The effect of secondary school fees on educational attainment

Regina T. Riphahn*

Univ. of Erlangen-Nuremberg
Lange Gasse 20
D-90403Nuremberg
Germany
regina.riphahn@wiso.uni-erlangen.de

Abstract

This study utilizes the heterogeneity of the fee abolition process in West German secondary schools to identify the effect of school fees on educational attainment and to obtain an estimate of the price elasticity of upper secondary education. The analysis is based on representative individual-level data from three annual surveys of the German *Mikrozensus*. While coefficients cannot be estimated precisely, the results suggest that, on average, upper secondary school attainment increased by at least eight percent in response to the fee abolition. Females' educational attainment appears to be more price sensitive than males'.

Key Words: tuition, school attainment, demand for education, natural experiment

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I. Introduction

How effective are financial incentives when it comes to improving educational attainment at the secondary school level? How do school fees affect demand for upper secondary education? These issues are intensely debated in many countries. Yet, so far the literature provides no clear answer.

Most of the available evidence on the effect of school fees describes the situation in developing countries. Here, the literature shows beneficial results of abolishing school fees or introducing education subsidies for school attendance and enrollment (Alderman et al. 2001, Deininger 2003, Al-Samarrai and Zaman 2007, and Schultz 2004). The evidence on school fees from industrialized countries is more limited. Dearden et al. (2007) find positive effects of a means-tested subsidy on participation in full-time education in the United Kingdom. Card and Lemieux (2000) obtain generally negative but insignificant correlations between tuition and the educational enrollment of teenagers in the United States. As an example of the broader literature evaluating college enrollment, Linsenmeyer et al. (2006) find increased college enrollment after the introduction of financial aid packages.

This study contributes a new piece of evidence to the debate on financial incentives and upper secondary education. It takes advantage of a natural experiment in post-war Germany to identify the effect of the abolition of school fees on secondary school attainment. In contrast to much of the literature, we estimate the price elasticity of upper secondary education separately for male and female pupils. In Germany as in many other countries, male educational attainment historically exceeded females'. The abolition of school fees in combination with a higher price elasticity of education for females may be behind the impressive catching up process in female educational attainment observed in Germany and elsewhere since WWII.

The potential heterogeneity of financial incentive effects for male and female pupils has found little attention in the literature even though most of the studies that do distinguish between

the two groups find substantial differences. Among analyses for developing countries, Alderman et al. (2001) find higher parental expenditures for the education of boys than girls, Deininger (2003) points out that the abolition of school fees in Uganda completely eliminated the educational gender bias against girls, and Schultz (2004) finds larger enrollment effects of educational subsidies for girls than for boys. Analyzing the trends in the U.S. between 1968 and 1996, Card and Lemieux (2000) find that tuition had a much more negative effect on college enrollment of females than males. Thus, when separate effects are allowed for, we typically find financial incentive programs to benefit girls more than boys. This may be an important aspect in the explanation of gender-specific educational participation patterns and deserves investigation also for secondary schooling in an industrialized country.

The focus here is on school fees in West German secondary schools, which are organized based on ability-related tracks. Up until the end of World War II, fees had to be paid for upper 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 and the mode of fee abolition across states identifies the effect of fees on educational attainment and thus the price responsiveness of secondary education.

This analysis is informative for the discussion of the optimal provision of secondary education (e.g. OECD 2006). In addition, our results are of interest to the debates on the effect of tuition subsidies and of university fees (e.g. Dynarsky 2003, Kane 2007). In much of Europe the debates on the price elasticity of tertiary education lack reliable measures: data on prior experiences with academic fees are unavailable and the measurement of the effect of public aid on enrollment is hampered by endogeneity.[1] Also, the role of school fees for school attainment is related to a growing literature on measures to increase school attendance in developing[2] as well

[1] See Dynarski (2002) and Kane and Rouse (1999), or recently Linsenmeier et al. (2006).

[2] Additional examples are Miguel and Kremer (2004) or Kim et al. (1999).

as industrialized countries.[3]

Below, we first investigate the responsiveness of educational attainment to changes in the price of education. We then submit our results to a number of robustness tests. The fee abolition coefficient cannot be estimated precisely. On average, fee abolition increased Advanced School attainment by at least eight percent, where the responsiveness of female education exceeds that of males' education.

II. 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 costs (C_i) and expected returns (Y_i) of schooling. Let A_i be the ability of individual i in population group g (g= male, female) and formalize

$$Y_i = \mu_g + b_g A_i \qquad (1)$$

$$C_i = \nu - \gamma A_i + F , \qquad (2)$$

where μ_g and b_g characterize the correlation between expected returns and individual ability, which may differ between the sexes. Similarly, total cost may be lower for individuals of high individual ability as summarized by γ. ν reflects an intercept term and F captures fixed costs of schooling such as tuition fees. Based on $Y_i > C_i$, we can calculate the critical ability level beyond which investment in education is worthwhile:

$$\hat{A}_g = \frac{1}{b_g + \gamma}\left[\upsilon - \mu_g + F\right]. \qquad (3)$$

If women have more limited possibilities to make use of their education in the labor market than men (e.g. due to family obligations, or gender wage gaps), we expect that $\mu_f < \mu_m$ and $b_f < b_m$. This immediately yields

[3] See also Meghir and Palme (2005) for the Swedish experience in the 1950s and 1960s, Aakvik et al. (2003) on Norway, and de la Croix and Docquier (2007) on France.

$$\hat{A}_f > \hat{A}_m \quad \text{and} \quad \frac{d\hat{A}_f}{dF} > \frac{d\hat{A}_m}{dF}. \tag{4}$$

The first inequality implies that females' critical ability is higher than males', which - assuming identical ability distributions - suggests that a smaller share of women than men obtain education. The second inequality indicates that the marginal effect of a given change in school fees may well be larger for women than for men.

Figure 1 depicts a situation where pupils are sorted by ability on the abscissa. We expect that those for whom the expected returns to schooling (Y_i) exceed costs will attend Advanced Schools, i.e. everybody to the right of point A, here amounting to the 10 percent most able pupils. If fees are abolished, fixed costs (F) decline and the share of pupils in Advanced Schools increases to e.g. the 20 percent most able individuals (point B). If $b_f < b_m$ and if the expected returns for females are below those for men at all ability levels then the dashed line, labeled Y_i', represents women's expected returns to education. In combination with the cost schedule we find that women's Advanced School attainment should be lower than men's, both before (point A') and after (point B') the abolition of fees. Females' response to the abolition of school fees in terms of the relative attainment increase may exceed that of males.

Based on this framework, we expect that the abolition of school fees causes a decline in the direct cost of education and an increase in participation and attainment. Given the variation across federal states and time, we hypothesize that Advanced School attainment increases after the abolition of school fees (H1), is higher in states without than with fees (H2), is higher among males than females (H3), and increased more for females than males (H4). These hypotheses are tested below.

III. 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. After passing an entrance exam, it was possible to advance from Basic School after 4 years to either Middle School (*Realschule* / *Mittelschule*) or Advanced School (*Gymnasium* / *Oberschule*) where education continued for an additional 6 or 9 years, respectively (see **Figure 2**). The system hardly changed over time, and the Advanced School degree (*Abitur*) remained the key requirement for university studies.[4]

Throughout the 19th century, there was a fee to be paid for any type of school. 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. 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: Advanced School education was shortened by one year in 1938, and starting in 1941 it was at times cut by another 6 months to facilitate military service.

