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„Am Ziele deiner Wünsche wirst du jedenfalls eines vermissen: dein Wandern zum Ziel.“

Marie von Ebner-Eschenbach

Abstract

This dissertation consists of three studies on educational choices. It shows how educational choices affect individuals' long-term health and how educational paths and aspirations are altered due to institutional changes.

The first study, "Unlucky to Be Young? The Long-Term Effects of School Starting Age on Smoking Behavior and Health," analyzes the long-term causal effect of school starting age on both the health and the smoking behavior of adults who are on average in their late thirties. The analysis employs a fuzzy regression discontinuity design based on data from the German Socio-Economic Panel (SOEP). Exogenous school entry rules, which are based on a child's date of birth, are used as the instrument. The results show that an increase in school starting age reduces the risk of smoking and improves health in the long term.

The second study, "Timing of Early School Tracking and Educational Paths," analyzes how the timing of tracking in an early tracking system influences educational paths. In 2004, the German federal state Lower Saxony shifted tracking by two years, from tracking after grade six to tracking after grade four. The analysis is based on administrative data from the German Federal Statistical Office. Using a difference-in-differences approach, the study finds that the reform increased the share of individuals in the highest academic track, but led to higher grade repetition rates at the same time. The last result suggests that the reform lowered the quality of students in the academic track.

The third study, "University Tuition Fees and High School Students' Educational Aspirations," analyzes whether higher education tuition fees influence the intention to acquire a university degree among high school students and, if so, whether the effect on individuals from low-income households is particularly strong. The study analyzes the introduction and subsequent elimination of university tuition fees in Germany across states and over time in a difference-in-differences setting. Using data from the Youth Questionnaire of the SOEP, we find a large negative effect of tuition fees on the intention of 17-years-olds to acquire a higher educational degree. Individuals from low-income households mainly drive the results.

Zusammenfassung

Diese Dissertation beschäftigt sich innerhalb von drei Papieren, wie sich Bildungsentscheidungen langfristig auf die Gesundheit von Personen auswirken und wie Personen ihre Bildungsentscheidungen anpassen auf Grund von institutionellen Veränderungen.

Die erste Studie, „Unlucky to Be Young? The Long-Term Effects of School Starting Age on Smoking Behavior and Health“, analysiert den kausalen Effekt vom Einschulungsalter auf das langfristige Rauchverhalten und der Gesundheit für Personen, welche im Durchschnitt Ende 30 sind. Unsere Analyse verwendet ein Fuzzy Regression Discontinuity Design und Daten vom Sozio-oekonomischen Panel. Weil manche Eltern den Einschulungszeitpunkt strategisch wählen, nutzen wir Einschulungsregeln als Instrument für das Einschulungsalter. Die Ergebnisse zeigen, dass ein höheres Einschulungsalter langfristig sowohl das Risiko zu Rauchen reduziert als auch die Gesundheit verbessert.

Die zweite Studie, „Timing of Early School Tracking and Educational Paths“, analysiert wie der Zeitpunkt der Entscheidung über die Schulform Bildungsentscheidungen beeinflusst. In 2004 hat das deutsche Bundesland Niedersachsen die Entscheidung über die Schulform um zwei Jahre verschoben. Vor der Reform wurde die Wahl nach sechs Schuljahren getroffen und nach der Reform nach vier Schuljahren. Die Analyse verwendet Daten vom Statistischen Bundesamt und Difference-in-Differences Schätzungen. Die Ergebnisse zeigen, dass die Reform den Anteil von Schülern im Gymnasium erhöht hat.

Die dritte Studie, „University Tuition Fees and High School Students' Educational Aspirations“, analysiert, ob Studiengebühren den Wunsch von 17-jährigen Schülern zu studieren beeinflussen. Zusätzlich untersucht die Studie, ob der Effekt für Schüler aus einkommensschwachen Familien besonders groß ist. Die Studie nutzt sowohl die Einführung als auch die Abschaffung der Studiengebühren in Deutschland und verwendet Difference-in-Differences Schätzungen. Die Ergebnisse zeigen, dass die Neigung, ein Universitätsabschluss anzustreben, für 17-jährige Schüler durch die Studiengebühr fällt. Insbesondere Schüler aus einkommensschwachen Familien ändern ihren Wunsch zu studieren aufgrund von Studiengebühren.

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Chapter 1

Introduction

Individuals make a number of educational choices over their lifetimes (e.g., about when to start schooling, the school track, and the timing of their departure from the educational system.) These educational choices might have a large impact on an individual's life. One reason is that education determines labor market opportunities (e.g., Riddell and Song, 2011) and is related to non-labor market outcomes like health status (e.g., Kemptner et al., 2011) and criminal behavior (e.g., Machin et al., 2011). Furthermore, early skills acquisition reduces the cost of achieving skills later in life (Cunha and Heckman, 2007), meaning that it is costly to redeem low investments in childhood with higher investments in adulthood.

A common feature of the studies within this dissertation is that they provide evidence about Germany specifically. One important trait of the German education system in terms of this dissertation is that Germany has one of the most rigorous tracking systems in the world. Tracking means that students are assigned to different school types that differ with respect to the curriculum, number of class hours, and the school degree. The academic track prepares students for higher education, while the medium and low track prepare students for an apprenticeship in a job.

Policy makers have various instruments to influence educational decisions. For example, they can change the rules relating to school entry or the years of compulsory schooling as well as whether children are tracked, and, if so, the timing of tracking. Each intervention might have consequences for an individual's life. For example, a change in the school entry rule might alter the school starting age, an increase in compulsory schooling might change the years of school attendance, and an abolishment of tracking raises ability dispersion in the classroom.

This dissertation has two objectives. First, it shows how educational decisions affect both long-term health and smoking behavior (Chapter 2). Second, it demonstrates how institutional changes alter educational paths and educational aspirations (Chapters 3 and 4). The chapters consider different stages of education, from school start to the transition into university. Chapter 2, “Unlucky to Be Young? The Long-Term Effects of School Starting Age on Smoking Behavior and Health,” considers school start; Chapter 3, “Timing of Early School Tracking and Educational Paths,” analyzes track decisions; Chapter 4, “University Tuition Fees and High School Students’ Educational Aspirations,” examines aspirations to enroll at a university.

Because better education is related to favorable health status and better health behavior, education seems to be one instrument to influence health. The impact of education on smoking behavior is especially interesting for different reasons. Smoking increases the risk of contracting noncommunicable diseases like primary cancer or chronic lung diseases, and so smoking is considered to be the leading cause of preventable death (WHO, 2015a). Moreover, the healthcare costs induced by smoking and its health consequences are substantial. For instance, Effertz (2015) estimates that Germany’s annual direct healthcare costs from smoking are EUR 25 billion.

Chapter 2, “Unlucky to Be Young? The Long-Term Effects of School Starting Age on Smoking Behavior and Health,” shows how school starting age affects both the long-term health and the smoking behavior of adults who are in their late thirties on average. This chapter analyzes several potential mechanisms relating to the ways in which school starting age impacts smoking behavior and health status. An OLS regression will not reveal the causal impact of school starting age on long-term health status and smoking behavior, because parents might strategically enroll their children with respect to the child’s school readiness and health status. Therefore, Chapter 2 uses a fuzzy regression discontinuity design and relies on age-based school entry rules as the instrument to obtain unbiased estimates of the causal effects.

The literature relating to the ways in which education affects health status and health behavior is extensive. The majority of studies examine school attainment and its impact on health (e.g., Webbink et al., 2010; Clark and Royer, 2013). Other studies show how health is influenced by parental education (e.g., Chou et al., 2010; Güneş, 2015) or by health education (e.g., Mora et al., 2015). In contrast to these studies, Chapter 2 analyzes how a single decision made at the beginning of the educational career (i.e., age at school enrollment) affects both long-term health and smoking behavior. However, Chapter 2 is not the first study that analyzes the effect of school starting age on smoking behavior and health. Argys and Rees (2008) show that a higher age at school enrollment lowers the likelihood of females beginning to smoke in adolescence. Nevertheless, to the best of our knowledge, we are the first to analyze the impact of school starting age on long-term health status and smoking behavior. To analyze these long-term effects is especially relevant, because the negative health consequences of smoking might not appear before an individual has smoked for several years. Moreover, it is not clear a priori whether individuals who start smoking because of older peers are the ones who also smoke in adulthood.

Germany and Austria begin to track students at the age of ten, which is earlier than all other OECD countries (OECD, 2013b). On average, OECD countries track students for the first time at the age of fourteen. This early tracking in Germany is important for Chapter 2, because students who are relatively young at the start of primary school have a higher risk of attending a lower school track (Jürges and Schneider, 2007; Mühlenweg and Puhani, 2010; Dustmann, 2004). Since the share of smokers is higher in the lower school tracks, and Germany tracks students very early, students who are relatively young at the start of primary school might be exposed to unfavorable peer effects for a long time. Chapter 3 looks more closely at the track decision and shows how the timing of tracking choice affects educational decisions in an early tracking system.

One major argument for tracking is that grouping students according to their abilities will provide the best environment for them. However, whether ability grouping does indeed raise students' achievements is controversial. For instance, supporting the hypothesis of efficiency gains due to tracking, Hoxby and Weingarth (2005) find evidence that students' achievements improve when class members have similar characteristics. In contrast, Burke and Sass (2013) find that a higher share of middle-type students in the classroom has a positive effect on the test scores of low-type students. Moreover, tracking is often criticized, because studies find that postponing tracking leads to higher educational achievements and that this positive effect of postponement is often larger for individuals from disadvantaged families (e.g., Meghir and Palme, 2005; Pekkarinen et al., 2009; Malamud and Pop-Eleches, 2011).

Chapter 3, "Timing of Early School Tracking and Educational Paths," analyzes track decisions in an early tracking system, where "("early tracking) means tracking before grade seven. In 2004, the German federal state Lower Saxony shifted tracking by two years, from tracking after grade six to tracking after grade four. This chapter analyzes how the reform affected the share of individuals in the highest (i.e., academic track). In addition, it shows whether the grade repetition rate in the academic track is also affected by the reform. A change in the grade repetition rate provides evidence suggestive of how the reform has altered the ability distribution in the academic track.

Chapter 3 contributes to the existing literature, because little is known about how variation in early tracking affects educational paths. Many studies use educational reforms to estimate the impact of tracking on educational paths. These reforms can be divided into three categories: (i) a shift from early to late tracking, (ii) a variation in late tracking, and (iii) a variation in early tracking. Most studies belong to the first two categories.[1] However, the effect of a reform might depend on the type of the reform. In the case of a shift from early tracking to late tracking, the track choice is likely to be transferred from the parent to the child. In the second case, variation in late tracking, the effect of the reform will depend on the behavioral response of the child. In contrast, in the case of variation in early tracking, the effect of the reform will mainly depend on the behavioral response of the parents.

In Germany, after students have finished the academic track, school graduates decide whether they want to enroll in higher education. While primary and secondary schooling is largely free of charge, tuition fees are often charged in higher education. In 2011, for example, only one-third of the OECD countries did not charge tuition fees, one-third charged relatively low tuition fees (USD 1500 and below), and one-third charged fees above USD 1,500 to nationals of the respective country (OECD, 2013a).[2] Fees in education are at odds with the objective of free access to education. For instance, the United Nations' Universal Declaration of Human Rights states "Technical and professional education shall be made generally available and higher education shall be equally accessible to all on the basis of merit" (Article 26, paragraph 1). One con-

[1]One exception is the study by Piopiunik (2014). However, he studies the effect of tracking on test scores.

[2]These figures are calculated for the 26 OECD countries for which data was available (OECD, 2013a).

cern is that some individuals might be discouraged from studying due to tuition fees. However, if tuition fees do not significantly affect individuals' educational choices, there might be plausible reasons for the introduction of tuition fees: these fees could lead to greater equality, because students who benefit from higher education contribute to the funding of their studies. In particular, low tuition fees seem to be appropriate, as this permits institutions of higher education to charge students for their studies without discouraging them.

Chapter 4, "University Tuition Fees and High School Students' Educational Aspirations," shows how small tuition fees (of about EUR 1,000 per academic year) influence the educational aspirations of 17-year-old high school students. Furthermore, the chapter addresses tests whether individuals from low-income households are more affected than individuals from wealthier households. In 2006 and 2007, seven out of sixteen federal states introduced tuition fees in Germany. However, all federal states abolished the tuition fees in subsequent years. We exploit the introduction and the abolishment of tuition fees in a difference-in-differences setting. Federal states that did not introduce tuition fees serve as control group.

While many studies analyze the effect of tuition fees on college enrollment in countries with high tuition fees, such as the US, UK, and Australia (McPherson and Schapiro, 1991; Kane, 1994; Cameron and Heckman, 2001; Dearden et al., 2004; Chapman and Ryan, 2005), relatively little is known about the potential effects of low tuition fees and, in particular, about how tuition fees influence adolescents' educational aspirations and plans in countries with a history of free access to higher education. The German education system provides a unique opportunity to study this question, because some federal states introduced tuition fees while other federal states did not. As a result, control states are available for difference-in-differences estimations. Several studies address a variation in tuition fees across universities or time, or use both kinds of variation (Kane, 1994; Denny, 2014). However, to obtain unbiased estimates in this setting requires the restrictive assumption that the level of tuition fees is not affected by the demand for university degree programs.

All studies of this dissertation estimate causal effects. Chapter 2 investigates school entry cutoff rules in a regression discontinuity framework to estimate the causal effect of school starting age on both long-term health status and smoking behavior. In contrast, Chapters 3 and 4 rely on difference-in-differences estimations. Both chapters exploit German federal states' discretion about educational reforms. Reforms in education are often not introduced by all federal states, and other federal states are often available to form the control group in a difference-in-differences setting. So as to establish a proper control group, both chapters utilize the fact that the federal states share common traits. In Germany, the Standing Conference of Länder Ministers of Education and Cultural Affairs (*Kultusministerkonferenz*) has to ensure that the educational systems of the federal states have enough common traits to ensure that the mobility of individuals between federal states is not hampered. That federal states share common traits makes the common trend assumption in difference-in-differences estimations more credible. In Chapter 3, German federal states that did not alter the timing of tracking and also have no other major educational changes constitute the control group. Similarly, Chapter 4 uses German federal states that did not introduce tuition fees as the control group.

Chapters 2 and 4 use data from the German Socio-Economic Panel (SOEP), which is a household panel study that started in 1984. The SOEP covers about 25,000 individuals from about 12,000 households (Wagner et al., 2007). One advantage of this data set is that it encompasses a broad range of topics, such as education, health, employment, and income. Chapter 2, "Unlucky to Be Young? The Long-Term Effects of School Starting Age on Smoking Behavior and Health," utilizes the comprehensive information about education, health status, and smoking behavior in particular. Chapter 4, "University Tuition Fees and High School Students' Educational Aspirations," uses information about past school achievements and educational aspirations. Furthermore, by analyzing detailed information about the family, including household income and parental education, we can study whether specific effects depend on the family background of an individual. In contrast, Chapter 3, "Timing of Early School Tracking and Educational Paths," uses administrative data from the German Federal Statistical Office. Covering every student in Germany, this data set is well-suited to give answers about average effects. The disadvantage of this data set is that gender is the only available sociodemographic information about the students.

Several persons contributed to this dissertation. Chapter 2 is a joint work with

Mathias Schumann. I had the idea for the study and prepared the data set. Mathias Schumann was primarily responsible for writing the manuscript, and I was primarily responsible for carrying out the estimations. I conducted the study in Chapter 3 solely on my own. Chapter 4 is a joint work with Thomas Siedler. He had the idea for the project, I conducted the empirical analysis, and the manuscript was written jointly.

Chapter 2

Unlucky to Be Young? The Long-Term Effects of School Starting Age on Smoking Behavior and Health

2.1 Introduction

Smoking results in significant healthcare costs (Wacker et al., 2013; Xu et al., 2015) and is considered the leading cause of preventable deaths (WHO, 2015a). According to the World Health Organization, in 2013, the share of tobacco smokers among persons aged 15 years and above was 18.1 percent in the United States, 20.3 percent in the United Kingdom and 30.7 percent in Germany (WHO, 2015b).[1] It is thus imperative to understand the determinants of smoking behavior, particularly for policymakers, to reduce the prevalence of smoking and thereby improve the health status of the population and decrease smoking-related healthcare costs.

Smoking habits are generally formed during childhood and adolescence and persist into adulthood. In Germany, the average age to start smoking was 17.3 years among 35–39-year-olds in 2013 (Destatis, 2014). In the United States, 88.2 percent of adults who had smoked daily at some point reported trying their first cigarette by the age of 18 years (U.S. Department of Health and Human Services, 2012). Gruber and Zinman (2000) show that adolescent smoking is a strong predictor of adult smoking and a percentage point increase in adolescent smoking translates into a 0.25–0.5 percentage points higher likelihood to smoke by those adolescents as adults. Chassin et al. (1996)

[1] The World Health Organization standardizes national smoker rates by applying age-specific smoker rates by sex in each population to a statistical standard population to enable cross-country comparisons.

find that smoking rates do not significantly decline among those in their late twenties and this pattern is stable across birth cohorts. Thus, analyzing factors determining smoking behavior in early life is important to prevent adolescents from smoking in their adulthood.

Sacerdote (2011) compiles a literature review and points out the role of school peers in social outcomes such as smoking and health. Norton et al. (1998), Gaviria and Raphael (2001) and Powell et al. (2005) find that an increase in the share of student smokers in school increases an individual's risk to smoke in adolescence. A recent strand of literature analyzes the effects of individual school starting age on social outcomes in adolescence.[2] Students who start school relatively young are exposed to the behavior of older class peers. Thus, school starting age affects social outcomes through relative age differences among class peers. Related studies have examined the effects of school starting age on several outcomes including non-cognitive skills, educational attainment and labor market outcomes (for example, Bedard and Dhuey, 2006; Puhani and Weber, 2007; McEwan and Shapiro, 2008; Elder and Lubotsky, 2009; Dobkin and Ferreira, 2010; Mühlenweg and Puhani, 2010; Black et al., 2011; Mühlenweg et al., 2012; Fredriksson and Öckert, 2014; Dustmann et al., 2016; Landersø et al., 2016).

However, few studies have analyzed the impact of school starting age on smoking behavior and health. Argys and Rees (2008) find that female adolescents who enroll in school at a relatively young age face a higher risk of smoking in grades 6–12. Black et al. (2011) find that 18-year-old male conscripts who started school relatively young show slightly poorer mental health in military medical inspection. Several studies show that young school starters are more likely to be diagnosed with attention-deficit/hyperactivity disorder (ADHD) in childhood and adolescence (Elder and Lubotsky, 2009; Elder, 2010; Evans et al., 2010; Morrow et al., 2012; Schwandt and Wuppermann, 2016).[3] Exploiting school entry cutoff dates, Anderson et al. (2011) show that an additional year of education does not impact children's body mass index or their likelihood of being obese.

[2]This branch of literature and our study exploit legal school starting age cutoffs to analyze the effects of relative differences in individual school starting age. By contrast, Fletcher and Kim (2016) analyze the effects of shifts in school entry cutoffs that change the general school starting age.

[3]These studies interpret the higher number of diagnoses among younger school starters as misdiagnoses, which is confirmed by Dalsgaard et al. (2012).

Despite the contributions of these studies, the evidence on school starting age effects on smoking behavior and health remains relatively sparse. Moreover, the literature focuses on the short-term effects of school starting age in adolescence and young adulthood owing to data restrictions. Thus, whether the effects of school starting age on smoking behavior and health in adolescence persist into adulthood remains an open question. From a policy perspective, it is important to determine whether the effects of school starting age remain stable or vanish over time.

In this study, we go beyond adolescence and examine the causal long-term effects of school starting age on smoking and health among adults in their late thirties. We employ a fuzzy regression discontinuity design to account for the endogeneity of school starting age because some parents time their children's school enrollment with respect to (unobserved) child characteristics, such as preschool health and perceived school readiness. Exogenous cutoff dates for school entry, as per which a child must be six years old to enter primary school, are used as an instrument for school starting age. The analysis utilizes survey data from the German Socio-Economic Panel (SOEP). We find that an increase in school starting age by one month reduces the long-term risk of smoking by about 1.3 percentage points (4 percent) and increases the long-term likelihood of reporting good or very good health by about 1.6 percentage points (2.4 percent). The effects on self-rated health can be explained on the basis of changes in physical rather than mental health. Moreover, an increase in school starting age lowers the average age of friends in adulthood; we interpret this result as evidence that suggests the importance of school peers' age composition as a mechanism through which school starting age affects smoking behavior and health. Furthermore, we show that school environment partly explains the effects of school starting age by exploiting the association between the type of secondary school degree and peer smoking intensity.

Our study makes the following three contributions to the literature. First, we complement the literature on the effects of school starting age on smoking behavior, overall health, and physical and mental health. Second, we are able to estimate long-term effects of school starting age on these outcomes. Third, we shed light on mechanisms through which school starting age affects smoking behavior and health.

School starting age is expected to affect smoking behavior and health through, first, the age composition of school peers, and second, school environment. School entry cutoff dates create exogenous variation in the relative age composition of class peers. Students born just before the cutoff are supposed to start school one year earlier than those born immediately after the cutoff—thus, students' age in the same class can differ by almost a year. Age is also an important factor affecting smoking behavior in adolescence. Figure 2.1 illustrates that the share of smokers in Germany significantly increases by age.[4] The relationship between age and smoking prevalence implies that students who started school relatively young are confronted by peers who smoke earlier. Young school starters are therefore more likely to start smoking than old school starters in the long term, because the former are exposed much earlier in life to peers who smoke.

As for school environments, Germany has a school tracking system that assigns students to different secondary school types at the age of 10. Jürges and Schneider (2007), Puhani and Weber (2007), Mühlenweg and Puhani (2010) and Dustmann et al. (2016) show that entering primary school older increases the likelihood of entering secondary schools of higher tracks. High-track schools offer a better school environment than low-track ones. Figure 2.2 shows that the share of smokers is 20 percent in low-track schools and 8 percent in high-track schools among students who are 12–15 years old. Students who started primary school relatively young are thus more likely to be exposed to a school environment with a higher number of smokers. School starting age is therefore expected to increase the risk of smoking and decrease long-term health through both the age composition of class peers and school environments.

The remainder of this paper is structured as follows. Section 2.2 describes the SOEP data and outcome variables. Section 2.3 explains the identification strategy, the German school entry rule used as an instrument for school starting age and the validity of the identifying assumptions for the fuzzy regression discontinuity design. Section 2.4 shows the descriptive statistics. Section 2.5 presents the main results and several robustness checks. Section 2.6 discusses potential mechanisms through which school starting age may affect smoking behavior and health with focus on the role of

[4] Figures 2.1 and 2.2 are based on data from the German Health Interview and Examination Survey for Children and Adolescents (KiGGS), 2003–2006, administered by the Robert Koch Institute. KiGGS is a nationwide clustered random sample of 17,641 children and adolescents (0–17 years) and their parents (Hölling et al., 2012).

school peers. Section 2.7 concludes the paper.

2.2 Data

We use data from the German Socio-Economic Panel (SOEP), which is an annually conducted, representative household survey (Wagner et al., 2007). The SOEP includes about 30,000 individuals living in roughly 11,000 households in Germany. Adult members of the household are interviewed about their socioeconomic and demographic characteristics. The SOEP offers a rich set of information such as income, employment, education and health and has been used to analyze health-related questions such as the effects of public smoking bans on smoking behavior (Anger et al., 2011), spousal job loss on mental health (Marcus, 2013) and retirement on health (Eibich, 2015).

2.2.1 Sample

We include respondents who provided complete information regarding the analysis' outcomes and covariates. The sample comprises respondents who grew up in households that participated in the SOEP when they were children or adolescents (about 17.5 percent of the sample) and respondents who entered the panel after maturity (about 82.5 percent). We conduct robustness checks to show that our results are robust to the exclusion of adolescents and young adults.

In the main specifications, we use the first available observation of each respondent separately for each outcome and neglect repeated observations for two reasons. First, Eibich (2015) and Godard (2016) show that retirement reduces the likelihood of smoking and improves health. The inclusion of observations close to retirement may therefore bias the effect of school starting age on smoking behavior and health. Second, the use of observations that are closest to a respondent's schooling period allows us to more accurately gauge the mechanisms through which school starting age affects smoking behavior and health. Thus, we use cross-sectional data comprising respondents interviewed in different survey years at varying ages. The robustness checks show that the main results are robust to both the inclusion of all available observations for each respondent and exclusion of respondents at least 60 years old.

While the literature on the effects of school starting age on smoking behavior and health is limited to short-term effects owing to data restrictions, the SOEP allows us to look beyond adolescence. The respondents in the estimation sample are, on average, 37 years old.[5] It is therefore possible for us to analyze the long-term effects of school starting age on smoking behavior and health.

2.2.2 Outcomes

We use adult smoking behavior and subjective and quasi-objective[6] health measures as outcomes to analyze the long-term relationship between smoking behavior and health and the effect of school starting age on both. Information on health and health-related behavior is available in the SOEP either annually or for certain waves.[7] Whereas annual self-rated health data are available for 1992–2013, those on smoking behavior are available from 1998 onwards for roughly every second year. The SF12 measures of physical and mental health are available since 2002 and for every second year. Consequently, the sample size varies from 1 674 to 3 856 in the preferred specifications across outcomes.

To analyze smoking behavior, we use an indicator variable that takes the value one if the respondent was a smoker at the time of the study and zero otherwise. We adopt the self-rated health, physical health and mental health scores as health measures. For self-rated health, respondents are asked to assess their current health on a five-point scale, where 1 is 'bad' and 5 is 'very good'. Because self-rated health is an ordinal variable, we use an indicator based on self-rated health as an additional outcome for a more intuitive interpretation of the effect. The indicator takes the value of one for 'good' and 'very good' health and zero otherwise.

The physical and mental health scores are taken from the continuous quasi-objective SF12. The SF12 is a concise instrument to measure physical and mental health and is based on a set of 12 questions about various health aspects, including body pain and

[5]Table 2.10 shows that respondents' average age at the time of the SOEP interview is about 35.5–37.9 years, depending on the analyzed outcome and specification. Furthermore, it shows that respondents' age does not statistically differ between persons born before or after the cutoff.

[6]'Quasi-objective' means that the respective health measure enables health comparisons across different groups of persons (for example, age groups).

[7]Table 2.11 in the Appendix provides an overview of the availability of outcome measures across SOEP waves.

emotional functioning. The 12 questions are aggregated to eight sub-scales, which in turn, are used to calculate the physical and mental health scores using an exploratory factor analysis. Both scores are continuous and normalized to have values ranging from 0 to 100, with a mean of 50 and a standard deviation of 10 in the 2004 SOEP sample (Andersen et al., 2007). A higher value indicates better physical or mental health. Studies have found that the SF12 and therefore the physical and mental health scores are valid and reliable and perform well compared to other brief health measures (Ware et al., 1996; Salyers et al., 2000).

To shed light on potential mechanisms through which school entry age might affect smoking behavior and health, we analyze three measures on the respondents' social networks as additional outcomes: 1) number of friends 2) average age of friends and 3) relative age of friends (average age of friends divided by respondent's age in years). Both age measures exclude family members and relatives. All three measures are related to the respondent's network at the time of the SOEP interview. The descriptive statistics of the outcomes are shown in Table 2.1 and detailed in Section 2.4.

2.3 Research design

Our main variable of interest in the analysis is school starting age measured in months. An ordinary least squares (OLS) regression of smoking behavior and health on school starting age is unlikely to uncover the causal effects of school starting age and would result in biased estimates because school starting age is likely to be endogenous. Parents might determine the school entry age of their children strategically by accounting for factors that are unobserved in the data. They could be concerned about their children's preschool health or health-related factors such as school readiness and thus might move up or postpone their children's school enrollment (Graue and DiPerna, 2000; Stipek, 2002).

To resolve the endogeneity of school starting age, the economic literature utilizes exogenous school entry rules as an instrument. The German school entry rule has been used to study the effects of school starting age on the likelihood of attending higher track schools (Jürges and Schneider, 2007; Mühlenweg and Puhani, 2010), test scores at the end of primary school (Puhani and Weber, 2007), and long-term labor market outcomes (Dustmann et al., 2016).

We adopt a fuzzy regression discontinuity design to analyze the long-term effects of school starting age on smoking behavior and health outcomes using the German school entry rule as an instrument for school starting age. The school entry rule determines whether a child is supposed to start school in year t or year $t+1$, depending on the month of birth. We estimate a local average treatment effect (LATE), which is the causal effect for compliers, that is, children who start school according to the legal age-based school entry rule (Hahn et al., 2001).

2.3.1 School Entry Rule and Instrument

In Germany, a child's date of birth determines the intended date of entry into primary school. The academic school year in each federal state of Germany is from August 1st to July 31st.[8] Children who turn six by June 30th in year t are supposed to start primary school on August 1st in year t, while those who turn six on July 1st or after in year t must start primary school on August 1st in year $t+1$.

Note that different school entry cutoffs existed before harmonization in Germany.[9] Because older and younger cohorts are pooled in the analysis, the instrument is coded such that different cutoffs are incorporated. The differences in entry cutoffs for older cohorts create additional variation in school starting age. The inclusion of cutoffs other than the June 30th increases the generalizability of our results, because we can rule out seasonal idiosyncrasy.

Parents, however, may still decide to enroll their children later or earlier than the school entry rule stipulates. Nonetheless, there is considerable discontinuity in school starting age at the school entry cutoff, as shown in the upper left graph in Figure 2.3.

[8] Before the German reunification in 1990, the starting month differed by federal state. Before 1964, the starting month in the Federal Republic of Germany was April or August. However, in 1964, the *Hamburger Abkommen* harmonized the start of primary school to August 1st. The starting month in the German Democratic Republic was September 1st but in 1990, it was also changed to August 1st.

[9] In the Federal Republic of Germany, some federal states had school entry cutoffs other than June 30th (about 21 percent of the sample), although this was later harmonized with the ratification of the *Hamburger Abkommen* on October 28, 1964. Before the German reunification in 1990, the school entry cutoff in the German Democratic Republic was May 31st (about 21 percent of the sample). However, following the reunification, the federal states of the former GDR adopted June 30th as the cutoff, which is relevant for about 58 percent of the sample.

The abscissa shows the distance between a person's birth month and school entry cutoff month and the ordinate shows the (observed) average school starting age (in months). Compliance with the school entry rule is not perfect because the jump at the cutoff is less than eleven months. Nonetheless, the school entry rule is a strong instrument as indicated by the considerable discontinuity of about three months at the cutoff and the negative trends to the left and right of the cutoff. Two months after the cutoff, this discontinuity even increases to five months.

