

RESEARCH ARTICLE

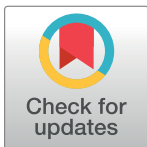
The long-run effects of secondary school track assignment

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Abstract

This study analyzes the long-run effects of secondary school track assignment for students at the achievement margin. Theoretically, track assignment maximizes individual outcomes when thresholds between tracks are set at the level of the indifferent student, and any other thresholds would imply that students at or around the margin are better off by switching tracks. We exploit non-linearities in the probability of track assignment across achievement to empirically identify the effect of track assignment on educational attainment and wages of students in the Netherlands, who can be assigned to four different tracks. We find that attending higher tracks leads to increases in years of schooling by around 1.5 years for students at the lowest and the highest choice margin, and wage gains of around 15% and 5%, respectively. For the margin between the two middle tracks, attending the higher of the two tracks has no effect on educational attainment and decreases wages by around 12%. The negative returns for the medium margin and the relatively low returns for the higher margin (compared to the required educational investments) are partly mediated by motivation and study choice.

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Data Availability Statement: This study makes use of administrative register data. The registered data on, e.g., wages contain sensitive information. Statistical analysis can only be done by logging onto a remote server of Statistics Netherlands, output of analyses is only cleared after an external check for confidentiality breaches, and all researchers need to pre-register and sign the relevant confidentiality forms before given access to the server. As such, the data are not allowed to be stored anywhere else than on the remote servers of Statistics Netherlands. See <https://www.cbs.nl/en-gb/our-services/customised-services-microdata/microdata-conducting-your-own-research> for more information. All data used for

Introduction

The grouping of students according to educational achievement is common across educational systems worldwide. The motivation for such practices is that different students may require different environments and different levels of instruction to optimally develop their skills. Anglo-Saxon countries generally address this by selecting students into ability groups for different school subjects, while many Continental European countries sort secondary school students into different tracks that each have their own distinct curriculum. The use of tracking is continuously debated in both the public and the academic domain. Early empirical studies in economics have typically focused on the efficiency and equity implications of tracking, or on the effect of the exact age at which tracking takes place; see, e.g., [1–6]. Less attention has been paid to the allocation process that sorts students into different tracks. Tracking involves, either explicitly or implicitly, the use of ability thresholds in order to sort students. Students above a particular threshold are deemed fit to attend the higher track, while those below are projected

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to be better off in a lower track. A crucial question is whether the exact location of such a threshold in the ability distribution is optimal for students that fall to either side of that threshold.

The aim of this paper is to estimate the long-run effect of track assignment for students at the achievement margin. The empirical analysis of this study is focused on the Dutch educational system, where track assignment is partly determined by scores on an achievement test taken at the end of primary school (grade 6). This test contains certain threshold scores, which indicate the required ability level for a specific track. In reality, the adherence to these threshold scores by schools is rather lenient. This precludes the use of a classical regression discontinuity design (RDD). Still, the threshold scores induce non-linearities in the probability of treatment across achievement that are not proportional to differences in the ability and potential of these students. By using the probability of track assignment given the obtained test score as an instrument for actual track assignment, this can be exploited to identify the effects of track assignment on future outcomes. We match survey data on Dutch secondary education students with administrative data on educational attainment and job market information in later life. Job market information is available up to an age of 42 years old. The Dutch educational system contains four different tracks in the period under analysis, which implies that there are three choice margins for which the effect of track assignment can be estimated. We find that attending the higher track increases educational attainment by around 1.5 years for the lowest and the highest choice margin. The subsequent labor market returns from attending the higher track are around 15% in the former and around 3–7% in the latter case. In contrast, we find that attending the higher track at the medium margin has no effect on years of schooling, and lowers wages by around 12% in the long run. These effects are partly mediated by study choice and motivation.

The choice of which track to attend typically involves schools, students and parents. Parental aspirations generally lean towards higher tracks that involve better peer quality and provide more direct paths towards high levels of post-secondary education [7]. Social status considerations and overconfidence could lead students to attend tracks that are too demanding and thereby hinder learning; see, e.g., [8] for evidence on education as a positional good and [9] for evidence on overconfidence in self-assessment. [10] find that a shift from parental to teacher influence for track assignment in Germany has led to a reduction in grade retention in secondary school, providing suggestive evidence that parents are indeed prone to push their children into too demanding tracks. Conformity to the educational level of the parents could make especially those with highly educated parents prone to attend too high tracks (and those with low educated parents to attend too low tracks). Schools face other considerations when setting thresholds for track attendance. Lower entrance requirements attract more students, while higher entrance requirements improve average peer quality within each track, and decrease the risk of costly grade retention. Hence, there are several reasons why the achievement threshold is not set at the indifferent student.

Research on tracking has traditionally focused on estimating effects for school achievement and educational attainment. To assess the effectiveness of tracking or track assignment, it is especially important to look at how tracking affects labor market outcomes, as that is ultimately what students are prepared for in these tracks. In fact, an increase in the number of completed years of schooling represents a cost from an economic perspective, and can only be beneficial when such an investment produces positive returns in meaningful later-life outcomes. While average wage returns to extra schooling are high, they are also strongly heterogeneous [11]. Increasing the size of the higher track can lead to increases in educational attainment simply because more students are eligible for higher levels of post-secondary

education. Additionally, shifting from an academic to a vocational focus might lead to decreases in academic achievement, but could be to the benefit of other skills. Recent studies that estimate long-run effects underline this. [12] show that a tracking reform in Romania led to an increase in the number of students that completed an academic track, but not to increases in labor market outcomes. Similarly, [13] analyzes a policy change in Sweden that gave the vocational track a more academic curriculum and identifies an increase in educational attainment in secondary school, but no effect on earnings. Additionally, [14] show that the life-cycle dynamics of students following vocational tracks and students following academic tracks are different, underlining the importance of measuring wage effects at multiple ages across the life cycle.

These studies analyze policy changes that changed the content of tracks. In contrast, we analyze assignment of students to tracks for a given set and content of tracks. As such, this paper relates most closely to a recent study by [15], who estimate the long-run effect of track attendance by exploiting the fact that relatively younger students are less likely to attend higher tracks because of the month of birth effect. Our study estimates a similar effect, but at a different margin. [15] identify a local average treatment effect (LATE) for those that would attend a higher track if they would be born earlier in the year. We elicit a different local effect, namely for those who would attend a higher track if their achievement would have been marginally higher. This particular LATE reflects a local effect that is critical for decision-making with respect to tracking. It answers whether ability thresholds are indeed set at the indifferent student or whether students around the margin would be better off by switching to another track. Additionally, our study adds to the literature by estimating the effects of track attendance at multiple margins, and for multiple cohorts in time.

The organization of this paper is as follows. Section 2 specifies the theoretical framework of this study. Section 3 gives an overview of relevant characteristics of the Dutch educational system. Section 4 discusses the data and methodology. Main results are discussed in Section 5, while robustness analyses are presented in Section 6. Section 7 concludes.

Theory

Countries differ in how they assign students to tracks, but track assignment generally relies strongly on measures of student achievement. Additionally, in countries in which tracks have strongly differentiated curricula, student preferences are a leading determinant. In our empirical setting, track assignment is based on a measure of overall ability, which is why the theoretical framework is constructed from this perspective as well.

We define an overall ability indicator θ_i , on which track allocation decisions are based. Future outcomes (Y_i) for students (e.g. future wages) depend on θ_i and the track T the student attends. We specify a simple linear relation:

$$Y_i = \alpha^T + \beta^T \theta_i \quad (1)$$

In the remainder of this section we assume, without loss of generality, that there are three tracks: low (L), medium (M) and high (H). Each track has its own curriculum (including not only the set of courses, but also level and pace of instruction). The level of these curricula are geared towards the average ability of students in that track. As such, lower tracks produce more favorable outcomes for low-ability students and higher tracks for high-ability students. In our linear framework, this implies that $\alpha^L > \alpha^M > \alpha^H$ and $\beta^L < \beta^M < \beta^H$. This situation is depicted on the left side of Fig 1. In the figure, it is assumed that allocation of students to tracks is efficient: no student can switch and make themselves better off. In other words, outcomes

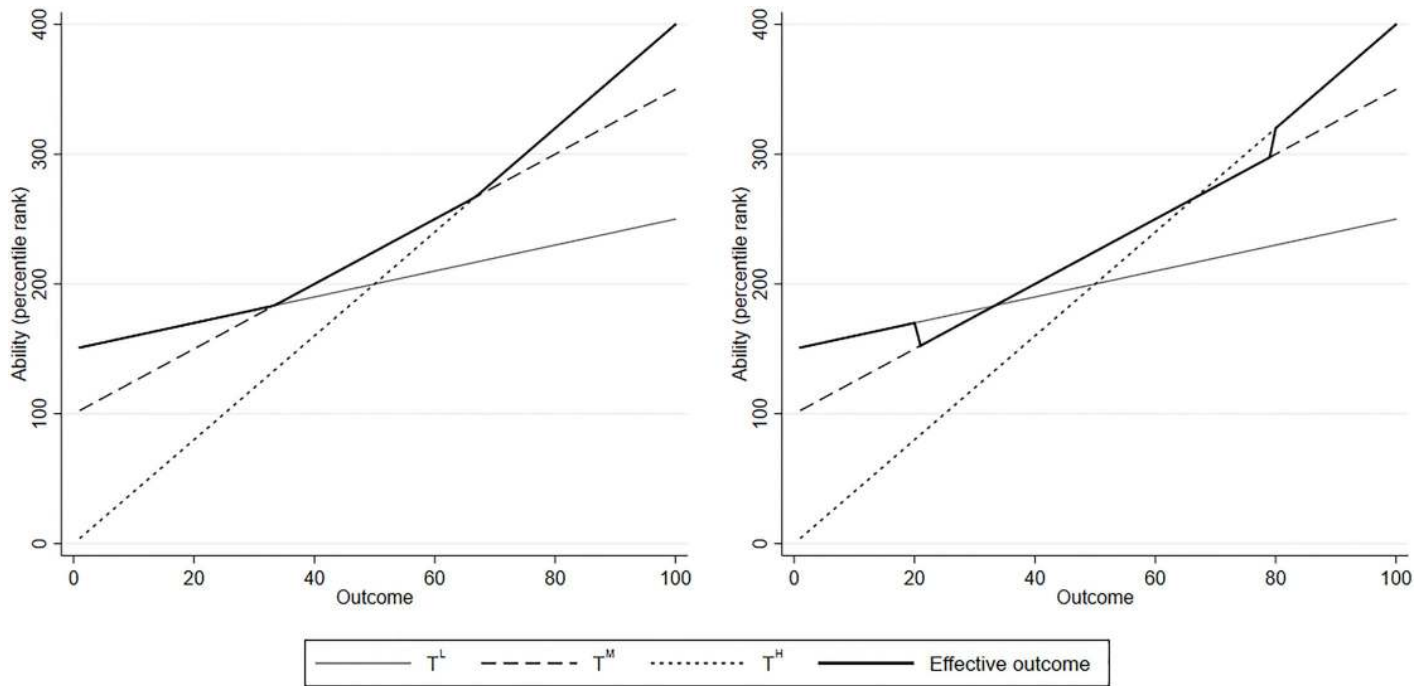


Fig 1. Allocation of students to tracks: Theoretical framework. The figure shows a theoretical depiction of the relation between outcome Y and attendance of low (L), medium (M) and high (H) tracks, across the ability distribution. On the left side, track assignment is efficient and the effective outcome line traces out the maximum outcomes. On the right side, the lower threshold is more strict and the higher threshold is more lenient, leading to discontinuities in outcomes.