After the war, the authority for the administration of schools was returned to the federal states. They increased the duration of Advanced School education back to 9 years (grades 5-13) and re-regulated the fee system: starting with the city-state of Bremen (1947) and ending with Rhineland-Palatinate (1962), over time all states abolished fees for public secondary schools (Benatzky 2001). While until 1945 the annual fee level was set uniformly at 240 *Reichsmark,* there was regional variation in the speed and extent of fee abolition, which we use to identify its effects. **Figure 4** describes the fee abolition pattern across the 11 West German federal states

[4] While today the degree can be attained via alternative educational pathways, these were not available in the past. The educational decision taken after primary school, at about age 10, was crucial.

between 1947 and 1962.[5] Frequently, fees were abolished stepwise over several years as in Hamburg or Bavaria. **Figure 5** describes the development of fees as a share of average earnings for selected states and over time. It shows the heterogeneity of the abolition process between 1947 and 1962. **Table 1** presents state-specific fee abolition years and the birth cohorts concerned.

The abolition of school fees was not the only post-war development in the West German educational system. Education policies in the 1950s focused on reconstruction and organizational issues. Beginning in the 1960s, the educational system expanded similar to other industrialized countries. This reflected growing birth cohorts, rising demand for advanced education, as well as increasing public investments in the education system.

IV. Data

Our data are taken from the *Mikrozensus*, an annual survey of a one percent random sample of German households. Public use files provide 70 percent of the data. The *Mikrozensus* uses a rotating scheme which re-interviews individuals up to four times; however, 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.[6]

Our sample considers German nationals born between 1930 and 1959 who live in West Germany. We drop observations with missing values on key variables such as age, sex, schooling, or state. This yields more than 400,000 observations, between 10,000 and 18,000 per birth cohort. The key advantage of the data is a sample size, which allows to compare state-level differences by birth cohort. The main disadvantage is the lack of social background variables, which we discuss below.

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[5] We neglect former East Germany, where school fees were abolished in 1957 (Geissler 2000).

[6] Even if panel data were available, they would not add to our analysis because our outcome variable does not change over time.

The dependent variable describes whether an individual completed Advanced School.[7] **Figure 6** describes Advanced School graduation rates over subsequent birth 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 until they started to increase for the birth cohorts of the 1940s. Over time females caught up to males. **Figure 7** depicts average graduation rates indexed at the first birth cohort experiencing no school fee. For both sexes we see a stronger rise in graduation rates after fee abolition than before, which confirms Hypothesis 2 (H2).

In order to determine the price sensitivity of educational attainment we regress individual Advanced School attainment on the fee levels at the time of Advanced School enrollment.[8] As the fee was legally set to a fixed nominal amount and its real value (and even currency) changed over time, we deflate the fee measure dividing by annual average earnings and generate a fee-to-earnings ratio.[9] We define three alternative indicators of fees, all measured when a youth was to enter Advanced School: the first indicator characterizes the fee-to-earnings ratio in the first year of Advanced School attendance, i.e. in grade 5. As parents may base educational choices on longer periods of expected fee payments, we additionally consider the sum of fee-to-earnings ratios accruing in grades 5, 6, and 7, and as an additional robustness test the sum for grades 5 through 10, always as observed at the time of Advanced School entry (for descriptive statistics see Appendix **Table A-1**). This implies a myopic parental perspective because possible future fee changes are not considered. We test below whether perfect parental anticipation of changes in fee regulations affects results. The relationship between Advanced School attainment and fee-

[7] Ideally one would like to observe individual Advanced School enrollment which, however, is not available. Köhler (1990) describes that the share of enrollees, who later drop out remains rather constant over time. Therefore the results for attainment and enrollment should be comparable.

[8] Since full state-specific fee level information is unavailable, some of the missing information was replaced by plausible assumptions. In particular, the initial fee amounts after the war were set to the levels during the war, if other information was unavailable. Due to missing information for Saarland, we disregard observations from this state (see Benatzky 2001).

[9] Average earnings are available from the records of the retirement insurance.

to-earnings ratio at grade 5 is depicted by sex in **Figure 8**.[10] Each mark represents the average Advanced School attainment by birth cohort and state. As expected, Advanced School attainment is higher for males than for females and increases as the fee-to-earnings ratio declines.

V. Baseline Results

In order to determine the causal effect of school fees on Advanced School attainment we regress individual Advanced School attainment on fee-to-earnings ratios. Fixed state and birth cohort effects account for the heterogeneity of Advanced School attainment across states and over time. In our baseline specification we model the probability that individual *i* attains an Advanced School degree (S) as:

$$\Pr(S_i = 1) = F(\beta_0 + \beta_1 \text{ fee-to-earnings}_i + \beta_2 \text{ cohort}_i + \beta_3 \text{ state}_i), \quad (5)$$

where *F* represents a cumulative distribution function, *cohort* is a vector of birth cohort fixed effects, *state* stands for state fixed effects, and β_0 - β_3 are coefficients. Since the dependent variable describes whether an individual obtained an Advanced School degree ("*Abitur*") we use a logit estimator.[11] The estimation result for β_1 indicates the effect of secondary school fees. The validity of the estimation of a causal fee effect hinges on a number of conditions, which we discuss after presenting the baseline results in **Table 2**.

Panel A presents the coefficient estimates for the fee-to-earnings ratio for those entering grade 5 of Advanced School. The estimations are performed separately for the full sample and for the female and male subsamples. In all cases we obtain the expected negative coefficients, which suggests that attainment is higher when fees are low and confirms hypothesis 1 (H1). The heterogeneity in Advanced School graduation rates across states and over time is reflected in

[10] Figures for fee-to-earnings ratios for grades 5-7 and 5-10 look similar and are not depicted to save space.

[11] The models were also run as linear probability models. The results were in the same direction and at similar orders of magnitude as those obtained using the logit estimator.

highly significant state and birth cohort fixed effects (not presented to save space).

Generating standard errors we have to account for the fact that the fee-to-earnings ratio varies only at the state-by-cohort level. Since unobservables may be correlated for observations in the same state-by-cohort group and to avoid the Moulton (1986) problem, we present as SE1 standard errors clustered at the state-birth cohort level.[12] These standard errors are only appropriate if there is no serial correlation in the unobservables by state and birth cohort. A recommended approach to account for potential autocorrelation in standard errors is to cluster standard errors at the state level. Such standard errors are presented as SE2. While SE1 standard errors generate statistically significant results, the SE2 standard errors, which are based on ten different state observations, are much larger. Based on these autocorrelation robust standard errors the estimates of β_1 are not statistically significant.

To evaluate the economic significance of the fee-to-earnings ratios, the effect of reductions in the fee-to-earnings ratios by 10 percentage points (about two standard deviations) were simulated and standard errors generated by bootstrap. The results suggest that Advanced School attainment would have increased substantially by between 6.55 percent for males and 9.83 percent for females if fees declined e.g. from 10 to 0 percent of earnings. In Panel B we repeat the analysis considering now the sum of fee-to-earnings ratios for grades 5, 6, and 7 and simulating a drop in this amount by 30 percentage points (about two standard deviations). The results are comparable to those presented in Panel A, with overall effects of almost 9 percent and again larger effects for females than males which supports hypothesis 4 (H4). The results are confirmed in Panel C, where the sum of the fee-to-earnings ratios for the first six years in Advanced School is considered.[13]

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[12] Unclustered but heteroscedasticity-robust standard errors are generally of similar magnitude.

[13] To represent the effect of yearly costs of schooling - as in Panel A - the coefficients in Panels B and C need to be multiplied by 3 and 6, respectively. The analysis assumes the normal age of transition to fifth grade. Results of alternative specifications are discussed in section 6.2 below.