Therefore, we use the school entry cutoff to define the binary instrument $older_i$. The instrument takes the value one if the respondent turned six after the cutoff in year t and should have been enrolled in year $t + 1$; it takes the value zero if the respondent turned six before the cutoff in year t and should have been enrolled in year t.

For respondents whose household participated in the SOEP during their childhood or adolescence, direct information for year and federal state of school start and school starting age sa_i is available; by contrast, the same data are unavailable for those who participated in the SOEP after maturity. For the former group, we construct the instrument $older_i$ by combining information on respondents' date of birth, year and federal state of school start. For the latter group, the highest school degree attained, the year in which it was completed, and the federal state where it was completed are used to determine the year and federal state of school start. In combination with respondents' date of birth, we construct the variable school starting age sa_i and instrument $older_i$. We discuss the possibility of measurement error in sa_i and $older_i$ for individuals without direct information regarding school starting age in Section 2.5 and show that the potential measurement error is negligible.

2.3.2 Fuzzy Regression Discontinuity Design

We employ a fuzzy regression discontinuity design because compliance with the date of birth cutoff is not perfect. However, we can still use the substantial discontinuity in school starting age at the cutoff as an instrument (Imbens and Lemieux, 2008). The fuzzy regression discontinuity design can be implemented using two-stage least squares estimation (Angrist and Pischke, 2009). In the first stage, observed school starting age sa_i (measured in months) is regressed on the instrument $older_i$, where subscript i

denotes individual i:

$$sa_i = \alpha_0 + \alpha_1 older_i + \pi X_i' + \gamma_b + \gamma_s + \gamma_w + e_i. \tag{2.1}$$

The estimate for α_1 is expected to be positive. X_i is a vector of covariates predetermined with respect to birth, including respondents' gender, paternal and maternal school education, and migration background.[10] Further, the regression includes birth year indicators, γ_b, indicators for the federal state where the child enrolled in primary school, γ_s, and survey wave indicators γ_w; e_i is an idiosyncratic error term.[11]

In the second stage, the outcome of interest y_i is regressed on predicted school starting age in months $s\hat{a}_i$:

$$y_i = \beta_0 + \beta_1 s\hat{a}_i + \delta X_i' + \gamma_b + \gamma_s + \gamma_w + u_i, \tag{2.2}$$

where X_i', γ_b, γ_s and γ_w are the same as those in the first stage and u_i is an idiosyncratic error term.

There are two main approaches to implementing a (fuzzy) regression discontinuity design. One can restrict the sample to a narrow bandwidth around the cutoff or use the entire sample and model polynomial trends of the running variable around the cutoff (Lee and Lemieux, 2010). In our main specifications, we restrict the sample to a two-month window around the birth month cutoff (that is, respondents born one month before and after the cutoff) and a four-month window around the birth month cutoff (that is, respondents born two months before and after the cutoff). We implement this approach instead of modeling trends for the entire sample because our running variable is discrete rather than continuous, which renders the estimation of flexible trends using polynomials infeasible (Angrist and Pischke, 2009). The advantage of estimating the effects in a narrow window is a reduction in bias because observations close to the cutoff are more comparable in terms of observable and unobservable characteristics. The disadvantage is the loss of precision due to the loss of observations. In the robustness

[10]The highest secondary school degree for the respondents' mothers and fathers are measured using three indicators: 1) upper secondary school degree (*Abitur*) 2) intermediate secondary school degree (*Realschulabschluss*, *Fachoberschulabschluss*) and 3) lower secondary school degree (*Hauptschulabschluss*) or no secondary school degree.

[11]Respondents who started school in the former GDR are assigned a GDR indicator.

checks, we use the entire sample and include linear trends of the running variable.

2.3.3 Identifying Assumptions

The two-stage least squares estimate for β_1 uncovers the causal effect of school starting age on the outcome of interest if three assumptions are fulfilled. First is the *relevance assumption*: the instrument must be sufficiently partially correlated with school starting age. The first-stage F-statistic is well above the conventional thresholds in each specification in our analysis.[12] Depending on the outcome and specification, the F-statistic is greater than 60 in the two-month window and larger than 200 in the four-month window in our main specifications (see Table 2.3).

Second is the *exclusion restriction*: birth month has no direct effect on smoking behavior and health. The instrument should affect the outcomes only through school starting age. In contrast to the United States, there is no interaction between school entry age and compulsory school leaving laws in Germany. Students in the United States may leave school on their 16th birthday and thus the date of birth affects the length of formal schooling. In contrast, students in Germany must complete nine years of schooling, irrespective of their date of birth and therefore the length of formal schooling is unaffected.

Third is the *independence assumption*: respondents' date of birth is random around the school entry cutoff. Randomness implies that parents do not systematically manipulate their children's date of birth with respect to the school entry cutoff. The advantage of using birth month as an instrument rather than birth quarter is that strategic birth timing is more unlikely between adjacent months than between adjacent seasons.[13]

Figure 2.4 suggests that there is neither bunching at the cutoff with respect to the number of observations per month nor systematic differences in the predetermined covariates around the cutoff. The comparison of the covariates' means around the cutoff in Panel 1 of Table 2.1 and the results of regressions that use the predetermined covariates as outcome variables in Table 2.2 confirm the absence of systematic differences

[12] Staiger and Stock (1997) suggest that an F-statistic of larger than 10 suffices.

[13] For example, Angrist and Krueger (1991) and Robertson (2011) used season of birth as an instrument.

around the cutoff. Both tests show that differences in covariates around the cutoff are generally small in size and not statistically significant.[14] There are slightly statistically significant differences in the share of mothers with higher secondary school degrees and fathers with lower secondary school degrees in the two-month window specification with covariates. However, these differences are statistically significant only at the 10 percent significance level and statistically non-significant in all other specifications. Dustmann et al. (2016) analyze parental characteristics around the school entry cutoff in the German Microcensus 2005 and do not find statistically significant differences. Overall, the evidence suggests that the identifying assumptions hold.

2.4 Descriptive statistics

Table 2.1 presents the descriptive statistics of the sample for respondents who were born before and after the school entry cutoff. The variables' means and standard deviations are shown for the two-month and four-month window. The descriptive statistics of the covariates are reported in Panel 1 and those of the outcomes are in Panel 2.

Focusing on respondents born within the two-month window, the mean values of school starting age show that respondents to the right of the cutoff are, on average, three months older than respondents to the left of the cutoff. The age difference is also illustrated in the upper left graph in Figure 2.3. Thus, the actual mean difference in school starting age is three months and not eleven months, which would be expected if all children complied with the school entry rule.

About 52 percent of the respondents in the sample are female and roughly 13 percent have some migration background. The respondents' fathers are more likely to have a higher secondary school degree than their mothers. Compared to 17 percent of fathers, only 11 percent mothers have a high secondary school degree. However, 26 percent of the mothers hold an intermediate secondary school degree compared to the 21 percent of fathers. The share of mothers and fathers with either a low or no secondary school degree is roughly the same (59 percent mothers and 58 percent fathers).[15]

[14] Moreover, Table 2.13 shows that a father's and mother's age and occupational prestige are also balanced around the cutoff.

[15] The values for school degree type do not aggregate to 100 percent because some respondents' parents have other or unspecified types of school degrees.

Panel 2 in Table 2.1 shows the mean differences in the outcome variables. The sample size in the main analysis varies from 1 674 to 1 890 in the two-month window and 3 391 to 3 856 in the four-month window. In the robustness checks, the sample size varies between 10 400 and 11 784 when all months are included in the estimation.

The descriptive statistics of the two-month window show that the share of smokers among respondents born before the cutoff is 32.4 percent and thus 4.3 percentage points higher than that of smokers among respondents born after the cutoff. This difference is almost statistically significant at the 5 percent significance level with a p-value of 0.051.

Furthermore, respondents born before the cutoff report, on average, lower health than those born after the cutoff. The absolute difference of about 0.138 is statistically significant at the 1 percent level and is about 15 percent of the variable's standard deviation. Furthermore, the share of persons who report being in good or very good health is about 5.4 percentage points lower among respondents born before the cutoff than those born after; this difference is statistically significant at the 5 percent level. The quasi-objective SF12 health measures show that the mental health score does not significantly differ between both groups; however, respondents born before the cutoff have, on average, a significantly lower physical health score than those born after. In terms of one standard deviation, the difference in the physical health score between both groups is about 11.6 percent.[16] The results for the four-month window confirm the results for the two-month window.

The descriptive statistics of the outcomes imply that individuals born before the school entry cutoff are more likely to smoke and have worse health outcomes than those born after the school entry cutoff. We consider these descriptive results to be rather informative because they resemble an unconditional reduced form estimate for the impact of the distance between the birth month and school entry cutoff on smoking behavior and health.

[16]The difference in the physical health score is divided by 10, which is the variable's standard deviation in the initial calibration of the SF12 score in the 2004 SOEP sample.

2.5 Results

2.5.1 Main Results

Regressing smoking behavior and health outcomes on school starting age using OLS results in statistically non-significant point estimates that are close to zero (Table 2.12). Thus, the OLS results suggest that school entry age has no long-term impact on smoking behavior and health. However, OLS estimation does not take into account that parents strategically enroll their children in school with respect to factors unobserved in the data and therefore yields biased estimates. For instance, parents of relatively precocious and independent children might enroll them early and parents of relatively underdeveloped children might enroll them late. Consequently, both moving up and postponing school entry are likely to bias the OLS estimates towards zero.

The following results based on the fuzzy regression discontinuity design account for the endogeneity of school starting age and show that school starting age has sizable and statistically significant effects on smoking behavior and health. Table 2.3 presents the main results of the fuzzy regression discontinuity design. It shows the estimates of the causal effect of a one-month increase in school starting age on the outcomes for the two- and four-month window for three specifications. The first specification includes school starting age as a sole covariate in the regression. The second specification includes indicators for the respondent's gender, birth year, federal state of school entry, and survey year. The third specification comprises indicators for migration background and parental education. The results are robust across specifications: changes in the coefficients' magnitudes and statistical significance are negligible. Our preferred specification is the two-month window including all covariates because it most convincingly ensures that persons to the left and right of the cutoff are comparable.

The preferred specification shows that a one-month increase in school starting age decreases the risk of smoking later by about 1.3 percentage points (4.0 percent). This effect is statistically significant at the 5 percent level. Moreover, a one-month increase in school starting age increases respondents' health status. The coefficient for the effect of school starting age on the self-reported health scale is 0.042 and statistically significant at the 1 percent level. The effect corresponds to about 4.5 percent of one standard deviation. Complementary, the likelihood to report at least good health increases by about 1.6 percentage points (2.4 percent) and is statistically significant at the 5 percent level.

The health effect is driven by physical and not mental health. The coefficients for the effect of school starting age on mental health are statistically non-significant.[17] The physical health score, however, significantly increases with school starting age; the coefficient of 0.364 corresponds to about 3.6 percent of one standard deviation. The results of the four-month window confirm the results of the two-month window. The coefficients have the same sign, are smaller in magnitude but still sizable and are of similar statistical significance.

2.5.2 Robustness

The computation of school starting age sa_i and the instrument $older_i$ for respondents without direct information for year, federal state, and school starting age might create measurement error in sa_i and $older_i$. We account for the measurement error in school starting age sa_i using our implemented instrumental variable approach, where sa_i is instrumented by $older_i$.

The potential measurement error in the instrument $older_i$ might be more problematic. Determining a respondent's relevant school entry cutoff by using information on both the federal state and year of the latest school degree might create measurement error in $older_i$ if a respondent relocated across states with different cutoffs between the start of primary school and the completion of secondary school. In the Federal Republic of Germany, school entry cutoffs differed before their harmonization in 1964. Nonetheless, many federal states shared the same cutoff before harmonization anyway.

The sub-sample of young respondents with direct information provides information on the extent of mobility during school: only 3.6 percent of the respondents reported to have moved across federal states between the start of primary school and completion of secondary school. Moreover, the share of respondents who started school in the Federal Republic of Germany before the harmonization is 17.1 percent. By multiplying

[17] Black et al. (2011) find that school starting age has a significant, but small effect on the mental health of 18–20-year-old males. By contrast, we show that school starting age has no significant effect later in life by including both males and females in the analysis.

both shares, we estimate that only 0.6 percent of respondents are misclassified in our sample. However, this figure is likely to be an upper bound because mobility should be lower among older cohorts than among younger ones. Moreover, not every mover relocated from one federal state to another with a different cutoff. Because the risk of misclassifying persons is low, we include respondents at risk of being misclassified to obtain statistical power. Nonetheless, the exclusion of these respondents in Table 2.4 gives us similar results.

In the main analysis, we use the first available observation for each respondent. Column 2 in Table 2.5 includes all available observations for each respondent in the estimation for the main specification, which restricts the sample to a two-month window around the cutoff.[18] The standard errors are clustered at the respondent level. In comparison with the main results in column 1, the statistical significance and magnitudes of the coefficients remain largely unchanged.

Furthermore, columns 3–8 in Table 2.5 analyze the sensitivity of the main results by including only certain age ranges in the estimation. To check whether the main results are driven by young respondents in the sample, columns 3 and 4 include observations for those aged 30 years and above and columns 5 and 6 include only observations for those 40 years or older. Columns 7 and 8 include observations for those younger than 60 years to avoid the potential effects of retirement on smoking behavior and health. The point estimate for the effect of school starting age on smoking behavior is hardly affected and remains statistically significant in all specifications. Although some point estimates for the effect on self-rated health decrease in size and become statistically non-significant for specifications using only the first observation per respondent, the corresponding point estimates using all observations per respondent remain statistically significant.[19] Similarly, the point estimates and statistical significances of school starting age on physical and mental health are barely affected.[20] Overall, the results of Table 2.5 confirm the main results.

[18] The results of restricting the sample to a four-month window around the cutoff are shown in Table 2.14.

[19] Note that the loss of statistical significance is not surprising given the substantial decrease in the sample size.

[20] Two point estimates for the effect on mental health are statistically significant at the 10 percent level when young respondents are excluded from the estimation; however, this effect is statistically non-significant when persons older than 60 years are excluded. Moreover, the effect is always statistically non-significant when a four-month window is used instead of a two-month window (see Table 2.14).

Moreover, Table 2.6 shows several alternative specifications that include all available months in the estimation, instead of restricting the analysis to two- or four-month windows. In addition to school starting age, the specifications include 1) no further covariates 2) one linear trend in the running variable (with and without covariates) and 3) separate linear trends in the running variable on both sides of the cutoff (with and without covariates).

The first stage equation with separate linear trends in the running variable and covariates is as follows:

$$sa_i = \alpha_0 + \alpha_1 older_i + \alpha_2 dist_i + \alpha_3 dist_i \cdot older_i + \pi X_i' + \gamma_b + \gamma_s + \gamma_w + e_i.$$

The corresponding second stage equation is

$$y_i = \beta_0 + \beta_1 \hat{sa}_i + \beta_2 dist_i + \beta_3 dist_i \cdot older_i + \delta X_i' + \gamma_b + \gamma_s + \gamma_w + u_i.$$

The running variable $dist_i$ denotes the distance between a respondent's month of birth and the school entry cutoff; it is measured in months and takes on integer values between -5 and 6. Note that the inclusion of quadratic trends would be problematic in the context of this analysis because the running variable has a small number of values. The results in Table 2.6 are in line with the main results. Overall, the various robustness analyses confirm the main results.

Next, we address the degree of representativeness of the causal long-term effects. The implemented fuzzy regression discontinuity design identifies the local average treatment effect, which is the causal effect for the subgroup of compliers, that is, persons who change their behavior in compliance with the school entry rule. Table 2.7 shows that 36 percent of our sample and 40 percent of the treated respondents are compliers.[21] The ratio of the likelihood that a complier has a certain characteristic and the general likelihood that a respondent has the same characteristic is close to one for the analysis' predetermined covariates. Thus, the group of compliers is similar to the entire sample with respect to the analysis' predetermined covariates. This similarity indicates that the estimated local average treatment effect could be representative of the entire

[21] To characterize compliers relative to the entire sample, we adopted the methodology as explained in Angrist and Pischke (2009).

sample.

2.6 Mechanisms

In this section, we investigate potential mechanisms through which school starting age might affect smoking behavior and health. First, drawing on the SOEP, we show suggestive evidence that school starting age affects smoking behavior and health through peers' age composition. Second, we discuss several studies on Germany that show that school starting age affects children's likelihood to enter a higher secondary school track and thus their school environment. In addition, we analyze the importance of the school environment mechanism for the effect of school starting age on smoking and health by including the respondent's school degree as a covariate in the regression. Third, we review studies analyzing the effect of school starting age on both grade retention and academic achievement because retained students might experience more stress and mental strain and thus are more likely to smoke. Fourth, we discuss the results of studies that analyze the effects of school starting age on labor market outcomes.

Peer effects are likely to be an important mechanism because school starting age affects the relative age of school peers. Manski (1993, 1995) points out that it is difficult to disentangle peer effects on individual behavior into 1) direct effects of peer behavior (endogenous effect) 2) effects of observed peer characteristics (contextual effect) and 3) effects of unobserved peer characteristics (correlated effect).[22] Most peer effect studies are unable to distinguish between these effects, despite the availability of exogenous variation in peer measures (Sacerdote, 2011). While the effect of school starting age on the relative age of school peers is mainly an endogenous effect, other mechanisms discussed in this section are a combination of endogenous, contextual, and correlated effects.

First, school starting age affects the relative age composition of peers and therefore the exposure to peer smoking in school. This mechanism can arguably be considered

[22] For instance, the smoking behavior of a person's reference group might affect his/her own smoking behavior (endogenous effect). Moreover, an individual's smoking behavior may be influenced by the observed socioeconomic status of the reference group (contextual effect). However, it might also be affected by the unobserved work environment that both the person and reference group share (correlated effect).

an endogenous effect of peer behavior. Figure 2.1 shows that the share of smokers tremendously increases with age during childhood and adolescence. While the share of smokers is close to 0 percent among 10–11-year-olds, it steadily increases with age to over 40 percent among 16–17-year-olds. Consequently, children who start school relatively young have peers and friends in school who are both older and more likely to smoke. Thus, analyzing the effects of school starting age on the relative age composition of respondents' friends is indicative of the degree of peer smoking in school.

Table 2.8 shows fuzzy regression discontinuity results in which the characteristics of the respondent's network of friends measured in adulthood—that is, the network of friends at the time of the SOEP interview—are regressed on school starting age. The estimates are an indication of the impact of school starting age on the characteristics of friends in school under the assumption that childhood friendships persist into adulthood. Whereas the number of friends is unaffected, both the average age and relative age of friends are significantly affected by school starting age.[23] Individuals who started school relatively young are more likely to have older friends later in life and therefore have increased exposure to smoking in school through older classmates and friends.[24]

Second, school starting age affects a child's likelihood to attend specific school types in secondary education in Germany. Jürges and Schneider (2007), Mühlenweg and Puhani (2010) and Dustmann et al. (2016) find that students who are relatively young at the start of primary school are less likely to attend higher secondary school tracks. Figure 2.2 shows that the share of smokers in low-track schools is about 5 percentage points higher than medium-track schools and about 11 percentage points higher than high-track ones. Students in low-track schools are therefore more exposed to peer smoking.

Moreover, students in low-track schools are subject to worse contextual and correlated school and background characteristics than those in higher track schools. According to the German Health Interview and Examination Survey for Children and Adolescents (Hölling et al., 2012), 8.6 percent of students in low-track schools have

[23]The relative age of friends is calculated by dividing the average age of friends by the respondent's own age.

[24]The results are robust to the use of all available months in the estimation with linear trends (see Table 2.15).

at least one parent with an upper secondary school degree compared to 45.7 percent students in high-track schools. Furthermore, Dustmann et al. (2016) show that the number of hours taught, teaching intensity and learning goals considerably differ between school tracks. Jürges et al. (2011) show that the increase in the number of high-track schools in post-war Western Germany reduced the rate of smokers through an increase in education—a result that highlights the importance of both years of education and school environment on smoking behavior.

In Table 2.9, we include the respondent's highest secondary school degree as a covariate in the estimation to gauge the importance of school environments as a mechanism.[25] In the two-month window, the effect of school starting age on smoking becomes statistically non-significant. Although the point estimate remains sizable, it decreases in absolute size from -0.013 to -0.009. Moreover, the effect on smoking remains statistically significant in the four-month window. The effect of school starting age on physical health becomes statistically non-significant in the four-month window, but remains statistically significant in the two-month window. The effects on both self-rated health measures remain statistically significant but those on mental health remain statistically non-significant. The statistically significant point estimates for the main specifications decrease in absolute size between 13 percent for physical health and 31 percent for smoking behavior. Thus, school environments are a relevant mechanism, although they do not appear to be the main mechanism through which school starting age affects smoking behavior and health.

Third, school starting age might affect the likelihood of grade retention. Eide and Showalter (2001), Elder and Lubotsky (2009) and Bernardi (2014) find that an increase in kindergarten or school starting age lowers the risk of grade retention in the United States and France. However, Elder and Lubotsky (2009) show that kindergarten starting age increases the likelihood of grade retention mainly in the first and second grade.[26] For Germany, Fertig and Kluve (2005) find that school starting age has no effect on the likelihood of an individual repeating a grade in school.

[25]For respondents who had not yet finished their secondary education, we included their current school type as a covariate.

[26]Elder and Lubotsky (2009) reveal that a one-year increase in kindergarten entry age decreases the likelihood of grade retention by 13.1 percentage points in the first and second grade and by 15.5 percentage points in any grade in the first eight years of schooling.

Thus, school starting age might affect grade retention only through the likelihood of repeating a grade in primary school. In contrast, students begin smoking in secondary school, as shown in Figure 2.1. This result implies that grade retention is an unlikely mechanism through which school starting age affects the likelihood to smoke. In fact, grade retention should lower the risk of smoking among young school starters because it increases their relative age.

Fourth, school starting age might affect smoking behavior and health through labor market outcomes. Black et al. (2011) find that an increase in school starting age lowers short-term earnings in Norway; however, this effect disappears by the age of 30. Fredriksson and Öckert (2014) show that school starting age affects the timing of labor supply, but not prime-age earnings in Sweden. Dobkin and Ferreira (2010) find for California and Texas that school starting age has no impact on wages and employment probability. Dustmann et al. (2016) show that school starting age affects the likelihood of students of attending a specific type of secondary school track in Germany; however, there are no long-term effects of tracking on wages, labor force participation, unemployment and occupational choice. They attribute the absence of labor market effects to the flexibility of the German education system, which mitigates mis-tracking of students.

Overall, school starting age is likely to affect smoking behavior and health through the relative age composition of peers in school and school environment. By contrast, grade retention is unlikely to increase the risk of smoking because it is, if anything, affected by school starting age in early grades, while the incidence of smoking occurs in later grades. Labor market outcomes is also an unlikely mechanism through which school starting age affects long-term smoking behavior and health: while there is some evidence of marginal short-term effects, these effects rapidly disappear. Thus, peer effects are an important mechanism through which school starting age affects long-term smoking behavior and health.

2.7 Conclusion

In this study, we examine the long-term effects of school starting age on smoking behavior and health. Because parents may decide their children's school starting age strategically while considering, for example, the perceived child's school readiness, we implement a fuzzy regression discontinuity design to account for the endogeneity of school starting age. We use exogenous school entry rules, which are based on children's date of birth, as an instrument for the observed school starting age. Our results show that school starting age affects smoking behavior and health in the long term.

The fuzzy regression discontinuity design results show that a one-month increase in school starting age significantly reduces the long-term risk of smoking by about 1.3 percentage points and increases the long-term likelihood of reporting good or very good health by about 1.6 percentage points. These estimates imply that an increase in school starting age by 11 months—that is, comparing children who are born in consecutive months around the cutoff and comply with the school entry rule—reduces the risk of smoking by 14.3 percentage points and increases the likelihood of reporting at least good health by about 17.6 percentage points. In addition, controlling for the endogeneity of school starting age is important: the effects of school starting age on smoking behavior and health estimated using OLS are severely biased towards zero and are statistically non-significant.

Our study shows that the short-term effect of school starting age on adolescent smoking found in previous studies persists into adulthood. Furthermore, our results are qualitatively in line with the literature on the effects of peers on smoking. The results are, however, difficult to quantitatively compare owing to methodological differences. Argys and Rees (2008) find in their preferred OLS specification that female adolescents who were relatively young at school start are 4.1 percentage points more likely to smoke than female adolescents who were relatively old at school start. The point estimates from instrumental variable regressions, although statistically non-significant, imply that both males and females who were young at school start are 1 percentage point more likely to smoke. Studies that adopt school-based peer smoking measures to study the effects of peer behavior on adolescent smoking find large short-term effects. For instance, Gaviria and Raphael (2001) find that moving a high-school student from a school where no children smoke to one where 25 percent of children smoke increases the student's likelihood of smoking by 4 percentage points. Powell et al. (2005) show a much larger effect of 14.5 percentage points for the same thought experiment.

We further find that adults who started school relatively young are not only more likely to smoke but also less healthy. This health effect is driven by physical rather than mental health. Elder and Lubotsky (2009), Elder (2010), Evans et al. (2010) and Schwandt and Wuppermann (2016) find that children and adolescents who started school younger have a higher likelihood to be diagnosed with ADHD.[27] Black et al. (2011) report that 18-year-old Norwegian conscripts who started school at a young age are diagnosed with slightly worse mental health in their military medical inspection than those who began school at an older age. While these studies find short-term effects of school starting age on mental health measures, we do not find statistically significant long-term effects.

Similar to previous studies on peer effects, the causal estimates in this study are a combined effect of peer behavior, peer characteristics and school environment. We analyze and discuss important channels through which school starting age might affect smoking behavior and health to shed light on the mechanisms underlying our estimates.

First, students who start school young have older class and school peers. These young students are exposed to peer smoking earlier than those who start school when they are older, because the prevalence of smoking considerably increases with age during childhood and adolescence. We show that adults who started school relatively young have, on average, older friends than those who started school older. We interpret this result as suggestive evidence that younger students are influenced by older peers and friends.

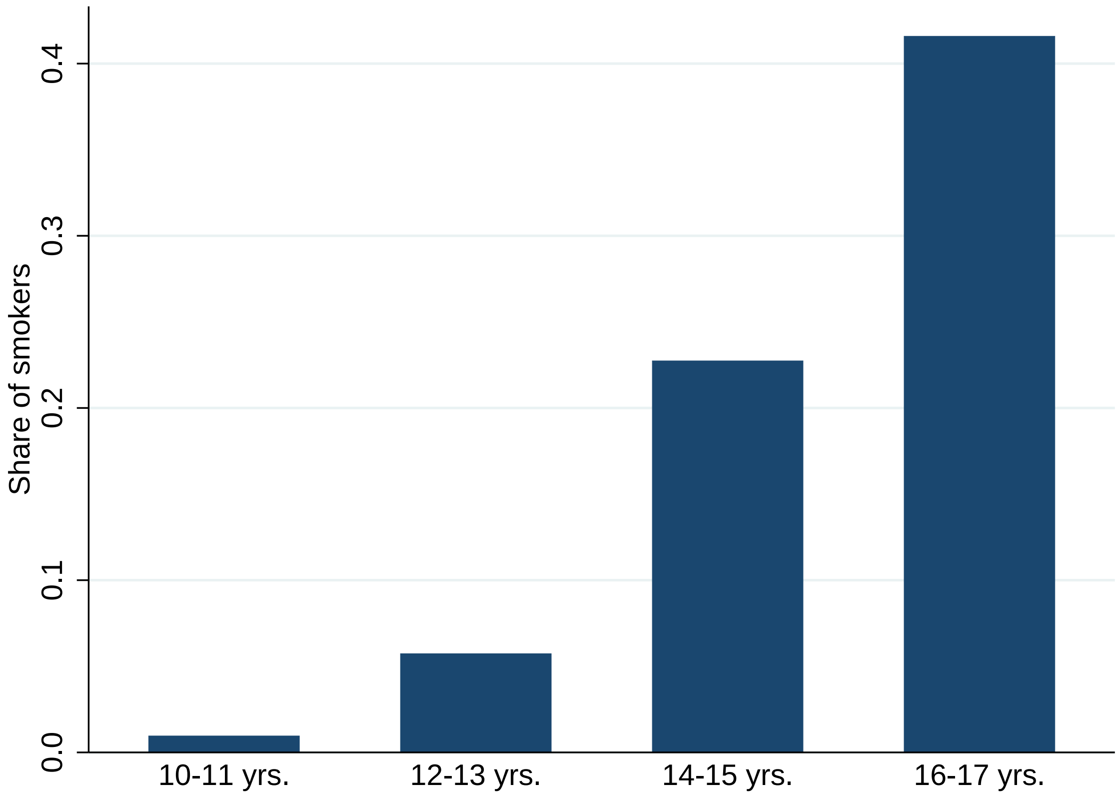
Second, Jürges and Schneider (2007), Mühlenweg and Puhani (2010) and Dustmann et al. (2016) show that students who start primary school relatively young are less likely to attend a higher secondary school track in Germany. At the same time, higher track schools have lower shares of smokers and more school resources. We show that school environment partially explains the effects of school starting age on smoking behavior and health. School environment does, however, explain only a small share of the effects

[27] These studies interpret this finding as evidence for misdiagnosis of ADHD.

of school starting age. Furthermore, we discuss that grade retention and labor market outcomes are unlikely mechanisms through which school starting age affects smoking behavior and health in the long term.

