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Heterogeneity in long-term returns to education: An inconvenient truth

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Heterogeneity in Long-Term Returns to Education – An Inconvenient Truth

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Heterogeneity in Long-Term Returns to Education

An Inconvenient Truth*

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October 26, 2022

Abstract

This paper studies the long-term relationship between parental and child education in Germany, where children are tracked into academic and non-academic track schools at the age of 10. On average, children are more likely to attend an academic track school if their parents attended one. Estimating marginal treatment effect curves, we find that there is no effect for disadvantaged individuals, suggesting that educational policies attempting to improve the educational prospect of disadvantaged individuals may fail to reduce inequalities in the long run. Low labor market returns despite better education is the main explanation for the null effect for these individuals.

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1 Introduction

Western societies have the ambition to provide equal chances for every child.¹ However, low social status is often passed down to the next generation, because children from disadvantaged backgrounds have a hard time to overcome initial inequalities (for surveys, see Black and Devereux 2011; Holmlund et al. 2011; Corak 2013). For instance, children of parents in OECD countries without a secondary school degree have a four times lower likelihood of going to university than children of university graduates.² Because education is one of the most important determinants of economic success and social status in adulthood (OECD 2015), educational inequalities in childhood often lead to long-term disadvantages. This intergenerational dependence of social inequality increased in recent years, and reached its peak because of the COVID-19 pandemic.

This paper analyzes the long-term relationship between parental and child education in Germany, a country where early tracking at the age of 10 provides a clear distinction between academic and non-academic track schools (Dustmann et al. 2017). In particular, we estimate the causal effect of parents' academic track school enrollment on their children's likelihood of academic track school enrollment using the German Educational Expansion in the 1960s and 1970s as quasi-natural experiment, which improved the local supply of academic track schools. In contrast to previous studies that estimate local average treatment effects (LATE), which are average effects for the compliers to a specific policy, we go one step further and estimate average treatment parameters for clearly defined populations of interest, such as the average treatment effect on the treated (ATT), the average treatment effect on the untreated (ATU), and the average treatment effect (ATE) using the marginal treatment effect (MTE) of Björklund and Moffitt (1987) and Heckman and Vytlacil (1999, 2001, 2005, 2007). This allows the intergenerational dependence of education to vary with the ability and social status of an individual (i.e., unobserved characteristics that prevent one from attending an academic track school) and we can draw more precise conclusions for a larger share of the population than is usually targeted by educational policies.

We find that the German Educational Expansion was successful in drawing more and more students into academic track schools through an improved supply of these schools. This variation is then used to identify the causal effect of academic track school enrollment on the likelihood of the child's academic track school enrollment using instrumental variable methods. After controlling for differing initial conditions in the districts at the

¹This led to the establishment of the OECD Centre on Well-being, Inclusion, Sustainability, and Equal Opportunity (WISE), which attempts to analyze and reduce inequalities (<https://www.oecd.org/wise/>, last access: 17 August 2022).

²<https://www.oecd.org/inclusive-growth/#inequality-puts-our-world-at-risk>, last access: 16 August 2022.

time of the parent’s tracking decision and the outcome of the educational expansion at the time of the child’s tracking decision, the number of academic track schools should be independent of any unobserved factor potentially affecting the child’s school track choice. Estimating 2SLS regressions, we find that children of those individuals drawn by the additionally built schools into an academic track school are 39 percentage points more likely to enroll in an academic track school themselves.

Although such a positive effect from 2SLS regressions suggests that supply-side policies can be effective in reducing inequalities *on average*, results from the MTE estimations suggest strong heterogeneity in the sense that only advantaged individuals gain from attending an academic track school, while disadvantaged individuals do not exhibit any positive treatment effect. This finding is stable across a variety of robustness checks, including flexible (non-)parametric shape assumptions of the MTE, alternative first-stage specifications, and alternative definitions of the instrument. Allowing this heterogeneity in unobserved characteristics from the MTE curve to differ between potential outcomes (Brinch et al. 2017), we can show that our results are driven by the fact that disadvantaged individuals would have gained more in the counterfactual scenario had they attended a non-academic instead of an academic track school. This means that children of disadvantaged individuals would gain more if their parents had been enrolled in non-academic track schools instead, which fit their abilities better. This is also confirmed by estimating heterogeneous returns in the labor market to attending an academic track school. Although disadvantaged individuals improve their cognitive abilities similarly to their more advantaged peers, they do not gain with respect to earnings. These lower returns in the labor market explain why the improved educational status of the parent does not translate into an improved educational prospect of the child.

Our study provides important implications for policy makers. First, educational policies attempting to draw more children into academic schools (e.g., through improved access) can be effective in providing better educational opportunities for the directly affected individuals, but may be of limited effectiveness in reducing long-term inequalities. Second, our results suggest that tracking provides a good match between abilities and curriculum, and is therefore important for individuals to reach their full potential. This challenges previous findings criticizing tracked school systems for their negative impact on educational equality (see Hanushek and Wößmann 2006; Biewen and Tapalaga 2017).

The literature on the subject of intergenerational mobility is twofold. The first strand of literature deals with the effect of parents’ income. A wide range of studies belongs to the first strand of literature (i.e., parents’ income, see Heckman 2008, for an overview). These studies find positive effects of parental income on child development, including behavior and emotional well-being (Dooley and Stewart 2007; Violato et al. 2011) as well

as cognitive skills (Blau 1999; Shea 2000; Taylor et al. 2004) and health (Propper et al. 2007).

Further studies find a positive impact of parents' income on child's education (Mazzonna 2014; Elstad and Bakken 2015; Chetty et al. 2020). For instance, the results of Chetty et al. (2020) show that college enrollment depends on parents' income. Children with high income parents have a higher probability to be accepted from colleges whose graduates achieve high incomes. In contrast, Løken (2010) finds no causal relationship between parental income and children's educational attainment.

The second strand of literature analyzes the effect of parents' education on children's outcomes estimating long-term returns to education. Several studies state that family background such as parents' education has a larger impact on child outcomes than previously studied income (Blau 1999; Heckman 2008). It is a consistent finding that children have a comparative advantage if their parents have a higher education level (see Francesconi and Heckman 2016, for an overview). For instance, Black et al. (2005), Oreopoulos et al. (2006), Lundborg et al. (2014) and Güneş (2015) use changes in compulsory schooling laws in Norway, the United States, Sweden, and Turkey, respectively, to identify mostly positive effects of parental education on a wide range of child outcomes, such as (non-)cognitive skills, grades and health.³

A smaller number of studies addresses the research question of our study — the effect of parents' educational attainment on children's educational attainment — mainly by using an instrumental variable approach. Because education is an important determinant for labor market success (OECD 2015), the relationship between parental and child education is particularly relevant for intergenerational mobility and social inequality. Behrman and Rosenzweig (2002) and Plug (2004) find evidence for an effect of father's schooling on the child's schooling by using twin and adoptee data. Using information of adopted children as well as their biological and adoptive parents, Björklund et al. (2006) is able to differentiate between prebirth factors, such as genes and prenatal environment, and postbirth factors, including childhood environment. The results suggest that both pre- and postbirth factors lead to intergenerational transmission of education. Finally, Dickson et al. (2016) analyze the effect of parents' education on their child's education by exploiting the shift in parents' education levels influenced by the minimum school leaving age reform in England in 1972. The authors find a positive effect of the increased education of parents on children's education. The effect is evident in preschool assessments at age 4, and even

³Further studies estimating the causal effect of parental education on child outcomes are McCrary and Royer (2011) and Carneiro et al. (2013), among others. For instance, Carneiro et al. (2013) find intergenerational effects for maternal education as well. To identify causal effects, they use different instruments of changes in schooling costs during the mother's adolescence across counties and cohorts. The results show that mother's education increased the child's skills in math and reading, especially in early childhood (ages 7–8). The effects are smaller for older children (ages 12–14).

up to high-stakes examinations taken at age 16 positive effects can be found.⁴

We add to this literature by using quasi-random variation from the German Educational Expansion to identify effects of parental education on their children’s education that vary with unobserved ability and social status. To the best of our knowledge, we are the first to estimate this intergenerational persistence coefficient in such a flexible way, allowing us to shed further light onto the mechanisms through which the intergenerational transmission of education works. Although we can confirm a positive effect *on average* similar to the literature (i.e., LATE), our findings suggest that an improved educational status for disadvantaged individuals does not translate into educational improvements for their children. Studies that do not take this heterogeneous pattern into account may falsely conclude that educational policies underlying their treatment effects are effective measures in reducing inequality in the long run.

We also contribute to the literature evaluating the (long-term) effects of tracking students at an early age into different schools based on their ability. Opponents of tracking systems often argue that tracking low-ability students into paths with lower earnings perspectives foster inequality and economic disadvantages. Early studies concluded that low-ability students do not face negative outcomes when being tracked.⁵ While Figlio and Page (2002) compared students with similar characteristics in tracked and non-tracked schools in the United States, Lefgren (2004) used variation in tracking policies in Chicago public schools, arriving at the same conclusion. No negative effects were also found using a reform in Romania that postponed tracking (Malamud and Pop-Eleches 2011) and a reform in Sweden that assimilated both tracks by increasing the academic content taught in the lower track (Hall 2012). In contrast, Hanushek and Wößmann (2006) used PISA data to compare tracking systems on the country level, showing that educational inequality is higher in countries that track students at an early age. Similarly, Kerr et al. (2013) studied the abolishment of the tracking system in Finland, finding positive effects for disadvantaged students after attending a uniform school instead. For Germany, Biewen and Tapalaga (2017) pointed out that enrollment in higher tracks as well as ‘second choice’ options, which are supposed to give disadvantaged individuals the chance to revise earlier tracking decisions, are very selective, supporting the argument that tracking is fostering inequality. Dustmann et al. (2017) used these ‘second choice’ options to explain why switching from the lower to the higher track has no effect on long-term outcomes such as earnings, suggesting no disadvantage for lower-track students. Our analysis of long-term returns to education in the presence of a tracking system allows us to add further insights on the effectiveness of tracking at an early age in bringing up the full potential of students.

⁴In addition, the studies of Sacerdote (2007), Pronzato (2012), and Amin et al. (2015) estimate intergenerational returns to education, among others.

⁵See Betts (2011) for a review of these early studies.

