The effect of secondary school fees on educational attainment

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Abstract

This study utilizes the heterogeneity of the fee abolition process in West German secondary schools to identify its effect on educational attainment and to obtain an estimate of the price elasticity of demand for upper secondary 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 on average Advanced School participation increased at best minimally in response to the fee abolition. Females' educational attainment is more price sensitive than males'. A fifty percent reduction in fees is associated with an overall increase in graduation rates by no more than 4 percent. The elasticity of the demand for females' education exceeds that for males'.

Key Words: school fees, tuition, demand for education, educational attainment, natural experiment
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1. Introduction

We do not know how school fees affect demand for secondary education. Yet, in times of tight public budgets and continued discussion of private secondary schools, informed political decisions require reliable evidence regarding the price sensitivity of education demand. In this study I take advantage of a natural experiment in post-war Germany to identify and estimate both, the effect of school fees on secondary school attainment and the price elasticity of demand for secondary education.

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, these fees were abolished state by state between 1947 and 1962. The variation in the timing of fee abolition across states identifies the effect of fees on graduation probabilities. I evaluate the price responsiveness of education demand for male and female youth.

This ties in with a number of debates in the literature. Goldin (1998) and Goldin and Katz (2003, 2004) discuss the United States' high school movement between 1910 and 1940: whereas secondary school enrollment in Europe remained low and about constant, high school enrollment in the U.S. rose from 18 to 73 percent and the fraction of graduating youth soared from 9 to 51 percent. In contrast, as of 1940 less than 10 percent of German birth cohorts were eligible to start tertiary education. To explain the different developments Goldin and Katz (2004) focus on the U.S.'s more equal distribution of income and wealth at that time and on decentralized decision making on schools. Also, they point out that college education in some states was highly subsidized which increased the return to high school degrees early on. This argument neglects that in contrast to the U.S. college bound secondary schooling in Europe was rationed by substantial fees and provided in an institutional setting of strictly defined tracks (see e.g. Leschinsky and Mayer 1990). I investigate whether the existence of school fees for secondary schools in Europe - respectively their absence in U.S. high schools - contributes to explain the

different developments of educational attainment on both sides of the Atlantic.

An investigation of the cost of secondary education and its behavioral effects may be informative for the discussion of school vouchers in the United States.[1] Also, our results are of interest to the debates on the effect of tuition subsidies in the United States and of the introduction of university fees in Europe. These debates generally lack reliable measures of the price elasticity of education demand: in Europe, data on prior experiences with academic fees are often unavailable, and in the U.S., the measurement of the effect of public aid on college enrollment is hampered by the potential endogeneity of aid receipt.[2] Finally, this study on the role of school fees for educational attainment is related to a growing literature that investigates measures to increase school attendance in developing[3] as well as in industrialized countries.[4]

The analysis below first investigates the responsiveness of education demand to the mere existence of a school fee, and then it evaluates the sensitivity of educational attainment to changes in the price of education. I find surprisingly small and typically insignificant effects of school fee abolition. The responsiveness of the demand for female education slightly exceeds that of the demand for male education. A drop in the fee-to-income ratio by 50 percent or five percentage points increases participation by no more than 4 percent again with somewhat larger effects for females than for males.

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[1] See e.g. Ladd (2002), Neal (2002), Epple and Romano (1998), Epple et al. (2004).

[2] 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).

[3] Examples are Vermeersch and Kremer (2004) 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.

[4] See Meghir and Palme (2005) 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.

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 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 individuals' (latent) propensity to complete 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.[5] 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

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

Clearly, a reduction in C will increase the probability of Advanced School attendance. 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 $b_2^{g=1} > b_2^{g=0}$, it follows that

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

[5] 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. In contrast the less able may need additional time and tutoring to meet requirements.

Figure 1 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 1 everybody to the right of point A will attend 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 1).

If we hypothesize further, that for parts of the population such as females the expected benefits at all ability levels are below the average[6] - 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 participation increase may exceed that of males (for details on the theoretical analysis see Appendix A).

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

H1: Advanced School attainment increases after the abolition of school fees.
H2: Advanced School attainment in states without fees exceeds that of states with fees.
H3: Advanced School attainment for males exceeds that of females.
H4: The increase in Advanced School attainment is stronger for females than males.

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

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

3. Historical and Institutional Background on School Fees

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 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*),[7] where education continued for an additional 6 or 9 years, respectively (cf. Figure 2). The system hardly changed over time, and the Advanced School degree remained the key requirement for university studies.[8]

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 (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 3 depicts the share of school fees in average income for the case of Prussia. It reflects nominally rising earnings during the inflation (1919-1923) when fees remained unadjusted. Around 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 in 1941 it was at times shortened by an additional 6 months. Also, for the birth cohorts 1922-1928 final examination requirements were reduced and frequently dropped completely to facilitate military service.

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

[8] While today the degree can be attained via alternative educational pathways, such as polytechnical schools, these were not available in the past. Therefore the educational decision taken at the end of primary school was crucial.

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 4 describes the fee abolition pattern across the 11 federal states.[9] Frequently, tuition was abolished stepwise, e.g. annually by one seventh over the course of 7 years as in Hamburg, or in steps from 100, to 50, and 25 percent of the original amount as in Bavaria. Figure 5 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.

The abolition of school fees was not the only post-war development in the German educational system: beginning in the early 1960s and similar to other industrialized countries, the educational system expanded. This was due to more sizeable birth cohorts, rising demand for advanced education, as well as a broadening of access to education with increasing public investments in the education system.[10]

4. Data and Identification Strategy

4.1 Data Source, Sample, and Dependent Variable

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

[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).

The data are taken from the Mikrozensus, which is an annual survey of a one percent random sample of German households. Public use files provide 70 percent of the original data. Even though the Mikrozensus uses a rotating scheme which re-interviews the inhabitants of a given dwelling up to four times 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.[11]

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.

The dependent variable describes whether an individual completed Advanced School. Figure 6 describes the development of Advanced School graduation rates over subsequent cohorts. As suggested by Hypothesis 3 (H3) graduation 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.

For a first description of the development of Advanced School attainment in states with

[11] Even if panel data were available, given that our outcome variable does not change over time panel data would not add to the analysis.

and without school fees, Figure 7 depicts average graduation rates for birth cohorts for both groups of states. The graph shows that the average graduation rates in states without fees exceeds those in states with fees for any given cohort, confirming hypothesis 2 (H2).

For the analysis we distinguish three cohort groups based on the fee requirements pupils in a given state 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 before or after the abolition of school fees ("transitional group"). For these individuals we cannot tell for sure under which regime they attended school. 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 the cohort groups vary across states. They are described in Table 1. Appendix B provides additional explanations.

4.2 Identification Strategy

The simplest approach to identify the causal effect of fee abolition on Advanced School attainment rates would seem to be estimating the average difference in attainment before and after fee abolition. However, this neglects both, the heterogeneity in Advanced School attainment patterns across states and the trend in educational attainment over time, which is visible in Figure 6. A reliable difference-in-differences estimation therefore has to control for state and birth cohort effects. This suggests to model the probability of attaining an Advanced School degree (S) as:

$$\Pr(S_i = 1) = F(\alpha + \beta_1 \, trans_i + \beta_2 \, post_i + \gamma \, cohort_i + \delta \, state_i),$$

where *F* represents a cumulative distribution function, *cohort* represents the birth cohort, *trans* and *post* represent cohort group indicators, *state* stands for a vector of state fixed effects, and α,

β, γ, and δ are coefficients to be determined. The magnitude of the fee abolition effect can be determined once the estimation results are available. The validity of such an estimate of the causal fee abolition effect hinges on five conditions, which we discuss in turn.