VI. Identifying Conditions and Robustness Tests

Identification

The presented correlation between fees and educational attainment can only be interpreted as a causal effect if the fee level is exogenous to individual schooling outcomes. Next, we discuss five conditions that have to be met for this to hold true.

As a first identification condition, we need to establish that the abolition of school fees and the reduction of fee-to-earnings ratios over time are exogenous and not jointly determined with state school attendance. It is difficult to obtain historic accounts of the political processes leading to fee abolition.[14] However, we can investigate the relevance of potential determinants of fee changes: we generated a state-level panel data set for the ten states used in **Table 2** with annual observations (1945-1965). As dependent variable we consider the fee-to-earnings ratio observed in grade 5 in any given calendar year. Controlling for year effects the ratio is regressed on indicators (i) for whether the state governor was a socialdemocrat (state governments were almost equally split between socialdemocrats and conservatives), (ii) for the state Advanced School graduation rate in each of three preceding years and its growth, and (iii) for lagged per capita income and growth rates.[15] **Table 3** presents the estimation results of the linear models with standard errors clustered at the state level. The first five columns separately consider five indicator groups. Except for year effects and a missing income indicator the coefficient estimates are not statistically significant. The joint model is presented in column 6. Due to multicollinearity not all effects are identified. Again, we find no significant correlates of the fee-to-earnings ratio. The results are very similar when the fee-to-earnings ratios for grades 5-7 or 5-10 are used as dependent variables.

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[14] An interesting description of the case of Bavaria is provided e.g. by Klafki (1976) (also Müller 1995). In Bavaria the U.S. occupation force insisted on the abolition of school fees. Local politicians resisted this measure and managed to postpone its implementation for several years.

[15] Since information for state income is available for most states starting only in 1950 missing values are set to zero and the model is estimated with indicators for missing values.

In further analyses we found no correlation between the timing of fee abolition and the considered covariates, nor between lagged changes in Advanced School attainment and fee-to-earnings ratios (not presented to save space).[16] In view of the heterogeneity of post war education policies across states the lack of systematic patterns is not surprising. Up until 1949 West German states were governed by the Allied Occupation Forces, each with different interests and philosophies regarding the German educational system. While the U.S. forces simply ordered the Bavarian government to abolish fees to generate equal access to Advanced School, the French army used school policy predominantly to enforce French language instruction in German secondary education. The British forces focused on extending joint primary education from four to six years. Some federal states established statehood earlier (e.g. Hesse 1946), others later (e.g. Baden-Württemberg 1952). Some guaranteed free secondary schools as a constitutional right (e.g. Hesse) others held on to increasingly nominal fees (e.g. Rhineland-Palatinate). Up until the mid 1960s, increasing secondary school attainment was not on the political agenda of West German states.[17] Therefore we consider fee changes to be exogenous to Advanced School attainment. If fees were, in fact, abolished to increase overall secondary school attainment our estimates may overestimate the causal effect of fee changes.

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 moved to no-fee or low-fee states or sent their children to schools across state borders. Both mechanisms would yield an inflated measure

[16] As an indirect test of whether fee regulations were correlated with pre-reform trends, we controlled for whether an observation was the last birth cohort with full fee payments in the baseline model in **Table 2**. Its coefficient estimate was negative and never statistically significant. This suggests that fee abolition was not prompted by a general pre-existing trend to higher educational attainment.

[17] For a discussion of the very conservative post-war German school policies and possible explanations see e.g. Robinsohn and Kuhlmann (1967). While the total number of secondary schools increased by 12 percent between 1950 and 1960, the number of Advanced School increased by only 4.5 percent and thus experienced the smallest expansion of the three types of secondary schooling in the relevant period (see Federal Statistical Office, Fachserie 11 Reihe S.2, various years).

of the fee abolition effect. While the problem of cross-border school attendance should be relevant only for a small share of the sample, the first issue would present a major challenge in a highly mobile society. 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 (2007), who shows that about 80 percent of all adult respondents to a large German survey live in their state of birth. Therefore, because of the limited relevance of the fee expenditure and the short term nature of the benefit of moving we consider self-selection into treatment to be unlikely.

A related issue 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 might cause attenuation bias. An upward bias would result if individuals with Advanced School degrees moved to states where their birth cohort would not have had to pay fees and, vice versa, if those without Advanced School degrees moved to states where their birth cohort would have had to pay fees. It seems implausible to expect such specific migration patterns.

As a test of the sensitivity of our results to population mobility, we repeated the estimation of **Table 2** after dropping observations from states with high mobility, i.e. from Bremen, Hamburg, and Berlin as well as Schleswig-Holstein and Rhineland-Palatinate.[18] The results are summarized in **Table 4**. With an average of 12 percent the predicted effects are now

[18] Own calculations show that per capita migration rates were highest in these states. This is plausible as these are the states with the smallest area (see also STBA (various years)).

substantially larger than those obtained for the full sample in **Table 2**.[19] This allows us to reject the hypothesis that unobserved mobility generated an upward bias in our estimates and suggests that the results in **Table 2** are attenuated.

A third condition for the identification of treatment effects is that our estimates are not affected by omitted variables. Given the limitations of our data we are concerned about family background and state level characteristics. Omitted parental characteristics cause a systematic bias if they are correlated with the state- and cohort-specific fee-to-earnings indicators. Such a correlation is particularly plausible if either the distribution of parental characteristics or the relevance of the intergenerational transmission of education changed over time.[20] We know that for those parent generations considered here the distribution of educational attainment was stable and that intergenerational correlation of educational success in Germany has not changed over time.[21] Therefore, the omission of parental controls should not affect the nature of our estimates.

To account for permanent unobserved state level heterogeneity we consider state fixed effects. In addition, one may argue that the estimation suffers from omitted controls for increasing incomes in post-war Germany. The speed of economic growth differed across federal states, and this may generate heterogeneous responses to fee abolition: inhabitants of poorer states may respond stronger to price changes of secondary education.[22] To address this problem one would ideally control for state-specific annual incomes. As an approximation we control for

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[19] Predictions based on grade 5 and on grades 5-7 in **Table 4** are identical because only those states remain in the sample which never experienced heterogeneous fees for grades 5-7.

[20] 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.

[21] Blossfeld (1993, p.57) analyzed these cohorts stating: "If we look at the changes in the highest general level of educational attainment across birth cohorts (...), we can observe a fairly stable structure of percentages for men and women up until the 1936-1940 birth cohort. Only beginning with the 1941-1945 cohort did the percentages of those with higher levels of educational attainment start to rise, first slowly and then increasingly rapidly" (see also Heineck and Riphahn 2009).

[22] 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.

real state-specific per capita gross domestic product as of the year when an individual would enter fifth grade.[23] These estimations yield results similar to those in **Table 2** (not presented to save space).

Even though controls for average per capita income do not affect the results, three arguments suggest that controls for changes in per capita incomes may well do so: 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, e.g., if opportunity costs of education increase with rising wages. To investigate whether such mechanisms affect our estimates, we reestimated our models adding controls for the growth of regional GDP per capita. The simulation results yield no substantial differences compared to prior findings and are not presented to save space.

A fourth set of issues that can affect the reliability of our estimates relates to three institutional features of the German post-war education system. (a) It is possible that admission requirements for Advanced Schools were not constant over time. Until today admission standards vary across federal states. If regulations changed when fees declined, this would bias our estimate of the fee effect. However, there is no evidence for such developments. Education policy in the 1950s focused on reconstruction of the school infrastructure and reorganization of the education system after the physical and ideological turmoil of the war. Education policy was concerned with issues such as school year and vacation dates, naming conventions for schools and grades, or the order of foreign language instruction.[24] Only in the early 1960s the expansion of the education system and with it admission to Advanced Schools started to be discussed (e.g. Furck

[23] GDP data is available since 1950, and for Berlin (and Saarland) only after 1960.

