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ABSTRACT

Students' Behavioural Responses to a Fallback Option: Evidence from Introducing Interim Degrees in German Schools*

Without a school degree, students can have difficulty in the labor market. To improve the lives of upper-secondary school dropouts, German states instituted a school reform that awarded an interim degree to high-track students upon completion of Grade 9. Using retrospective spell data on school and labor market careers from the National Educational Panel Study (NEPS), our difference-in-differences approach exploits the staggered implementation of this reform between 1965 and 1982. As intended, the reform reduced switching between school tracks. Surprisingly, it also increased successful high-track completion, university entrance rates, and later income, arguably by reducing the perceived risk of trying longer in the high-track school.

JEL Classification: I20, I24, I28

Keywords: school dropout, school degree, school tracking

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1 Introduction

In 1967, sociologists Ralf Dahrendorf and Hansgert Peisert published an influential report on dropouts from German high-track schools (*Gymnasiums*), which, with its finding that about 20% of high-track students drop out, alarmed politicians. In response, the Standing Conference of the Ministers of Education and Cultural Affairs of the Länder agreed to reform its high-track degree policy. The goal was to support dropouts from the high-track, who, although probably of high ability, struggled to enter the labor market due to lack of a school degree. Some states enacted the reform right away; others did so much later. The East German state Thuringia was the last to implement the reform, and did so only after a high-track school dropout went on a shooting rampage in the city of Erfurt in 2002. This tragedy led to media coverage of the state's failure to institute the reform, eventually pressuring politicians to act.

In this paper, we analyze this controversial reform of the German school system and find that it led to unintended behavioral responses by high-track students. Germany's school system features tracking and grade repetition. The school reform under study awarded high-track dropouts, that is, those who failed to complete 13 years of schooling, with a low-track school degree if they finished at least the ninth grade.[1] Before the reform, students who dropped out of the high track did not receive any degree and thus had to qualify for the labor market otherwise, for example, by switching to a low-track school and repeating a grade there. After the reform, students received a basic school degree if they finished ninth grade in the high track, and received a middle school degree if they completed Grade 10 of the high track.

Theoretically, such a reform could lead to various outcomes via the following four mechanisms. The first possible outcome is that the number of students who leave the school system without any school degree should decrease. This was the main goal of the reform. If high-track dropouts switched schools to obtain any other low-track school degree, the reform should result in reduced downgrading. Moreover, being awarded at least some sort of degree, regardless of time spent in school, should improve the students' labor market prospects in terms of em-

[1]The high track in Germany is the most time-consuming secondary school track, which grants students a degree at the end of Grade 13. In contrast, in the low tracks (*Hauptschule*, basic school, and *Realschule*, middle school) students obtain a low-track school degree after having finished either Grade 9 or 10. In recent years, the number of years of schooling required in the high track was reduced to 12. This change, however, took place much later than the reform under study.

ployment and wages (see Layard & Psacharopoulos 1974, Hungerford & Solon 1987, Jaeger & Page 1996).

The second and third possible outcomes involve incentives for students at risk of dropping out. On the one hand, the reform incentivizes students to leave the high-track school, and enter the labor market, after having completed either Grade 9 or 10. This would result in a lower number of students with a high-track degree, a higher number of students with a low-track degree, and younger labor market entry age.

On the other hand, the reform reduced the risk of staying in the high track. Parents who might have favored downgrading their low-performing child from the high track to the low track before the reform might now be more likely to keep the child in the high track. Given that the child will be awarded a low-track degree after successful completion of ninth grade, interpretable as *interim degree*, the reform reduced the risk of staying in the high track (on the relevance of risk aversion for schooling decisions, see Lavecchia, Liu & Oreopoulos 2016, Woelfel & Heineck 2012). Thus, students could be motivated to try hard to successfully finish the high track. This behavioral response to the reform would lead to an increased completion rate of the high track and, eventually, to a higher university enrollment rate.

The fourth possible outcome of the reform is that after it has been implemented and students and their parents are aware of it, selection into the high track could increase. The possibility of receiving a (low-track) school degree in the high track, even in case of failure after the ninth grade, reduces the risk of high-track enrollment. Therefore, parents who might have sent their low-performing child to a low-track school before the reform might now take these new opportunities into account when deciding on their child's secondary school, which could lead to an increase in the high-track school enrollment rate.

Our study estimates the causal effects of the German high-track reform and thus discovers which of the above presented outcomes is the most prevalent. For identification we exploit the different timing of reform implementation between 1965 and 1999 across 11 West German states[2] employing a difference-in-differences (DiD) framework. We compare high-track school students in a reform-implementing state before and after the reform and compare the changes in

[2]We do not include the East German states in our analysis as their education system underwent substantial change after Reunification.

student outcomes to the changes in high-track school student outcomes in states that did not reform at the same point in time. The inclusion of state fixed effects allows furthermore to abstain from any state-specific features that could affect student outcomes and the inclusion of time fixed effects controls for potential nation-wide shocks. Note that, in the baseline specification, we restrict the sample to students who were already enrolled in high-track schools at the time of the reform in order to rule out reform-induced selection into high-track schools.

For estimation, we use rich and detailed survey data from the National Educational Panel Study (NEPS) adult cohort from 2010 (for more detail on the data, see Blossfeld, Rossbach & von Maurice 2011). The NEPS data have two specific features that enable us to tackle our research question in a methodologically clear-cut manner. First, the data include information on the nature of dropping-out: that is, we can differentiate between whether a student left school for good, whether the student switched to another school (i.e., another school track), and at which point in time the student effectively ended her schooling career.[3] Second, the NEPS data contain information on the state in which the student attended school as well as his or her state of current residence. This information is very important for an analysis of state reforms in a context where migration across states during an individual's lifespan is possible.

Our results show that the reform was successful, at least in the sense that students are less likely to switch between schools and tracks. This reform effect is thus in line with the first theoretical consideration. The results, however, do not support the second theoretical consideration. That is, we do not find that more students leave school with a low-track school degree to enter the labor market earlier. Instead, we find that students, rather surprisingly, stay in the school system longer and finish school with a higher school degree due to the reform. This supports the third theoretical consideration. The reform seems to reduce the perceived risk of trying longer in the high-track and thus leads to an increase in the high-track completion rate and in university enrollment rates. In line with this finding, we find significant reform effects on net household income in 2010. We hardly find evidence for the forth theoretical consideration as the likelihood of enrolling into the high track does not significantly increase after

[3]This is in contrast to other studies that rely on school data reporting how many students left a school without degree but with no information as to the subsequent educational path of the reported dropout. Usually, dropout is not well defined as students are not tracked over time.

the reform. When it comes to equal opportunity effects of the reform, we find that students from a low socioeconomic background benefit more from the reform than students from a high socioeconomic background.

Our study relates to four strands of the literature. First, similar to a growing literature in the economics of education, we employ the DiD approach to evaluate how an education policy reform affects student outcomes. Leading examples of DiD applications that exploit differences in the timing of educational reform implementation across regions within a country include the following: Acemoglu & Angrist (2001) evaluating compulsory schooling laws in the United States, Meghir & Palme (2005) studying a comprehensive school reform in Sweden, Pekkarinen, Uusitalo & Kerr (2009) looking at a tracking reform in Finland, Havnes & Mogstad (2011) estimating the effects of a child-care expansion reform in Norway, and Eyles, Hupkau & Machin (2016) studying a school autonomy reform in England. The DiD approach has also been used to study reforms of the German school system. Pischke (2007) and Pischke & von Wachter (2008), for example, studied German compulsory schooling reforms.[4] Analogously to this literature, we, too, make use of variation in the time of implementing the high track reform across states and time.