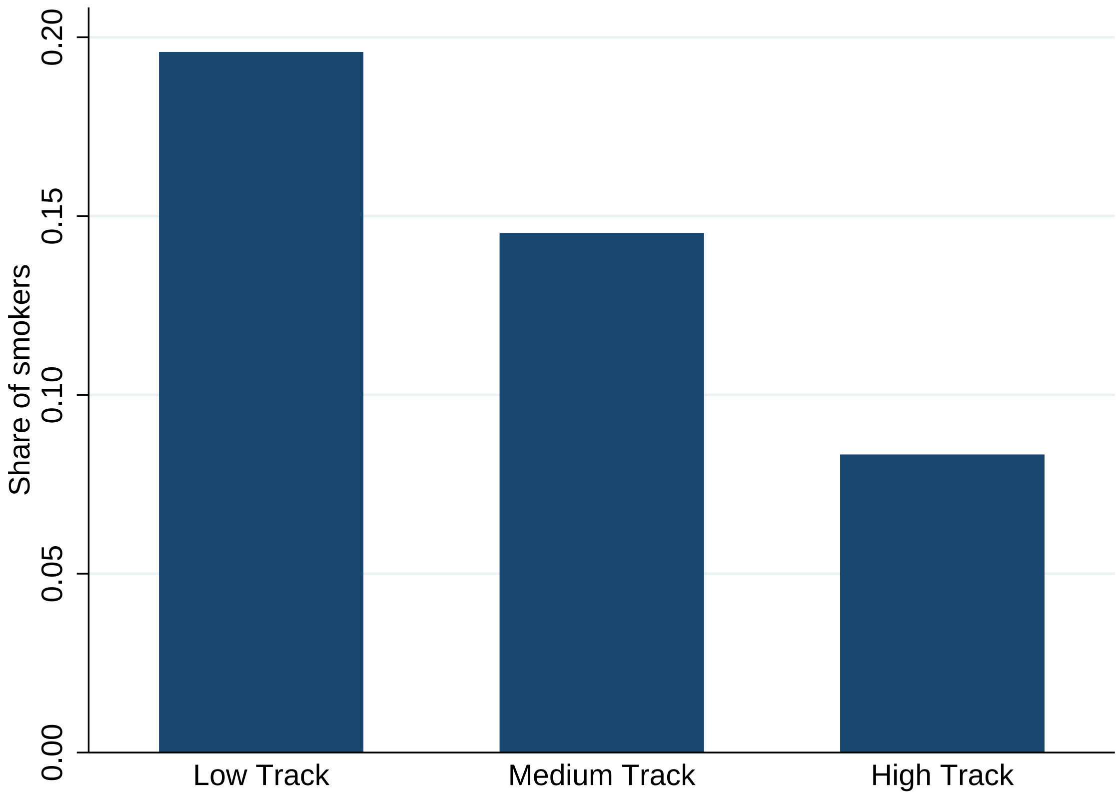
2.8 Figures and Tables

Figure 2.1: Share of smokers by age.



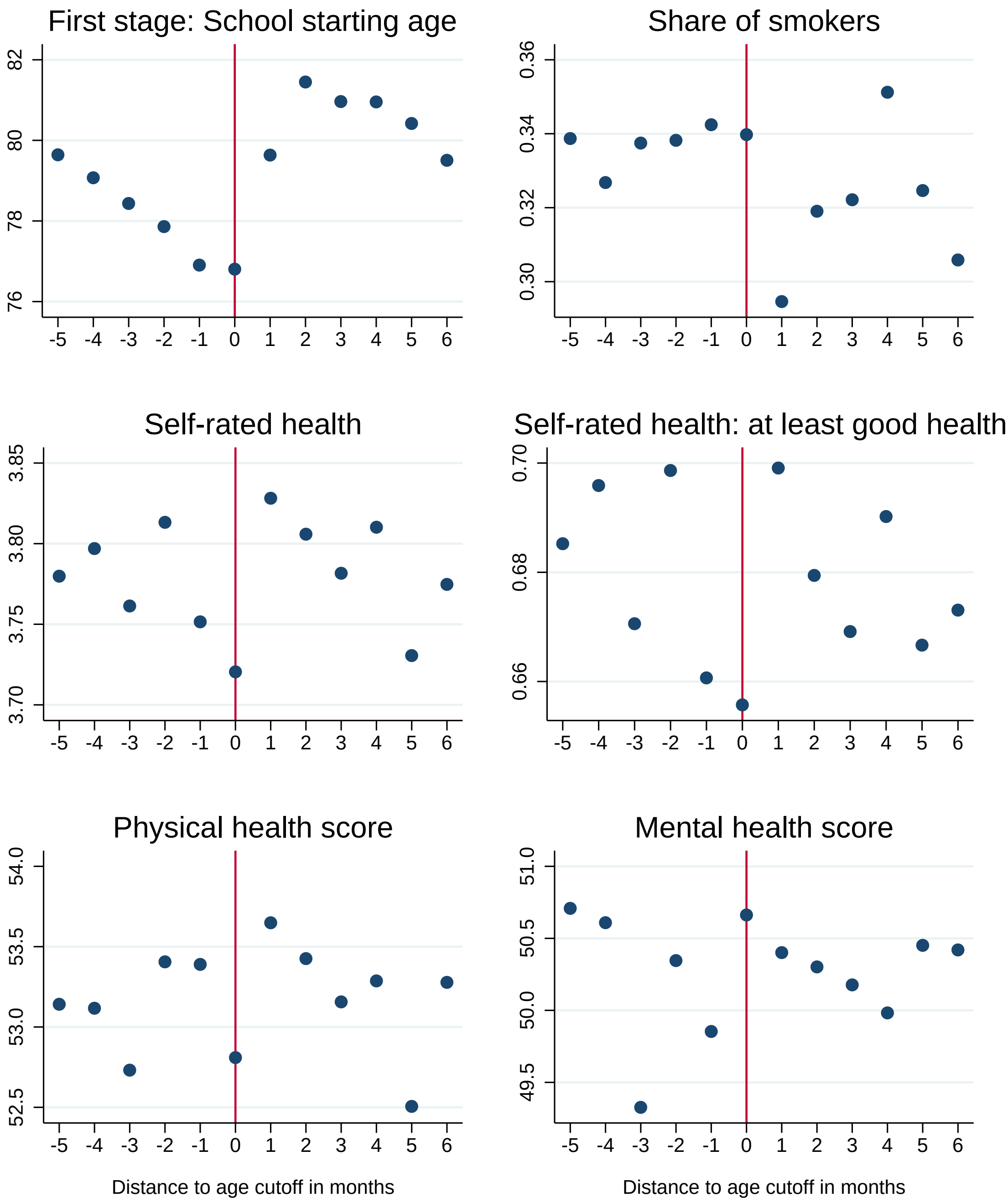
Notes: The figure shows a bar plot of the share of smokers by age. *Source*: German Health Interview and Examination Survey for Children and Adolescents (KiGGS) 2003–2006.

Figure 2.2: Share of smokers by type of secondary school.



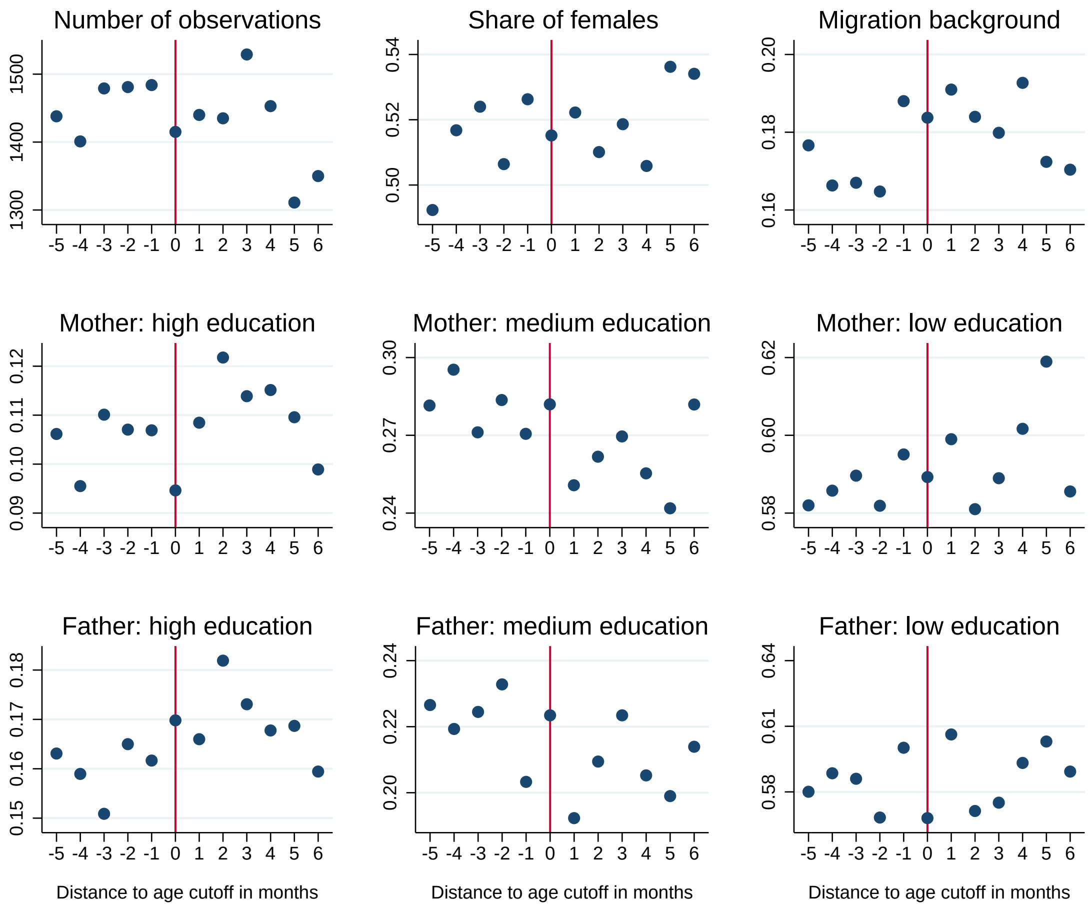
Notes: The figure shows a bar plot of the share of smokers among 12–15-year-olds for the three main secondary school types in Germany. 'Low track' refers to low-track schools (*Hauptschule*), 'medium track' is medium-track schools (*Realschule*) and 'high track' is upper-track schools (*Gymnasium*). *Source*: German Health Interview and Examination Survey for Children and Adolescents (KiGGS), 2003–2006.

Figure 2.3: Means of school starting age and outcomes.



Notes: The graphs show the average value of the variables for each value of the running variable. The running variable is defined as the distance between a respondent's birth month and the relevant school entry cut-off. The cut-off month, which includes the cut-off day, is assigned the value of zero. The cut-off day is always the last day of the respective cut-off month (e.g. June 30th). *Source*: German Socio-Economic Panel (SOEP).

Figure 2.4: Number of observations and means of covariates.



Notes: The graphs show the average value of the number of observations or the respective covariate for each value of the running variable. The running variable is defined as the distance between a respondent's birth month and the relevant school entry cut-off. The cut-off month, which includes the cut-off day, is assigned the value of zero. The cut-off day is always the last day of the respective cut-off month (e.g. June 30th). *Source*: German Socio-Economic Panel (SOEP).

Table 2.1: Descriptive statistics

	2 months			4 months		
	Before	After	p-value	Before	After	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel 1: Covariates					
School starting age	76.894	80.068	0.000	76.939	80.936	0.000
	(7.650)	(8.103)		(7.356)	(8.030)	
Female	0.523	0.532	0.683	0.524	0.529	0.755
	(0.500)	(0.499)		(0.500)	(0.499)	
Migration background	0.118	0.143	0.102	0.123	0.137	0.186
	(0.323)	(0.351)		(0.329)	(0.344)	
Highest school degree: mother						
High	0.097	0.110	0.340	0.101	0.116	0.134
	(0.296)	(0.313)		(0.302)	(0.321)	
Medium	0.281	0.249	0.116	0.275	0.254	0.138
	0.450	0.433		0.447	0.435	
Low	0.589	0.597	0.708	0.593	0.592	0.917
	(0.492)	(0.491)		(0.491)	(0.492)	
Highest school degree: father						
High	0.171	0.169	0.946	0.168	0.177	0.443
	(0.376)	(0.375)		(0.374)	(0.382)	
Medium	0.224	0.195	0.111	0.214	0.202	0.331
	(0.417)	(0.396)		(0.411)	(0.401)	
Low	0.565	0.599	0.129	0.582	0.585	0.884
	(0.496)	(0.490)		(0.493)	(0.493)	
n	973	956		1,982	1,952	

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	2 months			4 months		
	Before	After	p-value	Before	After	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel 2: Outcomes			
Smoking	0.324	0.281	0.051	0.331	0.294	0.019
	(0.468)	(0.450)		(0.471)	(0.456)	
n	866	833		1,765	1,684	
Self-rated health	3.720	3.858	0.001	3.739	3.844	0.000
	(0.935)	(0.889)		(0.944)	(0.885)	
Self-rated health:	0.654	0.708	0.013	0.659	0.698	0.010
at least good	(0.476)	(0.455)		(0.474)	(0.459)	
n	952	938		1,948	1,908	
SF12: Physical health	52.840	53.997	0.006	53.151	53.755	0.043
	(9.244)	(8.034)		(8.961)	(8.388)	
SF12: Mental health	50.776	50.466	0.518	50.482	50.471	0.974
	(9.872)	(9.743)		(9.814)	(9.638)	
n	853	821		1,740	1,651	
Number of friends	4.865	4.788	0.714	5.172	4.700	0.168
	(3.857)	(4.326)		(13.017)	(3.956)	
n	770	770		1,580	1,566	
Average age of friends	37.196	35.715	0.111	36.899	35.701	0.071
	(12.906)	(13.006)		(12.950)	(13.198)	
Relative age of friends	1.038	1.012	0.055	1.036	1.019	0.125
	(0.220)	(0.164)		(0.218)	(0.214)	
n	390	390		800	753	

Notes: The table shows the descriptive statistics of the estimation sample around the cutoff separately for the two-months window and the four-months window. It reports the variables' means and standard deviations in parentheses. Columns 3 and 6 report p-values of simple t-tests that test for differences in the variables' means before and after the school entry cutoff. Note that the sample size "n" varies across outcomes in Panel 2, because information on certain outcomes is not available for each respondent and survey wave.

Table 2.2: Instrument validity: pretreatment covariates as dependent variables.

	2 months		4 months	
	(1)	(2)	(3)	(4)
	Female			
School starting age	0.003	0.004	0.001	0.001
	(0.007)	(0.007)	(0.004)	(0.004)
	Migration background			
School starting age	0.008	0.005	0.004	0.001
	(0.005)	(0.004)	(0.003)	(0.002)
	Secondary school degree: mother			
	High			
School starting age	0.004	0.006*	0.004	0.002
	(0.004)	(0.004)	(0.003)	(0.002)
	Medium			
School starting age	−0.010	−0.003	−0.005	−0.003
	(0.007)	(0.005)	(0.004)	(0.003)
	Low			
School starting age	0.003	−0.006	−0.000	−0.000
	(0.007)	(0.005)	(0.004)	(0.003)
	Secondary school degree: father			
	High			
School starting age	−0.000	−0.002	0.002	0.000
	(0.005)	(0.004)	(0.003)	(0.002)
	Medium			
School starting age	−0.009	−0.004	−0.003	−0.001
	(0.006)	(0.005)	(0.003)	(0.003)
	Low			
School starting age	0.011	0.010*	0.001	0.001
	(0.007)	(0.005)	(0.004)	(0.003)
n	1,910	1,910	3,864	3,864
Birth year indicators	No	Yes	No	Yes
Federal state indicators	No	Yes	No	Yes
Further covariates	No	Yes	No	Yes

Notes: The table shows the second stage results of the fuzzy regression discontinuity design where predetermined covariates are regressed on school starting age. Further covariates include: female, migration background, highest degree of the father, and highest degree of the mother. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.3: Fuzzy RDD: smoking behavior and health.

	2 months			4 months		
	(1)	(2)	(3)	(4)	(5)	(6)
			Smoking behavior			
School starting age	-0.014^{*}	-0.013^{*}	-0.013^{**}	-0.009^{**}	-0.009^{**}	-0.009^{**}
	(0.007)	(0.007)	(0.007)	(0.004)	(0.004)	(0.004)
1st stage: F-Statistic	65.7	77.1	78.2	220.6	239.2	246.2
n	1,699	1,699	1,699	3,449	3,449	3,449
			Self-rated health			
School starting age	0.044^{***}	0.043^{***}	0.042^{***}	0.026^{***}	0.025^{***}	0.025^{***}
	(0.014)	(0.013)	(0.013)	(0.008)	(0.007)	(0.007)
1st stage: F-Statistic	74.0	84.2	85.1	254.1	278.3	284.1
n	1,890	1,890	1,890	3,856	3,856	3,856
			Self-rated health: at least good			
School starting age	0.017^{**}	0.017^{**}	0.016^{**}	0.010^{**}	0.009^{**}	0.009^{***}
	(0.007)	(0.007)	(0.006)	(0.004)	(0.004)	(0.004)
1st stage: F-Statistic	74.0	84.2	85.1	254.1	278.3	284.1
n	1,890	1,890	1,890	3,856	3,856	3,856
			SF12: physical health score			
School starting age	0.374^{***}	0.362^{***}	0.364^{***}	0.155^{**}	0.148^{**}	0.148^{**}
	(0.143)	(0.129)	(0.126)	(0.077)	(0.071)	(0.069)
1st stage: F-Statistic	63.8	77.1	79.1	214.4	234.5	242.0
n	1,674	1,674	1,674	3,391	3,391	3,391
			SF12: mental health score			
School starting age	-0.100	-0.139	-0.144	-0.003	0.004	0.007
	(0.155)	(0.141)	(0.139)	(0.086)	(0.082)	(0.081)
1st stage: F-Statistic	63.8	77.1	79.1	214.4	234.5	242.0
n	1,674	1,674	1,674	3,391	3,391	3,391
Birth year indicators	No	Yes	Yes	No	Yes	Yes
Survey year indicators	No	Yes	Yes	No	Yes	Yes
Federal state indicators	No	Yes	Yes	No	Yes	Yes
Female	No	Yes	Yes	No	Yes	Yes
Migration background	No	No	Yes	No	No	Yes
Education father	No	No	Yes	No	No	Yes
Education mother	No	No	Yes	No	No	Yes

Notes: The table shows the second stage results of the fuzzy regression discontinuity design. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.4: Fuzzy RDD: excluding potentially misclassified respondents.

	2 months			4 months		
	(1)	(2)	(3)	(4)	(5)	(6)
			Smoking behavior			
School starting age	−0.016*	−0.015*	−0.016*	−0.010**	−0.009**	−0.009**
	(0.009)	(0.009)	(0.009)	(0.005)	(0.005)	(0.005)
1st stage: F-Statistic	55.0	53.7	54.6	191.3	192.8	199.3
n	1,399	1,399	1,399	2,854	2,854	2,854
			Self-rated health			
School starting age	0.064***	0.060***	0.058***	0.030***	0.027***	0.027***
	(0.018)	(0.017)	(0.016)	(0.008)	(0.008)	(0.008)
1st stage: F-Statistic	60.5	60.7	61.0	224.2	231.5	236.9
n	1,571	1,571	1,571	3,219	3,219	3,219
			Self-rated health: at least good			
School starting age	0.027***	0.024***	0.024***	0.012***	0.011***	0.011***
	(0.009)	(0.008)	(0.008)	(0.004)	(0.004)	(0.004)
1st stage: F-Statistic	60.5	60.7	61.0	224.2	231.5	236.9
n	1,571	1,571	1,571	3,219	3,219	3,219
			SF12: physical health score			
School starting age	0.361**	0.281*	0.283*	0.106	0.059	0.064
	(0.165)	(0.154)	(0.151)	(0.085)	(0.080)	(0.078)
1st stage: F-Statistic	52.2	53.5	55.1	184.4	187.2	194.0
n	1,373	1,373	1,373	2,795	2,795	2,795
			SF12: mental health score			
School starting age	−0.031	0.001	−0.009	0.075	0.107	0.110
	(0.190)	(0.184)	(0.182)	(0.100)	(0.098)	(0.097)
1st stage: F-Statistic	52.2	53.5	55.1	184.4	187.2	194.0
n	1,373	1,373	1,373	2,795	2,795	2,795
Birth year indicators	No	Yes	Yes	No	Yes	Yes
Survey year indicators	No	Yes	Yes	No	Yes	Yes
Federal state indicators	No	Yes	Yes	No	Yes	Yes
Female	No	Yes	Yes	No	Yes	Yes
Migration background	No	No	Yes	No	No	Yes
Education father	No	No	Yes	No	No	Yes
Education mother	No	No	Yes	No	No	Yes

Notes: The table shows the second stage results of the fuzzy regression discontinuity design excluding respondents who started school prior to 1964 who are thus at risk of being misclassified with respect to whether they are to the left or to the right of the cutoff. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.5: Fuzzy RDD: including all observations per respondent in the estimation, and sensitivity by age groups—2 months window.

	The sample includes							
	All ages		Age $\geq$ 30		Age $\geq$ 40		Age $<$ 60	
	1st obs.	All obs.	1st obs.	All obs.	1st obs.	All obs.	1st obs.	All obs.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Smoking behavior							
School starting age	-0.013^{**}	-0.012^{*}	-0.014^{*}	-0.017^{**}	-0.014^{*}	-0.018^{**}	-0.013^{*}	-0.012^{*}
	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.007)
1st stage: F-Statistic	78.2	58.9	49.3	39.2	45.1	35.7	75.5	59.6
n	1,699	5,460	1,114	3,685	783	2,654	1,633	5,291
	Self-rated health							
School starting age	0.042^{***}	0.031^{***}	0.021	0.026^{**}	0.028^{*}	0.028^{***}	0.036^{***}	0.027^{**}
	(0.013)	(0.010)	(0.014)	(0.011)	(0.015)	(0.011)	(0.013)	(0.011)
1st stage: F-Statistic	85.1	63.9	54.8	55.7	50.9	57.2	81.1	60.8
n	1,890	11,014	1,189	9,235	825	8,203	1,822	5,306
	Self-rated health: at least good							
School starting age	0.016^{**}	0.014^{***}	0.007	0.011^{**}	0.010	0.012^{**}	0.014^{**}	0.015^{***}
	(0.006)	(0.005)	(0.007)	(0.005)	(0.008)	(0.005)	(0.007)	(0.006)
1st stage: F-Statistic	85.1	63.9	54.8	55.7	50.9	57.2	81.1	60.8
n	1,890	11,014	1,189	9,235	825	8,203	1,822	5,306
	SF12: physical health score							
School starting age	0.364^{***}	0.321^{***}	0.368^{**}	0.334^{**}	0.506^{***}	0.371^{**}	0.289^{**}	0.294^{***}
	(0.126)	(0.110)	(0.154)	(0.133)	(0.177)	(0.150)	(0.122)	(0.107)
1st stage: F-Statistic	79.1	60.5	55.0	41.1	46.6	33.8	76.4	61.5
n	1,674	4,692	1,115	3,283	797	2,413	1,608	4,528
	SF12: mental health score							
School starting age	-0.144	-0.173	-0.217	-0.244^{*}	-0.212	-0.258^{*}	-0.166	-0.178
	(0.139)	(0.117)	(0.157)	(0.129)	(0.164)	(0.136)	(0.142)	(0.116)
1st stage: F-Statistic	79.1	60.5	55.0	41.1	46.6	33.8	76.4	61.5
n	1,674	4,692	1,115	3,283	797	2,413	1,608	4,528
Birth year indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey year indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Federal state indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Female	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Migration background	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Education father	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Education mother	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows the second stage results of the fuzzy regression discontinuity design restricting the sample to a two-months window around the school entry cutoff. Analogous to the main analysis, "1st obs." columns include only the first observation per respondent in the estimation. "All obs." columns include all available observations per respondent in the estimation. Robust standard errors in parentheses. The models including all waves use clustered standard errors at the level of respondents. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.6: Fuzzy RDD: all months and trends.

	No trend	General linear trend		Seperate linear trends	
	(1)	(2)	(3)	(4)	(5)
			Smoking behaviour		
School starting age	−0.008**	−0.008**	−0.006*	−0.008**	−0.007*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
1st stage: F-Statistic	312.6	246.3	275.2	218.2	243.6
n	10,590	10,590	10,590	10,590	10,590
			Self-rated health		
School starting age	0.013*	0.025***	0.023***	0.027***	0.024***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
1st stage: F-Statistic	333.4	287.7	316.2	256.7	281.5
n	11,784	11,784	11,784	11,784	11,784
			Self-rated health: at least good		
School starting age	0.003	0.009***	0.008**	0.009**	0.008**
	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)
1st stage: F-Statistic	333.4	287.7	316.2	256.7	281.5
n	11,784	11,784	11,784	11,784	11,784
			SF12: physical health score		
School starting age	0.076	0.161**	0.147**	0.187**	0.167**
	(0.067)	(0.073)	(0.066)	(0.077)	(0.069)
1st stage: F-Statistic	300.7	238.6	266.8	211.2	235.6
n	10,400	10,400	10,400	10,400	10,400
			SF12: mental health score		
School starting age	0.014	0.021	0.016	0.016	0.012
	(0.074)	(0.081)	(0.078)	(0.086)	(0.082)
1st stage: F-Statistic	300.6	238.1	266.8	210.3	235.6
n	10,400	10,400	10,400	10,400	10,400
Birth year indicators	No	No	Yes	No	Yes
Survey year indicators	No	No	Yes	No	Yes
Federal state indicators	No	No	Yes	No	Yes
Female	No	No	Yes	No	Yes
Education father	No	No	Yes	No	Yes
Education mother	No	No	Yes	No	Yes
Migration background	No	No	Yes	No	Yes

Notes: The table shows the second stage results of the fuzzy regression discontinuity design. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.7: Characterization of compliers.

	Share
Compliers in sample	0.36
Compliers among treated individuals	0.40
	Ratio
Female	1.03
Highest degree: mother	
High degree	1.13
Medium degree	1.16
Low Degree	0.94
Highest degree: father	
High degree	0.97
Medium degree	1.18
Low Degree	0.99
Migration Background	0.95

Notes: The treatment variable school starting age sa_i is dichotomized. The ratio is the likelihood that a complier has a certain characteristic divided by the likelihood that an individual has this certain characteristic: $\frac{P[x_i=1|Complier]}{P[x_i=1]}$.

Table 2.8: Fuzzy RDD: network of friends.

	2 months			4 months		
	(1)	(2)	(3)	(4)	(5)	(6)
			Number of friends			
School starting age	−0.025	−0.005	−0.009	−0.120	−0.107	−0.110
	(0.069)	(0.063)	(0.061)	(0.088)	(0.074)	(0.074)
1st stage: F-Statistic	55.2	63.5	66.6	198.8	215.7	224
n	1,540	1,540	1,540	3,146	3,146	3,146
			Average age of friends			
School starting age	−0.403	-0.256^{**}	-0.243^{**}	-0.309^{*}	-0.192^{**}	-0.193^{**}
	(0.250)	(0.115)	(0.112)	(0.170)	(0.083)	(0.081)
1st stage: F-Statistic	42.9	44.2	45.3	101.1	103.6	108.0
n	780	780	780	1,553	1,553	1,553
			Relative age of friends			
School starting age	-0.008^{*}	-0.008^{**}	-0.008^{**}	−0.005	−0.005	-0.005^{*}
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
1st stage: F-Statistic	42.9	44.2	45.3	101.1	103.6	108.0
n	780	780	780	1,553	1,553	1,553
Birth year indicators	No	Yes	Yes	No	Yes	Yes
Survey year indicators	No	Yes	Yes	No	Yes	Yes
Federal state indicators	No	Yes	Yes	No	Yes	Yes
Female	No	Yes	Yes	No	Yes	Yes
Migration background	No	No	Yes	No	No	Yes
Education father	No	No	Yes	No	No	Yes
Education mother	No	No	Yes	No	No	Yes

Notes: Family members and relatives are excluded. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.9: Fuzzy RDD: secondary school degree as mechanism.

	2 months		4 months	
	(1)	(2)	(3)	(4)
	Smoking behavior			
School starting age	-0.013^{**}	-0.009	-0.009^{**}	-0.006^{*}
	(0.007)	(0.007)	(0.004)	(0.004)
1st stage: F-Statistic	78.2	75.5	246.2	239.0
n	1,699	1,699	3,449	3,449
	Self-rated health			
School starting age	0.042^{***}	0.034^{***}	0.025^{***}	0.021^{***}
	(0.013)	(0.013)	(0.007)	(0.007)
1st stage: F-Statistic	85.1	83.9	284.1	273.4
n	1,890	1,890	3,856	3,856
	Self-rated health: at least good			
School starting age	0.016^{**}	0.012^{*}	0.009^{***}	0.007^{**}
	(0.006)	(0.006)	(0.004)	(0.004)
1st stage: F-Statistic	85.1	83.9	284.1	273.4
n	1,890	1,890	3,856	3,856
	SF12: physical health score			
School starting age	0.364^{***}	0.317^{**}	0.148^{**}	0.113
	(0.126)	(0.123)	(0.069)	(0.069)
1st stage: F-Statistic	79.1	79.8	242.0	236.9
n	1,674	1,674	3,391	3,391
	SF12: mental health score			
School starting age	-0.144	-0.140	0.007	-0.006
	(0.139)	(0.139)	(0.081)	(0.082)
1st stage: F-Statistic	79.1	79.8	242.0	236.9
n	1,674	1,674	3,391	3,391
Birth year indicators	Yes	Yes	Yes	Yes
Survey year indicators	Yes	Yes	Yes	Yes
Federal state indicators	Yes	Yes	Yes	Yes
Female	Yes	Yes	Yes	Yes
Migration background	Yes	Yes	Yes	Yes
Education father	Yes	Yes	Yes	Yes
Education mother	Yes	Yes	Yes	Yes
Education respondent	No	Yes	No	Yes

Notes: The table shows the second stage results of the fuzzy regression discontinuity design. It shows specifications that include the respondent's highest secondary school degree in columns 2 and 4. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

2.9 Appendix

Table 2.10: Average age of respondents by outcome.

	2 months			4 months		
	Before	After	p-value	Before	After	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
			Smoking			
Age at interview	36.8	37.0	0.764	36.8	36.4	0.473
	(13.7)	(14.0)		(13.7)	(14.0)	
n	866	833		1765	1684	
			Self-rated health			
Age at interview	36.1	36.0	0.958	36.1	35.7	0.440
	(13.9)	(14.1)		(13.8)	(14.1)	
n	952	938		1948	1908	
			SF12: physical and mental health scores			
Age at interview	37.4	37.7	0.716	37.4	37.1	0.603
	(13.5)	(13.8)		(13.4)	(13.7)	
n	853	821		1740	1651	
			Number of friends			
Age at interview	37.7	37.9	0.745	37.7	37.6	0.834
	(13.2)	(13.7)		(13.3)	(13.7)	
n	770	770		1580	1566	
			Average and relative age of friends			
Age at interview	36.3	35.7	0.527	36.1	35.5	0.370
	(12.3)	(13.0)		(12.3)	(13.0)	
n	390	390		800	753	

Notes: The table shows the respondents' mean age before and after the school entry cutoff at the time of the SOEP interview by outcome for both the two-months and the four-months window around the school entry cutoff. Columns 3 and 6 report p-values of simple t-tests testing for differences before and after the school entry cutoff.