Similar to Dustmann et al. (2017), we cannot find positive (long-term) effects of enrolling in an academic instead of a non-academic track school for disadvantaged individuals. Allowing for heterogeneity in unobserved characteristics and evaluating potential outcome curves for the untreated and treated state separately (Brinch et al. 2017), we are able to show that this finding is due to low-ability individuals gaining from enrolling in a lower track instead of a higher track. Our study is therefore closest to Dustmann et al. (2017) and provides another explanation for the zero effect for marginal individuals. In particular, our results suggest that the match of abilities and the curriculum in the lower track targeted at disadvantaged individuals would help them in providing better educational prospects for their offspring. The false assignment into higher tracks, however, does neither improve their labor market returns (i.e., earnings) nor the educational prospects of their children.

The remainder of this paper proceeds as follows. In Section 2, we describe the background of the education system during the period of the German Educational Expansion. In Section 3, we present the empirical framework that will be used to estimate the causal effect of parental on child education. In particular, we discuss the identifying assumptions in the context of a plausibly exogenous policy change (i.e., the German Educational Expansion). In Section 4, we introduce the data and present descriptive statistics of the variables used in the empirical analysis. In Section 5, we present results from first-stage selection equations, OLS and 2SLS regressions, and MTE estimations. Furthermore, we discuss robustness of our main results. In Section 6, we shed further light on the selection behavior, discussing potential explanations for our findings. Finally, in Section 7, we discuss our results, compare them with the literature, and conclude.

2 Institutional Background

2.1 The German Educational System

Children in Germany attend elementary school until the age of 10 (*Grundschule*, 4 years), after which they are tracked into either a basic track (*Hauptschule*, 5 years), an intermediate track (*Realschule*, 6 years), or an academic track (*Gymnasium*, 9 years)⁶ following a recommendation from the elementary school teacher depending on their skills and grades.⁷

⁶Various studies investigated determinants for enrolling in such academic track schools or its effect on later life outcomes, labeling these schools differently in English. Dustmann (2004) calls them *high schools*, Jürges et al. (2011) call them *grammar schools*, and Kamhöfer and Schmitz (2016) call them *academic schools*. We will call them *academic track schools* to highlight the importance of the tracking involved.

⁷This applies to the birth cohorts from 1947 to 1965 followed in this study. Meanwhile, in most German states parents are free to choose the secondary school despite receiving a recommendation of the elementary school teacher. Also, in most states the basic track schools are replaced with integrated schools offering the basic as well as the intermediate track.

While the basic and the intermediate track teach more practical skills oriented at the labor market, the academic track prepares for tertiary education. Children from the basic and intermediate track usually receive vocational training with part-time on-the-job training and part-time practical schooling. Because graduating from an academic track school and receiving the *Abitur* is a requirement to enroll in post-secondary education, many children enroll in tertiary education after graduating from the academic track (65% in our sample).⁸

Although many studies focus on the college enrollment margin (for Germany, see Kamhöfer et al. 2019), we believe that the secondary school track choice in Germany is the more interesting margin (Dustmann et al. 2017). Individuals who attended an academic track school not only gain from improvements in cognitive abilities through the academic-oriented curriculum, but also in terms of option value. After graduating from an academic track school, individuals can not only choose all possible post-secondary options individuals from non-academic track schools have (i.e., vocational training), but also have the opportunity to enroll in tertiary education. Therefore, it is not surprising that these individuals obtain higher labor market returns in the long-run (Dustmann 2004).

At the end of elementary school, teachers recommend one of the three tracks to each student based on her academic ability independent of socioeconomic background. The idea is to provide each student with the same opportunity to realize her full academic potential. Although this recommendation is binding upwards, parents could always choose to enroll their children in the basic or intermediate track despite receiving a recommendation for the academic track. This means that only parents of children with an academic track recommendation face the decision of where to enroll their child in our setting.⁹

2.2 The German Educational Expansion

During the 1960s and 1970s, local authorities tried to expand supply of and access to institutions offering tertiary education with the aim to increase the educational attainment of the population (Bartz 2007). Since enrollment in tertiary education requires graduation from an academic track school, the supply of and access to these schools was also improved. This so-called German Educational Expansion (*Bildungsexpansion*) led to an increase in academic track schools from 1,423 in 1957 to 2,029 in 1975 in the whole country.

Historically, there are three main reasons for the educational expansion (see Bartz 2007; Kamhöfer et al. 2019). First, educational attainment of the German population after the war was comparably low because of the “anti-intellectualism” practiced by the

⁸See Biewen and Tapalaga (2017) for a detailed description of the German education system with a focus on the different school tracks and ‘second choice’ options that allow to reverse earlier decisions.

⁹Dustmann et al. (2017) calls these children “marginal students.”

Third Reich. Therefore, the population had to catch-up which required more and better schools. Second, a rapid change in production technologies required more skilled workers, shifting the educational demand curve upwards (for a similar argument in the US context, see, Goldin and Katz 2010). Third, politicians thought of education as a mean to rival the communist Eastern part of Germany and, therefore, placed a particular emphasis on the improvement of educational opportunities.

3 Empirical Framework

3.1 Ordinary Least Squares

In a first step, we estimate standard OLS regressions. We denote the parents' education with D , the child's education with Y , and control variables added to the regression as the vector X . If the parental schooling decision is exogenous to unobserved characteristics potentially affecting their child's schooling conditionally on X , we can recover the causal effect of parents' education on their child's education with a simple OLS regression of the form

$$Y = \alpha + \beta D + X'\lambda + \epsilon, \tag{1}$$

where ϵ is a random error term and β corresponds to the causal effect of parents' education on a child's education.

Conditional exogeneity in this context is unlikely to hold for various reasons, which would lead to biased estimates from OLS regressions. For instance, education of parents might be correlated with their unobserved ability, which in turn affects the child's educational achievement. Another threat would arise if parents grow up in regions where the distribution of human capital is favorable. This would affect not only their own education but also their child's education, particularly if the parents do not move away over time.

While some of these threats can be addressed by controlling for more and more confounding variables in X , we cannot be sure that a simple OLS regression will lead to unbiased estimates. Therefore, we follow an instrumental variable strategy allowing for heterogeneous treatment effects, which will be described in the next subsection.

3.2 The Marginal Treatment Effect

The framework we are using to study the relationship between parents' education and their child's education is the Marginal Treatment Effect (MTE) introduced by Björklund and Moffitt (1987) and further enhanced by Heckman and Vytlacil (1999, 2001, 2005,

2007).

A parent chooses either a non-academic school track ($D = 0$) or an academic school track ($D = 1$). The schooling of her child many years later corresponds then to Y_0 if the parent chose a non-academic track school and to Y_1 if she chose an academic track school. The potential outcomes $Y_j, j = \{0, 1\}$ can be formulated as the sum of observable characteristics $X\beta_j$ and unobservable characteristics U_j :

$$Y_j = X\beta_j + U_j, \quad j = \{0, 1\}, \quad \mathbb{E}[U_j | X] = 0. \quad (2)$$

Because every individual can only be observed in one treatment state at the same time, the observed schooling outcome of the child Y can be described within switching regression model of Quandt (1972) and replacing Y_1 and Y_0 with Equation (2):

$$\begin{aligned} Y &= (1 - D)Y_0 + DY_1 \\ &= Y_0 + D(Y_1 - Y_0) \\ &= X\beta_0 + D[X(\beta_1 - \beta_0) + U_1 - U_0] + U_0, \end{aligned} \quad (3)$$

Heterogeneity in the intergenerational returns to education in Equation (3) arise from two sources. First, observed characteristics can lead to differential returns depending on the school the parent enrolled in (i.e., $\beta_1 \neq \beta_0$). Second, unobserved characteristics in non-academic or academic track schools might lead to better or worse outcomes for the next generation (i.e., $U_1 \neq U_0$).

The latent benefit from attending an academic track school D^* is given by

$$D^* = \tilde{Z}\beta_d - V, \quad (4)$$

where $\tilde{Z} \equiv (X, Z)$ implies that there is at least one instrument Z that is excluded from the outcome equation. Because V affects the latent benefit in a negative way, it is often called distaste for treatment (see, e.g., Cornelissen et al. 2016). In general, parents choose an academic track school if the benefit from the evaluation of observed characteristics exceeds the unobserved distaste for it:

$$D = \begin{cases} 0 & \text{if } \tilde{Z}\beta_d < V \\ 1 & \text{if } \tilde{Z}\beta_d \geq V. \end{cases} \quad (5)$$

When we apply the cumulative distribution function (cdf) to Equation (5), the left-hand side becomes $F_V(\tilde{Z}\beta_d) \equiv P(\tilde{Z})$, the propensity score,¹⁰ while the right-hand side becomes

¹⁰This propensity score is usually estimated from binary choice models such as Probit and Logit regressions.

$F_V(V) \equiv U_D$, the quantiles of unobserved distaste for academic track schools V . Vytlacil (2002) shows that monotonicity holds because of the additive separability between \tilde{Z} and V . Changes in the instrument Z and therefore in the propensity score $P(\tilde{Z})$ shift individuals either into an academic track school or a non-academic track school but never both at the same time.

The MTE is the average gain of academic track school enrollment for parents who are indifferent between non-academic and academic track schools at different quantiles of the distribution of unobserved characteristics (U_D) for given values of observed characteristics ($X = x$):

$$\begin{aligned} \text{MTE}(X, U_D) &= \mathbb{E}[Y_1 - Y_0 \mid X, U_D] \\ &= X(\beta_1 - \beta_0) + \mathbb{E}[U_1 - U_0 \mid U_D]. \end{aligned} \tag{6}$$

While heterogeneity in observed characteristics through different valuations in the untreated and treated state, respectively, affect the intercept of the MTE, heterogeneity in unobserved characteristics affect the slope of the MTE curve, and therefore the selection behavior of individuals. Because individuals react to changes in the propensity score at different values of U_D , the MTE curve can be obtained from the first derivative of $\mathbb{E}[Y \mid X, P(\tilde{Z})]$ with respect to $P(\tilde{Z})$

$$\begin{aligned} \text{MTE}(X, U_D) &= \frac{\partial \mathbb{E}[Y \mid X, p]}{\partial p} \\ &= \frac{\partial [X\beta_0 + X(\beta_1 - \beta_0)p + \Pi(p)]}{\partial p} \\ &= \underbrace{X(\beta_1 - \beta_0)}_{\text{observed}} + \underbrace{\frac{\partial \Pi(p)}{\partial p}}_{\text{unobserved}} \end{aligned} \tag{7}$$

where p is the estimated propensity score and $\Pi(p)$ is a flexible function of it.