As a first condition for fee abolition identifying the response of education demand to price changes we need to establish that the abolition is exogenous and not jointly determined with state school attendance. It is difficult to obtain historic accounts of the political processes leading to fee abolition.[12] However, we can investigate the correlation between the timing of fee abolition and its potential determinants, using a hazard model.

We generated a state-level panel dataset covering the 21 years from 1945 through 1965 for eleven states. After censoring state observations which already abolished their fees 114 state-year observations remain in the sample. The dependent variable is valued one in the year when states abolished fees and zero otherwise. This dichotomous indicator was regressed on indicators (i) for whether the state governor was a socialdemocrat (state governments are almost equally split between socialdemocrats and conservatives), (ii) of the average Advanced School graduation rate in the year of the fee abolition, its lagged values, and growth rates of graduation rates, and (iii) of per capita income, its lagged values, and growth rates. All specifications also contain a linear time trend and a constant. The models are estimated using a logit estimator and estimation results are presented in Table 2.

The first five columns separately consider five indicator groups. Except for the growth in enrollment rates all indicators are statistically insignificant.[13] The joint model is presented in

[12] An interesting description of the case of Bavaria is provided by Klafki (1976). In Bavaria U.S. occupation force insisted for political reasons on the abolition of school fees. Local politicians resisted this measure and managed to postpone its implementation for several years.

[13] Since information for state income is available for most states starting only 1950 the model is estimated with controls for missing values. For the states of Berlin and Saarland GDP information is available only starting 1960.

column 6. Again, only the growth in enrollment rates yields statistically significant coefficients. However, since the fee abolition process is decided on before the year of actual abolition the significant one period enrollment growth coefficient does not raise concern regarding the endogeneity of fee abolition. These results are robust to changing the considered number of lags and to using alternative estimators.

Appendix C depicts the share of Advanced School graduates by birth cohort separately for states which abolished fees early vs. late and grouped for youths in states with fees (pre abolition) and without fees (post abolition).[14] The depictions show a step up in advanced school attainment rates for both groups of states between the pre and post cohorts. Although Advanced School attainment was about equal for the early and late abolishing states, the rates continue to differ after the late abolishing states got rid of the fees. Over the considered birth cohorts the late abolishing states do not catch up in Advanced School graduation rates. Further graphs describe Advanced School participation rates, their relative change, lagged per capita state income, and growth in state income for each state's year of fee abolition. Since these scatter plots also hardly suggest systematic correlations I consider the timing of the state-wise abolition of fees as exogenous to Advanced School participation.

As a second condition for a reliable identification of the fee abolition effect we require exogenous assignment of individuals to federal states, i.e. that there is no self-selection into treatment. Such self-selection could occur if families move to no-fee states or send their children to schools across state borders. Both mechanisms would yield an inflated measure of the fee abolition effect within our estimation framework. While the problem of cross-border school

[14]We considered the states of Bremen and Hesse (1947), Berlin (1948), Bavaria (1952) and Schleswig-Holstein (1952) as early abolishers, whereas the states of Baden-Wurttemberg and Hamburg (1957), Lower-Saxony, Northrhine-Westfalia, and Saarland (1959), and Rhineland-Palatinate (1962) are the late abolishers.

attendance would be relevant only for a small share of the sample, the first issue would present a major challenge in a society that is characterized by high mobility. However, it appears that Germans are particularly sedentary: The German Socioeconomic Panel (GSOEP), an annual household panel survey, provides evidence on regional mobility. Respondents are asked whether they still live in the town where they were raised, which is obviously more restrictive than residing in the same state. Nevertheless, in 1985 (2001) 58 (55) percent of the respondents still lived in the town where they grew up. Also, only about five percent of the GSOEP respondents who were surveyed both in 1984 and in 2001 changed their federal state of residence in between. These figures are indicative of an immobile population. Similar evidence is provided by Pischke (2003), who shows that about 80 percent of all adult respondents to a large German social survey still live in their state of birth. Therefore, and because of the relatively low relevance of the fee expenditure and the short nature of this benefit of moving we consider self-selection into treatment to be unlikely.

A related issue that deserves mentioning is that our data only provide information on an individual's state of residence at the time of the survey. We do not know where an individual actually lived when attending school. If individuals moved randomly between states, this would cause an attenuation bias and would render 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. Since we have no evidence confirming such specific migration patterns and given low overall mobility, the effect of missing residential information is not likely to be large. Instead, the measurement error may downward bias our estimates.

A third condition for the correct identification of the treatment effect is that our estimates are not affected by omitted variables. Given the limitations of our data we are particularly

concerned about parental and household background characteristics and state level indicators. Omitted parental characteristics cause a systematic bias if they 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.[15] 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 after the parents of the youths considered here. Thus, the omission of parental controls should not substantially affect the nature of our estimates.

In order to account for the effect of permanent unobserved state level heterogeneity we consider state fixed effects in our specifications. In addition, we perform robustness tests on the impact of two potentially relevant time varying state level indicators, real per capita GDP as well es its growth rate.

A fourth set of issues that can affect the reliability of our estimates relates to three conditions and institutional regulations of the German post-war education system: (a) It is possible that admission requirements to attend Advanced School were not constant over time. Until today admission standards vary across federal states, with strict grade requirements in southern Germany and less restrictive systems in other states. If regulations changed exactly at the time of fee abolition, it would bias our estimate of the fee abolition effect.[16] However, there

[15] 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.

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

does not seem to be evidence for such developments.

(b) Our measure of the fee abolition effect would be downward biased if e.g. due to supply constraints following the war such as the shortage of class rooms and teachers observable demand was rationed even after the fee abolition. In that case the former rationing by means of fees simply would have been replaced by alternative mechanisms and the response of education demand to fee abolition would be underestimated.

Particularly for states which abolished fees soon after the war a scenario of supply constraints is plausible, because immediately after the war there was a major shortage of school buildings. Kuhlmann (1970 p.27) discusses that through the mid 1950s insufficient school housing capacity was an acknowledged limitation to educational expansion. The National Education Advisory Board even 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 of school buildings (Bohnenkamp et al. 1966). In order to account for potential state-level differences in supply controls and their change over time, we consider state-specific cohort trends as robustness tests in our model.

(c) A third issue is that those able pupils for whom school fees would have been prohibitive may have always been supported by scholarships. If scholarship systems were successful in promoting the bright poor this would 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.[17]

[17] Preparing this research I surveyed German economics professors born 1920-1940 regarding their experience with fees and scholarships. The answers yielded significant heterogeneity with respect to the amount of financial hardship connected to the fee payments but also the most surprising patterns of exempting pupils from fee requirements. Municipal schools differed from denominational schools, paternal occupation, child sex, and birth order played a

The fifth and final identification issue concerns potential anticipation of fee abolition. If parents expected a fee abolition their behavioral response may have preceded the actual abolition date. In that case we underestimate the true effect of school fee abolition. In order to gauge the potential relevance of anticipation we regress Advanced School attainment on actual years of subsequent fee payment below.