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equal:

$$Y_i = \max(\alpha^L + \beta^L \theta_i, \alpha^M + \beta^M \theta_i, \alpha^H + \beta^H \theta_i) \tag{2}$$

In this scenario, the threshold ability levels lie before the first student in the distribution for which the higher track is optimal and after the last student in the distribution for which the lower track is optimal. When thresholds are located at different points, some students can be better off by switching. The effective outcome line will not follow the highest points and discontinuities in the outcome variable across the distribution will appear. The right side of Fig 1 describes the cases where the lower threshold is more lenient and the higher threshold is more strict. In the former case, the lowest-ranked students in the medium track would be better off in the low track and hence there is a downward jump between the highest low-track students and the lowest medium-track students. In the latter case, the top-ranked medium-track students would be better off in the higher track and there is an upward jump in the effective outcome line.

In reality, θ_i is not directly observed and typically proxied with (noisy) measures of school achievement. This also applies to the empirical setting of this paper. Using a noisy ability signal for student sorting automatically implies that allocation is not fully efficient and some students would be better off in another track than where they were assigned to. Given that noisy ability signal, outcomes are still maximized when the threshold achievement level is set at the indifferent student (assuming noise is symmetrically distributed). In that case, average outcome lines would still be equal to the situation in Fig 1, and the average effective outcome line is smooth across the distribution. Put differently, efficiency gains can be made by using less noisy

achievement measures or by putting thresholds at a more optimal position. Our focus in this paper is on the latter.

We do not consider peer effects explicitly in this model, which are incorporated within α^T and β^T . If one would assume a constant positive effect of better peers, α^L would decrease (i.e. Track L is shifted downward) and α^H would increase (i.e. Track H is shifted upward). Hence, peer effects reduce the part of the distribution for which the lower track is more optimal and increase the part of the distribution for which the higher track is more optimal. Explicitly modeling peer effects in the framework, however, provides no added value in the context of this study.

Additionally, we explicitly take an individual point of view. “Efficient” assignment as in [Fig 1](#) means that individuals cannot switch and be better off. The optimal allocation of students to tracks from a social welfare perspective represents a separate policy question. Changing effective thresholds also changes peer quality, pace of instruction, etc. This would lead to changes in the parameters α^T and β^T . If one assumes that all students benefit from higher average peer ability, then thresholds that maximize individual outcomes given the assignment of the rest of the distribution (as in [Fig 1](#)) are too lenient from a social welfare perspective, *ceteris paribus* (more so if we also consider general equilibrium effects in the labor market). Stricter thresholds would be more optimal since they increase peer quality on both sides of the threshold. Nonetheless, the framework as depicted is valuable as it identifies gains and losses for individuals who are at the choice margin for two specific tracks, which represents crucial information for students and their parents who face this choice. When we refer to “optimal” track allocation in the remainder of this paper, this pertains to this individual perspective and not to a social optimum.

As argued in the introduction, the impact of track assignment can potentially differ between different outcome measures. In the context of the theoretical framework, this means that α^T and β^T are dependent on the defined outcome variable. For example, positive impacts on educational attainment could arise even in the absence of better student learning, because higher tracks make students eligible for higher levels of post-secondary education. Although literature clearly shows that the average return to an extra year of schooling is positive and substantial, this return can be different for students who are induced to prolong their educational career because of more lenient requirements. This underlines the value of measuring also the labor market effects of such treatments.

Dutch educational system

The Dutch educational system is characterized by relatively early tracking and a high number of tracks. [Fig 2](#) provides a schematic overview from primary to tertiary education. Primary education lasts six years (preceded by two years of kindergarten). The focus of this study is on tracking in secondary education, and on how this influences post-secondary trajectories.

Secondary education

After finishing primary school, students in the Netherlands are selected into four main tracks: *lbo* (vocational), *mavo* (lower general), *havo* (higher general) and *vwo* (pre-university). Students can be relegated to lower tracks after track assignment, in case of low achievement, but moving to higher tracks is generally only possible when the current track has been completed. The *lbo* and *mavo* tracks have been merged in 1999 into the joint *vmbo* track. Within *vmbo*, there still exists a practical and a theoretical subtrack. For the vast majority of students in our sample, the system with four tracks applies. For the remainder of this paper, we refer to the available tracks as T1 for *lbo*, T2 for *mavo*, T3 for *havo* and T4 for *vwo*.

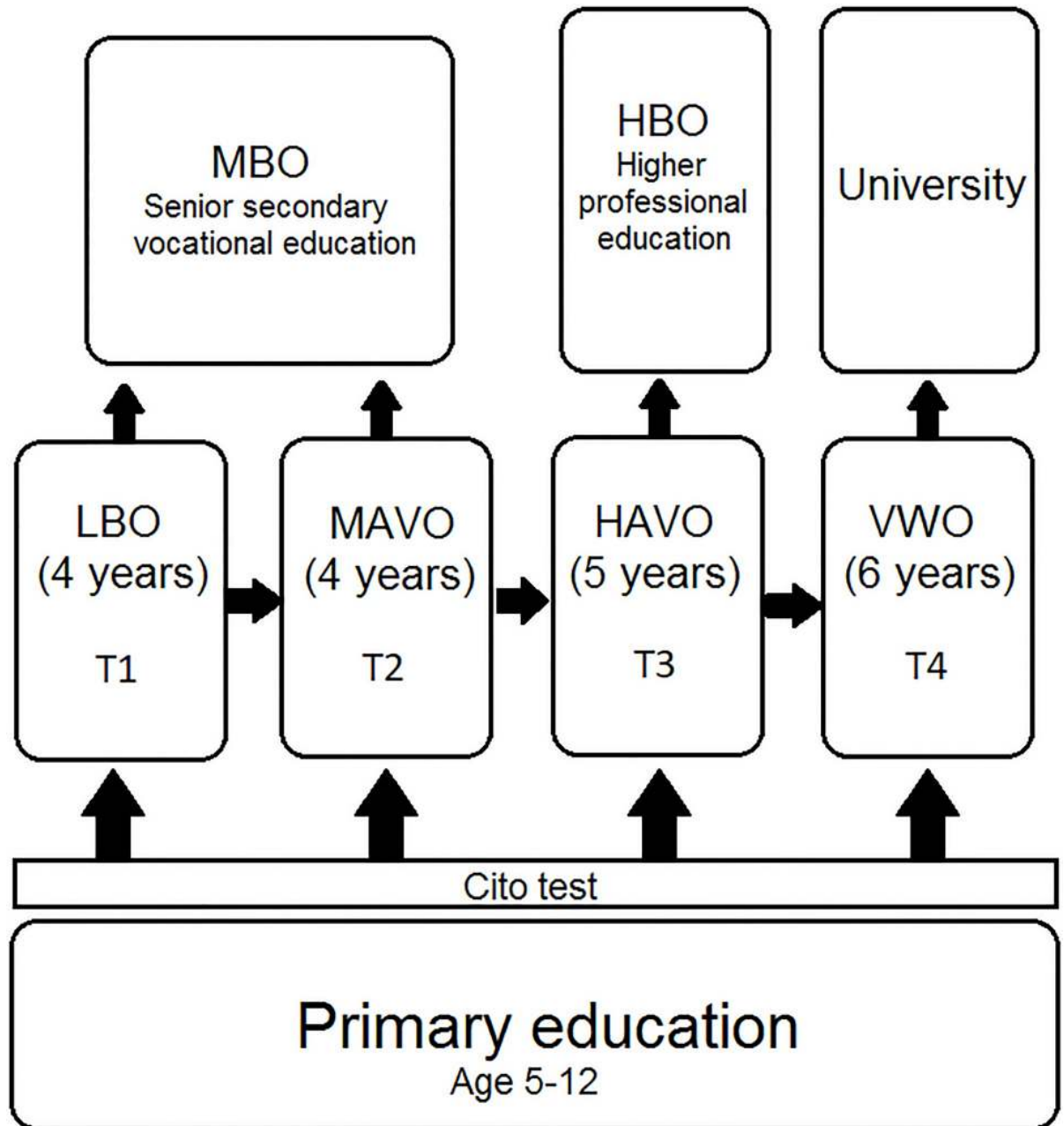


Fig 2. Dutch educational system. Source: Center of International Education Benchmarking.

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A leading determinant of track assignment is the 6th grade exit test that primary schools are obliged to administer. While several alternative tests exist, a large majority of 85% of Dutch primary schools administer the standardized “Cito test” [16]. The obtained Cito score is connected to a teacher recommendation for any of the four tracks, which is given by the 6th grade teacher and sent to the prospective secondary school. Recommendations are mixed when the teacher believes that the student’s ability is at the margin of the required level for a certain track. Secondary schools are obliged by law to consider at least one of the two sorting mechanisms (test score and/or teacher recommendation) when admitting or sorting students.

However, they are free to decide their exact assignment rules, and to deviate from the score thresholds that the manufacturers of the Cito test report.

There is an additional opportunity for students to be selected into a track that does not correspond to their Cito score, because final selection can be postponed until the second or third year of secondary education. This occurs through the existence of temporary comprehensive grades (so-called 'bridge-grades'), where students of two or more tracks are still kept together. This is most common for Track 3 and Track 4 students, while students in the lower two tracks are generally selected early. In our sample, roughly 90% of T3 and T4 students is in a comprehensive grade for at least one year, and 35% for 2 years. About 75% of students in Tracks 1 and 2 is in a specific track in the first year of secondary education already. When track assignment is postponed to later years, it is also based on student achievement, most commonly through school-specific requirements with respect to grade point averages.

Hence, while achievement is the key driver of track sorting in the Netherlands, these institutional features imply that assignment is far from deterministic and that there is considerable leeway in getting into tracks that do not match up with eligibility status of students if we would follow only the achievement test. We elaborate on the patterns of student sorting by achievement and their implications for the empirical approach in the next Section.

Post-secondary education

The Dutch educational system has three levels of post-secondary education. The lowest level is *mbo*, which has a vocational orientation. Higher education can be divided into two categories. *Hbo* provides higher professional education (also known as vocational university), while *wo* consists of university education. Students with a high school diploma from the T3 track or higher can enter *hbo*, while *wo* is only available for students who complete T4. Completing the highest level of *mbo* makes one eligible for entering *hbo* as well, while completing the first year of *hbo* gives direct access to university education. Hence, it is still possible for students from lower tracks to complete higher education, although the route is less direct and involves additional time. Around 15% of students from the T2 track in our data sample still complete higher education. For the most recent cohorts, this even equals 22% (see [S1 Fig](#)).

Materials and methods

Data

For the empirical analysis, we link several data sources. Secondary school data are collected from the Secondary Education Cohort Studies. These are large representative longitudinal surveys of Dutch pupils whose educational career was followed from the first year of secondary education (around age 12) until they leave full-time education. They are carried out by Statistics Netherlands and the Groningen Institute for Educational Research; see [\[17, 18\]](#). These data include measures of student achievement, student background, and the attended level of education for each year. We use cohort studies starting in 1977, 1983, 1989, 1993 and 1999.

All students that participated in the Secondary Education Cohort Studies are matched to administrative information from the System of Social Statistical Datasets (SSB) on educational attainment, wages and labor market status. The main outcome measure for educational attainment is years of schooling, which is coded towards the highest obtained degree, and is available until the year 2008. The labor market data contain the monthly earnings that are reported to the tax authorities, in every year from 2001 to 2007. These earnings are registered for the month of September in that particular year. The earnings for this month are typically seen as most representative, as they are generally not affected by end-of-year bonuses or vacation pay.

For the empirical analysis, we recode this variable. As we are mainly interested in wage effects and not in labor supply effects, we correct for the full-time equivalent (FTE) of the job (number of hours worked per week divided by the number of hours for a full-time worker with this particular job). To limit the influence of outliers, we topcode (corrected) wages at a monthly wage of 20,000 euro (0.4% of the sample), and exclude those with an FTE below 0.25 (2% of the sample) and with corrected wages below 1300 euro (3% of the sample). The latter approximates the minimum wage in the Netherlands in this period, and values below this threshold suggest that the individual was not employed for the full month. Our main outcome variable averages this recoded indicator across the seven years for which data are available. We refer to this indicator as the ‘mean wage’. The Results Section discusses the difference between the main estimates and those based on raw earnings.