Our main specification assumes that $\Pi(p) = \sum_{k=1}^K \alpha_k p^k$ is a polynomial of p of degree $K = 2$.¹¹ Although this strategy assumes a linear MTE curve and is therefore quite restrictive, we show that this shape is a good approximation of the real relationship by testing sensitivity of our main specification with respect to more flexible shapes of the MTE curve. In particular, we allow $\Pi(p)$ to be polynomials of degree 3 and 4, as well as being nonparametrically approximated, using the semiparametric approach described by Carneiro et al. (2011).

One advantage of the MTE as unifying parameter in the program evaluation literature

¹¹This functional form is assumed by empirical applications of inter alia Cornelissen et al. (2018) and Felfe and Lalive (2018).

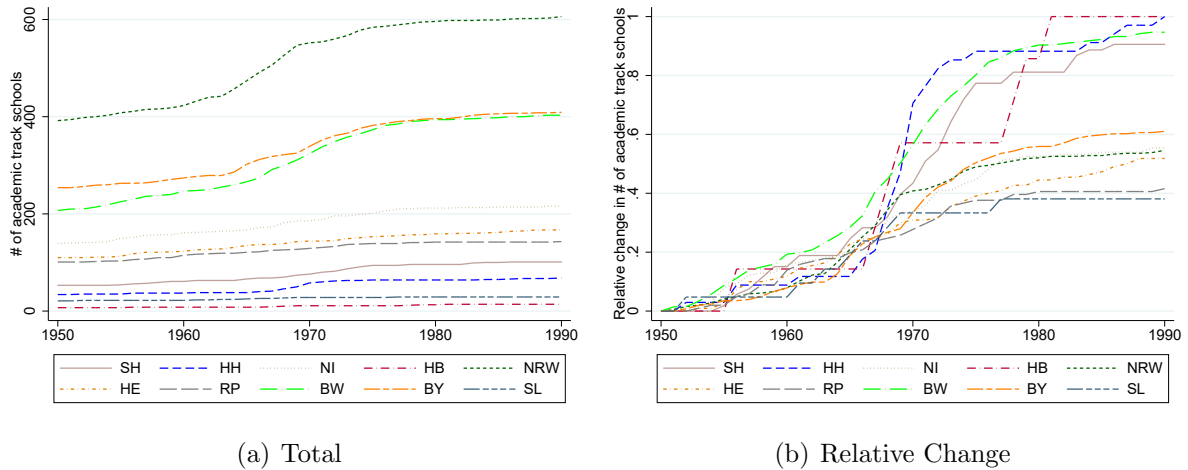


Figure 1: Distribution of Academic Track Schools Over Time

Source: Own presentation based on self-collected data on academic track schools. *Notes:* Figure (a) shows the development of the number of academic track schools for different federal states. Figure (b) shows the change in the number of academic track schools relative to 1950 for different federal states.

(Heckman et al. 2006) is that we can construct average treatment parameters such as the ATT, ATE, and ATU as weighted averages of the MTE at each evaluation point $U_D = u_D$. The corresponding weights are described in detail in the Appendix A. This allows us to draw more general conclusions about clearly defined subpopulations, which is in general not possible using linear IV methods.

3.3 Instrument Validity

Our identification strategy exploits spatial and temporal variation in supply-side factors, as is often done in economic research (e.g., Duflo 2001). In our analysis, changes in the supply of academic track schools serves as instrumental variation affecting the school track choice of the cohorts directly targeted by this change. This so-called German Educational Expansion between 1960 and mid-1970s was a period in which local authorities tried to expand supply of and access to institutions offering tertiary education (see Section 2.2 for more details). Since enrollment in tertiary education requires graduation from an academic track school, the supply of and access to these schools was also improved.

Although all German states were equally affected by the underlying reasons for the Educational Expansion, the timing and intensity differed strongly between regions. While Figure 1 depicts the supply of academic track schools in German states over time, a more precise picture of the spatial development is given in Figure B1 in the appendix. States like Hamburg, Hesse, Baden-Württemberg, or Schleswig-Holstein almost doubled the number of academic track schools from 1950. In contrast, other states such as Rhineland-

Palatinate or the Saarland only slightly expanded the supply of academic track schools.

The exact variable we are using as instrument is a weighted average of the number of academic track schools in the home district and surrounding districts when the parent is 10 years old (see Equation (8) below). For the German Educational Expansion to be used as identifying variation in the instrument, two assumptions must be satisfied. First, the educational expansion must have affected the individual choice to enroll in an academic track school. Second, we assume that the instrument is exogenous to the educational attainment of children around 30 years after the expansion. Regarding the first assumption, the educational expansion reduced the costs of education by improving the accessibility to better education at the local level. In particular, the better supply of academic track schools reduced the average commuting distance, allowing children from more remote places to attend them.¹² In Section 5.1, we test this assumption using a test for weak instruments in the first stage, obtaining a partial F -statistic of 15.12, being well above the rule-of-thumb threshold of 10 (Staiger and Stock 1997).

The second assumption states that the increase in academic track schools across districts is quasi-random after including a set of control variables. In particular, we assume that Z does not affect the unobservables in the potential outcome and selection equations after controlling for X , that is, $Z \perp\!\!\!\perp (U_0, U_1, V) \mid X$. The main threat to this assumption is that districts might have systematically differed in the implementation of the educational expansion because of different endowments of schools, firms, abilities, and demand and supply for high-skilled individuals, which in turn affect the schooling of the next generation. We account for differing initial conditions by including district fixed effects in every stage of the estimation process and, therefore, effectively use differences in the expansion speed as identifying variation.

Another threat is that the educational expansion affects the schooling of the next generation directly, because these children face the improved supply when deciding about the school track themselves, even after controlling for initial differences. Therefore, we also control for the number of academic track schools at the time of the child's school choice to directly control for the outcome of the educational expansion in their home district. The idea behind this strategy is that after controlling for the outcome of the educational expansion on the district level, the expansion (speed) itself should only affect the children's school track choice through the outcome at the time of their school track choice.

Furthermore, one could argue that the expansion speed and intensity coincided with a general trend of a better-educated population in a district. Kamhöfer and Schmitz (2016)

¹²This argument goes back to Card (1995). Since then, similar instruments have been extensively used by, for instance, Currie and Moretti (2003), Carneiro et al. (2011), Nybom (2017), and Kamhöfer et al. (2019).

argue that the timing and intensity of the educational expansion between districts mostly depended on electoral cycles and political preferences (see also Hadjar and Becker 2006; Jürges et al. 2011), while Kamhöfer and Westphal (2019) provide qualitative evidence that political interests were the main driver behind the expansion, also showing that population characteristics and economic conditions were independent of the expansion. Even though we believe that local trends that potentially coincided with the expansion in a district are no major threat in our study, we evaluate robustness of our main results by allowing for district-specific linear trends. This robustness check confirms our belief that trends play a minor role, because results remain mostly stable using this alternative specification.

Two other studies used the expansion of academic track schools in Germany to instrument the enrollment in these. Jürges et al. (2011) and Kamhöfer and Schmitz (2016) used the number of academic track schools on the state level divided by the state area as identifying variation. Although both studies find a strong first stage, supporting the validity of the educational expansion as identifying variation, we allow for finer differences in the expansion on the district level.

4 Data and Descriptive Statistics

The National Educational Panel Study

This study uses data from the National Educational Panel Study (NEPS): Starting Cohort 6 Adults (NEPS Network 2021).¹³ NEPS is a longitudinal study of individuals in Germany at different age cohorts with a focus on educational decisions and their impact on the cognitive as well as the noncognitive development over time. Since 2007, Starting Cohort 6 Adults surveys individuals born between 1944 and 1986 repeatedly. It collects information about educational activities, sociodemographic background, employment situation, family constellation, interests and perspectives, as well as competencies. In addition, NEPS surveys children of individuals who participated in the Starting Cohort 6 Adults, which allows us to estimate the relationship between parental education and the education of their children.

Our identification strategy is based on the German Educational Expansion that started before 1960 and lasted until 1980. This means that only cohorts that enrolled in a secondary school in these years are affected by the reform. Since children are tracked

¹³This paper uses data from the National Educational Panel Study (NEPS, see Blossfeld and Roßbach 2019): Starting Cohort Adults, doi:10.5157/NEPS:SC6:12.1.0. From 2008 to 2013, NEPS data was collected as part of the Framework Program for the Promotion of Empirical Educational Research funded by the German Federal Ministry of Education and Research (BMBF). As of 2014, NEPS has been carried out by the Leibniz Institute for Educational Trajectories (LifBi) in cooperation with a nationwide network.

Table 1: Summary Statistics

	Control			Treatment			(7) $p > t $
	(1) Obs.	(2) Mean	(3) SD	(4) Obs.	(5) Mean	(6) SD	
Panel A: Outcome variables							
= 1 if child in academic track	4759	0.493	0.50	2909	0.839	0.37	0.000
Panel B: Instrumental variable							
Availability of academic track schools	4759	0.382	0.25	2909	0.446	0.28	0.000
Panel C: Control variables							
= 1 if child is female	4759	0.494	0.50	2909	0.486	0.50	0.520
# of siblings of parent	4759	3.169	1.79	2909	3.212	1.83	0.320
= 1 if parent is female	4759	0.585	0.49	2909	0.449	0.50	0.000
= 1 if parent with grandparents until 15	4759	0.905	0.29	2909	0.941	0.24	0.000
= 1 if grandfather in academic track	4759	0.059	0.21	2909	0.269	0.44	0.000
= 1 if grandmother in academic track	4759	0.019	0.12	2909	0.116	0.32	0.000

Source: Own calculations based on the NEPS and self-collected data on academic track schools. *Notes:* This table shows summary statistics of the data used in the empirical analysis. Presented are the number of observations, the mean, and the standard deviation for children whose parents attended a non-academic track school (control) and an academic track school (treatment), respectively. The last column shows p -values from a t -test of mean equality.

at the age of 10, we restrict our sample to children with parents born between 1947 and 1965. Further, we restrict our sample to children that are born between 1969 and 1999 and to those whose observed parent was aged 20 to 40 at the time of birth. Because the education system in the former German Democratic Republic was different, we only focus on children born to parents from West Germany. This leaves us with a final sample of 7,668 individuals whose parents entered a secondary school between 1957 and 1975. Table 1 shows summary statistics of the main variables used in the empirical analysis.

Treatment Variable The treatment we are investigating is a binary variable indicating whether a parent attended a non-academic track school or an academic track school. 62% of the parents in our sample enrolled in a non-academic track school, whereas 38% enrolled in an academic track school. These enrollment rates are higher as compared to previous studies (see Riphahn and Schieferdecker 2012), but our research design only focuses on the West-German and non-foreign population, which tend to have higher education on average.