5. Results: The Effect of Fee Abolition

5.1 Aggregate Evidence

To evaluate the effects of school fee abolition we first apply state level enrollment data. Ideally, we would compare 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.[18] Because of this imprecision in the data we present average figures for 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.[19]

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 3. The numbers indicate sizeable increases in aggregate enrollment rates around the time of the fee abolition. On average the cohort share

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role always accompanied by regional differences.

[18] 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).

[19] 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.

attending Advanced School increased by 22.9 percent between the five year periods before and after the abolition of school fees. This supports the hypothesis that Advanced School participation increased after the abolition of school fees (H1).

However, this evidence blurs the true fee effect by looking at the total number of pupils in Advanced School and by disregarding the general education expansion over time. For more precise measures and to control for time trends we now turn to individual level data.

5.2 Individual Level Evidence

Our dependent variable describes whether an individual obtained an Advanced School degree ("*Abitur*"). To measure the correlates of this dichotomous indicator we use a simple logit estimator.[20] Our regressions apply alternative specifications to control for the educational expansion over time. A first approach controls for the cohort groups (pre, trans, post), for state fixed effects, and a linear cohort effect, as described before:

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

In order to avoid the limitations of a parametric, linear birth cohort indicator our main model substitutes the linear cohort effect by a set of individual birth cohort year fixed effects. We use simulation experiments to investigate the magnitude and significance of changes in Advanced School attendance after the fee abolition. Following Bertrand et al. (2004), standard errors are adjusted for clusters at the state and cohort level.

5.2.1 Overall Estimation and Simulation Results

The results of the estimation using linear cohort effects are presented in Panel A of Table 4, separately for the full sample, for only females, and only males. The coefficient estimates for

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

β_2 are positive and significant in all specifications suggesting that the probability of completing Advanced School is higher for individuals who are not subject to school fees compared to those in the "pre" cohorts. The state-specific heterogeneity in Advanced School graduation rates is reflected in highly significant state fixed effects (δ). In contrast to those in Table 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 graduation probabilities. The hypothetical graduation probability of an individual born in the midst of the transition cohort group is calculated using both a pre- and a post- cohort group assignment.[21] The results yield that the average graduation probability increased significantly after the abolition of fees by 6.3 percent for the full sample.[22] For females we find a more sizeable effect of the abolition of fees with 6.7 percent higher Advanced School attainment compared to 6.0 percent for males.

The next rows of Panel B provide robustness tests of our results. We substituted the restrictive linear specification of cohort trends first by a quadratic term and then by individual birth cohort fixed effects, reestimated the model, and repeated the simulation exercises. The quadratic cohort specification yielded similar conclusions as the linear one: overall, Advanced School completion increased significantly by about 5 percent with larger effects for female (6.5 percent) compared to male youths (3.9 percent). However, when we avoid the parametric specification and control for the time trend using a set of 29 cohort indicators, the previous evidence in favor of a small significant fee-abolition effect disappears. The β_2 coefficients are now insignificant (results not presented to save space) and the predicted fee effects (see last row of Panel B) are even smaller than before and no longer statistically significant. Only the pattern

[21] 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).

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

of larger (now insignificant) effects for females than for males holds up to the more rigorous specification. Therefore the overall conclusion from our estimation and simulations is that there are no substantive or significant effects of the abolition of secondary school fees in Germany on educational attainment. Those specifications, which yield small but significant effects are not robust to less parametric cohort controls.

In section 4 above, we discussed several concerns regarding the validity of our approach. The next section discusses the three issues which required additional robustness tests.

5.2.2 Robustness Tests

Income and Growth 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 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 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]

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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 GDP information is not available for years prior to 1950, and for Berlin and Saarland only after 1960. 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.

The estimations with GDP controls yield results that are quite similar to those presented in Table 4.[26] The GDP indicators are statistically significant with negative coefficients in the estimations for the full and the male sample, and insignificant in the female subsample. This suggests that ceteris paribus the graduation rate was higher in states with low GDP per capita (or missing values). Panel A of Table 5 summarizes the simulation results obtained when GDP controls are added to the models. The fee effects are within the range observed for both types of cohort specification in Table 4. Again, the effects for females are typically larger than those for males. Overall the results seem to be robust to income controls.

Even though controls for average per capita income do not affect the results, 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 depend on 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 rises they increase the opportunity cost of education. To investigate whether such mechanisms affect our estimates of the fee effect we reestimated our models adding controls for the growth of regional per capita GDP instead of its levels. Simulation results are presented in Panel B of Table 5. Again, they yield no important differences compared to prior findings.[27]

State-level institutional developments: In order to account for possible state-specific developments in education supply we control for state-level cohort effects as an additional robustness test. Table 6 presents the simulation of fee abolition effects that are obtained after

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

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

applying state-specific linear cohort trends, state-specific quadratic cohort trends, and adding linear state-cohort interactions to the cohort fixed effects model. The results confirm what we found before: small significant fee abolition effects as long as parametric cohort controls are considered and no effects once cohort fixed effects are used. Again, the effects for females exceed those for males.

Anticipation Effects: The abolition of secondary school fees took time, some states abolished them early on in their constitutions, some 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 in 1954 recommended the abolition of school fees (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 assumed so far - but already before.

To provide an alternative "benchmark" indicator of the effect of fees on educational attainment we investigate whether graduation probabilities vary with the true number of years that pupils would have had to pay fees upon enrolling in Advanced School. We modified our model specification and - in addition to state and cohort fixed effects - controlled for the actual number of years of fee payment ("fee years") following Advanced School enrollment. This measures the fee effect under the assumption that parents had perfect foresight.

Table 7 presents estimation and simulation results: column 1 controls for only a linear cohort effect, column 2 has the cohort fixed effect specification. In columns 3 and 4 linear cohort interactions with the state indicators are considered in addition. Generally, the number of years during which parents would have had to pay fees yields a significant negative effect on the probability of Advanced School attainment. The bottom panels suggest that switching from full fee payment to no fee payment is associated with increases in graduation rates at the order of 5

to 7 percent based on the specifications with cohort fixed effects. The results support the conclusion that the abolition of school fees may have had small effects on educational attainment.

6. Estimation of the Sensitivity of Educational Attainment to Fee Changes

After the first part of our analyses was devoted to the question of whether the existence of fees caused behavioral responses in Advanced School attainment, we now turn to the price sensitivity of education demand. We regress individual Advanced School attainment on the fee levels at the time of first Advanced School enrollment.[28] 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 earnings (fee-to-earnings ratio). National average earnings are available from the records of the retirement insurance.[29]

The specification of the educational attainment 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 estimation results are summarized in Panel A of Table 8 for a variety of cohort controls.

The estimates yield a clear and generally highly significant negative correlation of the fee-to-earnings ratio with individual educational attainment. While the estimated coefficients vary across specifications, the predicted effect of changes in the fee-to-earnings ratio on graduation rates appears to be stable. Panel B of Table 8 presents the simulated effect of a decline in the fee-

[28] 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 and instead disregard observations from Saarland in this analysis. The evidence on fee developments is discussed in Benatzky (2001).

