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ABSTRACT

The Long-Term Effects of Early Track Choice^{*}

Despite its efficiency in tailoring education to the needs of students, a tracking system has the inherent problem of misallocating students to tracks because of incomplete information at the time of the tracking decision. This paper investigates the effects of attending a more advanced track in middle school on long-term education and labor market outcomes for Germany, a country with a very rigorous tracking system where the risk of misallocating students to tracks is, due to the early age at which tracking takes place, particularly high. Our research design exploits quasi-random shifts between tracks induced by date of birth, and identifies the long-term effects of early track attendance for a group of marginal students most at risk of misallocation. Remarkably, we find no evidence that for these students, attending a more advanced track leads to more favorable long-term outcomes. We attribute this result to the up- and downgrading of students between tracks after middle school when more information about their potential is available. Overall, our findings underscore that flexibilities built into a tracking system, which allow students to revise initial track choices at a later stage, effectively remedy even a prolonged exposure to a less advanced school environment.

JEL Classification: I21, J10

Keywords: tracking, school quality, peer effects, regression discontinuity design

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1 INTRODUCTION

Countries differ widely with respect to the way they organize their school systems. While Germany and Singapore deliberately track students into different schools based on ability as early as the end of elementary school (ages 10/12), other countries, such as the U.S., the U.K., and Japan essentially keep their entire secondary education system comprehensive—although tracking within schools is common and on the rise in these countries. Proponents of tracking systems stress that allocating students to learning environments more adequate for their abilities can lead to more efficient educational production, whereas opponents argue that tracking may exacerbate initial disadvantages. Most particularly, they point to the danger that small differences between students in innate ability or social background before tracking may translate into large differences in adult outcomes through exposure to very different learning environments. This problem is particularly acute in a system that tracks students very early on, when information about the student’s learning potential is likely to be incomplete and track allocation is, at least to some extent, determined by sheer luck. But initial mis-tracking need not lead to life-long disadvantage, as long as the tracking system has built-in flexibilities that allow correction of initial choices at a later point, and when more information about individuals’ abilities is available. This important aspect in assessing tracking systems has hardly been discussed in the literature so far.

Thus, to assess whether early track allocation has indeed life-long consequences, and whether a tracking system successfully allows for correcting initial misallocations, requires a comparison of long-term educational and labor market outcomes of individuals that were at the margin between two tracks, and who were randomly allocated into a higher or a lower track. It is precisely such a natural experiment on which we draw in this paper. We explore this for Germany, a country with a particularly rigorous tracking system that very early on (at age 10) allocates students into three different school tracks based on ability and locks them into the assigned track throughout middle school (i.e., from grades 5 to 9). These

tracks are designed to support particular career paths: the high track prepares students for college or university education, while the medium and low tracks aim at providing skills for white and blue collar apprenticeships. As a result, the three tracks differ widely, especially in terms of teaching curricula as well as peer and teacher quality.

Our research design exploits quasi-random shifts between tracks induced by date of birth. We first show that children who are born after the age of school entry cut-off date are more likely to attend a more advanced track in middle school than children who are born before the age of school entry cut-off date, and who are thus younger in class. This is the *first stage* of our analysis. We then investigate the impact of date of birth on long-term labor market outcomes and educational attainment, obtaining *reduced form* estimates, and compute the impact of track assignment in middle school on long-term outcomes by dividing the reduced-form estimates by the first stage estimates. We discuss the validity of our research design in detail in Section 3.1. Based on this design, we can identify the causal effect of track exposure for students who are at the margin between two tracks (hereafter, *marginal* students). These are students whose parents or teachers may be unsure of which track the student should attend after elementary school and who are therefore most at risk of misallocation.

Despite the striking differences in learning environments between tracks, we find that attending a more advanced track in middle school, induced through date of birth, has no effect on wages, employment or occupation choice at later ages. What explains this surprising finding? We attribute it to the up- and downgrading of students between tracks that takes place at a later stage of their educational career: We first show a substantial amount of movement from the low or medium to the high track at the end of middle school (age 15/16), an upgrading facilitated by the school system. We further demonstrate a substantial amount of downgrading at the age of 18/19 through non-enrollment in college or university after graduating from the high track. Due to this up- and downgrading, attending a more advanced track has, for marginal students, hardly any impact on the probability of graduating

from a medium or high track, completing an apprenticeship, or graduating from college or university. These findings of zero effects on long-term outcomes for marginal students do not imply that there is no difference in observed outcomes of students who *sort* into different tracks in middle school: Our own calculations based on the German Socio-Economic Panel show that these differences are large, reflecting the ability-based selection that characterizes a tracking system.¹

These findings also emphasize a core aspect of the basis on which tracking systems should be assessed: the built-in possibilities to correct earlier allocations at a stage when more information is revealed about a student’s true potential. That is, although the young age at which the tracking decision is taken creates particularly large risks for student misallocation, the system we study also allows students whose potential has been erroneously assessed to revise the initial track decision at later stages in their educational careers. Our findings underscore that these built-in flexibilities allow marginal students to fully make up for the exposure to a less challenging school environment during middle school—even if the exposure to a less challenging school environment is prolonged (in our case 5 to 6 years) and the differences in school environments are large.

The paper is structured as follows: Section 2 provides an overview of the relevant literature and the German tracking system. Sections 3 and 4 then outline our empirical research design and data sources, respectively. Section 5 reports the empirical results, and Section 6 presents our discussion and conclusions. Besides reporting additional results in the Appendix, we also provide a theoretical model of track choice which formalizes the shifting of students to a different track because of birth month, and clarifies how the key differences between school tracks (i.e., teaching technology and peer quality) affect long-term outcomes.

¹OLS regressions based on the GSOEP suggest that completing the medium versus the low track is associated with a 16 percent wage differential, whereas completing the high versus the medium track is associated with a 23 percent wage differential.

2 BACKGROUND

2.1 Existing Literature

The literature on tracking has adopted three approaches to assess the effects of tracking on student outcomes, relying on *across-country*, *within-country* and *within-school* variation. Exploiting variation in school systems *across countries*, Hanushek and Wössmann (2006) show that the variance of the test score distribution increases between elementary and middle school in countries with a tracked school system relative to countries with a comprehensive school system, without an increase in the mean. They thus conclude that tracking increases achievement of high ability students at the expense of low ability students. Waldinger (2007) argues that these findings are sensitive to the way countries are categorized into a tracked and comprehensive school system.

A number of recent papers evaluate policy reforms which effectively reduced tracking *within a country*. For instance, while Malamud and Pop-Echeles (2011), Kerr, Pekkarinen, and Uusitalo (2012) and Hall (2012) study the effects of deferring tracking to a later age, Guyon, Maurin and McNally (2012) investigate the effects of an increase in the share of students admitted to the academic (grammar) school track. Both the across-country and within-country studies should be seen as complementary to our paper: While these studies are informative about which students may benefit or lose from a tracked versus a non-tracked school system, or from de-tracking, they do not investigate the long-term consequences initial misallocation to tracks and the role of flexibilities built into a tracking system to remedy such misallocation—which is the focus of this paper.

Studies surveyed comprehensively by Betts (2011) analyze the effects of tracking *within schools*, as often practiced in the U.S. Most of these earlier studies do not take into account the sorting of students into tracks, or into schools that offer tracking. A recent study by Duflo, Dupas, and Kremer (2011) addresses this, by randomly allocating schools to tracked

and non-tracked schools. In tracked schools, students are assigned to the higher track based on a test. Similar to our study, these authors carry out a regression discontinuity design, comparing students whose test results put them at the margin between two tracks. They find no effect of attending the high track on achievement for the marginal student, a result that we complement in our natural experiment for Germany for long-term outcomes. These authors also estimate the impact of within-school tracking on the entire test score distribution, and conclude that all students—including those in the lower track—benefit from tracking. While our research design does not allow us to assess these overall effects of a tracked versus a non-tracked education system, an important advantage of our study is that we are able to estimate the *long-term* effects (as opposed to the *short-term* effects, as in Duflo, Dupas, and Kremer, 2011) of attending a more advanced track for a group of marginal students most at risk of misallocation.

Our paper is also related to the literature on school quality; in particular that on elite schools where, similar to Duflo, Dupas, and Kremer (2011), attendance at an elite school is determined by an entry examination. This set-up lends itself to a regression discontinuity design, where the effects of attending an elite school are estimated by comparing outcomes of students who in the entry exam scored just below or just above the attendance cut-off—which are students at the margin between elite and non-elite schools. Previous papers on this issue reach conflicting conclusions. For instance, whereas Jackson (2010) and Pop-Echeles and Urquiola (2011) find that attending better schools improves children’s academic achievement, Clark (2010) and Abdulkadiroglu, Angrist and Pathak (2011) find no evidence that elite schools improve standardized test scores. Since these studies focus on academic achievement before final educational choices are made, they essentially identify a short-term effect of school quality. This effect may differ considerably from the long-term effect of school quality that also depends on subsequent educational choices, which is what we study in this

paper.²

2.2 General Overview of the German Education System

Figure 1 provides an overview of the German education system, where students are allocated into three tracks at the end of fourth grade (around the age of 10). We designate these tracks “high”, “medium”, and “low”. Education in the high track, the *Gymnasium* (comparable to the traditional British grammar school) lasts for nine years (grades 5 to 13) and prepares students for tertiary studies at such academic institutions as four-year colleges or five-year universities. Education at the low and medium track, on the other hand, lasts five (grades 5 to 9) and six years (grades 5 to 10), respectively, is less academic, and prepares students for an apprenticeship in blue-collar (e.g., crafts) or white-collar occupations (e.g., office clerk, but also medical assistant). For men and women born between 1961 and 1976, the cohorts focused on in the empirical analysis, the shares of who attends each track are roughly of equal size.

Although there is no strict rule (such as an entry exam) to determine which track children can attend after elementary school, elementary school teachers do make recommendations. However, in 10 out of 16 states, parents have the final word on this choice. In the remaining 6 states, if parents want to deviate from the teachers’ recommendation, students must either have earned the required marks or pass a special test.³

In principle, students may switch between tracks at any grade throughout middle school; however, in practice, very few students (less than 2%) do so.⁴ Hence, once students are allocated, they are locked into their chosen track for at least five years. However, once

²Other studies which exploit quasi-random variation to investigate the short-term effects of school quality on student achievement include Gould, Lavy, and Paserman (2004) and Cullen, Jakob and Levitt (2005, 2006). Recently, a number of studies on the long-term effects of school quality on adult outcomes have emerged (e.g., Chetty, Friedman, Hilger, Saez, Whitmore Schanzenbach and Yagan, 2011; Deming, Hastings, Kaine and Staiger, 2012). Due to a different research design, these studies do not necessarily identify the effects of school quality for the *marginal* student, and may thus not be directly comparable to our study.

³For more information, see http://www.kmk.org/fileadmin/veroeffentlichungen_beschluesse/2010/2010_10_18-Uebergang-Grundschule-S_eI1-Orientierungsstufe.pdf

⁴Own calculations based on the School Census for Bavaria and Hesse.

students complete their course at the low or medium track (i.e., after ninth or tenth grade), the school system facilitates switches between tracks. In particular, at the end of ninth grade, low track students may either switch to a medium track school or, provided that their school offers this possibility, stay on for another year to earn the same school-leaving qualification which they would have received from a medium track school. In addition, after tenth grade, students who graduated from a medium track school may either upgrade to a traditional high track school, or attend a high track school targeted at former medium track students. These latter often provide a special focus (e.g., agricultural, business, health, or social studies) in addition to general education, and we label them “specialized high track schools” (see Figure 1). Like graduation from a general high track school, graduation from a specialized high track school grants access to college or university, but possibly restricts the field of study.

Two further factors facilitate track switching in the German education system such that students can make up for exposure to a less challenging middle school track. First, legal regulations at the state level limit German universities’ ability to pick students from particular schools. Most importantly, school marks are the main criterion for entry into almost all courses of studies and must be taken into account mechanically to assign university places—that is regardless of whether they are from a traditional or specialized high track school and without concern for the reputation of the high track school. A second facilitating element for track changers is the overwhelmingly state-financed nature of the education system in Germany—as opposed to a largely property-tax funded school system in the U.S. This state financing—which is mostly based on student head counts for both schools and universities—generates a comparatively large equality in the endowment and quality between educational institutions, including universities. In particular, the concept of an elite university eludes Germany for the cohorts we study.⁵

⁵Still today, students looking for an elitist education go abroad, mostly to the Anglo-Saxon world.

2.3 Differences between Tracks

The first important difference between the three tracks is that they differ with respect to teaching intensity and learning goals.⁶ We summarize the details in Panel A of Table 1. In ninth grade (i.e., when attendance at low track schools ends), the number of hours taught per week is 36 at high, but only 32 at medium and low track schools, respectively. Moreover, although the number of weekly hours in core subjects like Mathematics, German, and English as a foreign language is similar across tracks, high track schools teach more hours in the second and third foreign language and natural sciences, and fewer hours in social sciences, physical education, and vocational subjects (e.g., the “World of Work”) than low or medium track schools. In the core subjects of mathematics and German, tracks differ with respect to the topics taught as well as teaching intensity. For example, whereas low track schools put special emphasis in ninth grade on such applications as writing CVs, filling out forms, and preparing for job interviews, high track schools pay special attention to detailed explanations, analysis, and interpretation of various types of texts, including historical documents, and stress creative writing. Likewise, whereas ninth-grade mathematics in medium track schools covers topics such as real numbers and powers and equations with two unknowns, these topics are taught in eighth grade in high track schools. Similarly, although both medium and high track schools introduce functions, high track schools do so more intensely and cover advanced functions (e.g., exponential and broken power functions) not taught in medium track schools. Low track schools, in contrast, sometimes cover no functions at all, focusing instead on equations with rational numbers.

A second important difference is that students attending the high or medium track are surrounded by academically stronger peers than students attending the low track. According to PISA data for 2003 and 2006, average reading and mathematics test scores at high track

⁶The information in this paragraph was gathered from the curricula published on the web pages of the Ministries for Culture and Education of West German states. In some cases, we also contacted the ministries by telephone.

schools are about one standard deviation higher than at medium track schools and about 1.7 standard deviations higher than at low track schools in ninth grade (see Panel B of Table 1). These differences in peer quality across school tracks are far larger than those in the quasi-experiments by Pop-Eleches and Urquiola (2011) and Cullen, Jakob and Levitt (2006) (around 0.20 of a standard deviation in both cases). The figures in the table also show that high track students are exposed to peers whose family backgrounds are far more academic than those of potential classmates at medium or low track schools, and that high track students come from higher income households: only 39 percent live in households with a below-median income, a number that increases to 65 percent for medium track students and 76 percent for low track students.⁷ It should also be noted that track choice alone can explain more than 80 percent of the variation in test scores across schools in ninth grade, suggesting that schools of the same type are fairly homogeneous.

A third important difference between the three tracks is that teachers in high track schools are likely to be of higher quality than teachers in medium or low track schools. More specifically, as Panel C of Table 1 shows, the minimum formal education for high track teachers is one year higher than that for low and medium track teachers, and their salaries are approximately 10 percent higher.