Outcome Variable

Our outcome of interest is a binary variable taking on the value 0 if the child attended a non-academic track school and the value 1 if she attended an academic track school instead. While 49% of children from parents who attended a non-academic track school attended an academic track school, this share is significantly higher for children from parents who attended an academic track school (84%). When discussing potential channels and mechanisms in Section 6, we introduce further outcome variables on the parental level. In particular, we use the reading and math competence (Weinert et al. 2011), a binary variable indicating whether the partner also attended an academic track school, the household’s log gross income, as well as a binary variable indicating full-time employment as outcome variables. Summary statistics for the variables are given in Table C1 in the appendix.

Instrument

Our instrument is a measure of the local availability of academic track schools in one of the 325 districts of West Germany at the time of the parents’ secondary school decision (age 10). Therefore, we collected information about academic track schools by using open access data from State Statistical Offices (*Statistische Landesämter*) and the Federal Statistical Office (*Statistisches Bundesamt*).¹⁴ This data contains every academic track school in Germany in 2017 and its address. To create our dataset of the academic track school availability in each year since 1950, we researched the founding year of each school in West Germany by scraping *Wikipedia*, researching school websites, and calling schools.¹⁵ Districts in Germany are comparably small and the existence of schools in surrounding districts could also influence the school track choice. Therefore, we follow Kamhöfer et al. (2019) and construct the instrument Z_{jt} as a measure of the local availability of academic track schools in district j at time t as

$$Z_{jt} = \sum_{k=1}^{325} K(dist_{jk}) \cdot S_{kt}, \quad (8)$$

where S_{kt} is the number of academic track schools in district k at time t , $dist_{jk}$ is the Euclidian distance in kilometers between the centroids of districts j and k , and $K(\cdot)$ is a kernel function. In practice, a district that is closer to j receives a higher weight than a district that is far away. This is in line with an educational decision model in which closer schools are more likely to be chosen than farther options because of the higher costs to enroll in them.

¹⁴The data was taken from <https://jedeschule.de/>, last access: 17 August 2021.

¹⁵There might be cases in which schools were existing before 2017 but closed and are therefore not contained in the original dataset. Therefore, we also added schools that were not contained in the original dataset but that we found on Wikipedia.

In our empirical analysis, we use the Gaussian kernel with a bandwidth of 50 kilometers in our main specification. This assumes that schools in the same district j receive a weight of $K(0) = \phi(0/50) = 0.39$. Schools with a distance of 50 kilometers receive a weight of 0.24, while schools with a distance of 100 kilometers only receive a weight of 0.05. As robustness check, we test sensitivity of our main results with respect to bandwidths between 10 and 90 and using the Epanechnikov kernel instead, which assigns schools that are far away and therefore outside of an individual's choice set a weight of 0. Results remain remarkably stable throughout different specifications.

Our descriptive statistics suggest that parents who attended a non-academic track school were facing a significantly lower availability of academic track schools than those who then enrolled in them. This is first suggestive evidence of the relevance of our instrumental variable.

Control Variables

All models control for a child's gender, the number of siblings of the parent, the parent's gender, a binary variable for family structure (i.e., whether the parent was living together with both grandparents at the age of 15), and two binary variables indicating whether a grandparent attended an academic track school.¹⁶ All variables but the gender of the child and the number of siblings of the parent exhibit a significant mean difference between higher and lower educated parents, suggesting that higher educated parents are more likely to have higher educated parents themselves (grandparents), are more likely to be male, and are more likely to have experienced a stable family structure during adolescence. Our preferred specification also includes district fixed effects to allow for differential initial conditions before the educational expansion started.

5 Results

5.1 First-Stage Evidence

We use the availability of academic track schools as instrumental variable for the parents' school track choice during the period of educational expansion. Therefore, Table 2 presents results from first-stage Probit regressions of the parents' school track choice on the instrument and a set of control variables. Column (1) presents results without any controls, Column (2) adds socioeconomic controls, and Column (3) further adds district fixed effects and, therefore, corresponds to our preferred specification.

The results of the first-stage Probit regression shows that our instrument (i.e., the

¹⁶Mare (2011) advocates to include grandparental education as well in models estimating intergenerational mobility to allow for a 'multigenerational view.'

Table 2: First-Stage Probit Regression

	Average partial effects		
	(1)	(2)	(3)
Availability of academic track schools	0.214*** (0.042)	0.182*** (0.037)	0.493*** (0.125)
= 1 if child is female		-0.005 (0.010)	-0.008 (0.010)
# of siblings of parent		0.003 (0.004)	-0.001 (0.005)
= 1 if parent is female		-0.137*** (0.016)	-0.126*** (0.016)
= 1 if parent with grandparents until 15		0.118*** (0.030)	0.130*** (0.029)
= 1 if grandfather in academic track		0.350*** (0.022)	0.327*** (0.024)
= 1 if grandmother in academic track		0.276*** (0.036)	0.237*** (0.037)
District fixed effects			✓
χ^2 -stat	24.753	23.020	15.5
p -value	0.000	0.000	0.000
Observations	7668	7668	7668

Source: Own calculations based on the NEPS and self-collected data on academic track schools. *Notes:* This table presents average partial effects from first-stage Probit regressions of the parents' school track choice on the availability of academic track schools and a set of control variables. χ^2 -tests of significance of the coefficient on the instrument are conducted and the results are presented at the end of the table. Robust standard errors clustered at the district level are given in parentheses. *, **, and *** denote significance at the 10%-, 5%-, and 1%-level, respectively.

availability of academic track schools) has a strong positive effect on the likelihood of enrolling in such an academic track school in all three models. In particular, one additional academic track school in the home district increases our instrument by 0.39 (see Section 4), which in turn increases the probability of enrolling in an academic track school by 19 percentage points on average. This effect is statistically significant at any common significance level and if we run a linear probability model instead of a Probit model and test for weak instruments, we obtain a partial F -statistic of 15.12 in our preferred specification (Column (3)), which is well above the rule-of-thumb threshold of 10 (Staiger and Stock 1997). This substantial positive effect suggests that the educational expansion indeed was successful in drawing more and more individuals into academic track schools.

In line with traditional gender roles at the time at which our school track choice took place, women were significantly less likely to enroll in an academic track school.

If the parent under consideration was living with both grandparents until the age of 15 and therefore experienced a stable home environment, she was more likely to enroll in an academic track school. Although not being at the core of the analysis, we also find a strong intergenerational correlation between grandparental and parental education. If the grandfather attended an academic track school, the parent was significantly more likely to enroll in such a school. A similar but slightly weaker effect can be observed for the grandmother’s education. These effects remain unchanged whether we control for district fixed effects or not.

5.2 OLS and IV

Although we are primarily interested in analysing the intergenerational persistence of education for the marginal individual, we start with presenting results of ordinary least squares (OLS) and 2SLS regressions as a benchmark. The results are presented in Table 3.

Results from an OLS regression suggest a strong positive correlation between the parent’s and the child’s education. If the parent attended an academic track school, the child is 31 percentage points more likely to enroll in such a school. This strong correlation suggests low educational mobility (i.e., a child’s education is highly influenced by her parent’s education). It is, however, likely that even after controlling for socioeconomic background variables this coefficient does not correspond to the causal effect of a parent’s education on her child’s education, because other unobserved characteristics (e.g., ability) might positively affect the parent’s as well as the child’s school track choice. Therefore, we apply the IV strategy outlined before to pin down the long-term causal effect of attending an academic track school on the school track choice of the next generation.

Columns (2) to (4) estimate these 2SLS regressions, instrumenting the parent’s school track choice with our measure of the local availability of academic track schools. Column (2) presents results without any controls, Column (3) adds socioeconomic controls, and Column (4) further adds district fixed effects and, therefore, corresponds to our preferred specification. Our findings suggest a strong positive effect of the parent’s academic track school enrollment on the child’s likelihood to enroll in a such school as well. This effect is slightly larger than the OLS estimate, but not statistically different from it, suggesting only a small bias of OLS.

In contrast to the first-stage regression, around 30 years later, girls were more likely to enroll into an academic track school by 8 percentage points. Furthermore, a stable home environment of the parent has long-lasting positive effects on the educational prospects of the parent (13 percentage points, see Table 2) and the child (10 percentage points, see Table 3). Most interestingly, the direct effect of the grandparent’s education seems to

Table 3: Second-Stage OLS and IV Regression

	OLS		2SLS	
	(1)	(2)	(3)	(4)
= 1 if parent in academic track	0.306*** (0.013)	0.485*** (0.030)	0.512*** (0.052)	0.385*** (0.138)
= 1 if child is female	0.082*** (0.012)		0.083*** (0.012)	0.082*** (0.012)
# of siblings of parent	-0.008** (0.004)		-0.009** (0.004)	-0.008** (0.004)
= 1 if parent is female	0.024* (0.013)		0.053*** (0.016)	0.034 (0.022)
= 1 if parent with grandparents until 15	0.108*** (0.023)		0.091*** (0.024)	0.099*** (0.029)
= 1 if grandfather in academic track	0.071*** (0.018)		0.004 (0.025)	0.043 (0.051)
= 1 if grandmother in academic track	0.022 (0.028)		-0.022 (0.029)	0.005 (0.040)
Constant	-0.013 (0.052)	0.440*** (0.016)	0.306*** (0.029)	0.368*** (0.050)
District fixed effects	✓			✓
Observations	7668	7668	7668	7668

Source: Own calculations based on the NEPS and self-collected data on academic track schools.
Notes: This table presents results from OLS and 2SLS regressions of the child's school track choice on the parents' school track choice and a set of control variables, while instrumenting the parents' school track choice with the availability of academic track schools. 2SLS regressions are performed using the propensity as the instrument. Robust standard errors clustered at the district level are given in parentheses. *, **, and *** denote significance at the 10%-, 5%-, and 1%-level, respectively.

disappear after two generations. Although we observed a strong effect on the likelihood that the parent enrolls in an academic track school, there is no direct effect on the child's educational choice anymore.