The matched cohorts comprise around 25,000 observation for the 1977 cohort and around 15,000 observations for later cohorts. Labor market information is available from those who just entered the labor market until those who are 42 years old (the 1977 sample in 2007). The 1999 dataset includes individuals who are generally too young to have entered the labor market in any of the years for which we have data, hence long-run outcomes are not estimated for this cohort. Labor market effects for the 1993 cohort are not estimated for methodological reasons (see the Methodology subsection). Around 10% of students in the 1993 cohort is still in education by 2008. Robustness checks show that the results are not sensitive to this data limitation: we obtain virtually identical results when we assume that all these students finish the study they are currently attending. The share of students still in education is negligible for all other cohorts.

The 1989, 1993 and 1999 cohorts also contain data on 9th grade achievement. The test scores in the 1989 and 1993 cohorts have a high number of missing observations. We find that the probability of taking the test is positively related to previous achievement of the student (i.e. the better students in class are more likely to take the test). This can severely confound the estimates of our analysis, since the empirical approach effectively compares students at the margin. For these two cohorts, we use two imputation approaches that assess sensitivity to the missing value issue. For the 1999 cohort, the number of missing observations is negligible. Grade 9 test scores are available for language, mathematics and general problem-solving.

Unfortunately, the specific results of the Cito test that largely determines track assignment is only available for the 1999 cohort. For the other cohorts, a very similar test is available which is taken at the start of secondary education. This test is based on the same pool of questions and therefore serves as a proxy for the actual Cito test. This test is labeled as the *Entrance Test*, since it is taken just after entering secondary education. Entrance Tests are exactly identical for the 1989, 1993 and 1999 cohorts and contain 20 questions each in math, language and information processing. The test for the 1983 cohort contains the same division, but with different questions, while the 1977 Entrance Test contains 45 questions in language and 25 questions in math. Additionally, students from the 1983 cohort already take the test at the end of grade 6.

Summary statistics are provided in [Table 1](#). The share of students attending T1 slightly increases over the cohorts, mainly at the expense of T2. As in other developed countries, we observe a steady increase in obtained years of schooling over time. Parental education levels are increasing as well across cohorts, reflecting that this upward trend was already present in earlier cohorts. There is some variation in the mean levels of other background variables, but no clear trend nor marked differences in the background of students across these cohorts.

Table 1. Summary statistics.

	1977		1983		1989		1993	
	Mean	Sth. dev.	Mean	Sth. dev.	Mean	Sth. dev.	Mean	Sth. dev.
Track 1	0.256	0.436	0.323	0.468	0.333	0.471	0.359	0.480
Track 2	0.422	0.494	0.367	0.482	0.381	0.486	0.323	0.468
Track 3	0.140	0.347	0.142	0.349	0.135	0.341	0.152	0.359
Track 4	0.182	0.386	0.168	0.374	0.151	0.358	0.165	0.371
Entrance Test	42.17	12.11	33.32	10.17	34.23	11.33	35.20	11.42
Years of schooling	12.27	3.29	12.64	3.54	13.05	3.83	12.99	3.84
Wage 2007	3765.85	2163.48	3452.53	1709.74	2878.54	1117.40	2460.09	874.17
Wage 2006	3669.68	2109.74	3300.06	1628.58	2705.51	1066.36	2271.88	834.21
Wage 2005	3290.86	1817.53	2905.21	1264.19	2428.57	836.90	2045.64	752.97
Wage 2004	3186.34	1680.56	2793.78	1151.76	2322.09	753.22	1917.74	688.77
Wage 2003	3103.36	1576.27	2696.45	1076.33	2213.90	704.18	1763.19	652.08
Wage 2002	2977.85	1488.49	2568.05	1026.50	2047.57	660.85	1544.12	628.42
Wage 2001	2832.72	1412.19	2418.05	927.00	1872.69	647.18	1312.55	604.68
Female	0.503	0.500	0.504	0.500	0.481	0.500	0.485	0.500
Big 4 cities	0.091	0.288	0.055	0.228	0.045	0.207	0.060	0.238
Non-Dutch	0.079	0.265	0.086	0.275	0.109	0.311	0.090	0.286
High parental educ.	0.164	0.370	0.147	0.354	0.195	0.396	0.220	0.414
Low parental educ.	0.211	0.408	0.329	0.470	0.153	0.360	0.086	0.281
Low social class	0.120	0.325	0.111	0.314	0.087	0.282	0.085	0.279
High social class	0.115	0.318	0.107	0.310	0.145	0.352	0.136	0.343
N	25,065		14,322		17,450		18,572	

The table shows means and standard deviations of the main variables, separately for each cohort. Wages are in euro's, per month, pre-tax, corrected for full-time equivalent of the job and topcoded at 20,000. The variables Track 1 to Track 4 report the fraction of students that are assigned to that track. The Entrance Test includes 70 questions in 1977 and 60 questions in all other cohorts. Years of schooling is coded to the highest degree obtained. 'Big 4 cities' indicates whether the child lives in one of the four big cities in the Netherlands.

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Methodology

Non-linearity design. The aim of this study is to estimate the effect of track assignment on educational attainment and wages. As specified in the theoretical framework, one can find the effect of track assignment by identifying discontinuities in outcome variables around the achievement margin between two specific tracks. As such, the setup would be well-suited for a regression discontinuity design (RDD), which exploits discontinuities in treatment at a specific threshold value of a forcing variable. This approach is, however, empirically not feasible here. For one, we only observe the true forcing variable in one of the cohorts (1999). More importantly, the variability in the thresholds that schools adhere to and the use of postponed tracking lead to a very high fuzziness of track allocation around the threshold. Figs 3–5 show the relation between treatment probability and the Entrance test, for all three margins. The figures consistently show that a true discontinuity in treatment is absent. Moreover, S2 Fig shows that in the cohort where we do observe the official Cito score, the pattern of treatment probability across scores is similar to that for the Entrance Test. Hence, the lack of a strong discontinuity in treatment at or around the achievement threshold is not driven by the use of the proxy test, but by the specific dynamics of student sorting in the Dutch system.

Although Figs 3–5 fail to show a strong discontinuity in treatment probability, they do inhibit a non-linear pattern that can potentially be exploited. Because track assignment is

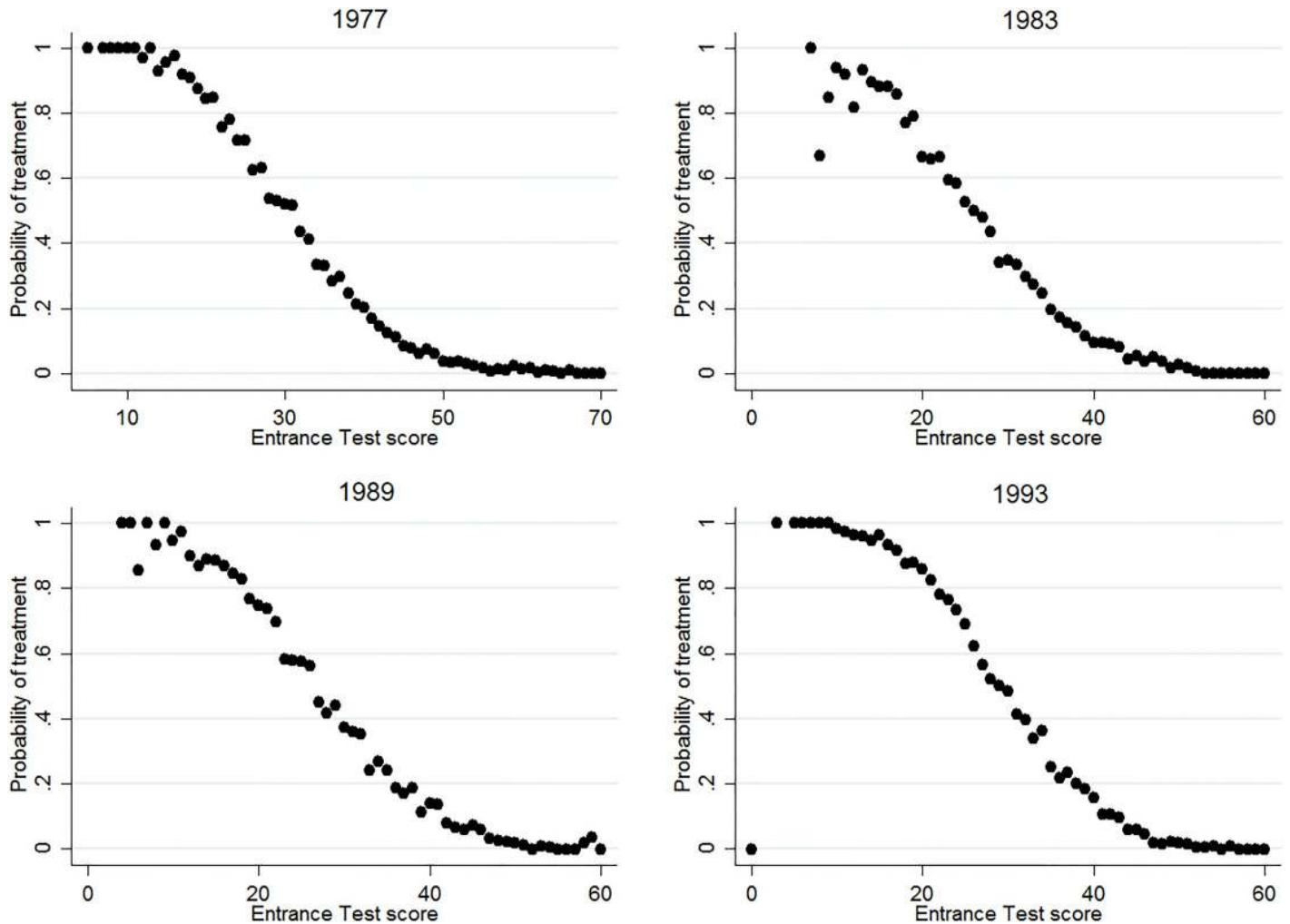


Fig 3. Share of students assigned to T1, across test scores and cohorts. The figure shows the share of students that are assigned to the T1 track, for every score on the Entrance test, separately for all cohorts.

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based on achievement, the probability of being assigned to a certain track is highly responsive to changes in achievement around a certain (implicit) threshold score. The treatment probability increases strongly around this score, while it remains flat in segments before and after. In other words, the increase in the probability of treatment is disproportionately low for increases in score that are far from the threshold and disproportionately high for increases in score around the threshold. This “disproportionality” can be exploited to estimate the effect of track assignment, through a two-stage model in which the fraction of students that is treated for a given test score acts as an instrument for the treatment indicator. Such a conditional mean approach does not rely on any defined threshold score, as it incorporates all changes in probability within the defined bandwidth.