3 RESEARCH DESIGN

3.1 *Randomization into Tracks*

To identify a causal effect, we require an experiment that effectively randomizes students of the same ability into different tracks after elementary school. Figure 2 provides an illustration of our natural experiment. Our starting point is that, because of the school entry cut-off rule, children whose birthdays fall before July 1 typically start school a year earlier than

⁷Panel B of Table 1 also shows that the share of girls in low track schools is about 14 percentage points lower than in high track schools, which may boost the performance of boys and girls at school (e.g., Schanzenbach, 2009; Lavy and Schlosser, 2011) and in the labor market (Black, Devereux, and Salvanes, 2012).

children whose birthdays are on or after July 1. Hence, although not every child complies with this law, children born shortly after the cut-off rule, in July, are on average considerably older at school entry than children born shortly before the cut-off rule, in June. A large body of literature has shown that age of school entry is an important determinant of early student achievement; see e.g., Bedard and Dhuey (2006); Puhani and Weber (2007); Elder and Lubotski (2009); and Schneeweiss and Zweimüller (2012). Since the decision which track to attend is made very early in Germany, at a stage when the age of school entry effect is unlikely to have dissipated, the date of birth may, through its link to age of school entry, affect the track students attend after elementary school and, given the limited possibilities of switching during grades 5 through 9, lock them into a particular track throughout middle school (see the first effect in Figure 2). Hence, the first stage of our analysis addresses the difference in track assignment through grades 5 and 9, T , between children born in month Z , where Z is June or July:

$$\pi = E[T_i|Z_i = \text{July}] - E[T_i|Z_i = \text{June}] \quad (1)$$

We then investigate the impact of birth month on long-term labor market outcomes like wages and unemployment. The reduced-form of our analysis is the difference in these long-term outcomes (denoted by Y) between students born in July and those born in June:

$$\eta = E[Y_i|Z_i = \text{July}] - E[Y_i|Z_i = \text{June}] \quad (2)$$

We finally obtain the impact of track assignment in middle school on long-term outcomes by dividing the reduced-form effect by the first-stage effect, $\tau = \eta/\pi$.

This strategy identifies the causal impact of track assignment on long-term outcomes under the assumption that age of school entry affects long-term outcomes only through track assignment in middle school (the exclusion restriction). There are two reasons why

this assumption may be violated. First, if date of birth, through the interaction between age of school entry and compulsory school leaving laws, affects the length of formal schooling (see the third effect in Figure 2). This is the case in the U.S., where students are allowed to leave school at the day of their 16th birthday.⁸ In Germany, however, this effect is absent, since students must stay in school until they at least complete 9th grade (as opposed to their 16th birthday, as in the U.S.).

Second, the exclusion restriction would also be violated if being older at school entry has a direct impact on long-run outcomes that does not operate through track assignment in middle school (see the second effect in Figure 2). We have reviewed more than 30 articles that study the impact of age at school entry on achievements. This literature has found such *direct* effects of age of school entry on long-run (prime age) earnings or educational attainment to be either non-existent or *positive*.⁹ None of the papers we have reviewed finds a negative effect of being older at school entry on short- or longer term outcomes—apart from the mechanical effect that is instigated through individuals who are older at school entry also being older at labor market entry, so that they have, at any given age, accumulated less labor market experience than individuals who are younger at school entry.¹⁰ We eliminate this experience effect using various methods (see Section 5.2 for details), so that our estimates of the impact of track assignment in middle school on wages are not affected by this mechanism. Therefore, even if the initial advantages of delayed school entry indeed persisted into adulthood, these effects would be positive. This would lead to an *upward* bias in our estimates. Because we do not find a significantly positive effect, this reasoning reinforces our finding that a more advanced track assignment in middle school has no positive effects on adult outcomes.

In addition to the exclusion restriction, we require the usual independence assumption to

⁸This feature of the U.S. education system was first exploited in the seminal paper by Angrist and Krueger (1991), who use date of birth as an instrument for the length of formal schooling.

⁹Studies that find positive effects of age of school on educational attainment or earnings include Bedard and Dhuey (2012), and Frederickson and Öckert (2013). Black, Devereux, and Salvanes (2011), in contrast, find little effect of age of school entry on educational attainment and earnings for individuals older than 30.

¹⁰See e.g. Frederickson and Öckert (2013) and Black, Devereux, and Salvanes (2011).

hold, which in our baseline specification entails that whether a child is born in June or July is random. In the regression discontinuity estimates that exploit the exact date of birth (see below), the independence assumption requires that whether a child is born just before or just after the school entry cut-off date (i.e., on June 30 or July 1) is random. Note that since we are comparing individuals born within the same season, this independence assumption is considerably weaker here than in studies that use birth quarter as an instrument. To assess whether the independence assumption is being violated, we compare in Table A1 the parental characteristics of children born in July versus those born in June using German Microcensus data for 2005, the only year for which information on birth month is available. We find no significant differences between children born in July and those born in June in terms of parental education and their age at the children’s birth.

3.2 Interpretation

Under the exclusion and independence assumption discussed above, the ratio between the reduced-form estimates η in equation (2) and the first stage estimates π in equation (1) identifies a weighted average of two local average treatment effects (see Imbens and Angrist 1994):¹¹

$$\tau = \frac{aE[Y_i^M - Y_i^L|M_i \text{ if } Z_i = \text{July}, L_i \text{ if } Z_i = \text{June}]}{a + b} + \frac{bE[Y_i^H - Y_i^M|H_i \text{ if } Z_i = \text{July}, M_i \text{ if } Z_i = \text{June}]}{a + b} \quad (3)$$

The first local average treatment effect, $E[Y_i^M - Y_i^L|M_i \text{ if } Z_i = \text{July}, L_i \text{ if } Z_i = \text{June}]$, is the impact of attending the medium rather than the low track in middle school on the long-term outcomes for those individuals who attend the medium track if born in July but the low track if born in June. The second local average treatment effect, $E[Y_i^H - Y_i^M|H_i \text{ if } Z_i = \text{July}, M_i \text{ if } Z_i = \text{June}]$, is the impact of attending the high rather than the medium track in middle school on the long-term outcomes for those individuals who attend the high track if born in July but the medium track if born in June.

¹¹Here, we make the additional assumption that no individuals are shifted from an L to an H track school because of birth month.

$Z_i = \text{June}]$, is the impact of attending the high rather than the medium track in middle school on long-term outcomes for those individuals who are shifted between these two tracks because of their birth month. The weights a and b represent the differences in the probabilities of attending the low and high track in middle school between individuals born in July and those born in June, that is $a = \Pr(L|\text{June}) - \Pr(L|\text{July})$ and $b = \Pr(H|\text{July}) - \Pr(H|\text{June})$.

Who are the “compliers” who attend a more or less advanced track because of their birth month? Students at the top of the (expected) ability distribution are likely to attend the high track, whereas students at the bottom of the distribution are likely to attend the low track, regardless of their date of birth. Hence, the “compliers” are likely to be students at the margin between two tracks. This group is particularly interesting because it consists of children whose teachers and parents may be unsure of which track the student should attend after elementary school and who are therefore most at risk of misallocation—and for whom small differences in the perceived learning potential at the end of elementary school could potentially lead to large differences in adult outcomes.

We develop a stylized model of track choice in Appendix B which formalizes the shifting of students to a different track because of birth month; this model is not necessary to understand the paper, but formalizes the intuition outlined above (see Proposition 1 and Figure A3). The model also clarifies how the key differences between school tracks (i.e., teaching technology, peer quality and teacher quality) and the costs of switching between tracks may affect completed education (see Proposition 2) and long-term labor market outcomes (see Proposition 3).

3.3 Implementation

Reduced-Form Estimates

To implement our empirical strategy, we first estimate reduced-form effects η by replacing the population means in equations (1) and (2) with their sample means, while controlling for

birth year and gender effects.¹² We report heteroscedasticity-consistent (robust) standard errors. We also report regression discontinuity estimates that exploit the student’s exact date of birth.¹³ Specifically, we estimate regressions of the following type:

$$Y_i = \alpha_0 + h(\text{Day}_i) + \alpha_1 \text{Post}_i + x_i' \alpha_2 + u_i. \quad (4)$$

Here, Day_i is the student’s date of birth (normalized to be 0 on the school entry cut-off date, July 1), $h(\cdot)$ is a polynomial function of birthday, Post_i is an indicator variable equal to 1 if the student was born on or after July 1, and x_i is a control variable vector that includes birth year and gender effects. The parameter of interest is α_1 , the impact of being born after the school entry cut-off date (i.e., on or after July 1) on long-term outcomes. We first estimate equation (4) on a sample of all students and then restrict the sample to students born within three months of the school entry cut-off date (i.e., to students born between April and September). We approximate the function $h(\text{Day}_i)$ as a polynomial function of various orders. As suggested by Lee and Card (2007), we cluster standard errors at the date of birth level.¹⁴

First-Stage Estimates

Due to lack of precise information on date of birth in the German Microcensus (see Section 4.2), we are not able to estimate the same specifications for the first-stage estimates for the same birth cohorts. We instead compare track attendance in middle school of students born earlier (i.e., between January and April) and later (i.e., between May and December) during the year (instead of June/July). In Section 5.1, we provide evidence that this is likely to lead to an underestimate in the first-stage estimates, which causes bias *away from zero* in

¹²Our results remain almost unchanged if we exclude these control variables from the regressions.

¹³We cannot do this for the first-stage effect because our data sets provide information only on birth month or whether the pupil was born earlier or later during the year; see Sections 4.2 and 4.3 for details.

¹⁴Our baseline estimate comparing individuals born in June with those born in July may also be seen as a regression discontinuity estimate in which the sample is restricted to students born within two months of the school entry cut-off date and the birthday effect ($h(\cdot)$ in equation (4)) is assumed to be constant.

the estimate of our local average treatment effect $\hat{\tau}$.

Because the first-stage and reduced-form estimates are based on two different samples, we refer to $\hat{\tau}$ as the two-sample two-stage least squares estimate (TS-2SLS) of track choice in middle school on long-term outcomes. We calculate the standard error of the TS-2SLS estimates using the delta method.¹⁵

4 DATA

Our empirical analysis combines four main data sources, described below. Throughout the analysis, we exclude foreign citizens from our sample because they may have migrated to Germany after beginning school and thus may not have been affected by the school entry cut-off date.

4.1 Social Security Records

Our primary data used to estimate the reduced-form equations (see equation (2)), which relate long-term outcomes to date of birth, come from three decades of social security records, covering 1975 to 2006. These data, collected for every individual covered by the social security system, include detailed information on variables such as education, wages, unemployment, occupation, and the exact date of birth. Not included are civil servants, the self-employed, and military personnel. A comparison of the number of observations in the Social Security Records and the number of births in the Birth Register data suggests that almost all individuals enter the data base at some point in their career.

From this data base, we select all men and women born between 1961 and 1976. The 1961 cohort is the first cohort for which the effective school entry cut-off falls between June and July. The 1976 cohort is 30 years old in the last year of our data and should

¹⁵We compute the variance of $\hat{\tau}$ as $Var[\hat{\tau}] = Var\left[\frac{\hat{\eta}}{\hat{\pi}}\right] = \frac{\hat{\pi}^2 Var(\hat{\eta}) + \hat{\eta}^2 Var(\hat{\pi})}{\hat{\pi}^4}$, whose square root is the estimate of the standard error (when variances in the formula must be replaced by their estimates). See Inoue and Solon (2010) for a discussion and an alternative estimator. One might expect the standard error of the first-stage estimate to increase if the first stage were estimated in the exact same way as the reduced form (e.g., by restricting the sample to individuals born in June or July). Note, however, that due to the very small reduced-form estimates, this will have hardly any impact on the standard error of the TS-2SLS-estimates.

thus have completed post-secondary education. For these cohorts, we observe the entire work history from labor market entry onwards, which allows precise computation of their potential and actual labor market experiences. The wage variable refers to April 30 of each year and is deflated using the Consumer Price Index, with 1995 as the base year.¹⁶ We distinguish four educational categories: “no post-secondary education” refers to individuals who graduated from the low or medium track and did not complete an apprenticeship; “apprenticeship” includes individuals who completed an apprenticeship (as part of the formal German vocational education system) but did not complete college or university, regardless of track completed;¹⁷ and “college” and “university” refer to individuals who have graduated from four-year college or five-year university, respectively. Our unemployment variable refers to registered unemployment.

4.2 Microcensus

Our second data come from the scientific use files of the German Microcensus for 1976, 1978, 1980, 1982, 1985, and 1987. We use these data for the first-stage of the TS-2SLS estimate (see equation (1)) which relates track assignment in middle school to date of birth. We restrict the sample to the same birth cohorts as for the social security records, 1961 to 1976. Rather than specifying the exact birth month, the Microcensus provides only a binary indicator for individuals’ being born either during the January through April period or the May through December period. Comparing these two periods is likely to yield an underestimate of the first-stage since the school entry cut-off falls in June/July. We assess this using data from the recent School Census for Bavaria and Hesse, which do contain information on birth month, but are unfortunately not available for the earlier school cohorts studied here.

¹⁶In our data, up to 5 percent of the observations are top-coded at the highest wage level for which social security contributions must be paid. In imputing the censored part of the wage distribution, we assume that residuals are normally distributed and allow for heterogeneity in the variance by age group; see Dustmann, Ludsteck and Schönberg (2009) for details.

¹⁷An apprenticeship in Germany is a structured 2-3 years training program for a particular occupation that combines on the job training in a firm with certified training personnel and state-provided academic education specific to the chosen occupation.

4.3 School Census

The School Census for Bavaria and Hesse covers all students attending general and vocational schools in these two German states. It is available for the academic school years 2004/05 to 2008/09 for Bavaria, and for the academic school years 2002/03 to 2008/09 for Hesse. The data set does include information on track and grade attended, as well as on birth month. We use these data to assess the downward bias in our first-stage estimates resulting from the lack of exact birth month information in the Microcensus data, for the 1990 to 1994 birth cohorts in Bavaria and the 1988 to 1994 birth cohorts in Hesse, respectively. We also use them to illustrate the impact of birth month on the probability that the student graduates from a specialized or general high track school, for the 1986-1987 birth cohorts in Bavaria and the 1984-1987 birth cohorts in Hesse, respectively.

4.4 1987 Census

Our fourth data source is the 1987 census which, unlike the social security records, contains information on the school track individuals graduated from. Like the German Microcensus, however, the 1987 census (the last census in Germany before the 2011 census) includes no information on exact birth month, asking respondents instead whether they were born before or after May 24. We use these data to estimate the effect of being born later versus earlier in the year on the probability that the student graduates from a low, medium or high track school for the 1961 to 1963 birth cohorts who were between 24 and 26 years old at the time of the census and should thus have completed their secondary education. Unlike the School Census, the 1987 Census does not allow us to distinguish between graduation from a general or specialized high track school.