Our findings from OLS and 2SLS regressions are in line with the literature finding low educational mobility in Germany (Dustmann 2004; Heineck and Riphahn 2009). To interpret such effects as average treatment effects on subpopulations of interest, one implicitly assumes homogeneous effects which are unrealistic in most applications (Heckman 1997). If this assumption fails, we are left with a local average treatment effect (LATE, Imbens and Angrist 1994) that corresponds to the average effect of treatment for those who switch treatment status as a response to the instrument (i.e., compliers).¹⁷ While

¹⁷Although the LATE is often an interesting policy parameter itself, its interpretation in the case of a continuous instrument (as in our case) is unclear, because the effect is representative for compliers that react to changes between all values of the instrument (Cornelissen et al. 2016).

OLS only provides us with an estimate of the intergenerational persistence of education, the LATE in our context has a clear policy-relevant interpretation: parents who attend an academic track school because of a better local supply of schools transmit this better educational prospect to their children. This implies that policy makers can not only improve the educational level of disadvantaged individuals by providing better access to education, but also that their children many years thereafter still gain from this improved situation. This interpretation, however, only holds if all individuals (and their children) gain in the same way from this improved access to education. If disadvantaged individuals gain less than their advantaged peers, such policies fail to reduce inequalities in the long run. Therefore, to allow for heterogeneous effects, we present results for the MTE, a policy parameter that allows us to construct average treatment parameters of interest such as the ATT, ATU, and ATE in the next step.¹⁸

5.3 Heterogeneous Long-Term Returns to Education

Estimating the MTE requires to estimate a binary choice model of the treatment decision first, and to obtain the propensity score from this model. In particular, we use our first-stage selection model from Table 2 and further interact the instrument with all parental control variables to obtain this propensity score. It is then used to recover heterogeneous responses to the treatment based on unobserved characteristics (see Equation (7)). Because the MTE can only be identified over the common support of the propensity score for treated and untreated children, we present the respective distributions of the propensity score in Figure 2.¹⁹

The distribution of the propensity score for children of parents who attended a non-academic track school is strongly skewed to the right, with most observations having a propensity score between 0 and 0.5. In contrast, the distribution of the propensity score for children of parents who attended an academic track school is more uniformly distributed. In particular, there are sufficient observations between propensity score values of 0.2 and 1. The common support (or overlap) is remarkable in our data. While previous studies sometimes lack common support at the extremes of the propensity score, we observe children in the untreated as well as the treated state for propensity score values of 0.02 to 0.98. This allows us to identify the MTE for quantiles of the unobserved resistance for almost the full unit interval and, therefore, average treatment parameters recovered from

¹⁸See Heckman et al. (2006) for an excellent description of what standard IV can identify under given assumptions and how it relates to the MTE as a unifying parameter.

¹⁹In general, when imposing a parametric assumption on the unobserved heterogeneity, one can extrapolate the MTE curve outside the common support interval. However, we believe that this strategy only confuses about which individuals we are able to draw conclusions about and therefore restrict the MTE curve and calculation of average treatment parameters to the common support.

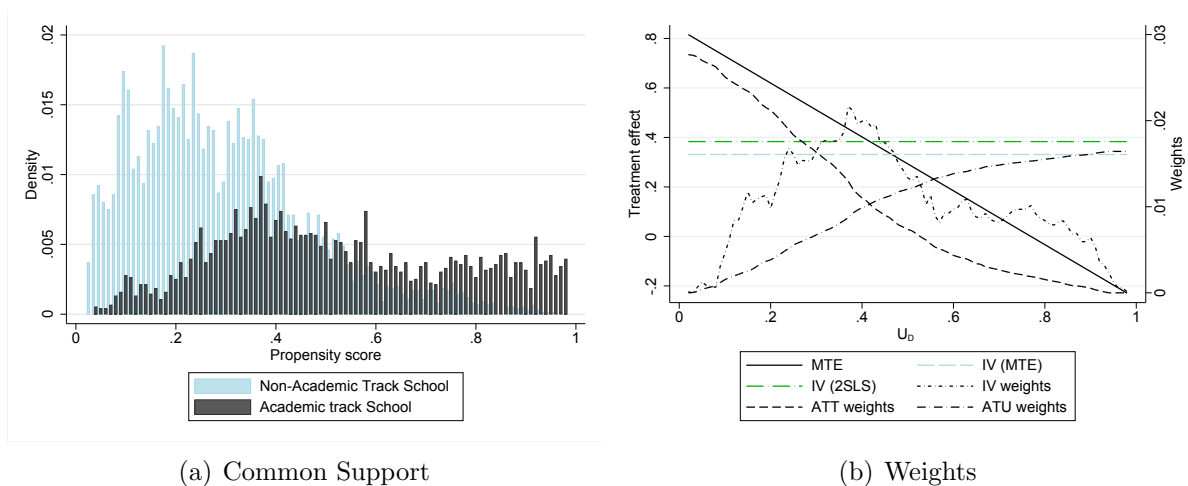


Figure 2: Common Support and MTE Weights

Source: Own presentation based on the NEPS and self-collected data on academic track schools. *Notes:* Figure (a) shows the distribution of the propensity score for children who attended a non-academic track school and an academic track school, respectively, summarized in 0.01-intervals of the propensity score. The propensity score is obtained from a Probit regression of the parents' school track choice on the availability of academic track schools and a set of control variables, while interacting the instrument with all parental control variables (except the district fixed effects). Common support ranges from 0.02 to 0.98. Figure (b) shows the weights put on the propensity score to calculate the 2SLS results in Panel B in Table 3 and the ATT and ATU in Table 4, together with the 2SLS estimates as horizontal lines. The MTE curve is also added.

the MTE curve should be very similar to their population counterparts. Nevertheless, to acknowledge the slight difference between the treatment parameters that we identify and the population parameters we are interested in, we denote our estimates as \widetilde{ATT} , \widetilde{ATE} , and \widetilde{ATU} .

Given that strong common support, we present the MTE of the parent's school track choice on the child's school track choice using the parametric specification with $K = 2$ in Figure 3.²⁰

The MTE curve is strongly decreasing with the quantiles of the unobserved resistance, suggesting treatment effect heterogeneity between 0.81 and -0.22 . These effects are statistically significant for a wide range of the quantiles of the unobserved resistance. A child whose parent is the most likely to attend an academic track school based on its unobserved characteristics has improved educational prospects (i.e., is more likely to enroll in an academic track school herself). In contrast, a child whose parent is the least likely to attend an academic track school based on her unobserved characteristics is unaffected by the improved education of their parent. Parents that react already at low quantiles of the unobserved resistance to changes in the propensity score are the ones with the highest

²⁰The estimation was conducted using an enhanced version of the Stata command `margin` of Brave and Walstrum (2014), provided by Cornelissen et al. (2018) and slightly adjusted by us.

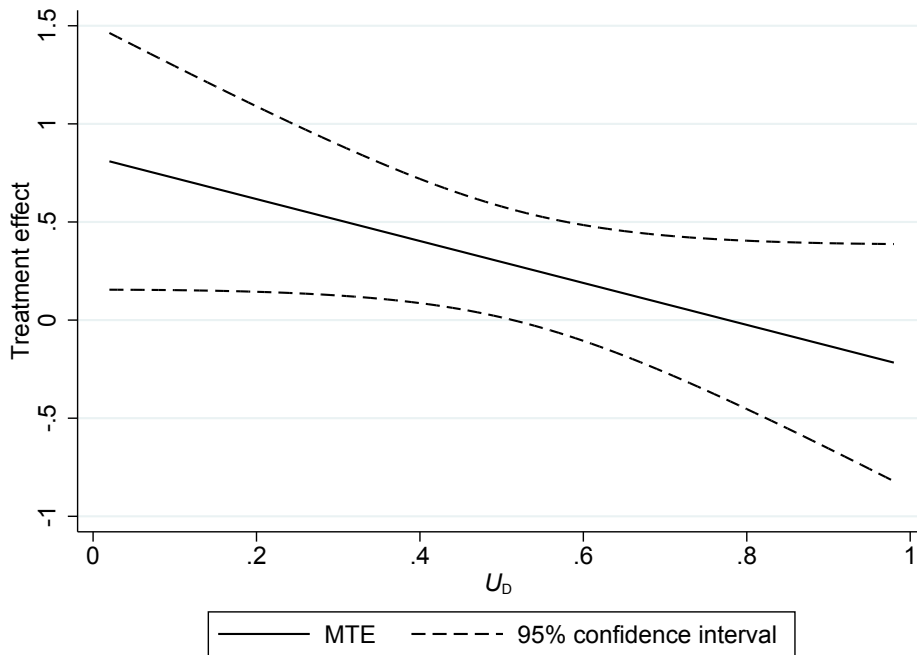


Figure 3: MTE Curve

Source: Own presentation based on the NEPS and self-collected data on academic track schools. *Notes:* The figure shows the MTE curve from a parametric MTE specification with $K = 2$ and its 95% confidence interval. Covariates are held constant at their means. The confidence interval is obtained from a district level clustered bootstrap with 299 replications. A test of no selection on gains (see, e.g., Cornelissen et al. 2018) results in a p -value of 0.071.

unobserved ability or simply have a low resistance for academic education. Children of those advantaged parents are the most likely to attend an academic track school themselves as a response to enrollment of their parent. However, even if less able or higher resistance individuals are drawn into academic track schools, their offspring will not gain from this improved education the same way as their more advantaged peers do. This finding limits the effectiveness of policies that generally draw individuals into higher academic track schools without directly targeting their abilities (e.g., the educational expansion) as one way of reducing inequalities. Such policies might be effective in providing better education opportunities for the directly affected individuals, but they do not change the intergenerational dependence of the social status. Children from disadvantaged backgrounds are unaffected by changes in the educational prospects of their parents, implying that intergenerational social status dependency is stronger than short-term educational improvements.

The pattern of selection on gains that we observe from the falling MTE curve can be tested statistically by the null hypothesis of a flat MTE curve (i.e., $H_0: \partial^2 \Pi(p) / \partial p^2 = 0$ in the case of $K = 2$). This corresponds to a test of no effect heterogeneity or accordingly no

selection on gains. The coefficient determining the slope of the MTE curve is significant with a p -value of 0.07, which suggests that we can reject the null hypothesis at the 10% significance level and, therefore, we conclude that there is strong selection on gains, statistically confirming our interpretation from before.

Although we assumed heterogeneity based on unobserved characteristics to follow a linear shape by imposing the assumption that $\Pi(p)$ is a polynomial of order 2, we evaluate robustness by allowing this heterogeneity to be more flexible than in our baseline model. In particular, we allow $\Pi(p)$ to be a polynomial of order 3 and 4, respectively, and to be estimated by a semiparametric approach (see Carneiro et al. 2011).²¹ MTE curves for these specifications are presented in Figure D2 in the appendix. For quantiles of the unobserved resistance between 0.2 and 0.8 the curves for the parametric specifications with $K = 2$, $K = 3$, and $K = 4$ are virtually identical. Moreover, the most flexible MTE curve (i.e., the semiparametric specification) follows the shape of the other three curves on a slightly higher level. This exercise strongly supports robustness of our main results with respect to the parametric choice.