Our focus on a reform that changed the opportunities of (potential) drop-out students in the high track contributes to the literature on tracking and on dropouts. The extant literature on tracking finds tracking systems to increase educational inequality, for example, Piopiunik (2014), Hanushek & Woessmann (2006), yet there is no evidence to date on how the possibility of being granted an interim degree from a high-track school influences the number of years individuals stay in school. It might be that especially risk-averse students try to finish the high track when they are offered an interim degree, in which case the reform under study may be working as a remediating tool in a school system that uses tracking, similar to the benefits of up- and downgrading in a tracked system found by Dustmann, Puhani & Schoenberg (2017). The literature on school dropouts frequently focuses on dropouts from the low track (i.e., lower

[4]German compulsory schooling reforms have also been studied by Siedler (2010), Piopiunik (2014), and Kamhoefer & Schmitz (2016). Another German school reform has been studied by Riphahn (2012), who investigates the abolition of upper-track school fees in 1950. A recent reform shortening the number of required years in high track schools has been studied by, among others, Thiel, Thomsen & Buettner (2014), Dahmann & Anger (2015), Andrietti (2015), and Huebener & Marcus (2017).

secondary school) who often end up without any school degree and leave the school system for good (e.g., Oreopoulos (2007); Anderson (2014); Dearden, Emmerson & Meghir (2009)). Our study, in contrast, focuses on the fate of high-ability high-track dropouts, which is more comparable to school career interruption studied by DesJardins, Ahlburg & McCall (2006). The latter find that college students who have dropped out once are more likely to experience subsequent enrollment disruptions, which are found to be detrimental to the students' probability of graduating. We contribute to the dropout literature because we can reconstruct the whole school and labor market careers of individuals surveyed in our data and link these developments to reforms on the state level. We are thus able to distinguish between real dropouts and individuals who are simply interrupting their schooling career.

The fourth strand of literature to which we contribute is the emerging literature on behavioral aspects of the economics of education. One study closely related to ours is Heckman, Humphries, LaFontaine & Rodriguez (2012), who look at the GED Testing Program in the United States. This program allows dropouts to attain a degree via the GED, which is a high school diploma equivalent. Although, no doubt, this program was begun with good intentions, the authors find that it actually created incentives to drop out from high school. These findings show the importance of considering the riskiness of decisions in the educational system when investigating student behavior. In the same vein, some other studies show that students' (and their parents') risk aversion and time preferences are important factors in the education production function (e.g., Golsteyn, Groenqvist & Lindahl (2014); Woelfel & Heineck (2012)). Consequently, behavioral incentives in the education system that address these factors can play an important role in educational careers and labor market outcomes. For example, Levitt, List, Neckermann & Sadoff (2016) show that student effort is increased by immediate rewards for performance (for an extensive overview of behavioral economics of education, see Lavecchia et al. 2016). In view of this literature, the German school reform under study that introduced an interim degree in the high track can be interpreted as a low-cost intervention that encouraged trying longer in the high track.

This study proceeds as follows. In Section 2 we provide information on the institutional background of the German school system and, in particular, on the reform under study. Section

3 presents the main data sets and provides descriptive statistics. Section 4 explains the empirical framework. Section 5 presents the results and Section 6 investigates the reform effects on the equality of opportunity. Section 7 discusses a series of robustness checks and potential mechanisms. Section 8 concludes.

2 Institutional Background

The institutional set up of Germany's education system permits a detailed analysis of the dropout reform's effects on high-track school students' behavior, including school switching, track down- and upgrading, and grade repetition. In the following, we provide a brief overview of the German school system as it existed for the West German birth cohorts under study and describe the reform and its timing in detail.

The German School System Germany's school system is decentralized, meaning that each of the country's 16 states is responsible for the education of its youth. Although there are some differences across states, the general structure is fairly uniform and illustrated in Figure 1. The school system tracks children at age 10, after four years of primary school, into mainly three secondary school tracks.[5] The different tracks are characterized by the academic requirements and the final degree awarded. In the basic school (Hauptschule), a student must pass five grades before compulsory schooling ends at ninth grade. The basic school education terminates in a basic school degree that, traditionally, enables students to go on to vocational training (Helbig & Nikolai 2015). In the middle school (Realschule), a student must pass six grades and finishes with a middle school degree. The middle school degree opens up more vocational training opportunities. In the high track (Gymnasium), students must pass nine grades and finish with an academic school degree (Abitur), which is the university entrance qualification.[6] Tracking is based on ability. Teachers in primary school recommend the highest school track they think is suitable for the child. In some states, this recommendation acts as a top limit to the schooling

[5]In two states, Berlin and Brandenburg, tracking takes place at age 12 after six years of primary school.

[6]Helbig & Nikolai (2015) provide insight into various dimensions of Germany's school system, including at which stage each type degree can be obtained.

available to the child. Parents have the responsibility of choosing the child's secondary school track from the (limited) set of available school tracks. Upgrading from lower tracks into higher ones is generally possible when the attended lower track is completed successfully. Students may also downgrade to a lower track at any time. After secondary school, individuals may either go into vocational training, which consists of classes at a vocational school as well as practical training from an employer, or enter tertiary education.[7]

In evaluating the reform it is important to remember that high-ability children are tracked into the high track. Nonetheless, it is possible to dropout before obtaining a school degree. The high track takes the longest to accomplish and is the most demanding academically.

The Reform Under Study In the 1950s and 1960s there was no legal regulation of the school degree high-track students could receive when dropping out from the high track.[8] Figure 2 shows that close to 10% of all students who attend the eighth grade in high-track schools drop out in all birth cohorts from 1950 onward; in Baden-Württemberg, during the same decades, more than 20% of students dropped out from the high track without a degree (Peisert & Dahrendorf 1967). The first regulations in regard to degrees for dropouts solely dealt with those students who did not achieve the high-track school degree in 13th grade. It was not until 1965 that regulations on degrees for dropouts from lower grades began to be discussed. In 1965, the *Kultusministerkonferenz* (the Standing Conference of the Ministers of Education and Cultural Affairs of the Länder) announced a reform making school-leaving diplomas from the high track comparable to school degrees from lower tracks.[9] The goal of the reform was to make vocational training available to dropouts. Although the reform was announced at the federal level in 1965,

[7]See Biewen & Tapalaga (2017) and Buchholz & Schier (2015) for a good overview and description of school careers in the German school system using the NEPS adult cohort data.

[8]There is some evidence for Baden-Wüerttemberg that the custom was to count certificates for having passed 10th grade as a middle school degree, at least for some occupations, for example, in the public sector. Other states apparently did not engage in the same practice.

[9]The original name of the reform was "Gleichwertigkeit von Abgangszeugnissen der Realschulen und Gymnasien mit Abschlusszeugnissen der Hauptschulen" ("Equality of School Leaving Diploma of Middle and High-Track Schools with Basic School Degrees"). Thus, students in the middle track schools were also subject to the reform. Additionally, changes in the school careers of high- and middle-track students might have had spillover effects on students in the low-track schools. We examine this issue in more detail in Section 7.

the German states enacted it at different points in time; see Table 1 (Helbig & Nikolai 2015). The first state to reform was Baden-Württemberg, Bavaria reformed only in 1982, and the states of East Germany reformed after Reunification. Thuringia was the last to implement the reform and did so only after a high-track school dropout went on a shooting rampage in the city of Erfurt in 2002. This tragedy led to media coverage of the state's failure to institute the reform, eventually pressuring politicians to act. In some states (e.g., Berlin), the reform granted low-track school degrees retroactively.

The reform aimed at supporting those students who, although probably of high ability, struggled to enter the labor market due to lack of a school degree. Previously, students generally had to obtain a low-track school degree via some other means before entering the labor market. Our analysis focuses on implementation of the reform in West Germany between 1965 and 1982. We exclude the former East German states as the school systems in this section of the country were not completely comparable during the period of study. Students from Bremen, where the reform took place in 1999, are observed only in the untreated state.