5 RESULTS

5.1 *The Impact of Date of Birth on Track Assignment in Middle School*

We first investigate the effect of date of birth on track assignment in middle school, which is the first stage of our analysis. Since the opportunities to switch tracks between grade 5 and 9 are limited (see Section 2.2), we proxy track assignment in middle school by the track attended at age 14.

Our results in Panel A of Table 2, based on the German Microcensus, show that children born later in the year (May through December) are 2.0 percentage points more likely to attend a high track at age 14 and 1.9 percentage points less likely to attend a low track at age 14 than children born earlier in the year (January through April), for a total effect of 3.9 percentage points. As Table A2 shows, this effect is strongly persistent throughout grades 5 to 9. Based on data from the School Census, the estimates indicate that the effect of date of birth on track assignment is roughly constant throughout middle school.¹⁸

Since the Microcensus lacks information on birth month, these estimates compare children born before and after the end of April, rather than children born before June 30th and after June 30th. As discussed in Section 4.2, this is likely to lead to underestimation in our first-stage regression because it introduces measurement error in the age of school entry. We investigate this possibility using the School Census, which contains information on birth month (although only for recent cohorts who have not entered the labor market yet). Using this data source, we find that children born in July are 5.2 percentage points more likely to attend a high track school and 4.2 percentage points less likely to attend a low track than children born in June, for a total effect of 9.4 percentage points (row (i) of Panel B, Table

¹⁸It follows from these estimates that the weights a ($= 0.019$) and b ($= 0.020$) in equation (3) are roughly equal: the relative weight for the local average treatment effect on long-term outcomes of attending a medium as opposed to a low track school is 0.49 ($0.019/0.039$), while the relative weight for the local average treatment effect of attending a high as opposed to a medium track school is 0.51 ($0.020/0.039$).

2).¹⁹ If instead we reconstruct the same aggregation as in the Microcensus and compare the track assignment of children born between May and December with those born between January and April, we find a smaller first-stage estimate of 7.2 percentage points (row (ii) of Panel B, Table 2). This suggests indeed that the first-stage estimate of 3.9 percentage points in Panel A of Table 2 is likely to be an underestimate of the impact of date of birth on track attendance.

Even after adjusting for measurement error, the first-stage results for the recent cohorts in the School Census appear stronger than those for the older cohorts in the Microcensus (7.2 vs. 3.9 percentage points). In Appendix A. 2, we provide evidence that this difference can be fully explained by the earlier cohorts' being less compliant with the school entry age cut-off rule than the more recent cohorts. Hence, the mechanism generating the date-of-birth effect on track choice—namely, that age of school entry increases the likelihood of attending a more advanced track—seems to be similar for both earlier and more recent cohorts.

5.2 The Effect of Track Assignment in Middle School on Labor Market Outcomes

The previous section shows that the date of birth has a precisely estimated and sizable effect on track assignment in middle school, with at least 3.9 percent of students shifted to a different track because of their date of birth. Thus, since the differences between tracks are large (see Section 2.3), and since only the high track leads to direct admission at colleges and universities, we would expect to find sizeable effects of date of birth on long-term labor market outcomes, such as wages, employment and occupational choice. This is what we investigate in this section, starting with the impact on wages. Our analysis draws on data from the social security records, and restricts the sample to individuals aged 30 and over.

¹⁹The effect of date of birth is further illustrated in Figure A1 in Appendix A. 1, where we display the share of 14-year-olds attending a high (Panel A) or a low (Panel B) track by month of birth. The figure shows a clear discontinuity in track assignment around the June/July school entry cut-off date, confirming that the differences in track assignment between June- and July-born children are indeed driven by differences in age of school entry.

WAGES: We begin with plotting raw log-wages (i.e., wages unadjusted for the experience effect) against date of birth in Panel A of Figure 3. As expected, the figure shows a clear wage disadvantage for individuals born just after the school entry cut-off date (i.e., in July)—since they have accumulated less experience than individuals born just before the school entry cut-off date (i.e., in June). In Panel B, we instead plot log wages adjusted for differences in experience, where we eliminate the experience effect by first estimating the returns to potential experience based on a 4th order polynomial using OLS and then subtracting these estimates from raw log wages. Panel B no longer shows a discontinuity around the school entry cut-off date. Thus, date of birth, and hence the track attended throughout middle school, has no noticeable impact on wages for the marginal student.

In Panel A of Table 3, we present reduced-form estimates for different specifications. While in column (1), we simply compare the educational choices of individuals born in July with those of individuals born in June, the estimates in columns (2) to (4) differ by birth month sampling window (January-December in columns (2) and (3); April-September in column (4)) and the order of the date of birth polynomial included as a control in the discontinuity regression (5th order, 6th order, and 2nd order in columns (2), (3), and (4), respectively). In row (i) we present estimates on raw wages unadjusted for experience. As already illustrated in Figure 3, individuals born just after the school entry cut-off date earn slightly less than individuals born just before the school entry cut-off date according to all specifications. However, once we adjust wages for experience as shown in row (ii), all reduced-form estimates are statistically insignificant and closely centered around zero, ranging from (neg.) 0.09% in specifications (1) and (4) to (pos.) 0.02% in specification (3).²⁰

These very small reduced-form estimates imply that—despite the stark differences be-

²⁰These specifications compare wages of individuals born just before the school entry cut-off date with those born just after the school entry cut-off date, in the same year. Hence, these individuals belong to different school cohorts. We have also compared wages (and other labor market and educational outcomes) of individuals born in July with those born in June one year later, and who thus belong to the same school cohort. The findings we obtain are very similar.

tween tracks—the impact of track attendance in middle school on wages is close to zero for the marginal student. Dividing the reduced-form estimates in Table 3 by the first-stage estimate of 0.039 (see Panel B of Table 3), we obtain point estimates of the impact of track attendance in middle school on wages ranging from (neg.) 2.4% (specification (1)) to (pos.) 0.6% (specification (3)), with 95% confidence intervals of $[-0.073, 0.025]$ and $[-0.057, 0.068]$, respectively. Note that both these point estimates and the lengths of the confidence intervals are likely to be exaggerated, as the first-stage estimate of 0.039 is underestimated, as we explain above.²¹ Nevertheless, it is obvious from the reduced-form estimates that our key conclusion—that is, that the track attended in middle school has little impact on wages for the marginal student—does not depend on the exact magnitude of the first-stage estimate.

Our results remain robust to alternative ways of accounting for differences in potential experience between individuals born shortly before and shortly after the age of school entry cut-off (see Table 4). In column (2), we allow for a more flexible functional form using a set of dummy variables to model the returns to potential experience (instead of a 4th order polynomial as in column (1)). Results are almost identical. In columns (3) and (4), we restrict the sample to individuals aged 40 years and older whose wages are flat with respect to potential experience. Indeed, for these individuals, both the raw and experience-adjusted wage differentials are very small and not statistically significant from zero. Our reduced-form estimates of the impact of date of birth on wages therefore do not depend on the particular way we eliminate the experience effect.

OTHER LABOR MARKET OUTCOMES: Track attendance in middle school may have an impact on other labor market outcomes besides wages. We focus on three outcomes: the share of days in registered unemployment since labor market entry, the share of days working full time since labor market entry, and current occupational choice (distinguishing between

²¹As can be seen from footnote 15, the standard error of the TS-2SLS estimate will be the larger the smaller the first-stage estimate. Hence, the downward bias of the first stage estimate will exaggerate the length of the confidence interval.

blue- and white-collar occupations).²²

While the effects on the track attended in middle school on labor force participation and unemployment are interesting in their own right, large effects on these outcomes could also potentially bias the wage results which refer only to individuals who are working. We report reduced-form (Panel A) and TS-2SLS (Panel B) estimates for these outcomes in rows (i) and (ii) of Table 5, estimating the same specifications as in Table 3. The figures show that the effect of date of birth on the share of days spent in unemployment or working is basically zero in the reduced-form regressions in Panel A. It follows that the TS-2SLS estimates in Panel B of track assignment in middle school on labor force participation and unemployment are small and statistically insignificant. Thus, track assignment has little effect on labor force participation or unemployment. Furthermore, these results imply that our wage results in Table 3 do not suffer from a selection bias into employment.

We report results for occupational choice in row (iii). Historically, the low track prepares students for apprenticeships in blue-collar occupations, whereas the medium track prepares them for apprenticeships in white-collar occupations (see Section 2.2). Yet, the reduced-form effect of date of birth on the probability that an individual works in a white collar profession is close to zero. Consequently, the TS-2SLS estimates of the impact of track attendance on occupational choice is, for the marginal student, likewise close to zero (see Panel B). Once again, this conclusion does not depend on the exact magnitude of the first-stage estimate.

Overall, our findings show that, despite the large differences between tracks, the track attended in middle school has no impact on a variety of long-term labor market outcomes, such as wages, occupational choice, and unemployment. That points at students who have been mis-tracked due to their birth month to adjust their initial track allocation at a later stage in their educational career. Our discussion in Section 2.2 highlights that the German school system has a number of built-in possibilities that allow for such re-tracking after grade

²²Since we find no statistically significant differences between men and women for these outcomes (and educational outcomes discussed later), we report results jointly for both genders from now onwards.

9 or 10. We now investigate whether and to what extent such up- and downgrading takes place.

5.3 *Up- and Downgrading*

As discussed above, the German education system allows for two upgrading possibilities from medium to high track schools after middle school (i.e., after 10th grade), by changing to *general* high track schools, i.e. the same schools attended by high track students throughout middle school, and by attending *specialized* high track schools, i.e. schools specifically designed for graduates from medium track schools to gain access to college or university (see Figure 1). There may also be downgrading, through leaving a high track school after 10th grade or by not enrolling in college or university after graduating from a high track school.

MOVEMENTS BETWEEN THE MEDIUM AND HIGH TRACK: We first investigate movements between the medium track and *general* high track at the end of middle school, drawing on data from the School Census. In Panel A of Table 6, row (i) we contrast the difference between students born in July and June in the probability of attending the high track in middle school (proxied by track attendance at age 14) with the difference in the probability of graduating from the general high track (measured at age 22). The figures point to a substantial amount of movements between the medium and general high track: whereas children born in July are 5.2 percentage points more likely to attend the high track in middle school (see also our first-stage estimates in Panel B and row (i) of Table 2), they are only 2.0 percentage points more likely to graduate from that track. This decline could either be due to downgrading of July-born students who attend the high track in middle school and leave that track after 10th grade, or due to an upgrading of June-born students who attend the medium track in middle school and enroll in a general high track school after 10th grade. Since the share of students who are enrolled in the high track in middle school is roughly the same as the share of students who graduate from a general high track school (around 36% according to own calculations based on the School Census for Bavaria and Hesse), the decline reflects both an

upgrading of students born in June, and a downgrading of students born in July.²³

These numbers consider movements between the medium and *general* high track only. Next, we additionally investigate movements between the medium and *specialized* high track. Remarkably, the difference between June- and July-born children disappears entirely once we include students who graduate from a specialized high track school in our measure for high track completion (row (ii) in Panel A). This suggests that, for marginal students who attend a different track because of their date of birth, the track attended in middle school has no impact on the probability of graduating from any high track school that grants access to college or university. It also points to the important role that specialized high track schools play in explaining why track attendance in middle school has little impact on long-term outcomes. Also note that our calculations based on the School Census for Bavaria and Hesse show that the share of students who graduate from a general or specialized high track is at around 52%, considerably larger than the share of students who attend the high track in middle school (around 36%). This suggests that both July- and June-born students upgrade from the medium to the specialized high track, but June-born students do so more frequently than July-born students.

The finding so far refers to recent birth cohorts who have not entered the labor market yet. We also confirm similar movements between the medium and any high track at the end of middle school for earlier cohorts, using data from the 1987 Census (row (iii) in Panel A): whereas being born later during the year (the 1987 Census does not contain exact information on birth month) increases the probability of attending a high track school during middle school by 2.0 percentage points (see our first-stage estimate in Panel A of Table 2), it has no impact on the probability of completing a general or specialized high track school.

MOVEMENTS BETWEEN THE LOW AND MEDIUM TRACK: As discussed in Section 2.2, the German school system also provides an opportunity to upgrade from a low to a medium

²³Since the School Census does not contain student identifiers, we are not able to investigate transitions between the medium and general high track directly.

track school after 9th grade. We investigate movements between these two tracks in Panel B of Table 6, drawing again on data from the 1987 Census. The results indicate that there is also a substantial amount of movement between the low and medium track: whereas children born in July are 1.9 percentage points more likely to attend at least the medium track in middle school than children born in June (see our first-stage estimate in Panel A of Table 2), they are only 0.3 percentage points more likely to graduate from at least a medium track school.

DOWNGRADING AFTER GRADUATION FROM THE HIGH TRACK: A third adjustment possibility exists after graduation from a (specialized or general) high track school in the form of choosing not to pursue a college or university education. We investigate this form of downgrading in Figure 4, where, based on data from the social security records, we plot the share of students who do not attend college—although they graduated from a high track school (general or specialized)—against date of birth. The figure reveals a clear discontinuity around the school entry cut-off date: among individuals who graduated from a high track school, those born after the cut-off date are about 0.7 percentage points more likely *not* to complete college or university than individuals born after the cut-off date.

In sum, these results provide strong evidence that students who are shifted to a more or less advanced track in middle school because of their date of birth make use of the flexibilities built into the German tracking system: They revise their track choices made after elementary school at later stages in the educational career, both in the form of upgrading to a more advanced track at the end of middle school and in the form of downgrading by not enrolling in college or university after graduation from a high track school.

5.4 The Impact of Track Assignment in Middle School on Highest Degree Completed

Given the substantial amount of up- and downgrading of marginal students after middle school, how does the track attended in middle school affect the highest degree completed? We investigate this in Figure 5 where we plot the highest degree completed (by age 30 and over)

against date of birth, using data from the social security records. We distinguish between three disjoint post-secondary education outcomes: no post-secondary education (Panel A), apprenticeship (Panel B, the typical pathway for graduates from the low or medium track), college or university (Panel C, the typical pathway for graduates from the specialized or general high track). Remarkably, neither figure shows a clear discontinuity around the school entry cut-off date (the vertical line), suggesting that the up- and downgrading that takes place after middle school fully eliminates the impact of date of birth, and hence the track attended in middle school, on education completed.²⁴

We report the corresponding reduced-form estimates in Table 7, using the same specifications as in Table 3. All specifications yield similar results, and confirm the overall picture apparent from Figure 4 that date of birth has no evident impact on education completed.²⁵ Interestingly, being born in July rather than June (and thus being older at school entry) appears to have, if at all, a negative effect on university or college education. This is fully consistent with the downgrading we illustrate in the previous section.

6 DISCUSSION AND CONCLUSION

Although a tracking system may be more efficient in tailoring education to the needs of students than a comprehensive system, it has the inherent problem of misallocating students to tracks because of incomplete or erroneous information about the student's learning potential at the time of the tracking decision. This problem may be addressed by the school system if it allows for possibilities to revise initial choices at a later stage, through providing students whose potential is higher (or lower) than initially projected with opportunities to switch tracks.