In the next step, we calculate average treatment parameters such as the \widetilde{ATT} , \widetilde{ATE} , and \widetilde{ATU} as weighted averages over the MTE curve (see Appendix A). Empirical weights are presented in Figure 2. While calculation of the \widetilde{ATT} puts more weight on children of parents with a low resistance for an academic track school, calculation of the \widetilde{ATU} puts more weight on children of parents with a high resistance for an academic track school. Calculation of the \widetilde{ATE} puts equal weights on all children and, therefore, the weights are not presented. Multiplying these weights with the corresponding MTE estimates provides estimates for the average treatment parameters, presented in Table 4.²²

A child randomly chosen from the parents who attended an academic track school is on average 64 percentage points more likely to attend an academic track school herself (\widetilde{ATT}) if the parent attended it. A child chosen at random from all parents, however, has only a 32 percentage points higher likelihood of attending an academic track school if the parent attended it (\widetilde{ATE}). Finally, there is no significant treatment effect for children from parents who did not attend an academic track school (\widetilde{ATU}). This finding emphasizes again the selection pattern we already saw from Figure 3. These results are robust to using alternative parametric specifications for $\Pi(p)$. Only estimates from the semiparametric specification are slightly larger and less precise.

²¹The semiparametric curve is obtained from a partially linear regression (Robinson 1988) of the outcome on the control variables, the interaction of the control variables and the propensity score, and a locally quadratic function of the propensity score to approximate $K(P)$ using the bandwidth 0.15.

²²In addition to the ATT and ATU weights and the MTE curve, Figure 2 also presents IV weights and the corresponding IV estimate obtained from the MTE curve. This estimate is very close to the one obtained from 2SLS, which serves as further support for the correct shape of the MTE curve (Cornelissen et al. 2018).

Table 4: Average Treatment Parameter

	Parametric			Semiparametric
	(1) $K = 2$	(2) $K = 3$	(3) $K = 4$	(4)
$\widetilde{\text{ATT}}$	0.582** (0.237)	0.570* (0.320)	0.696** (0.354)	0.851** (0.429)
$\widetilde{\text{ATE}}$	0.296** (0.144)	0.284 (0.271)	0.280 (0.271)	0.535 (0.458)
$\widetilde{\text{ATU}}$	0.125 (0.167)	0.113 (0.290)	0.030 (0.288)	0.346 (0.403)

Source: Own calculations based on the NEPS and self-collected data on academic track schools. *Notes:* This table shows estimates of the ATT, ATE, and ATU from a parametric MTE specification with $K = 2$. These parameters are obtained by using the weights described in Appendix A. Standard errors are obtained from a district level clustered bootstrap with 299 replications and are given in parentheses. *, **, and *** denote significance at the 10%-, 5%-, and 1%-level, respectively.

5.4 Robustness Checks

Besides testing the robustness of our main results to more flexible specifications of the unobserved heterogeneity, we also provide robustness checks for alternative first-stage specifications and alternative instrument definitions in Tables D2 and D3 in the appendix.

Cornelissen et al. (2018) point out that misspecifications in the first stage can lead to biases in the estimated MTE curve. Therefore, we propose three alternative first-stage specifications. First, in addition to district fixed effects, we allow for district-specific linear time trends. This specification does not only control for different initial situations in a district, but also for linear trends in the enrollment in academic track schools over time. The importance of controlling for regional trends in models with schooling decisions was highlighted by Mazumder (2008) and Stephens and Yang (2014). Using this first-stage specification, the overall pattern of selection on gains is confirmed, but the estimates are more moderate and more precise. Second, our main specification interacts the instrument of the local availability of academic track schools with all parental control variables. We test robustness by using only the noninteracted instrumental variation in the first stage. This exercise only slightly reduces the average treatment parameters, but confirms the overall finding of selection on gains. Third, similar to Cornelissen et al. (2018), we estimate the first stage nonparametrically. In particular, we create 20 equally sized bins of the instrument and a linear index of the control variables as the predicted values from a linear probability model of the treatment on the control variables and separate this index into 20 equally sized bins. The propensity score is then obtained from a linear probability

model of the treatment on the full set of interactions of the bin dummies.²³ Results from this exercise suggest only a weak selection pattern anymore. Although we can still confirm the positive effect of the parents' school track choice on the child's school track choice, the effects are closer to the OLS and IV estimates.

In a second set of robustness checks we test sensitivity of our results to our particular instrument choice, which was constructed as a weighted average of the availability of academic track schools in the home district and the surrounding districts. In our main specification, we used a Gaussian kernel to weight surrounding districts with a bandwidth of 50 kilometers. To test robustness of our main results with respect to alternative kernel and bandwidth choices, we use the Epanechnikov kernel and bandwidths of 10, 30, 70, and 90, respectively, to create our instrument. Remarkably, results remain virtually unchanged if we use an alternative weighting scheme or weight surrounding districts more (high bandwidth) or less (low bandwidth).

In sum, these robustness checks do not only confirm the overall pattern of selection on gains based on unobserved characteristics, but also highlight the remarkable stability of our point estimates.

6 Heterogeneous Returns to Tracking – Implications for Educational Mobility

Selection on gains suggests that children from parents who are the most likely to attend an academic track school are more likely to attend it themselves. By separating the heterogeneity in unobserved characteristics (i.e., $E(U_1 - U_0 | U_D = u_d)$) into the unobserved parts of the outcomes when untreated ($E(U_0 | U_D = u_d)$) and when treated ($E(U_1 | U_D = u_d)$), respectively, following the approach suggested by Brinch et al. (2017), we can learn about the outcomes treated and untreated individuals would expect in the counterfactual state. This allows us to shed further light onto the mechanisms and reasons behind this selection behavior. These separate curves are presented for U_0 and U_1 in Figure 4.

The curve for the unobserved component in the untreated state U_0 is increasing while the curve for the unobserved component in the treated state U_1 is flat. This suggests that children from low-resistance parents (low U_D) who attend a non-academic track school (untreated) have a lower likelihood of enrolling in an academic track school. In contrast, children from high-resistance parents (high U_D) who attend a non-academic track school (untreated) have a higher likelihood of enrolling in an academic track school. The likelihood of enrolling in an academic track school for children of parents who attend an

²³The correlation between this nonparametric propensity score and the baseline propensity score is 0.86.

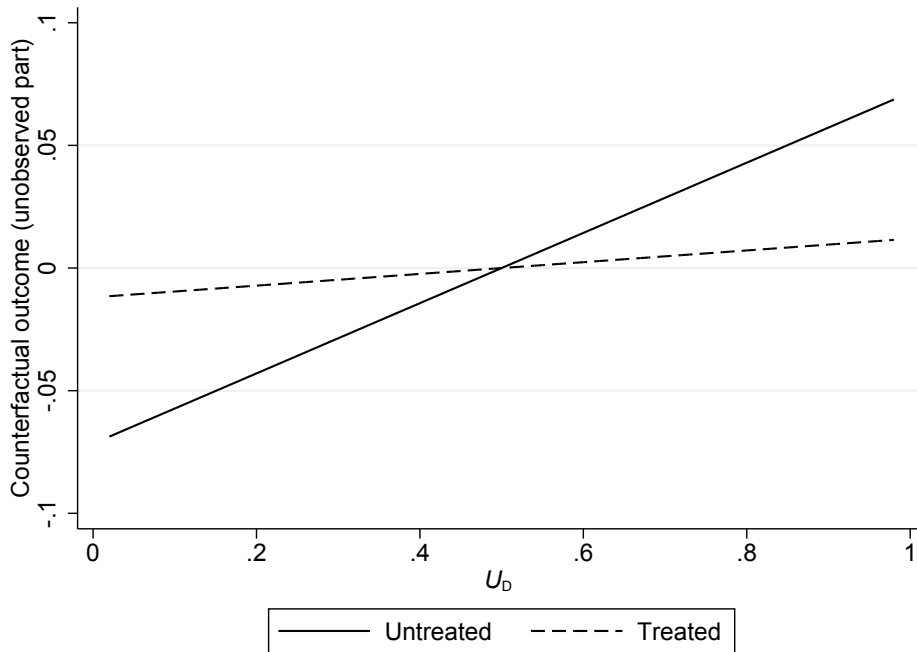


Figure 4: Counterfactual Outcomes and Unobserved Resistance

Source: Own presentation based on the NEPS and self-collected data on academic track schools. *Notes:* The figure shows the unobserved part of the probability of enrolling in an academic track school for treated and untreated children, based on the approach suggested by Brinch et al. (2017).

academic track school is the same for all. In other words, while the expected long-term returns to attending a non-academic track school are heterogeneous (i.e., only children from disadvantaged backgrounds gain from it), the expected long-term returns to attending an academic track school are the same.

All individuals gain in the same way from attending an academic track school. This suggests that there are no institutional barriers treating advantaged and disadvantaged individuals differently. Because graduates from academic track schools usually enroll in tertiary education, access to colleges and success in the labor market after graduating seems to be independent from the socioeconomic background for those individuals. In contrast, graduates from non-academic track schools usually enter the labor market directly with high-quality on-the-job training, which requires the practical skills learned in these schools. The low returns for high-ability individuals suggest that these individuals either perform poorly in this vocational training after graduating from a non-academic track school or have a hard time pursuing their expected high education level through ‘second chance’ options (Biewen and Tapalaga 2017).²⁴ In contrast, low-ability individ-

²⁴In Germany, after graduating from a non-academic track school there is still the possibility to obtain the academic track school certificate (*Abitur*) by enrolling in such a school or another school offering this degree, being able to enter tertiary education.

