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The Causal Effect of Early Tracking in German Schools on the Intergenerational Transmission of Education

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Abstract

Numerous studies have found a high intergenerational transmission of education in Germany which might be caused by the relatively early age at which the German school system tracks students into different school types. This study contributes to the scarce literature on the effect of a change in the age of tracking early in the schooling career on the intergenerational transmission of education and reveals new evidence on the heterogeneity of the effect. The identification strategy exploits a recent reform in one German state, which changed the time of tracking from after grade six to after grade four. The results of a difference-in-differences approach with data from the German Microcensus suggest that earlier tracking increases intergenerational transmission of both, low and high education. Male students and students in rural areas appear to drive the effect.

Keywords: intergenerational transmission of education, tracking, school attainment, Germany, difference-in-differences

JEL Code: I21, I24, C54

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1 Introduction

Numerous studies have found evidence for a strong correlation between the educational attainment of children and their parents in developed countries in the late 20th and early 21st century (e.g., Behrmann, 1997; Dustmann, 2004; Hertz et al., 2007). The literature names nature (i.e., genetic resemblance) and nurture (i.e., family environment through family income, family values, etc.) as the key drivers for this correlation (e.g., Björklund et al., 2007; Plug & Vijverberg, 2003; Sacerdote, 2011). While the genetic endowment (nature) cannot easily be manipulated, I argue that the design of the educational system may potentially reduce the role of family environment (nurture) by supporting children from disadvantaged families. The educational system might thereby influence the extent to which educational attainment is transmitted from one generation to the next. In this paper, I analyze one specific characteristic of the educational system: the time at which students are tracked into one out of different school tracks with different levels of difficulty.

The time of tracking in the education system may affect the magnitude of intergenerational transmission of education for two reasons. First, when the seminal tracking decision takes place at a younger age, parents and teachers have less information about the student and his or her academic potential (Dustmann et al., 2017; Malamud & Pop-Eleches, 2011; Pekkarinen et al., 2009). It seems plausible that in uncertain situations, the parents' educational degree is used as a signal for the abilities of a child. As a consequence, children might be more likely to be sent to the track the parents attended. Early tracking therefore affects the allocation of students to different tracks. Second, early tracking results in more homogeneous classes at a younger age and thereby changes the composition of the peer groups (Pekkarinen et al., 2009; Lange & von Werder, 2017). Earlier analyses have shown the relevance of higher achieving students for the own educational attainment (see e.g., Sacerdote, 2011). Although empirical evidence is missing, it seems plausible that especially students from a less advantageous background might suffer from the lack of higher achieving students because their family environment cannot compensate this lack of higher achieving peers.

This study analyzes a reform in the German state of Lower Saxony which lowered the age of tracking by two years. In most German states (e.g., Bavaria and Baden-Württemberg), students are tracked after four years of elementary school. Since the schooling reform in the state of Lower Saxony in 2004, students in Lower Saxony are also tracked after four years. Before the reform, students were tracked after six years of schooling. The total years of schooling were not affected by the change in the time of tracking. I exploit this reform using a difference-in-differences approach. With data from the German Microcensus, I can observe all relevant information for students who were tracked shortly before and after the reform, including their secondary school degree and the education degree of their parents.

Prior studies (e.g., Lange & von Werder, 2017; Malamud & Pop-Eleches, 2011; Meghir & Palme, 2005) have found a decrease in parental transmission of education when tracking is delayed. Later tracking is therefore advantageous for students from families with low education background. Evidence on the heterogeneity of the effect is scarce and inconclusive. To the best of my knowledge, this is the first study to analyze the effect of a reduction of the age of tracking in the early schooling career which affects students of all skill levels. Moreover, this paper contributes to the literature on the effects of a change in the age of tracking by analyzing a relatively clean and substantially more recent reform than most previous studies.

Establishing equal opportunities in education for children irrespective of their family background is a major goal of policy makers in the education sector. The adequate age of tracking is discussed controversially among policy makers around the world and the way tracking is implemented differs greatly between countries (Hanushek & Wössmann, 2006). This study is therefore of high policy relevance. The results are especially relevant for policy makers in countries with an early age of first tracking, because of the comparability with the reform studied in this paper. Many countries in Western Europe (e.g., Austria, Belgium, Germany, Italy, Netherlands, Switzerland), Eastern Europe (e.g., Czech Republic, Hungary, Slovak Republic, Slovenia) and in other countries around the

world (e.g., South Korea, Uruguay) track students before the age of 15 (OECD, 2013). The findings are also informative for policy makers in countries with a school system which tracks students at a later age as this study contributes to the very scarce literature on the heterogeneity of the effect of tracking for different subgroups.

The rest of this paper is organized as follows: In section two, I first reflect the existing literature and discuss the contribution of this paper before I explain the German education system and the reform in more detail. In section three, I present the method, the data set, the sample used for the analysis, potential threats to identification and descriptive statistics. I report the regression results in section four. In section five, I perform a number of robustness checks to support my results before I conclude in section six.

2 Institutional background

2.1 Previous literature

Bauer & Riphahn (2006) were among the first to study the role of timing of tracking on intergenerational transmission of education. They use cross-sectional variation within Switzerland and find that later tracking is associated with a lower level of intergenerational transmission of education. Similar results are reported by Mühlenweg (2008) who analyzes the effect of the voluntary choice to be tracked after fourth grade instead of after sixth grade on educational outcomes in the German state of Hesse. Because the parents decide individually whether the child is tracked after four or six years, her results can only be interpreted as correlations. The study suggests a negative correlation between later tracking and the impact of parents' education on a child's educational outcomes. Hanushek & Wössmann (2006) exploit the variation in tracking age between countries and provide evidence for a correlation between earlier tracking and an increase in educational inequality. Their findings are however questioned by Waldinger (2007) who indicates that minor changes in the set-up result in insignificant estimation coefficients. In a similar experimental design, but looking at education and labor market outcomes

later in life, Brunello & Checchi (2007) report that tracking is associated with increased intergenerational transmission of education.

Causal evidence on the impact of tracking is mostly based on quasi experiments. Numerous studies investigate reforms that shift tracking towards the end of the secondary schooling career (after tenth grade) or eliminate it at all - resulting in a comprehensive schooling system. Meghir & Palme (2005) study the effect of a large schooling reform in Sweden in the 1940s which prolonged comprehensive schooling from seven to nine years. They find an improvement in educational attainment after the reform. Especially children from less educated parents achieve higher schooling outcomes and also higher earnings, while earnings of children from higher educated parents decrease. The described effect can however not be completely assigned to the introduction of tracking, since the reform also increased the length of compulsory schooling and introduced a national curriculum. Pekkarinen et al. (2009) and Kerr et al. (2013) study a Finnish schooling reform from 1972-1977 which shifted tracking from age 11 to age 16 and introduced a unified curriculum. They find a decrease in the intergenerational earnings elasticity (Pekkarinen et al., 2009) and an increase in test scores at the Finnish Army Basic Skills Test for men with less educated parents (Kerr et al., 2013) after the reform. Another comparable study by Malamud & Pop-Eleches (2011) uses a regression discontinuity design to study the effect of a reform in Romania in 1973 which delayed tracking by two years. When tracking takes place after ten instead of eight years of schooling, the probability of finishing the academic track increases for students with less educated parents from poor rural areas. However, the probability to obtain a university degree does not change for this group as a consequence of the reform.

The Scandinavian studies and the study on Romania exploit policy reforms that took place between the 1940s and the 1970s. Since then, local governments have put effort in establishing equal opportunities in education for students from different family backgrounds and in diminishing the effect of parental education. There is at least some evidence that the degree of intergenerational transmission of education has changed over

time: While Heineck & Riphahn (2009) find no change in the degree of intergenerational transmission of education for birth cohorts in Germany between 1929 and 1978, a more recent study (Blanden & Macmillan, 2016) provides evidence that the situation partially improved in the UK between 1958 to 2000 as a result of the general expansion of education.