²⁴The figures reveal strong seasonal effects, with children born in April or May generally outperforming children born in January, similar to those documented by Buckles and Hungerman (2012). Note that, as emphasized in Section 3, seasonal effects do not pose a threat to our identification strategy as long as it is random whether a woman gives birth just before or just after the school entry cut-off date July 1.

²⁵The effects of track choice can be easily obtained by dividing these coefficients by the first stage estimate of 0.039.

Our paper is the first to analyze the long-term effects of such flexibilities for a tracking system that is not only among the most rigorous, but that also tracks students earliest, around the age of 10. At the same time, this tracking system has an array of built-in possibilities that allow students to modify initial choices at a later stage, after middle school.

Remarkably, we find that, for these marginal students (i.e. students that are close to the threshold between two different tracks), the track attended in middle school has no impact on long-term outcomes such as wages, days worked, unemployment, or occupational choice. We provide evidence that this is because these students take advantage of the flexibilities built into the tracking system, and demonstrate that re-tracking takes the form of both an upgrading of marginal students from the middle to the high track after 10th grade, and a downgrading of students who complete the high track but fail to enroll in college or university. Due to this up- and downgrading, the track attended in middle school has little effect on highest degree completed for marginal students.

Not only are our results important for the debate on the merits and disadvantages of tracking systems, they also suggest that marginal students can make up for substantial weaknesses in earlier education provided they gain access to high quality schools and universities at later stages in their educational careers—even if the exposure to a less challenging school environment is prolonged (in our case 5-6 years) and the differences between school environments in terms of teaching technology and peer and teacher quality are large.

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A APPENDIX: ADDITIONAL RESULTS

-Table A1 here-

-Table A2 here-

A. 1 *The Impact of Birth Month and Track Assignment in Middle School*

Figure A1 plots the share of students who at age 14 were attending the high (Panel A) or the low (Panel B) track against birth month based on the school census data. The figure shows a clear discontinuity in track choice around the school entry cut-off date: children born in July are 5.2 percentage points more likely to attend the high track and 4.2 percentage points less likely to attend the low track than children born in June, for a total effect of 9.4 percentage points.

-Figure A1 here-

A. 2 *Comparison of First Stage for Recent Cohorts (School Census) and Older Cohorts (Microcensus)*

A comparison of the estimates in Panel A and Panel B of Table 3 clearly indicates a stronger impact of date of birth on track choice for the recent birth cohorts in Bavaria and Hesse than for the older birth cohorts in West Germany, even taking into account the measurement error in the school entry cut-off (9.4% versus 5.0%)²⁶. We next investigate whether this difference can be explained by the older cohorts' being less compliant with the school entry cut-off rule than the more recent birth cohorts. Because Bavaria does not record the year of school entry, meaning we cannot compute its compliance rate, we restrict the analysis to Hesse, whose first-stage estimate is 7.3 percent (compared to a 9.4 percent first-stage estimate for Hesse and Bavaria combined). The compliance rate for the recent birth cohorts in Hesse is 33.5 percent, whereas that for the older birth cohorts (here proxied by the 1963 birth cohort in the 1970 Census for West Germany) is only 21.9 percent.²⁷ To estimate the impact of relative age (i.e., being one year older at school entry) on track choice, we divide the effect of birth month on track choice by the effect of birth month on age of school entry and obtain an effect of 23.0 percentage points (0.050/0.219) for the older birth cohorts. For the younger birth cohorts, we estimate a very similar number: 21.8 percentage points (0.078/0.335). Hence, the lower first-stage results for the 1961-1976 cohorts in West Germany than for the recent cohorts in Bavaria and Hesse are wholly explained by the older cohorts' being less compliant with the school entry age cut-off rule than the more recent cohorts.

²⁶The latter number is computed as follows. By dividing the first-stage estimate based on children born in June or July of 9.4% (Table 3, Panel B, row (i), column (3)) with the first-stage estimate based on children born earlier or later during the year of 7.2% (Table 3, Panel B, row (ii), column (3)), we can derive an adjustment factor of 1.29 (0.094/0.072). Multiplying the estimates in Panel A by this adjustment factor indicates that, for the 1961 to 1976 birth cohorts, children born in July are 5 percentage points more likely to attend a more academic middle school than students born in June.

²⁷Children in the 1963 cohort who were born July through December should not yet have been in school when the census was carried out in spring 1970, whereas children born January through June should have already been attending school. However, regressing an indicator for school attendance on an indicator for being born in July or June results in a coefficient of only 0.219; that is, a compliance with the school entry cut-off rule of 21.9 percent.

A. 3 Birth Month and Graduation from the Higher School Track

Because the school census contains no direct information on degrees obtained, we proxy graduation from the general high track by first counting the number of students who, by age 17, 18, and onward, had ever attended a general high track school in grade 13, making sure not to double count students who repeated a grade. Because students drop out of the census on leaving school, we divide this number by the total number of children born in the month, year, and state. We proxy graduation from a specialized high track school as the ratio of the number of students who, by age 22, had ever attended a specialized high track school in grade 12 to the total number of children born in the month, year, and state:

$$\hat{p} = \frac{\# \text{ ever reached grade 13 (12)}}{\# \text{ births}}$$

The standard error of this share is estimated as the square root of

$$Var(\hat{p}) = \frac{\hat{p}(1 - \hat{p})}{\# \text{ births}}$$

The variance of the difference in the shares between July and June born children is given by

$$Var(\hat{p}_{July} - \hat{p}_{June}) = \frac{\hat{p}_{July}(1 - \hat{p}_{July})}{\# \text{ births}_{July}} + \frac{\hat{p}_{June}(1 - \hat{p}_{June})}{\# \text{ births}_{June}}$$

B MODEL APPENDIX

B. 1 Set-up

Our model assumes three periods: periods 1 and 2, which are schooling periods, and period 3, which is the working period. The beginning of the first period corresponds to the end of elementary school (grade 4) when parents decide on which type of middle school their child should attend. The end of this period corresponds to the end of middle school (grade 9 or 10) when children have the opportunity to switch tracks. The second period corresponds to the period of secondary education, and all children enter the labor market at the beginning of period 3. This setup, it should be noted, assumes that the tracks are of equal length, an assumption motivated by our focus on the impact of the learning environment, and of track switching, on long-term outcomes. For simplicity, we distinguish only two tracks, low (L) and high (H).

In this model, children differ in both their "ability", denoted by a , and month of birth, which here is limited to children born in June or July. In line with the independence and exclusion assumptions discussed in Section 3.1, at the beginning of period 1, when the initial track choice is made, children born in July outperform children born in June by Δ , but by the beginning of period 2, when the initial decision can be revised, this advantage has disappeared.

LEARNING ABOUT CHILDREN'S ABILITY: The children's ability is initially uncertain. At the beginning of period 1, parents receive a noisy signal (e.g., a school grade) about their child's ability, denoted by θ , expressed as $\theta_i = a_i + \varepsilon_i$ for children born in July and $\theta_i = a_i - \Delta + \varepsilon_i$ for children born in June. Parents use this signal to update their own beliefs about their child's ability; updated beliefs are denoted here by $\hat{a} = E[a|\theta]$. Assuming that a_i and ε_i are normally distributed with mean μ and variance σ_a^2 and with mean zero and variance σ_ε^2 , $E[a|\theta, \text{July}] = \hat{a} = \frac{\sigma_a^2\theta + \sigma_\varepsilon^2\mu}{\sigma_a^2 + \sigma_\varepsilon^2}$ for children born in July, and $E[a|\theta, \text{June}] = \hat{a} = \frac{\sigma_a^2(\theta + \Delta) + \sigma_\varepsilon^2\mu}{\sigma_a^2 + \sigma_\varepsilon^2}$ for children born in June. This formula reflects that if the same signal is observed for two students, one is born in June and the other in July, the student born in June has a higher expected ability. We use $G(\hat{a})$ and $F(a|\hat{a})$, respectively, to denote the cumulative distribution functions of the updated ability \hat{a} and the true ability a , conditional on \hat{a} . $G(\hat{a})$ is normally distributed with mean μ and variance $\frac{\sigma_a^4}{\sigma_a^2 + \sigma_\varepsilon^2}$, while $F(a|\hat{a})$ is normally distributed with mean \hat{a} and variance $\frac{\sigma_a^2\sigma_\varepsilon^2}{\sigma_a^2 + \sigma_\varepsilon^2}$. By the end of the first period, the child's ability is fully revealed, and parents and children can revise their track choice accordingly.

SWITCHING COSTS: Switching from a low to a high track school, however, can be costly. Conversely, we assume that moving down from a high to a low track school is costless. We distinguish between two types of switching costs, institutional switching costs (c^i) and academic switching costs (c^a). Institutional switching costs reflect the restrictions the education system puts on upgrading from the low to the high track. For instance, a very rigid tracking system may not offer students the possibility to move up from the low to the high track, amounting to infinite institutional switching costs. Even if the tracking system provides students with opportunities to upgrade, it may be academically costly to do so, reflecting that the less intensive and less abstract teaching method at a low track school (see Section 2.2)

may make it difficult for students to keep up with the more advanced learning material at a high track school. We model institutional switching costs as a utility cost which students incur once at the end of period 1 when switching from the low to the high track. In contrast, academic switching costs are modeled as a productivity cost which students incur both in period 2 while in school and in period 3 while working.

DIFFERENCES BETWEEN TRACKS—PEERS: We summarize the productivity or performance of June- versus July-born students in each school track and each period in Table A3. We focus on the two most important differences between the two tracks: teaching technology and peers (see Table 1).²⁸ To model peer effects, we use a standard linear-in-means peer model in which a higher average peer ability increases the productivity of all students in the same way. We assume that parents take the expectations of the average ability of students in each school type as a given, thereby ignoring the possibility that they can manipulate other parents' expectations through their own track choice. Let $\hat{\Pi}_t^j$ denote parental expectations about the average ability of peers in track j ($j = L, H$) in period t ($t = 1, 2$). To contrast parental expectations from the average ability which is realized in equilibrium, we denote the latter by Π_t^j . We allow the contemporaneous effect on productivity of peers in period 1 or 2 to differ from their effect on productivity in future periods, and denote the effects of peers to whom students are exposed in period t on productivity in period k by γ_t^k .

DIFFERENCES BETWEEN TRACKS—TEACHING TECHNOLOGY: To model the differences in teaching technology between the school tracks, we assume that learning technology is more sensitive to ability in a high than in a low track school. Specifically, the learning technology in the two tracks is linear in the student's ability a and thus is given by $\alpha^j + \beta^j a$, with $j = L, H$ and $\alpha^L > \alpha^H$ and $\beta^L < \beta^H$. These assumptions reflect the fact that the teaching technology in a low track school is more adapted to children who are drawn from the lower part of the ability distribution.

As an example, contrast the productivity in period 3 of an individual who attended a low track school in period 1 and a high track school in period 2 ($\alpha^H + \beta^H a + \gamma_2^3 \hat{\Pi}_2^H + \gamma_1^3 \hat{\Pi}_1^L - c^a$) with that of an individual of the same ability who attended a high track school in both periods ($\alpha^H + \beta^H a + \gamma_2^3 \hat{\Pi}_2^H + \gamma_1^3 \hat{\Pi}_1^H$). The difference in the productivity between these two individuals reflects the direct effect of the track attended in period 1, conditional on the track attended in period 2, on the long-term outcomes in period 3. According to our set-up, this effect operates through two channels: first, the individual who attended a high track school in both periods has been exposed to better peers in period 1, which may boost labor market performance even when both individuals received the same type of education in period 2 (compare $\gamma_1^3 \hat{\Pi}_1^L$ and $\gamma_1^3 \hat{\Pi}_1^H$). Second, the individual who attended a high track school in both periods has not suffered the academic cost (c^a) of upgrading from a low to a high track school.

²⁸Teacher effects can be easily incorporated in our model. Better teachers have a similar effect as better peers.

B. 2 Equilibrium

At the beginning of periods 1 and 2, parents (or in period 2, students) choose the track that will yield the highest lifetime utility, which we model as the sum of the student's utility in each period. For simplicity, we assume that in each period, student utility is equal to student productivity (as described in Table A3) minus the institutional switching costs and ignore discounting.²⁹

PERIOD 2 DECISION: First, we analyze the parents' decision problem at the beginning of period 2, when the student's ability is fully known. Because in period 2, there is no difference between children born in June and those born in July, the decision problem is the same for both groups. Consider first a student who attended a high track school in period 1 who thus does not have to bear the switching costs if she wants to continue at a high track school in period 2. Figure A.2 illustrates the decision problem. In the figure, we plot, using the solid lines, the student's utility from attending a high or a low track school in period 2 against her ability a . Since the teaching technology in a high track school is more sensitive to ability than in a low track school (i.e., $\beta^H > \beta^L$), the student's utility in a high track school is increasing in ability at a faster rate than in a low track school. Hence, there is an ability threshold a_H^* such that all students whose ability is identified as below a_H^* sort into low track schools, while all students whose ability is identified above a_H^* upgrade to a high track school.

Formally, a student who attended a high track school in period 1 prefers a low over a high track school in period 2 if her utility at a low track school exceeds that at a high track school:

$$\begin{array}{rcc}
 \underbrace{\alpha^L + \beta^L a + \gamma_1^2 \hat{\Pi}_1^H + \gamma_2^2 \hat{\Pi}_2^L}_{\text{flow utility in period 2, low track in period 2}} & + & \underbrace{\alpha^L + \beta^L a + \gamma_1^3 \hat{\Pi}_1^H + \gamma_2^3 \hat{\Pi}_2^L}_{\text{flow utility in period 3, low track in period 2}} & > \\
 \underbrace{\alpha^H + \beta^H a + \gamma_1^2 \hat{\Pi}_1^H + \gamma_2^2 \hat{\Pi}_2^H}_{\text{flow utility in period 2, high track in period 2}} & + & \underbrace{\alpha^H + \beta^H a + \gamma_1^3 \hat{\Pi}_1^H + \gamma_2^3 \hat{\Pi}_2^H}_{\text{flow utility in period 3, high track in period 2}} & >
 \end{array}$$

Solving for a_H^* yields

$$a_H^* = \frac{2(\alpha^L - \alpha^H) - (\gamma_2^2 + \gamma_2^3)(\hat{\Pi}_2^H - \hat{\Pi}_2^L)}{2(\beta^H - \beta^L)}$$

Note that the quality of peers the student was exposed to in period 1 ($\hat{\Pi}_1^H$) affect the student's utility in a high and low track school in period 2 equally, and thus do not affect the student's switching decision.