Table 2.11: Availability of outcome variables in the SOEP.

Year	Smoking	Self-rated health	SF12	Number of friends	Average and relative age of friends
1992		X			
1994		X			
1995		X			
1996		X			
1997		X			
1998	X	X			
1999	X	X			
2000		X			
2001	X	X			
2002	X	X	X		
2003		X		X	
2004	X	X	X		
2005		X			
2006	X	X	X		X
2007		X			
2008	X	X	X	X	
2009		X			
2010	X	X	X		
2011		X		X	X
2012	X	X	X		
2013		X		X	

Table 2.12: OLS and fuzzy RDD results.

	Smoking behavior		Self-rated health		Self-rated health: at least good		Physical health		Mental health	
	2 months	4 months	2 months	4 months	2 months	4 months	2 months	4 months	2 months	4 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
OLS										
School starting age	-0.001	-0.001	0.001	0.001	0.001	0.000	-0.006	0.008	-0.002	-0.009
	(0.001)	(0.001)	(0.003)	(0.002)	(0.001)	(0.001)	(0.028)	(0.019)	(0.031)	(0.022)
Fuzzy RDD: 2nd stage										
School starting age	-0.013^{**}	-0.009^{**}	0.042^{***}	0.025^{***}	0.016^{**}	0.009^{***}	0.364^{***}	0.148^{**}	-0.144	0.007
	(0.007)	(0.004)	(0.013)	(0.007)	(0.006)	(0.004)	(0.126)	(0.069)	(0.139)	(0.081)
Reduced form										
Older	-0.044^{*}	-0.035^{**}	0.138^{***}	0.103^{***}	0.055^{**}	0.038^{***}	1.235^{***}	0.598^{**}	-0.489	0.027
	(0.023)	(0.016)	(0.041)	(0.028)	(0.021)	(0.015)	(0.413)	(0.282)	(0.478)	(0.333)
Fuzzy RDD: 1st stage										
Older	3.346^{***}	4.043^{***}	3.330^{***}	4.119^{***}	3.330^{***}	4.119^{***}	3.387^{***}	4.053^{***}	3.387^{***}	4.053^{***}
	(0.378)	(0.258)	(0.361)	(0.244)	(0.361)	(0.244)	(0.381)	(0.261)	(0.381)	(0.261)
n	1,699	3,449	1,890	3,856	1,890	3,856	1,674	3,391	1,674	3,391

Notes: The table shows 1) OLS results, 2) 2nd stage results of the fuzzy RDD, 3) reduced form results, and 4) 1st stage results of the fuzzy RDD. All specifications include as covariates indicators for the respondent's gender, migration background, the highest school degree of the father, and the highest school degree of the mother. They further include indicators for birth year, federal state of school entry, and survey year. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.13: Further parental characteristics at the school entry cutoff.

	2 months			4 months		
	Before	After	p-value	Before	After	p-value
	(1)	(2)	(3)	(4)	(5)	(6)
Year of birth: mother	1943.4	1942.8	0.358	1943.1	1943.3	0.686
	(14.9)	(15.0)		(14.8)	(15.2)	
n	962	944		1,958	1,934	
Year of birth: father	1940.3	1940.2	0.792	1940.0	1940.5	0.310
	(15.6)	(15.4)		(15.5)	(15.5)	
n	959	939		1,946	1,922	
Occupational score: mother	58.3	60.5	0.206	59.2	60.5	0.286
	(27.3)	(29.3)		(27.0)	(28.5)	
n	575	542		1,131	1,117	
Occupational score: father	60.7	61.1	0.812	60.6	60.5	0.951
	(32.4)	(31.9)		(31.8)	(31.0)	
n	845	836		1,704	1,711	

Notes: The table shows descriptive statistics of parental characteristics, which are not included in the estimation, at the cutoff for the estimation sample. It reports the variables' means before and after the school entry cutoff, and standard deviations in parentheses. Columns 3 and 6 report p-values of simple t-tests testing for differences in the variables' means before and after the school entry cutoff. Note that sample sizes vary across outcomes. The number of individuals in the last row and the variables' statistics are based on the sample for the outcome smoking behavior.

Table 2.14: Fuzzy RDD: including all observations per respondent in the estimation, and sensitivity by age groups—4 months window.

	The sample includes							
	All ages		Age ≥ 30		Age ≥ 40		Age < 60	
	1st obs.	All obs.	1st obs.	All obs.	1st obs.	All obs.	1st obs.	All obs.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Smoking behavior							
School starting age	−0.009**	−0.009**	−0.007	−0.009*	−0.008	−0.009*	−0.009**	−0.010**
	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)
1st stage: F-Statistic	246.2	146.0	151.1	97.6	119.8	81.8	238.7	144.1
n	3,449	10,968	2,208	7,350	1,538	5,247	3,315	10,630
	Self-rated health							
School starting age	0.025***	0.019***	0.019**	0.016**	0.027***	0.015**	0.021***	0.015**
	(0.007)	(0.007)	(0.009)	(0.007)	(0.010)	(0.007)	(0.007)	(0.007)
1st stage: F-Statistic	284.1	160.0	164.4	140.4	123.2	141.1	275.8	145.6
n	3,856	22,260	2,396	18,631	1,657	16,527	3,714	10,654
	Self-rated health: at least good							
School starting age	0.009***	0.008**	0.006	0.007**	0.008	0.007*	0.008**	0.007**
	(0.004)	(0.003)	(0.005)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)
1st stage: F-Statistic	284.1	160.0	164.4	140.4	123.2	141.1	275.8	145.6
n	3,856	22,260	2,396	18,631	1,657	16,527	3,714	10,654
	SF12: physical health score							
School starting age	0.148**	0.129*	0.146	0.140*	0.192*	0.144	0.101	0.107
	(0.069)	(0.067)	(0.090)	(0.085)	(0.104)	(0.098)	(0.069)	(0.067)
1st stage: F-Statistic	242.0	153.2	153.0	103.0	121.1	83.0	233.6	151.2
n	3,391	9,438	2,215	6,577	1,561	4,787	3,258	9,107
	SF12: mental health score							
School starting age	0.007	0.024	−0.045	−0.055	−0.077	−0.077	−0.014	0.026
	(0.081)	(0.075)	(0.097)	(0.087)	(0.107)	(0.097)	(0.083)	(0.076)
1st stage: F-Statistic	242.0	153.2	153.0	103.0	121.1	83.0	233.6	151.2
n	3,391	9,438	2,215	6,577	1,561	4,787	3,258	9,107
Birth year indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey year indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Federal state indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Female	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Migration background	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Education father	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Education mother	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows the second stage results of the fuzzy regression discontinuity design restricting the sample to a four-months window around the school entry cutoff. Analogous to the main analysis, "1st obs." columns include only the first observation per respondent in the estimation. "All obs." columns include all available observations per respondent in the estimation. Robust standard errors in parentheses. The models including all waves use clustered standard errors at the level of respondents. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.15: Fuzzy RDD: network of friends—all months and trends.

	No trend	General linear trend		Separate linear trends	
	(1)	(2)	(3)	(4)	(5)
			Number of friends		
School starting age	−0.082	−0.067	−0.069	−0.064	−0.067
	(0.052)	(0.066)	(0.064)	(0.064)	(0.062)
1st stage: F-Statistic	288.6	217	240.9	194.2	214.7
n	9,595	9,595	9,595	9,595	9,595
			Average age of friends		
School starting age	−0.184	−0.340**	−0.182**	−0.326**	−0.168**
	(0.145)	(0.156)	(0.075)	(0.164)	(0.078)
1st stage: F-Statistic	146.9	119.9	127.3	108.5	114.7
n	4,659	4,659	4,659	4,659	4,659
			Relative age of friends		
School starting age	−0.000	−0.004*	−0.005**	−0.004	−0.004*
	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)
1st stage: F-Statistic	146.9	119.9	127.3	108.5	114.7
n	4,659	4,659	4,659	4,659	4,659
Birth year indicators	No	No	Yes	No	Yes
Survey year indicators	No	No	Yes	No	Yes
Federal state indicators	No	No	Yes	No	Yes
Female	No	No	Yes	No	Yes
Migration background	No	No	Yes	No	Yes
Education father	No	No	Yes	No	Yes
Education mother	No	No	Yes	No	Yes

Notes: Family members and relatives are excluded. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Chapter 3

Timing of Early School Tracking and Educational Paths

3.1 Introduction

Individuals' educational attainment and skills are not merely important for their income and employment but are also related to health status and social trust (Knack and Keefer, 1997; Blundell et al., 2000; Boockmann and Steiner, 2006; Grossman, 2006). Although the view that education is an important policy variable is widely accepted, how to promote educational attainment and skill formation is still controversial. One contentious issue is whether individuals should be tracked according to their ability.[1] Proponents of tracking argue that tracking makes teaching more efficient due to more homogeneous classes and that students who perform well do not suffer from the negative externalities of poorly performing students (Lazear, 2001). Opponents of tracking claim that intergenerational educational immobility in education is reinforced by tracking, students might be mis-tracked due to an imperfect signal of the innate ability of a student, and poorly performing students do not benefit from the positive externalities of students who perform well.[2]

Early tracking (before grade 7) regimes are particularly interesting, because (i) track choices influence schooling investments and (ii) early investments are especially important for skill formation.[3] The track choice highly influences schooling investments, because it determines the school peers of the student, the content of the curriculum, schooling intensity, and teacher quality. Cunha et al. (2006) summarize the empirical

[1] In the US and Canada, individuals are tracked within the school, meaning that a student can be assigned to a high track in one subject and a low track in another subject. In Europe, individuals were or are tracked to different type of schools (Betts, 2011). This study analyzes tracking to different school types which is the more rigorous type of tracking.

[2] Using data from Florida public school students, Burke and Sass (2013) show that low-type students' test scores are positively affected by a higher share of middle-type students in the classroom.

[3] I define tracking before grade seven as early tracking and tracking after grade six as late tracking.

literature on skill formation and conclude that investments are not perfectly substitutable between different stages in life, and early investments are crucial for the skill formation of a person. Providing a model that reflects some of the empirical results, the authors give two different explanations why early investments have a relatively high effect on skill formation.[4] First, the impact of investments on the skill level of the next time period depends positively on the skill level of the current time period (dynamic complementarity), and, second, a higher current skill transmits to a higher skill in the next time period (self-productivity). Since early educational investments are crucial for skill formation, and tracking influences schooling investments, it is important to assess the impact of the variations in early tracking on educational paths.

Although several studies analyze the effect of tracking on education (e.g., Meghir and Palme, 2005; Malamud and Pop-Eleches, 2011; Hall, 2012) little is known about the effect of timing in tracking on educational paths in an early tracking system. A popular approach to estimate the impact of tracking is to exploit within-country variation due to tracking reforms across time and regions. The type of a reform can be differentiated into three categories: (i) a shift from early to late tracking, (ii) a variation in late tracking, and (iii) a variation in early tracking. Most studies use reforms that belong to the first two categories (i.e., they consider reforms which change tracking from early tracking to late tracking, such as from tracking after grade four to tracking after grade nine) (e.g., Pekkarinen et al., 2009), or reforms which change tracking in a late tracking system (such as from tracking after grade eight to tracking after grade ten) (e.g., Malamud and Pop-Eleches, 2011). Studies analyzing a shift in an early tracking system are scarce (e.g., Piopiunik, 2014). However, the impact of the reform likely depends on the category of the reform. In the case of a shift from early to late tracking, the role of decision maker likely transfers from the parents to the child. In the second case, the influence of a variation in late tracking will mainly depend on the behavioral response of the child. In contrast to these two cases, a variation in early tracking will probably not change the decision maker, and the impact of early tracking will primarily concern the behavioral response of the parents.

[4]The model does not imply that late interventions are not important at all. Late interventions are essential to reap the benefits of early interventions. However, without early interventions, late interventions are not effective.

This study exploits the 2004 abolishment of the orientation stage (OS), which is a school type that prepares students for their track choice, in the German federal state Lower Saxony.[5] Before the reform, students from Lower Saxony spent the first four school years in primary school, and then they attended two years of OS and chose a school track after grade six. After the reform, students enrolled into a school track directly after four years of primary schooling. The OS was designed to prepare students for the track choice, and the OS tracked students within the school starting in grade six. Students were separated according to their past school performances in math and English in grade five, and in the OS students did not have to fulfill specific requirements to proceed to the next grade.[6] After the reform, students now had to fulfill requirements in grades five and six to advance to the next grade in the school tracks. Thus, the reform shifted tracking from tracking at the end of grade six to tracking at the end of grade four, abolished tracking within the school, and introduced the possibility of grade repetition for grades five and six.

The objective of this study is to show how the timing of tracking in early tracking systems influences educational paths. I will show how the abolishment of the OS (i) affected the share of individuals in the academic track after the track decision, (ii) influenced the grade repetition rate for grades five and six, and (iii) altered the grade repetition rate for grades seven to nine for the academic track. A change in the grade repetition rate for grades seven to nine in the academic track due to the reform would provide suggestive evidence about the ability level of students who change their enrollment decisions due to the reform. This study particularly stands out from the related literature on two points. First, it analyzes the impact of a variation in early tracking; that seems to be especially important, because the tracking decision defines schooling investments, and early investments are crucial for skill formation. Second, since countries typically de-track their educational system, it is unusual to analyze a reform that led to a more rigorous tracking system, and it is difficult to assess the impact of a shift to a more rigorous tracking system by using former studies.

Using administrative data and difference-in-differences estimations (DD), I show

[5] Although other German federal states changed their tracking system over the past several years, this paper only considers the change in tracking in Lower Saxony, because changes in tracking in the other federal states coincided with other educational reforms in these federal states.

[6] However, parents could make a proposal to retain the children in the latest grade.

that the reform increased the share of individuals in the academic track after the track decision by 3.4 percentage points (11.1 percent). Compared to the related literature, this result is surprising, because the majority of studies show that de-tracking promotes educational attainment. This difference in the results shows that the specific institutional change does indeed matter. Since students usually did not have to repeat a grade in the OS, I expect an increase in grade repetition for grades five and six. The results confirm this expectation. Furthermore, the results show that grade repetition due to the reform also increased for the academic track. This increase suggests that students who chose the academic track because of the reform have a lower ability than other students in the academic track. Although the estimates reveal that the abolishment increased the enrollment in the academic track for females more than for males, the males' grade repetition rate seems to be similar or even more affected than the females' grade repetition rate.

This paper is organized as follows. Section 3.2 provides a review of the related literature, and Section 3.3 presents the institutional background. Section 3.4 describes the administrative data set and offers graphic evidence. Section 3.5 explains the empirical estimation approach. Section 3.6 discusses potential threats to the identification strategy. Section 3.7 shows the results, and Section 3.8 presents various robustness checks. Section 3.9 concludes the paper.

3.2 Literature Review

Table 3.1 provides a short overview of the related literature. Many studies exploit changes in the tracking system within a country in order to estimate the impact of tracking on educational paths. For example, Malamud and Pop-Eleches (2011) study the effect of de-tracking in a late tracking regime by using an educational reform in Romania in 1973 and a regression discontinuity design. The reform changed tracking from tracking after eight school years to tracking after ten years. The authors show that the reform increased the likelihood of students' eligibility for university enrollment. In addition, they find that the majority of the individuals affected were from poor and rural regions and from less educated families. However, the results do not show a significant effect from the reform on university attendance or graduation for these subgroups.[7] The finding that an increase in upper secondary schooling does not imply an increase in higher education is similar to the results of Hall (2012). She studies a pilot project that decreased the differences between the academic and vocational tracks of the upper secondary school system in Sweden (1987-1991). The pilot project introduced a three-year vocational track that supplemented the existing two-year vocational track and the three-year academic track. In contrast to the two-year vocational track, the three-year vocational track had more academic content, and the graduating students gained basic eligibility for university studies. Although her results show that the pilot project increased the educational attainment in upper secondary schooling of vocational students, there is no evidence that it affected educational attainment beyond upper secondary schooling. Meghir and Palme (2005) study another reform in Sweden (1962) and estimate its impact on educational outcomes. This reform abolished tracking students after grade six, increased compulsory schooling by one or two years, and introduced a nationally unified curriculum. They rely on the fact that the reform was implemented as a test at different time points in the municipalities before being introduced nationwide. Their results show that the reform increased schooling above the new compulsory level for high-ability individuals from low-skilled fathers. Sund (2013) analyzes how de-tracking within a school influences achievements in math and the probability of graduation from high school, also by using also a reform in Sweden in 1995. He shows that there is, on average, no impact from the reform on the average math grade or the probability of graduation. However, he finds that decreasing within-school tracking reduces the probability of failing in math for individuals from families with low education. Pekkarinen et al. (2009) use a comprehensive school reform in Finland (1972-1977) to estimate the impact of tracking on intergenerational income immobility. Although they do not estimate the impact of tracking on education directly, the study is still comparable to the previous studies, because education and income are closely related. Among other things, the reform changed tracking at the age of eleven to tracking at the age of sixteen. They exploit the staggered introduction of the reform in the municipalities and show that the reform reduced the intergenerational income

[7]The university places were restricted. This restriction may have hampered the reduction in inequality in university enrollment.

elasticity by 23 percent. Thus, their results suggest that de-tracking led to a decrease in inequality.

Dustmann (2004) studies the relationship between parents' educational background, educational achievements, and earnings in Germany. His results show that there is a high intergenerational immobility of educational achievements across generations and that educational differences translate into differences in earnings. Dustmann argues that Germany's strong immobility of educational achievements is a result of the early tracking system, because parents have a relatively strong influence on the choice of track compared to late tracking systems. However, he does not investigate exogenous variation in tracking, so the causal effect of early tracking on the impact of parents' educational background on their children's education is still questionable. Piopiunik (2014) studies the effect of tracking on test scores in Germany. He exploits an educational reform in Bavaria in 2000. Before the reform, schools allocated higher-ability students to the academic school track after grade four. The remaining students were kept together until the end of grade six and were then allocated to the medium or low school track. After the reform, schools allocated all students to one of the three school tracks after grade four. Therefore, the reform did not affect the choice of the academic track, but it did alter the choice between the two lower school tracks. In addition, students who do not choose the academic track spend two years more in a school track after the reform as compared to the time before the reform. The results of the study show that the reform had a negative impact on test scores of 15-year-old students from low and medium school tracks.

Overall, most studies find that de-tracking promotes education and that this affect is sometimes restricted to the individuals from disadvantaged groups. That especially disadvantaged groups benefit from de-tracking lowers inequality in schooling and income. Countries have often changed more than just the tracking system, so studies often show a combined effect. Because most studies consider reforms that analyze changes from early tracking to late tracking or de-tracking in late tracking regimes, there is little known about the effect of variation in early tracking on educational paths. Moreover, most studies consider reforms which imply a reduction in tracking.

The study that uses the most comparable reform to this study is by Piopiunik (2014).[8] However, in contrast to Piopiunik (2014), this study does not analyze the effect of tracking on test scores but rather on educational paths. In addition, he considers the low and medium school track, and I analyze how the shift in tracking changed the share of individuals in the academic track. In Germany, the difference between the low track and medium track is rather minor compared to the difference between the medium track and the academic track. Section 3.3 discusses the differences between school tracks.

3.3 Institutional Background

3.3.1 Educational system in Germany

Although each of the 16 federal states in Germany has discretion with regards to its educational system, the general educational structure is rather similar between the federal states. Figure 3.1 shows the educational system that is prevalent in the majority of the federal states. Usually, students start to attend primary school at the age of six or seven years and are allocated to three different school tracks, according to their past school achievements, after four years of schooling . The academic track is eight to nine years long, the medium track is seven years and the low track is five years. While the academic track enables students to enter higher education, the two lower school tracks prepare students for an apprenticeship in white- or blue-collar jobs. However, after finishing one of the two lower school tracks, students can change to a higher school track if their grades are sufficient to do so. Students can also enroll in a comprehensive school instead of the tracking system, but the comprehensive school sector is relatively small. Instead of allocating students to different school tracks, comprehensive schools track students within the school. In 2009, about 1.98 million students from grades seven to nine were enrolled in one of the three school tracks, and about 0.22 million students from grades seven to nine visited a comprehensive school.[9]

[8]Lange and Werder (2014) have a working paper that analyzes the introduction of the OS in Lower Saxony in the 70s. They find no effect of the reform (i.e., delaying tracking by two years) on educational outcomes for the average.

[9]Own calculation based on data from the German Federal Statistical Office. The enrollment numbers do not include students who visit a school type that combines the two lower school tracks.

The formal requirements for teachers, the curriculum, and the teaching intensity vary between the tracks. The minimum formal training for teachers from the academic track is one year longer than for teachers from the two lower school tracks, and the topics in the academic track are more advanced. Lastly, the academic track requires four more hours of schooling weekly than the two lower school tracks (Dustmann et al., 2016). As a result, enrolling in an academic track leads to higher schooling investments. Furthermore, these investments might be even reinforced by higher-ability peers in the academic track.

Teachers recommend a school track for the students before the choice has to be made. This recommendation should be based on past school performance and the personality of the student. In Lower Saxony and nine other federal states, the recommendation is not binding, and the parents can freely decide about the school track of their child. In the other six federal states, parents can usually pick the recommended school track or a lower school track without fulfilling further requirements; however if the parents pick a higher school track than was recommended, the child has to perform successfully on an entry exam or in test lessons.

Students can also change school tracks before finishing their school track, but such changes are rather rare. This is especially the case for a change to a higher track. For example, in 2009, about 2.9 percent of the grade eight students in the low track and 2.5 percent of the grade eight students in the medium track visited a higher school track during the previous year. Only 0.2 percent of the grade eight students in the academic track and 0.6 percent of the grade eight students in the medium track visited a lower school track during the previous year.[10]

3.3.2 Educational reform in Lower Saxony: The abolishment of the OS

Figure 3.2 shows the educational system in Lower Saxony before and after the abolishment of the OS in 2004. Before the reform, children in Lower Saxony went to the OS following four years of primary schooling. In the first year of the OS (i.e. in grade

In section 3.4 of this paper I discuss the federal states that combined the two lower school tracks.

[10]Own calculation based on data from the German Federal Statistical Office. See Schnepf (2002) for a more extensive description of school up- and downgrading in Germany.

five), students shared classes independent of their past school performance. In grade six, students were separated in math and English according to their grades in grade five. The school could decide whether it wanted to differentiate students by two or three different performance levels in these subjects. At the end of grade six, parents had to choose between the three school tracks (i.e., the parents usually made the track choice when the child was twelve years old). After the reform, children in Lower Saxony go to one of the three school tracks after four years of primary schooling, and therefore the parents instead have to choose a school track when their child is ten years old.

Usually, students have to fulfill grade requirements to proceed to the next grade. However, in the OS in Lower Saxony, these requirements did not exist. A committee decided whether the child had to repeat the grade, and only if parents made a proposal that their children should repeat the last grade. The committee consisted of teachers, representatives of the parents, and students. However, after the abolishment of the OS, students also had to fulfill certain requirements to proceed to the next grade in the school tracks in grades five and six. The required grades range from one (very good) to six (unsatisfactory), and a child can proceed to the next grade if he or she has at most one grade worse than four (sufficient). Further, grades worse than four can be equalized by grades better than four in other subjects to avoid grade repetition.

Table 3.2 displays the timing of the reform in Lower Saxony. Before 2004, every student in Lower Saxony picked a school track after grade six. In 2004, the tracking system changed, and so 2004 was the only year where three different cohorts (the school starting cohorts 1998, 1999 and 2000) chose a school track in the same year. The school starting cohort from 1998 chose a school track after four years of primary schooling and two years of OS, the school starting cohort from 1999 chose a school track after four years of primary schooling and one year of OS, and the school starting cohort from 2000 chose a school track after four years of primary schooling without attending the OS. After 2005, students opted for a school track after four years of primary schooling.

3.3.3 Further educational reforms in Germany

Table 3.3 summarizes the changes in Germany's educational system during the observation period. The majority of federal states reduced the length of the academic track by one year to reduce the graduation age, and this typically means that the academic track is now eight instead of nine years long after the reform.[11] Because the academic track lasts eight years, and the German name for the academic track is *Gymnasium*, the reform is often called the G8 reform. For the remainder of this study, I will also use the abbreviation "G8" for this reform. Between the years 2001 and 2007, 14 of 16 federal states shortened the academic track by one year.[12] Saxony and Thuringia did not change the length of the academic track, because their academic tracks were already one year shorter. Lower Saxony introduced G8 in 2004 (i.e., the introduction corresponds to the timing of the abolishment of the OS). Since other federal states have also introduced G8, it is possible to control for G8 in the analysis.

The federal states of Brandenburg (in 2005), Bremen (in 2005), Mecklenburg-West Pomerania (in 2006), and Schleswig-Holstein (in 2007) combined the low school track and medium school track into one school track. In Brandenburg, the new school track is called *Oberschule*, in Bremen it is *Sekundarschule*, and in Mecklenburg-West Pomerania and Schleswig-Holstein it is *Regionalschule*. In 2011, Lower Saxony introduced a new school type and called it *Oberschule*. The *Oberschule* in Lower Saxony offers schools the opportunity to mitigate the difference between the low and medium track. The schools have different alternatives in terms of how to organize the *Oberschule*, and the choice of the alternative determines the magnitude of the mitigation of the two lower school tracks.

In addition to Lower Saxony, the federal states of Saxony-Anhalt (in 2003) and Bremen (in 2004) moved from tracking after grade six to tracking after grade four. By contrast, Mecklenburg-West Pomerania (2006) and Hamburg (2010) delayed tracking by two years and track students after grade six. Section 3.4 shows how my analysis addresses the reforms that (i) combined the low and medium track or (ii) changed the timing of tracking in other federal states besides Lower Saxony.

[11] For the federal states that track after six years of schooling the length of the academic track changed from seven to six years.

[12] Rhineland-Palatinate did not introduce G8 in the whole federal state, but only in some selected schools.

3.4 Data, Selection, and Descriptive Evidence

3.4.1 German Federal Statistical Office data set

This study considers two outcomes: (i) the share of individuals in the academic track after the track decision and (ii) the grade repetition rate. I analyze the share of individuals in the academic track to show how the timing of tracking influences the track choice. The grade repetition rate is interesting, because it offers suggestive evidence as to who is affected by the timing of tracking. The German Federal Statistical Office provides information about the actual aggregated number of students and number of students in a class who repeat a school grade by year, federal state, grade, school type, and gender. However, both the number of students overall and the number of students repeating a grade are not available by school starting cohort. Instead, I calculate the school starting cohorts with the information of the year and attended grade.[13] For example, students who are in grade five at the end of 2006 constitute the school starting cohort 2002. This study considers the cohorts who started schooling between 1994 to 2005 to analyze the enrollment of students into the academic track and the school starting cohorts from 1990 to 2005 in the case of grade repetition. I do not use the school starting cohorts from 1990 to 1993 in the case of school enrollment, because the German Federal Statistical Office does not provide enrollment numbers separated by grade for the corresponding years.

I calculate the share of individuals in the academic track at grade k by dividing the number of students in the academic track in year t and grade k by the total number of students in year t and grade k. The number of grade repeaters at each grade as offered by the German Federal Statistical Office does not state how many students have to repeat the grade next year, but instead supplies data on how many students in a grade were in the same grade the year before. Therefore, to receive the grade repetition rate for grade k in each year, I divide the number of students who are grade repeaters for year $t+1$ and grade k (i.e., they attended grade k in year t and $t+1$) by the number of students of year t and grade k. One problem with this approach is that students are assigned to the wrong track if they leave the school track and repeat a

[13]The calculated school starting cohort might deviate from the actual school starting cohort, because students might repeat a school grade.

school grade in the same year. However, this is only a problem for grade repetition in a specific track and does not apply for grade repetition in general. The grade repetition rate measurement also may contain errors if students who repeat a grade do so in a different federal state.

The advantage of the data from the German Federal Statistical Office is that it covers every student in Germany, because schools have to provide the German Federal Statistical Office with the information about the number of students and the number of students who repeated a grade. The disadvantage of the data is that the only available socio-demographic information is the students' gender. The data set is well-suited to give answers about average effects or effects separated according to gender, but effect heterogeneity with respect to the family background cannot be studied.

3.4.2 Exclusion of years, federal states, and school tracks

The introduction of the *Oberschule* in 2011 in Lower Saxony makes it necessary to constrain the estimation sample. The impact of the introduction of the *Oberschule* on educational paths is not clear. Although other federal states had similar reforms in the last several years, it seems to be impossible to control for this reform in a sensible way. Because the reform was already discussed in the media by 2010, this study does not consider the years after 2009 (i.e., the school starting cohort 2005 is the latest school starting cohort in the sample).[14]

I exclude the federal states Brandenburg, Bremen, Mecklenburg-West Pomerania, and Schleswig-Holstein, because these federal states combined the low and medium track. Although from an organizational perspective the combination of the two lower school tracks did not affect the academic track, the reform nevertheless was likely to have influenced the decision to enroll in an academic track. Students who would have picked the medium track in a three-track system have now a higher incentive to enroll in the academic track in these federal states, because the alternative to the academic track is to be in a class with the low-achievement students. I also exclude Saxony-Anhalt, because Saxony-Anhalt simultaneously abolished the OS and made school recommendations binding for the students. Thus, the only two federal states

[14] The robustness section will show that the results are not sensitive to the inclusion of school cohort 2006 in the analysis.

that changed their tracking system and are still in the sample are Hamburg and Lower Saxony. However, the track change in Hamburg first affected the school starting cohort 2006, and the estimation sample only includes federal states up to the school starting cohort 2005.

This study shows how the abolishment of the OS impacted on the share of individuals in the academic track but not how the shares in the medium and low tracks are affected. The first reason to study only the effect on the share of individuals in the academic track is that the difference between the academic track and the other two tracks is large compared to the difference between the medium and low track (see Subsection 3.3.1). The second reason is that some federal states either do not have a low track or had a reform that affected only the two lowest school tracks (see Piopiunik, 2014). As a result, analyzing the effect of the abolishment of the OS on the share of individuals in the medium or low track would require the exclusion of some federal states from the estimations. Since some federal states have already had to be excluded for different reasons, a further reduction would lead to a very small sample of control states. The last reason is that, in recent years, federal states have started to mitigate the difference between the low track and medium track. Extreme examples are that Brandenburg (in 2005), Bremen (in 2005), Mecklenburg-West Pomerania (in 2006), and Schleswig-Holstein (in 2007) combined the low and medium tracks into one school track. Since it is difficult to control for this mitigation between the low and medium tracks, I only study the effect of the abolishment on the share of individuals in the academic track.