A first condition for applying this approach is that the relationship between the outcome variable and the forcing variable has a different functional form (net of treatment) than the relationship between the treatment variable and the forcing variable. Fig 6 displays the relation between educational attainment or wages and the Entrance Test score, for the sample as a whole. S3 Fig shows the same relation split across tracks. These figures show a linear pattern

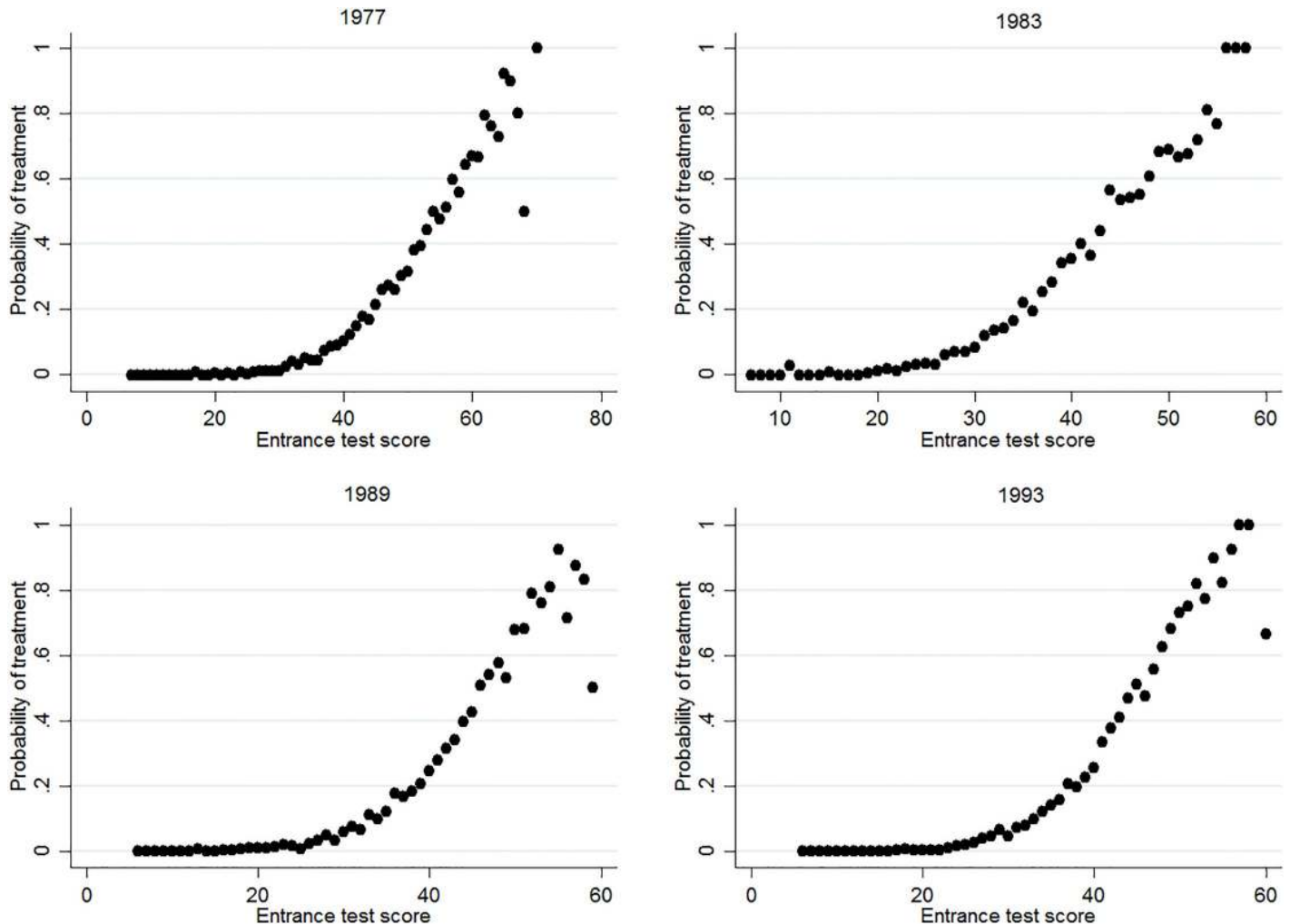


Fig 4. Share of students assigned to T3, across test scores and cohorts. The figure shows the share of students that are assigned to the T3 track, for every score on the Entrance test, separately for all cohorts. Students that attend T4 are excluded in the calculation.

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for educational attainment in all cohorts and for wages in the three oldest cohorts. The pattern for the 1993 cohort is strongly non-linear as many high achieving students have not entered the labor market yet, leading to two opposing forces in the relation between achievement and wages. Consequently, the optimal bandwidth is too narrow for a feasible first stage and wage effects are not estimated for the 1993 cohort.

A second condition is that there can be no similar non-linearities in other determinants of Y_i , as these will be attributed to the treatment effect. Fig 7 shows the relation between our main outcome variables and an imputed vector of observable characteristics. This imputed vector combines information on gender, month of birth, whether the student lives in one of the four big cities in the Netherlands, ethnicity, and dummy variables for parental social status (six categories, based on occupation) and parental education (three categories). It is constructed by regressing the outcome (years of schooling or wages) on this set of controls and then fitting the predictive values. Hence, control variables that relate more strongly to the outcome receive a higher weight in the vector. The figures show that the relation between the imputed control vector and the main outcomes is highly linear when the vector is constructed with respect to

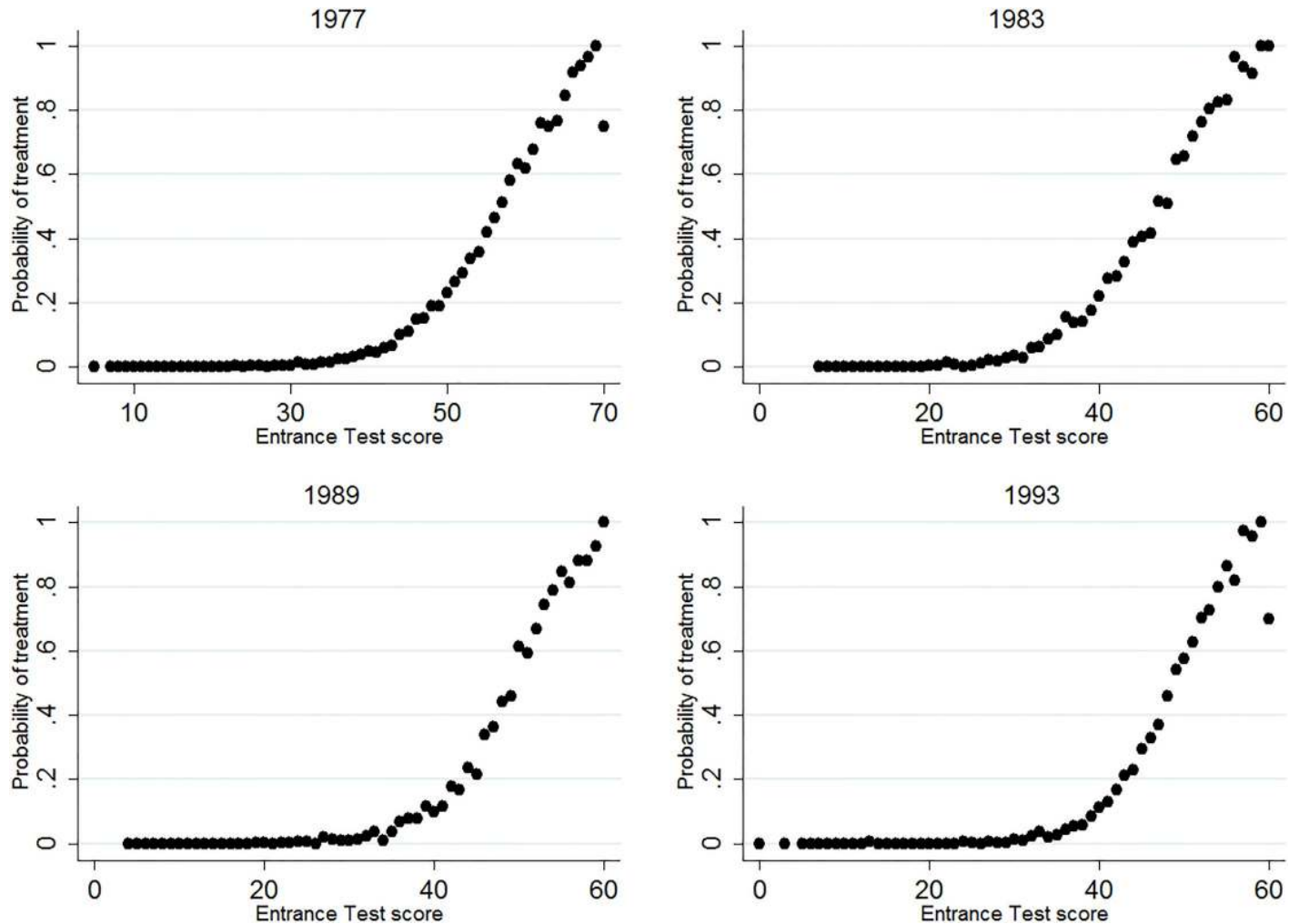


Fig 5. Share of students assigned to T4, across test scores and cohorts. The figure shows the share of students that are assigned to the T4 track, for every score on the Entrance test, separately for all cohorts.

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years of schooling, in all cohorts. With respect to wages, the patterns are also highly linear for the 1977 and 1989 cohorts. For the 1983 cohort, there is some degree of non-linearity at the very high end of the distribution, but the number of observations for these high scores is low. The pattern for the 1983 cohort is also more erratic, as the test taken in that year is less predictive of future outcomes.

[S4–S7](#) Figs show the relation to the Entrance Test for all controls separately. For the purpose of this exercise, parental education and parental occupation have been expressed as continuous variables rather than categorical dummies as in the estimation model (occupational categories are ranked by average wage). These separate relations also show highly linear patterns. Modest exceptions occur for gender in 1983 and urbanization in 1993. Note that the latter has comparatively modest explanatory power towards outcomes. The former may explain the non-linearity at the higher end for the 1983 cohort in [Fig 7](#). We assess sensitivity to the inclusion of control variables in the Robustness section, which indicate to what extent these particular cases lead to sensitivity in the results. The strongly linear patterns between S_i and the main outcome variables and between key observable characteristics and the main outcome

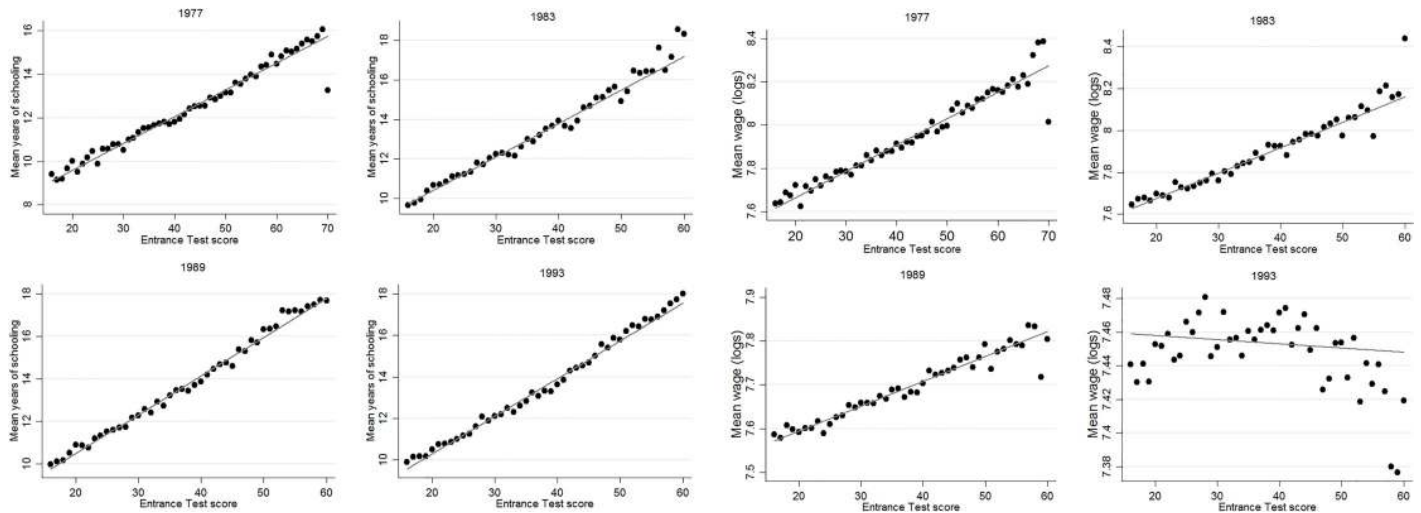


Fig 6. Mean years of schooling and wages across test scores and cohorts. The figure shows the mean years of schooling (left half) and mean log wages (right half) for every score on the Entrance Test, separately for all cohorts.

<https://doi.org/10.1371/journal.pone.0215493.g006>

variables in the large majority of cases provide evidence in favor of the validity of the assumption that the non-linearity in the relation with S_i is exclusive to the probability of track assignment. Given a wide enough bandwidth, we can exploit this non-linearity to estimate the effects of track attendance.