Next, we consider a student who attended a low track school in period 1. This student has to bear the institutional and academic switching costs when upgrading from a low to a high track school in period 2. This lowers her utility in a high track school, as indicated by the dashed line in Figure A2. It follows that the ability threshold at which this student is indifferent between the two tracks, a_L^* , is above the threshold at which the student who

²⁹More generally, student utility could in each period be a positive monotonic transformation of student productivity, meaning that the transformation could differ in each period.

attended a high track in period 1 is indifferent between the two tracks, a_H^* . It is easy to show that the difference between the two indifference thresholds is increasing in the switching costs:

$$a_L^* = a_H^* + \frac{c^i + c^a}{2(\beta^H - \beta^L)} \quad (\text{B-1})$$

PERIOD 1 DECISION: Next, we consider the parents' decision problem in period 1, when the student's ability is uncertain. Here, parents take into account the optimal switching behavior in period 2 and choose a low over a high track school if the child's expected utility of attending a low track school exceeds that of attending an high track school. In Section B. 5, we again show that a threshold exists such that all students with expected ability below the threshold attend a low track school, whereas all students with expected ability above the threshold attend a high track school. Because of the initial disadvantage faced by June-born students in period 1, this threshold is smaller for students born in July than for those born in June, $\hat{a}_{\text{July}}^* < \hat{a}_{\text{June}}^*$.

We illustrate the impact of month of birth on track choice in period 1 in Figure A3. Students with expected ability below \hat{a}_{July}^* attend a low track school in period 1 no matter whether they are born in June or July ("never takers"). In contrast, students whose expected ability falls above \hat{a}_{June}^* attend a high track school in period 1 regardless of their date of birth ("always takers"). Students who are shifted from a low to a high track school because of a July birth ("compliers") are students with expected ability between \hat{a}_{July}^* and \hat{a}_{June}^* who are close to indifferent between the two tracks. This leads to the following proposition:

Proposition 1: Students who are shifted between tracks because of their month of birth ("compliers") are students whose expected ability falls close to indifferent between school tracks, in the range $[\hat{a}_{\text{July}}^*, \hat{a}_{\text{June}}^*]$.

See Section B. 5 for an illustration.

Note that in this model, parents send their June-born child with expected ability between \hat{a}_{July}^* and \hat{a}_{June}^* to a low track school even though they know that their child would have attended a high track school had she been born in July, and even though they know that the initial disadvantage of a younger age at school entry fully disappears by the end of the first period. Why are parents willing to do this? In our model, the answer is that when deciding which type of school their child should attend, parents take into account the student's utility in the first period.³⁰ Hence, parents trade off a higher utility in the present for a lower utility in the future. Alternatively, we could assume that parents are unaware that students born in June perform more badly on average at the beginning of period 1 than students born in July simply because they are younger. In this case, parents may base their school type decision for period 1 solely on the signal θ and ignore that a child born in June who has the

³⁰This was one of the key arguments of Larry Summers in the "Tiger Mom" debate. Quoting from Gerard Baker's article "Larry Summers vs. the Tiger Mom" in the Wall Street Journal of January 29, 2011, "People on average live a quarter of their lives as children. That's a lot," Mr. Summers said. "It's important that they be as happy as possible during those 18 years. That counts too."

same school grades as a child born in July (but is relatively younger) has a higher expected ability. Modeling track choice in period 1 in this way has no impact on the key results for our model.

B. 3 Implications

We now turn to the implications of the model, beginning with the impact of track attendance in period 1 on track attendance in period 2.

TRACK COMPLETION: Consider the local average treatment effect of attending a high track school in period 1 (denoted by H_1) on the probability of completing that track (i.e., of attending an H school in period 2, denoted by H_2). Dividing the reduced-form effect (i.e. $\Pr(H_2|Z_i = \text{July}) - \Pr(H_2|Z_i = \text{June})$) by the first-stage effect (i.e. $\Pr(H_1|Z_i = \text{July}) - \Pr(H_1|Z_i = \text{June})$), which corresponds to equations (1) and (2) in Section 3.2, yields

$$\frac{\Pr(H_2|Z_i = \text{July}) - \Pr(H_2|Z_i = \text{June})}{\Pr(H_1|\text{July}) - \Pr(H_1|\text{June})} = \frac{\int_{\hat{a}_{\text{July}}^*}^{\hat{a}_{\text{June}}^*} (F(a_L^*|\hat{a}) - F(a_H^*|\hat{a}))dG(\hat{a})}{G(\hat{a}_{\text{June}}^*) - G(\hat{a}_{\text{July}}^*)}$$

This effect depends on the difference between the thresholds a_L^* and a_H^* , which in turn depends on the switching costs c^i and c^a (see equation (B-1)). The higher the switching costs, the larger the difference between the thresholds a_L^* and a_H^* , and the larger the impact of track attendance in period 1 on track attendance in period 2. If there are no switching costs, then $a_L^* = a_H^*$ and the track attended in period 1 has no impact on track completion. We summarize our findings the following proposition:³¹

Proposition 2: The local average treatment effect of attending a high track in period 1 on attending that track in period 2 increases in the academic and institutional switching costs.

WAGE EFFECTS: Consider the local average treatment effect of attending a high (versus a low) track school in period 1 on wages (assumed to be equal to productivity) in period 3, which we again obtain by dividing the reduced-form effect (i.e. $\Pr(w_3|Z_i = \text{July}) -$

³¹This intuitive argument considers only the direct effect of switching costs on track completion through its effect on a_L^* and a_H^* and ignores that switching costs also affect the ability thresholds \hat{a}_{July}^* and \hat{a}_{June}^* . We have confirmed the positive relationship between switching costs and the impact of the track attended in period 1 on the track attended in period 2 by running extensive simulations that take these indirect effects into account.

$\Pr(w_3|Z_i = \text{June})$) by the first-stage effect:

$$\begin{aligned}
\frac{E(w_3|Z_i = \text{July}) - E(w_3|Z_i = \text{June})}{\Pr(H_1|Z_i = \text{July}) - \Pr(H_1|Z_i = \text{June})} &= \underbrace{\gamma_1^3(\hat{\Pi}_1^H - \hat{\Pi}_1^L)}_{\text{peers}} \\
&+ \underbrace{\frac{\int_{\hat{a}_{\text{July}}^*}^{\hat{a}_{\text{June}}^*} (1 - F(a_L^*|\hat{a}))c^a dG(\hat{a})}{G(\hat{a}_{\text{June}}^*) - G(\hat{a}_{\text{July}}^*)}}_{\text{academic switching costs}} + \\
&\underbrace{\frac{\int_{\hat{a}_{\text{July}}^*}^{\hat{a}_{\text{June}}^*} (F(a_L^*|\hat{a}) - F(a_H^*|\hat{a}))(\alpha^H - \alpha^L + \gamma_2^3(\hat{\Pi}_2^H - \hat{\Pi}_2^L)) + (\beta^H - \beta^L) \int_{\hat{a}_H^*}^{\hat{a}_L^*} adF(a|\hat{a})dG(\hat{a})}{G(\hat{a}_{\text{June}}^*) - G(\hat{a}_{\text{July}}^*)}}_{\text{educational pathway effect}}
\end{aligned} \tag{B-2}$$

The first term in equation (B-2) reflects the exposure to better peers in period 1 on productivity in period 3 (i.e., $\gamma_1^3(\hat{\Pi}_1^H - \hat{\Pi}_1^L)$). The second effect reflects the academic of switching from a low to a high track school, weighted by the probability that a “complier” upgrades to the high track (i.e., $\int_{\hat{a}_{\text{July}}^*}^{\hat{a}_{\text{June}}^*} (1 - F(a_L^*|\hat{a}))c^a dG(\hat{a}) / (G(\hat{a}_{\text{June}}^*) - G(\hat{a}_{\text{July}}^*))$). These two effects may be viewed as the *direct* effects of track attendance in period 1 on productivity in period 3, holding track attendance in period 2 constant. These two effects become larger, the larger the impact of peers in period 1 on productivity in period 3 (i.e., the larger γ_1^3), and the larger the academic switching costs c^a .³²

The track attended in period 1 affects wages also through the track attended in period 2. This *educational pathway* effect, captured by the third term in equation (B-2), becomes larger, the larger the impact of the track attended in period 1 on the track attended in period 2 (i.e., the larger the academic and institutional switching costs, a_L^* versus a_H^*), and the larger the effect of peers in period 2 on wages in period 3 (i.e., γ_2^3).³³

Note that if both academic and institutional switching costs are zero, then $a_L^* = a_H^*$ and the track attended in period 1 has no impact the track completed in period 2; hence, the third term in (B-2) disappears, as does the second term. The effect of track attendance in period 1 on wages in period 3 reduces to the impact of peers in period 1 on future wages; that is, equation (B-2) becomes $\gamma_1^3(\hat{\Pi}_1^H - \hat{\Pi}_1^L)$. These observations lead to the following proposition:

Proposition 3: (i) (Direct Effect) The direct effect of attending a high (versus a low) track school in period 1 on wages in period 3, holding constant the track attended in period 2, is larger the higher the importance of peers in period 1 (γ_1^3) and the higher the academic switching costs (c^a). (ii) The educational pathway effect of attending a high (versus a low) track school in period 1, operating through track completion in period 2, on wages in period

³²The intuitive argument here again ignores the effect of the switching costs on the indifference thresholds \hat{a}_{July}^* and \hat{a}_{June}^* . We have confirmed that this relation holds in equilibrium using extensive simulations.

³³Again, this argument here considers the direct effects only and ignores the effects of δ_2^3 and γ_2^3 on the indifference thresholds, but we confirmed that this relation holds in equilibrium using extensive simulations.

3 is larger the higher the institutional and academic switching costs (c^a and c^i) and, provided that switching costs are positive, the higher the importance of peers in period 2 (γ_2^3). (iii) If both the academic and institutional switching costs are zero, the overall effect of attending a high track school in period 1 on wages in period 3 is equal to the impact of peers in period 1 on wages in period 3 (i.e., $\gamma_1^3(\hat{\Pi}_1^H - \hat{\Pi}_1^L)$).

B. 4 Interpretation

What do our empirical findings imply when viewed through the lens of the model? Recall that we find that the track attended in middle school has little impact on completed education, both because students born in June upgrade and because students born in July downgrade. From Proposition 2, this implies that in the German tracking system, both the institutional and academic switching costs are very small. Our findings in Table 6 further suggest that "specialized" high track schools, which are designed to provide access to college or university to students from low or medium track schools, are an important reason for this.

Since the track attended in middle school has no impact on track completion, the effect of track attendance in middle school on wages reduces to the effect of peers in middle school (i.e., in period 1) on wages (see Proposition 3 (iii)). Our finding of a small wage effect thus suggests that this effect is close to zero (i.e., γ_1^3 in our model are close to zero).

It should be noted that our findings are not informative about the importance of peers students are exposed to after middle school (i.e., about γ_2^3 in our model). In principle, our findings are consistent with large peer effects in high school (i.e., period 2 in our model). Our findings imply, however, that conditional on the peers students are exposed to in high school (i.e., in period 2), their peers and teachers in middle school (i.e., in period 1) do not matter.

B. 5 Illustration of Proposition 1

ABILITY CUT-OFFS: In period 1, when student ability is uncertain, parents whose child is born in July and has the expected ability \hat{a} send their child to a high track school if

$$\begin{aligned}
& \underbrace{\alpha^H + \beta^H \hat{a} + \gamma_1^1 \hat{\Pi}_1^H}_{\text{first period, high track}} + \underbrace{F(a_H^*|\hat{a}) \left(\alpha^L + \beta^{L-\infty} \frac{\int_{a_H^*}^{a_H^*} adF(a|\hat{a})}{F(a_H^*|\hat{a})} + \gamma_1^2 \hat{\Pi}_1^H + \gamma_2^2 \hat{\Pi}_2^L \right)}_{\text{2nd period, downgrade to low track}} + \\
& \underbrace{(1 - F(a_H^*|\hat{a})) \left(\alpha^H + \beta^H \frac{\int_{a_H^*}^{\infty} adF(a|\hat{a})}{1 - F(a_H^*|\hat{a})} + \gamma_1^2 \hat{\Pi}_1^H + \gamma_2^2 \hat{\Pi}_2^H \right)}_{\text{2nd period, stay in high track}} + \underbrace{F(a_H^*|\hat{a}) \left(\alpha^L + \beta^{L-\infty} \frac{\int_{a_H^*}^{a_H^*} adF(a|\hat{a})}{F(a_H^*|\hat{a})} + \gamma_1^3 \hat{\Pi}_1^H + \gamma_2^3 \hat{\Pi}_2^L \right)}_{\text{3rd period, downgrade to low track}} \\
& + \underbrace{(1 - F(a_H^*|\hat{a})) \left(\alpha^H + \beta^H \frac{\int_{a_H^*}^{\infty} adF(a|\hat{a})}{1 - F(a_H^*|\hat{a})} + \gamma_1^3 \hat{\Pi}_1^H + \gamma_2^3 \hat{\Pi}_2^H \right)}_{\text{3rd period, stay in high track}} > \\
& \underbrace{\alpha^L + \beta^L \hat{a} + \gamma_1^1 \hat{\Pi}_1^L}_{\text{first period, low track}} + \underbrace{F(a_L^*|\hat{a}) \left(\alpha^L + \beta^{L-\infty} \frac{\int_{a_L^*}^{a_L^*} adF(a|\hat{a})}{F(a_L^*|\hat{a})} + \gamma_1^2 \hat{\Pi}_1^L + \gamma_2^2 \hat{\Pi}_2^L \right)}_{\text{2nd period, stay in low track}} + \\
& \underbrace{(1 - F(a_L^*|\hat{a})) \left(\alpha^H + \beta^H \frac{\int_{a_L^*}^{\infty} adF(a|\hat{a})}{1 - F(a_L^*|\hat{a})} + \gamma_1^2 \hat{\Pi}_1^L + \gamma_2^2 \hat{\Pi}_2^H - c^i - c^a \right)}_{\text{2nd period, upgrade to high track}} + \\
& \underbrace{F(a_L^*|\hat{a}) \left(\alpha^L + \beta^{L-\infty} \frac{\int_{a_L^*}^{a_L^*} adF(a|\hat{a})}{F(a_L^*|\hat{a})} + \gamma_1^3 \hat{\Pi}_1^L + \gamma_2^3 \hat{\Pi}_2^L \right)}_{\text{3rd period, stay in low track}} + \\
& \underbrace{(1 - F(a_L^*|\hat{a})) \left(\alpha^H + \beta^H \frac{\int_{a_L^*}^{\infty} adF(a|\hat{a})}{1 - F(a_L^*|\hat{a})} + \gamma_1^3 \hat{\Pi}_1^L + \gamma_2^3 \hat{\Pi}_2^H - c^a \right)}_{\text{3rd period, upgrade to high track}}
\end{aligned}$$

Since the child's utility in a track school is increasing in her expected ability at a faster rate than her utility in a low track school, there exists an ability threshold \hat{a}_{July}^* such that, in period 1, all children whose expected ability is below this threshold attend a low track school while all children whose expected ability is above the threshold attend a high track school.