As we are interested in evaluating whether the reform improved the situation for dropouts, we would like to know what students' outside options were when dropping out of school without a degree. As dropouts are rarely tracked over time, there is little knowledge about pre-reform outside options. There is some narrative evidence from Peisert & Dahrendorf (1967) in regard to the state of Baden-Württemberg. Peisert & Dahrendorf (1967) interview students from high-track schools and track dropouts for some years after they left the high track. They find that the majority of dropouts are still going to school during the years after dropout in an attempt to obtain either a middle- or even a high-track school degree. The second biggest group of dropouts are employed in public service or trade occupations that count a 10th grade certificate as a middle school degree. Peisert & Dahrendorf (1967) mention the following reasons for dropout in the early 1960s before obtaining a high-track school degree: personal reasons, problems in school subjects, family reasons, the school, teachers, the peer group in school, and commuting to school. Personal reasons include that students do not want to go to school anymore or that they have decided on an occupation for which an academic degree is not necessary. Problems in school subjects was the most frequently stated reason for dropout.

3 Data

We combine two unique data sources to estimate the reform effects. First, we make use of rich and detailed survey data from the adult cohort of the National Educational Panel Study (NEPS) from 2010.[10] We extract attendance at the high track without achieving a school degree and our education-related outcomes from spell data on individual schooling histories. Second, we use the reform years reported in Table 1 as the starting point for the treatment. We create a treatment indicator from the information provided by Helbig & Nikolai (2015) that shows whether or not children who dropped out of the high track received a low-track school degree.[11]

The NEPS data provide detailed information on the schooling histories of a representative sample of adults living in Germany. The data have two specific features that enable us to tackle our research question in a methodologically clear-cut manner. First, the data include information on the nature of dropping out: that is, we can differentiate between whether a student left a school for good, whether the student switched to another school (i.e., another school track), and at what point in time the student effectively ended her schooling career.[12] Second, the NEPS data report the state in which the individual attended school. This is crucial for an analysis of reforms in a context where migration across states during an individual's lifespan is possible.

From the NEPS adult cohort we use a subsample of 3,878 adults who attended secondary school in a West German state (including Berlin) and were born between 1944 and 1983. In 2010, NEPS asked all participants about their school careers, documenting school types and locations of schools. We use these spell data to construct our main outcome variables, which are intended to capture important characteristics of school careers. Table 2 shows descriptive statistics for these outcome variables. The first variable *years of schooling* states the number

[10] See Blossfeld et al. (2011) for more detail.

[11] The indicator does not distinguish between basic and middle school degrees as the type of degree varies greatly by state thus it would have been too complex to appropriately document this difference (Helbig & Nikolai 2015).

[12] This is in contrast to other studies that rely on school data reporting how many students left a school without a degree but have no information about the subsequent educational path of the reported dropout.

of years a person reported being in educational spells. The variable *longer in education than standard* is an indicator equal to 1 when a person stayed in the education system longer than would have been expected given her final degree. It is the difference between *years of schooling* and calculated standard years of schooling from the last CASMIN.[13] The variable *no degree in last school episode* is an indicator equal to 1 when a person leaves the school system at a maximum of age 19 without reporting having obtained a degree in the last episode. When a person reports having obtained a basic school degree in the last school episode, the indicator *basic school degree in last school episode* is equal to 1. *Downgrading* is an indicator equal to 1 when a person switches from the high track to a lower-ranked school, that is, basic or middle school.[14] Similarly, *upgrading* indicates whether a person switched from a lower-ranked school type to the high track. The *number of school episodes* counts how many schools a person attended and *age at leaving school system* is the age at which a person switched from school episodes to vocational training (including university education), employment, military service, or other types of spells.

The variables for school degrees are indicators as well, capturing reported school degrees at two different points in an individual's life. The first point is at the end of the last school episode, that is, between ages 11 and 19. The second point is in 2010, which should capture highest achieved school degree. The indicators differ as people can obtain school degrees later in life.

The occupational outcomes capture whether an individual holds a vocational or university degree in 2010 and the logarithm of the net household income in 2010. The vocational degree indicator is constructed from the CASMIN.

From the background questionnaire we construct control variables, including: sex, birth cohort, migration background, highest level of parental education, and number of siblings. We

[13]The CASMIN (Comparative Analysis of Social Mobility in Industrial Nations) has collected statistics about education since the 1980s.

[14]We cannot distinguish between these school types in the spell data.

construct three-year bins as birth cohort dummies - each including three successive birth cohort years.[15] Table 2 contains summary statistics for the outcome and control variables.

4 Identification Strategy

To identify the causal effect of the dropout reform, we exploit the different timing of reform implementation between 1965 and 1999 in the 11 former West German states and employ a DiD approach. Our DiD approach relies on a comparison of students in the highest track pre- and post-reform within and between states. For the baseline specification, we restrict the sample to include only students who had a high-track episode during their school career. One issue that arises in evaluating reforms in general is selection into treatment. As soon as people know that the high track can grant a lower-track school degree, selection into the high track could change. To avoid selection effects, our treatment is not only the interaction of age and attending at least the high track once in life; we define our treatment group as high-track students who are 11-19 years old in the reform year. This restriction puts into the control group 93 students who chose to attend the high track after the reform at an age older than 11. Figure 3 shows our treatment and control groups for the 11 West German states. Black represents the cohorts at age 11 that are treated. White represents the pre-reform control group. Gray is the treatment group we exclude due to possible selection effects. In our main specification we estimate:

$$Y_{i,s,t} = \beta_0 + \beta_1 post\ reform_s + X_i\beta_5 + \mu_s + \mu_t + \varepsilon_{i,s,t}. \tag{1}$$

$Y_{i,s,t}$ are the various outcome measures for school- and occupational-career characteristics of student i in state s of birth cohort t. The coefficient of interest is β_1, showing the effect of the reform for students who attend the academic track in the reform year. The matrix X_i contains all control variables. State and birth cohort fixed effects are captured by μ_s and μ_t, respectively. In our main specification, standard errors are clustered at the state level as the treatment varies

[15] We do so because the number of observations per year of birth is small. Including dummies for each year of birth does not change the results.

at that level.[16]

The coefficient β_1 represents the causal effect of the reform if the assumptions of a DiD framework hold. Note that, in our setting, the common trend assumption has to hold, which implies that – in absence of the treatment – treatment and control groups would both lie on the same longer-run trend with respect to the outcome variables. The DiD literature often argues that this assumption holds with the same pre-trends before the reform. The NEPS data do not allow a thorough evaluation of pre-reform trends as the number of observations per birth cohort and state are small and decrease with age of the survey participants. Shares of people with a specific school degree, therefore, fluctuate a great deal by birth cohort and state. In Section 7 we provide several robustness checks of our identification strategy. We perform placebo tests, include linear state trends, and leave one state out at the time. A second crucial assumption of our approach is that the treatment effect we capture with β_1 should not represent any development other than the reform. To avoid capturing other developments within states that occurred at the same time as the reform under study, we investigated whether other education reforms affecting secondary schooling were introduced simultaneously and found no evidence that there were.

5 Main Results

In this section we describe and discuss the results from estimating Equation 1. First, we consider the reform's direct effects on the school careers of affected students. Second, we estimate effects on the school degrees they obtain. Third, we estimate whether there are any longer-run effects on occupational outcomes. The effects reported in this section are average treatment effects for the group of high-track students. Finally, we estimate the reform's effect on selection into the highest track.

[16] As we have only 11 German states, the number of clusters is small. We apply wild-cluster-bootstrap methods to account for this.

5.1 School Careers and Degrees

We expect to find that the reform resulted in less switching between school tracks by high-track students because, after the reform, they can obtain a lower-track school degree while attending the high-track school. This feature of the reform should reduce down- and upgrading. As to time spent in school, number of school episodes, and age at leaving the education system, the reform effect depends on the relative role of the following two incentives: (1) the incentive to leave school and enter the labor market before obtaining a high-track school degree and (2) the incentive to stay in the high-track school and try to finish 13 years of schooling with less risk of leaving school without any degree.

