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Science of success: The causal effects of high school math and science on labor market outcomes

by

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Preliminary and incomplete!

Abstract

This paper tests for the causal effects of more intense focus on math and science in upper secondary education. Using data on all applicants to Swedish academic upper secondary tracks during 1985-92 we rely on admissions discontinuities to isolate the causal effects of studying at science or engineering tracks on future labour market outcomes. The (preliminary) results suggest that a more intense focus on high school math and science gives rise to a wage premium of around 10 percent. However, we find no statistically significant effects on IQ-scores after two years of study. Instead, our results suggest that the effect on labor market outcomes partially works through increased transitions to university education.

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1 Introduction

Several studies point to math and science as important factors for economic growth and development of countries. As an example, Hanushek and Kimko (2000) shows that internationally comparable mathematics and science test scores are strongly related to the economic growth of nations.[1] In accordance with this view, policy makers commonly view a highly skilled workforce, with a special emphasis on math and science, as crucial in an era of increased global competition.

Math and science are hence seen as important at the macro level. However, little is known about the payoff of studying more math and science at the individual level. In many industrialized countries, policy makers are indeed worried about the perceived decline in interest in these subjects among youth, and the development of the performance of students in math and science in international rankings is monitored closely, and often set the agenda for the public debate around education.

Descriptive statistics from clearly shows that students taking more high school math and science earn more and are more likely to be employed.[2] However, this is likely to reflect differences – observed and unobserved – between students as well as causal effects of the education per se. In this paper, we test for the causal effects of taking more math and science, defined as studying at the science or engineering tracks, in (Swedish) secondary school, on future labor market outcomes, measured by employment, wages and earnings at age 30. In order to identify causal effects, we make use of a discontinuity in the admittance process. Our empirical analysis draws on a (raw) data set covering the universe of applications to Swedish upper secondary education during 1985-92. This rich data set allows us to compare students who have the same preferences for types of tracks, but who were admitted either to the science/engineering tracks, or to another academic track (with significantly less focus

[1] See also e.g. Hanushek and Woessmann (2008),

[2] See Table 2 for descriptive statistics for Sweden, or Rose and Betts (2004), Levine and Zimmerman (1995), and Altonji (1995) for evidence on the US.

on math and science), due to marginal differences in compulsory school grades (the selection criteria).

There is a handful previous papers on the effects of high school tracks on individual future outcomes (see e.g. Rose and Betts (2004), Levine and Zimmerman (1995), and Altonji (1995)). There are however few papers that convincingly identify causal effects. The main exceptions are two recent papers: Schrøter Joenssen and Skyt Nielsen (2009) and Goodman (2009). The former, Schrøter Joenssen and Skyt Nielsen (2009), uses a Danish high school pilot scheme to test for a causal effect of taking more advanced high school math on future earnings. Under the pilot scheme, students were no longer required to combine advanced math with advanced physics, but could also choose to combine advanced math with chemistry. This increased flexibility decreased the cost of choosing advanced math (in particular so since advanced physics was considered an especially though course), and as a result the share of students taking advanced math increased among the students that were exposed to the pilot scheme. The results show that the students taking more advanced math, as a result of the pilot scheme, increased their earnings with roughly 20 percent (compared to an average high school student without advanced math).

The latter, Goodman (2009) tests for the effects of compulsory math coursework in US high schools on earnings measured at age 24-36. The paper uses a difference-in-difference approach, exploiting the differential timing of state-level increases in high school graduation requirements. The source of the variation stems from a recommendation to the states, that was made in 1983 by the National Commission of Excellence in Education, to increase the minimum number of years required for high school graduation in various subjects, among them math. This recommendation was followed by most states; however the timing of the implementation differs between states. Goodman uses the year-by-state variation in the reforms to identify the effects of math coursework on earnings. He finds that each additional year of math increased earnings of black students with 5–9 percent. Estimates for white students are similar but less precise.

Our study complements the studies by Schrøter Joenssen and Skyt Nielsen (2009) and Goodman (1009), in several dimensions: First, we study the effect of taking a lot of, and more advanced, math and science (i.e. attending the science or engineering track) compared to (very) little, and less advanced, math and science (attending the social science, humanities or business track). Schrøter Joenssen and Skyt Nielsen (2009) on the other hand measure the effects of advanced math compared to intermediate math[3], whereas Goodman measures the effects of increasing the required level of basic math.

Second, we measure the impact of more math and science on the individuals who are at the margin of being admitted to a specific high school track. Our results are hence relevant for policies that shift the marginal individuals between different high school tracks by changing the supply of slots. One additional difference is that the group that was affected by the Danish pilot scheme probably had quite a high interest in math, while the individuals in our study are likely to be more generalists in terms of interests, since they as first and second option have types of high school tracks that differ markedly in content. The group affect in the study by Goodman, finally, are those who are probably not very interested in math, and who only take the minimum required courses.

Our study is also related to the large literature on the effects of tracking. In particular to the study by Hall (2009) who found that the Swedish change to a longer and more academically oriented vocational school system in the early 1990s lead to more drop-outs and no effects on transitions to higher education. See also Brunnello et al (2004), Brunello and Checchi (2007), Galindo-Rueda and Vignoles (2005), Kerckhoff et al (1996), Hanushek and Wössman (2006) for studies of the effects of comprehensive school systems. In addition, our empirical strategy is very closely related to Öckert (2009), who construct instruments for years of college based on college admissions discontinuities using Swedish data. The results in Öckert (2009) indicate that the effects of years of college attendance are small.

The paper is structured as follows: In section 2 we present the institutional background, section 3 present the data and empirical methods, section 4 shows our results and section 5 concludes.

[3] The individuals that were “treated” by the Danish pilot scheme are those who would otherwise have taken intermediate math in combination with either advanced social science, advanced biology or advanced music courses.