For a child who is born in June, the parents' decision problem is similar except that the child's utility in the first period in a high and low track school is replaced by $\alpha^H + \beta^H(\hat{a} - \Delta) + \gamma_1^1 \hat{\Pi}_1^H$ and $\alpha^L + \beta^L(\hat{a} - \Delta) + \gamma_1^1 \hat{\Pi}_1^L$, respectively. It then follows that the expected ability threshold at which children born in June are indifferent between attending a low and high track school in the first period, \hat{a}_{June}^* , exceeds the threshold at which children born in July are indifferent; that is, $\hat{a}_{\text{June}}^* > \hat{a}_{\text{July}}^*$. Consequently, children who are shifted from a low to a high track school because of a July (rather than a June) birth date are children in the expected ability range $[\hat{a}_{\text{July}}^*, \hat{a}_{\text{June}}^*]$, as given in Proposition 1.

EXPECTED VERSUS REALIZED PEER QUALITY: In equilibrium, parental expectations of peer ability in each school type and period must correspond to the realized average ability of children in each school type and period; that is, $\hat{\Pi}_t^j = \Pi_t^j$. These realized expectations are determined by the ability thresholds \hat{a}_{July}^* , \hat{a}_{June}^* , a_L^* , and a_H^* . Next, we illustrate the computation of Π_t^j for students who attend a low track low track school in period 2 (Π_2^L). These students fall into four groups: students born in June or July who attended a low

track school in period 1 (who have a measure of $\int_{-\infty}^{\hat{a}_{\text{June}}^*} F(a_L^*|\hat{a})dG(\hat{a})$ and $\int_{-\infty}^{\hat{a}_{\text{July}}^*} F(a_L^*|\hat{a})dG(\hat{a})$, respectively), and students born in July or June who attended a high track school in period 1 (who have a measure of $\int_{\hat{a}_{\text{June}}^*}^{\infty} F(a_H^*|\hat{a})dG(\hat{a})$ and $\int_{\hat{a}_{\text{July}}^*}^{\infty} F(a_H^*|\hat{a})dG(\hat{a})$, respectively). The average ability of the first group of students (i.e., students born in June who attend a low track school

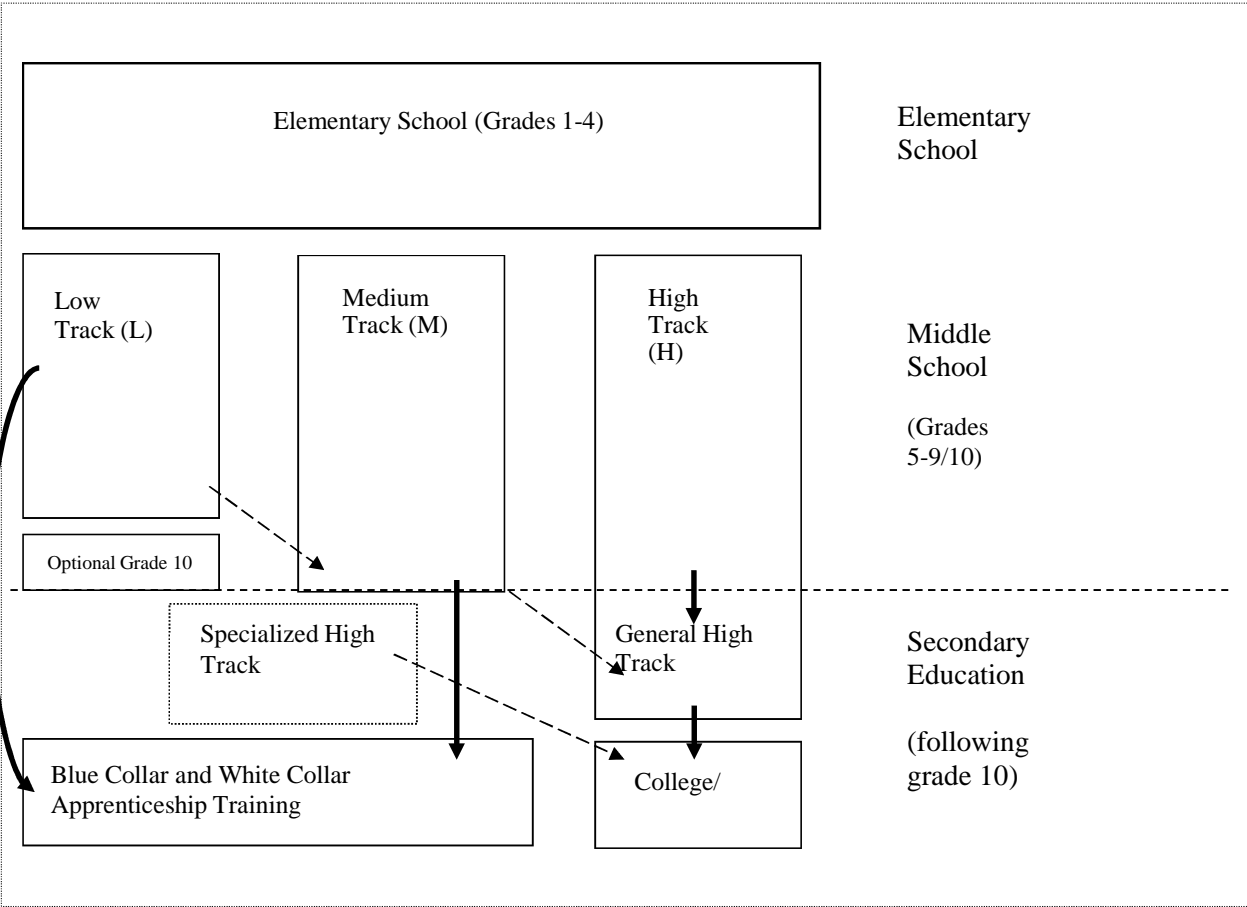
in period 1 and stay in a low track school in period 2) equals $\int_{-\infty}^{\hat{a}_{\text{June}}^*} \int_{-\infty}^{a_L^*} \frac{a dF(a|\hat{a})}{F(a_L^*|\hat{a})} dG(\hat{a}) / G(\hat{a}_{\text{June}}^*)$.

Hence, Π_2^L equals:

$$\begin{aligned}
\Pi_2^L = & \frac{\int_{-\infty}^{\hat{a}_{\text{June}}^*} F(a_L^*|\hat{a})dG(\hat{a}) + \frac{\int_{-\infty}^{\hat{a}_{\text{June}}^*} \int_{-\infty}^{a_L^*} \frac{adF(a|\hat{a})}{F(a_L^*|\hat{a})} dG(\hat{a})}{G(\hat{a}_{\text{June}}^*)} + \int_{\hat{a}_{\text{June}}^*}^{\infty} F(a_H^*|\hat{a})dG(\hat{a}) + \frac{\int_{\hat{a}_{\text{June}}^*}^{\infty} \int_{a_H^*}^{\infty} \frac{adF(a|\hat{a})}{F(a_H^*|\hat{a})} dG(\hat{a})}{1-G(\hat{a}_{\text{June}}^*)}}{\int_{-\infty}^{\hat{a}_{\text{June}}^*} F(a_L^*|\hat{a})dG(\hat{a}) + \int_{\hat{a}_{\text{June}}^*}^{\infty} F(a_H^*|\hat{a})dG(\hat{a}) + \int_{-\infty}^{\hat{a}_{\text{July}}^*} F(a_L^*|\hat{a})dG(\hat{a}) + \int_{\hat{a}_{\text{July}}^*}^{\infty} F(a_H^*|\hat{a})dG(\hat{a})} + \\
& \frac{\int_{-\infty}^{\hat{a}_{\text{July}}^*} F(a_L^*|\hat{a})dG(\hat{a}) + \frac{\int_{-\infty}^{\hat{a}_{\text{July}}^*} \int_{-\infty}^{a_L^*} \frac{adF(a|\hat{a})}{F(a_L^*|\hat{a})} dG(\hat{a})}{G(\hat{a}_{\text{June}}^*)} + \int_{\hat{a}_{\text{July}}^*}^{\infty} F(a_H^*|\hat{a})dG(\hat{a}) + \frac{\int_{\hat{a}_{\text{July}}^*}^{\infty} \int_{a_H^*}^{\infty} \frac{adF(a|\hat{a})}{F(a_H^*|\hat{a})} dG(\hat{a})}{1-G(\hat{a}_{\text{July}}^*)}}{\int_{-\infty}^{\hat{a}_{\text{June}}^*} F(a_L^*|\hat{a})dG(\hat{a}) + \int_{\hat{a}_{\text{June}}^*}^{\infty} F(a_H^*|\hat{a})dG(\hat{a}) + \int_{-\infty}^{\hat{a}_{\text{July}}^*} F(a_L^*|\hat{a})dG(\hat{a}) + \int_{\hat{a}_{\text{July}}^*}^{\infty} F(a_H^*|\hat{a})dG(\hat{a})}
\end{aligned}$$

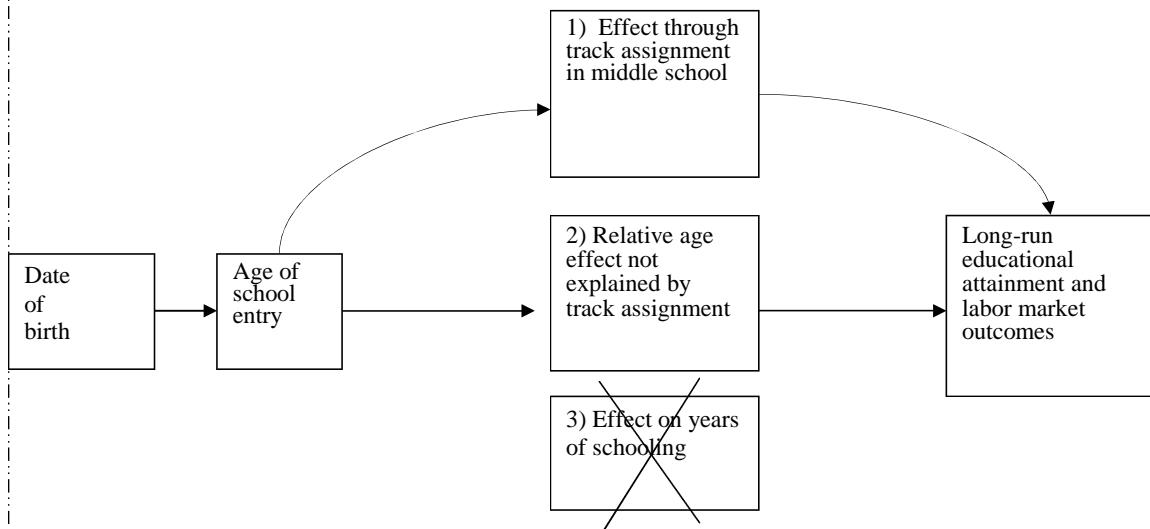
The average ability of the three other groups of students can be computed accordingly.

Figure 1: The German Education System



Note: The figure provides an overview of the German education system. Students who have attended a low track school (L) in middle school (grades 5-9/10) would typically start an apprenticeship in a blue collar occupation after 9th grade. Students who have attended a medium track school (M) in middle school would typically start an apprenticeship in a white collar occupation after 10th grade. Students who have attended a high track school (H) in middle school would typically continue at this school until grade 13 and then enter college or university. Graduation from a specialized high track school grants access to college or university, but possibly restricts the field of study.

Figure 2: The Effects of Age of School Entry on Long-Run Educational Attainment and Labor Market Outcomes



Note: The figure highlights the three channels through which age of school entry may affect adult outcomes: through track assignment in middle school, through a relative age effect that may not fully dissipate over time, and through its interaction with compulsory school attendance laws. Note that due a different institutional set-up, this last effect is absent in the German education system.

Table 1: Differences between School Types: Peer Exposure, Teacher Quality, and Teaching Technology

	Low	Medium	High
Panel A: Teaching Intensity and Teaching Curriculum			
a) Teaching Intensity (9th Grade)			
<i>total hours per week</i>	32	32	36
2nd and 3rd foreign language	0	0	6
Natural and computer science	3	6	8
Social science	3	6	4
Physical education	4	4	2
Vocational Subjects	6	0	0
b) Learning Goals (9th grade)			
German:	writing CVs, filling out forms, discussions and rules to resolve conflict.	discussions and rules to resolve conflict	detailed explanations, analysis and interpretation of various types of texts; creative writing
Mathematics:	equations with rational numbers; Phytagoras; prisma, pyramids, cones, cylinder; descriptive statistics.	real numbers and powers; linear equations with two unknowns; functions excluding exponential and broken polynomial functions; trigonometry, prisma, pyramids, cones, cylinder	functions, including exponential, trigonometric and broken polynomial functions; Phytagoras, trigonometry, prisma, pyramids, cones, cylinder; stochastics
<hr/>			
	Low	Medium	High
Panel B: Peer Exposure (9th grade)			
<i>Test Scores</i>			
Reading score	-0.900	-0.183	0.768
Mathematics score	-0.908	-0.217	0.840
<i>Parental Background</i>			
Mother's education (years)	10	12	14
Father's education (years)	10	12	14
% of households with less than 25 books	40	23	5
% of households below median income	76	65	39
Share Girls	41	49	54
Panel C: Teacher Quality			
Minimum Length of Study	4 years	4 years	5 years
Salary	46,872	46,872	49,530

Note: The table reports differences in teaching intensity and learning goals in grade 9, peer exposure at age 15 and teacher quality between school tracks. Reading and Mathematics test scores are normalized to have mean zero and standard deviation 1.

Data Sources: Teaching Intensity: Ministry of Education, State of Bavaria. Learning Goals: Ministries of Education of various West German states (Baden-Württemberg, Bavaria, Hesse, Lower Saxony, North Rhine-Westphalia, Rhineland-Palatinate). Peer Exposure: Programme for International Student Assessment (PISA), 2003 and 2006. Teacher Quality: various Ministries of Education of various West German states.