Table 3 shows that, overall, the reform affected the school careers of students in the highest track as expected, in that their school careers became more stable and linear. Students spend almost one year more in school (Column 1), which is about 70% of a standard deviation, meaning that they are about half a year older when leaving the school system (Column 3). It is not likely that this is due to students repeating grades more often after the reform as excess time in the highest track does not increase significantly (Column 2). These findings together indicate that students drop out later or finish school more often due to the reform. This is in line with a stronger incentive for trying to finish high-track school at lower risk. Additionally, school careers stabilize: students reduce their school episodes (Column 4) by half an episode, which is 60% of a standard deviation. This means that they switch less often between different schools. This is also true for downgrading, that is, switching to a lower-track school (Column 5). Downgrading is reduced by almost 90% of a standard deviation. Upgrading is slightly reduced as well, albeit not significantly. As students downgrade less often they logically cannot upgrade to the highest track as often as before. These results are robust to wild-cluster bootstrapping of the standard errors, as recommended by Cameron & Miller (2011). The third row of the table shows the p-value for testing whether the reform coefficient is zero, obtained from wild-cluster bootstrapping the standard errors. The significance levels hardly change when employing wild-cluster bootstrapping methods.

The treatment group contains students aged 11 to 19. Results thus might be driven by a specific age group of students as it is possible, for example, that the incentive to leave school and enter the labor market earlier is stronger for the older students than for the younger students. If this is the case, we would expect that the effects we find are driven by the younger students and might go into another direction for the older ones. Table 14 in the Appendix demonstrates that this is not the case. The effects always go in the same direction for all three age groups: ages 11-14, ages 15-17, and ages 18 and older.[17] The significance levels are also almost the same for the different age groups. This suggests that the reform unlikely incentivized students to enter the labor market immediately.

One of the reform's goals was to provide students who otherwise would not have obtained a school degree with a school degree. Therefore, we expect that the share of people without any school degree decreased under the reform while the share of people with school degrees should increase (particularly for low- or middle-track school degrees). Table 4 sets out the reform's estimated effects on the likelihood of holding various school degrees. Columns 1 to 4 use information from the retrospective school spells about school degree obtained in the last reported school episode. Columns 5 to 8 use information on the highest school degree obtained by 2010. Column 1 shows that the likelihood to end the last reported school episode without any school degree is slightly, yet not significantly, reduced. This suggests that even before the reform, high-track dropouts rarely completed their school career without obtaining any degree. It is more likely and in line with anecdotal evidence that high-track dropouts obtained a lower-track school degree at a lower-track school and finished their school career with such a degree. Indeed, we find that after the reform, students are less likely to finish their school careers with either a basic school degree (Column 2) or a middle school degree (Column 3). They are, in fact, more likely to finish their school career with a high-track school degree (Column 4). The same pattern holds for the highest school degree obtained by 2010 (see Columns 4 to 8). People who attended the highest track in the reform year hold significantly fewer middle school degrees but the share of people with a high-track degree increased by 60% of a standard deviation compared to people attending the high track before the reform. Again, the significance levels hold when applying wild-cluster bootstrapping methods (third row). In sum, Table 4 provides evidence

[17]The exception is upgrading, which, of course, is very unlikely for the older students as they would have to downgrade first and then upgrade.

that people are incentivized to stay in the high track and try to obtain a high-track school degree with a reduced risk of ending- up without any school degree.

The reform thus seems to motivate students to stay in the high track even if they are at risk of not completing it. Staying in the high track is surprisingly beneficial for students. As the estimates show, many of them actually obtain the high-track school degree and thus qualify for university and, consequently, a wider choice of career paths. Therefore, in the next section we investigate whether there the reform had longer-run effects on occupational outcomes.

5.2 Occupational Outcomes

More years of schooling and higher degrees should have a positive effect on students' labor market careers. Table 5 shows the reform's effect on occupational outcomes. Because the reform pushed students away from obtaining lower school degrees, the students are also less likely to hold a vocational school degree (Column 1) but more likely to obtain a university degree (Column 2). This push into higher education might be responsible for the higher net household income we observe in 2010 (Column 3). The first observation period in 2010 is not the optimal point in time to measure monetary returns to the reform as the control group in our sample is older and therefore more likely to be retired than the treatment group. Therefore, in the estimation equation of Column 3 we control for age and age squared in addition to our usual controls.

5.3 Selection into the Academic Track

As the reform reduced the risk of dropping- out of the high track without any school degree, we expect that (lower-ability) students should select into the high track. Lavecchia et al. (2016) and Woelfel & Heineck (2012) show that risk aversion plays an important role in schooling decisions. Relaxing our sample restriction from above to include students who attended the high track in the reform year, we analyze whether the share of students attending the high track at different points of their school career increased with the reform. Table 6 shows that after the reform, a higher share of students attended the high track – independent of measuring attending the high track as a dummy for having at least one high-track school episode, as attending the high track as the first secondary school, or as attending the high track in eighth grade. However, the positive effect is only slightly significant for the likelihood of having any high-track school episode.[18] The effect size is small; 5 percentage points, which is about a 10th of a standard deviation. One explanation for the small selection effects into the high track after the reform might be that at the time of the reform, parents were rarely allowed to choose which secondary school track their children would attend. Only in Bremen and Lower Saxony, and only since the late 1970s, did parents have more freedom of choice in this matter.

6 Reform Effect on Equality of Opportunity

The reform was intended to provide high-track dropouts with better labor market opportunities. Estimating average treatment effects, therefore, might attenuate the effect the reform has on the target population: those students at risk of dropout. Ex-post it is hard to define the group at risk because not all members of that group actually drop out. Indicators such as grades or repeating a grade are not documented in our data. In the following, we investigate the extent to which students with parents without a high-track degree profited from the reform differently than students with parents with a high-track degree. Dropping- out of the high track might be especially likely for those children who do not have strong parental support to pursue a high-track school career. Therefore, the group of students with parents without a high-track degree might belong to the reform's target population. However, there are only a few students with parents who hold a high-track degree.

Comparing the results in Panel A of Table 7 with those in Panel B shows that all coefficients go in the same direction for both groups. Also, the magnitude is mostly comparable except for excess time in education (Column 2), which is negative and insignificant for students of parents with a high-track school degree. Overall, effects are larger and therefore more significant for students with parents who do not hold a high-track degree.

[18]When we apply robust standard errors instead of clustered ones, the effects are significant for all three measures.

To detect differences in final school degrees, Panel A of Table 8 needs to be compared to Panel B. Again, the effects go in the same direction for both groups. Yet, the other estimates – particularly the reform's estimated effect on achievement of a high-track school degree (Columns 4 and 8) – is twice as large for students with parents without a high-track school degree. These results suggest that the reform was especially beneficial for students with parents who do not hold a high-track school degree.

7 Robustness Checks and Mechanisms

In this section we address some identification issues of our DiD approach and relax our sample restriction. First, we perform placebo tests, second, we include linear state-time trends, and, third, we exclude one state at a time. Fourth, we do not restrict the sample to high-track school students and include everybody who is 19 or younger in the year of the reform in the treatment group. Fifth, we investigate the reform effect for the middle- and basic-track students only, and look at the heterogeneity of the effect size for different age groups of high-track students.

7.1 Placebo Test

One major issue with our identification strategy is that the effect might not be driven by the specific reform but by other developments we do not observe. As we only compare means before and after the reform, it is possible that even developments that do not exactly coincide in timing with the reform under study are driving our results. To check whether this is the case, we estimated placebo reform effects for 10, 5, and 2 years prior to the reform in each state. Table 9 shows placebo reform effects for 10 years prior to the reform in Panel A, for 5 years prior to the reform in Panel B, and for 2 years prior to the reform in Panel C. The pattern we observe for the true reform year cannot be observed in any other year. When the coefficients are significant, the effect goes in the other direction. Most effects, however, are very close to zero. Although our sample is reduced by half - as earlier birth cohorts are less densely sampled - standard errors stay fairly comparable to the standard errors of our main results. These findings confirm that we are not capturing -any other unobserved developments with the reform effect and provide evidence that we identify the true reform effect.