3.4.3 Descriptive evidence

Figure 3.3 shows the share of individuals in the academic track after the transition; the data is presented separately for students from Lower Saxony and the control states for the school starting cohorts 1994 to 2005. It shows that the share of individuals in the academic track is increasing over the school starting cohorts for both groups. In the pre-treatment period, the share is always higher in the control group as compared to Lower Saxony. When Lower Saxony abolishes the OS, the share in Lower Saxony overtakes the share in the control states. The share stays higher in Lower Saxony until the school starting cohort 2004. Therefore, the graph suggests that the reform increased the likelihood that students would enroll in the academic track. The figure also supports the common trend assumption of the DD approach, because the shares move almost parallel in the pre-treatment period.

The top panel of Figure 3.4 displays the grade repetition rate for grade five in Lower Saxony and the control states for the school starting cohorts from 1990 to 2005. For the pre-treatment cohorts, the grade repetition rate in grade five is on average about 0.9 percentage points lower in Lower Saxony than in the control states, and the gap increases over the cohorts in the pre-treatment period. For the first treated cohort, the share increases from about 0.5 percent to 2.5 percent in Lower Saxony, although it decreases for the control states. For the subsequent cohorts, the share of repeaters is almost constant in Lower Saxony and still decreases for the control states. The lower panel describes grade repetition for grade six. Once again, the share in the pre-treatment period is smaller in Lower Saxony and increases strongly for the treated cohorts. In both cases (i.e., grade repetition in grades five and six), the pre-treatment trends of Lower Saxony and the control states deviate. Overall, the graph suggests that the reform increased the risk of grade repetition in grades five and six.

Figure 3.5 displays the grade repetition rate for grades seven, eight, and nine for individuals in the academic track; the data is presented separately for Lower Saxony and the control states. For all three grades, the grade repetition rate in the pre-treatment period is lower in Lower Saxony than in the control states, but overtakes the control states in the first treatment period. The magnitude of the increase between pre-treatment and post-treatment cohorts seems to decrease by grade level. The pre-treatment trends between Lower Saxony and control states are rather similar for grade eight and nine. However, the trends vary for grade seven, when the share of repeaters increases significantly for the school starting cohort 1996. Here as well, the figure provides evidence that the reform increased the likelihood of grade repetition.

3.5 Empirical Strategy

The abolishment of the OS in Lower Saxony provides a quasi-experimental setting. This study applies DD estimations and uses federal states without tracking changes as the control group, while exploiting the variation in tracking policies over time and across federal states.

Equation 3.1 gives the estimation equation:

$$y_{sc} = \beta_0 + \theta abolishment_{sc} + \lambda_s + \gamma_c + \psi_s trend_c + \omega_s trend_c^2 + X_{sc}^T \beta + u_{sc} \tag{3.1}$$

where s and c index the federal states and cohorts, y_{sc} is either the share of individuals who chose the academic track at the time of the transition or the grade repetition rate, λ_s is a state fixed effect, γ_c is a cohort fixed effect, and $trend_c$ and $trend_c^2$ are state-specific cohort trends. X_{sc}^T is a vector for further controls on the federal state level. The vector contains the variables *gdp* to account for different economic developments across states, *expenditures/population* to represent the educational expenditure per capita in a federal state, and *common school* for the share of schools that offer comprehensive schooling. In recent years, almost all federal states have shortened the academic track by one year, so X_{sc}^T contains a dummy variable $G8$ to control for this reform to the academic track. The main variable of interest is *abolishment*; this variable is set at one if a cohort chooses a school track after four or five years of schooling and is otherwise zero. Therefore, θ is either the effect of the reform on the share of individuals who chose the academic track at the time of the transition or the grade repetition rate, depending on the application.

The DD method assumes that the change in tracking is not related to other unobservable factors that influence the educational outcome. This approach is less restrictive than estimations that solely exploit variation over time or geographical variation in tracking policies.[15] Furthermore, some tracking studies apply an international DD approach (e.g., Waldinger, 2007; Hanushek and Wößmann, 2006), but the cultural dif-

[15]Estimations that exploit variation over time in tracking policies require the stricter assumption that the regression model includes every time-varying factor that influences the educational outcome. In addition, methods that solely use the cross-federal state variation in tracking have to control for every relevant factor that varies across federal states. In both cases, it is unlikely that every relevant factor can be included in the analysis.

ferences between countries are greater than the cultural differences between regions within a country. Therefore, the common trend assumption of the DD approach is more likely to hold in a DD setup within a country.

3.6 Discussion of Potential Threats

One threat to the identification strategy is the selection of students. Before the reform, children in Lower Saxony could enroll in a school in another federal state to avoid the OS, and students from other federal states could enroll in a school in Lower Saxony to attend the OS. Because of moving costs, students' geographical selection is only a concern for students who live close to the border of Lower Saxony. This alternative is only available for a small fraction of students, because Lower Saxony is the second largest German federal state and is located at the edge of Germany (i.e., not every part of the border separates Lower Saxony from another federal state). In addition, one of the neighboring states (Bremen) also had an OS, making it improbable that students from either state would choose to cross the border merely because of a choice related to an OS. As a result, selection is unlikely to be an important issue in this study.

Another problem that might bias the estimates is measurement errors in the variables. This study uses administrative data, which covers every student in Germany, and so this data set should be much more reliable than survey data. However, the data set only provides the enrollment number in a school track for every school year, it does not give any information about the school starting cohort of a student. In the case of grade repetition, this is not a problem, because I obtain the same results if I use the grade repetition by year and apply year instead of cohort fixed effects.[16] However, it is necessary in the case of track choice to calculate the school starting cohorts and to introduce cohort fixed effects instead of year fixed effects to avoid an upward bias of the estimate. Table 3.4 illustrates why year fixed effects would bias the results. For every year in the pre-treatment period, the children in Lower Saxony are older than in the control states at the time of the track decision. Using federal state fixed effects, this study accounts for the initial age difference. However, the table shows that at the year

[16]For grade repetition I display the results by school starting cohort to use the same approach as in the case of track choice.

of the reform (2004), the students in Lower Saxony become relatively younger compared to the control group. This shift in age to a younger school starting cohort would lead to an upward bias of the treatment effect estimator if younger cohorts are more likely to choose an academic track and if I apply year effects instead of cohort fixed effects.[17] However, the inability to distinguish the different cohorts leads to a measurement error in the dependent variable, which can lead to biased and imprecise estimates (Wooldridge, 2010). The error leads to biased estimates if the expected measurement error depends on one of the control variables. In this application, it is conceivable that the measurement error is related to the reform variable (i.e., $abolishment_{sc}$), because before the reform children spend two years more in school before they choose a school track, and so there is a higher risk of grade repetition before the reform than after. However, the risk of grade repetition was especially low in the OS, because children did not have to fulfill grade requirements to proceed to the next grade. Therefore, the main problem of the measurement error is that the estimates might become imprecise.

Another potential threat to the identification strategy is confounding factors. Since federal states have significant discretion with regards to their educational system, educational reforms in the federal states might bias the results. To address this problem, I exclude federal states with major changes of their educational system and restrict the time period of the analysis. Furthermore, the robustness check shows how sensitive the results are to further exclusions of federal states and to variations of the time period. One major educational reform was the G8 reform, and because this reform was implemented at different time points in most federal states, I am able to control for it in the regressions. Nevertheless, the abolishment of the OS might correlate with other varying factors on the federal state level. I include variables at the federal state level to show whether the results change.

3.7 Results

Table 3.5 displays the regression results for the effect of the abolishment of the OS on the share of students who attend the academic track after the track decision. The first

[17]The robustness section shows how the estimate changes if year fixed effects are applied instead of cohort fixed effects.

model includes federal state and cohort fixed effects. It predicts a 4.3 percentage point increase in the share of students enrolled in the academic track due to the reform. The next three models successively add state-specific linear and quadratic cohort trends, the *G8* dummy variable and the controls *gdp*, *expenditures/population*, and *common school*. The estimate drops to 3.4 percentage points. According to the last and my preferred specification, the share of students in the academic track increases by 11.1 percent compared to the average enrollment in Lower Saxony before the abolishment (30.7 percentage points). The estimated impact is significant at the 1-percent level in all models. After adding state-specific linear and quadratic cohort trends, the estimates are robust to further changes of the model. That *gdp* and *common school* have a significant influence at the 5-percent level in the regressions and that the influence of these variables on the abolishment effect estimate is small suggests that the abolishment of the OS was not related to the development of the gdp or the development of common schools. That federal states with major educational reforms are excluded and that the time period is restricted might have contributed to the stability of the estimations. Estimating the last model (model four) separately for females and males, I find that the estimated impact of the abolishment of the OS on the share of individuals in the academic track is higher for females (with 3.8 percentage points) as compared to males (with 3 percentage points). Although the difference of the effects is not especially large, the difference is significant at the 10-percent significance level.[18]

Table 3.6 shows the estimated impact of abolishing the OS on the grade repetition rate. The estimated impact is positive in all regressions and changes only slightly when the covariates *G*8, *gdp*, *expenditures/population*, and *common school* are included. The models for grades five and six consider all students. The results predict an increase in the rate of grade repetition due to the abolishment of the OS of 1.8 percentage points for grade five and 2.4 percentage points for grade six. Compared to the average grade repetition rate before the abolishment, the likelihood of grade repetition increased by almost 150 percent for grade five and by over 490 percent for grade six. In both cases, the estimated abolishment effect is significant at the 1-percent significance level. In

[18]To test the difference of the effects, I estimated the effect for both groups in one fully interacted model. The result of the fully interacted model provides the same estimates as the separate regressions. Additionally, the fully interacted model shows whether the effect of the reform differs between males and females.

addition, the estimated effect for both grades is almost twice as large for males versus females and differs significantly between genders at the 1-percent significance level. For grade five (six), the estimated impact for males is 2.4 (3.2) percentage points and for females it is 1.3 (1.7) percentage points. The models for grades seven to nine only consider students from the academic track. Predicting an increase in grade repetition of 1.1 percentage points due to the abolishment for grade seven, the results show that this effect is significant at the 1-percent significance level. Again, the predicted impact is higher for males, with 1.7 percentage points, than for females, with 1 percentage point, and the difference in the effects is significant at the 1-percent level. For grade eight, the estimated impact drops to 0.5 percentage points on average, but is still significant at the 10-percent significance level. Moreover, the estimated impact is very similar and not statistically different between males and females. For grade nine, the estimated impact is not significant at conventional significance levels, but with 0.4 percentage points on average, it is still relevant for practical purposes. The table shows that the estimated likelihood of grade repetition increased by about 64 percent for grade seven, 16 percent for grade eight, and 13 percent for grade nine when compared to the average grade repetition rate before the abolishment of the OS.

3.8 Robustness

One concern is that one of the control states may drives the results. To show how sensitive the results are to the selection of control states, I successively exclude each of the control states and estimate the models again. Table 3.7 displays the estimated impact of the abolishment of the OS on enrollment in the academic track for these regressions. The table shows that the estimated impact of abolishment is significant at the 1-percent significance level in all estimations. Additionally, the point estimate is also robust to the exclusion of federal states. The estimations without Hesse and Saxony both reveal the highest predicted impact with 3.7 percentage points, and the regression without Hamburg gives the lowest estimated impact with 3 percentage points. The regressions for males and females with different control groups show similar deviations compared to the main model. Table 3.8 summarizes the regression results for the case of grade repetition. The results of the regressions deviate from the result of the main model by 0.4 percentage points at most. The estimates are always significant at the 1-percent significance level for grades five and six. For all regressions for grade seven, the abolishment of the OS has a significant influence at the 5-percent level. The choice of control states has the highest influence on the results for grade eight. Here, the significance varies between not significant and significant at the 1-percent level. For grade nine, the estimate is almost never significant at the 10-percent significance level. Overall, there is no evidence that one control state drives the results.

When Lower Saxony abolished the OS, the school starting cohort 1999 had already attended grade five, and therefore this school starting cohort had to choose a school track after grade five; it is the only school starting cohort to do so. However, the estimations in the main section treat the school starting cohort 1999 in the same way as all of the following school starting cohorts from Lower Saxony. To show how the first treated school starting cohort impacts the results, I exclude the school starting cohort 1999 from the estimation. Table 3.7 shows the impact of the abolishment on the share of students in the academic track after the track decision and demonstrates that the results stay almost unchanged. The estimated average effect is the same; the effect drops by 0.1 percentage points for females, and it increases by 0.2 percentage points for males. Furthermore, the change in the estimation does not affect the statistical significance of the abolishment effect. Table 3.9 displays the results of the estimated impact of the abolishment on the share of grade repetition without the school starting cohort 1999. The estimated impacts are almost the same for grades six, eight, and nine as compared to the main estimations. For grade five, the estimated impact increases by 0.3 percentage points to 2.1 percentage points, and for grade seven the estimated impact increases by 0.2 percentage points to 1.3 percentage points. The standard errors increase more for the higher grades, and it is only at grade eight that the statistical significance changes such that the abolishment effect is no longer significant at conventional significance levels.

Another concern is the choice of the observation period. School starting cohorts after 2005 are not included in the main analysis, because the school starting cohort 2007 was affected by another educational reform in Lower Saxony, and the discussion about the reform had already started one year earlier. Whether the school starting cohort 2006 was affected by this second reform is not clear. The last three columns of table 3.7 display the estimated impact of the abolishment on the share of students in the academic track when the school starting cohort 2006 is also included in the analysis.[19] The table reveals the same coefficients and statistical significance as the main estimations. Table 3.9 shows the estimated abolishment effect for grade repetition for grade five to seven when the school starting cohort 2006 is used.[20] The estimated effects change slightly for the whole sample by -0.1 to 0.2 percentage points, and the statistical significance remains unchanged. Overall, inclusion of the school starting cohort 2006 only barely affects the results.

The main analysis only includes the change in tracking in Lower Saxony. The federal states Bremen and Saxony-Anhalt also abolished the OS, and Hamburg and Mecklenburg-West Pomerania introduced the OS. Table 3.10 shows that the estimated impact of the abolishment of the OS on the share of individuals in the academic track increases, when Bremen and Saxony-Anhalt are included.[21] Furthermore, the table adds Mecklenburg-West Pomerania and Hamburg as well as a dummy variable for the introduction of the OS. The estimates reveal a negative impact of the introduction of the OS on the share of individuals in the academic track.[22] However, this effect is not significant for females at conventional significance levels. Using all sixteen federal states and/or the school starting cohort 2006 also has only a negligible effect on the estimates for the abolishment or introduction of the OS on academic school enrollment.

Another issue is the computation of clustered standard errors. Since this study uses a rather small number of clusters, the clustered standard errors might be biased. Cameron et al. (2008) suggest the method of wild clustered standard errors to address the issue of clustered standard errors with a small number of clusters. Indeed, in their simulation analysis, they show that wild clustered standard errors lead to p-values much closer to the true p-value versus using usual clustered standard errors. Table 3.11 shows that the estimated abolishment effect for the main specification becomes

[19]The regressions do not include the school starting cohort 2006 for Hamburg, because Hamburg introduced the OS for this school starting cohort.

[20]The analysis cannot include the school starting cohort 2006 for grade repetition in grade eight and nine, because grade repetition for this cohort is not included in the data set.

[21]See the figures 3.6 and 3.7 of the appendix for a graphic representation of the effects.

[22]See the figures 3.9 and 3.8 of the appendix for a graphic representation for the effects.

insignificant with wild clustered standard errors. However, using other federal states that introduced the OS (i.e., Bremen and Saxony-Anhalt), the estimations reveal that the effect of abolishment on school enrollment is at least significant at the 5-percent level even with wild clustered standard errors.

Normally, studies compare cohorts to evaluate the impact of tracking. However, the data set from the German Federal Statistical Office does not provide the school starting cohorts directly, but instead gives the enrollment numbers and grade repetition rate for each year and grade. I compare school starting cohorts, because this is the approach in related studies, and to compare years instead would likely lead to an upward bias of the reform effect for track enrollment. The first three columns of table 3.12 show the estimation results for track enrollment if the regression substitutes the cohort fixed effects, state-specific linear and quadratic cohort trends with time fixed effects, and state-specific linear and quadratic time trends. The estimated impact increases by 1.2 percentage points for the whole sample, 0.8 percentage points for females, and 1.4 percentage points for males. These results support the hypothesis that the approach by year instead of cohorts leads to an upward bias.

To further check the validity of the results, I apply placebo tests. The placebo tests only use the school starting cohorts before the abolishment of the OS, and the placebo dummy is set at one for the last three school starting cohorts for Lower Saxony in this reduced data set and is otherwise zero. The last three columns of table 3.12 display the estimated effect of the placebo for the share of individuals in the academic track. The estimate is almost zero overall, for females and for males. Furthermore, the placebo is always statistically insignificant. Table 3.13 shows that the placebo is almost zero and insignificant in the regressions for grade repetition for grades five, eight, and nine. However, the placebo is significant at the 10-percent significance level for grade six, although its estimated effect of the placebo is much smaller than the estimated abolishment effect in the main specification. The placebo is significant at the 1-percent level for grade seven, and the estimated impact is even higher than in the main estimation. As discussed earlier, Figure 3.4 shows that the common trend assumption very likely does not hold for grade seven. Grade repetition for grade seven in Lower Saxony jumps upwards for the three cohorts before the first treatment cohort, although it decreases for the control states. After this increase, it decreases in the next years until it increases once again for the first treatment cohort. For the three school starting cohorts after the jump, the share of grade repetition in grade seven for Lower Saxony and the control group moves almost parallel. To omit the influence of the jump in the estimation, I estimate the model for grade repetition in grade seven again, but I use only the three school starting cohorts after the jump for the pre-treatment period. Table 3.14 shows the estimates for this approach for three different specifications. The first three columns of the table follow the main specification, columns four to six exclude the first treated school cohort, and the last three columns include the school starting cohort 2006. In all specifications, the estimated abolishment effect is positive. The estimated effect for males is always significant at least at the 10-percent significance level and is of a similar magnitude as in the main estimation. In contrast, the effect for the whole sample and the effect for females both depend on the specification. It is only in the specification without the first treatment cohort that the effects are significant at the 5-percent level and of similar magnitude as in the main specification with all pre-treatment cohorts. The placebo demonstrates that the common trend assumption might not hold in every case of grade repetition. However, the change in the grade repetition rate for grade six is so extreme that it is unlikely that the estimated effect is purely spurious, although the estimate might not be unbiased. For grade seven, I restrict the pre-treatment period such that the pre-treatment trends of Lower Saxony and the control group move almost parallel. Models with the restricted pre-treatment period find similar results for grade seven as compared to the main section, depending on the specification.

3.9 Conclusion

This study examined the effect of variation in early tracking on educational paths by exploring a shift from tracking at the end of grade six to the end of grade four in Lower Saxony in 2004. Using administrative data from the German Federal Statistical Office and DD estimations, this study finds a positive and significant effect for the shift in the timing of tracking on the share of individuals in the academic track. The estimate reveals an increase of 3.4 percentage points for individuals in the academic track after the track decision. Compared to the pre-treatment share (30.7 percent), this increase translates to a rise of 11.1 percent, or about 3,000 more students in the academic track per year. In addition, grade repetition increased for all students for grades five and six, and the results further suggest that grade repetition also increased for grade seven and eight for students in the academic track.

Most tracking studies find that de-tracking leads to an increase in educational achievements, and therefore the results of this paper are not in line with the related literature. In contrast to other studies, this study analyzes variations in early tracking and a change to a more rigorous tracking system. The major reason that the results dramatically differ compared to previous studies might be that the analyzed reform is unusual. A major contribution of this study is that it shows that the effect of variation in early tracking systems might significantly differ from the effect of variation in late tracking systems or of changes from early to late tracking. There are several reasons that parents are more likely to choose the academic track for their children after grade four than after grade six. Teachers can change their recommendation behavior, students' school performance or aspirations might alter due to the reform, and the decision of the parents might also depend on the timing of the track decision even if these other two factors do not change. However, this study cannot show why the decision of the parents depends on the timing of the track decision. Future research is necessary to address this question as well as whether the higher share of individuals in the academic track also implies an increase in academic track degrees, enrollment in a university, and better labor market outcomes.

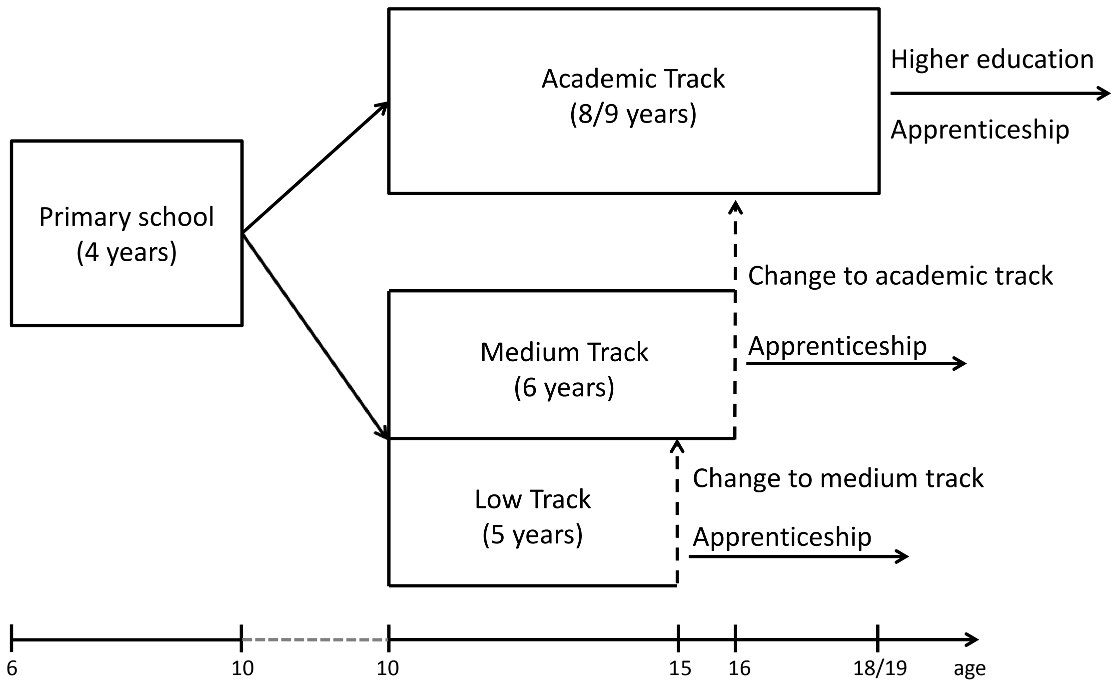
That grade repetition increased dramatically for grades five and six is a mechanic effect, because before the reform students did not have to fulfill grade requirements to proceed to the next grade. More interestingly, the abolishment of the OS also increased grade repetition for grades seven and eight in the academic track. This increase might indicate that the students who changed their track decision because of the reform have a lower ability level than students who always would have enrolled in the academic track. Although enrollment in the academic track increased more for females than males, grade repetition in the academic track is affected similarly or even slightly more for males. How should we assess higher grade repetition? On the one hand, spending per student increases and some of the future earnings are lost (OECD, 2011b) if grade repetition leads to an additional year in public school, and repeating a grade can lead to lower self-esteem due to stigmatization and the adjustment costs of a new peer group. On the other hand, retained students might benefit from the additional instruction time, and grade repetition probably increases their self-discipline.[23] The impact of grade repetition on student achievements is still disputed (Manacorda, 2012; Schwerdt and West, 2013).

Overall, this study shows that a shift to more rigorous tracking in an early tracking system can lead to a higher share of individuals in the academic track. Further, the results suggest that students who would have entered the non-academic track before abolishment, but now enter the academic track after abolishment have a lower ability on average than students who entered the academic track before abolishment.

[23] Another advantage of a school system with grade retention is the disincentive for students to perform too badly in school (Belot and Vandenberghe, 2011).

3.10 Figures and Tables

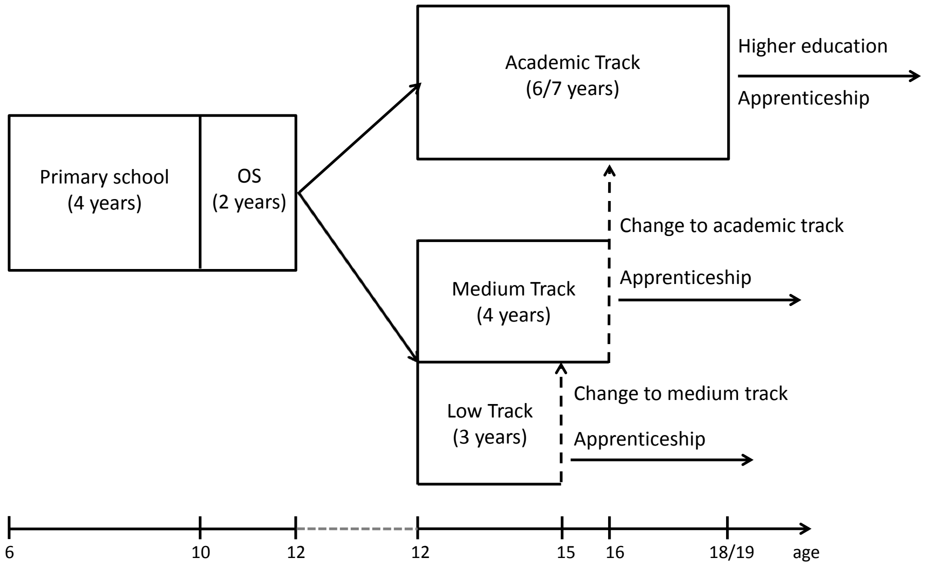
Figure 3.1: Educational system: Tracking after grade four



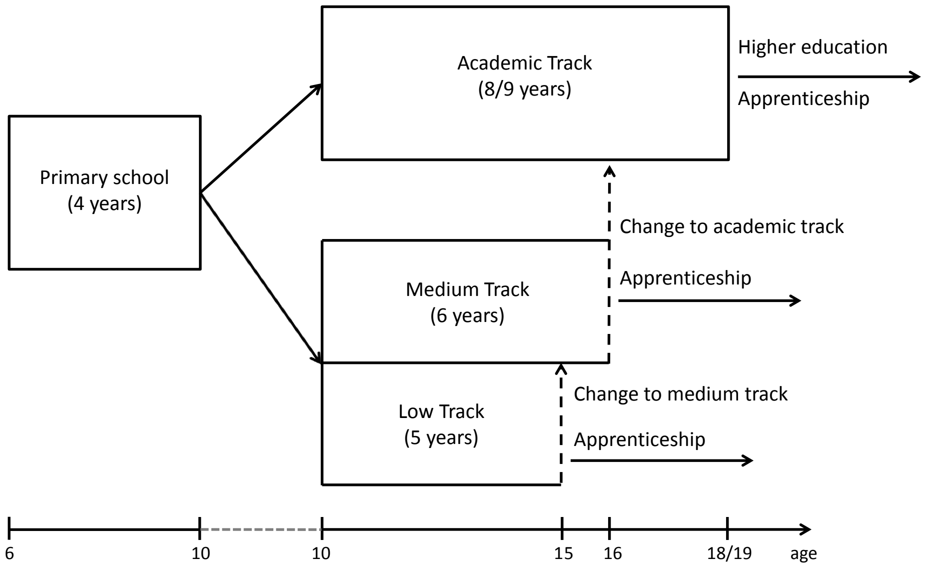
Notes: The graphs display the school system which most federal states in Germany apply. The years in brackets show how many years usually a school track takes. The arrows show an individual's opportunities after finishing a school track.

Figure 3.2: Educational System in Lower Saxony

(a) **Before the abolishment of the OS**



(b) **After the abolishment of the OS**



Notes: The years in brackets show how many years usually a school track takes. The arrows show an individual's opportunities after finishing a school track.