The previously cited studies analyze reforms in Scandinavia and Romania which changed from schooling system with tracking to a comprehensive school system with no tracking until 10th grade. Studies most closely related to my analysis exploit reforms that shifted the age of tracking relatively early in the schooling career - from after grade four to after grade six. Lange & von Werder (2017) study the effect of the postponement of tracking from after fourth grade to after sixth grade in the German state of Lower Saxony between 1972 and 1982 with a difference-in-differences strategy. They find an increase in intergenerational mobility of education as a consequence of later tracking. Also Piopiunik (2014) looks at a recent school reform in the German state of Bavaria which preponed tracking from after grade six to after grade four in the two lower tracks. He shows a decrease in PISA test scores for 15 year old students affected by the reform. Roller & Steinberg (2017) analyze the abolition of the intermediate school in the German state of Lower Saxony in 2004. Looking at PISA test scores, they find that early tracking improves test scores for high achieving students and deteriorates test scores of the weakest students, thereby increasing initial differences.

Only a few studies analyze effect heterogeneities. Some look at gender differences: While Lange & von Werder (2017) find that only males respond to the reform, Meghir & Palme (2005) provide evidence for a larger effect for females. Ruhose & Schwerdt (2016) study differences in the effect of timing of tracking based on migration background using cross-country variation. They report that differences in educational attainment between immigrants and natives are not caused by early tracking, but later tracking improves educational outcomes of less educated migrants with lower language proficiency.

To the best of my knowledge, this is the first study to analyzes the effect of a reduction of the age of tracking in the early schooling career which affects students of all skill levels. I contribute to the existing literature in several ways. First, my identification strategy allows me to get close to the effect of tracking by itself. Most previous studies have looked at reforms that changed more than just the time of tracking (e.g., Meghir & Palme, 2005; Pekkarinen et al., 2009) or cannot securely identify treated individuals in the data because the reform was introduced in stages (e.g., Lange & von Werder, 2017; Piopiunik, 2014). Second, the reform studied in this paper occurred in 2004, which is more recently than most reforms that were previously analyzed. In the past, policy makers have undertaken a lot of effort to increase intergenerational mobility of education. Thus, by analyzing a more recent reform, I provide evidence on whether the effect still exists nowadays. Third, the detailed analysis of subgroups provides further evidence for inconclusive literature and sheds some light on potential mechanisms of the effect. The study by Roller & Steinberg (2017) investigates the same reform as my paper, but looks at the outcome of PISA test scores, not school track graduated from. I argue that it is important to investigate the direct reform effect on the school track, because the track graduated from is more evident and has a large impact on future career opportunities.

2.2 German education system

The German education system is administered at the state level and differs between states. In the following, the general structure of the German education system is briefly explained (see Figure 1 for a graphical illustration).

In Germany, students start elementary school in the summer after they have turned six years old. Elementary school is a comprehensive school, meaning that no differentiation based on students' skill levels and abilities takes place. The duration of elementary school differs between four and six years in the different states. After elementary school, in most German states, students are tracked based on their skills and abilities into one out of three secondary school tracks with varying levels of difficulty: Lower secondary schools

(*Hauptschule*) represent the most basic track. In this track, students get basic education which will qualify them for vocational training and a blue collar job after a total of nine to ten years of education. Medium secondary schools (*Realschule*) prepare students for white collar occupations after ten years of education. Upper secondary school (*Gymnasium*) describes the most advanced track which allows graduates to take up university studies after 12 to 13 years of education. Between 2001 and 2007, many German states shortened the years of schooling at the highest track from 9 to 8 years ("G8 reform"). Besides these three tracks, comprehensive schools exist which students of all skill levels attend jointly. During the secondary schooling career, it is possible to switch between tracks, but this is done by only about 2% of the students (Dustmann, 2004). Also, it is possible to continue education on the next higher track after graduation from one of the lower tracks.

The decision which track a child attends is usually taken by the teachers, the child and the parents. Teachers usually give a recommendation for the adequate track of the child, based on the child's skills and abilities. Parents also have a bearing on the track choice, however, the extent to which they can overrule the recommendation of the teachers differs between states.

2.2.1 Intermediate school in Lower Saxony

The ambition to emphasize the development of the students' personalities in fifth and sixth grade emerged as early as 1945 in the German educational policy discussion. Students around the age of ten to twelve should be encouraged to explore their own interests and skills as a basis for their educational career and future life (Ziegenspeck, 2000).

As a consequence, in 1959 the German committee for the educational system (Deutscher Ausschuss für Erziehungs- und Bildungswesen, 1966) proposed the implementation of a second comprehensive school after elementary school and thereby a postponement of the age of tracking. In the following decades, it was repeatedly discussed whether an intermediate schooling system should be implemented in all states between primary and secondary school. In 1963, the states' ministers of education agreed that the fifth and

sixth grade should be considered an orientation period with particular attention towards the promotion, observation, and orientation for the future educational career.

To comply with this agreement, some states decided to introduce an independent intermediate school to put special emphasize on the development and orientation of a child after primary school and before tracking to secondary school. Most states however continued to track students after four years and put special emphasize on the development and orientation of a child in the first two years of every secondary school track (Ziegenspeck, 2000; Avenarius et al., 2001).

Lower Saxony was one of the few states which introduced an intermediate school and called it *"Orientierungsstufe"* (orientation phase). The *"Orientierungsstufe"* was introduced successively between 1971/72 and 1980/81 in all counties. These intermediate schools were usually independent from other school types and only in rural areas sometimes administered together with the lower and medium secondary schools. Teachers in these intermediate schools were educated for lower, medium, or upper secondary school teaching. In intermediate schools, students of all skill levels were taught together. Only in mathematics and the first foreign language in sixth grade, students were assigned to different courses based on their skill level (Avenarius et al., 2001).

In 2003, the newly elected state government in Lower Saxony abolished the intermediate school. This was a reaction to the relatively poor PISA test scores of Lower Saxony in comparison to other states with tracking at an earlier age. With the reform, policy makers hoped to increase the average education level of students in Lower Saxony. Beginning with the schooling year 2004/2005, students who finish their fourth year of elementary school are now tracked immediately and proceed to a secondary school. The birth cohort 1992/1993, which had already attended intermediate school for one year at the time of the reform, was tracked after fifth grade. The respective law in Lower Saxony also included the introduction of the G8 reform where the same birth cohort 1992/1993 was the first cohort affected by the decrease in total years of schooling at the highest track. Also, this law determined that no more comprehensive schools were opened in Lower Saxony.

3 Method and data

3.1 Method

The aim of this paper is to identify the causal effect of the shift of the time of tracking from after grade six to after grade four on the degree of intergenerational transmission of education. To identify this effect, I use a difference-in-differences strategy. The first difference results from the difference between birth cohorts affected and not-affected by the reform. The second difference follows from the difference between students in the state of Lower Saxony, which changed the time of tracking during the observation period, and students in other states, which tracked after four years for the full time of observation.

I estimate the following model:

$$\begin{aligned} lower\_sec\_degree = \beta treat_i + \gamma post_i + \delta(treat_i * post_i) \\ +\lambda(treat_i * post_i * high\_educ\_par_i) + z_i'\psi + x_i'\alpha + \epsilon_i. \end{aligned} \tag{1}$$

In the main specification, I use three binary dependent variables to represent the educational attainment: $lower\_sec\_degree$ indicates whether an individual i has left school without a secondary degree or with not more than a degree from lower secondary school. In other regressions, the dependent variables $medium\_sec\_degree$ and $upper\_sec\_degree$ specify whether the individual has obtained a medium or an upper secondary degree, respectively.

$Treat$ states whether the individual lives in the treated state and $post$ shows whether he or she was born in a cohort before or after treatment. I further include a variable $high\_educ\_par$ identifying higher educated parents. In this paper, parents are considered higher educated if the mother and or the father[1] has an upper secondary degree and less educated if neither the father nor the mother has an upper secondary degree. To identify potential difference in the effect based on parental background, the following interaction terms are included $(treat_i * post_i)$, $(treat_i * post_i * high\_educ\_par_i)$,

[1] If only one parent lives in the household, the education level of this person is decisive.