7.2 Linear State Trends

In DiD approaches it is possible that the reform effect is capturing -state-specific time trends and, if so, the effect cannot be interpreted as a causal one. To ensure that state-specific time trends are not driving our results we include linear state trends in Equation 1. The results are shown in Table 10. The table shows that our main findings of increased stability of school careers and a push into more high-track degrees are robust to the inclusion of linear state trends.[19]

7.3 Leave One Out

German education policy, in general, varies by state. It is thus possible that our reform effects are driven by specific states. To ensure that no single state is driving our results, we estimate all regressions in a restricted sample leaving one state out at a time. We report results only for the largest states, which contribute most observations to our analysis: Northrhine-Westphalia and Bavaria. Additionally, we show results for leaving out Berlin, a part of which was in East Germany at the time of the reform. Panel A of Table 11 shows the results for years of schooling and the number of school episodes. Panel B shows the results for downgrading and holding a high-track degree. The magnitudes of the coefficients hardly change and the effects stay significant. This holds as well for leaving out any other state (not shown here).

7.4 Effect on the Whole Population

To this point, we have identified reform effects for only a very small subgroup of the whole population of students within a state, namely, students in the high track. There are two arguments in favor of estimating an overall effect. First, the reform also targeted students in middle-track schools who finished the ninth grade and granted them a basic school degree. Second, there might be spillover effects on students from other school tracks. Resource constraints could pre-

[19]We can also include non-linear state-decade- fixed effects with the first decade of people born before 1950, the second people born between 1950 and 1959, the third people born between 1960 and 1969, the fourth people born between 1970 and 1979, and the fifth people born after 1979. These decade dummies are interacted with the state dummies. Our results do not change if we include the state-decade dummies.

vent high-track schools from becoming over-enrolled, whereas middle-track schools may try to attract students from the low-track schools to fill their seats. On the local labor markets it is possible that high-track school dropouts with a school degree crowd- out students from the middle-track schools. Table 12 shows that for the whole population the share of people without any school degree decreased significantly with the reform; all other effects, however, are close to zero and insignificant. This suggests that effects might go in different directions for different subgroups of students.

7.5 Heterogeneity and Subsamples

Finally, we investigate the reform's effect on the subgroup of students in the middle- and basic-track schools, as well as which age groups drive our effect for high-track students. Table 13 shows that the subsample of students who never attended the high track is larger than the high-track sample by about $2,500$ individuals. For these students, the reform effects are very close to zero and only significant for downgrading. First, this is evidence that there are no other reforms or unobserved developments coinciding with our reform that affect all students in general. Second, high-track students are the relevant treatment group and our reform effect can be interpreted as a treatment effect on the treated.

The treatment group consists of students between 11 and 19 years of age. Students in 10th grade or higher could leave the high track with a basic or middle school degree immediately after the reform, whereas students below 10th grade still had to successfully finish at least ninth grade. This difference in the treatment might lead to differences in the reform effect. Older students might be incentivized to leave school early while younger students might be encouraged to stay. In Table 14 we distinguish between three age groups: students aged 11 to 14, aged 15 to 17, and aged 18 or older. The results show that the effects go in the same direction for all age groups.[20] We interpret these findings as meaning that the reform incentivized staying in school longer in general.

[20] The exception is *upgrading*, which shows a positive coefficient for the reform dummy. However, students aged 18 or older would not down-grade and then upgrade back to the high track anymore. It is plausible that the effect is zero for this group.

8 Conclusion

In the context of the German school system, we investigate the effects of a school reform that rewarded high-track school dropouts with a low-track school degree if they at least finished ninth grade. For identification, we exploit the fact that the reform was introduced in 11 West German states at different points during 1965 to 1982. Detailed retrospective spell data on school and labor market careers from the NEPS allow us to identify exactly who attended the high track, regardless of receiving a degree, and in which federal state. Both are features many other data sets lack. Our results show that the reform was successful, at least in the sense that students are less likely to switch between schools and tracks.

Surprisingly, however, we also find that the reform led to an increase in the number of high-track students who successfully finish high school and enter university. We furthermore find a positive effect on the students' income in later adult life. This is in line with theoretical considerations on risk perception that changes students' motivation: the reform seems to have reduced the perceived risk of trying longer in the high-track school and thus led to an increase in the high-track completion rate. The reform thus can be interpreted as a low-cost intervention that had a beneficial effect on students.

It could be argued that this study's very specific context limits the international policy conclusions that can be made. The dropout reform took place in a system with tracking and grade repetition, and where having a degree hold strong implications for labor market entrance. Thus, this reform might not work in another country's school system. However, our results reveal one important insight that is of importance to education policies in general: along the school career, rewarding each milestone achieved can motivate students to try longer and harder for success with the next milestone. A prominent example of this concept is the European Bologna reform that introduced the bachelor degree as a reward for university graduation. The results of our evaluation are evidence that rewarding milestones achieved can indeed change students' behavior and improve their student and life outcomes.

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Tables and Figures

Figure 1: The Structure of the German School System

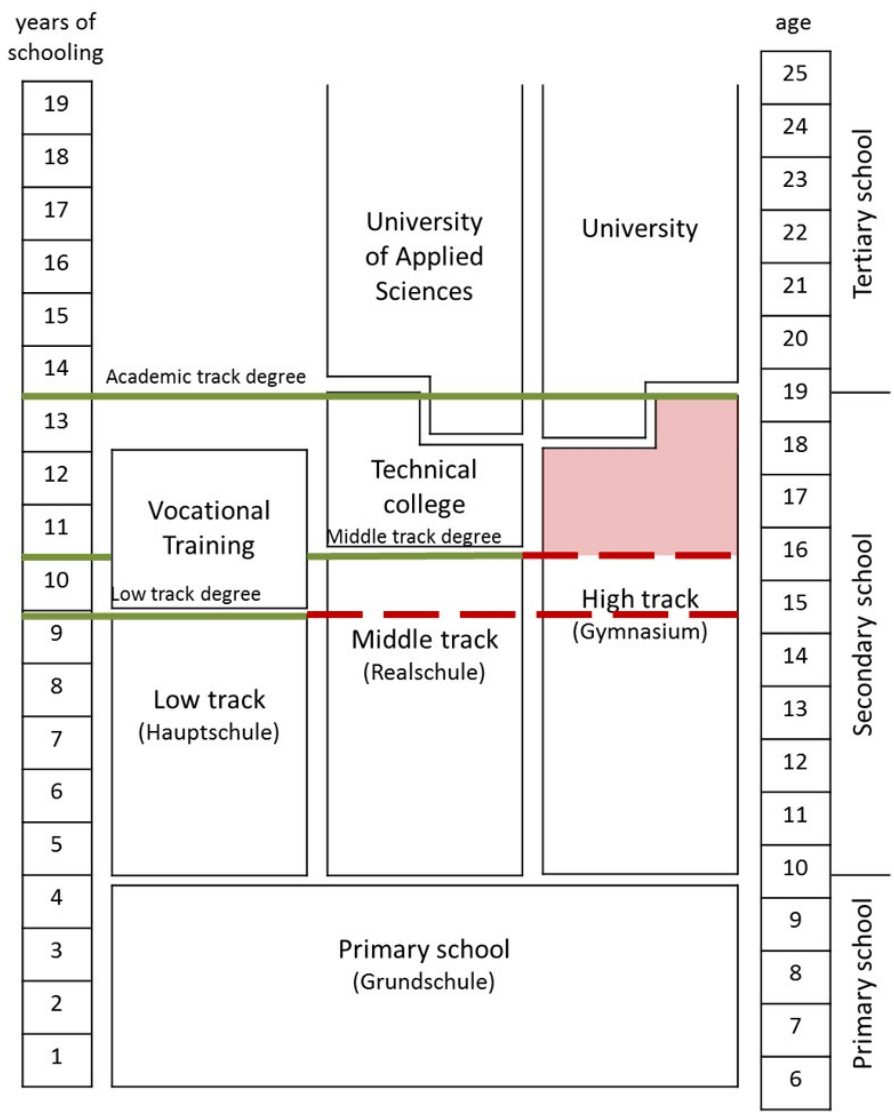
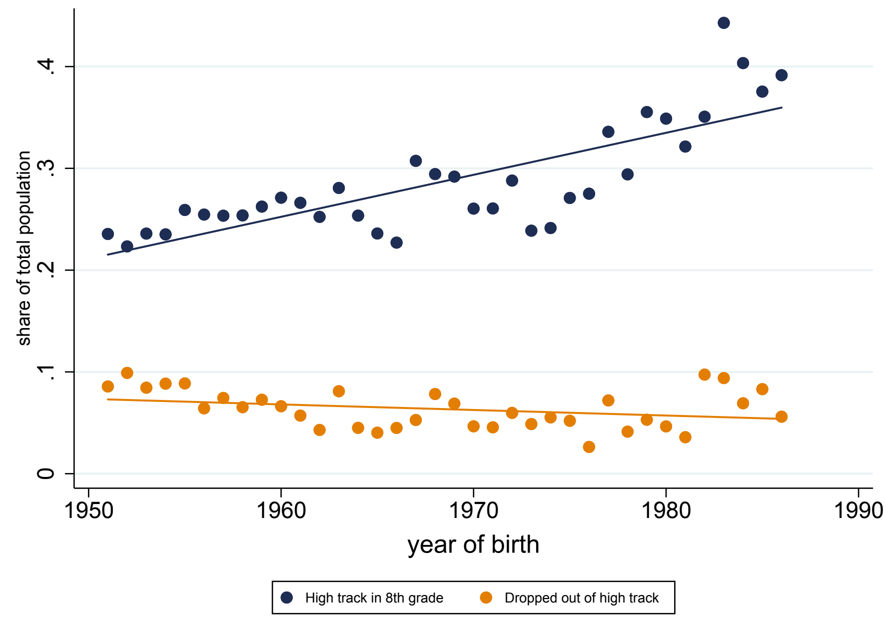
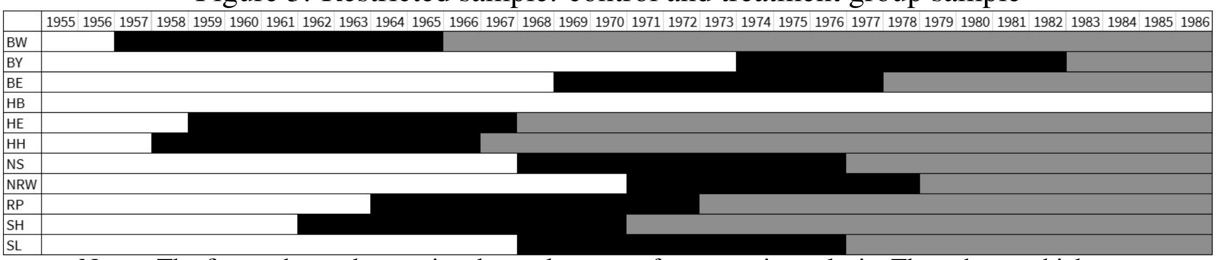