**Table 2: Date of Birth and Track Attendance at Age 14
(First Stage)**

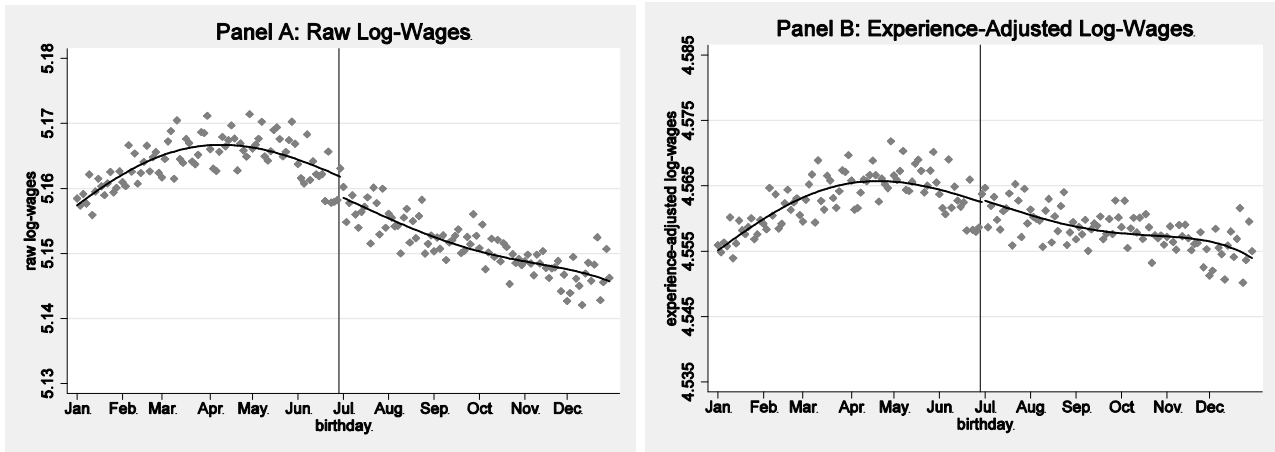
	(1) High versus Medium or Low	(2) High or Medium versus Low	(3) Sum
<i>Panel A: Birth Cohorts 1961-1976 (Microcensus)</i>			
May-Dec versus Jan-April	0.020 (0.005)**	0.019 (0.005)**	0.039 (0.009)**
N=37,808			
<i>Panel B: Measurement Error and Seasonal Effects (School Census, Birth Cohorts 1988-1994)</i>			
(i) July versus June (s.e.)	0.052 (0.002)**	0.042 (0.002)**	0.094 (0.004)**
N=170,832			
(ii) May-Dec versus Jan-April (s.e.)	0.033 (0.001)**	0.039 (0.001)**	0.072 (0.002)**
N=990,854			

Note: In Panel A, we report, for selected cohorts born between 1961 and 1976, the difference in the share of students who attend the high (versus the medium or low) track (column (1)) or the low (versus the medium or high) track (column (2)) at age 14 between students born later (May to December) and earlier (January to April) during the year. In column (3), the dependent variable is coded 2 if the students is in the high track, 1 if she is in the medium track and 0 if she is in the low track, corresponding to the sum of the coefficients in the first two columns. In Panel B, we report the impact of date of birth on track attendance at age 14 for the birth cohorts 1988 to 1994, using two different cut-offs. Robust standard errors in parentheses.

Coefficients with * are statistically significant at the 5 percent level, those with ** at the 1 percent level.

Source: Panel A: Microcensus, selected years 1976 to 1987, men and women. Panel B: School Census for Bavaria and Hesse, 2002-2009.

Figure 3: Date of Birth and Wages (Men only)



Note: The figures plot the relationship between date of birth and raw (Panel A) and experience-adjusted (Panel B) log-wages. In Panel B, the experience effect is eliminated by estimating returns to potential experience using OLS, imposing a functional form of a 4th order polynomial in potential experience, and subtracting these from raw log-wages. The figures are based on individuals aged 30 and over with a valid wage. Each dot refers to the average wage for two birthdays (e.g., 3rd and 4th of January). We also plot predicted raw and experience-adjusted log-wages obtained from a regression that controls, in addition to an indicator variable equal to 1 for individuals born after July 1st, for a polynomial of order 5 in the date of birth. The vertical lines indicate the school entry cut-off date.

Source: Social Security Data, men, 1975 to 2006.

Table 3: Track Attendance in Middle School and Wages

<u>Panel A: Reduced Form Estimates</u>					
		(1) Jun-Jul, none	(2) Jan-Dec, pol. 5	(3) Jan-Dec, pol. 6	(4) Apr-Sept, pol. 2
(i) Raw wages (age 30 and higher)	Coeff.	-0.005	-0.003	-0.003	-0.004
	(s.e.)	(0.001)**	(0.001)**	(0.001)**	(0.001)**
(ii) Wages net of experience (age 30 and higher)	Coeff.	-0.0009	0.0000	0.0002	-0.0009
	(s.e.)	(0.001)	(0.001)	(0.001)	(0.001)
	N	810,679	4,807,959	4,807,959	2,444,420
<u>Panel B: Two-Sample Two-Stage Least Squares Estimates</u>					
		(1) Jul-Jun, none	(2) Jan-Dec, pol. 5	(3) Jan-Dec, pol. 6	(4) Apr-Sept, pol.2
(i) Wages net of experience (age 30 and higher)	Coeff.	-0.024	0.000	0.006	-0.023
	(s.e.)	(0.025)	(0.033)	(0.032)	(0.029)

Note: In Panel A, we report various reduced-form estimates for the impact of date of birth on log-wages, for men aged 30 and older with a valid wage. Our first outcome variable is the raw log-wage. We then eliminate the experience effect, by estimating returns to potential experience using OLS and imposing a functional form of a 4th order polynomial in potential experience. In column (1), we report the difference in raw and experience-adjusted log-wages between individuals born in July and June; standard errors clustered at the individual level in parentheses. In columns (2) and (3), we display the coefficient on being born on or after July 1st from a regression that controls for a polynomial in the day of birth of order 5 and 6, respectively, and includes all individuals, born between January and December. In column (4), we restrict the sample to individuals born between April and September, and include a polynomial of order 2 in the day of birth. Standard errors are clustered at the day of birth.

In Panel B, we report the corresponding Two-Sample Two-Stage Least Squares estimates for the impact of attending a more advanced track in middle school on (experience-adjusted) log-wages, by dividing the reduced form estimates in Panel A by the first stage of 0.039 (Table 2, Panel A, column (3)). Standard errors are computed using the Delta Method, see footnote 15 for details.

Coefficients with * are statistically significant at the 5 percent level, those with ** at the 1 percent level.

Source: Reduced Form: Social Security Data, men, 1975-2006. First Stage: Microcensus, selected years 1976 to 1987.

Table 4: Date of Birth and Wages: Robustness Checks

	Birth Cohorts 1961-1976, age 30 and up		Birth Cohorts 1961-1964, age 40 and up	
	(1)	(2)	(3)	(4)
	Baseline OLS, pol. 4	Alt. functional form OLS, dummies	Raw	Exp.-adjusted OLS, pol. 4
July vs June				
Coeff.	-0.0009	-0.0009	0.0000	-0.0002
(s.e.)	(0.0008)	(0.0008)	(0.0018)	(0.0019)

Note: The table reports various robustness checks for the impact of birth month on log-wages. Columns (1) to (2) refer to birth cohorts 1961 to 1976 and individuals aged 30 and older. For comparison, we report our baseline estimate in column (1) where returns to potential experience are estimated using OLS and a functional form of a 4th order polynomial in potential experience is imposed. In column (2), we relax the functional form assumption and include a full set of dummy variables for potential experience instead. Columns (3) and (4) refer to birth cohorts 1961 to 1964 and individuals 40 and older, whose wages are flat with respect to potential experience. We first report the raw and then the experience-adjusted wage differential, where returns to potential experience are, as in our baseline specification, estimated using OLS and a functional form of a 4th order polynomial in potential experience is assumed. Standard errors clustered at the individual level in parentheses. Coefficients with * are statistically significant at the 5 percent level, those with ** at the 1 percent level. *Source:* Social Security Data, men, 1975-2006.

Table 5: Track Attendance in Middle School and Occupational Choice, Experience, and Unemployment

<i>Panel A: Reduced Form Estimates</i>				
	(1)	(2)	(3)	(4)
	Jun-Jul, none	Jan-Dec, pol. 5	Jan-Dec, pol. 6	Apr-Sept, pol. 2
(i) Share days worked				
Coeff.	0.001	0.000	0.000	0.001
(s.e.)	(0.000)	(0.001)	(0.001)	(0.002)
(ii) Share days in unemployment				
Coeff.	0.000	0.000	0.000	0.000
(s.e.)	(0.000)	(0.000)	(0.000)	(0.002)
(i) White collar occupation				
Coeff.	0.000	0.001	0.002	0.000
(s.e.)	(0.001)	(0.001)	(0.001)	(0.001)
N	1,573,136	9,317,405	9,317,405	4,739,278

<i>Panel B: Two-Sample Two-Stage Least Squares Estimates</i>				
	(1)	(2)	(3)	(4)
	Jul-Jun, none	Jan-Dec, pol. 5	Jan-Dec, pol. 6	Apr-Sept, pol. 2
(ii) Share days worked				
Coeff.	0.015	0.005	0.005	0.013
(s.e.)	(0.010)	(0.013)	(0.014)	(0.061)
(iii) Share days in unemployment				
Coeff.	-0.005	-0.004	-0.004	0.004
(s.e.)	(0.004)	(0.005)	(0.005)	(0.061)
(i) White collar occupation				
Coeff.	0.013	0.037	0.040	-0.001
(s.e.)	(0.021)	(0.029)	(0.028)	(0.025)

Note: In Panel A, we report various reduced-form estimates for the impact of date of birth on the share of days spent working and in unemployment since labor market entry and on whether the current occupation is a white collar occupation, for individuals aged 30 and over. In column (1), we report the difference in these outcomes between individuals born in July and June; standard errors clustered at the individual level in parentheses. In columns (2) and (3), we display the coefficient on being born on or after July 1st from a regression that controls for a polynomial in the day of birth of order 5 and 6, respectively, and includes all individuals, born between January and December. In column (4), we restrict the sample to individuals born between April and September, and include a polynomial in the day of birth of order 2. Standard errors are clustered at the day of birth.

In Panel B, we report the corresponding Two-Sample Two-Stage Least Squares estimates for the impact of attending a more advanced track in middle school on these labor market outcomes, by dividing the reduced form estimates in Panel A by the first stage of 0.039 (Table 2, Panel A, column (3)). Standard errors are computed using the Delta Method, see footnote 15 for details.

Coefficients with * are statistically significant at the 5 percent level, those with ** at the 1 percent level. Number of observations refer to the number of workers.

Source: Reduced Form: Social Security Data, 1975-2006. First Stage: Microcensus, selected years 1976 to 1987.

Table 6: Track Attendance in Middle School and Track Completion

	(1)	(2)
	High Track Attendance in Middle School	High Track Completion
Panel A: Movements between the Medium and High Track		
(i) Movements between the medium and <i>general</i> high track (School Census)		
July versus June	0.052	0.020
(s.e.)	(0.002)**	(0.004)**
(ii) Movements between the medium and <i>general or specialized</i> high track (School Census)		
July versus June	0.052	-0.001
(s.e.)	(0.002)**	(0.005)
(iii) Movements between the medium and <i>general or specialized</i> high track (1987 Census)		
June-Dec verus Jan-May	0.020	0.000
(s.e.)	(0.005)**	(0.001)
Panel B: Movements between the Low and Medium Track (1987 Census)		
	(1)	(2)
	Medium or High Track Attendance in Middle School	Medium or High Track Completion
June-Dec verus Jan-May	0.019	0.003
(s.e.)	(0.005)**	(0.001)**

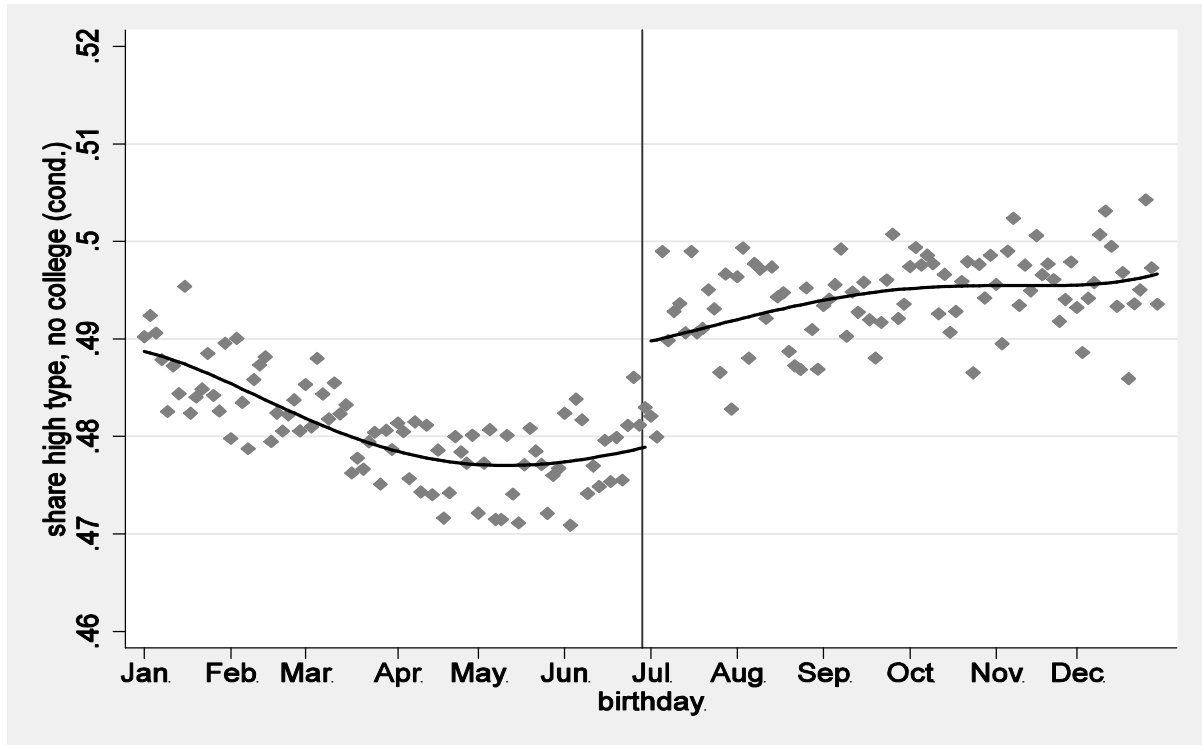
Note: In column (1) of Panel A, we report the difference in high track attendance at age 14 between students born in July and June for recent cohorts in rows (i) and (ii) (see Panel B, row (i) and column (1), of Table 2), and the difference in high track attendance at age 14 between students born earlier or later during the year for older cohorts in row (iii) (see Panel A, column (1), of Table 2). In column (2), we then display in rows (i) and (ii) the difference in graduation rates from a general high track school or in graduation rates from a general or specialized high track school by age 22 between students born in July or June, for the birth cohorts 1984 to 1987. See Appendix A.3 for calculation of standard errors. In row (iii), we compare in column (2), for the birth cohorts 1961 to 1963, the share of students who graduated from a general or specialized high track school, between students who were born earlier or later during the year.

In Panel B, we first report display in column (1) the difference in the share of students who attend at least the medium track at age 14 between students born earlier or later during the year (see also Table 2, Panel A, column (2)). We then report, for the birth cohorts 1961 to 1963, the difference in the share of students who completed at least the medium track between students born earlier (January versus May) and later (June versus December) during the year (column (2)). Robust standard errors in parentheses.

Coefficients with * are statistically significant at the 5 percent level, those with ** at the 1 percent level.

Source: Panel A, rows (i) and (ii): School Census for Bavaria and Hesse, 2002-2008. Panel A, row (iii) and Panel B: column (1): Microcensus, selected years 1976 to 1987, column (2): 1987 Census.