2 Background: Swedish upper secondary schools

2.1 The Swedish school system

Swedish children start school in the fall of their 7th year and then participate in 9 years of compulsory schooling and graduate at age 16. Up to this point, the system is fully comprehensive; the voluntary component of the studies was very limited. After the end of compulsory school, students can choose to apply for upper secondary education.[4] During the period we study here (1985-1992) the central government had the financial responsibility for both compulsory and upper secondary education and the curricula was determined centrally by the National Board of Education. However, the local municipalities were responsible for the provision of schooling at both compulsory and secondary level.[5] The supply of slots at the various tracks was determined by the county school boards, but the allocation of these slots over schools within multiple school municipalities was decided by the municipalities. During the period, school choice was very limited. However, choices between educational tracks (see more below) were well-defined and slots were often rationed.[6]

2.2 Educational tracks

Swedish upper secondary education in the studied period (1985-1992) was divided into 26 different tracks (see Appendix A). The tracks came in two broad categories, vocational and academic, each catering for about half of the student body.

This study focuses on the 3-year long academic tracks; namely the science; engineering; social science; humanities and business tracks. These tracks made up for X percent of all students. The curricula of these tracks are briefly summarized in table 1. As can be seen in the table, the 3-year academic tracks provided an average of 32 hours of classroom time per week.[7] The table also shows that there is a large difference in the

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[4] In practice, the vast majority attend upper secondary education. According to Statistics Sweden (www.scb.se), 92 percent of all 16-year olds in Sweden attended upper secondary education in the fall of 1993.
[5] A few educational tracks such as nursing tracks were run by the counties.
[6] In 1991, a decentralization reform transferred a significant amount of decision power and responsibility for schooling to the local levels see Björklund (et al, 2004) but this paper focus on the pre-decentralization period.
[7] All numbers refer to averages over the duration of the track.

amount of math and science taught in the science and engineering tracks, compared with the social science, humanities and business tracks. The engineering (science) track had on average 18 (13) hours of training in math, science or engineering whereas the social science, business and humanities had 4–6 hours per week on average. The difference does not merely reflect that the science and engineering tracks had more time devoted to these subjects, but also that courses were more advanced. Tracks with less training in math and science had more time devoted to arts, foreign languages, psychology/philosophy, history and social sciences. Participants in the engineering program were given the option of taking a fourth year with only technical studies.

Choosing educational track for Swedish upper secondary school meant making a significant choice not only in terms of curricula but also due to formal future barriers. Even within the broader sets of academic tracks there were barriers to certain *types* of higher education since only the graduates from science and engineering tracks acquired the necessary requirements for direct transitions into higher education in science or engineering. Only the science track provided access to all types of higher education.

Table 1 Description of curriculum by track.

Track	*Total hours/ week*	*Fraction of hours devoted to:*			
		Math, science, technical	Social sciences	Arts or languages	Occupation-specific studies
Academic 3-year	*32*	*0.288*	*0.206*	*0.338*	*0.053*
Business	32	0.125	0.156	0.313	0.266
Social studies	32	0.203	0.297	0.391	0.000
Humanities	32	0.141	0.297	0.453	0.000
Science	32	0.406	0.172	0.313	0.000
Engineering	32	0.563	0.109	0.219	0.000

Note: Numbers are averages over the duration of the tracks (3 years) calculated from the official curricula. Arts and languages include studies in Swedish. The voluntary 4th year for the engineering track has not been considered (curricula is 100% technical). Voluntary courses is a source on imprecision, in particular for business and humanities studies but do not primarily affect the first column. The omitted category primarily consists of physical education. Averages by type of program (in italics) are unweighted means over the programs.

2.3 Admission rules

When choosing educational tracks, the students listed their preferred choices from 1 to 6 (at most) and submitted their applications to the municipalities. If there were more applicants than slots, admission was determined by the final grades from compulsory school.[8] In general, admission to a secondary school track was determined at the municipality level; that is, the students applying to a certain track were competing for the slots available in their home municipality.[9] In the admission process, the students were first allocated to educational tracks, based on their preference listings and on their GPA. Students admitted to each track were then allocated to schools in the municipality that offered the respective track, according to the proximity principle (geographically and transportation wise). Some municipalities did not have a secondary school in the municipality. Students residing in these municipalities would instead apply to the school in some adjacent municipality, with which the home municipality had an agreement, and the receiving municipality was financially compensated for this.

The admission criterion, the grade point average (GPA), was calculated as the raw average over grades in each subject given on a discrete 1-5 scale with an approximate national mean of 3 and standard deviation of 1. Additional 0.2 credits could be allocated to applicants of the under-represented gender for educational tracks with a very uneven gender-distribution.[10] In the analysis, we use the effective entry credits, i.e. the final grades plus any additional credits. For simplicity, we will however use “final grades” to denote the entry credit measure.

In addition to these general rules, the schools were allowed to exempt a small number of slots from the grade admission criterion. These slots could be allocated either to students in “special circumstances”, or to students whose compulsory school grades were not directly comparable to the other student’s grades, such as students from foreign school systems or other special schools such as the (rare) Waldorf schools.

[8] See Gymnasieförordning 1992:394 and Gymnasieförordningen 1987-743. For some educational tracks, there are additional entry tests, such as practical exams in for example music or theatre. These types of tracks are not included in the empirical analysis.

[9] The exception is Stockholm county, where all but two had a cooperation agreement, with a common admission region. The municipalities in the agreement had a common admission secretariat, and admittance to a track was made based on the pooled number of slots in these municipalities. Students admitted to a track were allocated to the nearest school (which could be in another municipality’s school if all slots in the home municipality filled by students residing nearer.)

[10] The school could also give additional credits for repeated applications to a certain track, for example if the applicant had acquired relevant empirical skills but we only consider direct transitions between Swedish compulsory schools and upper secondary education (i.e. applicants aged 16) in the analysis.

In table A1 in the appendix we show the number of applicants per slot by track. Note that the table displays the national average so that there is likely to be cases with competition for slots even for tracks where there are fewer applications than slots.

3 Data and empirical specification

Our main data source is a population wide register of admissions procedures to upper secondary education. We combine these data with a number of outcome measures such as i) data on annual earnings, wages and employment, ii) annual data on educational attainment originally drawn from graduation records of all upper secondary schools and all providers of adult education and various forms of tertiary education in Sweden, iii) data on registration in tertiary education, and iv) scores from military conscription tests for males. In addition to the outcomes we use data on the demographic characteristics of the students and their parents (identified from a population wide multi-generational register).