Figure 2: Dropouts from the high track in Germany



Notes: The blue dots show for birth cohorts from 1950 to 1986 the share of people who attended the high track in Germany at age 13/14 (8th grade). The orange dots shows for the same birth cohorts the share of people who dropped out from the high track among the people who attended the high track in 8th grade. The respective lines are the linear fit of the raw data points. Data source: NEPS SC6:7.0.0. Own calculations.

Figure 3: Restricted sample: control and treatment group sample



Notes: The figure shows the restricted sample we use for our main analysis. The cohorts which turn 11 in the specific year are pre-reform cohorts if the cell is white and belong treatment group not prone to selection if the cell is black. Grey cells are the birth cohorts which belong to the treatment group prone to selection and are excluded from our analysis. Source: (Helbig & Nikolai 2015).

Table 1: Timing of the dropout reforms

state	reform
Baden Wuerttemberg	1965
Hamburg	1966
Hessen	1967
Schleswig Holstein	1970
Rheinland Pfalz	1972
Niedersachsen	1976
Saarland	1976
Berlin	1977
Nordrhein Westfalen	1978
Bayern	1982
Brandenburg	1991
Mecklenburg Vorpommern	1994
Bremen	1996
Sachsen	1999
Sachsen Anhalt	2002
Thueringen	2003

Notes: The table shows the timing of the dropout reforms in the German federal states. Source: (Helbig & Nikolai 2015).

Table 2: Summary statistics

	Mean	SD	Min	Max
School career				
Years of schooling	12.30	1.09	8	13
Excess time in education	0.31	0.46	0	1
Age at leaving school system	18.55	0.90	14	19
School episodes	2.56	0.85	1	8
Downgrading from high track	0.13	0.33	0	1
Upgrading into high track	0.12	0.32	0	1
School degree				
No degree in last school year	0.05	0.21	0	1
Basic school degree in last school year	0.02	0.14	0	1
Middle school degree in last school year	0.19	0.39	0	1
High track school degree in last school year	0.69	0.46	0	1
No degree in 2010	0.00	0.03	0	1
Basic school degree in 2010	0.01	0.11	0	1
Middle school degree in 2010	0.15	0.36	0	1
High track school degree in 2010	0.83	0.37	0	1
Occupational Outcomes				
University degree	0.54	0.50	0	1
Holding a vocational degree	0.40	0.49	0	1
Net household income (in logs)	9.264	0.674	0	11.717
Personal characteristics				
Female	0.49	0.50	0	1
Year of birth	1958.22	6.23	1944	1982
Migration background	0.08	0.26	0	1
Highest parental education				
No degree	0.00	0.05	0	1
Basic school degree	0.42	0.49	0	1
Middle school degree	0.20	0.40	0	1
High track school degree	0.11	0.32	0	1
University degree	0.27	0.44	0	1
Number of siblings				
No siblings	0.16	0.37	0	1
One sibling	0.38	0.49	0	1
Two or more siblings	0.46	0.50	0	1
Location of secondary school				
Schleswig-Holstein	0.03	0.16	0	1
Hamburg	0.01	0.07	0	1
Lower Saxony	0.12	0.33	0	1
Bremen	0.04	0.19	0	1
Northrhine-Westphalia	0.36	0.48	0	1
Hesse	0.03	0.17	0	1
Rhineland-Palatinate	0.05	0.21	0	1
Baden-Wuerttemberg	0.05	0.22	0	1
Bavaria	0.25	0.43	0	1
Saarland	0.03	0.17	0	1
Berlin	0.04	0.19	0	1
Observations	1462			

Notes: The table shows summary statistics for the outcome and control variables constructed from the NEPS data.

Table 3: Reform effect on school career

	(1) Years of schooling	(2) Excess time in education	(3) Age at leaving school system	(4) School episodes	(5) Downgrading	(6) Upgrading
post reform	0.725***	0.055	0.605***	-0.546***	-0.292***	-0.032
	(0.121)	(0.050)	(0.076)	(0.111)	(0.039)	(0.035)
p-value wild-cluster bootstrap	[0.000]	[0.476]	[0.000]	[0.004]	[0.004]	[0.48]
state FE	Yes	Yes	Yes	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes	Yes	Yes	Yes
observations	1462	1461	1460	1462	1460	1460
R^2	0.105	0.065	0.087	0.094	0.098	0.122
mean outcome	12.296	0.313	18.547	2.563	0.128	0.120
SD outcome	1.088	0.464	0.895	0.851	0.334	0.325

Notes: The table shows the results of estimating equation 1 for years of schooling, excess time in school, age at leaving the school system, the number of school episodes, downgrading, and upgrading described in detail in section 3. We report the coefficient *post reform* which is the reform effect for students age 11-19 attending the high track in the year of the reform. Standard errors are clustered on state level. The third row reports p-values of wild-cluster bootstrapping the standard errors. All specifications include state- and birth-cohort fixed effects and the set of control variables. Data stems from NEPS SC6:7.0.0. Asterisks show the significance level:* = p<0.1, **=p<0.05, ***=p<0.01.

