follows immediately $S^* = S^* (b_1 , b_2 , C, \mu)$.

Also it follows that in this model higher ability individuals obtain more schooling. To evaluate the determinants of the 'critical ability level' (A*) beyond which school enrollment is preferable, we solve (2) and (3) for :

(5) $A^* = [b_1 / (c_2 b_1 - c_1 b_2)] [H_i - \nu_i - (c_1 / b_1) Y_i + (c_1 / b_1) \mu_i - C]$.

$$? \quad - \quad + \quad -$$

Now we can derive $A^* = A^* (b_1 , b_2 , C, \mu)$.

(where $\partial A^* / \partial b_1 < 0$ if $b_1 > 1$, which we do not know).

These results support the hypothesis that higher returns to ability and possibly to schooling are correlated with a higher propensity to attend Advanced School and a lower critical ability level. Also, individuals with high 'person-specific earnings effects' (μ) have a higher propensity to attend Advanced School and a lower critical ability level. The gender wage gap might be an example of differences in average person-specific earnings effects.