Figure 3.3: Effect of moving to early school tracking on track choice

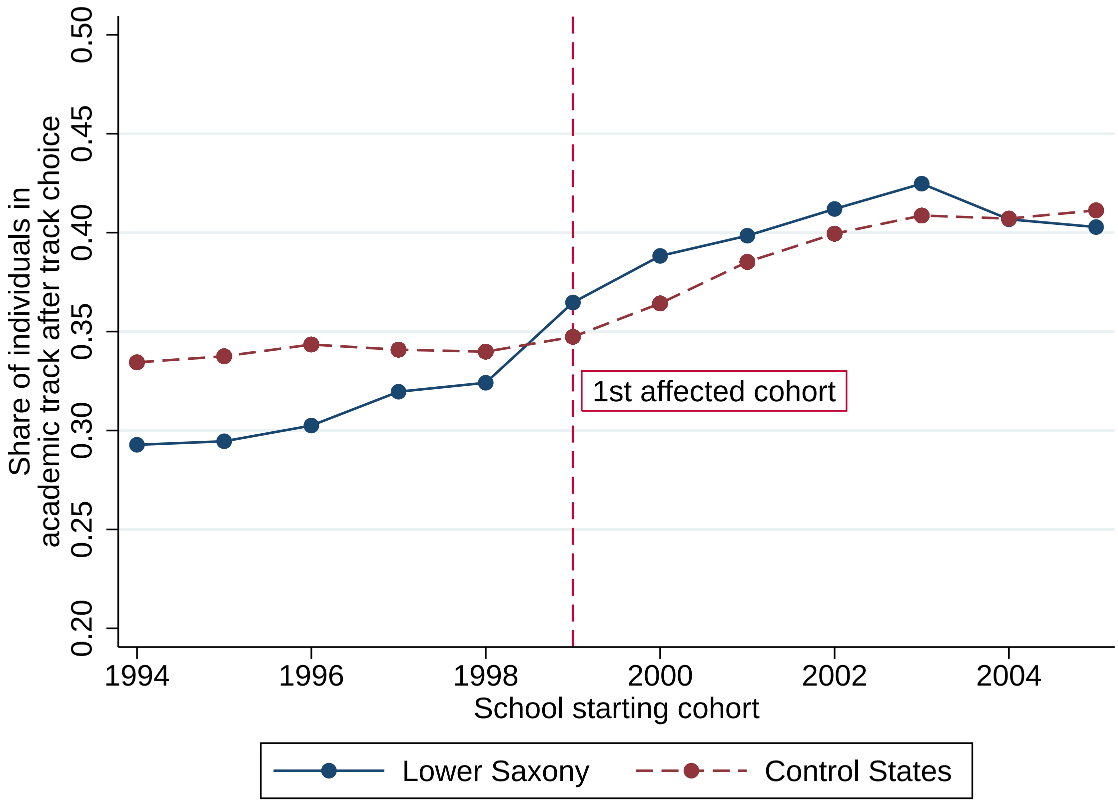


Figure 3.4: Effect of moving to early school tracking on grade repetition (grade 5-6)

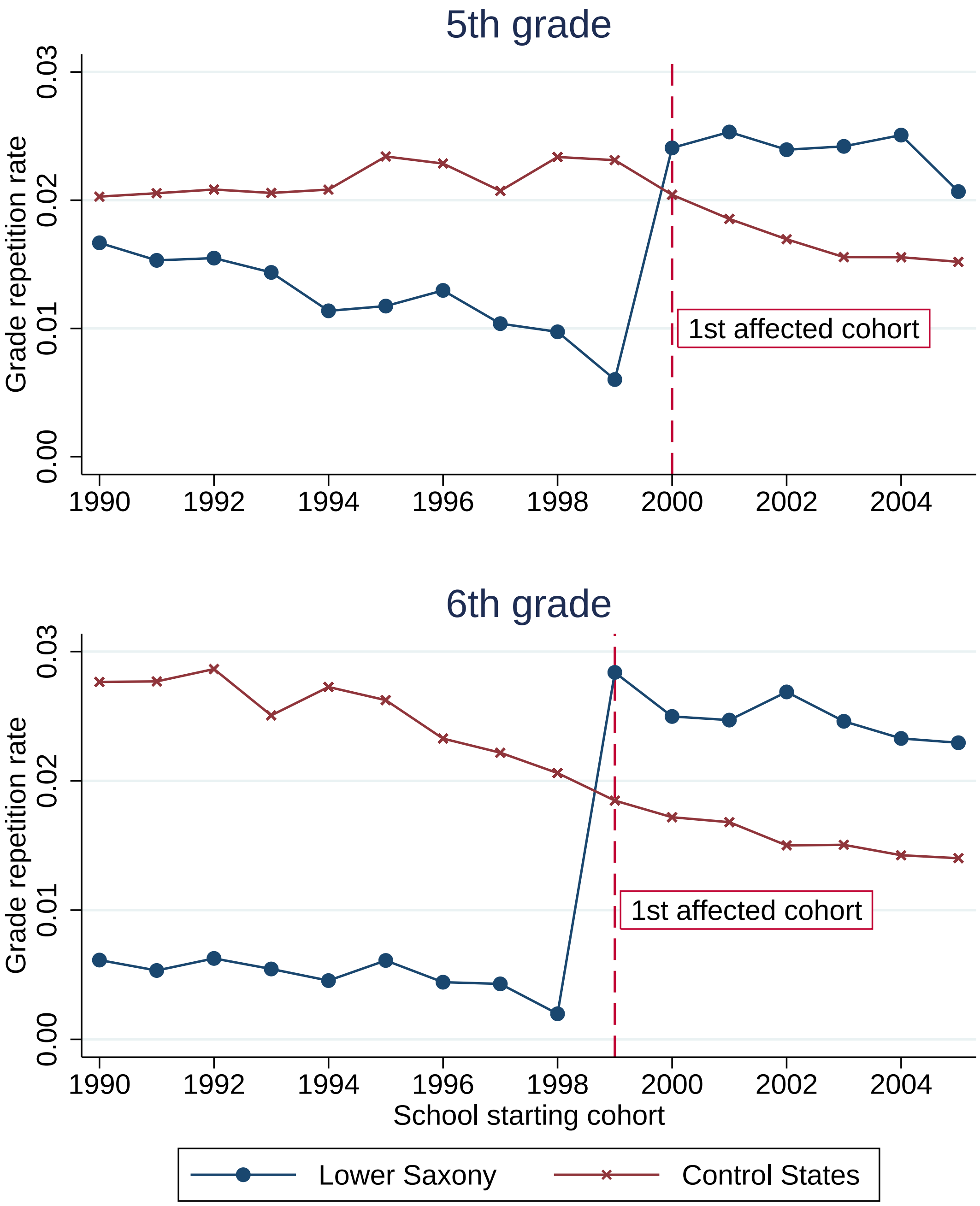


Figure 3.5: Effect of moving to early school tracking on grade repetition (grade 7-9)

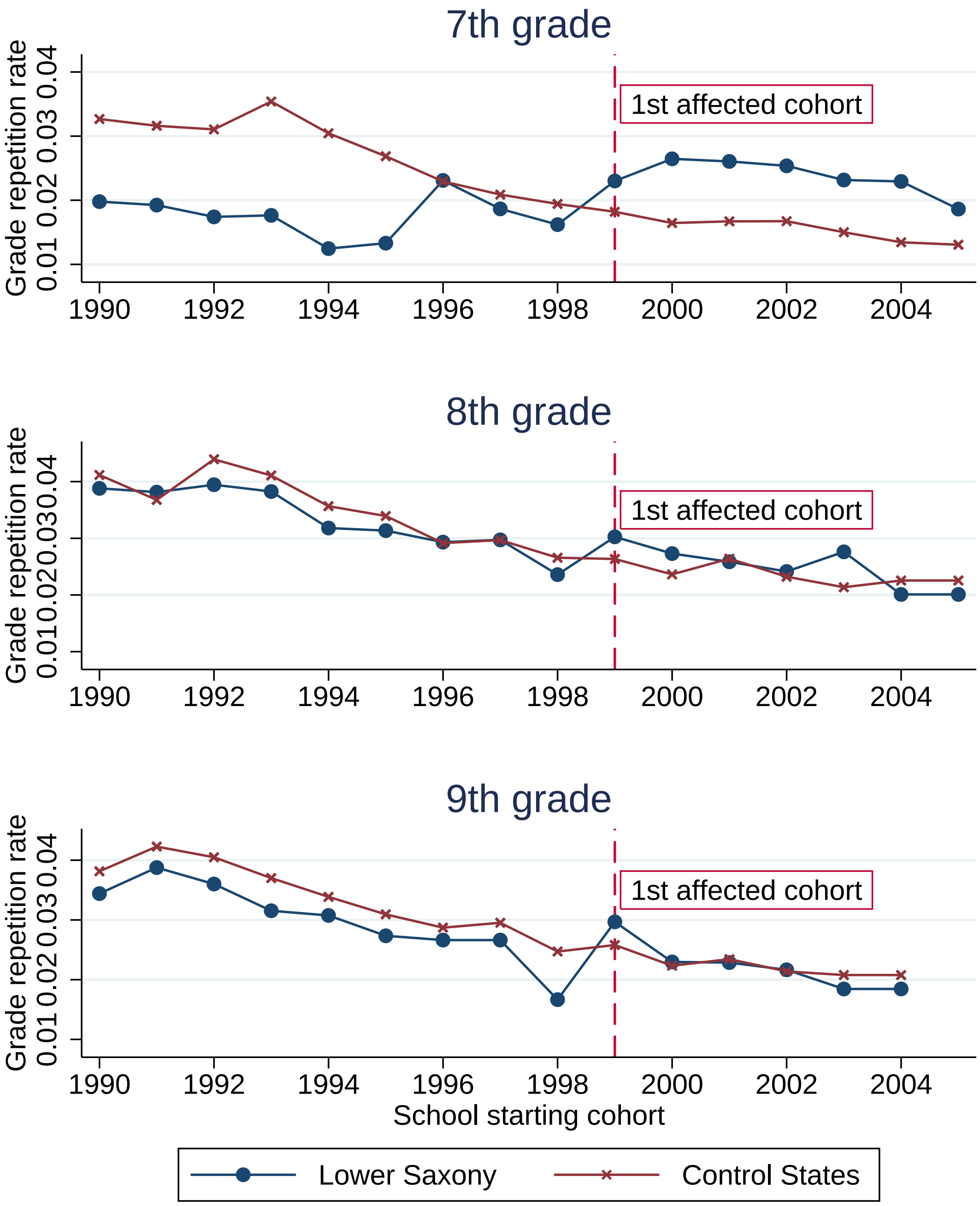


Table 3.1: Literature overview

Study	Country	Reform		Results
		Implemented	Content	
Dustmann (2004)	Germany	No reform		High intergenerational immobility of educational achievements across generations.
Meghir and Palme (2005)	Sweden	1962	Abolished tracking after grade 6, increased compulsory schooling and introduced a nationally unified curriculum.	Reform increased schooling above the new compulsory level for high ability individuals from low skilled fathers.
Pekkarinen et al. (2009)	Finland	1972	Changed tracking at age 11 years to 16 years and altered content of primary and secondary education.	Reform reduced intergenerational income elasticity by 23 percent.
Malamud and Pop-Eleches (2011)	Romania	2011	Shifted tracking from grade eight to grade ten.	Reform increased the likelihood of students' eligibility for university enrollment. Individuals from poor and rural regions and from less educated families were mainly affected by the reform.
Hall (2012)	Sweden	1987	Decreased difference between vocational and academic track.	Reform increased educational attainment in upper secondary schooling. No evidence that reform affected attainment beyond upper secondary schooling schooling.
Sund (2013)	Sweden	1995	De-tracking within school.	On average no impact of the reform on average math grade or the probability to graduate. Reform decreased the probability to fail in math for individuals from low educated families
Piopiunik (2014)	Germany	2000	Shifted tracking from grade six to grade four for some students.	Reform had negative impact on test scores of 15-year-old students.

Table 3.2: Lower Saxony: Overview of the reform

	4 years of primary schooling		
	OS: 2 years	OS: 1 year	no OS
Years with transitions	≤ 2004	2004	≥ 2004
School starting cohorts	≤ 1998	1999	≥ 2000

Table 3.3: Germany's educational reforms (year of reform)

		Combined	School tracking	
	G8	low and medium track	change in	track after reform
Baden-Wuerttemberg	2004			
Bavaria	2004			
Berlin	2006			
Brandenburg	2006	2005		
Bremen	2004	2005	2004	2 years earlier
Hamburg	2002		2010	2 years later
Hesse	2004			
Lower Saxony	2004	2011	2004	2 years earlier
Mecklenburg-West Pomerania	2004	2006	2006	2 years later
North Rhine-Westphalia	2005			
Rhineland-Palatinate	2008			
Saarland	2001			
Saxony				
Saxony-Anhalt	2003		2003	2 years earlier
Schleswig-Holstein	2007	2007		
Thuringia				

Notes: Rhineland-Palatinate did not introduce G8 in the whole federal state, but just in some selected schools. Schools in Lower Saxony can still offer the low and medium track. Bavaria had a change in the tracking system in 2000, but this track change did not affect the academic track.

Table 3.4: The problem of year fixed effects

	School starting cohort		Transition after Grade	
Year of transition	Lower Saxony	Control States	Lower Saxony	Control States
2002	1996	1998	6	4
2003	1997	1999	6	4
2004	1998 1999 2000	2000	6 5 4	4
2005	2001	2001	4	4
2006	2002	2002	4	4

Table 3.5: Effect of abolishment on academic track enrollment: Main estimation

	All				Female	Male
	(1)	(2)	(3)	(4)	(5)	(6)
Abolishment	0.043***	0.037***	0.036***	0.034***	0.038***	0.030***
	(0.009)	(0.006)	(0.006)	(0.007)	(0.008)	(0.006)
- Relative change	0.141	0.120	0.118	0.111	0.111	0.113
GDP				−0.043**	−0.054***	−0.031
				(0.015)	(0.015)	(0.018)
Expenditures/population				0.033	0.008	0.055
				(0.145)	(0.154)	(0.138)
Common school				0.121**	0.116**	0.128**
				(0.051)	(0.048)	(0.053)
G8	No	No	Yes	Yes	Yes	Yes
State specific linear and quadratic trend	No	Yes	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N	132	132	132	132	132	132

Notes: Clustered standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.6: Effect of abolishment on grade repetition: Main estimation

	All				Female	Male
	(1)	(2)	(3)	(4)	(5)	(6)
				Grade 5		
Abolishment	0.017***	0.021***	0.018***	0.018***	0.013***	0.024***
	(0.002)	(0.001)	(0.003)	(0.003)	(0.002)	(0.003)
- Relative change	1.348	1.687	1.444	1.486	1.302	1.602
				Grade 6		
Abolishment	0.030***	0.027***	0.025***	0.024***	0.017***	0.032***
	(0.002)	(0.002)	(0.004)	(0.004)	(0.003)	(0.005)
- Relative change	6.010	5.515	5.037	4.950	4.473	5.204
				Grade 7		
Abolishment	0.018***	0.011***	0.011***	0.011***	0.010***	0.017***
	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)
- Relative change	1.049	0.615	0.641	0.635	0.788	0.730
				Grade 8		
Abolishment	0.003	0.006*	0.006**	0.005*	0.005	0.007
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
- Relative change	0.098	0.189	0.187	0.161	0.194	0.162
				Grade 9		
Abolishment	0.004	0.009**	0.004	0.004	0.007	0.006
	(0.003)	(0.003)	(0.004)	(0.004)	(0.008)	(0.008)
- Relative change	0.135	0.287	0.137	0.127	0.274	0.156
Gdp	No	No	No	Yes	Yes	Yes
Expenditures/population	No	No	No	Yes	Yes	Yes
Common school	No	No	No	Yes	Yes	Yes
G8	No	No	Yes	Yes	Yes	Yes
State specific linear and quadratic trend	No	Yes	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N (School grade 5-8)	176	176	176	176	176	176
N (School grade 9)	165	165	165	165	165	165

Notes: Clustered standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.7: Effect of abolishment on academic track enrollment: Excluding federal states and years

	Excluded											
	Baden Wuerttemberg			Bavaria			Berlin			Hamburg		
	All	Female	Male	All	Female	Male	All	Female	Male	All	Female	Male
Abolishment	0.034***	0.038***	0.031***	0.033***	0.037***	0.029***	0.034***	0.039***	0.030***	0.030***	0.033***	0.027***
	(0.007)	(0.008)	(0.006)	(0.007)	(0.008)	(0.006)	(0.007)	(0.008)	(0.006)	(0.006)	(0.007)	(0.006)
N	120	120	120	120	120	120	120	120	120	120	120	120
	Excluded											
	Hesse			Northrhine Westphalia			Rhineland Palatinate			Saarland		
	All	Female	Male	All	Female	Male	All	Female	Male	All	Female	Male
Abolishment	0.037***	0.042***	0.033***	0.034***	0.039***	0.030***	0.033***	0.038***	0.029***	0.034***	0.039***	0.031***
	(0.005)	(0.006)	(0.005)	(0.007)	(0.008)	(0.006)	(0.008)	(0.009)	(0.007)	(0.008)	(0.009)	(0.007)
N	120	120	120	120	120	120	120	120	120	120	120	120
	Excluded									Included		
	Saxony			Thuringia			1st Treatment year			Cohort 2006		
	All	Female	Male	All	Female	Male	All	Female	Male	All	Female	Male
Abolishment	0.037***	0.041***	0.033***	0.034***	0.038***	0.031***	0.034***	0.037***	0.032***	0.034***	0.038***	0.030***
	(0.006)	(0.007)	(0.005)	(0.007)	(0.009)	(0.007)	(0.010)	(0.011)	(0.009)	(0.008)	(0.009)	(0.007)
N	120	120	120	120	120	120	121	121	121	142	142	142

Notes: Every model includes the control variables: Gdp, expenditures/population, common school, G8, state specific linear and quadratic time trends, cohort fixed effects and state fixed effects. The specification with 2006 excludes the school starting cohort 2006 for Hamburg. Clustered standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.8: Effect of abolishment on grade repetition: Excluding federal states

	All			Female			Male		
	Min	Med	Max	Min	Med	Max	Min	Med	Max
				School grade 5					
Coefficient	0.017	0.018	0.021	0.011	0.013	0.014	0.022	0.024	0.027
p-value	0.000	0.000	0.001	0.000	0.000	0.001	0.000	0.000	0.000
				School grade 6					
Coefficient	0.023	0.024	0.028	0.016	0.017	0.02	0.03	0.031	0.036
p-value	0.000	0.000	0.001	0.000	0.001	0.002	0.000	0.000	0.000
				School grade 7					
Coefficient	0.009	0.011	0.013	0.009	0.010	0.012	0.014	0.017	0.018
p-value	0.001	0.004	0.014	0.000	0.002	0.004	0.000	0.001	0.003
				School grade 8					
Coefficient	0.004	0.005	0.007	0.003	0.005	0.008	0.004	0.007	0.010
p-value	0.002	0.107	0.195	0.024	0.292	0.453	0.006	0.165	0.254
				School grade 9					
Coefficient	0.002	0.004	0.007	0.004	0.006	0.013	0.003	0.005	0.012
p-value	0.084	0.435	0.689	0.037	0.483	0.619	0.157	0.540	0.738
Gdp		Yes			Yes			Yes	
Expenditures/population		Yes			Yes			Yes	
Common school		Yes			Yes			Yes	
G8		Yes			Yes			Yes	
State specific linear and quadratic trend		Yes			Yes			Yes	
Cohort fixed effects		Yes			Yes			Yes	
State fixed effects		Yes			Yes			Yes	

Notes: The table summarizes the results if successively control states are excluded from the main regression. It displays the minimum, median and maximum value for the estimated coefficients and p-values. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.9: Effect of abolishment on grade repetition: Excluding 1st treatmenmt cohort and include cohort 2006

	Excluded 1st Treatment Cohort			Included Cohort 2006		
	All	Female	Male	All	Female	Male
			School grade 5			
Abolishment	0.021***	0.015***	0.028***	0.019***	0.014***	0.025***
	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)
			School grade 6			
Abolishment	0.024***	0.017***	0.032***	0.026***	0.018***	0.034***
	(0.004)	(0.003)	(0.005)	(0.004)	(0.003)	(0.005)
			School grade 7			
Abolishment	0.013***	0.015***	0.019***	0.012***	0.011***	0.017***
	(0.003)	(0.003)	(0.004)	(0.003)	(0.002)	(0.003)
		School grade 8				
Abolishment	0.005	0.007	0.007			
	(0.003)	(0.005)	(0.005)			
		School grade 9				
Abolishment	0.003	0.004	−0.001			
	(0.006)	(0.005)	(0.006)			
Gdp	Yes	Yes	Yes	Yes	Yes	Yes
Expenditures/population	Yes	Yes	Yes	Yes	Yes	Yes
Common school	Yes	Yes	Yes	Yes	Yes	Yes
G8	Yes	Yes	Yes	Yes	Yes	Yes
State specific linear and quadratic trend	Yes	Yes	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N (school grade 5)	154	154	154	176	176	176
N (school grade 6 & 7)	165	165	165	187	187	187
N (school grade 8)	165	165	165			
N (school grade 9)	154	154	154			

Notes: Clustered standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.10: Effect of abolishment and introduction on academic track enrollment: Including affected federal states

	Main model			Include all abolishment states			Include all abolishment states and one introduction state		
	All	Female	Male	All	Female	Male	All	Female	Male
Abolishment	0.034***	0.038***	0.030***	0.041***	0.029**	0.052***	0.041***	0.028*	0.053***
	(0.007)	(0.008)	(0.006)	(0.007)	(0.013)	(0.013)	(0.009)	(0.008)	(0.018)
Introduction							−0.028*	−0.022	−0.038***
							(0.014)	(0.020)	(0.012)
N	132	132	132	156	156	156	168	168	168

	Include all abolishment states, all introduction states and school cohort 2006			Include all federal states			Include all federal states and school cohort 2006		
	All	Female	Male	All	Female	Male	All	Female	Male
Abolishment	0.049***	0.036***	0.059***	0.041***	0.028**	0.051***	0.048***	0.036***	0.058***
	(0.009)	(0.008)	(0.018)	(0.006)	(0.012)	(0.014)	(0.010)	(0.007)	(0.018)
Introduction	−0.026**	−0.022	−0.032***	−0.031**	−0.024	−0.041***	−0.029**	−0.024	−0.034***
	(0.012)	(0.016)	(0.010)	(0.014)	(0.019)	(0.011)	(0.011)	(0.015)	(0.010)
N	182	182	182	192	192	192	208	208	208

Notes: Every model includes the control variables: Gdp, expenditures/population, common school, G8, state specific linear and quadratic time trends, cohort fixed effects and state fixed effects. Clustered standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.11: Effect of abolishment and introduction on academic track enrollment: Including affected federal states (wild clustered standard errors)

	Main model			Include all abolishment states			Include all abolishment states and one introduction state		
	All	Female	Male	All	Female	Male	All	Female	Male
Abolishment	0.362	0.356	0.402	0.018	0.056	0.010	0.014	0.056	0.012
Introduction							0.182	0.334	0.024
N	132	132	132	156	156	156	168	168	168
	Include all abolishment states, all introduction states and school cohort 2006			Include all federal states			Include all federal states and school cohort 2006		
	All	Female	Male	All	Female	Male	All	Female	Male
Abolishment	0.020	0.046	0.012	0.016	0.048	0.030	0.022	0.034	0.022
Introduction	0.050	0.166	0.018	0.140	0.318	0.014	0.038	0.116	0.012
N	182	182	182	192	192	192	208	208	208

Notes: The table shows the p-values of the regressions. Every model includes the control variables: Gdp, expenditures/population, common school, G8, state specific linear and quadratic time trends, cohort fixed effects and state fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.12: Effect of abolishment on academic track enrollment: Year fixed effects and placebo reform

	Year			Placebo reform		
	All	Female	Male	All	Female	Male
Abolishment	0.046***	0.047***	0.046***			
	(0.005)	(0.005)	(0.005)			
Placebo				0.000	−0.001	0.000
				(0.015)	(0.019)	(0.015)
Gdp	Yes	Yes	Yes	Yes	Yes	Yes
Expenditures/population	Yes	Yes	Yes	Yes	Yes	Yes
Common school	Yes	Yes	Yes	Yes	Yes	Yes
G8	Yes	Yes	Yes	Yes	Yes	Yes
State specific linear and quadratic trend	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	No	No	No
Cohort fixed effects	No	No	No	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N	144	144	144	55	55	55

Notes: Clustered standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.13: Effect of abolishment on grade repetition: Placebo reform

	All	Female	Male
		School grade 5	
Placebo	0.004	0.002	0.007
	(0.004)	(0.002)	(0.006)
		School grade 6	
Placebo	0.003*	0.001	0.005**
	(0.001)	(0.001)	(0.002)
		School grade 7	
Placebo	0.016***	0.012***	0.022***
	(0.004)	(0.003)	(0.005)
		School grade 8	
Placebo	0.005	0.005	0.006
	(0.003)	(0.003)	(0.004)
		School grade 9	
Placebo	0.003	0.002	0.004
	(0.003)	(0.003)	(0.004)
Gdp	Yes	Yes	Yes
Expenditures/population	Yes	Yes	Yes
Common school	Yes	Yes	Yes
G8	Yes	Yes	Yes
State specific			
linear and quadratic trend	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
N	99	99	99

Notes: Clustered standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.14: Effect of abolishment on grade repetition in grade 7: Only 3 pre-treatment cohorts

				Exclude 1st Treatment Cohort			Include Cohort 2006		
	All	Female	Male	All	Female	Male	All	Female	Male
Abolishment	0.007	0.003	0.014*	0.013**	0.012**	0.023***	0.006	0.002	0.013**
	(0.005)	(0.003)	(0.006)	(0.005)	(0.004)	(0.007)	(0.004)	(0.003)	(0.006)
Gdp	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Expenditures/population	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Common school	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
G8	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State specific									
linear and quadratic trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	110	110	110	99	99	99	121	121	121

Notes: Clustered standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

3.11 Appendix

Figure 3.6: Effect of moving to early school tracking on track choice: Bremen

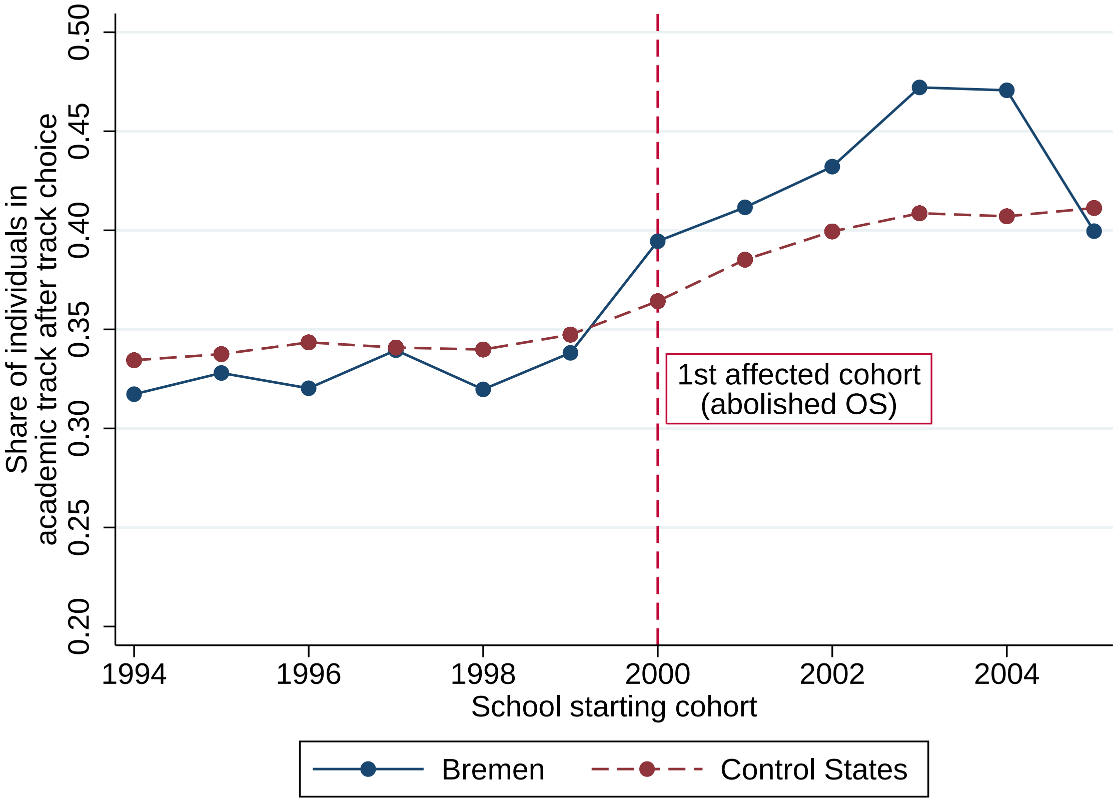


Figure 3.7: Effect of moving to early school tracking on track choice: Saxony-Anhalt

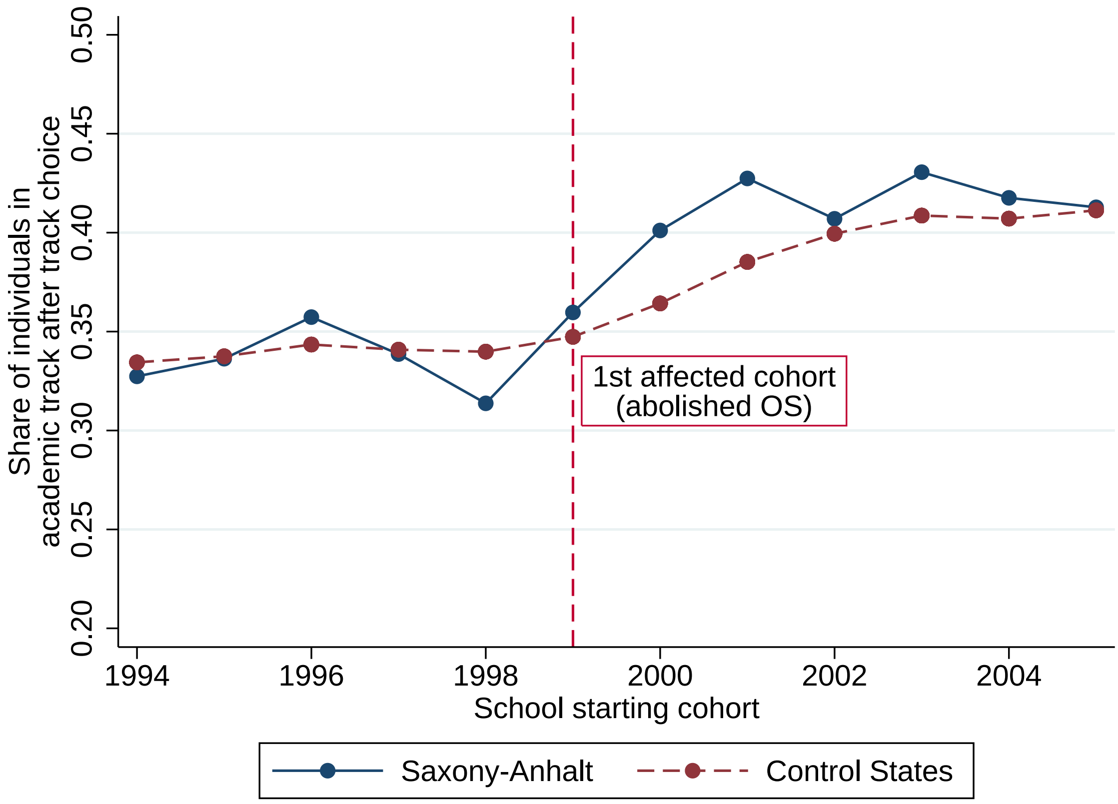


Figure 3.8: Effect of de-tracking in an early school tracking system: Mecklenburg-West Pomerania

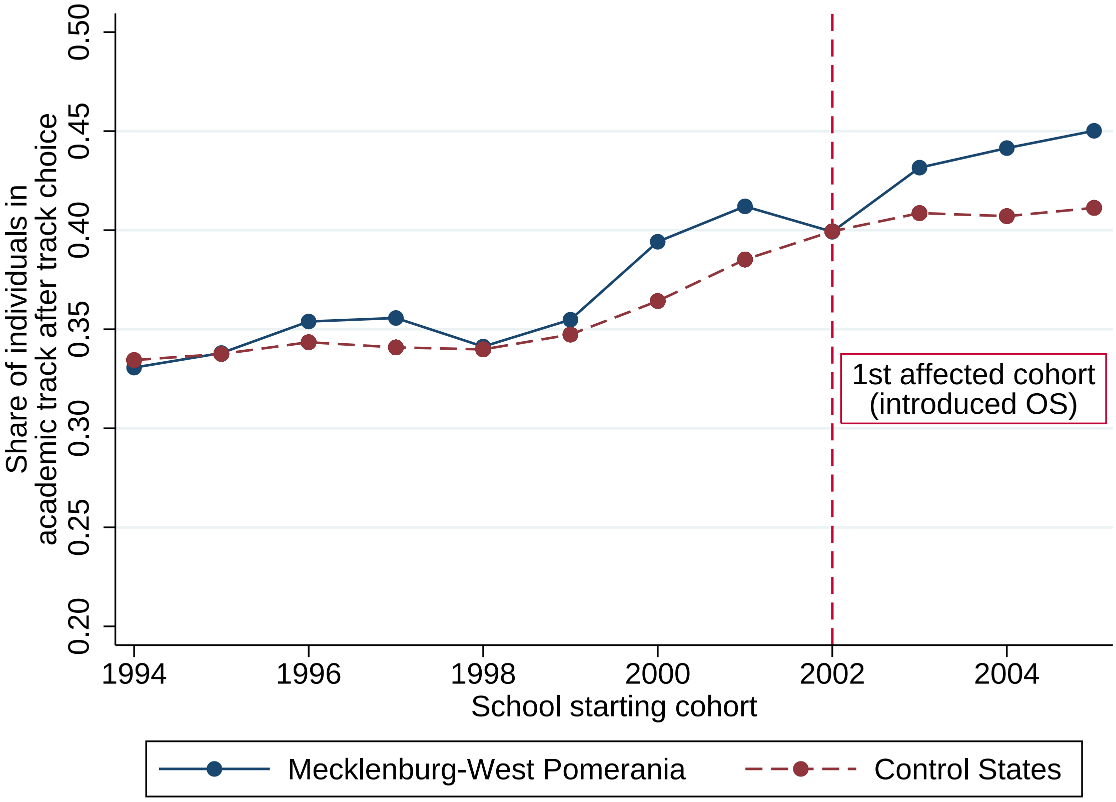
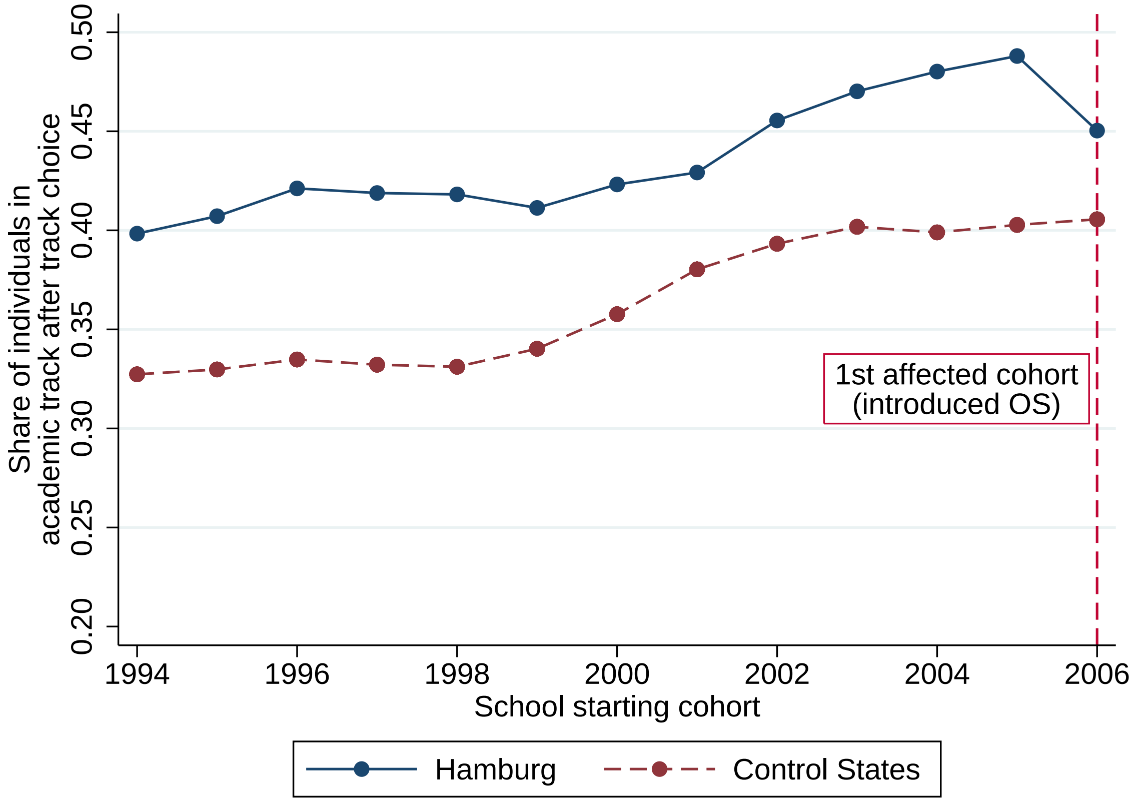


Figure 3.9: Effect of de-tracking in an early school tracking system: Hamburg



Notes: Control states do not include Hamburg.