$(treat_i * high\_educ\_par_i)$ and $(post_i * high\_educ\_par_i)$ (the last two variables are included in the vector z_i'). Consequently, the parameter δ shows the reform effect for children with less educated parents. λ reflects the difference in the reform effect between both groups, while $\delta + \lambda$ shows the reform effect for children of higher educated parents. To test the significance of the reform effect for children of educated parents, an F-test on the joint significance of the coefficients δ and λ is presented for all regressions which differentiate between parental education.

In the regression, I include a vector of control variables x_i and a constant. The control variables control for differences in education outcomes induced by personal characteristics and family background. Regarding personal characteristics, I control for gender, the number of months (1-12) an individual is born after the cutoff month for school enrollment[2] and first and second generation migration background. Following the definition of the German Federal Statistical Office (Statistisches Bundesamt, 2018), this analysis defines first generation immigrants as immigrants who did not acquire the German citizenship at birth and second generation immigrants as individuals born in Germany as a child of a first generation immigrant. Family background is considered by the number of siblings, identifiers for the firstborn child in the family, identifiers for single parents and families in which both parents are working, and a set of categorical indicators presenting household income and the level of urbanization, measured by the number of people living in the county of residence.

To analyze the heterogeneity of the effect, I estimate the model separately by gender, migration background, and level of urbanization of the county. It seems plausible that male and female students react differently to a change in the age of tracking as they are in different stages of puberty at the time (Koerselman & Pekkarinen, 2017). It is expected that male students react stronger to the shift of tracking age than females as they are generally expected to need more time to mature. This might cause higher uncertainty if

[2] By controlling for the number of months born after school enrollment, I account for age differences at the time of cut-off.

the tracking decision takes place at a younger age and result in stronger reliance on the educational degree on the parents for the track choice of male students.

Furthermore, natives and migrants might react differently to a change in the age of tracking. Lemmermann & Riphahn (2018) show that for second generation immigrants the age of arrival has a significant impact on educational attainment. The authors name the early tracking age in Germany as one potential cause of the early critical age of migration in Germany. In contrast, Ruhose & Schwerdt (2016) compare students in 45 countries and find that early tracking does not affect the overall achievement gap between natives and migrants.

Lastly, I check for different effects between individuals living in rural and urban areas. I hypothesis that the reform has a larger impact on intergenerational transmission of education in rural areas as opposed to urban areas. Teachers are more likely to know the educational degree of the parents in rural areas than in urban areas because of a lower degree of anonymity.

3.2 Data and sample

For the analysis, I use the 2008 to 2014 waves of the German Microcensus. The Microcensus is a 1% cross-section survey of the German population, conducted annually, which includes data on the population structure. These data are collected during the course of the year and the information are always given for the respective quarter instead of for a specific date. Households are interviewed for four years in a row, with one fourth of the sample being replaced every year. The research data does not allow to identify individuals over time. Consequently, it is impossible to exclude repeated individuals. However, since each birth cohort enters the sample only in two survey years (see Table 1 and explanation below), individuals cannot appear more than twice in the data.

From this data, I construct a data set that includes birth cohorts 1990/1991 and 1991/1992 (pre-treatment cohorts) as well as 1993/1994 and 1994/1995 (post-treatment cohorts). A birth cohort includes all children that had to start school in the summer of the

same year.[3] The 1992/93 cohort is excluded from the sample as this group had only one year of intermediate secondary school in Lower Saxony.

The sample is constructed from individuals living in Lower Saxony, Bavaria or Baden-Württemberg at the time of observation.[4] Lower Saxony was affected by the reform while Bavaria and Baden-Württemberg did not change their time of tracking within the observation period. All three states have a three-track secondary school system and the same cohorts were affected by the G8 reform: While the pre-treatment group in all states finished upper secondary school after a total of 13 years of schooling, the post reform group finished upper secondary school after 12 years of schooling. Reducing the sample to the states that were equally affected by the G8 reform should cancel out the effect of this reform.[5] Furthermore, these three states faced a comparable trend in expenses per student ratio and the student per teacher ratio (Vogel et al., 2013, p.127). I exclude all other states from the sample, either because they had a different tracking system, they did not have a three-track school system or because the G8 reform took place in a different year. Considering these states could bias the results. In a robustness check, I provide evidence that the results are robust to the consideration of other states.

I observe all individuals in the calender year they ought to finish tenth grade or in the following year. At this time, the individuals are 14 to 16 years old and i) are still in secondary school (grade 10 to 12), ii) have dropped out or graduated from school and thereby terminated secondary education, or iii) have obtained a secondary degree and are now studying at the next higher track. In cases i) and iii) the outcome variable reflects the track currently attended, in case ii) it mirrors the highest track graduated from. I assume that the attended track at the time of observation reflects the highest secondary track the student will graduate from. The described procedure allows me to assign almost all

[3] The birth cohort 1991/1992 for example includes all children that were born after June 30th, 1991 and not later than June 30th, 1992. This cohort started school in the summer of 1998.

[4] The data does not provide information on the state of residence when children were at grade four to six. I therefore have to rely on the state of residence at the time of observation. In a robustness check, I exclude individuals who have moved within the last year.

[5] Studies of the G8 reform have found only a small effect on cognitive skills of ninth-graders (Huebener et al., 2017), no effects on graduation rates (Huebener & Marcus, 2017), lower grades at graduation and higher class repetition rates after the reform (Huebener & Marcus, 2017).

individuals to a track. One exception are students studying at a comprehensive school at the time of observation, because they cannot be assigned to one of the three tracks. By excluding these students, I lose 2.8% of the sample. Assuming that the share and the composition of students attending comprehensive schools did not change in the observation period, the exclusion of these students should not affect the results. The potential effect of the reform on the probability to attend comprehensive school is discussed in section 3.3.

I observe the outcomes around the time they finish tenth grade. At this time, some correcting track changes have been made.[6] Observing individuals around the time they finish tenth grade instead of after graduation has the advantage that individuals at this age generally still live with their parents. Roughly 98% of German adolescents between ages 15 and 19 live in their parents' household (Eurostat, 2017). Living at the parents' house is essential in order to observe the highest educational degree of the parents in the data and thereby to exclude sample selection issues caused by selective moving-out from the parents' home.[7]

At the time of observation, most students attending lower and medium secondary school have already graduated, only children in upper secondary school have not yet graduated at this age. This is a potential drawback, because it is not certain that individuals who are observed attending upper secondary school will eventually graduate from that school. However, only 3% of the students who participated in the final exam of the highest track did not pass it in 2017 (Kultusministerkonferenz, 2017) and these students have the option to repeat the exam and graduate from upper secondary school in the following year. The second drawback is that educational degrees, which are obtained later in life are not observed in the data. If an individual in lower or medium secondary school is observed in grade 10, the data does not provide information if he or she continues education on the next higher track thereafter. Furthermore, the data does not provide information on educational degrees that are obtained in adulthood. However, the share of students that

[6] The share of students changing school track was below 4% in Germany in the school year 2010/2011 (Bellenberg & Forell, 2012).

[7] For 3.4% of the sample, I do observe neither the mother's nor the father's educational degree and therefore have to exclude these individuals from the sample.

attend school after the age of 20 is low in Germany (roughly 0.25% in 2017 (Statistisches Bundesamt, 2017)). Nevertheless, the results can be interpreted as an effect on the first educational degree in adolescence.

Table 1 illustrates the composition of the basic sample. For each birth cohort and survey year, the table presents the grade the individual currently attends.[8] Students are included in the sample if they are observed in the survey year in which they ought to finish 10th grade or in the following year (grey-shaded) . For instance, an individual born in the birth cohort 1991/1992 is included in the sample if observed in the data in year 2008 or 2009. Since the school year in Germany starts in the middle of the calender year, individuals are observed in two grades in one calender year. Below the solid line, the table also presents the tracking age for each birth cohort and state.

The main sample includes 17,048 individuals. 24% of the individuals in the sample hold a lower secondary degree, 34% hold a medium secondary degree and 42% obtained a higher secondary degree (see Table A1). I present the mean values of all covariates in Table A1 in the appendix.