The Two Stage Least Squares (2SLS) model becomes:

$$\theta_i^M = \alpha_0^M + \alpha_1^M w_i^M + f_k^M(S_i) + \alpha_2^M X_i' + \eta_i^M$$

$$Y_i = \beta_0^M + \beta_1^M \hat{\theta}_i^M + f_k^M(S_i) + \beta_2^M X_i' + \epsilon_i^M \quad (3)$$

where $w_i = E[\theta_i|S_i]$ (the conditional mean), which serves as the instrument for track

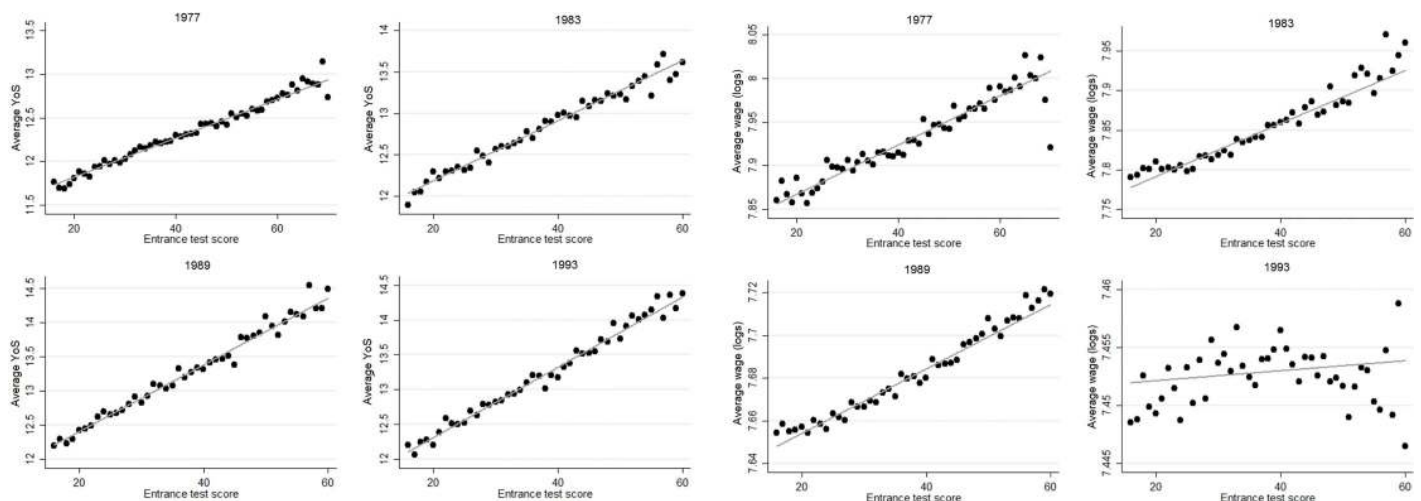


Fig 7. Control vector by Entrance Test score. The figure shows the average values of a control vector for every score on the Entrance Test, separately for all cohorts. The control vector is constructed by regressing either years of schooling (left half) or log wage (right half) on a set of controls and then fitting the predictive values. Controls are: gender, month of birth, urbanization, ethnicity, dummies for parental social status (six categories) and dummies for parental education (three categories).

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attendance. The outcome Y_i represents measures of either educational attainment or wages. For each outcome, three different treatment effects (θ_i^M) are estimated for each of the three margins M : T2 vs. T1; T3 vs. T2 and T4 vs. T3. The function $f_k(S_i)$ represents the control function for the forcing variable, i.e. the Entrance Test. We employ a linear control function in all of the main analyses. We further include a vector of observable background characteristics (labeled X'). This is the same set of controls as used in Fig 7. Note that Model 3 is feasible, as long as w_i^M is a strong enough predictor of θ_i^M , conditional on S_i and X'_i , which would ensure a sufficiently strong first stage. For this to be the case, w_i^M needs to have a different functional form in relation to θ_i^M than the other variables in the model (within the specified bandwidth).

The approach we use bears similarities to that of [19], although in a different setting. They show that non-linearities in hedonic markets can be exploited in an instrumental variable (IV) approach through the calculation of conditional mean functions. The approach assumes that all unobserved determinants of the outcome are linearly related to S_i across the bandwidth, and therefore captured by the control function. In other words, the model assumes that ϵ_i is mean independent of S_i ($E(\epsilon_i|S_i) = 0$). This assumption is stronger than for an RDD, which requires that other (unobserved) determinants of the outcome are smoothly continuous at the threshold. On the other hand, our model relies on weaker assumptions than an OLS model, which requires that such determinants are completely unrelated to the treatment.

Bandwidth selection. As our objective is to estimate track assignment effects at the achievement margin, we mainly want to select observations close to the (implicit) achievement margin. As with the RDD, we therefore need to establish bandwidths and assess whether the first stage of the model is still valid for the resulting subsample. We follow the typical approach used for RDD models by executing a cross-validation (CV) procedure to identify the bandwidth that minimizes the mean squared error of the control function [20]. The CV procedure weighs the additional power of including more observations against the loss in precision from moving further away from the threshold. As there is no explicit threshold score, we set the implicit threshold at the score where the treatment probability surpasses the 50% mark. We then step-wise extend the bandwidth from this point on each side of the threshold, and assess which particular score range minimizes the mean squared error.

We execute the CV procedure for different sample compositions with respect to the number of included tracks and elicit the optimal one. While it is sufficient to include only those two tracks at the threshold, inclusion of additional tracks provides additional power and precision, which can be especially valuable when dealing with tracks for which test scores are concentrated within a small range. Although including additional tracks can lead to a bias if the relationship between S_i and Y_i strongly differs across tracks, the specifications that include additional tracks should be rejected by the CV-procedure in such a case in favor of specifications that only include the two tracks at the margin. In the robustness analysis, we address how sensitive the results are to the number of tracks that are included in the estimation.

The optimal specifications and bandwidths are separately estimated for each cohort and for each outcome variable, and evidently also for each treatment effect. Results suggest using the full set of tracks for estimation of the effect of T4 vs. T3 (with the exception of educational attainment in 1993, where the recommended specification includes only the top two tracks), and to include the lowest three tracks both for estimation of T2 vs. T1 and estimation of T3 vs. T2. The optimal bandwidths for all treatment effects and cohorts are reported with the main IV results. S1 Table shows the first stage coefficients of the instruments and the corresponding F-statistics, applying each of these bandwidths. The F-statistics indicate that first stage power is strongly above conventional thresholds (with the exception of the estimation with respect to wages in the 1993 cohort, as argued before). F-statistics are especially strong for T4 vs. T3

estimation. They are comparatively lower for estimation of T3 vs. T2, but even the lowest value is still around 50. The first stage is sufficiently strong because these bandwidths are rather wide, thereby ensuring that the relation between S_i and θ_i is still non-linear within the estimation window. These bandwidths are nonetheless optimal according to the CV exercise, which confirms that the (net) relation between S_i and the outcomes is strongly linear, and that the linear control function still provides a very good fit also when moving further away from the implicit achievement threshold.

One may still question whether this conditional mean approach estimates treatment effects at the margin, especially when also tracks that are not part of the choice margin are included in the estimation. The IV model estimates a LATE for students for which incremental changes in measured achievement induce changes in track attendance. Hence, segments of the distribution where the treatment probability remains flat in Figs 3–5 have no weight in the estimation of treatment effects. At the same time, the model puts more weight on students that are at the (implicit) achievement threshold, where the slope in Figs 3–5 is steepest. Still, there might be heterogeneity in treatment effects within the increasing parts of the figures, and the estimates can as such be partly driven by the potentially differential treatment effects of those who are relatively further from the margin. We assess this concern by conducting a heterogeneity analysis and inducing variation in sample composition in the Robustness section.

Results

We now present the estimated effects of track assignment at all three margins. The coefficients always represent the effect of attending the higher of the two tracks at the margin. Our main outcome variables are wages and educational attainment. We also add estimation of track assignment for school achievement in grade 9.

Table 2 reports results of each treatment on years of schooling and wages for a simple OLS model that regresses the outcome variable on track assignment and the score on the Entrance Test. These OLS estimates are presented for comparative purposes. A direct comparison with the IV estimates is not straightforward as IV elicits a LATE while OLS elicits an average treatment effect (ATE). OLS estimates are still valuable as they likely provide an upper bound of the ATE of attending the higher track at the margin, as one would expect that students in the higher track are better endowed in unobserved characteristics that are positively related to educational attainment and wages (also conditional on S_i and X'_i). Additionally, a comparison of OLS results with and without controls for observable characteristics can provide an indication of how sensitive OLS models are to selection bias.

Table 2 shows that OLS estimates are consistently in favor of attending the higher track. Estimates are relatively highest for T4 vs. T3 treatment, for both outcomes. Coefficients indeed reduce when adding controls when the outcome is years of schooling, but this is less clear for the wage regressions. Wage estimates become even larger at the lowest margin when controls are added. This is, however, solely due to the control for gender (i.e. women are more likely to attend T2 vs. T1 for a given score, while they earn lower wages). As expected, those attending the higher track at the margin have higher educated parents from higher social classes, and controlling for these variables lowers treatment effects in the OLS model in all cases.

Educational attainment

The estimates of the long-run effects of tracking for the main IV model are presented in Table 3. All estimations are based on optimal bandwidths as suggested by the CV procedure. S2 and S3 Tables show that results are strongly consistent for alternative bandwidths (with the

Table 2. OLS estimates of the long-run effect of track assignment.

	T2 vs. T1		T3 vs. T2		T4 vs. T3	
	YoS	Wage	YoS	Wage	YoS	Wage
Panel A: No Controls						
1977 cohort	1.22*** (0.087)	0.082*** (0.012)	0.841*** (0.071)	0.116*** (0.0094)	1.61*** (0.080)	0.188*** (0.0092)
1983 cohort	1.66*** (0.071)	0.067*** (0.0075)	1.18*** (0.103)	0.101*** (0.010)	2.27*** (0.099)	0.162*** (0.0094)
1989 cohort	1.77*** (0.108)	0.0082 (0.0081)	1.80*** (0.115)	0.060*** (0.0069)	2.46*** (0.102)	0.076*** (0.0072)
1993 cohort	1.81*** (0.099)	-	1.85*** (0.102)	-	1.61*** (0.107)	-
Panel B: With controls						
1977 cohort	1.13*** (0.080)	0.093*** (0.0084)	0.776*** (0.071)	0.109*** (0.0083)	1.51*** (0.080)	0.170*** (0.0083)
1983 cohort	1.55*** (0.072)	0.096*** (0.0066)	1.05*** (0.101)	0.109*** (0.0093)	2.11*** (0.100)	0.153*** (0.0090)
1989 cohort	1.43*** (0.097)	0.022*** (0.0066)	1.53*** (0.106)	0.063*** (0.0068)	2.12*** (0.098)	0.076*** (0.0073)
1993 cohort	1.56*** (0.094)	-	1.60*** (0.100)	-	1.48*** (0.100)	-

***Significant at 1% level

The table shows the effect of track attendance on years of schooling (YoS) and the log of the average monthly wage, measured by using an OLS model that regresses the outcome on track attendance and the Entrance Test score, for the three choice margins. Results are separately presented for a model without and a model with controls (for a list of control variables, see Fig 7. The included tracks and applied bandwidths are as suggested by the CV procedure (and the same as in the IV model, see Table 3). “Wage” takes the average wage over the period 2001 to 2007. Standard errors are between parentheses and are robust and corrected for clustering at the school level.

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caveat that very narrow bandwidths are not feasible in the model because the relation between S_i and θ_i would become linear).