[29] The fee-to-earnings ratio represents state-specific figures for the year when youths were 11 years old.

to-earnings ratio from 10 to 5 percent. The specification with cohort fixed effects in column 1 yields significant effects of around 4 percent on the Advanced School attainment probability. Across subsequent cohort specifications, the simulated effects are somewhat smaller but remain at the order of magnitude seen in prior simulations. When only general cohort controls are considered in the estimations (models 2-4) the effects are around 3 percent, with flexible state-specific controls (models 5-6) the effects are a little below, but typically statistically significant. Overall, a reduction in the fee-to-earnings ratio by fifty percent is correlated with increases in graduation rates at the order of no more than 4 percent with somewhat larger effects for females.[30] Again, there is no evidence of a large price elasticity of education demand.

7. 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 educational attainment: 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 school fee effects. 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) also among males than among females, and (4) increases more for females than for males following fee abolition.

Naive analyses of aggregate data and of before vs. after micro-level data suggest sizeable

[30] In estimations with sex interactions the simulated differences where much larger. The results presented in Table 10 are based on separate estimations for the two sexes and are preferred since they more flexibly capture the sex-specific trend effects.

participation increases around the period of fee abolition. When we control for overall time trends and fixed state differences using individual level data we find only small and often insignificant increases in Advanced School attainment rates at the time of the abolition of fees. A robust pattern in the results yields slightly larger effects for females than for males. Similarly, a 50 percent reduction in the fee-to-earnings ratio is associated with a statistically significant increase in attainment rates of no more than 4 percent, again with a higher price elasticity for girls' than for boys' education.[31] While the numerical effect for the entire birth cohort is small our data do not allow us to evaluate its magnitude for the population that is truly at risk of not attending Advanced School because of financial constraints. For these individuals the effect might be much larger.

Several circumstances suggest that our findings might underestimate the true demand effect of fee abolition. First, the effect of fee abolition on educational attainment may hide much larger effects on enrollment because children from price-sensitive households may receive less support in completing their degrees than those of wealthy parents. Second, our data may be subject to measurement error relating to the individuals' state of residence in their youth, which possibly causes attenuation bias in the coefficient estimates. Third, parental anticipation of fee changes may downward bias our estimates of the demand effects. Fourth, if there were capacity constraints in the schooling system after the war, then the measured response of demand to the abolition of fees may underestimate the true demand response because the (unobserved) demand for secondary education increased much faster than the available capacity permitted.

Alternatively, if our results are unbiased measures of the response of education demand to fee abolition, it may be that demand for education was price inelastic or the fee was too small to yield more sizeable responses. Küchenhoff (1952) and Bergmann (1955) point out that even

[31] 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.

after tuition fees were abolished other education related payments (examination fees, certification fees, accident insurance premiums) were still collected, such that Advanced School fees only reflected a small part of the total expenses related to this type of schooling.

An additional concern may be that with the abolition of school fees educational quality declined as a consequence of reduced funding. Evidence on school funding in the 1950s is difficult to obtain. However, Kahlert (1974) states that in 1953 only 3.1 percent of general school expenditures and 8.8 percent of Advanced School expenditures were covered by the total of fees and other contributions. Therefore it is unlikely that the fee abolition caused a noticeable decline in the quality of education.[32]

Overall our results suggest that the existence of school fees for Advanced Schools may have set on average only a small 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 education differences for higher education between the sexes: parental demand for girls' education seems to be more price elastic.

In conclusion, it appears unlikely that the existence of school fees explains the differential development in secondary school participation in the United States and Germany during the early decades of the twentieth century.

[32] Kuhlmann (1970, p.59) illustrates policy makers' reasoning about school reforms in the 1950s, which was strictly driven by alleged educational benefit. Economic cost-benefit calculations apparently did not play a role. One state secretary of education was in fact publicly scolded for raising economic consideration in the discussion of educational reform. This suggests that education expenditures were tied to need rather than to inflowing fee payments.

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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 +
Schleswig-Holstein	1952	-1939	1940 - 1942	1943 +

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

Table 2 Logit Models of the Determinants of Fee Abolition

	1		2		3		4		5		6	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
State headed by socialdemocrat	-0.27	0.64	-	-	-	-	-	-	-	-	0.69	0.82
Cohort share with A.S. degree (t)	-	-	-49.62	32.31	-	-	-	-	-	-	74.64	83.92
Cohort share with A.S. degree (t-1)	-	-	58.19	32.21	-	-	-	-	-	-	-119.0	76.21
Cohort share with A.S. degree (t-2)	-	-	-9.62	29.67	-	-	-	-	-	-	52.01	45.99
Joint Test (p-value)	-	-		0.277	-	-	-	-	-	-		0.393
Growth in cohort share with A.S. degree (t vs. t-1)	-	-	-	-	-6.97	2.65**	-	-	-	-	-17.51	7.77*
Growth in cohort share with A.S. degree (t vs. t-2)	-	-	-	-	1.27	2.24	-	-	-	-	6.02	5.76
Joint Test (p-value)	-	-	-	-		0.032**	-	-	-	-		0.039*
Income per capita (t)	-	-	-	-	-	-	-0.23	0.27	-	-	-0.44	0.65
Income per capita (t-1)	-	-	-	-	-	-	0.32	0.22	-	-	0.59	0.58
Income per capita is missing	-	-	-	-	-	-	1.22	2.61	-	-	0.86	3.68
Joint Test (p-value)	-	-	-	-	-	-		0.386	-	-		0.246
Growth in income per capita (t vs. t-1)	-	-	-	-	-	-	-	-	6.91	11.74	16.33	10.76
Growth in income per capita (t vs. t-2)	-	-	-	-	-	-	-	-	-5.93	6.62	-9.51	12.28
Joint Test (p-value)	-	-	-	-	-	-	-	-		0.648		0.274
Log Likelihood		-32.80		-30.93		-28.78		-32.18		-32.48		-26.62
Number of observations		114		114		114		114		114		114

Notes:
1. **, *, and ○ indicate statistical significance of the coefficients at the 1, 5, and 10 percent level. Standard errors are clustered at the state level.
2. All models control for a constant and a calendar year indicator and are estimated for 114 observations.
3. The joint tests investigate the joint significance of the previous two or three explanatory variables.

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

Table 3 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 4 Estimation and Simulation Results - Logit on Advanced School Attainment

	1 - All		2 - Females		3 - Males	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
A: Estimation Results						
pre (reference)	-	-	-	-	-	-
β_1: trans	0.04	0.03	0.03	0.03	0.05	0.04
β_2: post	0.07	0.03*	0.07	0.04∘	0.07	0.04∘
γ: linear birth cohort	0.05	0.00**	0.06	0.00**	0.04	0.00**
δ: state FE	yes	**	yes	**	yes	**
α: constant	yes	**	yes	**	yes	**
Log Likelihood		-162,356.4		-68,468.9		-92,147.2
B: Simulation of Change in Advanced School Attainment from Pre to Post Cohorts						
Fee effect		6.3%**		6.7%*		6.0%**
linear cohort effect		(0.02)		(0.03)		(0.03)
Fee effect		4.9%*		6.5%*		3.9%∘
quadratic cohort effect		(0.02)		(0.03)		(0.02)
Fee effect		1.15%		3.8%		-0.9%
cohort fixed effect		(0.02)		(0.04)		(0.02)

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 changes in graduation rates. Standard errors (in parentheses) are bootstrapped using 100 draws from the original data.
2. 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 degrees 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
3. The estimation on the full sample used 433,315 observations, the regressions for females and males 217,248 and 216,067 observations, respectively.
4. The predicted fee effects using a quadratic cohort effect in the last row of Panel B are based on a separate set of estimates which are not presented to save space.