3.1 Definition of the sample

Our population of interest is made up by students who apply to upper secondary education in the years 1985 to 1992. We only include applicants during the year they turn 16. Thus we neither include cases of repeated or delayed applications nor students who are younger than their classmates.

Our empirical strategy strives to compare students on both sides of admission threshold values. The data include information on first choice of track, second choice of track etcetera as well as information on the track to which the applicant was admitted, and on the track the student graduated from. We define a threshold value for each combination of school, application year, and preferred track.[11] We refer to this as an application-round. In practice we look at all admitted students and infer the threshold

[11] Thresholds are defined at the level of the application code (which determines admission) which sometimes is more precise than the tracks as described above.

value from the lowest observed grade among all admitted students at a particular application-round.[12]

This should give us the correct admission threshold in the great majority of cases. There is one special case for which there is a risk that we, in some cases, miss-specify the admission threshold; namely for students residing in municipalities with no secondary school. In some cases, these students could have the option of applying to different municipalities. If so, our procedure will miss-specify the admission threshold for the subset of students that applied to the first option in one municipality but the second option in another municipality *and* who were admitted to their second most preferred option. (This follows from the fact that we cannot observe the school code, and hence neither the admission secretariat, for the first listed option for these students.) This is however a very special case, and the number of such observations in the data should be low. However, in order to make sure that this does not influence the results, will also run the regression without students residing in a municipality with no secondary school.

An important prerequisite for the empirical analysis to be valid is that admission to the educational tracks was fully determined by the final grades. We test whether the threshold levels are binding by computing the number of cases where a person with a higher final grade than the threshold value was nevertheless *not* admitted to the preferred track. We find that the criterion holds well in our data; the GPA of those that were admitted to their second preferred track is in 90 percent of the cases lower than or equal to the GPA of the last admitted student to their most preferred track but not for tracks with lower threshold values.

In our working data we include individuals who are either marginally admitted to their first choice, or marginally rejected from this choice. In order to identify the effects of interest we can only use those on the margin between different types of tracks (defined below). In practice we include students that are less than 1.0 above or below

[12] We drop application-rounds where the grade distance from the threshold to the 3rd lowest grade among admitted students is larger than 0.1 to remove cases where the threshold is defined from anomalous observations. In addition we only include cases where the threshold is at grade 2.0 or above. In practice, for the cases where there was competition for the slots, the admission threshold was almost always higher than 2.0, and so this only eliminates a few observations.

the threshold in our baseline analysis (results for more narrow data windows are given in the appendix). This means that we remove students with a GPA-distance from the threshold corresponding to approximately a standard deviation of GPA. We only include those actually admitted to their second choice.

3.2 Labor market outcome measures

In order to measure the effects on labor market outcomes, we look at the log of annual earnings (deflated to 2005 SEK); the log of monthly full-time equivalent wages (deflated to 2005 SEK); and a dummy variable for whether the individual is employed or not (as measured in November). All of these variables are measured at age 30. Wages differ from the other outcomes in that they are only measured for a sample (drawn in September or October). The sample covers all public employees and about half the private sector employees, with a large skewness in favor of large-firm employees.[13]

3.3 Intermediate outcome measures

In order to better understand the process at hand we analyse a number of intermediate outcomes. We define *drop-outs* as students who have not yet completed an upper secondary education 4 years after admittance. For those not dropping out, we measure final high school GPA. Note though that the courses underlying these grades will be track specific so this analysis will be very tentative. We further look at both transitions into tertiary education (starting), and the probability of completing a tertiary education at age 30. During the period under study, virtually all 18-year old males were drafted to the military service[14], and we therefore have information on draft test scores for nearly all males. The test consists of four subtests, measuring the individuals' logical, verbal, spatial and technical ability. We analyze standardized measures (percentile ranks) of the overall scores.

[13] Sampling probabilities depend on firm size. Firms with less than 10 employees are only sampled with a probability of 3 percent, whereas firms with 500 employees or more are sampled with a probability of one.
[14] Mentally and physically handicapped were exempted. Refusing to participate in the draft could result in prison for up to one year (see Chapter 10, Lag om totalförsvaret, 1994:1809).

3.4 Empirical specification

In principle, if we knew all the factors that determined the individual's choice of track, we could obtain the causal effects of type of track by controlling for all these factors in the model. However, selection to educational track is likely to be affected by unobservable factors, and in order to obtain causal effects, we use the admissions discontinuities. This section describes our approach in detail.

For the sake of comparison, we start by running a simple OLS-model on our stacked panel of students gaining admission to secondary school during 1985–1992, without taking into account selection on unobservables. The model, which is given in equation (1) estimates the correlation between being admitted to the science or engineering track, with the reference category defined as being admitted to the social science, humanities or business tracks. The model includes a set of individual background covariates that are typically correlated with labor market outcomes; namely parental education level, foreign parents, month of birth and gender.

$$y_i = \psi_t + \beta M_i + f\left(G_i\right) + X_i\lambda + \varepsilon_i \qquad (1)$$

M_i denotes a binary indicator of whether individual *i* was admitted to the science or engineering track, with the reference group defined as being admitted to the social science, humanities or business tracks. y_i denotes the outcome variable (earnings, employment etc.), $f\left(G_i\right)$ is a flexible function of the grades from compulsory school (the selection criterion for secondary education tracks), X_i is a set of individual covariates, and ε_i is the data set. The model is estimated using year specific intercepts, ψ_t.

Controlling for a set of covariates in this fashion will mitigate the problem of selection on observables. However, it will not be enough to identify causal effects if there is also selection on unobservable factors. In order to estimate the overall causal effect of the science and engineering tracks (compared to our reference tracks) we therefore estimate a regression discontinuity model based on multiple discontinuities, where each admittance threshold is a discontinuity. This identification strategy relies on the assumption that around the threshold, whether an individual was admitted or not, is as good as random.

Based on pooling the information from all such experiments, we measure the effect of being admitted to the science/engineering tracks. As previously mentioned, we limit the sample so that the social science/humanities/business tracks constitute the counterfactual. This means that each application round will contain individuals with either the combination science/engineering as first option and social science/humanities/business as second option, or the other way around.