Columns 1, 3 and 5 show how track assignment affects educational attainment, for all three margins. Higher tracks consistently lead to more years of education at both the lowest and the highest margin. The point estimates center around 1.5 extra years of education in each case. For T2 vs. T1 treatment, the imprecision of the estimates is relatively high. Estimates are just shy of statistical significance in 1983 and 1989, but effect sizes are rather consistent. For T4 vs. T3 treatment, effects gradually increase by cohort, which could be related to the fact that higher education attendance has increased over the same period. All estimates for the effect of T3 vs. T2 are statistically insignificant. Standard errors are large, especially for the 1983 and 1989 cohorts, but the point estimates are consistently low. Hence, it appears that attending a higher track does not translate into higher educational attainment for students at this margin. Additional analysis (not shown) indicates that students that attend T3 over T2 at the margin still predominantly complete Track 3 and also obtain slightly more *hbo* diplomas, but are simultaneously less likely to complete any post-secondary education.

Wages

Columns 2, 4 and 6 of Table 3 show the effects of track assignment on monthly wages. Results indicate that attending T2 vs. T1 at the margin leads to higher monthly wages of around 15% for the 1977 cohort (ages 36-42) and the 1983 cohort (ages 30-36) and around 9% for the 1989

Table 3. IV estimates of the long-run effect of track assignment.

	T2 vs. T1		T3 vs. T2		T4 vs. T3	
	YoS	Wage	YoS	Wage	YoS	Wage
1977 cohort	1.76** (0.706)	0.147*** (0.039)	0.204 (0.605)	-0.122*** (0.040)	1.00*** (0.199)	0.071*** (0.027)
BW	[10–47]	[12–59]	[33–70]	[20–70]	[15–64]	[15–64]
N	15,433	15,969	14,749	15,809	24,448	19,772
1983 cohort	1.17 (0.753)	0.152*** (0.051)	0.010 (0.929)	-0.122 (0.086)	1.25*** (0.339)	0.027 (0.034)
BW	[22–48]	[15–55]	[26–60]	[25–60]	[21–60]	[21–60]
N	9,758	9,697	8,397	7,505	12,811	10,926
1989 cohort	1.04 (0.729)	0.088** (0.038)	-0.028 (0.981)	-0.053* (0.029)	1.54*** (0.273)	0.013 (0.021)
BW	[5–40]	[19–55]	[32–60]	[20–60]	[20–53]	[20–53]
N	11,560	11,828	7,725	11,591	14,847	13,445
1993 cohort	1.19* (0.716)	-	0.139 (0.710)	-	2.06* (1.06)	-
BW	[10–40]		[28–60]		[10–60]	
N	11,558		10,384		6,014	

*Significant at 10% level

**Significant at 5% level

***Significant at 1% level

The table shows the IV estimates of the effect of track attendance on years of schooling (YoS) and the log of the average monthly wage, using Model 3, for the three choice margins. Wages are averaged across 2001–2007. This (generally) corresponds to ages 36–42, 30–36 and 24–30 for cohort 1977, 1983 and 1989 respectively. Following the results of the CV approach, estimation of T2 vs. T1 and of T3 vs. T2 includes the lowest three tracks; estimation of T4 vs. T3 includes only the top two tracks for YoS in 1993 and includes all tracks for all other estimates. Wage effects are not estimated for the 1993 cohort because of a lack of first stage power. Standard errors are between parentheses and are robust and corrected for clustering at the school level. Bandwidths (BW) are based on the CV procedure. First stage results are provided in S1 Table.

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cohort (ages 24–30). For T4 vs. T3 treatment, results show a positive effect of around 7% for the 1977 cohort. Wage estimates are positive but statistically insignificant for the 1983 and 1989 cohort. Results by year show that estimates for the 1989 cohort steadily increase across the time period 2001–2007 and are positive and statistically significant in the more recent years. These estimates point to wage gains of around 4 to 5%. As such, it seems that there are (small) wage gains for the 1989 cohort when they have built up a few years of labor market experience. This is in line with [14], who show that wages increase more sharply with experience in academic tracks. Estimates for the 1983 cohort are consistently low. Comparing the 1977 and 1983 cohorts at the same age (2001 vs 2007 wages, respectively), the 1977 cohort still has considerably stronger wage gains (estimates are consistently around 7% in all years for the 1977 cohort). Hence, the difference clearly represents a cohort effect rather than an age effect.

Estimates of the effect of track attendance on wages are statistically significant and negative for the T3 vs. T2 margin. This contrasts with the positive effects for the other two margins, and also with the lack of an effect on years of schooling. This suggests that marginal students are often assigned to a T3 track when they would be better off in a T2 track, from a lifetime earnings perspective. It is not surprising that this is not yet reflected in the years of schooling results as the T3 students attend a higher track and are eligible for higher levels of post-secondary education. One could therefore say that the lack of a positive effect for educational attainment already signals a high potential for sorting too many students into T3 at the T2/T3 margin.

Our main findings appear to contrast with those of [15], who find no long-run effect for either educational attainment or wages from attending the higher track. However, as stated before, they estimate treatment effects at a very different margin. The lack of any effect in their study does not imply that ability thresholds are placed 'optimally'. For example, it can also reflect that underambitious allocation of younger students is canceled out by overambitious allocation of older students. Additionally, [15] suggest that their zero effects are a consequence of the high upward flexibility in the German system. The Dutch tracking system is comparatively more rigid with respect to upward mobility between tracks, which can also explain the difference in results.

As mentioned before, the wage data are corrected for the FTE of the job, exclude very low wages, and are topcoded at 20,000 euro's per month. Wage results are similar when using uncorrected wages. Estimates for T2 vs. T1 treatment are somewhat larger. Similarly, we find that T2 vs. T1 treatment implies an increase in the FTE and a (slight) increase in employment. We identify negative point estimates for FTE in case of T4 vs. T3 treatment, which is statistically significant in the 1983 cohort (results are available on request). Additionally, we do not find evidence of strong heterogeneity of treatment effects across background characteristics, such as gender and parental education, although the precision of these estimates tends to be low (results are available on request). Hence, while it could be expected that especially those with highly educated parents are prone to be sent to a too demanding tracks, the evidence suggests otherwise. A potential explanation is that higher educated parents also have more means to support their child if it is struggling in a more demanding track.

Comparing the IV estimates (Table 3) to the OLS estimates (Table 2), the results for the two higher choice margins are more favorable for attending the higher track in the OLS model. This likely reflects the positive bias in the ATE estimates due to students self-selecting into tracks. For T2 vs. T1 treatment, the positive wage estimates are higher for the IV model. As a strong negative bias in the ATE appears unlikely, this is likely driven by the local nature of IV estimates. The IV model estimates the effect of T2 vs. T1 for students that are induced to switch to a higher track when they have higher achievement levels. This could be students that are especially ambitious and they might also attend relatively better schools (they are more likely to attend schools with stronger entrance requirements which is likely to imply better peer quality). This could explain the relatively high local estimates in this particular case.

School achievement

Table 4 shows estimates of the effect of track assignment on school achievement in grade 9. While the focus of this study is on the long-run effects of track attendance, achievement could provide a potential mechanism towards such long-run effects. As explained in the data section, the 9th grade test results in the 1989 and 1993 cohort contain a large share of (selective) missing values. To deal with this, we provide two imputation approaches. The first imputes missing tests from the relevant domain of the Entrance test. Taking into account that those that did not take the test might have developed especially poorly between grades 7 and 9, we subtract half a standard deviation from the imputed values in the second imputation approach. Comparing these alternative approaches provides an indication of the robustness of the results to the issue of missing values. Panel B of Table 4 shows results for the 1999 cohort, in which the share of missing tests is negligible. The data for this cohort also contain a problem-solving test. There are only two margins to estimate here, as tracks T1 and T2 were merged in 1999.

Panel A of Table 4 shows that results are indeed sensitive to imputation, but the positive treatment effect for T4 vs. T3 appears robust, especially for language. Positive effects at this margin are also identified for the 1999 cohort. In Panel A, there is no (robust) evidence for an

Table 4. IV estimates of the effect of track assignment on 9th grade test scores.

Panel A: 1989 and 1993						
	T2 vs. T1		T3 vs. T2		T4 vs. T3	
	Language	Math	Language	Math	Language	Math
1989 Base	0.136 (0.239)	0.0092 (0.286)	-0.020 (0.198)	0.302* (0.165)	0.641*** (0.098)	0.141 (0.096)
BW	[5–40]	[5–40]	[20–50]	[20–50]	[20–53]	[20–53]
N	6,122	6,403	4,705	4,716	8,471	8,596
1989 Impute I	0.043 (0.123)	0.131 (0.167)	-0.102 (0.118)	0.072 (0.110)	0.418*** (0.072)	0.121* (0.063)
1989 Impute II	-0.091 (0.136)	-0.020 (0.179)	-0.154 (0.134)	0.018 (0.132)	0.434*** (0.087)	0.153* (0.079)
N	11,560	11,560	8,501	8,501	14,847	14,847
1993 Base	0.781** (0.273)	-0.339 (0.329)	-0.0070 (0.266)	0.0022 (0.152)	0.836** (0.361)	0.436* (0.230)
BW	[10–40]	[10–40]	[20–60]	[20–60]	[10–60]	[10–60]
N	5,031	4,617	4,141	3,692	2,953	2,661
1993 Impute I	0.107 (0.122)	0.247 (0.160)	-0.0088 (0.135)	-0.027 (0.085)	0.512*** (0.218)	0.090 (0.121)
1993 Impute II	0.108 (0.126)	0.266* (0.158)	-0.151 (0.170)	-0.035 (0.113)	0.465* (0.279)	0.069 (0.186)
N	11,558	11,558	8,752	8,752	6,014	6,014
Panel B: 1999						
	T3 vs. T1/T2			T4 vs. T3		
	Language	Math	PS	Language	Math	PS
Entrance test	0.292** (0.135)	0.321 (0.206)	-0.122 (0.158)	0.186* (0.097)	0.454* (0.247)	0.083 (0.166)
BW	[10–60]	[18–60]	[15–60]	[20–60]	[30–51]	[30–60]
N	4,451	3,508	4,341	4,284	2,682	3,510
Cito test	0.299** (0.132)	0.461* (0.237)	-0.0024 (0.140)	0.405* (0.209)	0.406* (0.237)	0.080 (0.082)
BW	[515–542]	[510–539]	[520–550]	[510–547]	[534–548]	[520–550]
N	2,996	1,523	4,195	1,755	2,571	4,195

*Significant at 10% level

**Significant at 5% level

***Significant at 1% level

The table shows the IV estimates of the effect of track assignment on school achievement in grade 9, using Model 3. ‘Impute 1’ imputes missing scores from the same domain on the Entrance test. ‘Impute 2’ additionally subtracts 0.5 standard deviation from the imputed values. Results for the imputed approaches use the same bandwidths and included tracks as in the base approach, following the CV procedure. Panel B shows results for the 1999 cohort, when using either the Entrance Test (low stakes) or the Cito test (high stakes) as forcing variable. The new *vmbo* track that is merged in 1999 is labeled as ‘T1/T2’. Students in the practical specialization of the new merged track take a different math test and are excluded. In panel A, estimation of T2 vs. T1 includes the lowest three tracks; estimation of T3 vs. T2 includes the two middle tracks, and estimation of T4 vs. T3 includes all tracks. In panel B, all estimations include all tracks. The full range for the Cito test score is [501–550]. ‘PS’ refers to a problem-solving test. Standard errors are between parentheses and are robust and corrected for clustering at the school level.

<https://doi.org/10.1371/journal.pone.0215493.t004>

effect on achievement at the other margins. In contrast, panel B provides positive effects at the lower margin for both language and math. The impact of track assignment on the problem-solving test is low and statistically insignificant for both margins. This could reflect that such skills are less driven by instruction and peer effects or, more generally, difficult to influence at later ages.