Chapter 4

University Tuition Fees and High School Students' Educational Aspirations

4.1 Introduction

In many countries around the world, increasing the rate of participation in higher education is an important educational policy goal. It has even been enshrined in the United Nations' Universal Declaration of Human Rights: "Technical and professional education shall be made generally available and higher education shall be equally accessible to all on the basis of merit" (Article 26, paragraph 1). At the same time, in many OECD countries, tuition fees play a relevant role in the funding of higher education. In 2011, for example, only eight OECD countries (31 percent) did not charge tuition fees, one-third charged relatively low tuition fees (USD 1500 and below), and one-third charged fees above USD 1500 to nationals of the respective country (OECD, 2013a).[1]

What role do university tuition fees play in adolescents' educational plans? How does the introduction of relatively low tuition fees affect adolescents' educational aims and aspirations? Do even low tuition fees exacerbate educational inequality? One might expect that tuition fees mainly deter individuals from low-income households from enrolling in higher education. On the other hand, the majority of individuals who aim at attending university come from middle or high-income backgrounds and are unlikely to be credit-constrained. Moreover, the net present value of lifetime earnings is higher and the risk of unemployment is lower for highly educated individuals than

[1]These figures are calculated for the 26 OECD countries for which data was available (OECD, 2013a).

for individuals with lower education. Thus, one could hypothesize that free access to higher education is most beneficial to students from middle and high-income families. If (low) tuition fees do not significantly affect individuals' educational choices, there might be plausible reasons for the introduction of tuition fees: they could lead to greater equality because students who benefit from higher education contribute to the funding of their studies. However, if poor students lower their aspirations and do not continue with education as a result of higher anticipated costs in form of tuition fees, there are equality arguments for free access to higher education.

This paper analyzes the effect of the introduction and subsequent elimination of university tuition fees on adolescents' intentions to pursue higher education in Germany. We investigate (i) whether adolescents' educational plans are affected by relatively low tuition fees (1,000 euros per academic year) and (ii) whether the effect depends on their socio-economic backgrounds, based on longitudinal data from the German Socio-Economic Panel (SOEP). A key advantage of the German setting is that tuition fees were only introduced in some federal states. States that did not introduce university tuition fees therefore serve as a comparison group. Since many countries introduced tuition fees at the national level (e.g., Australia in 1989, United Kingdom in 1998), it is difficult to disentangle potential behavioral effects from secular trends in higher education. Several studies exploit variation in tuition fees across universities or time, or use both kinds of variation (Kane, 1994; Denny, 2014). However, to obtain unbiased estimates in this setting requires the restrictive assumption that the level of tuition fees is not affected by the demand for access to university degree programs. Moreover, most of the existing literature studies the influence on college enrollment in countries with high tuition fees such as the US, UK, and Australia (McPherson and Schapiro, 1991; Kane, 1994; Cameron and Heckman, 2001; Dearden et al., 2004; Chapman and Ryan, 2005).[2] Relatively little is known about potential effects of low tuition fees and, in particular, about how tuition fees influence adolescents' educational aspirations and plans in countries with a history of free access to higher education.

In Germany, the introduction of tuition fees was a political decision made by the governments of the federal states, and not by the universities. Thus, we argue and

[2]Note that the net costs, i.e., the difference between tuition fees and student aid, might differ across these countries even though they have the highest tuition fees (OECD, 2011a).

document that the demand for higher education is unlikely to have affected the introduction of university tuition fees, and that changes in higher university fees at the state level over time are likely to be exogenous for 17-year-old high school students. To the best of our knowledge, this is the first study to investigate the responsiveness of high school students to the introduction and elimination of relatively low university tuition fees. As such, our research contributes to the literature on how educational reforms and (public) interventions influence adolescents' educational choices (Oreopoulos and Dunn, 2013; Avery and Kane, 2004). Using data from the SOEP, we show that university tuition fees considerably influence adolescents' educational plans. The results suggest that tuition fees of 1,000 euros per academic year result in a reduction in the intention of high school students to pursue higher education of about seven percentage points in Germany. This corresponds to a drop of 11 percent, since 65 percent of all high school students aim at acquiring a higher educational degree. Moreover, individuals from low-income households tend to lower their educational aspirations considerably following the introduction of university tuition fees. For instance, the intention to continue with higher education drops by around 32 percentage points (50 percent) among those whose family income is in the lowest ten percent of the income distribution. These effects could be driven by students having distorted views of the costs and benefits of higher education (Horn et al., 2003; Oreopoulos and Dunn, 2013), being unaware of the financial aid that is available for low-income families (Ikenberry and Hartle, 1998; Dinkelman and Martinez, 2014), and the complexity of the application process in applying for grants and scholarships (Bettinger et al., 2012).[3] In many cases, low-income students have a more limited understanding of the costs and benefits of higher education (Horn et al., 2003), the admission processes, and the financial aid opportunities (Oreopoulos and Dunn, 2013; Avery and Kane, 2004). We also evaluate the predictive validity of the data and document a strong relationship between individuals' educational intentions and their actual educational choices later in life using the longitudinal structure of the data. The decline in young people's educational aspirations is therefore likely to have long-term consequences.

The empirical findings suggest that introducing relatively low tuition fees (i.e.,

[3]Financial aid take-up is very low in Germany, with only two percent of students who had to pay tuition fees reporting use of financial aid (Heine and Quast, 2011).

around three percent of the average annual tuition fees charged by public institutions in the US (OECD, 2011a)−which are not accompanied by comprehensive and widely advertised financial aid program for students from low-income households−can have a considerable impact on individuals' educational plans and educational inequality in society. The empirical results should be particularly relevant to those countries and policy makers considering the introduction of (low) tuition fees without a comprehensive financial aid scheme for low-income individuals.

The structure of the article is as follows. Section 4.2 discusses the related literature, and section 4.3 describes the institutional background. Section 4.4 presents the data, section 4.5 discusses the empirical strategy, and section 4.5 presents the estimation results. Section 4.7 discusses several robustness checks, and the final section concludes.

4.2 Related Literature

The international evidence of the consequences of tuition fees and the role of family income for educational choices is mixed. McPherson and Schapiro (1991) estimate the effect of net costs, the difference between tuition costs and student aid, on college enrollment for white students in the US. Their results reveal a significant negative influence of net costs on college enrollment for students from low-income families, but not for students from middle- or high-income families. Kane (1994) also finds a negative effect of tuition fees on enrollment. Moreover, he demonstrates that in the United States, tuition fees mainly deter black individuals from low-income families from university study. Cameron and Heckman (2001) emphasize the importance of considering educational transitions prior to deciding whether to go to college. They show that income is more important at earlier educational stages than at the time when transitions from high school to college in the US normally take place. The authors argue that family income plays a crucial role in the extent to which young people are able to develop their abilities, which in turn is a key factor in the decision to attend college. Further, Cameron and Heckman (2001) show that, ceteris paribus, the influence of family income on educational choice is rather low at the time of the college decision. Neill (2009) and Coelli (2009) estimate the impact of tuition fees on enrollment in Canada. Both studies find a negative effect of tuition fees on enrollment, with the effect size varying with family income. Using data from Ireland, Denny (2014) documents that the elimination of tuition fees in the nineties did not increase the probability that students with low socio-economic background enter university.

Borrowing constraints might be one reason why individuals from low-income families may be more affected by tuition fees than individuals from higher-income families. However, many studies on data from developed countries show that borrowing constraints play no major role in the decision to enroll in higher education. Keane and Wolpin (2001) and Cameron and Taber (2004) study the relevance of borrowing constraints in the US. Their findings suggest that borrowing constraints are not a hindrance to college attendance. Similarly, Dearden et al. (2004) find that the share of individuals who are borrowing constrained is rather small. Their results demonstrate that borrowing constraints are more relevant for 16-years-olds than for 18-years-olds when deciding whether to stay in full-time education. It is important to point out that the US and UK have extensive student financial aid programs and high tuition fees (OECD, 2011a). The findings therefore do not necessarily imply that borrowing constraints are irrelevant when no (or low levels of) student financial aid is available.

A few studies have analyzed the average impact of tuition fees in Germany. Hübner (2012) estimates that after several German states introduced tuition fees, university enrollment decreased there by about 4.8 percentage points. Tecu (2009) also reports a negative effect of tuition fees on student enrollment. In contrast, Bruckmeier et al. (2013) do not find a negative impact. All three studies use administrative data from the German federal statistical office. Hübner (2012) uses data up to 2007, and Bruckmeier et al. (2013) use data up to 2008. Since most states introduced tuition fees in 2007, these studies can only analyze short-term effects of the reform. Furthermore, they do not address the question of whether the effect of tuition fees on the enrollment decision depends on students' socio-economic background and whether the introduction of tuition fees influences young people's educational aspirations.[4]

[4]The study by Heine et al. (2008) analyzes whether the effect of tuition fees on individual educational plans depends on the educational background of the parents, using data from the Hochschul-Informations-System (HIS). A major limitation of this study is that the HIS changed the survey design in the time period when tuition fees were introduced in Germany. The change in the survey design might severely bias their estimates.

4.3 Institutional background

In 2003, six of Germany's sixteen federal states challenged a federal law that prohibited the introduction of university tuition fees. In January 2005, the German Federal Constitutional Court decided in favor of the plaintiffs. Ending an over 35-year-long tradition of tuition-free access to public higher education, seven federal states introduced tuition fees in the wake of the court decision. Yet only a few years later, several federal states eliminated the fees again.

Table 4.1 documents the timing of the introduction and elimination of tuition fees in Germany. The first two German states to introduce tuition fees were Lower Saxony and North Rhine-Westphalia in October 2006. One year later, seven of Germany's sixteen states (Baden-Wuerttemberg, Bavaria, Hamburg, Hesse, Lower Saxony, North Rhine-Westphalia, and Saarland) had introduced tuition fees. The other nine states never introduced tuition fees. In the remainder of this article, we use the term "tuition states" for the seven federal states that introduced tuition fees and the term "non-tuition states" for the states that never introduced tuition fees.

In Germany, the introduction and subsequent elimination of tuition fees was decided by the state governments and not by the universities and was therefore a political decision. The Christian democratic conservative parties (CDU/CSU) supported the introduction of tuition fees, and the social democrats (SPD) opposed it.[5] Consequently, most states that were governed by the CDU/CSU between 2005 and 2007 introduced tuition fees, with the exception of Saxony and Thuringia.[6] The tuition states set the fee almost uniformly to 500 euros per semester, i.e., 1,000 euros per academic year.[7] Thus, the amount of the fee was quite similar across tuition states. The introduction of tuition fees was accompanied by a financial aid scheme offering both loans and need-based financial aid. Loans were granted independent of both family income and, in

[5] We discuss potential problems of policy endogeneity in the robustness section below.

[6] At the beginning of 2006, Saxony was governed by the CDU and from April 2006 on by a coalition of the CDU and the SPD. In contrast, Thuringia was governed by the CDU in 2006 and 2007 but did not introduce tuition fees. In 2006, the former prime minister of Thuringia announced in the press that Thuringia would also introduce tuition fees in the next legislative period (Thüringer Allgemeine, 23.05.2006), but this did not happen.

[7] Although institutions of higher education in Bavaria and North Rhine-Westphalia could set their own tuition fees up to a maximum of 500 euros per semester, most of the universities in these two states chose to introduce fees of 500 euros per semester. Hamburg reduced its tuition fees to 375 euros per semester three semesters after their introduction.

most federal states, the creditworthiness of the applicant. If individuals earned less than a specified minimum income after completing their studies, the loan debt was deferred or canceled. Furthermore, students did not have to repay the full amount of their student debt if the sum exceeded a certain threshold.[8] Need-based financial aid was only available to students from disadvantaged economic backgrounds, as it was offered primarily with the aim of limiting the debts of individuals from low-income households. Scholarships played only a minor role in Germany. In the survey of the HIS in 2008, only two percent of individuals who had to pay tuition fees stated that they use a scholarship to pay the fees (Heine and Quast, 2011).

In October 2008, one year after the introduction of tuition fees, Hesse became the first state to eliminate tuition fees. This occurred due to the conservative (CDU) and liberal (FDP) parties losing the majority of seats in the state parliament. In the subsequent years, other states followed suit, and by the end of 2012, tuition fees were only in place in two states. Note that students enrolled in a private institution were not affected by the introduction and elimination of university tuition fees. However, there are not many private higher education institutions in Germany. In 2010, only about 4.9% of the students who were enrolled in a higher education institution studied at a private institution in Germany (Statistisches Bundesamt, 2014).

4.4 Data

This study uses data from the Youth Questionnaire of the German Socio-Economic Panel (SOEP). Every year since 2000, the SOEP has surveyed young people in households that are part of the SOEP with a special youth questionnaire the year they turn 17 (SOEP Group, 2012). This questionnaire asks detailed questions about past and current school achievements and about young people's educational aspirations. Those who state that they intend to complete occupational training or attain a university degree in the future are asked: "Which of the following degrees do you plan to attain?".[9]

[8] See, for example, http://www.bafoeg-aktuell.de/studium/studiengebuehren/studienbeitragsdarlehen.html.

[9] Respondents can choose one of the following answer categories: apprenticeship; full-time vocational school (*Berufsfachschule*) or health sector school (*Schule des Gesundheitswesens*); higher-level trade or technical school (*Meisterschule*, *Technikerschule*); training for civil servants (*Beamtenausbildung*); college of advanced vocational studies (*Berufsakademie*); technical or professional college (*Fachhochschule*); university.

Our dichotomous dependent variable "aiming at a higher educational degree" is equal to one if a young person aims to earn a degree from a technical college or university, and zero otherwise. Overall, around 70 percent of those surveyed intend to pursue higher education in the future.

Using the self-reported intention to pursue higher education at the age of 17 as an outcome has several advantages. First, in contrast to individuals who have already finished secondary schooling, 17-years-olds might be more likely to change their secondary schooling choices and investments. Cameron and Heckman (2001), for example, argue for the US that educational choices made prior to the time when students decide whether to attend college should be considered. Second, studying students' intentions helps us to understand how young people make their educational plans and what role educational institutions can play in their aspirations and career choices. Third, tuition fees should have no impact on where 17-years-olds live or on their probability to participate in the survey since the overwhelming majority of 17-years-olds still live with their parents (Iacovou, 2002; Francesconi et al., 2010) and it is very unlikely that families move due to the introduction of tuition fees.

Although the information about 17-year olds' intentions cannot predict actual enrollment in higher education perfectly, intentions are a meaningful measure of actual behavior. Table 4.2 displays the share of individuals who enroll in higher education over time, separately for individuals with and without a reported intention to acquire a higher degree as adolescents. Five years after the first interview (i.e., at the age of 17), 70 percent of the individuals who intended to pursue higher education are actually enrolled in higher education, compared to 47 percent who did not intend to pursue higher education at the age of 17. These differences increase over time, and ten years after the first interview, 85 percent of those who intended and 52 percent of those who did not intend to pursue higher education at the age of 17 are actually enrolled in an institution of higher education. Overall, the share is around 20 to 30 percentage points higher for individuals who did intend to pursue higher education as adolescents than for individuals who did not.

Our sample covers all individuals who state in the Youth Questionnaire that they (i) attend an upper secondary school, (ii) intend to acquire an upper secondary leaving certificate or (iii) have already an upper secondary leaving certificate. An upper secondary leaving certificate allows individuals to enter higher education in Germany. We supplement the sample with individuals who did not receive the Youth Questionnaire at the age of 17 years, because they did not belong to a SOEP household at that time because they entered the SOEP afterwards. When participating in the SOEP for the first time, they receive the questions of the youth questionnaire. We restrict the sample to individuals who were at most 20 years old and were still going to school at the time of the interview. Using pooled cross-sections from 2000 to 2012, the sample comprises 2,143 young individuals.

Table 4.3 displays mean values and standard deviations of key variables separately for individuals living in tuition states and non-tuition states. The first two columns show summary statistics only for respondents who answered the youth questionnaire before October 2006, i.e., before any tuition fees were introduced. Columns (3) and (4) report summary statistics of the pooled cross-sections for the entire period (2000-2012). Overall, the table shows that individuals living in tuition states and non-tuition states differ somehow. The share of individuals who state that they intend to pursue higher education is about three percentage points higher in tuition states (at 67.4%, column (1)). Furthermore, the proportion of young people with highly educated parents (i.e., with at least one parent having a university degree) and with a migration background is higher in the tuition states. Finally, the household equivalent income (measured in 2010 prices) is approximately 24% higher in tuition states. These differences are not surprising because none of the federal states of the former German Democratic Republic (GDR) introduced tuition fees. The former GDR is less affluent and has a different social structure than the former West Germany, as reflected in Table 4.3.

Figure 4.1 illustrates how the share of individuals who state that they aim to pursue higher education develops over time separately for tuition and non-tuition states. The bottom part of the figure displays the number of states with tuition fees in place by the end of the year. In the top part of the figure, the solid blue line depicts the share of individuals from tuition states and the dashed red line displays the share of individuals from non-tuition states who state that they intend to pursue a higher degree as adolescents. Before 2007, the share of individuals who stated that they intended to pursue higher education was slightly higher in the tuition states. After 2007, however, when all seven tuition states had introduced a fee, the lines began moving in different directions. First, the share increased for the non-tuition states but decreased for the tuition states. The diverging trends between treatment and comparison groups following the policy change suggests that tuition fees might have a negative impact on young people's educational intentions. After 2009, the gap closed somewhat and in 2012—when most states had already eliminated university tuition fees—the share is again higher in tuition than in non-tuition states.

Before the introduction of the fees, the shares of the two groups did not follow a completely parallel development over the whole period. Especially in the years 2002, 2003, and 2004, the shares developed differently in the two groups, although they moved in the same direction. Even though the development in the outcome variable prior to the policy change is not perfectly parallel, we believe and argue that the common trend assumption is likely to hold. In the robustness section below, we present and discuss several sensitivity analyses supporting this claim.

4.5 Empirical strategy

Using the introduction and elimination of tuition fees in Germany as quasi-experimental evidence, we apply a difference-in-differences (DiD) research design to identify the effect of tuition fees on young people's educational plans. Here, tuition states serve as treatment and non-tuition states as a control group. We assign the treatment status according to the state of residence at the age of 17, when young people are still in secondary school.[10]

We start the empirical analysis by estimating the following baseline linear probability model:

$$y_{ist} = \delta_0 + \delta_1 Tuition\_State_s + \delta_2 After_t + \theta_1 Tuition\_State_s \times After_t + u_{ist} \quad (4.1)$$

Subscript i denotes the individual, s the federal state of residence, and t the time

[10]Since tuition fees provide individuals who are still in school with no financial incentive to move, they should not influence the state of residence.

of the interview (month-year). The dependent variable is equal to one if adolescents answer that they intend to pursue higher education and zero otherwise. $Tuition\_State_s$ and $After_t$ are both dummy variables. $Tuition\_State_s$ is one if the individual's state of residence is a tuition state and $After_t$ is one if, at the time of the interview, the state has already introduced tuition fees, and zero otherwise. θ_1 measures the effect of tuition fees on the intention to pursue higher education (university or technical college degree). Standard errors are clustered at the state level.[11]

In a second step, we estimate the following model:

$$y_{ist} = \beta_0 + \theta_1 Tuition_{st} + X_{ist}\beta + \lambda_s + \gamma_t + u_{ist}. \tag{4.2}$$

The dependent variable is defined as before. The dummy variable $Tuition_{st}$ is equal to one if tuition fees are implemented in the individual's state of residence at the time of the interview, and zero otherwise. λ_s and γ_t are federal state and time fixed effects, respectively. X_{ist} is a $(1 \times k)$ vector with further control variables. In the main regressions, the vector consists of a gender dummy, a dummy for whether a person has a migration background, for whether at least one parent has a university degree, the household equivalent income (measured in 2010 prices), and the ratio of unemployed individuals and number of open vacancies at the state level. θ_1 captures the effect of tuition fees on the intention to acquire a higher educational degree.

Thereafter, we study potential heterogeneous effects such as:

$$\begin{aligned} y_{ist} = \beta_0 &+ \theta_1 Tuition_{st} + \beta_2 Char_{ist} + \theta_2 Tuition_{st} \times Char_{ist} \\ &+ \beta_3 \times Char_{ist} \times Tuition\_State_s \\ &+ \sum_{k=2001}^{2012} \beta_k \times Char_{ist} \times Year_t^k + X_{ist}\beta + \lambda_s + \gamma_t + u_{ist} \end{aligned} \tag{4.3}$$

Equation (4.3) extends equation (4.2) by adding a dummy $Char_{ist}$ and an interaction term between $Char_{ist}$ and the dummy $Tuition_{st}$. The dummy $Char_{ist}$ is one

[11] Because the federal states introduced and eliminated their tuition fees at different points in time, we cannot estimate equation (4.1) using all states and years. Thus the estimation excludes the tuition states Hesse and Saarland and includes only the years 2000 to 2005 and 2008 to 2010. As a result, none of the 14 remaining states had a tuition fee in place between 2000 and 2005, and all five remaining tuition states charged fees between 2008 and 2010.

if individuals belong to a specific group according to their characteristics, and zero otherwise. The dummy $Tuition_{st}$ is defined as before and thus combines time and place. Therefore, the interaction between $Char_{ist}$ and $Tuition_{st}$ combines time, place, and whether a person belongs to the particular socio-economic group. Furthermore, we interact the dummy $Char_{ist}$ with the information whether a person lives in a tuition state ($Tuition\_State_s$), and year dummies ($Year_t^k$). We distinguish between the following different individuals' characteristics: gender, household income, and whether parents have a university degree.

The interpretation of θ_1 now changes compared to equation (4.2). θ_1 now measures the effect of the tuition fees on the intention to pursue higher education for the reference group. The parameters $\theta_1 + \theta_2$ instead capture the effect of tuition fees on the intention to pursue higher education for individuals who do not belong to the particular reference group. If tuition fees have a negative impact on the intention to pursue higher education, and if the effect is stronger for the reference group, β_3 would be positive.

4.6 Results

Table 4.4 displays the results of the simple DiD estimation method (equation (4.1)). The table shows that the share of individuals who intend to pursue higher education is increasing over time in tuition and non-tuition states. In the tuition states, the share increases by 4.5 percentage points from 67.4 percent (2000-2005) to 71.9 percent (2008-2010). In the non-tuition states, the share increases by 13.8 percentage points, from 63.4 percent (2000-2005) to 77.1 percent (2008-2010). Thus, the simple DiD estimate predicts a 9.3 percentage point decrease in the intention to pursue higher education due to the introduction of tuition fees.

Table 4.5 reports the estimation results from four different specifications of equation (4.2). The first regression includes only the dummy $Tuition_{st}$, together with a maximum set of time and state fixed effects as regressors. The DiD estimate reveals a decrease of approximately 6.9 percentage points in the intention to pursue higher education due to the introduction of tuition fees. This corresponds to a decrease of around 11 percent, since around 70 percent of 17-year-old high school students from tuition states planned to pursue higher education before any tuition fee was introduced. The next specifications in Table 4.5 successively add the following control variables: gender and migration background (column 2), household equivalent income and whether parents have a university degree (column 3), and labor market conditions (column 4). The estimated impact and the precision of the tuition fees effect on adolescents' educational aspirations remain very stable across the different specifications. Overall, the results suggest that young people's intention to pursue higher education decreases by around seven percentage points (11 percent). This effect appears fairly large. For example, the educational aspirations of those coming from an academic background, i.e., having at least one parent with a university degree, are around 19 percentage points higher than for those whose parents do not have a university degree. The introduction of university tuition fees therefore corresponds to nearly 40 percent of the differences in educational aspirations between adolescents from academic and non-academic households.

Next, we estimate equation (4.3) with varying reference groups to test for effect heterogeneity. The results are shown in Table 4.6. As a benchmark, the first column in the table reports the DiD point estimate from our preferred specification as in Table 4.5. The regressions in columns (2)-(4) in Table 4.6 use the poorest 10, 20, and 30 percent (according to the household equivalent income) as reference groups, respectively. The results in column (2) predict an about 32 percentage point (49 percent) reduction in the intention to pursue higher education for the individuals from the poorest 10 percent of households. Therefore, the estimated impact of the fees is much higher (more negative) for low-income individuals than for the average individual. The effect is highly significant and differs from the tuition effect for individuals from higher-income families at the 5 percent significance level. Column (3) in Table 4.6 shows that the tuition effect differs considerably between the poorest 10 and 20 percent. The point estimate of the tuition effect for the poorest 20 percent is -18 percentage points (-28 percent) and highly significant. The effect still differs from the tuition effect of individuals from higher income groups. For the poorest 30 percent, the estimated tuition effect drops further to about -9.5 percentage points (-15 percent) and is no longer statistically significant. The results show that individuals from low-income households, especially individuals from the poorest 10 percent, mainly drive the results. Testing whether the effect also depends on the gender of a person or on the university education of the parents in the last two columns of Table 4.6, we do not find evidence of additional effect heterogeneity.

Next, we investigate whether the heterogeneous effects with respect to household income might be driven by the fact that children from poorer households are less successful in school or have lower educated parents, which might not be adequately controlled for in our main specification. Table 4.7 adds further explanatory variables when differentiating between the reference groups of the poorest 10, 20 and 30 percent. Panel A adds a dummy variable for whether the parents completed upper secondary school and variables for adolescents' test scores in mathematics and German[12]; Panel B also includes interaction terms between the tuition dummy and school grades in math and German; and Panel C additionally covers interactions between the tuition dummy and the educational background of the parents, i.e., whether they completed upper secondary school but have no university (technical college) degree or a university degree. For the poorest 10 percent, the estimated impact of tuition is about 25 percentage points higher than the tuition effect for individuals from higher-income families in all three panels. Thus, the difference is just two percentage points lower than the results in Table 4.6. Adding additional control variables also changes the results for the other two income groups (columns (2) and (3) in Table 4.7) only slightly. Overall, the results in Table 4.7 show that the DiD estimates for low-income individuals are unlikely to be driven by differences in school performance or by parents' educational background.