Figure 3 shows the strong correlation between parents' education and their offspring: Children with less educated parents are most likely to leave school with a lower secondary degree (44%), while only 17% of these children obtain a higher secondary degree. For children of medium educated parents, the divergence between tracks is smaller. While roughly 28% of these children obtain a lower secondary degree, 32% obtain a higher secondary degree. Again, the most frequently obtained educational degree is the degree obtained by the parents. 40% of the children with medium educated parents obtain a medium secondary degree. For children with higher educated parents, the correlation is largest: more than two third (70%) of these individuals obtain a higher secondary school degree as well. Only 8% of all individuals with higher educated parents obtain a lower secondary degree. In difference-in-differences estimations, I test whether this relationship between parental and offspring education can be interpreted causally.

[8] Students in lower and medium secondary track have already graduated in grade 11 to 13.

3.3 Threats to identification

The main assumption in a difference-in-differences setting is that the outcomes of both, treatment and control group would have developed in parallel in the absence of the reform. In Figures 2 (a), (c), and (e), I provide evidence for a parallel trend between treatment and control group with data from the last two cohorts (90/91 and 91/92) that were not affected by the reform. In the figures, the first vertical line shows the last pre-treatment cohort, the second vertical line shows the first post-treatment cohort.

Since I do not have data access to observations of earlier birth cohorts at the same age, I cannot show the trend for a longer period of time. With aggregated data from the Federal Statistical Office (FDZ der Statistischen Ämter des Bundes und der Länder, 2017), I provide further evidence that there is a parallel trend between treatment- and control states for all three outcomes before the reform (see Figures 2 (b), (d), and (f)). The figures show the attended track for ninth graders.[9]

In Table 2, I show a placebo test, where only the pre-treatment period is included in the sample. Birth cohort 1990/1991 is used as a pre-reform placebo group and birth cohort 1991/1992 is used as the post-reform placebo group. The placebo-reform effect is insignificant and thereby depicts further evidence for the validity of the common trends assumption.

Additionally, since I am performing a difference-in-differences approach with cross-sectional data, following Angrist & Pischke (2009), I need to show that the treatment and the control group do not differ in their intertemporal variation on the observed variables. Table 3 presents the difference in means of the control variables between cohorts affected by the reform and those that are not affected for treatment and control states. The last column shows the difference of the two differences. The coefficients of the control variables do not differ significantly in their intertemporal variation. The only exceptions are the results for first and second generation immigrants. Due to the small number of migrants

[9] The aggregated data solely provide information on the track currently attended, but not on the highest degree obtained after graduation. I cannot observe the outcome at a later age, because some students graduate after nine years from lower secondary school and fall out of the data thereafter.

in the sample, I do not expect that the intertemporal variation in the variables describing the migration background affects my results substantially.

Another potential threat might be that the rate of comprehensive school enrollment changed after the reform. A selective group of parents could have decided to send their children to a comprehensive school in order to replace the intermediate school. There is evidence that this did not happen. First, the enrollment rate for children in comprehensive school did not change in the treated state after the reform (Statistisches Bundesamt, 2016). In 2002, roughly 4% of students went to an integrated comprehensive school after tracking, while in 2005, about 5% started this type of school. Second, one component of the reform was that no more comprehensive schools should be opened in Lower Saxony. This also speaks against a large amount of students sorting into comprehensive schools to avoid early tracking.

Through the reform, elementary school teachers are given the new responsibility to recommend an adequate track for every student. In the first years after the reform, teachers face a higher uncertainty in the decision for a track, which decreases with experience in the years after the reform. Higher uncertainty in the first years after the reform might potentially result in stronger reliance on the parents' educational degree when taking the decision for the adequate track. In a robustness test, I provide evidence that the inclusion of an additional post-treatment cohort does not affect the results. Later survey waves which are essential for observing later cohorts at the relevant age are not available at the time of this study. Consequently, I cannot rule out the possibility that I pick up only a short term effect which disappears with growing experience of the teachers.

Lastly, sample selection might affect the external validity of the results. First, to observe the educational degree of the students' parents in the data, students need to live at their parents' household at the time of observation. I provide evidence that those students who are dropped from the basic sample due to missing information on their parents' educational degree do not drive the results. Second, the results could also be biased if families sent their children to schools in the neighboring state because of the time of

tracking in their state.[10] After the reform, it seems unlikely that parents influence the state where their child is educated to change the time of tracking, because at this time, most states tracked students after grade four. Only two federal states (Berlin and Brandenburg) tracked after grade six. It is unlikely that a high share of students from Lower Saxony attended school in Berlin or Brandenburg after the reform, because of long commuting distances from most locations in Lower Saxony. Lower Saxony borders only for very few kilometers with Brandenburg and does not share a state border with Berlin. Before the reform, it is conceivable that parents in the treated states with tracking after grade six who were worried about the late tracking age sent their children to schools in neighboring states. The reform effect might therefore be underestimated if some of the students in the pre-treatment group were tracked after grade four already. Families living in Bavaria and Baden-Württemberg (control states) would most likely not have sent their children to a state with tracking after grade six (Lower Saxony, Bremen, Berlin or Brandenburg), because of the geographical distance to these states. Unfortunately, the data does not allow to control for distance to the state border.

It is also possible that families migrate to a different state in order to influence the time of tracking for their children. This seems unlikely, because the education systems of the states differ by many other factors beyond the time of tracking. However, as the data do not inform about movement between different states, I cannot fully rule out this potential source of bias. In a robustness test, I exclude individuals who have moved between states within the last year before the reform.

4 Results

4.1 Overall effect

Table 4 presents the overall effect of a shift of tracking from after grade six to after grade four. Earlier tracking increases the probability to obtain not more than a lower secondary

[10] In the data set, students are assigned to a state based on the state they live in.

degree by 3.6 percentage points. The probability to graduate from medium secondary school decreases by 7.8 percentage points and the likelihood to obtain a higher secondary degree increases by 4.2 percentage points after the change of the tracking age. Earlier tracking therefore results in a higher probability to graduate from lower and upper secondary school. The reason for this development becomes evident when analyzing the effect separately by parental education.

Table 5 displays the results separately by parental education and provides evidence for differences in the reform effect conditional on the education degree of the parents. For children of less educated parents, the probability to obtain at best a lower secondary degree increases by 4.4 percentage points after the reform, while the likelihood to graduate from medium secondary school degreeses by 7.3 percentage points, significant at the 1% level. In contrast, children of higher educated parents face an increase in the probability to obtain a higher secondary degree by 7.6 percentage points when tracking takes place after elementary school, significant at the 1 percent level and no change in the probability to obtain at most a lower secondary degree. These results support the hypothesis that early tracking encourages intergenerational transmission of education. After the reform, both, children of less educated parents and children of higher educated parents are less likely to obtain a medium secondary degree. For children of less educated parents, the reform results in an increase of the probability to obtain a lower secondary degree, while children of higher educated parents have a higher probability to graduate from upper secondary school. This phenomena explains the overall reform effects described in Table 4.

Next, I split the group of less educated parents into in low educated and medium educated parents to shed more light on the group of students most affected by the reform. In order to avoid complex interaction terms, the regressions are run separately for a sample of students where i) both parents have not more than a lower secondary degree (low), ii) one or both parents have more than a lower secondary degree, but none has an upper secondary degree (medium) and iii) at least one has an upper secondary degree and none

has a lower secondary degree (high)[11]. Table 6 shows that children of medium educated parents are less likely to obtain a medium secondary degree and more likely to obtain a lower secondary degree after the reform. The coefficient showing the reform effect on the probability to obtain a lower secondary degree for children with parents that have at best a lower secondary degree is large, but insignificant, potentially mediated by the small sample size for this subgroup. As observed before, the probability to graduate from upper secondary school only changes significantly for children of higher educated parents after the reform, again pointing towards a stronger path dependence between the educational degree of the parents and the child when tracking takes place at an earlier age.