The positive results for language and math for the lower margin appear to contradict panel A, but these results are difficult to compare because of the merger of the two lowest tracks. This has effectively created a new margin. Moreover, the 1999 cohort constitutes the first cohort after the policy change and we could therefore also pick up on transition effects. We conclude that there appear to be positive achievement effects for T4 vs. T3 at the margin, while we do not find any achievement effects at the lower margins under the old system. The identified effect for T4 vs. T3 appears predominantly driven by peer effects. Adding average peer quality in class as a control leads to low and statistically insignificant estimates. The literature on peer effects suggests that an increase in peer quality of 1 standard deviation leads to an increase in individual achievement by around 0.40 of a standard deviation [21]. The difference in peer achievement between T3 and T4 is around 0.75 of a standard deviation, indicating the effect sizes are in line with what the literature predicts on the basis of peer effects.

The 1999 cohort also contains data for the high-stakes Cito test, which allows a comparison of using either test as forcing variable. Panel B of [Table 4](#) shows that using either the Entrance Test or the Cito test leads to similar estimates. This result is in line with the observation from [S2 Fig](#) that the Cito score is not necessarily a stronger predictor of track assignment. Hence, having only a proxy for the true selection test does not appear to impact the estimates in a strong way. Additionally, having the official forcing variable available for this cohort allows us to conduct an RDD model as well. [S4 Table](#) shows a comparison of results between the traditional RDD approach and our main approach. Results are consistent, which lends further validity to our approach.

It should be emphasized that these tests elicit *academic* achievement, while attending vocational tracks can potentially benefit students' skills in non-academic disciplines. The data to test the impact of track attendance on vocational skills are not available. Relatedly, track attendance can potentially also effect non-cognitive skills, which are also highly important for future earnings [22]. The data are relatively limited when it comes to such outcomes, e.g. measurement of Big Five personality skills are missing. Cohorts 1977, 1989 and 1993 do contain 9th grade measures for school enjoyment and "need for achievement". The latter can be seen as a proxy for motivation, and has been shown to be highly predictive for educational outcomes [23]. Effects for school enjoyment are low, and only statistically significant for T4 vs. T3 treatment in 1989 (with positive sign). Effects for 'need for achievement' are positive for T2 vs. T1 treatment and strongly negative for T3 vs. T2 treatment in all cohorts. Hence, the long-run treatment effects at these margins could be partially driven by intermediate effects on non-cognitive skills.

Discussion

We now discuss the implications of the results presented above, and explore potential mechanisms for the patterns in the estimates.

We find that, for students at the lowest and at the highest choice margin, being assigned to the higher track provides a return in the form of higher wages, but at the cost of time and resources spent on education. The wage gains can be a complete result of the increase in educational attainment, but other mechanisms can be in effect as well. As such, we cannot state that the wage gain represents the "return" on the extra investment in schooling, but if one wants to assess whether individuals are (economically) better off when attending the higher track, years of education and wages represent the relevant costs and benefits of that choice. From that perspective, one extra year of education has a "return" of around 10% on monthly wages for T2 vs. T1 treatment. For T4 vs. T3 treatment, the payoff is around 7% for the 1977

cohort, and negligible if we look at average wages for the 1983 and 1989 cohorts. If we take the more recent wage data for the 1989 cohort, the return for an extra year of education is around 3% at age 30.

The return to schooling is generally found to be around 8 or 9% in the literature; see, e.g., [24–26]. However, these averages hide a considerable amount of heterogeneity, and might not be representative of returns at the achievement margin. Returns to schooling at the margin of dropout are often found to be especially high [26]. This could also explain why our wage estimates are larger at the lowest margin. [11] estimate an average return to an extra year of college of 14%, but a marginal return that ranges from 1.5% to 8.5% (depending on the specific margin the policy change affects). The “return” we identify for T4 treatment is in line with those effect sizes.

The results for the highest margin may be seen as especially low given the positive effects on achievement. However, earlier cited studies have shown that attendance of higher tracks often leads to short-term increases in achievement but no or weak labor market returns. Study choice may also provide an explanation for the low returns. For the T4 vs. T3 margin, we find a relative increase in attendance of post-secondary studies with lower average wages, mainly towards humanities (see [S5 Table](#)). This also likely explains the slight negative effect on the FTE of the job at the T4 vs. T3 margin, as the hours worked for those with majors in these areas tend to be lower. From an individual perspective, it remains inconclusive whether students around the margin are better off in the higher track. Many individuals do not pursue higher education even when expected returns are high, due to, e.g., income risk and psychic costs of studying [27]. Whether attending the higher track represents a net gain or a net loss for the marginal student ultimately depends on his or her utility function.

From a strictly financial perspective, a back-of-the-envelope cost-benefit analysis indicates that the net returns are positive in those cases where we identify positive education and wage effects. For example, the 7.1% wage increase for T4 vs. T3 in 1977 would imply an increase of around 310 euro per month, or 3700 every year. Direct educational costs are around 7,850 euro (retrieved from [28], converted to 2007 euros) and opportunity costs are around 23,500 euro (based on the data at hand). Hence, this would clearly yield a positive net return over the lifetime (costs are naturally incurred earlier, but we take educational and opportunity cost data from 2007 and implicitly assume the growth rate in educational costs and in wages equals the discount rate). Benefit-cost ratios are even higher when calculated for the T2 vs. T1 margin. Increased tax returns would also cover the public cost of the extra education, but we again emphasize that this does not consider potential general equilibrium effects in both education (i.e. peer effects) and the labor market.

There is no ambiguity for the T3 vs. T2 margin, as there is a strong negative wage effect, for the same average years of schooling. What drives these negative wage effects, and why specifically at this margin? As mentioned before, the near-zero effects on average years of schooling hide some degree of substitution of *mbo* diplomas for more *hbo* diplomas but fewer post-secondary diplomas in general. It could be that the return to *mbo* diplomas is comparatively strong at this margin. Study choice may again provide an additional explanation. We find that attendance of T3 over T2 at the margin leads to more study choice towards health and, especially, humanities, and less towards exact sciences and, especially, economics (see [S5 Table](#)). Average wages are considerably higher in the latter compared to the former. Hence, study choice can explain part of these negative wage results. We can only speculate on the reason why these study choice patterns emerge, but it could be that being in a more demanding track and being ranked lower compared to classroom peers leads students to move away from studies that are perceived as having ‘more challenging’ curricula.

Additionally, the negative return of attending a higher track for these ‘middle tracks’ could be related to a pattern of job polarization. Such patterns have been identified in labor markets across developed countries, including the Netherlands [29]. This, however, seems unlikely given that there is no effect on educational attainment. Hence, the different sides of the margin do not seem to end up in jobs with different skill levels. Moreover, the T3 vs. T2 margin corresponds to around the 60th percentile in the distribution, which is markedly above the margin between low-skilled and medium-skilled jobs in the Netherlands as identified by [29].

The question remains why effects are negative at this specific margin? Either parents and students are more likely to push for the higher track when close to this achievement margin, and/or schools set more lenient entry requirements. The former could occur because completing T3 provides direct access to higher education and could therefore be seen as an especially crucial threshold. For schools, the incentives to set lower thresholds indeed appear comparatively high at this threshold. Tracks 3 and 4 typically fall within the same school while T2 and T3 do not. As such, schools are incentivized to set higher thresholds between T3 and T4 to increase average student quality within each track, while they are incentivized to set lower thresholds between T2 and T3 to attract more students. We lack the data to empirically assess these potential explanations.

It should be emphasized that, in this setting, “too lenient” thresholds do not strictly imply that the threshold scores that schools set are, on average, too low. As there is leeway for parents with high aspirations for their children to still get into higher tracks also when they have relatively low scores, or to switch to another school with more lenient requirements, it can also be the case that such behavior pushes higher formal thresholds into lower effective thresholds. Hence, negative wage returns for attending T3 over T2 could potentially also be avoided by stricter adherence to thresholds, rather than increasing the thresholds as such. In any case, our results indicate to parents that pushing students into higher tracks around the margin is not necessarily beneficial for later-life outcomes.

Robustness

We now assess the sensitivity of our results towards different specifications and robustness tests. Many of the tests we conduct are similar to those for the traditional RDD, as the identification threats are similar (although based on more lenient assumptions) compared to our non-linearity approach. As stated before, where the RDD assumes no discontinuity in other determinants, our design assumes linearity in other determinants across the specified range of the running variable. We critically assess this assumption by analyzing sensitivity to the inclusion of observable characteristics and to excluding tracks that are not part of the relevant margin. The latter two tests also address to what extent our design still estimates treatment effects at the achievement margin. Additional robustness tests for bandwidth sensitivity (S2 and S3 Tables) and changes in the manner in which the non-linearity in treatment probability is exploited (S6 Table) are provided in the supporting material.

Observable characteristics

The estimation approach assumes that any other determinants of the outcome variable are linearly related to achievement and thereby captured by the control function for the Entrance Test score. When the inclusion of control variables strongly changes the estimates, this is a strong indication that this assumption is invalid. The main results presented before include the set of controls X_i . Table 5 compares those to a model without controls. The changes in the estimates are all small. This result confirms descriptive statistics shown before that indicated that the relation between achievement and control variables is linear. Fig 7 has shown that

Table 5. IV estimates of the effect of track assignment: With and without controls.

	T2 vs. T1			T3 vs. T2			T4 vs. T3		
	NC	X'	X' + TR	NC	X'	X' + TR	NC	X'	X' + TR
1977 YoS	1.98*** (0.697)	1.76** (0.706)	1.58** (0.676)	0.101 (0.618)	0.204 (0.605)	-0.088 (0.719)	0.934*** (0.196)	1.00*** (0.199)	0.871*** (0.267)
R ²	[0.114]	[0.145]	[0.145]	[0.072]	[0.114]	[0.127]	[0.214]	[0.235]	[0.239]
N	15,433	15,433	14,712	14,749	14,749	14,356	24,448	24,448	21,846
1983 YoS	1.26* (0.717)	1.17 (0.753)	1.18* (0.670)	0.121 (0.948)	0.010 (0.929)	-0.467 (1.05)	1.18*** (0.336)	1.25*** (0.339)	1.09** (0.449)
R ²	[0.130]	[0.156]	[0.163]	[0.051]	[0.098]	[0.125]	[0.223]	[0.250]	[0.274]
N	9,758	9,758	9,548	8,397	8,397	8,220	12,811	12,811	11,027
1989 YoS	1.21* (0.707)	1.04 (0.729)	0.893 (0.878)	-0.032 (0.987)	-0.028 (0.981)	-0.325 (1.14)	1.60*** (0.290)	1.54*** (0.273)	1.25*** (0.431)
R ²	[0.176]	[0.217]	[0.223]	[0.088]	[0.168]	[0.186]	[0.239]	[0.284]	[0.303]
N	11,560	11,560	11,164	7,725	7,725	7,569	14,847	14,847	10,402
1993 YoS	1.47** (0.688)	1.19* (0.716)	1.27* (0.691)	0.062 (0.719)	0.139 (0.710)	-0.095 (0.776)	1.76* (1.05)	2.06* (1.06)	2.12 (1.35)
R ²	[0.175]	[0.207]	[0.216]	[0.095]	[0.154]	[0.187]	[0.084]	[0.105]	[0.106]
N	11,558	11,558	10,960	10,384	10,384	9,862	6,014	6,014	5,706
1977 wage	0.165*** (0.039)	0.147*** (0.039)	0.144*** (0.035)	-0.137*** (0.042)	-0.122*** (0.040)	-0.179*** (0.046)	0.085** (0.027)	0.071*** (0.027)	0.093** (0.040)
R ²	[0.069]	[0.263]	[0.265]	[0.017]	[0.211]	[0.225]	[0.127]	[0.299]	[0.310]
N	15,969	15,969	15,320	15,809	15,809	15,255	19,772	19,772	19,061
1983 wage	0.176*** (0.050)	0.152* (0.051)	0.145*** (0.042)	-0.111* (0.059)	-0.122 (0.086)	-0.153 (0.095)	0.026 (0.035)	0.027 (0.034)	-0.0012 (0.036)
R ²	[0.062]	[0.185]	[0.188]	[0.021]	[0.118]	[0.142]	[0.110]	[0.203]	[0.226]
N	9,697	9,697	9,493	7,505	7,505	7,351	10,926	10,926	10,695
1989 wage	0.093 (0.037)	0.088** (0.038)	0.082*** (0.032)	-0.057* (0.030)	-0.053* (0.029)	-0.074** (0.033)	0.0036 (0.042)	0.013 (0.021)	-0.0080 (0.020)
R ²	[0.019]	[0.060]	[0.063]	[0.012]	[0.048]	[0.050]	[0.022]	[0.042]	[0.046]
N	11,828	11,828	11,508	11,591	11,591	11,283	13,445	13,445	13,111

*Significant at 10% level

**Significant at 5% level

***Significant at 1% level

The table shows the IV estimates of the effect of track assignment on years of schooling (YoS) and wages for the IV model in three specifications: without controls (NC), with control vector X' (Model 3), and with an additional control for track recommendation (TR). For a list of control variables, see Fig 7. For an explanation on the estimation approach and an overview of all bandwidths, see Table 3. YoS = Years of Schooling. Standard errors are between parentheses and are robust and corrected for clustering at the school level. R²s are reported between brackets.