The estimation equation for the data set of stacked application rounds is the following:

$$y_{ij} = \alpha_j + \beta M_{ij} + f\left(\tilde{G}_{ij}\right) + X_i \lambda + \varepsilon_{ij} \qquad (2)$$

In equation (2), M_{ij} denotes a binary indicator which is one if individual *i*, in application round *j*, was admitted to the science or engineering track.[15] $f\left(\tilde{G}_{ij}\right)$ is a flexible function of the compulsory school grades, measured as the deviation from the application round-specific admittance threshold. In the baseline estimation, we will include a fourth order polynomial.[16]

The above RD-OLS will yield the causal effects of being *admitted to* the science or engineering tracks. However, it is likely that some students that were admitted to one type of track will later succeed in changing to another type of track.

In order to identify the effects of *attending* the science/engineering tracks, we estimate an RD-model where we use the information on which type of track the student was admitted to, as an instrument for the type of track the student actually attended, measured as the track the student graduated from. This amounts to running the following 2SLS-equation system:

$$y_{ij} = \alpha_j + \beta \hat{M}_{ij} + f\left(\tilde{G}_{ij}\right) + X_i \lambda + \varepsilon_{ij} \qquad (3)$$

$$M_{ij} = \phi_j + \delta MA_{ij} + f\left(\tilde{G}_{ij}\right) + X_i \gamma + \upsilon_{ij} \quad (4)$$

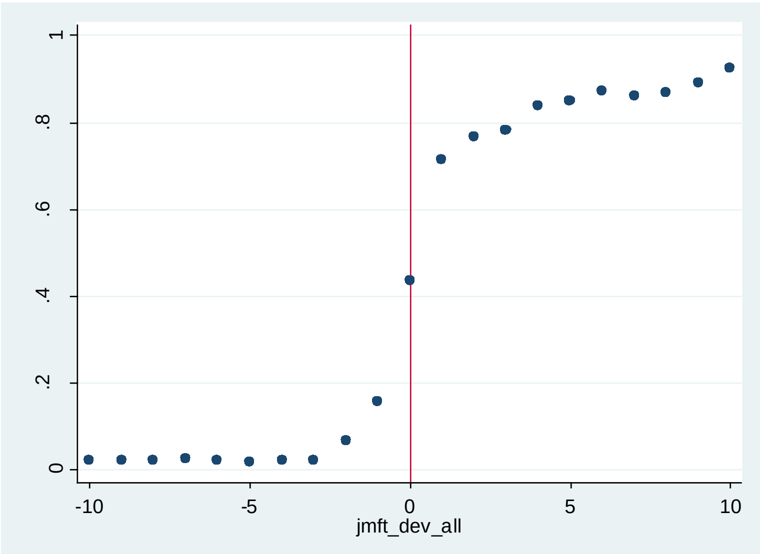
[15] As was previously described, each threshold value for each combination of school, application year, and preferred track generates application round. We assume that the effect of admitted to the science or engineering track is symmetric, i.e. the effect is independent of whether the individual obtained his/her first option or not.

[16] We will also, as a robustness test, allow the slopes of the trends to vary with respect to the type of track attended and whether the student was admitted to the most preferred track or not.

In equation (4), MA_{ij} equals one if individual *i* in application round *j* was admitted to the science or the engineering track, and equals zero otherwise. Furthermore, both equations include application round fixed effects, as well as flexible compulsory grade trends (in the baseline estimation, we will allow the trends to differ between years and between type of track attended, and we will include a fourth order polynomial), and a set of individual level covariates.

For the instrumental variable approach to work, the first prerequisite is naturally that being admitted to a type of track significantly increased the probability of attending that type of track. Figure 1 indicates that this is indeed the case.[17]

Figure 1 Probability of graduating from the science or engineering track and deviation of GPA from admission threshold.



3.5 Description

Table 2 shows the outcomes by type of high school track for all admitted students in the tracks studied. As the table shows, all outcomes except dropout rates speaks in favor of

[17] The fact that the probability of graduating from the science or engineering track is a lot lower for observations just at the threshold compared to observations just above the threshold probably mirrors the fact that we only observe the GPA with one decimal precision. This means that the observations just at the threshold in Figure 1, are probably a mix of students just above and just below the threshold, if measured more precisely.

much better outcomes or the math oriented students. The differences in military draft scores depending is an extreme case with enormous differences: male students of science and engineering rank almost 20 percentiles higher than students of social science, humanities and business. The differences in the mean high school grade point averages are somewhat smaller; however this measure is specific to the subjects taught in the respective track, which makes cross-track comparison harder. The dropout rate is a bit lower among science/engineering students, but these students also have a higher probability of continuing to tertiary education. Finally, employment, earnings and wages, measured at age 30 are higher among students from the science and engineering tracks, especially annual earnings, which are on average 30 percent higher among science/engineering students.

Table 2 Description of outcomes by track for all admitted

	Draft test score	*GPA*	*Drop out*	*Completing tertiary*	*Employment*	*Annual Earnings*	*Wage*
Science and Engineering	70.16	58.08	0.06	0.67	0.89	254,116	27,002
Social Studies, Business and Humanities	51.71	53.42	0.06	0.52	0.85	194,642	25,026
Science	73.40	67.9	0.05	0.75	0.87	234,033	26,989
Engineering	68.99	52.23	0.06	0.63	0.90	265,826	27,011
Social studies	53.42	57.28	0.06	0.64	0.85	187,212	24,974
Humanities	52.73	55.83	0.08	0.54	0.79	154,207	24,141
Business	50.75	50.01	0.05	0.44	0.87	211,231	25,313

Note: Test scores are measured during the 3d year after admission, drop out rates and GPA are from high school. Both Draft test scores and GPA are measured in percentiles. Employment and earnings are measured at age 30. Earnings are in 1000 SEK:s deflated to 2005 value (exchange rate was 5.92 SEK/USD).