4.7 Robustness

Studies have shown that a small number of clusters might lead to an over-rejection of the null-hypothesis (see, e.g., Donald and Lang 2007). Since we use only 16 clusters, over-rejection of the null hypothesis is also a concern. One method to address this potential problem is the wild cluster bootstrap procedure suggested by Cameron et al. (2008), who show in a simulation exercise that their method can cope with a small

[12]Test scores are measured on a six-point scale on which a score of 1 represents the highest mark and a score of 6 is the lowest mark. We distinguish between three different groups (scores 1 and 2, scores 3 and 4, scores 5 and 6).

cluster size. Table 4.8 displays p-values from the wild cluster bootstrap procedure for the four different model specifications as in Table 4.5. In the first three regressions in Table 4.8, the p-value for the tuition fees effect is slightly higher than 1 percent, and in the last regression, the p-value is below the 1 percent significance level. Thus, the statistical significance of the tuition effect does not change compared to the main results in Table 4.5. Also, the significance of the other covariates does not change considerably. The results from the wild cluster bootstrap procedure suggest that the small number of clusters is unlikely to be a problem in our application.

Another concern is that unobserved events/reforms/shocks occurred during the observation period and affected educational aspirations of adolescents from tuition and non-tuition states differently. Figure 4.1 shows some deviations in the pretreatment trends between tuition states and non-tuition states in the years 2002-2005. Moreover, the figure displays a sharp increase in the intention to pursue higher education for tuition states in 2012, with a decrease for non-tuition states in the same year. To shed some light on whether unobserved events in certain years might bias the findings, Table 4.9 displays the regression results of our preferred specification without the observations from particular years, i.e., deleting observations from the year 2002 in column (2), and excluding observations from the year 2003 in column (3). As a benchmark, the first column in the table also reports the DiD estimates from our preferred model (Table 4.5, column 4). The point estimates of the tuition fees in columns (2)-(6) in Table 4.9 vary between -6.2 and -7.5 percentage points, and are always significantly different from zero at the 5 percent level. These results provide first suggestive evidence that it is unlikely that specific shocks or policy events in particular years are driving the results.

An alternative way to check whether unaccounted factors might bias the findings is to include potential relevant variables directly into the regressions, if they are observable. The introduction of tuition fees in Germany was mainly a political decision and therefore our estimated tuition effect might partly capture differences in attitudes, preferences and norms that might systematically vary between tuition and non-tuition states, and over time. In section 4.3, we discussed that the majority of conservative state governments introduced tuition fees. The regression in Table 4.10, column (2) therefore also controls for a dummy variable CDU/CSU to check whether political changes might drive the results. The variable is equal to one if the CDU/CDU is part of the government in the federal state of the respondent at the time of the interview, and zero otherwise.[13] This expansion of the model has only a negligible effect on the point estimates and shows that the tuition effect does not just capture a 'ruling party' effect.[14] Moreover, different developments in educational spending and differences in the relative size of high school graduates between tuition and non-tuition states might bias the estimates. The specifications in columns (2) and (3) in Table 4.10 therefore also control for educational expenditure divided by the Gross Domestic Product, and the number of high school graduates divided by the size of the population at the federal state level. None of these variables turns out to be significant and the estimated impact of tuition fees on educational aspirations hardly changes.

Our main specification implicitly assumes that the announcement of tuition fees does not affect individuals' educational aspirations. We now test whether this assumption is reasonable. The last column of Table 4.10 adds the dummy *Announced Tuition* to the main model. This variable is equal to one if, at an individual's federal state of residence, a tuition fee is officially announced, but not yet introduced at the time of the interview.[15] The estimated announcement effect is negative, but statistically insignificant. Further, the estimated tuition effect increases by about one percentage point to approximately -8.5 percentage points. Thus, the point estimate of -7.4 in the main regression is rather conservative and could be interpreted as a lower bound. In unreported regressions, we also estimated weighted DiD regressions using the cross-sectional weights provided by the SOEP Group. The point estimate of tuition fees on educational aspirations in the weighted regressions is more negative (and precisely estimated at the 5 percent significance level) than in our preferred specification.

Next, we conduct placebo tests to check the validity of the identification approach. Using the time period between January 2000 and September 2006, i.e. before any tuition fees were introduced, we add a dummy *Placebo* to the main specification.

[13]Overall, in 60 percent of all cases the CDU/CSU was part of a state government.

[14]In unreported regressions, we also included lagged values (up to four lags) of the CDU/CSU variable. None of these lagged variables had a significant impact on young people's educational aspirations (individually and jointly) and the DiD point estimate remained very stable.

[15]See Table 4.1 for the timing of announcement in the different states.

Table 4.11 reports the results from three different placebo regressions. In the first model, *Placebo* is one, if the placebo reform happened four years prior to the actual introduction, and zero otherwise. Similarly, in the third (second) model, the dummy *Placebo* is one assuming that placebo tuition fees were introduced three (two) years prior to the actual implementation, and zero otherwise. Note that we do not report estimates assuming that the placebo reform happened one year prior to the actual implementation as this dummy variable would coincide with the *Announced Tuition* fees dummy variable in many states. In the first model (4 years earlier), the point estimate for the *Placebo* dummy is almost zero, in the second model (3 years earlier) positive and in the last model (2 years earlier) almost zero. The *Placebo* dummy is not statistically significant at conventional levels. Thus, the placebo tests do not provide evidence that our identification approach is invalid.[16]

A linear probability model (LPM) has the disadvantages that it does not restrict predictions to lie between 0 and 1 and, in absence of interaction terms, the marginal effects are constant. Table 4.12 reports results from probit estimates. The first column replicates the main specification and shows that the tuition fee effect is still significant at the 1 percent level and the average marginal effect (AME) is almost identical to the result of the LPM with -7.4 percentage points decrease in adolescents' educational aspirations. The next three models estimate the tuition effect for individuals from different income households. Again, the tuition effect is highly significant and negative for the poorest 10 and 20 percent. The AMEs for these three poor income groups are very similar to the results of the LPM. The last two columns in Table 4.12 test whether the tuition effect depends on gender and the educational background of the parents. Again, the results do not provide evidence that the effects vary with respect to these characteristics. Overall, using probit models instead of LPMs does not change the main findings.

[16]Note, however, that the standard error of the *Placebo* dummy in the first regression is quite large, which provides a rather low power for statistical tests. In unreported regressions, we also conducted placebo tests using all available years for the comparison states (rather than only up to September 2006). This resulted in a sample size of nearly 1500 observations. The results were very similar to the ones in Table 4.11 and are available from the authors upon request.

4.8 Conclusions

This article analyzes the impact of higher education tuition fees on adolescents' educational aspirations. Using data from the Youth Questionnaire of the German Socio-Economic Panel (SOEP), the results suggest that tuition fees considerably decrease the likelihood that adolescents plan to acquire a higher educational degree. This finding is in line with previous studies that analyze the short-term effect of tuition fees on university enrollment in Germany (see, e.g., Tecu 2009 and Hübner 2012). The empirical results demonstrate that even relatively low fees of 1,000 euros per academic year can have large adverse effects on young people's educational aspirations, especially for individuals from low-income families. Overall, the results show a seven percentage points decrease in the intention to acquire a higher degree, and a 32 percentage points decrease for individuals from the poorest 10 percent income households. Although the intention of 17-years-olds do not predict actual behavior perfectly, the intention and the actual decision are closely related. Thus, changes in individuals' stated intentions are likely to also impact on actual behavior.

Different possible mechanisms exist why tuition fees might especially affect educational aspirations of individuals from low-income households. One is that low-income individuals perform worse in school. Therefore, tuition fees would not deter individuals from poor income households, per se, but rather lower performing pupils. We find no empirical evidence for this potential explanation since differences in school performance cannot explain the effect heterogeneity. Another possible mechanism is the existence of borrowing constraints. However, this is not very likely since previous work for industrialized countries shows borrowing constraints have only a small influence on university attendance in industrialized countries (see, e.g., Keane and Wolpin 2001 and Cameron and Taber 2004). Further potential mechanisms are differences in financial literacy, educational preferences and discount factors between income groups. The impact of tuition fees on perceived net benefits from higher education might vary between individuals according to family income, because of these differences.

The results of this study are informative for policy debates. They show that even modest tuition fees of 1,000 euros per academic year can have huge adverse effects on the educational plans of high school students. The provision of loan schemes alone is unlikely to prevent low-income individuals from changing their educational aspirations. Different institutional factors might contribute to these results. One factor could be that the financial net gains of higher education are relatively low in Germany compared to other OECD countries (OECD, 2013a). Another explanation might be that the federal states did not promote scholarships widely enough when introducing tuition fees. Both institutional features might have amplified the effect of tuition fees on adolescents' educational aspirations.

4.9 Figures and Tables

Figure 4.1: Common trend and the number of states with tuition fees

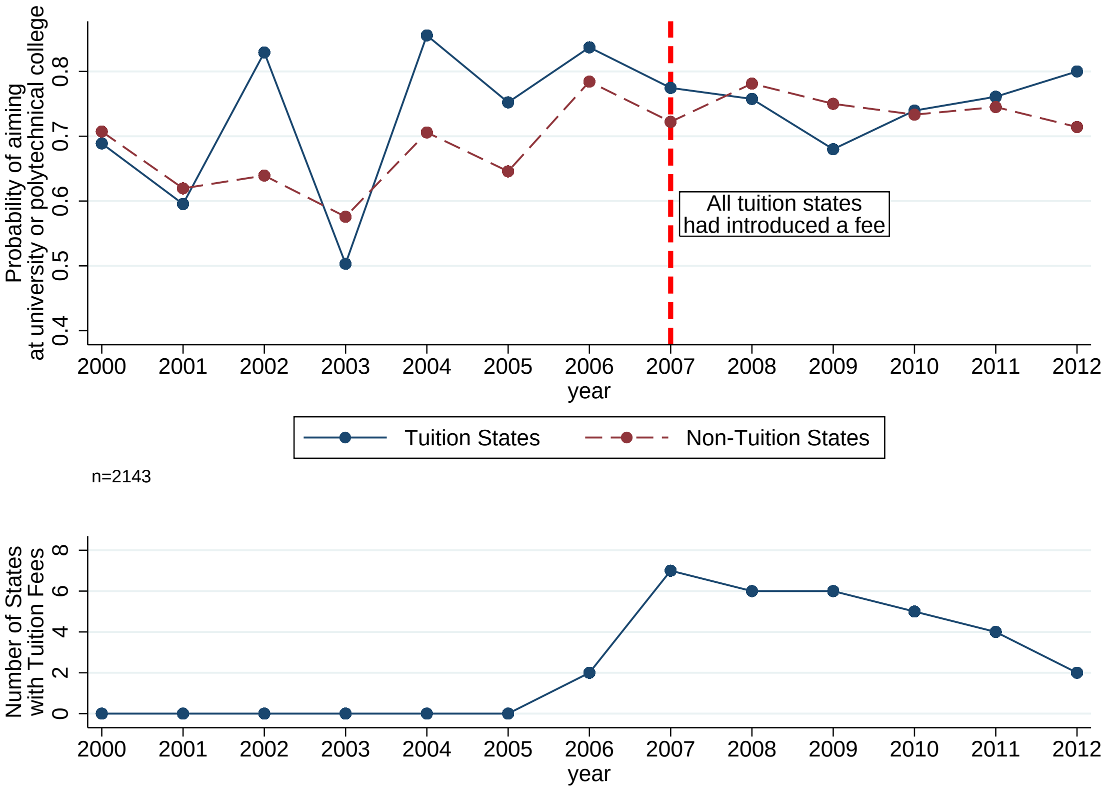


Table 4.1: Tuition fees legislation in Germany

State	Legislation passed	Fee introduced	Legislation for termination passed	Fee terminated
Baden-Wrttemberg	12/2005	4/2007	4/2011	4/2012
Bavaria	5/2006	4/2007	5/2013	10/2013
Hamburg	7/2006	4/2007	4/2011	10/2012
Hesse	10/2006	10/2007	6/2008	10/2008
Lower Saxony	12/2005	10/2006	7/2013	10/2014
North Rhine-Westphalia	3/2006	10/2006	4/2011	10/2011
Saarland	7/2006	10/2007	2/2010	4/2010

Notes: Federal states without tuition fees: Berlin, Brandenburg, Bremen, Mecklenburg-West Pomerania, Rhineland-Palatinate, Saxony, Saxony Anhalt, Schleswig Holstein and Thuringia.

Table 4.2: Share of high-school students being enrolled in higher education

After	3 years	4 years	5 years	6 years	7 years	8 years	9 years	10 years
Aiming at a higher educational degree as adolescent	0.043 (840)	0.514 (742)	0.700 (454)	0.765 (391)	0.785 (288)	0.807 (212)	0.826 (138)	0.846 (91)
Not aiming at a higher educational degree as adolescent	0.062 (355)	0.365 (310)	0.466 (206)	0.536 (179)	0.558 (154)	0.600 (115)	0.607 (84)	0.524 (42)

Notes: Number of individuals in parentheses. Note that the number of individuals differs from the sample size in Table 4.3 because we do not have longitudinal information for all first-time respondents (e.g., those first interviewed in 2011 and 2012) and because of dropout from the panel.

Table 4.3: Descriptive statistics

	Before October 2006		Full Time Period (2000-2012)	
	Tuition States	Non-Tuition States	Tuition States	Non-Tuition States
Aiming at a higher educational degree	0.674	0.641	0.723	0.690
	(0.469)	(0.480)	(0.448)	(0.463)
Tuition	0.000	0.000	0.385	0.000
	(0.000)	(0.000)	(0.487)	(0.000)
Male	0.452	0.446	0.468	0.467
	(0.498)	(0.498)	(0.499)	(0.499)
Migration background	0.235	0.067	0.232	0.099
	(0.424)	(0.250)	(0.422)	(0.299)
Parents with a university degree	0.379	0.312	0.361	0.297
	(0.486)	(0.464)	(0.481)	(0.457)
Household equivalent income	1.829	1.474	1.774	1.459
	(1.955)	(0.756)	(1.582)	(0.750)
Unemployment/vancancies	8.821	21.540	7.322	15.997
	(3.674)	(10.066)	(3.169)	(9.616)
Number of individuals	659	359	1456	687

Notes: Figures are means with standard deviations in parentheses. Equivalent household income is household income in 1,000 euros and in 2010 prices divided by adjusted household size. Household member who are older than 14 years receive a weight of 0.5 and other household member a weight of 0.3.

Table 4.4: Simple DiD: Share of high-school students who intend to acquire a higher educational degree

	2000-2005	2008-2010	Difference
Tuition States	0.674	0.719	0.045
Non-Tuition States	0.634	0.771	0.138
Simple DiD			-0.093

Notes: Without Hesse and Saarland. Uses the years 2000-2005 and 2008-2010.

Table 4.5: Average effects on intention to acquire a higher educational degree

	(1) Full Sample	(2) Full Sample	(3) Full Sample	(4) Full Sample
Tuition	-0.069**	-0.069**	-0.065**	-0.075***
	(0.026)	(0.026)	(0.025)	(0.022)
Male		0.001	-0.007	-0.008
		(0.011)	(0.009)	(0.009)
Migration background		-0.018	-0.005	-0.004
		(0.030)	(0.030)	(0.030)
Parents with a university degree			0.182***	0.182***
			(0.022)	(0.022)
Household equivalent income			0.014	0.014
			(0.013)	(0.013)
Unemployment/vancancies				0.003
				(0.002)
Time Fixed Effects	YES	YES	YES	YES
State Fixed Effects	YES	YES	YES	YES
Number of individuals	2143	2143	2143	2143

Notes: Clustered standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.6: Heterogenous effects of tuition fees on the intention to acquire a higher educational degree

	(1)	(2)	(3)	(4)	(5)	(6)
		Reference group:				
	Full Sample	Poorest 10%	Poorest 20%	Poorest 30%	Female	Parents without a uni. degree
Tuition	-0.075***	-0.321***	-0.180***	-0.095	-0.094**	-0.096**
	(0.022)	(0.103)	(0.056)	(0.057)	(0.041)	(0.036)
Male	-0.008	-0.005	-0.006	-0.008	-0.106	-0.007
	(0.009)	(0.008)	(0.009)	(0.010)	(0.096)	(0.009)
Parents with a university degree	0.182***	0.173***	0.170***	0.164***	0.168***	0.209*
	(0.022)	(0.020)	(0.021)	(0.022)	(0.031)	(0.105)
Higher income		0.109	0.238	0.152		
		(0.214)	(0.139)	(0.105)		
Tuition*Higher income		0.267**	0.122*	0.0203		
		(0.099)	(0.060)	(0.064)		
Male*Tuition					0.043	
					(0.070)	
Parents with a university degree* Tuition						0.046
						(0.063)
Time Fixed Effects	YES	YES	YES	YES	YES	YES
State Fixed Effects	YES	YES	YES	YES	YES	YES
Number of individuals	2143	2143	2143	2143	2143	2143

Notes: Further covariates in all models: Female, migration background, parents with a university degree, household equivalent income and unemployment/vacancies. Additional interaction terms in model 2-4: between Higher income and Treat, and Higher income and year dummies. Additional interaction terms in model 5: Male and Treat, and Male and year dummies. Additional interaction terms in model 6: Parents university and treat, and Parents university and year dummies. Clustered standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.7: Incorporate ability and education of parents

	(1)	(2)	(3)
		Reference group:	
	Poorest 10%	Poorest 20%	Poorest 30%
		Panel A: Baseline	
Tuition	-0.294**	-0.175**	-0.098
	(0.109)	(0.063)	(0.059)
Higher income	0.127	0.233*	0.156
	(0.195)	(0.129)	(0.096)
Tuition*Higher income	0.254**	0.134*	0.044
	(0.102)	(0.066)	(0.067)
		Panel B: Ability (proxied by scores)	
Tuition	-0.226**	-0.110	-0.033
	(0.103)	(0.084)	(0.086)
Higher income	0.162	0.250*	0.181*
	(0.208)	(0.128)	(0.099)
Tuition*Higher income	0.245**	0.136*	0.0423
	(0.093)	(0.066)	(0.073)
		Panel C: Ability (proxied by scores) & Education Parents	
Tuition	-0.217**	-0.076	0.012
	(0.081)	(0.105)	(0.124)
Higher income	0.175	0.246**	0.192**
	(0.208)	(0.103)	(0.077)
Tuition*Higher income	0.252***	0.127*	0.028
	(0.075)	(0.068)	(0.085)
Time Fixed Effects	YES	YES	YES
State Fixed Effects	YES	YES	YES
Number of observations	2143	2143	2143

Notes: Further covariates in all panels: Female, migration background, parents with a university degree, parents with a upper secondary leaving certificate, test scores, household equivalent income, unemployment/vacancies, higher income, interaction between Higher income and Treat, and between Higher income and year dummies. Further interactions in panel B and C: scores in mathematics and Treat, scores in mathematics and year dummies, scores in German and Treat, and scores in German and year dummies. Further interactions in panel C: between Parents university and Treat, Parents university and year dummies, Parents upper secondary leaving certificate and Treat, Parents upper secondary leaving certificate and year dummies. Clustered standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.8: Wild clustered bootstrap (p-values)

	(1) Full Sample	(2) Full Sample	(3) Full Sample	(4) Full Sample
Tuition	0.014	0.016	0.014	0.000
Male		0.903	0.422	0.398
Migration		0.697	0.971	0.985
Parents university			0.000	0.000
Household equivalent income			0.266	0.256
Unemployment/vancancies				0.336
Time Fixed Effects	YES	YES	YES	YES
State Fixed Effects	YES	YES	YES	YES
N	2143	2143	2143	2143

Notes: p-values are displayed.

Table 4.9: Robustness: excluding certain years

	(1)	(2)	(3)	(4)	(5)	(6)
		Without the year:				
	Full Sample	2002	2003	2004	2005	2012
Tuition	-0.075***	-0.062**	-0.075***	-0.063***	-0.072***	-0.074**
	(0.022)	(0.027)	(0.025)	(0.021)	(0.024)	(0.026)
Male	-0.008	-0.001	-0.010	-0.018**	-0.015	-0.002
	(0.009)	(0.010)	(0.013)	(0.008)	(0.011)	(0.012)
Migration background	-0.004	0.003	-0.003	-0.011	0.009	-0.008
	(0.030)	(0.030)	(0.034)	(0.029)	(0.034)	(0.031)
Parents with a university degree	0.182***	0.176***	0.172***	0.192***	0.185***	0.179***
	(0.022)	(0.022)	(0.019)	(0.022)	(0.023)	(0.022)
Household equivalent income	0.014	0.013	0.049***	0.013	0.014	0.013
	(0.013)	(0.013)	(0.011)	(0.013)	(0.013)	(0.013)
Unemployment/vancancies	0.003	0.003	0.003	0.004	0.0020	0.002
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)
Time Fixed Effects	YES	YES	YES	YES	YES	YES
State Fixed Effects	YES	YES	YES	YES	YES	YES
Number of individuals	2143	2000	1924	1995	1986	1986

Notes: Clustered standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.10: Robustness: further covariates

	(1) Full Sample	(2) Full Sample	(3) Full Sample	(4) Full Sample	(5) Full Sample
Tuition	-0.075***	-0.075***	-0.076***	-0.081***	-0.090***
	(0.022)	(0.022)	(0.021)	(0.020)	(0.027)
Male	-0.008	-0.008	-0.007	-0.007	-0.006
	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)
Migration background	-0.004	-0.004	-0.004	-0.004	-0.005
	(0.030)	(0.030)	(0.030)	(0.029)	(0.029)
Parents with a university degree	0.182***	0.182***	0.182***	0.182***	0.182***
	(0.022)	(0.022)	(0.021)	(0.021)	(0.021)
Household equivalent income	0.014	0.014	0.014	0.014	0.014
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Unemployment/vancancies	0.003	0.003	0.003	0.002	0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
CDU/CSU		-0.014	-0.012	-0.014	-0.014
		(0.038)	(0.037)	(0.036)	(0.036)
Educational expenditure/gdp			0.026	0.023	0.029
			(0.078)	(0.076)	(0.077)
High school graduates/population				14.31	13.40
				(24.31)	(24.36)
Announced tuition fees					-0.039
					(0.054)
Time Fixed Effects	YES	YES	YES	YES	YES
State Fixed Effects	YES	YES	YES	YES	YES
Number of individuals	2143	2143	2143	2143	2143

Notes: Clustered standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.11: Placebo reforms

Placebo tuition fees were introduced:	(1) 4 years earlier	(2) 3 years earlier	(3) 2 years earlier
Placebo tuition fees	-0.057	0.048	-0.001
	(0.039)	(0.045)	(0.052)
Male	0.023	0.023	0.022
	(0.026)	(0.026)	(0.026)
Migration background	0.007	0.005	0.006
	(0.025)	(0.025)	(0.025)
Parents with a university degree	0.170***	0.168***	0.168***
	(0.027)	(0.028)	(0.028)
Household equivalent income	0.002	0.002	0.003
	(0.008)	(0.008)	(0.008)
Unemployment/vancancies	0.004**	0.002	0.003
	(0.002)	(0.002)	(0.002)
Time Fixed Effects	YES	YES	YES
State Fixed Effects	YES	YES	YES
Number of individuals	1155	1155	1155

Notes: Clustered standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.12: Probit estimation

	(1)	(2)	(3)	(4)	(5)	(6)
		Reference group:				
	Full Sample	Poorest 10%	Poorest 20%	Poorest 30%	Male	Parents without a uni. degree
	Probit estimation: coefficients					
Tuition	-0.236***	-0.922***	-0.492***	-0.279	-0.320**	-0.289***
	(0.075)	(0.304)	(0.172)	(0.176)	(0.128)	(0.106)
Male	-0.029	-0.017	-0.021	-0.028	-0.336	-0.026
	(0.024)	(0.020)	(0.024)	(0.027)	(0.283)	(0.022)
Parents with a university degree	0.653***	0.629***	0.621***	0.601***	0.656***	0.556
	(0.073)	(0.069)	(0.076)	(0.079)	(0.072)	(0.389)
Higher income		0.249	0.758**	0.553*		
		(0.538)	(0.370)	(0.290)		
Tuition*Higher income		0.755**	0.286	0.023		
		(0.303)	(0.178)	(0.185)		
Male*Tuition					0.195	
					(0.216)	
Parents with a university degree* Tuition						0.111 (0.267)
	Probit estimation: average marginal tuition effect					
Reference Group	-0.072	-0.317	-0.176	-0.099	-0.097	-0.100
Non-Reference Group		-0.049	-0.058	-0.071	-0.038	-0.039
Time Fixed Effects	YES	YES	YES	YES	YES	YES
State Fixed Effects	YES	YES	YES	YES	YES	YES
Number of individuals	2143	2143	2143	2143	2143	2143

Notes: Clustered standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. *For unreported covariates see notes to Table 4.6.*

Chapter 5

Conclusion

This thesis analyzed educational choices from school start to university enrollment. It showed (i) how an educational decision impacts long-term health, and (ii) how institutional changes can alter educational decisions or aspirations.

Chapter 2 analyzed the effect of school starting age on long-term smoking behavior and long-term health status of adults. The main finding was that a younger school starting age leads to a higher probability to smoke and worse health status for adults. Furthermore, there is no significant effect on mental, but on physical health. The study discussed several potential mechanisms to explain how school starting age affects long-term health behavior and long-term health. Related studies demonstrated that being younger at school start increases the likelihood to attend a low school track (Jürges and Schneider, 2007; Mühlenweg and Puhani, 2010; Dustmann, 2004). Since the share of smokers is higher in lower school tracks, enrollment in a lower school track is a potential mechanism. Indeed, controlling for school track, we showed that the effect of school starting age on long-term smoking behavior and long-term health status is mitigated. However, altered track choice cannot explain the majority of the effect sizes. After excluding other potential mechanisms, the study suggests that relative age in the class room is an important factor via peer effects. Children who start schooling relatively early will be relatively young compared to their classmates. At the same time, the share of individuals who smoke increases with age. Therefore, at a given age, individuals who enroll in school early will have a higher share of smokers in the class room than individuals who enroll in school late. Indeed the study shows that a younger school starting age leads to younger friends in adulthood which suggests that younger school starting age leads to younger friends in childhood.

The policy conclusions that can be drawn from the results of Chapter 2 are not straightforward. A solution to mitigate the impact of school starting age on health behavior and health status would be to reduce the age dispersion in the class room. This would imply to introduce more than one school entry date in a given year. However, this solution is rather costly and might thus not be feasible. Nevertheless, the study shows that students who are born before and close to the school entry cutoff have a relatively high risk of developing negative health behaviors. These students at risk could be targeted by preventive measures such as extra counseling or extra-curricular activities. Preventive measures in school could lower the share of smokers and therefore reduce smoking-related health expenditures.

Future research could analyze how school starting age influences long-term smoking behavior and long-term health in more detail. This would be especially useful for policy recommendations. However, this additional analysis would require a data set with information about health and health behavior in childhood, youth and adulthood. Another question we could not address is effect heterogeneity. It would be particularly interesting to know, whether the effect of school starting age on long-term smoking behavior and long-term health status depends on the parental educational background. It is plausible that higher educated parents are more able to protect their children from risky health behavior than lower educated parents. Such a study would require a rather large data set, because the regression discontinuity design is very data demanding.

Chapter 2 showed that educational decisions can have long-term impacts. In contrast, Chapter 3 and Chapter 4 analyzed how educational decisions or aspirations can be altered by institutional changes. Chapter 3 showed that a shift from tracking after grade six to tracking after grade four in an early tracking system led to an increase in enrollment in the academic track. The analysis exploited an educational reform in the German federal state Lower Saxony in 2004. Since related studies often find that delaying tracking leads to an increase in educational achievements (e.g., Meghir and Palme, 2005; Pekkarinen et al., 2009; Malamud and Pop-Eleches, 2011), the result of Chapter 3 seems to be at odds with the existing literature. However, most studies do not study (i) the effect of a shift in an early tracking system and (ii) a shift to a more rigorous tracking system. Suggesting that the reform increased the grade repetition rate in the academic track, the results also indicate that the quality of students in the academic track seems to be hampered by the reform.

Countries usually try to promote educational achievements, because educational achievements are often associated with better labor market outcomes and also non-labor market outcomes like health status. For instance, the European Commission suggested to pursue a share of 40 percent of individuals with higher education among young individuals until 2020 (European Commission, 2010). Delaying tracking is often regarded as one instrument to promote educational achievements. However, Chapter 3 showed that tracking earlier in an already early tracking system promotes the share of individuals in the academic track. Thus the formula to track preferably as late as possible to support educational achievements does not hold in every case. Another rationale to track as late as possible is to promote educational achievements of individuals from low educated families to increase intergenerational educational mobility. Since the data of Chapter 3 cannot distinguish individuals according to the educational background of their parents, Chapter 3 cannot answer whether educational equality was supported by the reform. Thus this question should be addressed by future research.

Chapter 4 demonstrated that even a small tuition fee of EUR 1,000 per academic year has a negative impact on educational aspirations of 17-year-old high school students. Especially individuals from low income households are deterred by tuition fees. The Chapter exploited the introduction and abolishment of tuition fees in seven German federal states. The analysis applies difference-in-differences estimations. Federal states that did not introduce tuition fees serve as control group. The study also discusses potential mechanisms why educational aspirations of individuals from low income households are especially affected by tuition fees. One potential mechanism is that individuals from low income households perform worse in school. However, estimations with school grades as additional controls show similar results as the main estimations. Another potential mechanism are borrowing constraints, but studies show that borrowing constraints have only a small influence on university attendance in industrialized countries (e.g., Keane and Wolpin, 2001; Cameron and Taber, 2004). Further potential mechanisms are differences in financial literacy, educational preferences and discount factors between income groups. The impact of tuition fees on perceived net benefits from higher education might vary between individuals according to family income, because of these differences.

The results of Chapter 4 showed that the introduction of even a small tuition fee is at odds with the objectives (i) to promote educational attainment and (ii) to provide access to higher education independent of the family background. However, to charge tuition fees could still lead to higher equality, because students who benefit from higher education contribute to the funding of their studies. One way to mitigate the deterrence effect of tuition fees on educational aspirations would be to improve financial literacy in school. Another instrument would be to provide more scholarships.

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Declaration of Authorship

I hereby certify that this thesis has been composed by me and is based on my own work, unless stated otherwise. No other persons work has been used without due acknowledgment in this thesis. All passages that are taken out of publications or other sources, literally or in general, are marked as such. No part of this thesis has been submitted to this or any other university for another degree.

Hamburg, September 28, 2016

Michael Bahrs