Table 5: Parental Outcomes

	(1) Reading competence	(2) Math competence	(3) Assortative mating	(4) HH log gross income	(5) Full-time employed
\widetilde{ATT}	2.318*** (0.817)	2.037*** (0.660)	0.303** (0.148)	1.293*** (0.338)	0.057 (0.370)
\widetilde{ATE}	2.422*** (0.555)	1.657*** (0.434)	0.133 (0.108)	0.730*** (0.212)	0.002 (0.261)
\widetilde{ATU}	2.490*** (0.572)	1.379*** (0.468)	0.028 (0.138)	0.392 (0.249)	-0.039 (0.312)

Source: Own calculations based on the NEPS and self-collected data on academic track schools. *Notes:* This table shows estimates of the ATT, ATE, and ATU from a parametric MTE specification with $K = 2$. These parameters are obtained by using the weights described in Appendix A. Column (1) uses the parents' reading competence as outcome, Column (2) uses the parents' math competence as outcome (Weinert et al. 2011), Column (3) uses a binary variable indicating whether the partner also attended an academic track school as outcome variable, Column (4) uses the household's log gross income as outcome, and Column (5) uses a binary variable indicating full-time employment as outcome variable. The measures for the reading and math competence are standardized with a mean of 0 and a standard deviation of 1. The corresponding MTE curves are given in Figure E3 in the appendix. Standard errors are obtained from a district level clustered bootstrap with 299 replications and are given in parentheses. *, **, and *** denote significance at the 10%-, 5%-, and 1%-level, respectively.

uals who gain the most from attending a non-academic track school are streamed in the right track, matching their rather practical instead of cognitive skills.

In the next step, we estimate MTE curves for various outcomes measured in adulthood to empirically support the explanations developed here. In particular, we want to shed light on the question why low-ability individuals cannot transmit their improved education to the next generation. Table 5 shows the average treatment effects of subpopulations of interest of attending an academic track school on cognitive abilities as well as labor market outcomes and a measure of assortative mating.

All individuals gain from attending an academic track school with respect to cognitive abilities. Effects range from 2.32 standard deviations to 2.49 standard deviations for the reading competence while they range from 1.38 standard deviations to 2.04 standard deviations for the math competence. While low-ability individuals gain more with respect to reading competence, they gain less with respect to math competence. Nevertheless, our results suggest statistically significant improvements for all subpopulations. We also estimate MTE curves for a measure of assortative mating, the household's log gross income, and a dummy indicating full-time employment. Those who attended an academic track school have on average a 30 percentage points higher likelihood to have a partner who also attended an academic track school (ATT), while the effect is virtually zero for

those who did not attend an academic track school. Similarly, individuals who attended an academic track school have on average around 129% higher household income because of this education (ATT), while there is no significant effect for individuals who did not attend an academic track school (ATU). Although we cannot find any (heterogeneous) effect on full-time employment, it seems that enrollment in an academic track school only translates into positive labor market returns for advantaged individuals when attending an academic track school.

Because all individuals gain in a similar way with respect to cognitive abilities, it is unlikely that this improvement affects the schooling of the next generation. In contrast, the falling MTE curve for household income and the zero effect on the likelihood of full-time employment suggests that disadvantaged individuals end up working in lower-paid occupations. We saw from Figure 4 that this occupational choice is not driven by institutional frictions that would treat disadvantaged individuals differently after graduating from an academic track school, but rather that individuals select themselves into these occupations and would have chosen a higher-paying occupation if they had graduated from a non-academic track school. Although we cannot be sure about the reasons for this negative selection behavior, we suspect that the match of abilities possessed and potentially learned in a non-academic track school (i.e., practical skills) would have led to a comparative advantage in the labor market that these individuals do not have when attending an academic track school instead. It seems therefore that labor market performances instead of improved cognitive abilities play a major role in transmitting educational status to the next generation.

Finally, our results suggest that tracking based on ability has no negative impact on the long-term returns to education but rather allows individuals to make the best out of their given abilities in the labor market, which then can translate in improved education and social status of the next generation. This result challenges previous findings usually criticizing tracked school systems for their negative impact on educational equality (see Hanushek and Wößmann 2006; Biewen and Tapalaga 2017).

7 Conclusion

In this paper, we study the long-term relationship between parental and child education in an educational system where children are tracked into academic and non-academic track schools at an early age. We identify the causal effect by using the German Educational Expansion as quasi-natural variation, which improved the supply of academic track schools in Germany in the 1960s and 1970s. Our results show that, on average, children are more likely to attend an academic track school if their parents attended one. However,

estimating marginal treatment effects, we can account for heterogeneity with respect to the resistance toward education of parents. We find that there is no effect for children of high-resistance parents (i.e., low-ability individuals and those with a lower social status). Analyzing potential channels through which the improved educational status could affect the children’s educational attainment, we find that disadvantaged individuals do not perform as well as their more advantaged peers in the labor market, suggesting that they would have been better off by attending a non-academic track school instead, which matches their abilities better.

These results are in line with work by Pischke and Von Wachter (2008), Kamhöfer and Schmitz (2016), and Cygan-Rehm (2022) who identify LATEs for marginal individuals in Germany. Pischke and Von Wachter (2008) used a compulsory schooling reform in the 1960 and 1970 to analyze how one year of schooling affects an individual’s wage, finding zero returns. Kamhöfer and Schmitz (2016) analyzed the same question using exogenous variation of the Educational Expansion to identify this effect. They used a similar instrument as we do in this study, however, only relying on state-level variation. They came to the same conclusion as Pischke and Von Wachter (2008). Finally, Cygan-Rehm (2022) reanalyzed both studies and conducted a large set of robustness checks. Depending on the sample restriction and model specification, she obtained point estimates that are slightly larger than those of Pischke and Von Wachter (2008), concluding that results are sensitive to the chosen specification. In summary, all studies conclude that there is a low or even no effect of schooling on wages for marginal individuals, which is in line with our findings. In particular, our flexible analysis sheds further light onto the distribution of treatment effects, which previous studies were not able to do.

This paper highlights important implications for future research and policy makers. First, in addition to analyzing average effects, effect heterogeneity should be taken into account to better understand which individuals gain more or less from policy interventions. Second, our results indicate that policy makers should not only focus on supply-sided educational interventions (e.g., through a better access), but also to ensure that short-term improvements translate into long-term returns to education for the next generations.

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A MTE Weights

The average treatment parameters that can be obtained from the MTE curve are defined as

$$\begin{aligned}
 \text{ATT}(x) &= \text{E}(Y \mid X = x, D = 1) \\
 &= \int_0^1 \text{MTE}(x, u_D) \omega_{\text{ATT}}(x, u_D) du_D, \\
 \text{ATE}(x) &= \text{E}(Y \mid X = x) \\
 &= \int_0^1 \text{MTE}(x, u_D) \omega_{\text{ATE}}(x, u_D) du_D, \\
 \text{ATU}(x) &= \text{E}(Y \mid X = x, D = 0) \\
 &= \int_0^1 \text{MTE}(x, u_D) \omega_{\text{ATU}}(x, u_D) du_D,
 \end{aligned}$$

with the weights taken from Heckman et al. (2006) and Carneiro et al. (2011) as

$$\begin{aligned}
 \omega_{\text{ATT}}(x, u_D) &= \left[\int_{u_D}^1 f(p \mid X = x) dp \right] \frac{1}{\text{E}(P \mid X = x)}, \\
 \omega_{\text{ATE}}(x, u_D) &= 1, \\
 \omega_{\text{ATU}}(x, u_D) &= \left[\int_0^{u_D} f(p \mid X = x) dp \right] \frac{1}{\text{E}((1 - P) \mid X = x)}.
 \end{aligned}$$

B Spatial Distribution of Academic Track Schools

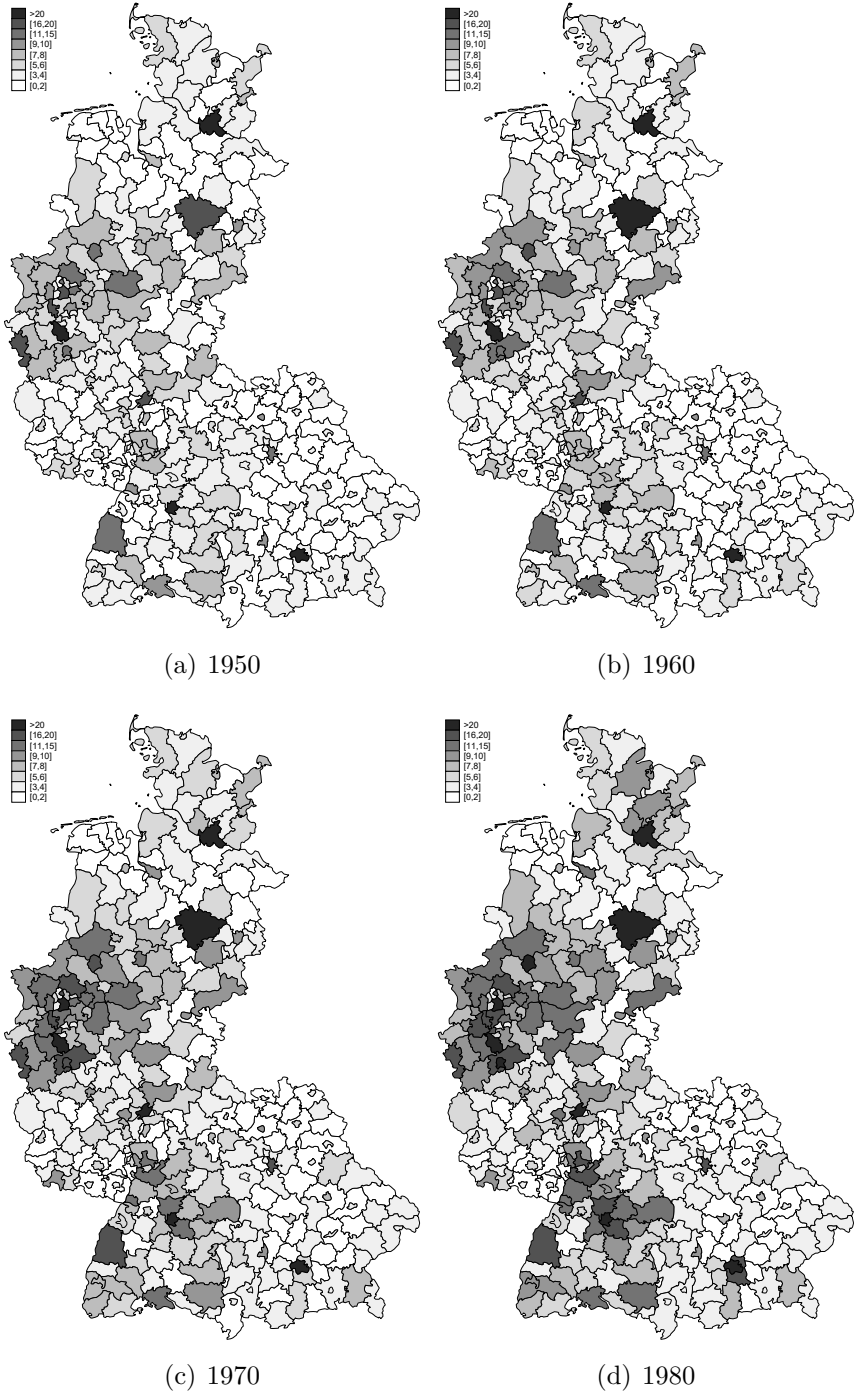


Figure B1: Distribution of Academic Track Schools Over Districts

Source: Own presentation based on self-collected data on academic track schools. *Notes:* These figures show the spatial distribution of academic track schools in the 325 districts in West Germany for the years 1950, 1960, 1970, and 1980, respectively.