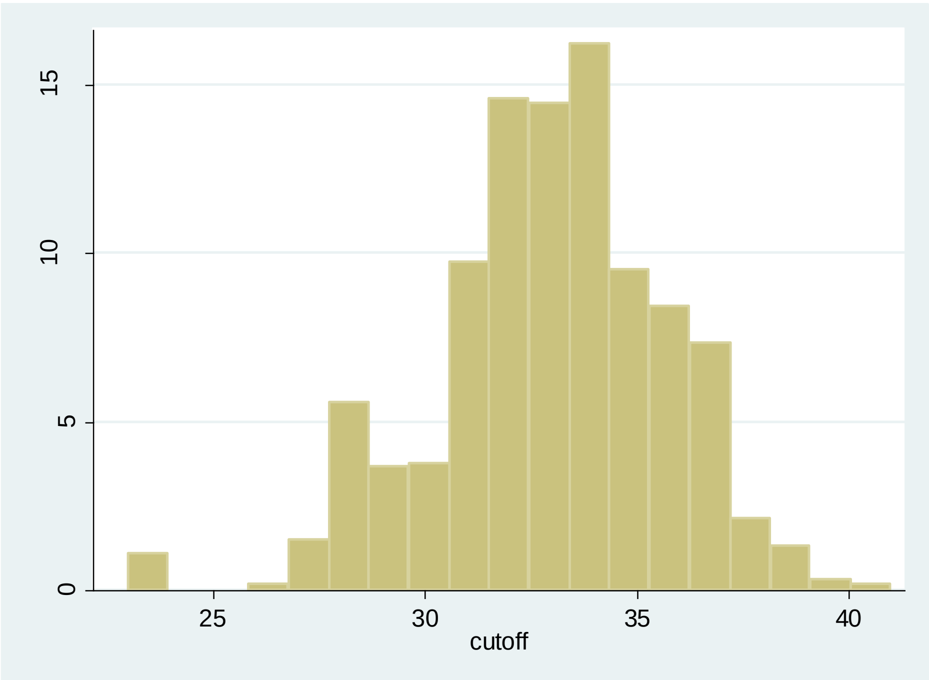
The differences between tracks shown in Table 2 are (likely) a mix of selection and potential causal effects. To sort out the causal part of these differences we focus on marginal individuals who fall just above or just below the admission threshold for their preferred choice. In Table 3, we show descriptive statistics for the sample of marginal individuals. The table shows that the same number of students have a first preference for the science/engineering tracks as for the social studies/humanities/business tracks. The table also show that business studies is by far the most common option among marginal students with a non-science/engineering track as first option. Only a handful of marginal students have the humanities track as most preferred option which perhaps is expected. In the sample of students with science/engineering as first preference, engineering is the more common preferred option. We also see that most of the students, on average 89 percent, are above the threshold (i.e. are admitted to their preferred option).

Table 3 Description of the RD sample

	Preferred choice		*Fallback*	
	N	*Admitted*	*N*	*Admitted*
Social Science, Business, Humanities	4959	0.89	5244	0.10
Science and Engineering	5244	0.90	4959	0.11
Social Studies	1467	0.88	2021	0.10
Business	3458	0.89	2950	0.10
Humanities	34	0.82	273	0.11
Science	1985	0.90	2558	0.10
Engineering	3259	0.90	2401	0.12
All	10203	0.89	10203	0.11

Figure 2 shows the distribution of the admittance thresholds faced by the students in the RD-sample. As can be seen in the figure, the thresholds are fairly high. The results are hence not estimated for students in the very lower end of the GPA-distribution, but rather for students in the middle of even high end of the distribution.

Figure 2 Distribution of admittance threshold all students in RD-sample



3.6 Assessing the RD setup

Before turning to the main empirical analysis we estimate the effects of the model on predetermined variables to see whether our RD-configuration captures selection effects as intended. Conditional on our model, being admitted should not be related to other important characteristics of the students if our identifying assumptions hold. To verify this we have estimated the effects on four strong predictors of student outcomes: mother's education, father's education, parent's immigration status, month of birth, and a dummy for being male. The results presented in Table 4 below show that the only significant difference we find is that students the science/engineering tracks more often are female. However, in order to address potential unbalances in background characteristics we will also include these characteristics as control variables in the empirical models.

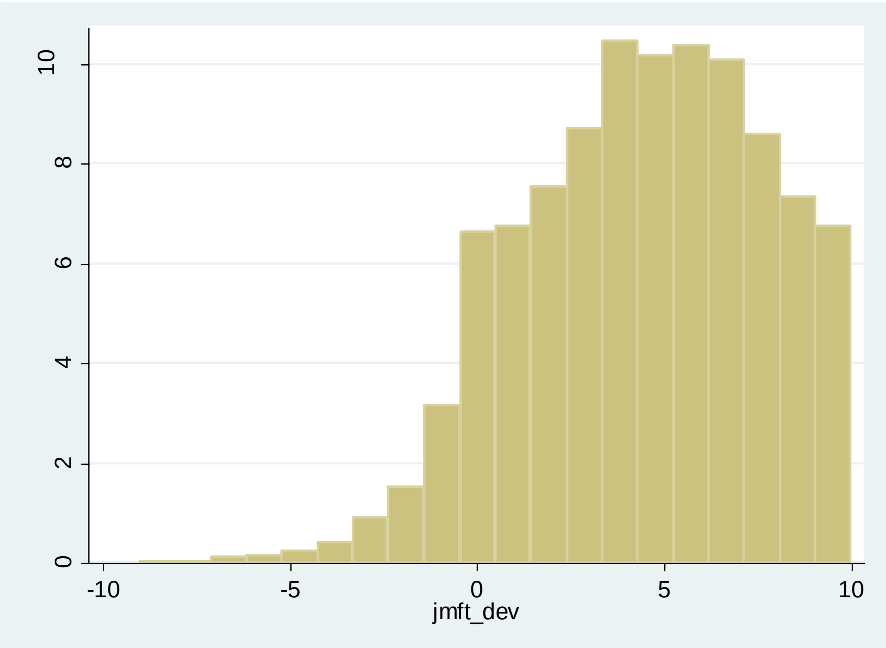
Table 4 Estimated track effects on predetermined outcomes RD-OLS