[24] These issues were regulated in the *Düsseldorfer Abkommen* of 1955.

1998, Fränz and Schulz-Hardt 1998, Deutscher Bildungsrat 1975). A binding agreement of the conference of state ministers of education (*Kultusministerkonferenz*) to raise the share of pupils attaining Advanced School education was signed in 1964 (*Berliner Erklärung* and *Hamburger Abkommen*), initiating the expansion process. Since fee abolitions took place earlier it is unlikely that modified admission standards and education expansion confound our measures.[25]

(b) Our measure of the fee abolition effect would be downward biased if, e.g. due to supply constraints such as the shortage of class rooms and teachers, Advanced School attendance was rationed even after fee abolition. Particularly for those states which abolished fees early after the war a scenario of supply constraints is plausible: immediately after the war there was a major shortage of school buildings, teaching materials, and teachers. Kuhlmann (1970) discusses that throughout the mid 1950s, insufficient school housing capacity was an acknowledged problem.

In order to account for potential state-level differences in supply conditions and their change over time, we add state-specific cohort trends to our baseline model as a robustness test.[26] **Table 5** presents the resulting simulated fee effects: while the effects are now smaller and less precise than before, the general pattern confirms prior findings, with larger effects for females than males.[27] It is not surprising to find more attenuated effects since a large number of control variables, in particular the state cohort effects, are correlated with the fee-to-earnings measure.

(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

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[25] Analyzing transitions between primary and secondary schools, Blossfeld (1990, p. 174) finds "The transitional behavior of the 1949-1951 cohort was not affected by the educational reforms that started in the middle of the 1960s." Kuhlmann (1970, p.1/15) concludes that the increase in the share of Advanced School graduates through the mid 1960s was due to increased stamina of pupils rather than eased admission criteria. He states that even after agreement on wider access to Advanced Schools was reached, admission rules did not become less selective (pp.1/23).

[26] We implicitly account for the constraints related to varying cohort sizes by controlling for birth cohort fixed effects.

[27] As an additional test for effects of the war, section 6.2 presents estimates after omitting states which abolished fees at a time of potentially rationed Advanced School access.

successful in promoting the bright poor this would reduce the measurable effect of fee abolition. While such support systems existed, their 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.

The fifth and final identification issue concerns potential anticipation of fee abolition. The abolition of secondary school fees took time. Some states abolished them in their constitutions, some gave in to the pressure of the occupation forces after WWII (Furck 1998), others followed a 1954 recommendation of the National Education Advisory Board to abolish fees (Bohnenkamp et al. 1966). If parents expected fee reductions, their behavioral response may have preceded the actual date when fees changed. In that case we underestimate the true effect of school fee abolition. We apply two tests to gauge the potential relevance of anticipation.

(i) So far, we considered the behavioral response to fee levels observable in the year youths leave primary school and assumed myopic parents. To test the robustness of our results to potential parental anticipation we generated alternative fee-to-earnings indicators which reflect cohort- and state-specific fee levels as actually realized over the first years of advanced school. **Table 6** presents the results. The fee effects based on grade 5 are identical to those in **Table 2**, as no anticipation is involved. However, when considering the annual effect of actual fees in the first three or six grades, we obtain generally more sizeable effects than in **Table 2**. This supports the conclusion that we may have underestimated the fee effect so far.

(ii) In an alternative approach, we investigate to what extent graduation probabilities vary with the number of years that pupils had to pay fees upon enrolling in Advanced School ("fee-years"). This provides the fee effect under the assumption that parents had perfect foresight with respect to the existence but not the level of fees. Panel A of **Table 7** shows the estimation results using the same specification as before, only substituting fee-years for fee-to-earnings ratios. Panel B presents the simulated effect of switching from full to no fee payment (9 vs. 0 years).

The number of years during which parents had to pay fees reduces the probability of Advanced School attainment. The abolition of fees is associated with increases in overall graduation rates of about 7 percent, with larger effects for females than for males. The predicted effects are only slightly smaller than those presented so far. This may indicate that parents (rationally) respond more strongly to "how much" rather than to "for how long" they have to pay. Overall, the findings presented in **Tables 6 and 7** confirm that parental anticipation of the abolition of school fees may have dampened our measure of the fee abolition effect and that educational attainment of females is more price responsive compared to that of males.

Additional Robustness Tests

Repercussions of the War. So far we estimated an average effect of fee reductions for the entire sample. Given that the early birth cohorts experienced the war in their childhood, their probability of graduating from Advanced School and their responsiveness to changes in school fees may differ from those of later birth cohorts. First, it seems plausible that parents whose school choice would respond to fee reductions in normal times are more risk averse in times of general stress and are less likely to invest in child human capital in (near-)war than in peace times. In this scenario, a given change in fees may call forth clearer responses for those attending school after the war. Second, we know that immediately after the war capacity constraints plagued the school system as buildings were destroyed, and heating as well as teaching materials were unavailable. If there was rationing in access to Advanced School immediately after the war, our estimates of fee effects might be downward biased.

As a test we replicated **Table 2** after dropping observations in those states from the sample, which abolished fees very early (Berlin, Bremen, and Hesse). The results in **Table 8** confirm expectations: the estimated effect of fee abolition now increases by more than 50 percent for all samples and for females it even doubles. This confirms that prior results may be attenuated

by repercussions of the war. In further estimations (not presented to save space) we interacted the fee-to-earnings ratio with an indicator for wartime observations (cohorts 1930-1938). While the coefficient of the added interaction term was never precisely estimated it was always positive suggesting smaller effects of fee abolition for birth cohorts who had experienced the war.

Uncertainty of Advanced School Entry Age. Up to now we assumed that pupils transferred to Advanced School at the regular age, i.e. after four years of primary school. However, particularly after the war with destruction and massive population displacement it is possible that educational grade progression was protracted. To test the sensitivity of our results to the assignment of an Advanced School entry age the estimations in **Table 2** were repeated, assuming that the birth cohorts 1933-1938 took an additional year to finish primary school. The results (not presented) show even larger fee abolition effects. Thus, the results in **Table 2** appear to be attenuated also by the uncertainty of school progression at the time around the end of the war.

Alternative Specifications. So far we interpreted the fee effect based on the fee-to-earnings measures relevant either for grade 5 only, for the sum of grades 5 to 7, or for the sum of grades 5 to 10. In additional estimations, we considered four alternative specifications: (a) a model controlling for the 9 individual grade-specific fee-to-earnings ratios, (b) considering only grades 5 to 10 in six individual measures, (c) considering only grades 5 to 7 in three individual measures, and (d) considering three indicators of the sums of grades 5 to 7, 8 to 10, and 11 to 13. The predicted effects of joint changes in yearly ratios by ten percent or of the three-year ratios by 30 percent confirm prior results: we obtain average effects between 8.6 and 9.1 percent for the full sample and somewhat larger effects for females (results not presented to save space).

VII. 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, which is difficult to obtain (Johnson 2004). 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 upper secondary schools were abolished at different points in time between 1947 and 1962 across eleven federal states. This heterogeneity is used to identify the fee effect.