C Further Summary Statistics

Table C1: Summary Statistics of Parental Outcome Variables

	Control			Treatment			(7) $p > t $
	(1) Obs.	(2) Mean	(3) SD	(4) Obs.	(5) Mean	(6) SD	
Reading competence	3136	-0.376	0.88	2102	0.587	0.85	0.000
Math competence	2656	-0.388	0.84	1893	0.560	0.93	0.000
= 1 if partner in academic track	5382	0.298	0.21	3282	0.422	0.28	0.000
Log monthly household income	4569	7.937	0.51	2751	8.341	0.48	0.000
Full-time employed	3160	0.542	0.50	2306	0.599	0.49	0.000

Source: Own calculations based on the NEPS and self-collected data on academic track schools.
Notes: This table shows summary statistics of the data used in the empirical analysis. Presented are the number of observations, the mean, and the standard deviation for children whose parents attended a non-academic track school (control) and an academic track school (treatment), respectively. The last column shows p -values from a t -test of mean equality.

D Robustness Checks

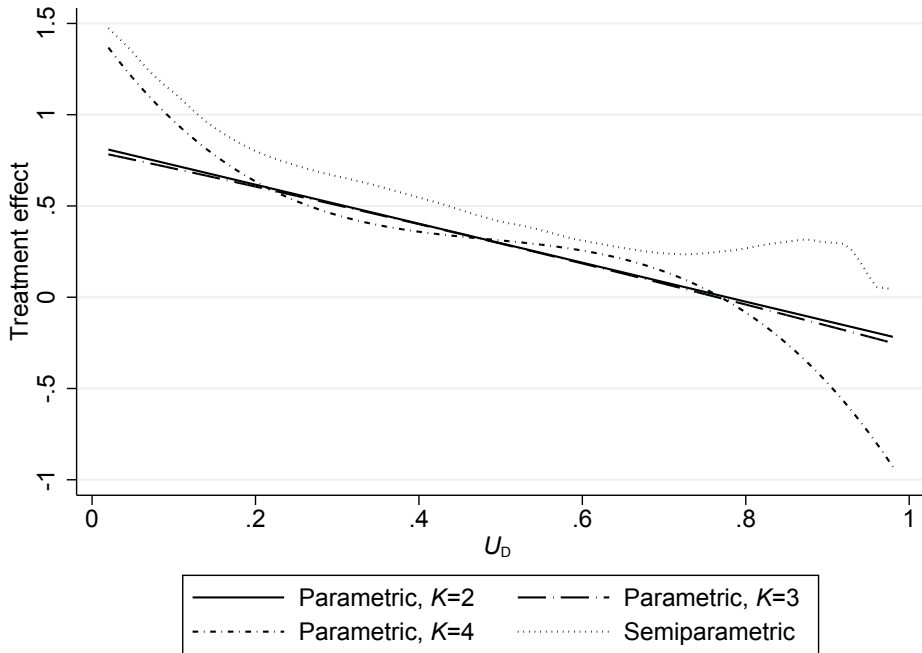


Figure D2: MTE Curves - Robustness Check of Functional Forms

Source: Own presentation based on the NEPS and self-collected data on academic track schools. *Notes:* The figure shows MTE curves from parametric MTE specifications with $K = 2$, $K = 3$, and $K = 4$, as well as from a semiparametric MTE specification (see Carneiro et al. 2011), estimating a partially linear regression (Robinson 1988) of the outcome on the control variables, the interaction of the control variables and the propensity score, and a locally quadratic function of the propensity score to approximate $K(P)$ using the bandwidth 0.15. Covariates are held constant at their means.

Table D2: Robustness Check – First-Stage Specifications

	(1) Main	(2) Time trends	(3) No inter- actions	(4) Nonparametric first stage
\widetilde{ATT}	0.584** (0.237)	0.451*** (0.130)	0.528** (0.245)	0.347*** (0.067)
\widetilde{ATE}	0.293** (0.135)	0.337*** (0.088)	0.218 (0.146)	0.296*** (0.045)
\widetilde{ATU}	0.118 (0.168)	0.267** (0.115)	0.032 (0.161)	0.264*** (0.073)

Source: Own calculations based on the NEPS and self-collected data on academic track schools. *Notes:* This table shows estimates of the ATT, ATE, and ATU from a parametric MTE specification with $K = 2$. These parameters are obtained by using the weights described in Appendix A. Column (1) presents estimates from the main specification (see Column (1) in Table 4). The model in Column (2) includes district-specific time trends in the first-stage regression. The model in Column (3) uses only the noninteracted instrumental variation in the first-stage regression. The model in Column (4) uses a nonparametric first-stage regression in which the instrument as well as an index of the control variables are separated into 20 equally sized bins and used to estimate the propensity score by interacting all bin dummies. Standard errors are obtained from a district level clustered bootstrap with 299 replications and are given in parentheses. *, **, and *** denote significance at the 10%-, 5%-, and 1%-level, respectively.

Table D3: Robustness Check – Instrument Definition

	(1) Main	(2) Epanech. kernel	(3) bw = 10	(4) bw = 30	(5) bw = 70	(6) bw = 90
\widetilde{ATT}	0.584** (0.237)	0.588** (0.241)	0.475** (0.214)	0.565** (0.233)	0.584** (0.231)	0.573*** (0.222)
\widetilde{ATE}	0.293** (0.135)	0.291** (0.147)	0.261* (0.138)	0.300** (0.141)	0.294** (0.141)	0.144 (0.135)
\widetilde{ATU}	0.118 (0.168)	0.112 (0.170)	0.135 (0.170)	0.137 (0.166)	0.119 (0.162)	0.118 (0.156)

Source: Own calculations based on the NEPS and self-collected data on academic track schools. *Notes:* This table shows estimates of the ATT, ATE, and ATU from a parametric MTE specification with $K = 2$. These parameters are obtained by using the weights described in Appendix A. Column (1) presents estimates from the main specification (see Column (1) in Table 4). The model in Column (2) uses the instrument as the weighted availability of academic track schools using the Epanechnikov kernel for weighting. The models in Column (3) to (6) use the instrument as an weighted average of the availability of academic track schools using the bandwidths 10 kilometers, 30 kilometers, 70 kilometers, and 90 kilometers, respectively. Standard errors are obtained from a district level clustered bootstrap with 299 replications and are given in parentheses. *, **, and *** denote significance at the 10%-, 5%-, and 1%-level, respectively.

E MTE Curves for Other Outcomes

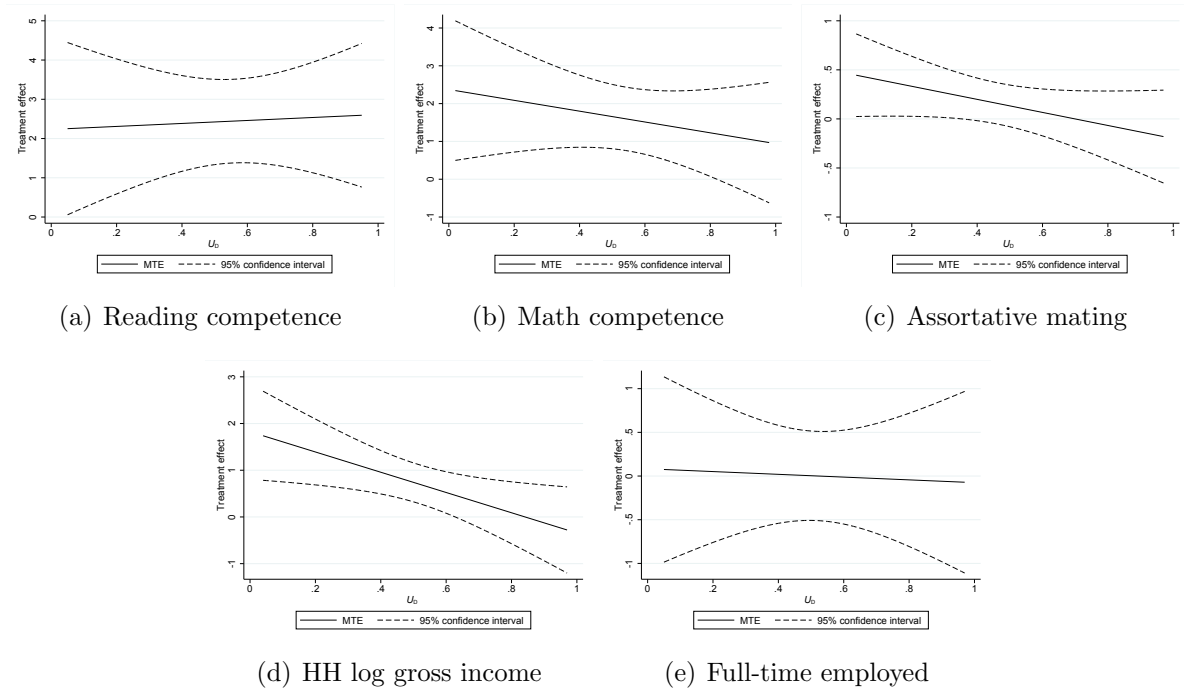


Figure E3: MTE Curves for Other Outcomes

Source: Own presentation based on the NEPS and self-collected data on academic track schools. *Notes:* The figures shows the MTE curve from a parametric MTE specification with $K = 2$ and their 95% confidence interval. Figure (a) uses the parent's reading competence as outcome, Figure (b) uses the parents' math competence as outcome (Weinert et al. 2011), Figure (c) uses a binary variable indicating whether the partner also attended an academic track school as outcome variable, Figure (d) uses the household's log gross income as outcome variable, and Figure (e) uses a binary variable indicating full-time employment as outcome variable. Covariates are held constant at their means. The confidence intervals are obtained from a district level clustered bootstrap with 299 replications.

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