VARIABLES	Mother secondary school only	Father secondary school only	Mother college education	Father college education	Month of birth	Father/mother born abroad	Male
Science/Engineering	0.00806	0.00606	0.0216	0.00299	-0.0889	-0.00656	-0.0747***
	(0.0166)	(0.0174)	(0.0182)	(0.0189)	(0.125)	(0.0122)	(0.0171)
Group fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Nr polyn trend GPA	4	4	4	4	4	4	4
Observations	9286	9080	9286	9080	9525	9526	9525
Number of groups	580	580	580	580	580	580	580

Note: Admission round specific fixed effects and GPA-trends, as defined in the text, are included in all regressions. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Another way to assess whether the RD-approach holds is to examine the distribution of the selection criterion, i.e. the GPA of the students. The idea is that if admission is indeed random around the admission threshold, we should find no jumps in the distribution of observations at the threshold. Figure 3 shows the distribution of the deviation of the GPA from the admission cutoff for all students in the sample. As can be seen in the Figure, there is some evidence of a jump in the distribution at the threshold. This may reflect that, even though the precise admittance threshold could not be known in advance, students would in many cases be likely to have an idea about approximately which GPA would be required for entering a certain track. This indicates that students at or above the threshold may be more motivated than students below the threshold. This would be a concern if our analysis was based only on students with science/engineering as their first preferred option, who were admitted or not. However, the fact that our analysis is based on students who have either science/engineering or social science/business/humanities as first preference, any such motivation effects will cancel out as long as they are similar for students with the two types of preference orderings. Nevertheless, in order to reduce the risk for bias due to anomalies at the threshold, we will exclude the observations at the threshold from the regression analysis.

Figure 3 Distribution of admittance threshold all students in RD-sample (+-1.0 GDP from admittance threshold)



4 Results

4.1 Labour market effects

We start by estimating the effects of being *admitted to* the science or engineering tracks, on (the log of) annual earnings, (the log of) monthly (full-time-equivalent) wages and employment. The results are given in Table 1. We first estimate the baseline equation

(1) on the full sample of students that were admitted either to the science/engineering track or to the social science/humanities/business tracks. For the sake of comparison, the first section (Raw) of Table 5 shows the raw differences (corresponding to Table 2) between the types of tracks, that is when we control for no observed or unobserved heterogeneity between students. The middle section of the table then shows the results of running equation (1) as we include the full set of students characteristics that were used as outcomes in Table 4, as well as year dummy variables and compulsory school GPA (first to fourth order). The idea is to see how much of the differences in outcomes remain when observed heterogeneity is controlled for. Finally, we show the results from estimating the RD-model in equation (2) using all students within ± 1.0 GPA from the admission threshold.[18]

Table 5 Estimated track effects on labor market outcomes

	Long run outcomes (age 30)		
	Annual earnings	Monthly Wage	Employment
Science/Engineering	0.332***	0.0824***	0.0354***
(Raw)	(0.00417)	(0.00559)	(0.00132)
Science/Engineering	0.0960***	0.0301***	0.0123***
(OLS)	(0.00476)	(0.00578)	(0.00153)
Science/Engineering	0.0514	0.0878***	0.00256
(RD)	(0.0387)	(0.0267)	(0.0142)
Year dummies		All models	
GPA pol. (OLS)		4	
GPA pol. (RD)		4	
N (Raw)	239277	118369	257875
N (OLS)	220311	109649	236502
N (RD)	8025	4093	8579

Note: Admission round specific fixed effects and GPA-trends, as defined in the text, are included in all regressions. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

[18] As can be seen in Tables A2-A6 in the Appendix, similar results are obtained when we use a more narrow data window, as well as when we allow the GPA trends to vary between the types of tracks. In addition, Table A7 shows that the results do not change much when we exclude all students residing in municipalities that have no own secondary school.

As can be seen in Table 5, controlling for observed heterogeneity significantly reduces the correlation between labor market outcome and admission to the science or engineering track. For example, the estimated correlation for log annual earnings falls from 29 percent to 7.5 percent, and the coefficient for log monthly wages goes from 7.6 percent to 2.5 percent. The estimated correlation for employment is also much reduced. Moving to the RD analysis, in order to control for unobserved heterogeneity between students, we see that the effects on log annual earnings and employment are still positive, but no longer significantly different from zero (although the coefficient on log annual earnings is close to significant at the 10 percent level). The coefficient on log full time equivalent wages is however strongly significant, and even larger than in the simple OLS regression. This suggests that there is indeed a causal effect of being admitted to the science or engineering track, and furthermore, for the marginal students, the effect is fairly large, 8.8 percent.

4.2 Intermediate effects

The results presented in Table 5 suggested a positive causal effect of the science and engineering tracks for monthly wages, while the effect on annual earnings was positive but imprecisely estimated. In this section we turn to intermediate outcomes, such as education results and university education, in order to get an idea about the mechanisms behind the estimated effects on labor market outcomes. Table 6 shows the estimated correlations from the Raw and the OLS regressions that were described above, as well as the estimated effects from the RD regression of equation (2), for the following education related outcomes: secondary school GPA (in percentiles); the probability to drop out; and the over all test score from the military draft[19] (in percentiles); the probability of starting university; and of having completed a university degree at age 30.

[19] These scores are only available for males who are Swedish citizens since they are measured as part of the military conscription process.

Table 6 Estimated track effects on education outcomes

Intermediate outcomes					
	GPA	Drop out	Draft test score	Starting tertiary	Completing tertiary (age 30)
Science/Engineering	4.658***	-0.00240***	18.45***	0.182***	0.150***
(Raw)	(0.112)	(0.000910)	(0.133)	(0.00188)	(0.00187)
Science/Engineering	-4.501***	0.0136***	15.24***	0.0988***	0.0873***
(OLS)	(0.101)	(0.00107)	(0.133)	(0.00201)	(0.00207)
Science/Engineering	-2.850***	0.0169	1.565	0.0237	0.00536
(RD)	(1.052)	(0.0123)	(1.240)	(0.0199)	(0.0200)
Year dummies			All models		
GPA pol. (OLS)			4		
GPA pol. (RD)			4		
N (Raw)	255562	268376	120742	270261	270261
N (OLS)	234835	245714	111846	247352	247352
N (RD)	8595	8942	4910	8942	8942

Note: Admission round specific fixed effects and GPA-trends, as defined in the text, are included in all regressions. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

The first two rows of Table 6 show that also after controlling for observed heterogeneity, students admitted to the science or engineering tracks have a significantly higher probability to start and complete university education, and (for the males) have much higher military draft test scores. However, they also have a higher likelihood of dropping out of secondary school, and lower secondary school GPA (although this is naturally based on different course work and is hence not comparable). Turning to the causal effects in the RD section of Table 6, we however see that all estimates, except for the GPA estimates, turn insignificant (although the coefficient for starting university is not far from being marginally significant).