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

In our baseline specification, we regress individual Advanced School attainment on an indicator of the fee level, holding constant birth cohort and state fixed effects. While the coefficient is imprecisely measured, fee abolition is associated with an increase in Advanced School attainment of about ten percent for females and about seven percent for males.[28] Three reasons suggest that these results are attenuated and underestimate the true effect: first, we observe individuals only in their current state of residence instead of the state of residence in their youth. When we omit individuals from states with above average mobility, our estimates of the average fee effect increase from eight to twelve percent. Second, controlling for possible parental anticipation and considering individually experienced fee-to-earnings ratios instead of those observed when a child entered fifth grade yields larger fee effects up to almost 14 percent for females and eight percent for males. Third, when we drop those states from the sample which abolished school fees immediately after the war and where rationing might have limited Advanced School access, the predicted fee abolition effect increases to at least 18 percent for females and 9 percent for males in the most conservative model.

In addition, while we estimate an average fee abolition effect, the effect may be larger for the population that is truly at risk of not attending Advanced School due to financial constraints.

[28] In terms of 2009 earnings the simulated fee change from ten to zero percent of annual incomes is equivalent to a nominal change in annual fees by 2,800 Euros.

Unfortunately, it is not possible to evaluate the effect for this subsample with our data. Also, the effect of fee abolition on educational *attainment* may hide much larger effects on *enrollment* if children from price-sensitive households receive less support in completing their degrees than those of wealthy parents.[29] All this suggests that our baseline results underestimate the true fee abolition effect. Our results may overstate the true fee effect if fee abolition coincided with an increase in school availability. However, since fee abolition was not part of a general effort to ease access to education and because the overall education expansion commenced only in the mid 1960s this seems to be a minor concern.

The estimated fee effect must be compared to actual changes in Advanced School education over time: average graduation rates about doubled between the birth cohorts of the 1930s and the 1950s. An average fee abolition effect of 8 or 9 percent is relatively modest when compared to this increase in graduation rates by 100 percent. However, our largest estimate for females amounts to a fee effect 23.9 percent (see **Table 8**). Given that this is still an average for the entire population it is plausible that for liquidity-constrained parts of the population the fees exerted substantial effects.

Küchenhoff (1952) and Bergmann (1955) pointed out that even 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 following the abolition of fees school funding and quality declined. Evidence on school quality and 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 fees and other contributions.

[29] Klafki (1985, p.153) describes that up and through 1960 only half of those who attended Advanced School at age 13-14 (grades 7 and 8) finally attained the *Abitur* degree. He suggests that the selection was explicitly based on parental social background.

Therefore it is unlikely that fee abolition caused a noticeable decline in the quality of education.

Finally, it would be of interest to know the returns to Advanced School degrees over time. Unfortunately, there is no earnings data available for the 1950s and 1960s to estimate these returns. Evidence for the 1980s suggests that Advanced School attainment earned a return of about 20 percent for men and about 30 percent for women (Lauer and Steiner 2000).

Overall, our results suggest that school fees for Advanced Schools did indeed set barriers to higher education which for the time of our data affected females more than males. The abolition of school fees explains at least a small part of the increase in educational attainment observed in the last decades.

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Table 1 Abolition Phases and Affected Birth Cohorts

	Year of Fee Abolition	Original Fee Amount (RM or DM)	Last Cohort with Full Fee at Entry	First Cohort without Fee at Entry
Bremen	1947	240	1936	1937
Hesse	1947	240	1935	1936
Berlin	1948	240	1937	1938
Bavaria	1951	200	1936	1940
Schleswig-Holstein	1952	240	1935	1936
Baden-Württemberg	1957	200	1941	1946
Hamburg	1957	240	1939	1946
Lower-Saxony	1959	240	1943	1948
NRW	1959	240	1947	1948
Saarland	1959	n.a.	n.a.	n.a.
Rhineland-Palatinate	1962	240	1950	1951

Notes: Further institutional detail is available upon request. See also Benatzky (2001).

Table 2 Estimation and Simulation Results - Logit on Advanced School Attainment

		1 - All		2 - Females		3 - Males	
		Coeff.	[SE1] [SE2]	Coeff.	[SE1] [SE2]	Coeff.	[SE1] [SE2]
A	**Estimation**						
	fee / earnings grade 5	-.904	[.377]* [.816]	-1.037	[.445]* [.892]	-.748	[.446]∘ [.715]
	Log Likelihood	-151,125.21		-63,414.26		-86,018.44	
	Simulation						
	Fee effect - grade 5	8.24 %	(.032)**	9.83 %	(.055)∘	6.55 %	(.034)*
B	**Estimation**						
	fee / earnings grade 5-7	-.324	[.135]* [.291]	-.357	[.158]* [.325]	-.274	[.158]∘ [.248]
	Log Likelihood	-151,125.94		-63,414.46		-86,018.23	
	Simulation						
	Fee effect - grades 5-7	8.88 %	(.033)**	10.16 %	(.056)∘	7.23 %	(.035)**
C	**Estimation**						
	fee / earnings grade 5-10	-.168	[.072]* [.158]	-.177	[.083]* [.181]	-.147	[.084]∘ [.133]
	Log Likelihood	-151,125.94		-63,414.46		-86,018.10	
	Simulation						
	Fee effect - grades 5-10	9.21 %	(.033)**	10.09 %	(.057)∘	7.76 %	(.036)**

Notes:
1. **, *, and ∘ indicate statistical significance at the 1, 5, and 10 percent level.
2. All models control for birth cohort and federal state fixed effects plus a constant.
3. Standard errors 1 [SE1] of the coefficient estimates are corrected for clusters at the state-birth cohort level, standard errors [SE2] are corrected for clusters at the state level. The standard errors for the simulation results (in parentheses) are bootstrapped using 100 draws from the original data.
4. The simulations provide the percent increase in the predicted probability of Advanced School attainment after the annual fee-to-earnings ratio was reduced by 10 percentage points per year, i.e. by 10, 30, and 60 points in Panels A, B, and C, respectively.
5. The full, female and male samples hold 412,475, 206,884 and 205,591 observations.

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

Table 3 Linear Models of Fee-to-Earnings Ratio at Grade 5

	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	-.011	.008	-	-	-	-	-	-	-	-	-.011	.008
Cohort share with A.S. degree (t-1)	-	-	-.040	.145	-	-	-	-	-	-	-.366	.310
Cohort share with A.S. degree (t-2)	-	-	-.075	.109	-	-	-	-	-	-	.022	.139
Cohort share with A.S. degree (t-3)	-	-	-.152	.132	-	-	-	-	-	-	x	x
Joint Test (p-value)	-	-		(.625)	-	-	-	-	-	-		(.406)
Growth in cohort share w A.S. deg.(t vs. t-1)	-	-	-	-	-.122	.081	-	-	-	-	-.385	.227
Growth in cohort share w A.S. deg.(t vs. t-2)	-	-	-	-	-.086	.105	-	-	-	-	x	x
Growth in cohort share w A.S. deg.(t vs. t-3)	-	-	-	-	.016	.112	-	-	-	-	.116	.166
Joint Test (p-value)	-	-	-	-		(.104)	-	-	-	-		(.174)
Income per capita (t-1)	-	-	-	-	-	-	-.003	.002	-	-	-.002	.004
Income per capita (t-2)	-	-	-	-	-	-	.003	.002	-	-	.003	.003
Income per capita (t-1) missing	-	-	-	-	-	-	-.063	.034○	-	-	x	x
Income per capita (t-2) missing	-	-	-	-	-	-	.037	.022	-	-	.045	.054
Joint Test (p-value)	-	-	-	-	-	-		(.257)	-	-		(.345)
Growth in income per cap. (t vs. t-1)	-	-	-	-	-	-	-	-	.134	.075	-.042	.116
Growth in income per cap. (t vs. t-2)	-	-	-	-	-	-	-	-	-.057	.049	.035	.112
Growth in income per cap. (t vs. t-1) missing	-	-	-	-	-	-	-	-	-.013	.010	.058	.056
Growth in income per cap. (t vs. t-2) missing	-	-	-	-	-	-	-	-	-.001	.009	x	x
Joint Test (p-value)	-	-	-	-	-	-	-	-		(.054)○		(.295)

Notes: **, *, and ○ indicate statistical significance of the coefficients at the 1, 5, and 10 percent level. Standard errors are clustered at the state level. All models control for a constant and a set of (highly significant) calendar year effects. 210 observations are used. Cells in column 6 are marked x if the coefficient could not be estimated due to multicollinearity.