4.2 Heterogeneity of the effect

In Table 7, I estimate the effect of earlier tracking separately for different subgroups. I split the sample successively by gender, migration background and level of urbanization in the county of residence.

A separate estimation by gender (Panel A) reveals that the time of tracking affects the education outcomes of males more than the education outcomes of females. Daughters of less educated parents and higher educated parents face a lower probability to graduate from medium secondary school of roughly 6 percentage points after the reform. For daughters of less educated parents, the reform effect on the two other tracks is insignificant. For daughters of higher educated parents, I observe a slight increase in the probability to graduate from lower and from upper secondary school. Both effects are, however, only significant at the 10 percent level. In contrast, sons of less educated parents are confronted with lower probability to graduate from medium secondary school (8.2 percentage points) and a 7 percentage points higher probability to obtain not more than a lower secondary school degree when the tracking decision takes place two years earlier. In line with the previous results, the probability to graduate from upper secondary school

[11] Definition of parents with an upper secondary degree remains unchanged due to the smaller number of observations.

does only change for sons of higher educated parents. For male students with higher educated parents, the probability to leave school with an upper secondary degree increases by 8.7 percentage points. The stronger effects for males are in line with the observation that males generally need more time to mature (Koerselman & Pekkarinen, 2017). This might cause higher uncertainty and results in stronger reliance on the educational degree of the parents for the track choice.

A separate estimation by migration background (Panel B) suggests that the effect of a change in the time of tracking on educational outcomes is comparable between natives and migrants. Individuals who migrated to Germany themselves (first generation immigrants) or were born in Germany and have a mother and/or a father who migrated to Germany (second generation immigrants) are defined as migrants in this analysis. Similar to the main results, I find an increased probability for native children of less educated parents to get a lower secondary degree of 3.4 percentage points and a 7.8 percentage points higher chance of graduating from upper secondary school for natives with higher educated parents. For migrants with less educated parents, the results indicate that early tracking results in an increased probability to leave school with not more than a lower secondary degree. The effect is large (8.8 percentage points) but only significant at the 10 percent level and relies on a small sample of migrants. The combination of lower educated parents and migration background might be understood as a signal for lower educational potential by the teacher and result in the assignment to a lower track, as shown also by Lüdemann & Schwerdt (2013). Overall, the results point in the same directions for both, natives and immigrants. This is in line with the finding of Ruhose & Schwerdt (2016), who find that early tracking does not affect the overall achievement gap between natives and migrants.

The extend to which the education of the parents influences the effect of the reform might be larger for children from rural than from urban areas. I define rural areas as communities (*Gemeinde*) with not more than 20.000 inhabitants. This is the definition of a small town as it is defined by the German Federal Statistical Office (Statistisches Bundesamt, 2016). Consequently, urban areas are defined as having more than 20.000

inhabitants. The hypothesis that the reform effect is larger in rural areas is based on the conjecture that teachers in rural areas are more likely to know the educational level of the student's parents because of a lower degree of anonymity. The estimation results (see Table 7, Panel C) support this hypothesis. In rural areas, earlier tracking leads to a significant increase in the probability to graduate from lower secondary school for children of less educated parents (4.5 percentage points) and a significant decrease for children of higher educated parents. Also, the probability to graduate from upper secondary school increases by 11.3 percentage points for children of higher educated parents in rural areas after the reform. The results for individuals living in urban areas are somewhat surprising. I find a positive effect on the probability to obtain an upper secondary degree for children of less educated parents, the coefficient is significant at the 5 % level. The effect on the probability to graduate from lower secondary school is also positive, but insignificant. For children of higher educated parents, I observe a decrease in the probability to obtain a medium secondary school, which is comparable to the main results. Potentially, the results for graduates living in urban areas are not as clear as for graduates in rural areas because of the higher degree of anonymity in urban areas.

5 Robustness

In order to provide evidence for the robustness of my results, I repeat the estimation with different samples (Table 8). First, I append one more post-treatment birth cohort to the basic sample (Panel A). Including an additional post-treatment cohort could be relevant if the effect of earlier tracking diminishes over time (e.g., if teachers learn from first difficulties in giving recommendations for a certain track and behave differently after the first two years).[12] The results show that the reform effect for lower, medium and upper secondary school degrees are stable in the short run.

[12] More pre- and post-treatment birth cohorts cannot be added due to data availability.

Second, I investigate whether the results are robust to the inclusion of additional federal states (Panel B). I expand the control group by more states which track students after four years of schooling. To control for the G8 reform effect, which took place at different times in the various states, I include an identifier for those birth cohorts that graduated from upper secondary school after a total of twelve years of schooling. The results are comparable to previous findings. In contrast to the main results, but in line with the findings for the subgroup-analyses of natives and individuals living in urban areas, the results suggest a positive effect of the reform for children from less educated parents on the probability to obtain an upper secondary degree.

Table 9, Panel A provides evidence that the results are not biased by sample selection caused by students moving out from their parents' household. Although evidence shows that most students live with their parents until the age of 19 (Eurostat, 2017), I provide evidence that those 561 students that are dropped from the basic sample due to missing information on their parents educational degree do not drive the results. I therefore re-estimate the effect with a sample that includes individuals for whom I do not observe a degree of their parents, by including a missing value identifier for these observations. This does neither affect the size nor the significance of the relevant coefficients.

The data does not provide evidence on the state of residence at the time of tracking. I have to assume that the state of residence at the time of tracking is equivalent to the state of residence at the time of observation. The most I can do to rule out this potential source of bias is to exclude individuals who moved between states in the last calender year before the survey, which applies to 26 individuals. The regression results are robust to the exclusion of these individuals (Table 9, Panel B).

All regressions are presented with heteroscedasticity robust standard errors. Due to the small number of states (3), standard errors are not clustered on the state level. However, following the minimum recommendation of Cameron & Miller (2015), clustering the standard errors at the state level does not affect the results (see Table 10, Panel A).

In a final robustness check (Table 10, Panel B), I include district fixed effects to account for regional differences at a lower level than the state level. The available data set does not allow a more detailed regional differentiation than the district level. Since 2005, Lower Saxony does no longer divide its territory into districts, which takes the relevance of district level. For this reason I don't include district fixed effects in all regressions. The data does however still indicate the former district in Lower Saxony, which is why I can include district fixed effects in the regression. The coefficients of a regression with district fixed effects are comparable in size and significance level to the main results.

6 Conclusion

Numerous studies have found a high intergenerational transmission of education in Germany (e.g., Behrmann, 1997; Dustmann, 2004; Hertz et al., 2007). I argue that the degree to which nurture (e.g., family environment) affects the educational attainment of a child is influenced by the design of the educational system.

In this study, I analyze whether a change in the age of tracking in the German school system influences the degree of intergenerational transmission of education between parents and their offspring. If the tracking decision takes place at a younger age, teachers and parents might not have sufficient information about a student's abilities to decide which track is best for the child. Instead, they might, intentionally or not, more often copy the educational degree of the parents for the child. While there is a relatively broad literature investigating the effect of a school system with tracking versus a comprehensive schooling system, literature on a shift in the time of tracking relatively early in the schooling system is rather scarce.

This paper analyzes the causal effect of earlier tracking on intergenerational transmission of education. I analyze the effect of a recent reform in Lower Saxony, which resulted in a change in the age of tracking from after sixth grade to after fourth grade. Students in the state of Lower Saxony that started fifth grade in 2004/2005 or later were affected

by the reform. Students in Baden-Württemberg and Bavaria were not affected and had tracking after fourth grade for the whole observation period. To identify the effect, I use several waves of data of the German Microcensus and apply a difference-in-differences approach.

I find that children with less educated parents suffer from earlier tracking as their probability to finish medium secondary school decreases and the likelihood to obtain not more than lower secondary degree increases when tracking takes place two years earlier. Children of higher educated parents seem to benefit from the reform, as their probability to obtain an upper secondary degree increases with earlier tracking, while their probability to obtain a medium secondary degree decreases. These results support the hypothesis that earlier tracking increases the relevance of parental education for the track choice of a child. In other words, earlier tracking increases intergenerational transmission of education.