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

Table 5 Simulated Changes in Graduation Rates from Pre to Post Cohort Based on Estimations with Controls for Income and Income Growth
(Predicted Fee Effect in Percent)

	1 - All	2 - Females	3 - Males
A: Controlling for state-level income			
Linear cohort effect	6.2%**	6.8%*	5.7%*
Cohort fixed effect	2.5%	3.1%	1.3%
B: Controlling for state-level growth			
Linear cohort effect	6.1%**	6.7%*	5.7%*
Cohort fixed effect	1.9%	2.4%	0.9%

Notes: See Table 4.

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

Table 6 Simulated Changes in Graduation Rates from Pre to Post Cohort Based on Estimations with Controls for State-Specific Cohort Effects
(Predicted Fee Effect in Percent)

	1 - All	2 - Females	3 - Males
Linear cohort effect	6.2%**	5.0%	6.5%*
Quadratic cohort effect	5.5%*	5.2%	5.9%*
Cohort fixed effect	-0.1%	2.3%	-2.1%

Notes: See Table 4.

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

Table 7 Estimation and Simulation Results - Logit on Advanced School Attainment Controlling for the Actual Number of Years of Fee Payment (feeyears)

	1		2		3		4	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
A: Estimation results (All)								
fee years	-0.015	0.004**	-0.008	0.003*	-0.020	0.005**	-0.006	0.004
state FE	yes		yes		yes		yes	
birth cohort linear	yes		-		-		-	
birth cohort FE	-		yes		-		yes	
state FE*linear cohort	-		-		yes		yes	
B: Simulation of graduation difference with 0 vs. 10 years of fee payment								
Fee effect - All	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)	

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 changes in graduation rates. Here standard errors (in parentheses) are bootstrapped using 100 repeated draws from the original data.
2. The full sample effect is predicted based on the above represented coefficient estimates. The model was reestimated for the two sexes to generate the predictions for males and females. Coefficient estimates for these are not presented to save space.
3. 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 | fee years =0) / Pr (S=1 | fee years =10)] - 1

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

Table 8 Estimation and Simulation Results - Logit on Advanced School Attainment

	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 (All)												
fee-to-income ratio	-1.01	0.38**	-0.77	0.26**	-0.83	0.27**	-0.64	0.25*	-0.62	0.23**	-0.43	0,23∘
state gdp p.c.	yes	∘	yes	∘	yes		yes	*	yes	*	yes	∘
cohort FE (29)	yes	**	-		-		-					
cohort	-		yes	**	yes	*	yes	**	-		-	
cohort^2	-		-		yes		yes	**	-		-	
cohort^3	-		-		-		yes	**	-		-	
state FE * cohort (10)	-		-		-		-		yes	**	yes	**
state FE * cohort^2 (10)	-		-		-		-		-		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		-159,712.6		-159,802.2		-159,801.9		-159,767.4		-159,756.4		159,700.6
B: Simulation of change in fee-to-income ratio from 10 to 5 percent												
Fee effect - All	4.3%	0.01**	3.3%	0.01**	3.6%	0.01**	2.7%	0.01**	2.7%	0.01**	1.8%	0.01∘
Fee effect - Females	4.9%	0.02*	4.0%	0.01**	4.3%	0.02*	3.6%	0.01**	3.3%	0.02*	2.2%	0.02
Fee effect - Males	3.6%	0.02*	3.0%	0.01**	3.0%	0.01**	2.0%	0.01∘	2.4%	0.01*	1.4%	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 Panel B **, *, and ∘ refer to the statistical significance of the predicted changes in graduation rates, 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 full sample used 425,323 observations, the regressions for females and males 213,215 and 212,108 observations, respectively.
4. Each fee effect in panel B is estimated using sample-specific coefficient estimates.
5. The simulations provide the percent increase in the predicted probability of Advanced School attainment for youth comparing a scenario

where 10 and 5 percent of incomes are to be paid for school fees. The predicted individual graduation rates in both scenarios are averaged over the sample. Both panels provide: [Pr (S=1 | income ratio = 0.05) / Pr (S=1 | income ratio = 0.10)] - 1.