Table 4: Reform effect on school degrees

	degree in last school episode				final school degree 2010			
	(1) none	(2) basic	(3) middle	(4) high track	(5) none	(6) basic	(7) middle	(8) high track
post reform	-0.026	-0.037^{**}	-0.256^{***}	0.359^{***}	0.002	-0.005	-0.235^{***}	0.238^{***}
	(0.026)	(0.016)	(0.040)	(0.051)	(0.001)	(0.011)	(0.030)	(0.031)
p-value	[0.456]	[0.184]	[0.004]	[0.000]	[0.372]	[0.688]	[0.004]	[0.000]
wild-cluster bootstrap								
state FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
observations	1460	1460	1460	1460	1462	1462	1462	1462
R^2	0.057	0.025	0.079	0.094	0.012	0.013	0.081	0.079
mean outcome	0.047	0.019	0.188	0.693	0.001	0.013	0.155	0.832
SD outcome	0.212	0.137	0.391	0.461	0.026	0.113	0.362	0.374

Notes: The table shows the results of estimating equation 1 for degrees obtained in the last school year (no degree, basic, middle, and high track school degree) and degrees held in 2010 (no degree, basic, middle, and high track school degree) described in detail in section 3. We report the coefficient *post reform* which is the reform effect for students age 11-19 attending the high track in the year of the reform. Standard errors are clustered on state level. The third row reports p-values of wild-cluster bootstrapping the standard errors. All specifications include state- and birth-cohort fixed effects and the set of control variables. Data stems from NEPS SC6:7.0.0. Asterisks show the significance level:* = p<0.1, **=p<0.05, ***=p<0.01.

Table 5: Reform effect on occupational outcomes

	(1) Holding vocational degree	(2) university degree	(3) ln(household income)
post reform	-0.140***	0.137***	0.119*
	(0.037)	(0.038)	(0.065)
state FE	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes
observations	1462	1462	1444
R^2	0.091	0.072	0.039
mean outcome	0.399	0.540	9.264
SD outcome	0.490	0.499	0.674

Notes: The table shows the results of estimating equation 1 for the likelihood of holding a vocational, or a university degree in 2010 and the quality of the job in terms of income held at age 20 and 35. The variables are described in detail in section 3. We report the coefficient *post reform* which is the reform effect for students age 11-19 attending the high track in the year of the reform. All specifications include state- and birth-cohort fixed effects and the set of control variables. Data stems from NEPS SC6:7.0.0. Standard errors are clustered on state level. Asterisks show the significance level:* = p<0.1, **=p<0.05, ***=p<0.01.

Table 6: Reform effect: selection into high track

	(1) Any high track episode	(2) First secondary school: high track	(3) Grade 8: high track
post reform	0.050*	0.034	0.025
	(0.027)	(0.025)	(0.022)
state FE	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes
observations	8621	8233	8621
R^2	0.180	0.175	0.177
mean outcome	0.361	0.362	0.432
SD outcome	0.480	0.481	0.495

Notes: For the overall population we estimate if the share of students in the high track increased after the reform. We distinguish between having any high track episode (column 1), reporting the high track as first attended secondary school (column 2), and attending the high track in 8th grade. *Post reform* captures if students are 19 or younger at the year of the reform. Data stems from NEPS SC6:7.0.0. Standard errors are clustered on state level. Asterisks show the significance level:* = $p<0.1$, **=$p<0.05$, ***=$p<0.01$.

Table 7: Heterogeneity of reform effect on school careers by parental school degree

	(1) Years of schooling	(2) Excess time in education	(3) Age at leaving school system	(4) School episodes	(5) Downgrading	(6) Upgrading
Panel A. Subsample: parents with high track school degree						
post reform	0.278	-0.090	0.258*	-0.387**	-0.156**	-0.033
	(0.191)	(0.063)	(0.118)	(0.172)	(0.063)	(0.040)
observations	557	556	557	557	557	557
R^2	0.108	0.108	0.091	0.090	0.085	0.109
mean outcome	12.454	0.300	18.662	2.542	0.110	0.065
SD outcome	0.954	0.459	0.740	0.882	0.313	0.246
Panel B. Subsample: parents without high track school degree						
post reform	0.962***	0.126**	0.800***	-0.641***	-0.359***	-0.035
	(0.148)	(0.047)	(0.112)	(0.112)	(0.061)	(0.046)
observations	905	905	903	905	903	903
R^2	0.119	0.062	0.098	0.116	0.142	0.134
mean outcome	12.199	0.320	18.476	2.576	0.140	0.154
SD outcome	1.153	0.467	0.973	0.832	0.347	0.361
state FE	Yes	Yes	Yes	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows the results of our subgroup analysis restricting the sample to people with parents who hold an high track school degree. We estimate equation 1 for this subgroup with years of schooling, excess time in school, age at leaving the school system, the number of school episodes, downgrading, and upgrading as outcome variables described in detail in section 3. We report the coefficient *post reform* which is the reform effect for students age 11-19 attending the high track in the year of the reform. All specifications include state- and birth-cohort fixed effects and the set of control variables. Data stems from NEPS SC6:7.0.0. Standard errors are clustered on state level. Asterisks show the significance level:* = p<0.1, **=p<0.05, ***=p<0.01.

Table 8: Heterogeneity of reform effect on school degrees by parental school degree

	degree in last school year				final school degree 2010			
	(1) none	(2) basic	(3) middle	(4) high track	(5) none	(6) basic	(7) middle	(8) high track
Panel A. Subsample: parents with high track school degree								
post reform	0.022	-0.038	-0.152***	0.211**	0.005	-0.002	-0.139***	0.136**
	(0.017)	(0.032)	(0.033)	(0.074)	(0.003)	(0.014)	(0.037)	(0.048)
observations	557	557	557	557	557	557	557	557
R^2	0.105	0.061	0.081	0.108	0.029	0.033	0.096	0.087
mean outcome	0.047	0.020	0.142	0.738	0.002	0.011	0.113	0.874
SD outcome	0.211	0.139	0.349	0.440	0.042	0.103	0.317	0.332
Panel B. Subsample: parents without high track school degree								
post reform	-0.053	-0.031***	-0.304***	0.429***	0.000	-0.007	-0.282***	0.289***
	(0.036)	(0.009)	(0.055)	(0.050)	(.)	(0.012)	(0.033)	(0.031)
observations	903	903	903	903	905	905	905	905
R^2	0.061	0.021	0.088	0.105	.	0.025	0.084	0.088
mean outcome	0.048	0.019	0.217	0.666	0.000	0.014	0.180	0.806
SD outcome	0.213	0.136	0.412	0.472	0.000	0.119	0.384	0.396
state FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows the results of our subgroup analysis restricting the sample to people with parents who hold an high track school degree. We estimate equation 1 for this subgroup with degree obtained in the last school year (no degree, basic, middle, and high track school degree) and degrees held in 2010 (no degree, basic, middle, and high track school degree) described in detail in section 3. We report the coefficient *post reform* which is the reform effect for students age 11-19 attending the high track in the year of the reform. All specifications include state- and birth-cohort fixed effects and the set of control variables. Data stems from NEPS SC6:7.0.0. Standard errors are clustered on state level. Asterisks show the significance level:* = p<0.1, **=p<0.05, ***=p<0.01.

Table 9: Placebo test

	school career			final school degree 2010			
	(1) Years of schooling	(2) School episodes	(3) Downgrading	(4) none	(5) basic	(6) middle	(7) academic
Panel A. Placebo reform 10 years earlier							
placebo reform effect	-0.090*	-0.131*	0.027**	0.001	-0.008	0.010	-0.004
	(0.042)	(0.055)	(0.009)	(0.002)	(0.011)	(0.027)	(0.039)
observations	620	620	618	620	620	620	620
R^2	0.072	0.051	0.033	0.021	0.023	0.048	0.048
Panel B. Placebo reform 5 years earlier							
placebo reform effect	-0.001	-0.146	-0.010	-0.006	-0.023***	0.008	0.020
	(0.105)	(0.082)	(0.040)	(0.004)	(0.003)	(0.023)	(0.019)
observations	620	620	618	620	620	620	620
R^2	0.071	0.050	0.032	0.022	0.024	0.048	0.048
Panel C. Placebo reform 2 years earlier							
placebo reform effect	-0.141	0.175**	-0.022	0.006	0.001	0.029	-0.037
	(0.074)	(0.043)	(0.027)	(0.004)	(0.014)	(0.035)	(0.021)
observations	620	620	618	620	620	620	620
R^2	0.072	0.051	0.032	0.023	0.022	0.048	0.049
state FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
mean outcome	12.227	2.642	0.138	0.002	0.016	0.155	0.827
SD outcome	1.066	0.910	0.345	0.040	0.126	0.362	0.378

Notes: The table shows the results of estimating equation 1 for our main outcome variables described in detail in section 3. The main effect of interest is the coefficient of *trend 10 years before reform* which is the reform effect for students age 11-19 attending the high track 10, 5, and 2 years prior to the reform. All specifications include state- and birth-cohort fixed effects and the set of control variables. Data stems from NEPS SC6:7.0.0. Standard errors are clustered on state level. Asterisks show the significance level:* = p<0.1, **=p<0.05, ***=p<0.01.