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

Table 4 Simulated Changes in Graduation Rates Following Fee Reductions - Based on Estimations without Observations from Berlin, Bremen, Hamburg, Schleswig-Holstein, and Rhineland-Palatinate (Predicted Fee Effects in Percent)

	1 - All	2 - Females	3 - Males
Fee effect - grade 5	12.7 % (.037)**	12.2 % (.069)○	11.5 % (.045)*
Fee effect - grades 5-7	12.7 % (.037)**	12.2 % (.069)○	11.5 % (.045)*
Fee effect - grades 5-10	12.9 % (.037)**	12.3 % (.069)○	11.8 % (.045)*

Notes: See **Table 2**. The estimation on the full sample used 343,270 observations, the regressions for females and males 172,150 and 171,120 observations, respectively.

Table 5 Simulated Changes in Graduation Rates Following Fee Reduction Based on Estimations with Controls for State-Specific Linear Cohort Effects (Predicted Fee Effect in Percent)

	1 - All	2 - Females	3 - Males
Fee effect - grade 5	4.44 % (.032)	6.18 % (.054)	3.11 % (.040)
Fee effect - grades 5-7	5.05 % (.032)	6.32 % (.055)	3.91 % (.043)
Fee effect - grades 5-10	5.49 % (.033)	6.16 % (.055)	4.73 % (.045)

Notes: See **Table 2**.

Table 6 Simulated Changes in Graduation Rates when Fee Ratios are Perfectly Anticipated (Predicted Fee Effect in Percent)

	1 - All	2 - Females	3 - Males
Fee effect - grade 5	8.24 % (.027)**	9.83 % (.045)*	6.55 % (.043)
Fee effect - grades 5-7	10.82 % (.036)**	13.51 % (.054)*	8.24 % (.047)○
Fee effect - grades 5-10	10.91 % (.044)*	13.89 % (.070)*	8.15 % (.051)

Notes: See **Table 2**.

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

		1 - All		2 - Females		3 - Males	
		Coeff.	[SE1] [SE2]	Coeff.	[SE1] [SE2]	Coeff.	[SE1] [SE2]
A	**Estimation**						
	fee-years	-.008	[.004]* [.010]	-.008	[.005]○ [.009]	-.007	[.004] [.009]
	Log Likelihood	-151,126.24		-63,414.75		-86,018.94	
B	**Simulation of the Attainment Effect of Reducing Fee-Years from 9 to 0 years**						
	fee-year effect	6.74 %	(.027)*	8.13 %	(.045)○	5.19 %	(.032)

Notes:
1. In Panel A, **, *, and ○ indicate statistical significance at the 1, 5, and 10 percent level. Standard errors 1 [SE1] of the coefficient estimates are corrected for clusters at the state-birth cohort level and standard errors 2 [SE2] for clusters at the state 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. All models control for a full set of birth cohort and state fixed effects plus a constant.
3. The simulations provide the average percent increase in the predicted probability of Advanced School attendance in a situation with zero compared to nine years of fee payment over all observations.

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

Table 8 Simulated Changes in Graduation Rates when States with Early Abolition Dates are Omitted (Bremen, Berlin, Hesse) (Predicted Fee Effect in Percent)

	1 - All		2 - Females		3 - Males	
Fee effect - grade 5	13.54 %	(.043)**	17.95 %	(.062)**	9.26 %	(.045)*
Fee effect - grades 5-7	15.84 %	(.048)**	20.45 %	(.069)**	11.02 %	(.051)*
Fee effect - grades 5-10	18.10 %	(.052)**	23.90 %	(.077)**	12.61 %	(.057)*

Notes: See **Table 2**.

Figure 1 Representation of Fee Effects

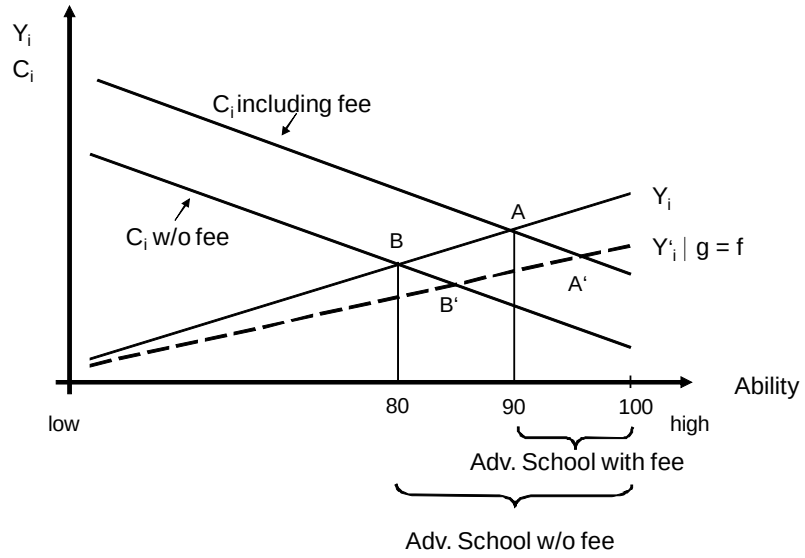
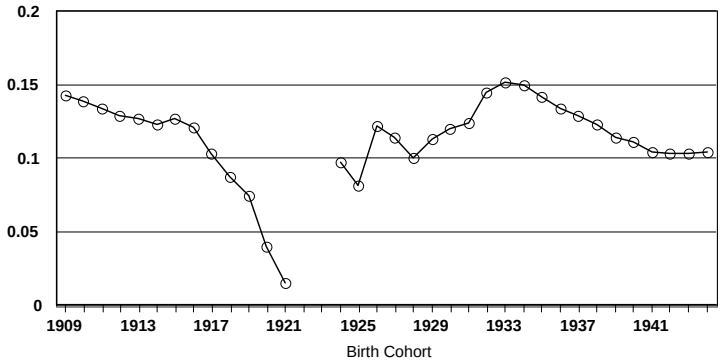