These results support the finding of a correlation between the time of tracking in a school system and the extend of intergenerational transmission of education (Bauer & Riphahn, 2006; Hanushek & Wössmann, 2006; Mühlenweg, 2008). They are also in line with the effect of a change from a school system with tracking to a comprehensive schooling system (Meghir & Palme, 2005; Pekkarinen et al., 2009; Kerr et al., 2013; Malamud & Pop-Eleches, 2011). Being most closely related to those studies providing causal evidence on the effect of a change in the age of tracking in the German context (Lange & von Werder, 2017; Piopiunik, 2014; Roller & Steinberg, 2017), my results support previous findings.

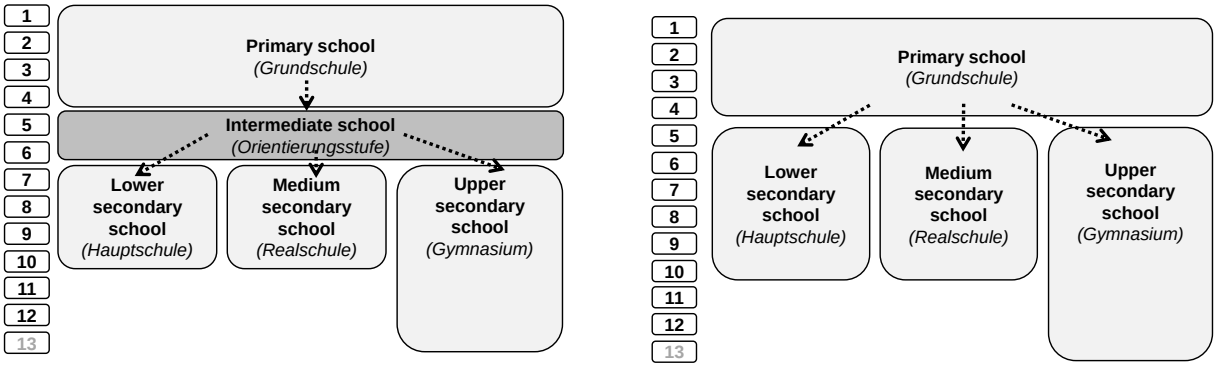
A subgroup analysis reveals that earlier tracking affects male students more than female students and is not harming migrants more than native students. Interestingly, I observe a stronger reform effect for students in rather rural areas, compared to students living in urban areas. I argue that in rural areas, the educational degree of the parents is better known by the teachers and is therefore used more often as a signal for the academic potential of a child.

Due to data limitation, some questions have to be left for future research. I cannot identify whether the observed effect diminishes after some years, when elementary school teachers have more experience in giving track recommendations. I can additionally not identify which mechanisms drive the effect. One potential mechanism might be the lower amount of information available about the student when tracking takes place at an earlier age. Consequently, teachers and parents might rely stronger on the educational degree of the parents for the track choice of the child. Another potential mechanism might be different learning environment when tracking takes place at a later age. If tracking is delayed by two years, the class composition is more heterogeneous in these two years and the teacher has to serve the needs of students of all skill levels. Especially children of less educated parents might benefit from the presence of higher achieving students, because their family environment cannot compensate the lack of higher achieving peers.

Even though there are still questions to answer, this study provides clear evidence that the educational system has an impact on the degree to which educational attainment is transmitted from one generation to the next. Consequently, if policy makers in countries with an early tracking system are aiming at providing equal opportunities in education for all children irrespective of the family environment, it might be worthwhile to reconsider the age of first tracking in the education system.

7 Figures and tables

Figure 1: German educational system



(a) Treated state before reform

(b) Control states, treated state after reform

Notes: The figures display the education system before (a) and after the treatment (b) in the treated state, Lower Saxony. Figure (b) also displays the system in the control states Bavaria and Baden-Württemberg. The numbers on the left of each figure depict the grades. In both, treatment and control states, the total years of schooling required to obtain an upper secondary degree changed from 13 to 12 in the year of the reform.
Source: own illustration.

Table 1: Composition of basic sample

		Pre-treatment		Post-treatment	
Birth cohort →		**1990/1991**	**1991/1992**	**1993/1994**	**1994/1995**
Year of survey	2014	Graduated	Graduated	Graduated	Graduated
	2013	Graduated	Graduated	Graduated	12/Grad.
	2012	Graduated	Graduated	12/Grad.	11/12
	2011	Graduated	13/Grad.	11/12	10/11
	2010	13/Grad.	12/13	10/11	9/10
	2009	12/13	11/12	9/10	8/9
	2008	11/12	10/11	8/9	7/8
LS		Tracking after grade 6		Tracking after grade 4	
BW, BY		Tracking after grade 4		Tracking after grade 4	

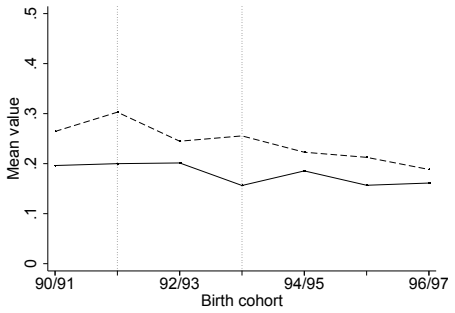
Notes: For each survey year and birth cohort, the table displays the school year a student enrolled in upper secondary school is currently attending. The gray shaded areas mark the observations that enter the basic sample. E.g., the bottom left "11/12" means that a student born in the birth cohort 1990/1991 is enrolled in 11th grade in the beginning of 2008 and in 12th grade after the summer break. Students who attend lower and medium secondary school have already graduated when students in upper secondary school are in grade 11,12, and 13.
Source: own illustration.

Table 2: Placebo test with pre-treatment period

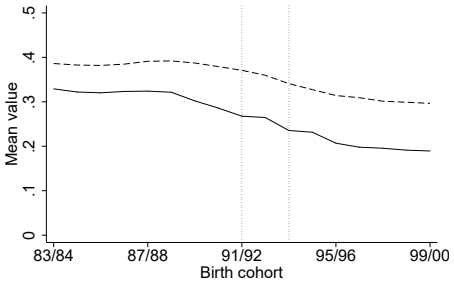
	Lower sec. degree	**Medium sec. degree**	**Upper sec. degree**
Post_plac*treat	-0.036	-0.001	0.037
	(0.023)	(0.027)	(0.026)

Notes: N=7,616 (only pre-treatment cohorts included). *Post_plac* is an identifier for a placebo-reform one year before the actual reform. All regressions include controls for parents' education, gender, migration background, number of siblings, firstborn child, months born after cutoff for school enrollment, working parents, family income, level of urbanization and a constant. Standard errors are robust. Significance level: *<0.1, **<0.05, ***<0.01.
Source: German Microcensus 2008-2014, own calculation.

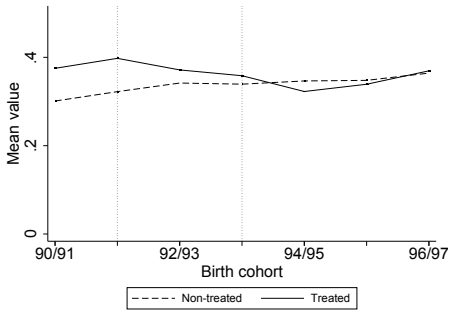
Figure 2: Common trends, graphical illustration