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

Figure 1 Representation of Fee Effects

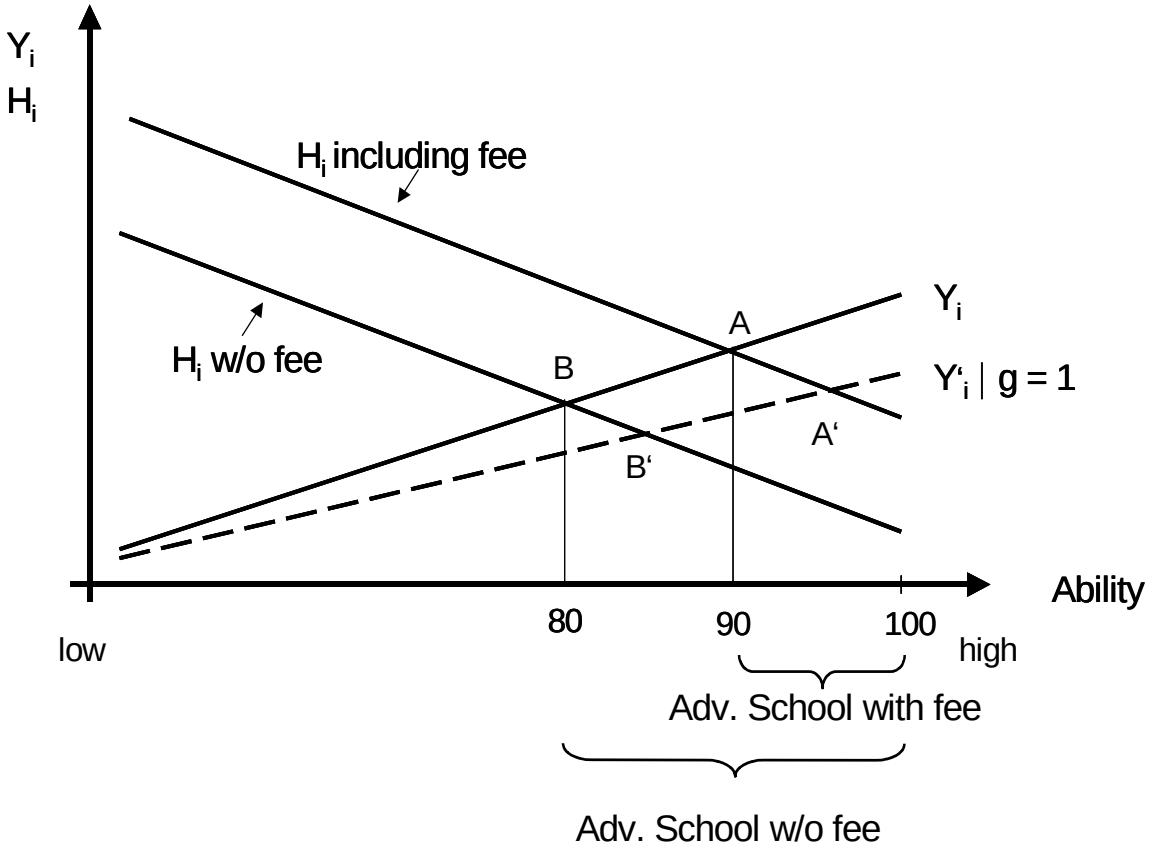
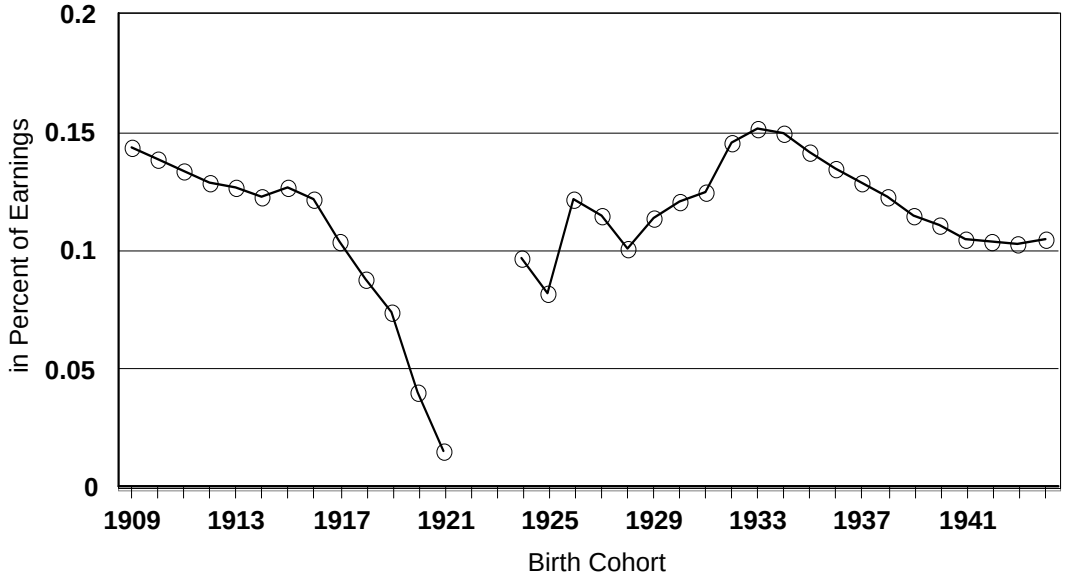


Figure 2 Sketch of the Traditional German Schooling System

Age	Grade	
6	1	Basic School
7	2	
8	3	
9	4	
10	5	Middle School / Advanced School
11	6	
12	7	
13	8	
14	9	
15	10	
16	11	
17	12	
18	13	

Source: Own presentation

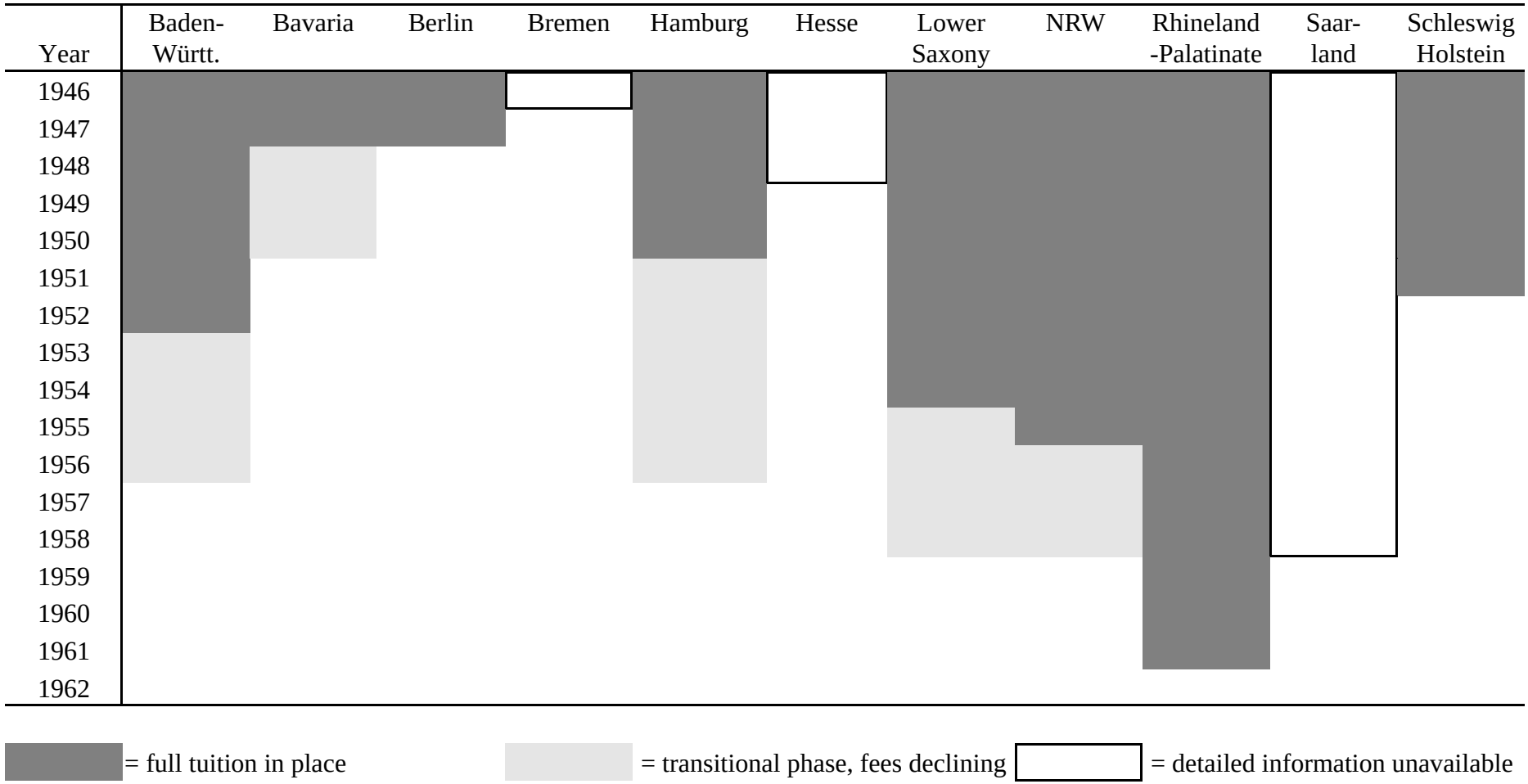
Figure 3 Prussian School Fees for Advanced Schools as Share of Average Earnings



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.