4.3 Starting or completing? RD-OLS vs RD-IV

So far, we have shown the results of being admitted to the science or engineering programs. However, as discussed in section 3.4, not all who are admitted to a track actually complete it, and the results shown in Tables 5 and 6 may hence understate the effect of taking more math and science in secondary school. We therefore turn to the analysis where we use admission to a track as an instrument for graduating from the track. As before, this analysis is limited to students ± 1.0 GPA from the admittance threshold.

Columns 4–6 in Table 7 show the results of the RD-IV model in equations (3) and (4), i.e. where the probability of graduating from the science/engineering tracks is instrumented by whether the student was admitted to the same types of tracks or not. For the sake of comparison, the RD-OLS estimates from Table 5 are reproduced in columns 1-3.

Table 7 Estimated track effects on labour market outcomes RD-OLS and RD-IV

	RD-OLS			RD-IV		
	Annual earnings	Monthly Wage	Employment	Annual earnings	Monthly Wage	Employment
Science/Engineering	0.0514	0.0878***	0.00256	0.105	0.177***	0.00947
	(0.0387)	(0.0267)	(0.0142)	(0.0802)	(0.0614)	(0.0297)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Group fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Nr polyn trend GPA	4	4	4	4	4	4
Observations	8025	4093	8579	7598	3913	8079
Number of groups						

Note: Admission round specific fixed effects and GPA-trends, as defined in the text, are included in all regressions. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

The results show that the effect on monthly wages remain positive and strongly significant in the RD-IV analysis, and the estimated effects suggest a 16 percent wage premium of attending the science or engineering tracks. The effect on annual earnings remains positive and is also fairly similar in size, but is imprecisely estimated and a zero-effect cannot be ruled out. The coefficients on employment are insignificant and close to zero.

Tables 8 and 9 show the corresponding RD-IV and RD-OLS results for the intermediate education outcomes in Table 6. As can be seen in the tables, the results from the IV-RD analysis are essentially similar to the RD-OLS counterparts, with the exception that the effect on starting university is now marginally significant. The effect is sizeable: the RD-IV specification suggests that graduating from the science/engineering tracks is associated with an 8 percentage point increase in the likelihood of starting a university degree.

Table 8 Estimated track effects on educational performance and draft test scores RD OLS and IV

	RD-OLS			RD-IV		
	GPA	Drop out	Draft test scores	GPA	Drop out	Draft test scores
Science/Engineering	-2.850***	0.0169	1.565	-6.707***	-	3.324
	(1.052)	(0.0123)	(1.240)	(2.007)	-	(2.589)
Covariates	Yes	Yes	Yes	Yes	-	Yes
Group fixed effects	Yes	Yes	Yes	Yes	-	Yes
Nr polyn trend GPA	4	4	4	4	-	4
Observations	8595	8942	4910	8415		4659
Number of groups						

Note: Admission round specific fixed effects and GPA-trends, as defined in the text, are included in all regressions. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 9 Estimated track effects on university education RD OLS and IV

	RD-OLS		RD-IV	
	Start university	University degree at 30	Start university	University degree at 30
Science/Engineering	0.0237	0.00536	0.0760*	0.00654
	(0.0199)	(0.0200)	(0.0426)	(0.0445)
Covariates	Yes	Yes	Yes	Yes
Group fixed effects	Yes	Yes	Yes	Yes
Nr polyn trend GPA	4	4	4	4
Observations	8942	8942	8415	8415
Number of groups				

Note: Admission round specific fixed effects and GPA-trends, as defined in the text, are included in all regressions. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

4.4 Heterogenous effects

The analysis above shows the average effects on the group of students. However, it may well be that the effects differ between different groups of students. In this subsection, we therefore look separately at the effects on students with respect first to gender, and second, with respect to the initial aptitude/interest in math and science.

4.4.1 Female and male students

Due to the available data window, the latest point that we can measure long-run labour market outcomes is at age 30. This is far from ideal since many, particularly females, may be at home with small children at this age, which reduces their labor supply. Some may also still be enrolled in higher education. This should have a larger effect on the annual earnings and employment variables, but less impact on the wage regressions, since wages are defined as the full-time equivalent monthly wage. This suggests that it might be interesting to run the regressions separately for females and males. In addition, it is interesting to see if there are heterogeneous effects with respect to gender. Tables 10 and 11 show the results for the labor-market RD-models that were given in Table 7, when estimated separately for females and males. The results indicate a much larger effect of being admitted to and attending the science or engineering track for female than for male students.

Table 10 Estimated track effects on labour market outcomes RD OLS and IV, only females

	RD-OLS			RD-IV		
	Annual earnings	Monthly Wage	Employment	Annual earnings	Monthly Wage	Employment
Science/Engineering	0.134	0.182***	0.0113	0.357**	0.327**	0.0257
	(0.107)	(0.0665)	(0.0354)	(0.169)	(0.134)	(0.0692)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Group fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Nr polyn trend GPA	4	4	4	4	4	4
Observations	3376	1765	3628	3182	1688	3397
Number of groups						

Note: Admission round specific fixed effects and GPA-trends, as defined in the text, are included in all regressions. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 11 Estimated track effects on labour market outcomes RD OLS and IV, only males

	RD-OLS			RD-IV		
	Annual earnings	Monthly Wage	Employment	Annual earnings	Monthly Wage	Employment
Science/Engineering	0.0664	0.0624**	0.00994	0.0836	0.105	0.0218
	(0.0456)	(0.0305)	(0.0177)	(0.0982)	(0.0643)	(0.0364)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Group fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Nr polyn trend GPA	4	4	4	4	4	4
Observations	4649	2328	4951	4416	2225	4682
Number of groups						

Note: Admission round specific fixed effects and GPA-trends, as defined in the text, are included in all regressions. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

4.4.2 Interest in and aptitude for math and science

As stated above, the effects of taking more math and science in secondary school may also differ between students with different aptitude for or interest in these subjects. On the one hand, it is possible that students who are more interested in these subjects, or who are more apt for them, profit more from the training in these subjects, and hance also have a larger payoff from studying them. On the other hand, the marginal payoff of additional training in these subjects may be larger for students with lower initial knowledge.