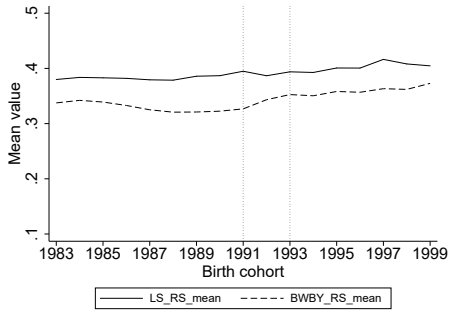
(a) Lower sec. degree, sample



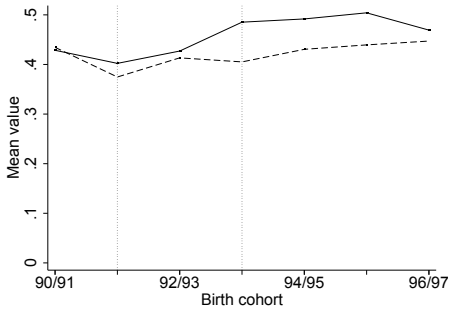
(b) Lower sec. degree, aggregated data



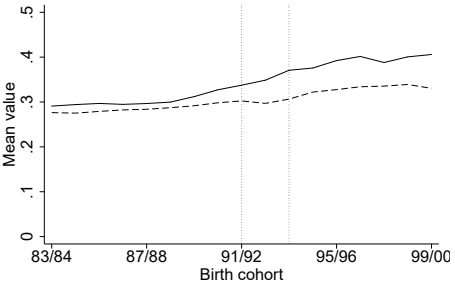
(c) Medium sec. degree, sample



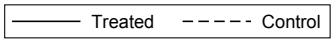
(d) Medium sec. degree, aggregated data



(e) Upper sec. degree, sample

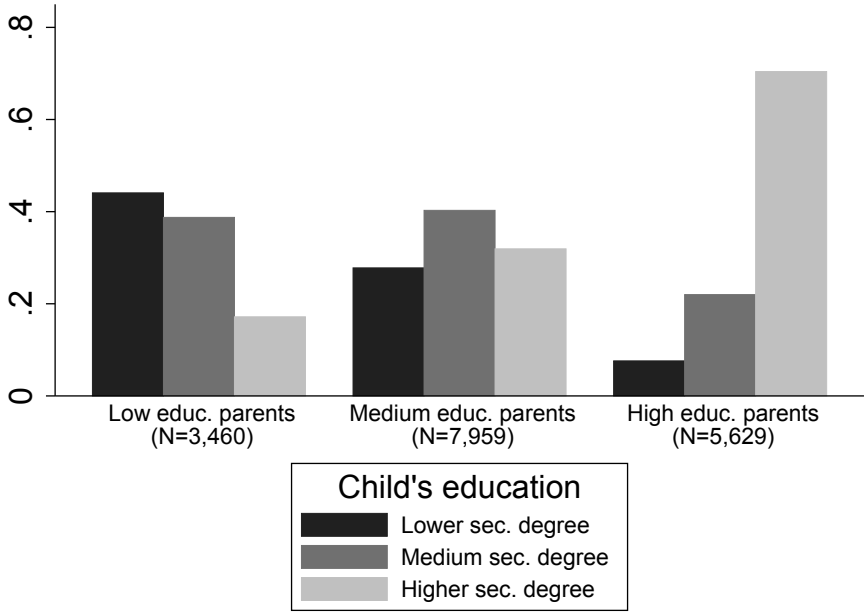


(f) Upper sec. degree, aggregated data



Notes: The figures display the share of students that finish a certain track (a, c, e) or are enrolled in a certain track during 9th grade (b, d, f). Figures (a), (c) and (e) are based on the main sample (Microcensus 2008-2014), while (b), (d) and (f) present a longer time trend with aggregated data from the Federal Statistical Office. The continuous line represents observations from the treated state, the dashed line refers to the control states. The first vertical line in each figure marks the last pre-reform cohort, the second vertical line tags the first post-reform cohort. The cohort in between does not enter the sample.
Sources: Figures (a), (c) and (e): Microcensus 2008-2014, figures (b), (d) and (f) Federal Statistical Office (Destatis), 2017; own calculation.

Figure 3: Intergenerational transmission of education - descriptive evidence



Notes: The figure depicts the share of children who obtain a certain degree given the education of the parents.
Source: German Microcensus 2008-2014, own calculation.

Table 3: Differences in sample means

	Treat = 0			Treat = 1			
	Post = 0	Post = 1	Diff (1)	Post = 0	Post = 1	Diff (2)	Diff-diff (1-2)
Male	0.521	0.515	0.007	0.512	0.508	0.004	0.003
Months born after school enrollm.	6.547	6.628	-0.081	6.585	6.652	-0.067	-0.014
Native	0.782	0.770	0.012*	0.813	0.838	-0.025**	0.014**
Fst. gen. immigrant	0.054	0.047	0.007*	0.066	0.039	0.026***	-0.019**
Sec. gen. immigrant	0.163	0.183	-0.020**	0.121	0.123	-0.002	-0.018
Number of siblings	2.142	2.144	-0.002	2.088	2.115	-0.027	0.026
Firstborn child	0.506	0.508	-0.002	0.496	0.512	-0.016	0.014
Single parent	0.224	0.224	0.000	0.256	0.243	0.013	-0.013
Working parents	0.755	0.778	-0.023***	0.692	0.737	-0.045	0.022
HH income below 2000 Euro	0.169	0.144	0.026***	0.234	0.196	0.037***	-0.012
HH income 2001-4000 Euro	0.541	0.490	0.050***	0.580	0.526	0.055***	-0.004
HH income 4001-6000 Euro	0.215	0.266	-0.051***	0.141	0.205	-0.063***	0.012
HH income above 6000 Euro	0.169	0.144	0.026***	0.234	0.196	0.037***	-0.012
Residence, below 5000 inhab.	0.261	0.252	0.010	0.164	0.166	-0.002	0.011
Residence, 5001 - 20,000 inhab.	0.372	0.386	-0.013	0.304	0.331	-0.027*	0.014
Residence, 20,001 - 100,000 inhab.	0.218	0.210	0.008	0.376	0.360	0.016	-0.007
Residence, above 100,000 inhab.	0.148	0.152	-0.004	0.156	0.143	0.013	-0.017
N	5,756	7,147		1,860	2,285		17,048

Table presents mean values for outcome and control variables, separately by treatment and cohort status. Last column tests the existence of intertemporal variation in sample means for control variables. Significance level: *<0.1, **<0.05, ***<0.01.
Source: German Microcensus 2008-2014, own calculation.

Table 4: Effect of early tracking on educational attainment

	Lower sec. degree	Medium sec. degree	Upper sec. degree
Post	-0.036***	0.037***	-0.001
	(0.007)	(0.008)	(0.008)
Treat	-0.105***	0.080***	0.025**
	(0.011)	(0.013)	(0.012)
Post*treat	0.036***	-0.078***	0.042***
	(0.014)	(0.017)	(0.016)

Notes: N=17,048. All regressions include controls for parents' education, gender, migration background, number of siblings, firstborn child, months born after cutoff for school enrollment, working parents, family income, level of urbanization and a constant. Standard errors are robust. Significance level: *<0.1, **<0.05, ***<0.01.
Source: German Microcensus 2008-2014, own calculation.

Table 5: Effect of early tracking on educational attainment by parental education

	Lower sec. degree	Medium sec. degree	Upper sec. school
Post	-0.043***	0.037***	0.005
	(0.010)	(0.010)	(0.009)
Treat	-0.141***	0.100***	0.041***
	(0.014)	(0.016)	(0.014)
Post*treat	0.044**	-0.073***	0.029
	(0.019)	(0.022)	(0.020)
Post*treat*high_educ_par	-0.048*	0.001	0.047
	(0.025)	(0.034)	(0.034)
F-test p-value	0.845	0.006***	0.007***

Notes: N=17,048. All regressions include controls for parents' education, gender, migration background, number of siblings, firstborn child, months born after cutoff for school enrollment, working parents, family income, level of urbanization, high_educ_par*treat, high_educ_par*post, and a constant. F-test tests joint significance of post*treat and post*treat*high_educ_par. Standard errors are robust. Significance level: *<0.1, **<0.05, ***<0.01.
Source: German Microcensus 2008-2014, own calculation.