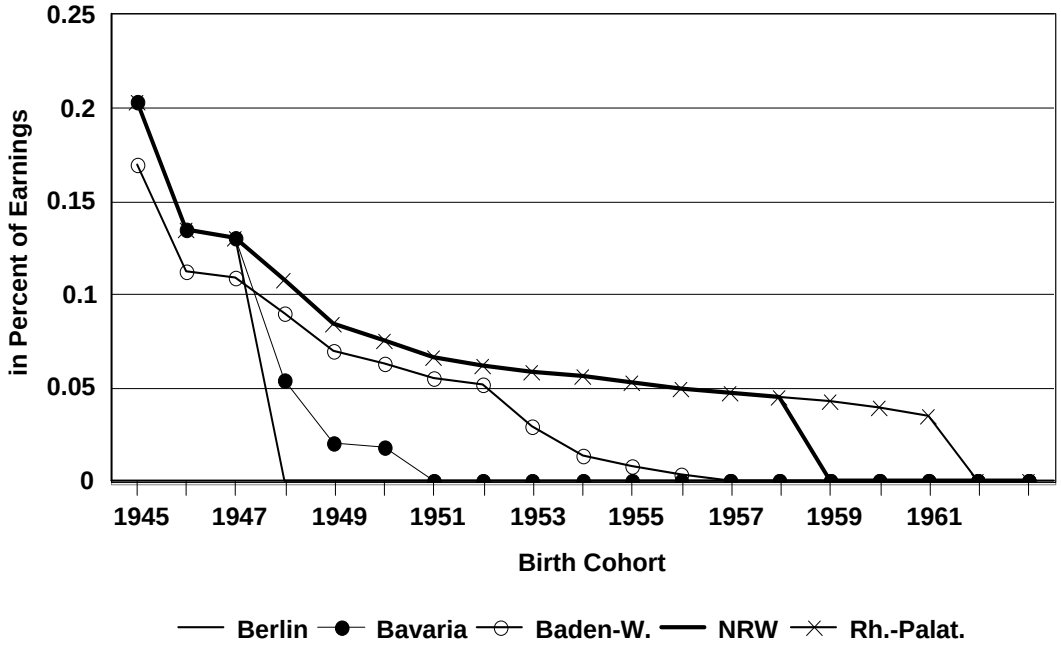
(ii) Tuition fees from various sources (Benatzky 2001).

Figure 4 State-Specific Patterns of Tuition Abolition



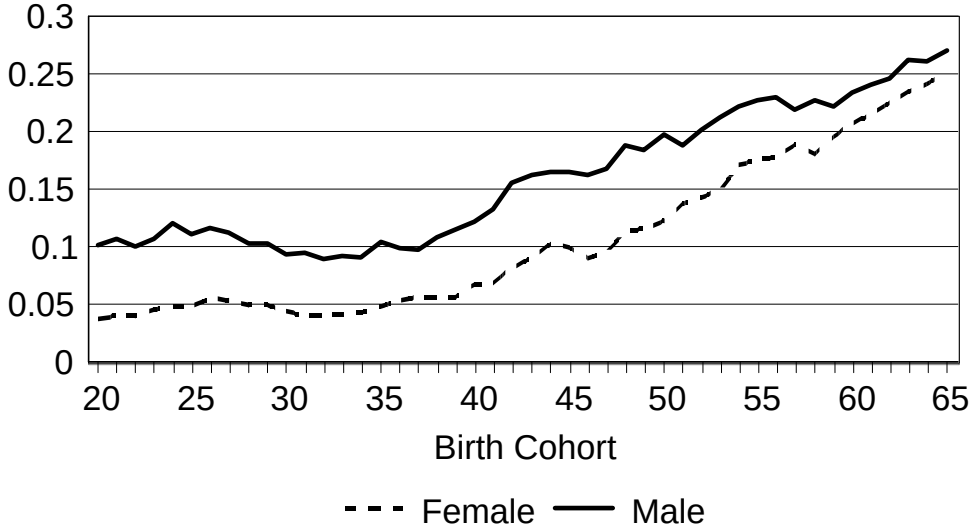
Source: Own graphical representation of fee information in Benatzky (2001).

Figure 5 Advanced School Fees as Share of Average Earnings (Selected States)



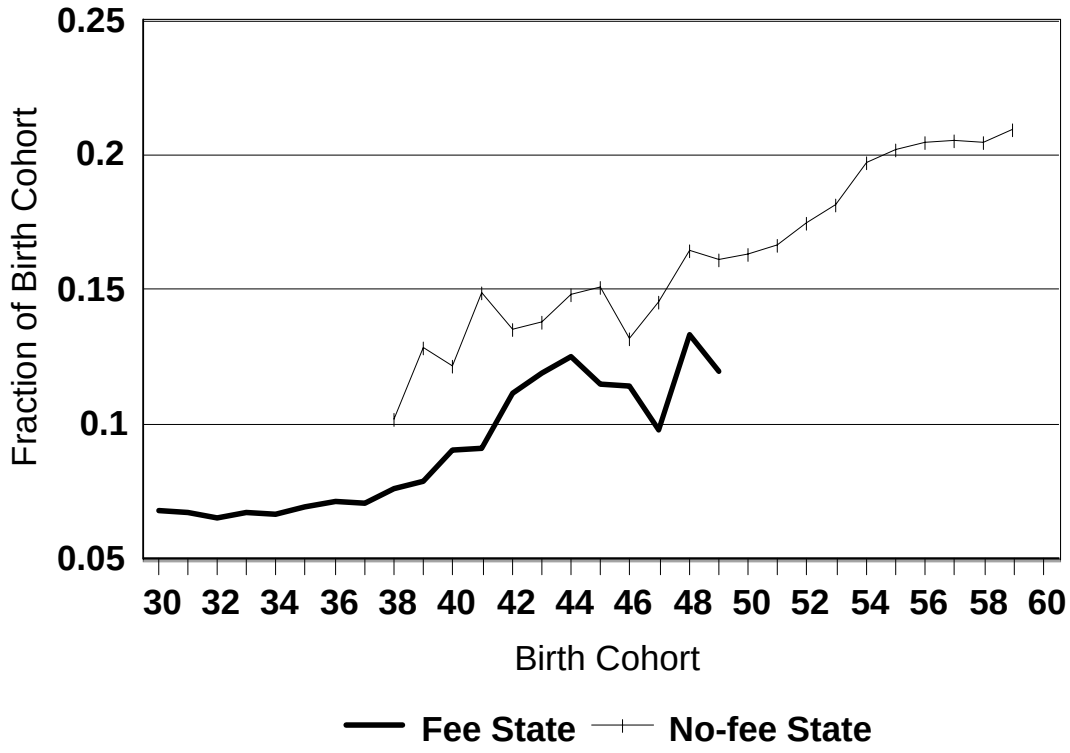
Source: Earnings data from retirement insurance statistics, tuition fees from various sources (Benatzky 2001).

Figure 6 Population Shares with Advanced School Degree by Birth Cohort



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

Figure 7 Cohort Advanced School Graduation Rates by Fee Status of State (Reflecting pre and post cohort groups)



Note: The depicted fraction of birth cohorts graduating from Advanced Schools in fee states is the average graduation 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 graduating from Advanced Schools in no-fee states presents the average graduation 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: Simple Theoretical Framework

Here we derive the effect of changes in the direct cost of schooling (C) 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 an Advanced School degree is the preferred choice (see Figure 5).