Figure 2 Sketch of the Traditional German Schooling System

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	

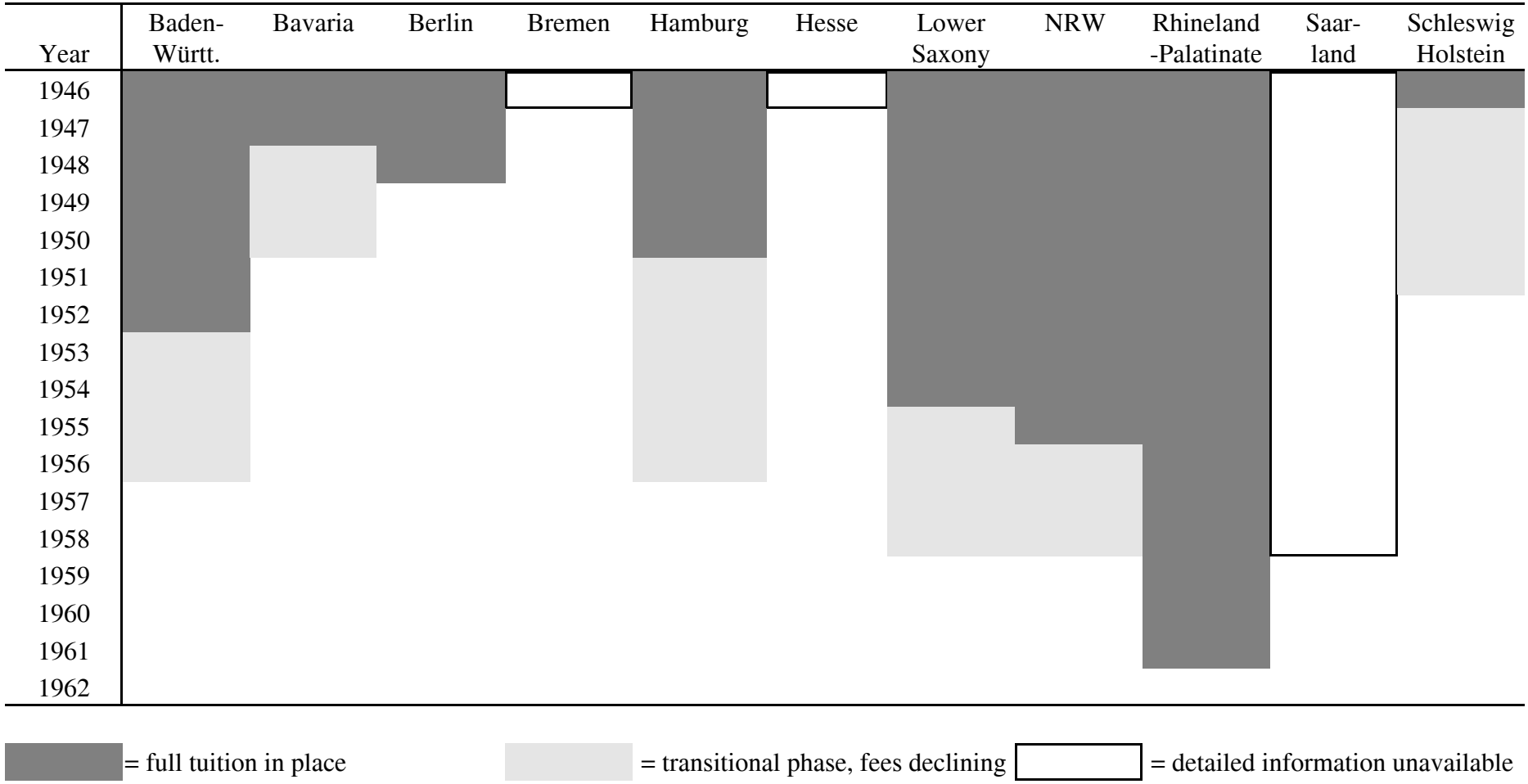
Figure 3 Prussian School Fees for Advanced Schools as Share of Average Earnings



Source:(i) Earnings data from retirement insurance statistics (annual average gross earnings: Deutsche Rentenversicherung 2008). Figures are not available during high inflation years 1922 and 1923.

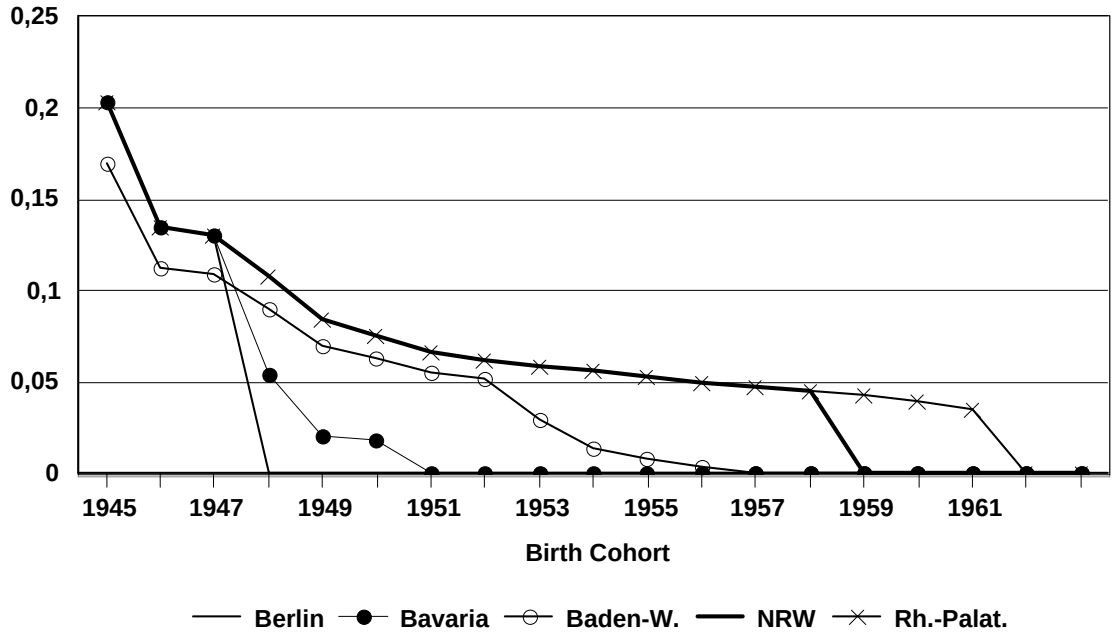
(ii) Tuition fees from various sources (Benatzky 2001).

Figure 4 State-Specific Patterns of Tuition Abolition



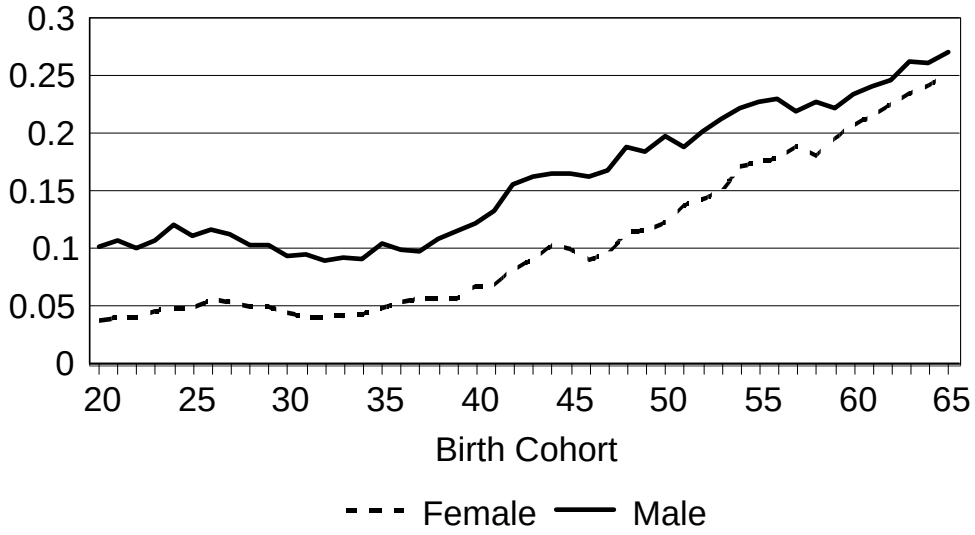
Source: Benatzky (2001). For details on the transitional phase see notes to **Table 1**.