Table 6: More precise differentiation by parental education

	Lower sec. degree	Medium sec. degree	Upper sec. degree
Panel A: Low educ. par. (N=3460)			
Post*treat	0.050	-0.041	-0.008
	(0.041)	(0.043)	(0.035)
Panel B: Medium educ. par (N=7959)			
Post*treat	0.041*	-0.077***	0.036
	(0.021)	(0.035)	(0.023)
Panel C: High educ. par. (N=5629)			
Post*treat	-0.004	-0.067**	0.070**
	(0.016)	(0.026)	(0.028)

Notes: All regressions include controls for gender, migration background, number of siblings, firstborn child, months born after cutoff for school enrollment, working parents, family income, level of urbanization, high_educ_par*treat and high_educ_par*post and a constant. Standard errors are robust. Significance level: *<0.1, **<0.05, ***<0.01.
Source: German Microcensus 2008-2014, own calculation.

Table 7: Effect of early tracking on educational attainment by subgroups

		Lower sec. degree	Medium sec. degree	Upper sec. degree
A: Gender				
Females	Post*treat	0.016	-0.064**	0.048
(N=8,256)		(0.025)	(0.031)	(0.029)
	Post*treat*high_educ_par	0.020	-0.039	0.019
		(0.032)	(0.048)	(0.048)
	F-test p-value	0.078*	0.004***	0.079*
Males	Post*treat	0.070**	-0.082***	0.011
(N=8,792)		(0.028)	(0.030)	(0.026)
	Post*treat*high_educ_par	-0.107***	0.031	0.076
		(0.038)	(0.049)	(0.049)
	F-test p-value	0.162	0.187	0.036**
B: Migration background				
Natives	Post*treat	0.034*	-0.081***	0.047**
(N=13,429)		(0.020)	(0.024)	(0.022)
	Post*treat*high_educ_par	-0.063**	0.032	0.031
		(0.027)	(0.037)	(0.038)
	F-test p-value	0.106	0.082*	0.012**
Migrants	Post*treat	0.088*	-0.049	-0.039
(N=3,619)		(0.049)	(0.048)	(0.044)
	Post*treat*high_educ_par	-0.022	-0.113	0.135
		(0.066)	(0.083)	(0.083)
	F-test p-value	0.146	0.015**	0.169
C: Level of urbanization				
Rural	Post*treat	0.045*	-0.038	-0.008
(N=10,209)		(0.025)	(0.029)	(0.027)
	Post*treat*high_educ_par	-0.099**	-0.022	0.121**
		(0.038)	(0.050)	(0.052)
	F-test p-value	0.067*	0.142	0.011**
Urban	Post*treat	0.047	-0.113***	0.066**
(N=6,839)		(0.029)	(0.032)	(0.030)
	Post*treat*high_educ_par	-0.018	0.027	-0.009
		(0.035)	(0.048)	(0.047)
	F-test p-value	0.147	0.014**	0.124

Notes: Separate regressions for each subgroup. All regressions include controls for parents' education, gender (except A), migration background (except B), number of siblings, firstborn child, months born after cutoff for school enrollment, working parents, family income, level of urbanization (except C), high_educ_par*treat and high_educ_par*post and a constant. F-test tests joint significance of post*treat and post*treat*high_educ_par. Standard errors are robust. Significance level: *<0.1, **<0.05, ***<0.01.

Source: German Microcensus 2008-2014, own calculation.

Table 8: Effect of earlier tracking on track choice - sample variation

	Lower sec. degree	Medium sec. degree	Upper sec. degree
A: Additional post-treatment cohort sample (N=21,905)			
Post*treat	0.053***	-0.079***	0.026
	(0.017)	(0.020)	(0.018)
Post*treat*high_educ_par	-0.058**	0.018	0.041
	(0.023)	(0.032)	(0.032)
F-test p-value	0.730	0.0134**	0.019**
B: Additional states sample (N=27,908)			
Post*treat	0.027*	-0.066***	0.039**
	(0.018)	(0.021)	(0.019)
Post*treat*high_educ_par	-0.042*	0.006	0.037
	(0.023)	(0.032)	(0.032)
F-test p-value	0.322	0.015**	0.004***

Notes: Separate regressions for each sample. All regressions include controls for parents' education, gender, migration background, number of siblings, firstborn child, months born after cutoff for school enrollment, working parents, family income, level of urbanization, high_educ_par*treat and high_educ_par*post and a constant. F-test tests joint significance of post*treat and post*treat*high_educ_par. Standard errors are robust. Significance level: *<0.1, **<0.05, ***<0.01.
Source: German Microcensus 2008-2014, own calculation.

Table 9: Evidence targeting sample selection

	Lower sec. degree	Medium sec. degree	Upper sec. degree
A: Missing parents included (N=17,609)			
Post*treat	0.044**	-0.073***	0.029
	(0.019)	(0.022)	(0.020)
Post*treat*high_educ_par	-0.048*	0.001	0.046
	(0.025)	(0.034)	(0.034)
F-test p-value	0.826	0.007***	0.008***
B: Excluding individuals who moved in previous year (N=17,022)			
Post*treat	0.043**	-0.072***	0.029
	(0.019)	(0.022)	(0.020)
Post*treat*high_educ_par	-0.046*	0.001	0.045
	(0.025)	(0.034)	(0.034)
F-test p-value	0.852	0.007***	0.009***

Notes: Separate regressions for each sample. All regressions include controls for parents' education, gender, migration background, number of siblings, firstborn child, months born after cutoff for school enrollment, working parents, family income, level of urbanization, high_educ_par*treat, high_educ_par*post and a constant. F-test tests joint significance of post*treat and post*treat*high_educ_par. Standard errors are robust. Significance level: *<0.1, **<0.05, ***<0.01.

Source: German Microcensus 2008-2014, own calculation.

Table 10: Standard errors clustered at the state level

	Lower sec. degree	Medium sec. degree	Upper sec. degree
Post*treat	0.044***	-0.073**	0.029
	(0.000)	(0.010)	(0.014)
Post*treat*high_educ_par	-0.048***	0.001	0.047*
	(0.001)	(0.015)	(0.015)
F-test p-value	0.457*	0.004***	0.000***
Post*treat	0.045**	-0.073**	0.028
	(0.019)	(0.022)	(0.020)
Post*treat*high_educ_par	-0.047*	0.002***	0.045
	(0.025)	(0.034)	(0.034)
F-test p-value	0.934*	0.007***	0.009***

Notes: N=17,048. All regressions include controls for parents' education, gender, migration background, number of siblings, firstborn child, months born after cutoff for school enrollment, working parents, family income, level of urbanization, high_educ_par*treat and high_educ_par*post and a constant. F-test tests joint significance of post*treat and post*treat*high_educ_par. Standard errors are clustered at the state level with the number of clusters minus one degrees of freedom. Significance level: *<0.1, **<0.05, ***<0.01. Source: German Microcensus 2008-2014, own calculation.

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A Appendix

Table A1: Descriptive statistics

Variable name	Mean	Std. dev.	Minimum	Maximum
Lower sec. degree	0.244	0.430	0	1
Medium sec. degree	0.339	0.473	0	1
Upper sec. degree	0.416	0.493	0	1
Post	0.553	0.497	0	1
Treat	0.243	0.429	0	1
Post_treat	0.134	0.341	0	1
High_educ_par	0.330	0.470	0	1
Male	0.516	0.500	0	1
Months born after school enrollm.	6.599	3.423	1	12
Native	0.787	0.315	0	1
Fst. gen. immigrant	0.051	0.219	0	1
Sec. gen. immigrant	0.162	0.368	0	1
Number of siblings	2.133	0.883	1	X
Firstborn child	0.506	0.500	0	1
Single parent	0.230	0.421	0	1
Working parents	0.756	0.430	0	1
HH income below 2000 Euro	0.169	0.375	0	1
HH income 2001 - 4000 Euro	0.522	0.500	0	1
HH income 4001 - 6000 Euro	0.227	0.419	0	1
HH income above 6000 Euro	0.082	0.317	0	1
Residence, below 5000 inhab.	0.234	0.423	0	1
Residence, 5001-20,000 inhab.	0.365	0.481	0	1
Residence, 20,001-100,000 inhab.	0.251	0.434	0	1
Residence, above 100,000 inhab.	0.150	0.417	0	1

Notes: Maximum value of number of siblings hidden due to data protection issues.
Source: German Microcensus 2008-2014, own calculation.