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some non-linearity is present at the higher end for wages in the 1983 cohort. The results from Table 5 show that this has no major impact on the estimates, as sensitivity in the 1983 cohort is not larger than in other cohorts. The underlying reason is that any such non-linearity is also reflected in the CV procedure for optimal bandwidths, which are consequently more narrow.

Table 5 also shows estimates when we additionally include a control for the teacher recommendation that students receive at the end of primary school. Track recommendations correlate strongly with the Cito exit test, but can differ when teachers feel that the test does not accurately reflect student ability or potential. As such, the variable provides a valuable control for aspects that are important for future success but not fully captured by test scores, such as

non-cognitive skills and motivation. If our estimates would strongly respond to controlling for the teacher recommendation, it would suggest that they are partly driven by non-linearity in such unobserved determinants across the control function. Addition of the teacher recommendation reduces first stage power somewhat, partly because the variable is not available for around 5 to 10% of the sample. [Table 5](#) shows that the sensitivity to this additional control is very minimal.

As shown by [\[30\]](#), coefficient stability when adding controls should always be judged in relation to the explanatory power of those control variables. [Table 5](#) also reports the R^2 of the different specifications. The results show that control variables have substantial explanatory power, especially towards wages and especially for higher tracks, and that the teacher recommendation further adds to this. The fact that the latter explains outcomes also conditional on student achievement and background suggests that it indeed captures other types of skills than a test score. The sensitivity of the estimates is not higher in cases where the R^2 increases more strongly. We cannot rule out that non-linearities in unobserved determinants of long-run outcomes bias our results. Nonetheless, the low sensitivity of the estimates to the inclusion of observed indicators lends further validity to our results. The high coefficient stability also in cases where the R^2 increases substantially implicitly indicates that any potential selection on unobservables (conditional on S_i) would have to be very strong to fully drive these estimates.

A more formal exercise to assess possible non-linearity in important observable characteristics is to conduct placebo tests that use constructed control vectors as outcome variables in [Model 3](#). Results of this exercise are shown in [S7 Table](#). Only one of the placebo tests is statistically significant (at the 10% level). Given that we test this for 21 different hypotheses, these results support the assumption that important observable determinants are linearly related to our outcomes, and therefore are captured by the control function approach. We similarly obtain statistically insignificant estimates when the teacher recommendation is used as outcome.

Sample composition

We have argued before that our approach elicits treatment effects close to the margin, because the LATE puts more weight on those observations for which the first stage slopes are steeper. The consistency of results for more narrow bandwidths provides evidence in favor of this. Moreover, we can assess how sensitive results are to the exclusion of tracks that are not part of the relevant margin.

In the main approach, we have included at least one other track in addition to the two marginal tracks in all but one case, as these sample compositions are favored by the CV exercise. [S8 Table](#) shows results for the two possible alternatives in each case. As all alternatives involve a different sample, optimal bandwidths are re-estimated in each case. The model that only includes the two tracks at the margin (A2) is naturally less precise, but the differences in the point estimates are not large and not consistently in one direction. Moreover, all main conclusions remain the same. Hence, the inclusion of individuals in the sample that are not part of the tracks at the choice margin does not affect our results. These extra observations mainly help in obtaining more precise estimates, but do not affect the point estimates of the LATE. The fact that even sample specifications that are rejected by our CV procedure produce results similar to our main estimates lends additional validity to the findings.

We further estimate interactions between treatment and achievement indicators, namely the Entrance Test score and the teacher recommendation given at the end of primary school (results are available on request). These estimations also provide insights into the extent to which the main estimates reflect the treatment effect for those at the margin (namely those

with a mixed teacher recommendation). With respect to the Entrance Test score, we identify positive point estimates as one would expect given the depiction in the theoretical model, but these are generally low and only statistically significant with respect to years of schooling at the highest margin. Hence, we do not identify strong heterogeneity by achievement but this is likely the result of a lack of statistical power and the fact that students from a specific track can be concentrated within a relatively small range. Put differently, a large part of the range of the functions portrayed in Fig 1 is not observed in reality. Interactions with the teacher recommendation are also largely statistically insignificant. More importantly, the (summed) point estimates for those with a mixed recommendation are very close to the main estimates, across cohorts, outcomes and margins. In other words, the identified effect in the main model appears highly representative of the group with a teacher recommendation right at the margin.

Conclusion

This study has assessed the long-run effect of secondary school track assignment for students who at the margin of the required achievement level for a specific track. The empirical approach relies on a design that exploits non-linearity in the relation between treatment probability and achievement, while assuming that achievement is linearly related to other determinants of the outcome variable. The fact that track assignment is based on (implicit) achievement thresholds leads to a situation in which increases in the probability of attending a higher track are not proportional to increases in ability or potential. Descriptive statistics and various robustness analyses lend support to the assumption that the non-linearity in the relationship with achievement is exclusive to the probability of track assignment. Our results indicate that students in the Netherlands obtain higher educational attainment and higher wages when attending the higher track, for the choice margin between Tracks 1 and 2 and the choice margin between Tracks 3 and 4. The returns in the labor market are around 15% for the lower margin and between 3% and 7% for the higher margin, at the expense of around 1.5 additional years of schooling. Estimates for the choice margin between the two middle tracks are low and statistically insignificant with respect to educational attainment (although imprecisely estimated), and negative with respect to wages, indicating a wage loss of around 12% from attending the higher track.

Several potential mechanisms can drive these treatment effects. Attending higher tracks directly provides access to higher levels of post-secondary education, which subsequently is linked to higher expected earnings in later life. Additionally, different tracks imply different curricula that teach different types of skills, as well as differences in peer and school quality. We find robust evidence of positive effects of higher track attendance on school achievement for the higher margin, which is suggestive of true learning effects. In light of the positive effects for achievement and educational attainment, the labor market returns at the higher margin appear to be low. Additionally, labor market returns are negative for the middle margin, where there is no effect on educational attainment. These patterns appear to be explained at least partly by study choice. Attending a higher track leads to more frequent sorting into study majors with lower future earnings. This is suggestive evidence that the more challenging track in secondary school leads students to select 'less challenging' post-secondary educational paths. It also relates to recent findings by [31] that a lower rank in class (which attending a higher track directly induces) leads to lower investment in human capital, conditional on ability. We also identify negative treatment effects on motivation at the middle margin, which could contribute to this pattern of study choice. Further disentangling the different potential mechanisms that are behind the effects of track assignment is an interesting avenue for future research.

The results from this study highlight that, while higher tracks are associated with higher wages, the students who are at the achievement margin of a track do not necessarily obtain such strong wage gains. In our study, wage returns depend strongly on the margin we are looking at: high at the lower margin, positive but low compared to educational investments at the higher margin, and negative at the middle margin. We can only speculate on the underlying reasons for these differences. Different considerations of students and their parents and of schools can influence the location of the thresholds. Parental aspirations for the educational attainment of their children are known to be high and this can lead parents to push children into too demanding tracks. Our pattern of results could reflect that such parental overconfidence would be less prominent among low-achieving children. Additionally, the negative results for the middle margin could partly accrue due to the structure of Dutch schools. As the majority of T3 schools also offer T4 but not T2, there is a comparatively stronger incentive for schools to lower the threshold for T3 to attract more students. This variation in treatment effects across margins also makes it difficult to project the sign and size of track assignment effects in other countries. Replication of our analysis for those countries (possibly with a similar empirical approach) would be needed to assess the external validity of our findings.

For the lowest and highest choice margin, wage gains come at the expense of extra investment in education. Whether this is perceived as a gain or a loss for the individual depends on his or her utility function. The particular approach developed in this paper does not answer what is the optimal choice from the perspective of society. A lower threshold can negatively affect the untreated, because peer quality is reduced on both sides of the threshold. On the other hand, higher educational attainment could induce externalities for society in terms of reduced crime rates or productivity spillovers. The social welfare implications of different achievement thresholds for track assignment can be an interesting avenue for future research, for example by looking at exogenous variation in assignment over time induced by policy changes. At the same time, such policy changes cannot be used to assess the implications of choosing the higher over the lower track for the individual student at the margin, because they reassign a whole segment of the distribution and thereby also change the nature of each track.

Ideally, the effects of track assignment would be estimated by exploiting a strong discontinuity in explicit threshold scores for track eligibility. Despite the comparatively strong reliance on achievement in track assignment in the Dutch system, the traditional RDD was not feasible, even in the case where the official running variable is available. The identification approach therefore relies on the assumption that unobservable determinants of long-run outcomes are linearly related to achievement, which we cannot formally prove. While relying on stronger assumptions, the conditional mean approach presented in this study can provide an alternative approach for empirical studies with similar data designs.

Supporting information

S1 Fig. Educational attainment by track. Final educational attainment across tracks that the students are (initially) assigned to in secondary school, separately for each cohort.

(TIF)

S2 Fig. Probability of treatment: Low vs. High Stakes (1999). Shares of students that are sent to either the merged vocational track (left; labeled T1*) or the top track T4 (right), when using the low-stakes Entrance Test as the forcing variable (top) and when using the high-stakes Cito test as the forcing variable (bottom). All students entered secondary education in 1999.

(TIF)

S3 Fig. Mean years of schooling and mean wages by test score and track. Mean years of schooling (left) and wages (right; in logs) for every test score on the Entrance Test and for every track, for cohort 1977.

(TIF)

S4 Fig. Control variables across S_i : 1977 cohort. Mean values of all control variables for every score on the Entrance Test, for the 1977 cohort. Parental education and parental occupation are measured as continuous variables.

(TIF)

S5 Fig. Control variables across S_i : 1983 cohort. Mean values of all control variables for every score on the Entrance Test, for the 1983 cohort. Parental education and parental occupation are measured as continuous variables.

(TIF)

S6 Fig. Control variables across S_i : 1989 cohort. Mean values of all control variables for every score on the Entrance Test, for the 1989 cohort. Parental education and parental occupation are measured as continuous variables.

(TIF)

S7 Fig. Control variables across S_i : 1993 cohort. Mean values of all control variables for every score on the Entrance Test, for the 1993 cohort. Parental education and parental occupation are measured as continuous variables.

(TIF)

S8 Fig. Track assignment by test score. Shares of students assigned to a certain track for each score on the Entrance Test, across cohorts.

(TIF)

S9 Fig. Density of forcing variable. Density of the Entrance Test score, across cohorts.

(TIF)

S1 Table. First stage results.

(PDF)

S2 Table. Sensitivity of results to bandwidth.

(PDF)

S3 Table. Bandwidths for sensitivity exercise.

(PDF)

S4 Table. Main approach versus RDD (school achievement).

(PDF)

S5 Table. Effect of track assignment on study choice.

(PDF)

S6 Table. Logistic approach.

(PDF)

S7 Table. Placebo tests.

(PDF)

S8 Table. Different sample compositions.

(PDF)

S1 Data. Data coding and analysis. Stata do-files containing all data coding and model specifications.

(DO)

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