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



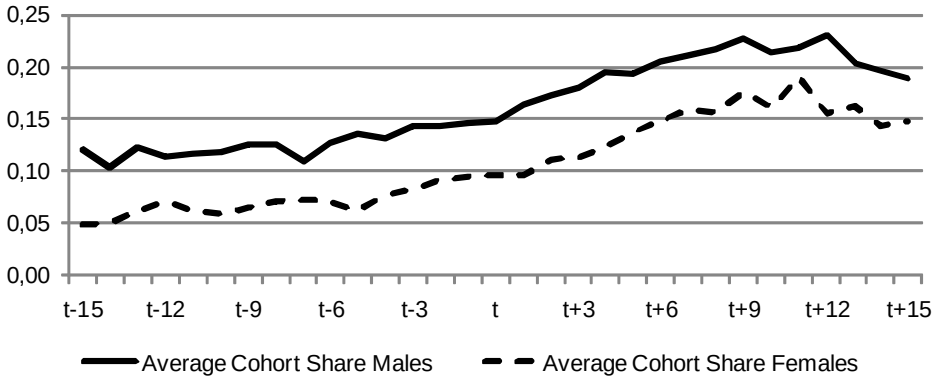
Source: Earnings data from retirement insurance statistics, tuition fees from various sources (Benatzky 2001).

Figure 6 Population Shares with Advanced School Degree by Birth Cohort



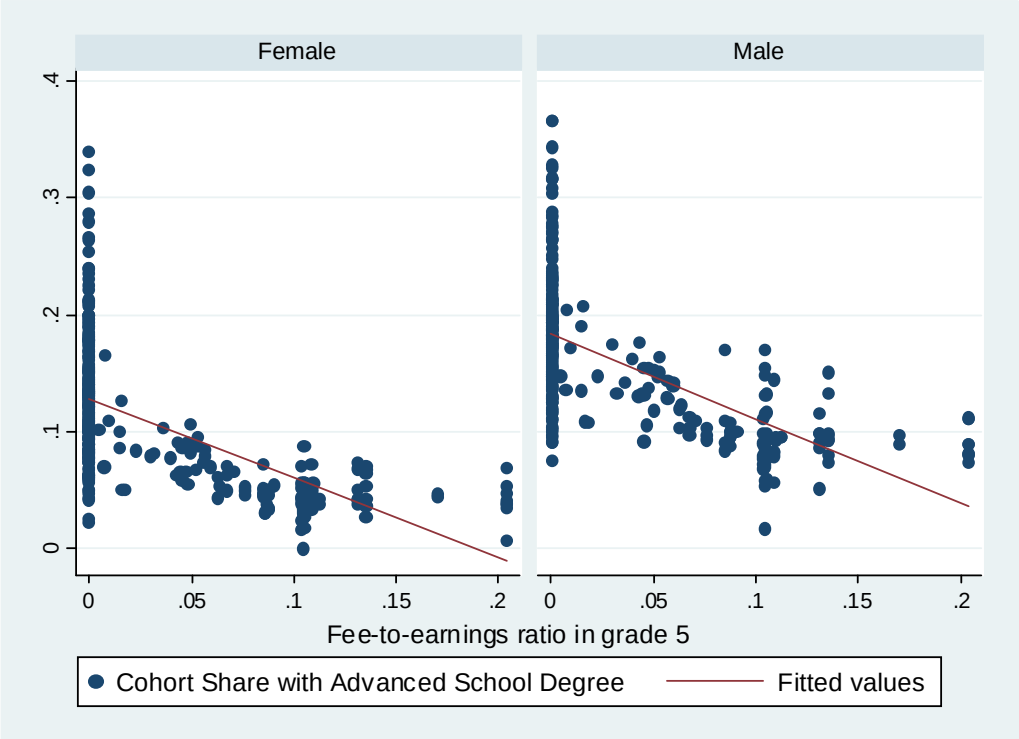
Source: Own calculations based on *Mikrozensus* data 1989, 1993, 1997.

Figure 7 Cohort Advanced School Graduation Rate Indexed at Fee Abolition (t)



Note: Averaged state cohort graduation rates are indexed t for the cohort which was first to experience no fee at entry to Advanced School.
Source: Own calculations based on *Mikrozensus* data 1989, 1993, 1997.

Figure 8 Cohort Advanced School Graduation Rate by Fee-to-Earnings Ratio as of Grade 5



Note: Each mark represents average Advanced School attainment in a given birth cohort and state.
Source: Own calculations based on *Mikrozensus* data 1989, 1993, 1997.

Appendix-Table A1: Descriptive Statistics

Variable	All		Female		Male	
	Mean	Std. D.	Mean	Std. D.	Mean	Std. D.
	[Min	Max]	[Min	Max]	[Min	Max]
Advanced school degree (0/1)	.126	.332	.098	.298	.154	.361
Year of birth	44.7	8.65	44.7	8.67	44.8	8.63
	[30.0	59.0]	[30.0	59.0]	[30.0	59.0]
Fee/earnings grade 5 (myopic)	.040	.053	.040	.053	.039	.052
	[0.00	.204]	[0.00	.204]	[0.00	.204]
Fee/earnings grades 5-7 (myopic)	.120	.157	.121	.157	.118	.156
	[.000	.611]	[.000	.611]	[.000	.611]
Fee/earnings grades 5-10 (myopic)	.240	.314	.242	.315	.237	.313
	[.000	1.222]	[.000	1.222]	[.000	1.222]
Fee/earnings grade 5 (foresight)	.040	.053	.040	.053	.039	.052
	[0.00	.204]	[0.00	.204]	[0.00	.204]
Fee/earnings grades 5-7 (foresight)	.108	.145	.109	.145	.106	.144
	[.000	.470]	[.000	.470]	[.000	.470]
Fee/earnings grades 5-10 (foresight)	.182	.260	.185	.262	.180	.259
	[.000	.786]	[.000	.786]	[.000	.786]
Fee-years (foresight)	3.17	3.84	3.20	3.85	3.13	3.83
	[0.00	9.00]	[0.00	9.00]	[0.00	9.00]
GDP p.c. (in 1000 1991 DM)	13.98	11.37	13.91	11.40	14.05	11.34
	[0.00	73.62]	[0.00	73.62]	[0.00	73.62]
GDP p.c. missing (0/1)	.297	.457	.301	.459	.294	.455
GDP p.c. growth	.161	.851	.160	.849	.162	.853
	[-.594	4.99]	[-.594	4.99]	[-.594	4.99]
GDP p.c. growth missing (0/1)	.339	.473	.343	.475	.336	.472
State 1: Schleswig Holstein (0/1)	.045	.207	.045	.206	.046	.209
State 2: Hamburg (0/1)	.024	.154	.025	.156	.024	.152
State 3: Lower Saxony (0/1)	.118	.323	.117	.322	.119	.323
State 4: Bremen (0/1)	.010	.101	.010	.101	.010	.100
State 5: NRW (0/1)	.271	.445	.274	.446	.269	.444
State 6: Hesse (0/1)	.097	.296	.097	.295	.098	.297
State 7: Rhineland-Palatinate (0/1)	.069	.253	.068	.253	.069	.254
State 8: Baden-Württemberg (0/1)	.152	.359	.152	.359	.152	.359
State 9: Bavaria (0/1)	.194	.395	.193	.395	.194	.396
State 10: Berlin (0/1)	.010	.138	.019	.138	.019	.137
Number of observations	412,475		206,884		205,591	

Source: Own calculations based on *Mikrozensus* data 1989, 1993, 1997. Observations from Saarland were not used because of insufficient fee information. Fee/earnings ratios are in percent. Fee-years are calculated based on individual birth cohort and state of residence assuming normal entry age to Advanced School. Variables labeled "myopic" are coded based on the regulations in place when a pupil entered grade 5. Variables labeled "foresight" are coded based on the observed developments for a given pupil. Real p.c. GDP by state and year was provided by the Statistical Office of Baden-Württemberg. It is unavailable before 1950 and for Berlin only since 1960.