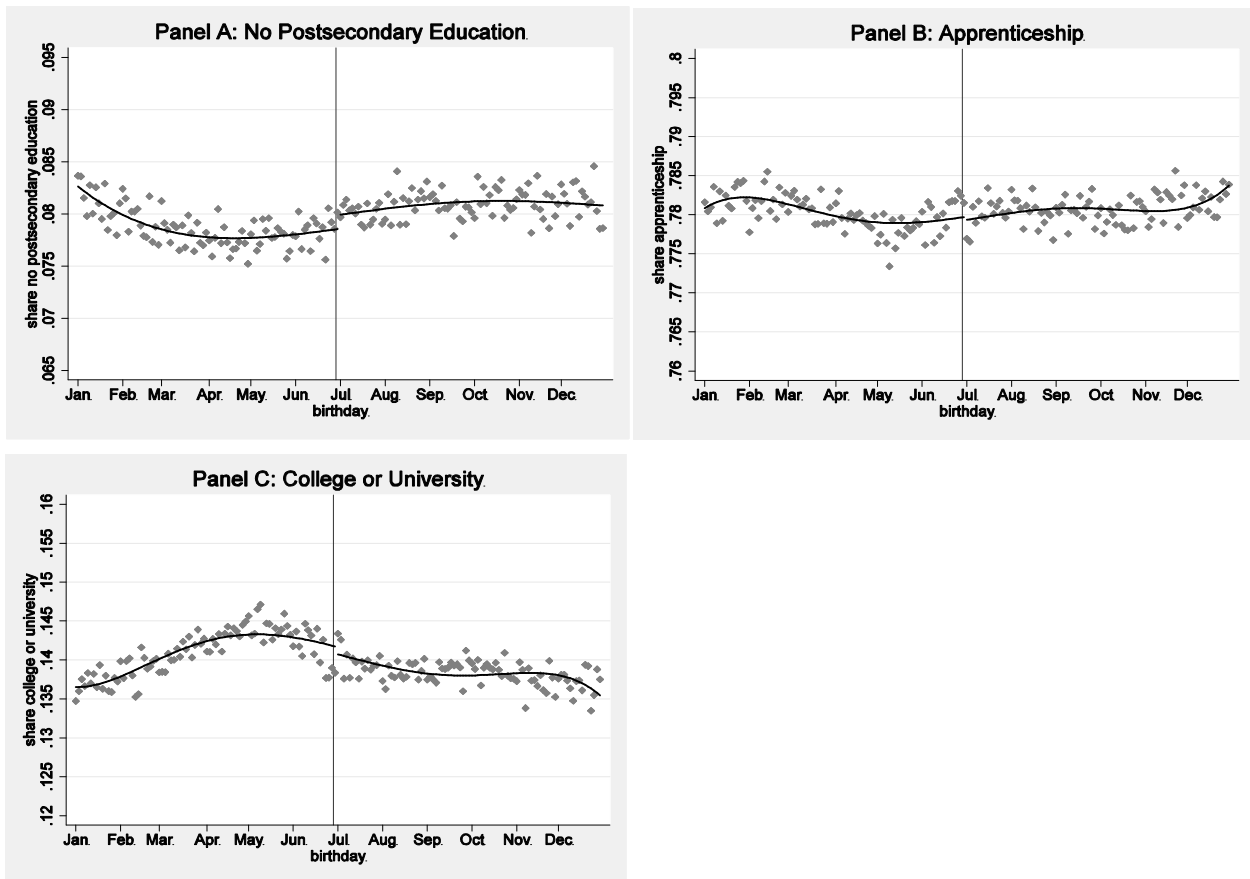
Figure 4: Date of Birth and Downgrading from the High Track



Note: The figure plots, for individuals who have graduated from a general or specialized high school, the relationship between exact date of birth and the share of individuals who "downgrade" by not graduating from college or university. Each dot refers to the average share for two birthdays (e.g., 3rd and 4th of January). We also plot predicted shares which we obtain from a regression that controls, in addition to an indicator variable equal to 1 for individuals born after July 1st, for a polynomial of order 5 in the day of birth. The vertical line indicates the school entry cut-off date.

Source: Social Security Data, 1975-2006.

Figure 5: Date of Birth and Completed Education



Note: The figures plot the relationship between exact date of birth and the share of individuals without postsecondary education (Panel A), the share of individuals who completed an apprenticeship (Panel B), and the share of individuals who graduated from college or university (Panel C). Each dot refers to the average share for two birthdays (e.g., 3rd and 4th of January). We also plot predicted shares which we obtain from a regression that controls, in addition to an indicator variable equal to 1 for individuals born after July 1st, for a polynomial of order 5 in the date of birth. The vertical lines indicate the school entry cut-off date.

Source: Social Security Data, 1975-2006.

Table 7: Track Attendance in Middle School and Completed Education (Reduced Form Estimates)

		(1)	(2)	(3)	(4)
		Jun-Jul, none	Jan-Dec, pol. 5	Jan-Dec, pol. 6	Apr-Sept, pol. 2
(i) No Post-secondary Education	Coeff.	0.0014	0.0012	0.0012	0.0016
	(s.e.)	(0.000)**	(0.001)*	(0.001)	(0.001)**
(ii) Apprenticeship	Coeff.	0.0004	0.0000	0.0000	0.0007
	(s.e.)	(0.001)	(0.001)	(0.001)	(0.001)
(iii) College or University	Coeff.	-0.0018	-0.0013	-0.0012	-0.0024
	(s.e.)	(0.000)**	(0.001)	(0.001)	(0.001)**
	N	1,961,320	11,609,855	11,609,855	5,905,126

Note: The table reports various reduced-form estimates for the impact of the exact (to the day) date of birth on the share of individuals without post-secondary education, the share of individuals who completed an apprenticeship or graduated from a general or specialized high school type, and the share of individuals who graduated from college or university. In column (1), we report the difference in the respective shares between individuals born in July and June; robust standard errors in parentheses. In columns (2) and (3), we display the coefficient on being born on or after July 1st from a regression that controls for a polynomial in the day of birth of order 5 and 6, respectively, and includes all individuals born between January and December. In column (4), we restrict the sample to individuals born between April and September, and include a polynomial in the day of birth of order 2. Standard errors are clustered at the day of birth.

Coefficients with * are statistically significant at the 5 percent level, those with ** at the 1 percent level.

Source: Social Security Data, 1975-2006.

Table A1: Birth Month and Family Background Characteristics

	Father	Mother
Age	-0.14	-0.098
(s.e.)	(0.13)	(0.11)
College/University	-0.011	-0.004
(s.e.)	(0.01)	(0.01)
At least Apprenticeship	-0.007	-0.004
(s.e.)	(0.01)	(0.01)
N	8,616	8,616

Note: The table reports the difference in age at birth (in years), college and university education, and at least apprenticeship education of fathers and mothers of children who were born in June or July, respectively. The analysis is based on the German Microcensus for the year 2005 (the only year in which the German Microcensus includes information on exact birth month), and the sample consists of all children still living with their parents who were born between 1991 and 2004 and thus were less than 14 years old at the time of the survey.

Source: Microcensus 2005.

Table A2: The Stability of the First Stage Across Grades

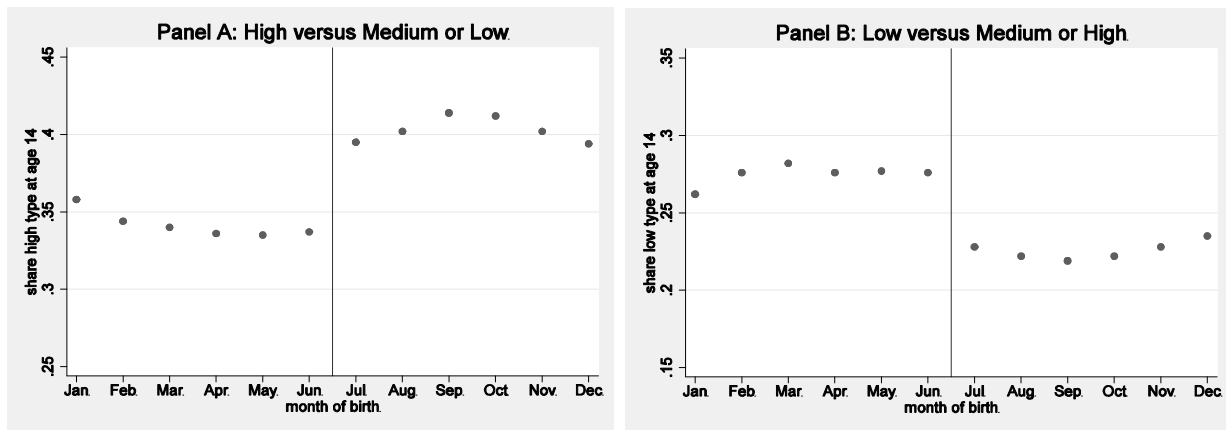
	(1) High versus Medium or Low	(2) High or Medium versus Low	(3) Sum
Grade 5	0.063	0.052	0.115
N=163,138	(0.002)**	(0.002)**	(0.004)**
Grade 6	0.063	0.052	0.115
N=158,781	(0.002)**	(0.002)**	(0.004)**
Grade 7	0.061	0.051	0.112
N=160,354	(0.002)**	(0.002)**	(0.004)**
Grade 8	0.057	0.048	0.105
N=162,491	(0.002)**	(0.002)**	(0.004)**
Grade 9	0.056	0.049	0.105
N=164,200	(0.002)**	(0.002)**	(0.004)**

Note: The table reports the difference in the share of students who attend a high versus a medium or low track (column (1)), and in the share of students who attend the low versus the medium or high track (column (2)) between students born in July and June, through grade 5 to grade 9. In column (3), the dependent variable is coded 2 if the student is in the high track, 1 if she is in the medium track and 0 if she is in the low track, corresponding to the sum of the coefficients in the first two columns. Robust standard errors in parentheses.

Coefficients with * are statistically significant at the 5 percent level, those with ** at the 1 percent level.

Source: School Census for Bavaria and Hesse, 2002-2009.

Figure A1: Track Attendance at Age 14 and Birth Month



Note: The figures plot the share of students attending a high (Panel A) and low (Panel B) track in middle school at age 14 against birth month. Results refer to birth cohorts 1988 to 1994.

Source: School Census for Bavaria and Hesse, 2002-2009.

Table A3: Track Attendance and Productivity

<u>Period 1, July (Schooling Period)</u>	
low in 1	$\alpha^L + \beta^L \hat{a} + \gamma_1^1 \hat{\Pi}_1^L$
high in 1	$\alpha^H + \beta^H \hat{a} + \gamma_1^1 \hat{\Pi}_1^H$
<u>Period 1, June (Schooling Period)</u>	
low in 1	$\alpha^L + \beta^L (\hat{a} - \Delta) + \gamma_1^1 \hat{\Pi}_1^L$
high in 1	$\alpha^H + \beta^H (\hat{a} - \Delta) + \gamma_1^1 \hat{\Pi}_1^H$
<u>Period 2, July and June (Schooling Period)</u>	
low in 2, low in 1	$\alpha^L + \beta^L a + \gamma_2^2 \hat{\Pi}_2^L + \gamma_1^2 \hat{\Pi}_1^L$
low in 2, high in 1	$\alpha^L + \beta^L a + \gamma_2^2 \hat{\Pi}_2^L + \gamma_1^2 \hat{\Pi}_1^H$
high in 2, low in 1	$\alpha^H + \beta^H a + \gamma_2^2 \hat{\Pi}_2^H + \gamma_1^2 \hat{\Pi}_1^L - c^a$
high in 2, high in 1	$\alpha^H + \beta^H a + \gamma_2^2 \hat{\Pi}_2^H + \gamma_1^2 \hat{\Pi}_1^H$
<u>Period 3, July and June (Working period)</u>	
low in 2, low in 1	$\alpha^L + \beta^L a + \gamma_2^3 \hat{\Pi}_2^L + \gamma_1^3 \hat{\Pi}_1^L$
low in 2, high in 1	$\alpha^L + \beta^L a + \gamma_2^3 \hat{\Pi}_2^L + \gamma_1^3 \hat{\Pi}_1^H$
high in 2, low in 1	$\alpha^H + \beta^H a + \gamma_2^3 \hat{\Pi}_2^H + \gamma_1^3 \hat{\Pi}_1^L - c^a$
high in 2, high in 1	$\alpha^H + \beta^H a + \gamma_2^3 \hat{\Pi}_2^H + \gamma_1^3 \hat{\Pi}_1^H$

Note: The table reports the student's productivity in each period, depending on the type of school the student attended in period 1 and 2.

Figure A2: Track Choice in Period 2

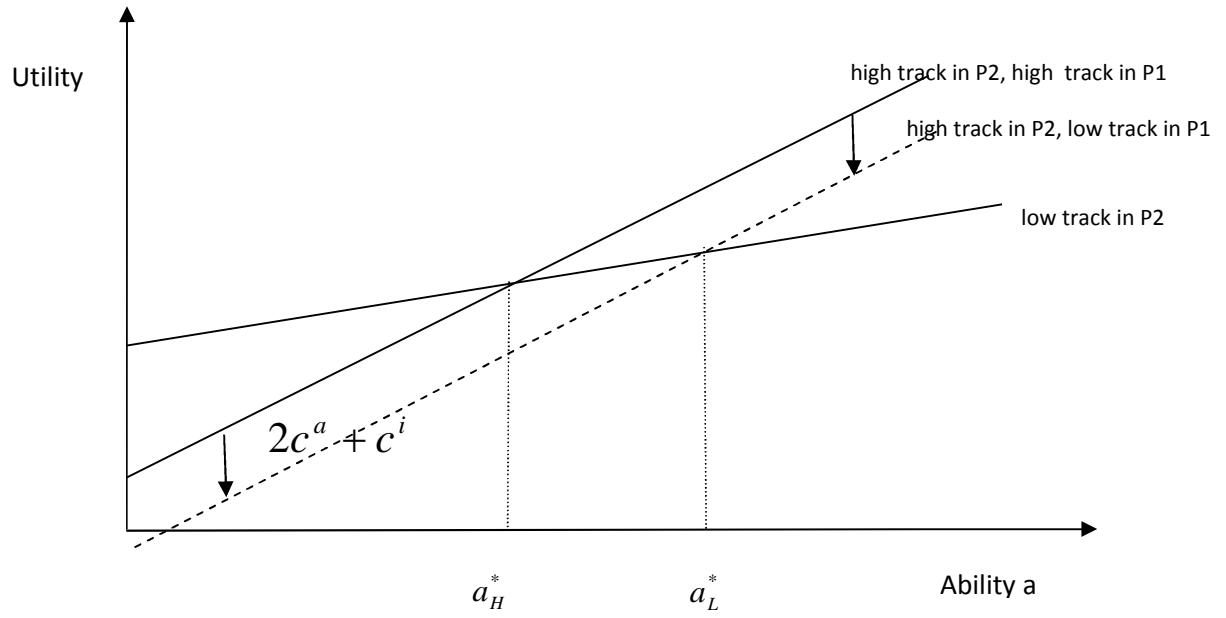


Figure A3: Track Choice in Period 1