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

$$\overset{+}{} \quad \overset{+}{} \quad \overset{-}{} \quad \overset{+}{}$$

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 an Advanced School degree is preferable, we solve (2) and (3) for :

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

$$\overset{?}{} \quad \overset{-}{} \quad \overset{+}{} \quad \overset{-}{}$$

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.

Appendix B: 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 at grade five:
- Cohorts, which certainly did pay a fee ("pre" group)
- Cohorts, for which we cannot be sure ("trans" group)
- Cohorts, which certainly did not 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.

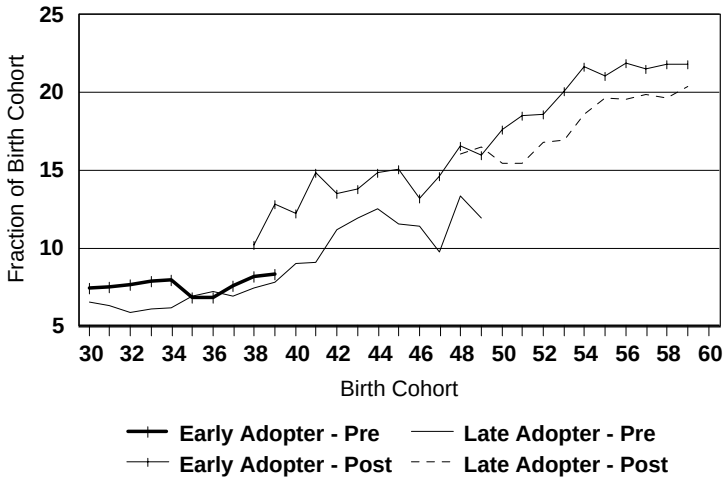
(4) Example:
In general Basic School starts between ages 5 and 7. The transfer to Advanced School (after 4 years) takes place between ages 9 and 11. As pupils may have to repeat a grade we consider ages 9-12 for possible transitions.

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").

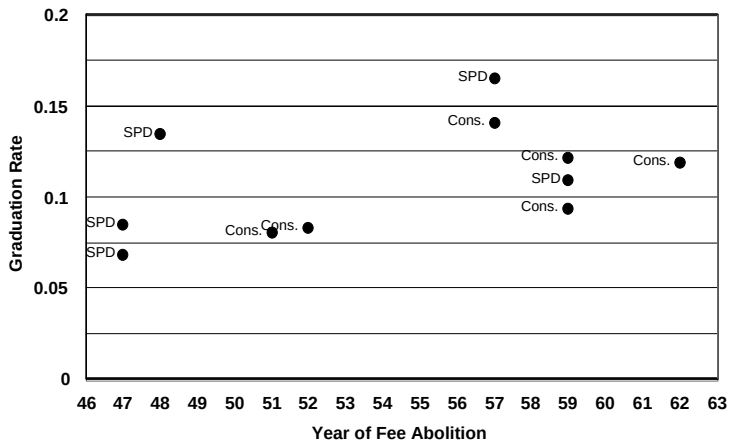
(5) Experimentation with differently structured school systems:
In four federal states educational policies after the war experimented with the structure of the schooling system by postponing the transition from primary education to Middle and Advanced Schools (cf. Klafki 1976, p.266): In Schleswig-Holstein 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 C: Timing of Fee Abolition

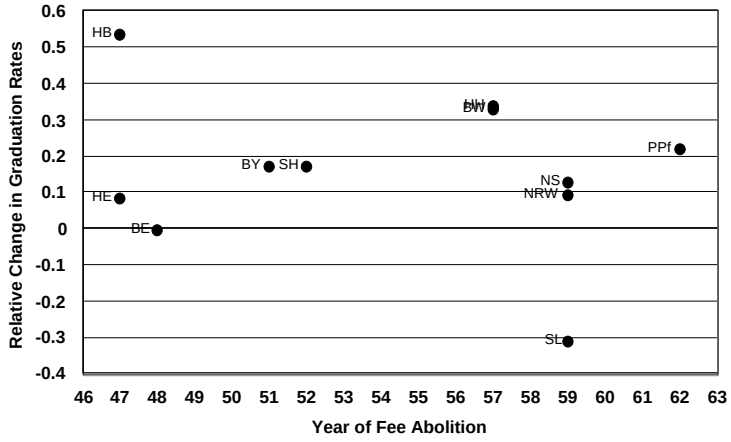
(a) Advanced School attainment for "pre" and "post" birth cohorts in states which abolished fees early and late:



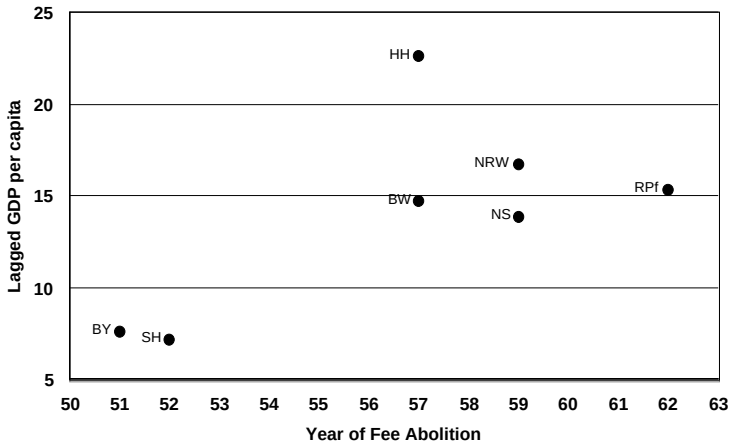
(b) Advance School attainment rate in the last birth cohort certainly paying a fee



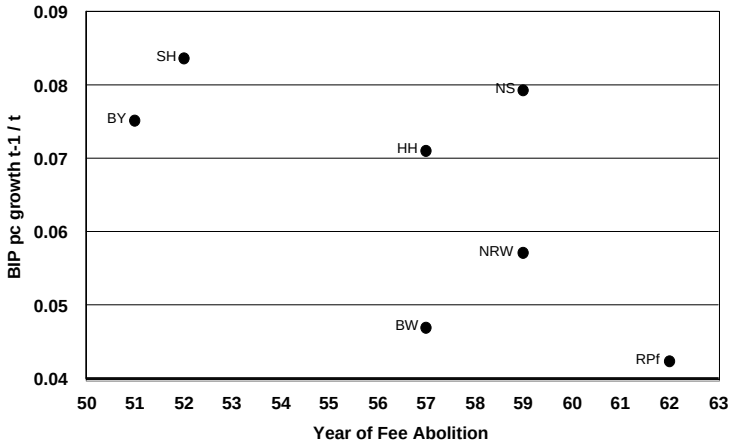
(c) Relative change in Advanced School attainment rates for the last three birth cohorts certainly paying a fee



(d) Lagged gross domestic income per capita



(e) Growth in gross domestic income per capita in the year preceding abolition (t vs. t-1)



Note: In graph (b) the data labels indicate whether the state government passing the fee abolition was run by socialdemocrats (SPD) or conservatives (Cons.). In the other graphs the data labels indicate the name of the state.

Source:

Graduation rates were calculated based on Mikrozensus data 1989, 1993, 1997, the state GDP information was received from the Statistical Office of Baden Württemberg, the state population information was obtained from each of the state statistical offices and the information on political parties was taken from www.net-lexikon.de.