Table 10: Robustness check: including linear state trends

	(1) Years of schooling	(2) No. of school episodes	(3) Downgrading	(4) no degree	(5) basic school degree	(6) middle school degree	(7) academic school degree
post reform	0.823^{***}	-0.548^{***}	-0.339^{***}	0.001	-0.005	-0.276^{***}	0.280^{***}
	(0.072)	(0.130)	(0.028)	(0.001)	(0.012)	(0.019)	(0.024)
linear state trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
observations	1462	1462	1460	1462	1462	1462	1462
R^2	0.138	0.100	0.113	0.016	0.017	0.103	0.102
mean outcome	12.296	2.563	0.128	0.001	0.013	0.155	0.832
SD outcome	1.088	0.851	0.334	0.026	0.113	0.362	0.374

Notes: The table shows robustness checks including in our estimation equation 1 linear time-state trends. The outcome variables are years of schooling, school episodes, downgrading, holding no degree, a basic, middle, or high track school degree in 2010. The variables are described in detail in section 3. We report the coefficient *post reform* which is the reform effect for students age 11-19 attending the high track in the year of the reform. All specifications include state- and birth-cohort fixed effects and the set of control variables. Data stems from NEPS SC6:7.0.0. Standard errors are clustered on state level. Asterisks show the significance level:* = $p<0.1$, **=$p<0.05$, ***=$p<0.01$.

Table 11: Robustness check: leave one state out at a time

Panel A.	Years of schooling			School episodes		
	(1)	(2)	(3)	(4)	(5)	(6)
	no NRW	no Bavaria	no Berlin	no NRW	no Bavaria	no Berlin
post reform	0.672***	0.770***	0.746***	-0.530***	-0.683***	-0.554***
	(0.187)	(0.156)	(0.124)	(0.133)	(0.151)	(0.111)
state FE	Yes	Yes	Yes	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes	Yes	Yes	Yes
observations	941	1095	1406	941	1095	1406
R^2	0.088	0.121	0.111	0.088	0.129	0.093
mean outcome	12.243	12.291	12.303	2.589	2.536	2.568
SD outcome	1.130	1.077	1.092	0.881	0.810	0.857

Panel B.	Downgrading			high track school degree		
	(1)	(2)	(3)	(4)	(5)	(6)
	no NRW	no Bavaria	no Berlin	no NRW	no Bavaria	no Berlin
post reform	-0.287***	-0.287***	-0.291***	0.238***	0.287***	0.237***
	(0.061)	(0.061)	(0.041)	(0.044)	(0.046)	(0.032)
state FE	Yes	Yes	Yes	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes	Yes	Yes	Yes
observations	939	1094	1404	941	1095	1406
R^2	0.105	0.084	0.096	0.088	0.083	0.077
mean outcome	0.140	0.103	0.130	0.815	0.853	0.828
SD outcome	0.347	0.304	0.337	0.388	0.354	0.378

Notes: The table shows robustness checks excluding one state at a time from our estimation equation 1. The outcome variables are years of schooling, school episodes, downgrading, and holding a high track school degree. The variables are described in detail in section 3. Columns 1 and 4 show results for leaving out Northrhine-Westphalia, columns 2 and 5 show results for leaving out Bavaria, and column 3 and 6 show results for leaving out Berlin. We report the coefficient *post reform* which is the reform effect for students age 11-19 attending the high track in the year of the reform. All specifications include state- and birth-cohort fixed effects and the set of control variables. Data stems from NEPS SC6:7.0.0. Standard errors are clustered on state level. Asterisks show the significance level:* = p<0.1, **=p<0.05, ***=p<0.01.

Table 12: Robustness check: effect on whole population

	school career			final school degree 2010			
	(1) Years of schooling	(2) School episodes	(3) Downgrading	(4) none	(5) basic	(6) middle	(7) academic
post reform	-0.118	0.027	0.003	-0.001	0.011	0.005	-0.015
	(0.146)	(0.047)	(0.008)	(0.003)	(0.029)	(0.020)	(0.022)
state FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
observations	10032	10032	9920	10032	10032	10032	10032
R^2	0.206	0.066	0.016	0.021	0.102	0.065	0.176
mean outcome	11.237	2.521	0.056	0.006	0.181	0.315	0.498
SD outcome	1.608	0.866	0.230	0.078	0.385	0.464	0.500

Notes: The table shows the reform effect on school careers and school degrees for the whole population (not only high track attendees). The coefficient of *post reform* is the treatment effect for students being younger than 19 in the year of the reform. All specifications include state- and birth-cohort fixed effects and the set of control variables. Data stems from NEPS SC6:7.0.0. Standard errors are clustered on state level. Asterisks show the significance level:* = $p<0.1$, **=$p<0.05$, ***=$p<0.01$.

Table 13: Reform effect on students in middle and basic school track

	school career			final school degree 2010			
	(1) Years of schooling	(2) School episodes	(3) Downgrading	(4) none	(5) basic	(6) middle	(7) academic
post reform	0.043	-0.005	-0.016**	0.006	-0.062	0.030	0.025
	(0.108)	(0.039)	(0.007)	(0.004)	(0.054)	(0.045)	(0.024)
state FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
observations	2564	2564	2538	2564	2564	2564	2564
R^2	0.084	0.061	0.092	0.030	0.089	0.067	0.089
mean outcome	10.258	2.511	0.016	0.006	0.304	0.473	0.217
SD outcome	1.309	0.754	0.126	0.076	0.460	0.499	0.412

Notes: All specifications include state- and birth-cohort fixed effects and the set of control variables. Data stems from NEPS SC6:7.0.0. Standard errors are clustered on state level. Asterisks show the significance level:* = p<0.1, **=p<0.05, ***=p<0.01.

Table 14: Reform effect on school careers by age group

	(1) Years of schooling	(2) Excess time in education	(3) Age at leaving school system	(4) School episodes	(5) Downgrading	(6) Upgrading
11-14 years post reform	0.560*	0.054	0.387**	-0.657***	-0.274***	-0.121***
	(0.270)	(0.071)	(0.142)	(0.152)	(0.061)	(0.036)
15-17 years post reform	0.680***	0.037	0.599***	-0.636***	-0.293***	-0.076*
	(0.153)	(0.053)	(0.092)	(0.124)	(0.049)	(0.039)
18+ years post reform	0.796***	0.072	0.649***	-0.444***	-0.295***	0.024
	(0.102)	(0.052)	(0.060)	(0.131)	(0.030)	(0.044)
state FE	Yes	Yes	Yes	Yes	Yes	Yes
birthcohort FE	Yes	Yes	Yes	Yes	Yes	Yes
observations	1462	1461	1460	1462	1460	1460
R^2	0.108	0.066	0.093	0.097	0.098	0.131
mean outcome	12.296	0.313	18.547	2.563	0.128	0.120
SD outcome	1.088	0.464	0.895	0.851	0.334	0.325

Notes: In the table we estimate the reform effect on school careers for three different age groups of high track school attendees. The first age group is 11-14 years old in the year of the reform, the second 15-17, and the third 18 and older. Data stems from NEPS SC6:7.0.0. Standard errors are clustered on state level. Asterisks show the significance level:* = p<0.1, **=p<0.05, ***=p<0.01.