In order to test for differential effects along this line, we use the final grades in math and science from compulsory school (grade 9) as measures of initial interest in and/or knowledge of math and science, and add the interaction of these variables and whether the student was admitted to the science or engineering tracks, to the baseline RD-regressions for log monthly wages. Table 12 shows the results from adding the interaction with, in column 1), the grade 9 grade in math; in column 2) the 9^{th} grade GPA of science[20]; and, in column 3), the 9^{th} grade GPA of math and science. As the grades for each subjects are only available from year 1988, the number of observations in these regressions is reduced. Nevertheless, the results in Table 12 indicate that it is the students with a lower initial knowledge of or interest in math and science (as measured by the grades from compulsory school), that benefit more from studying at the science/engineering tracks. This is interesting, since it suggests that expanding the math and science training has potential payoffs that are not limited to the more motivated or apt students, but on the contrary that the potential gains are even larger for those less motivated or apt.

[20] With science we mean the four subjects physics, chemistry, biology and technology.

Table 12 Estimated track effects on log monthly wages, interacting effect with 9th grade math and science GPA, RD-OLS

	(1)	(2)	(3)
Science/Engineering	0.131	0.218***	0.198***
	(0.0908)	(0.0649)	(0.0765)
Science/Enginering*GPAMath9	-0.0182		
	(0.0248)		
Science/Engineering*GPASc9		-0.0483***	
		(0.0174)	
Science/Enginering*MathSc9			-0.0376*
			(0.0204)
GPAMath9	0.0277		
	(0.0210)		
GPASc9		0.0263	
		(0.0165)	
MathSc9			0.00631
			(0.0176)
Covariates	Yes	Yes	Yes
Group fixed effects	Yes	Yes	Yes
Nr polyn trend GPA	4	4	4
Observations	1670	2641	

Note: Admission round specific fixed effects and GPA-trends, as defined in the text, are included in all regressions. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

4.5 Peer effects

Our previous analysis has suggested a positive wage premium of attending the science and engineering tracks, compared to the other 3-year academic tracks. Furthermore, we have seen that the effect is stronger for female students, and stronger for students with lower math and science grades from compulsory school. We have argued that the important difference between these types of tracks is the larger focus on math and science in the former type of tracks. However, it is possible that there are other differences between the tracks that may also affect the labour market outcomes of the students. One such potential factor is that students attending the different types of tracks will be surrounded by different types of peers. As was shown in Table 2, students at the science and engineering tracks differ markedly from students at the social science, business and humanities tracks. In order to test if our estimated effects are in fact due to peer effects, we add the mean GDP from grade 9 in the secondary school class the student is admitted to[21], to the RD-OLS baseline regression. As the results in Table 13 shows, the coefficient on the mean GPA is insignificant in most cases, although significant and negative in the regression on employment.

Table 13 Peer effects: Mean 9th grade GPA of classmates, RD-OLS

VARIABLES	Annual earnings	Monthly Wage	Employment	Drop out	Draft test score	Starting tertiary	Completing tertiary
Science/Engineering	0.0358	0.108***	0.0374**	0.0232*	0.651	0.0298	0.00816
	(0.0468)	(0.0406)	(0.0172)	(0.0140)	(1.467)	(0.0241)	(0.0240)
Mean 9th grade GPA in class	0.00912	-0.00990	-0.0177***	-0.00239	0.534	-0.00298	-0.00189
	(0.0145)	(0.0107)	(0.00437)	(0.00370)	(0.444)	(0.00785)	(0.00776)
Group fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Nr polyn trend GPA	4	4	4	4	4	4	4
Observations	8017	4089	8571	8934	4904	8934	8934
Number of groups							

Note: Admission round specific fixed effects and GPA-trends, as defined in the text, are included in all regressions. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

4.6 IT-boom effect?

According to the previous results, taking more science and math in high school does seem to have a positive effect on wages. However, one might wonder whether that is a general result, or whether it is specific for the time period that is being studied. In particular, Sweden experienced an IT-boom in the early 2000. In order to test if the effects that we measure stem only from this period, we add an interaction term for the IT-boom period, 1999-2001, to the baseline specification.[22] As can be seen in Table 14, the point estimate does indicate a larger effect on wages during this period, while the effects on annual earnings and employment are too noisy to detect any differential effect. Importantly, however, the main effect on monthly wages is still significant and positive.

[21] In fact, we do not observe the actual class that the student is attending, but proxy for this by the mean GPA in the track by school by year. In smaller schools, with only one class per track this is equal to the

[22] In addition, Figures A1 and A2 in the appendix shows the results from separate regressions for each outcome years for wages and annual earnings. For wages, the figure indicates a larger effect in year 2000, probably due to the IT-boom, and indicates smaller, but still positive and often significant, effects for the other years. For annual earnings the coefficients are, as before, fairly noisy.

Table 14 Different effect during IT-boom? Interaction effect years 1999-2001. RD-OLS

	RD-OLS		
	Annual earnings	Monthly Wage	Employment
Science/Engineering	0.0589	0.0684***	0.0153
	(0.0519)	(0.0253)	(0.0174)
Science/Engineering*d9901	-0.0223	0.0568	-0.0371
	(0.0754)	(0.0648)	(0.0296)
Covariates	Yes	Yes	Yes
Group fixed effects	Yes	Yes	Yes
Nr polyn trend GPA	4	4	4
Observations	8025	4093	8579
Number of groups			

Note: Admission round specific fixed effects and GPA-trends, as defined in the text, are included in all regressions. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

5 Conclusions

We find evidence supporting the notion that there are positive causal effects on future wages from studying with a more math- and science-intense curriculum during high school. The effect appears to be larger for females than for males, and is also larger for students with less lower initial knowledge/motivation for math and science, as measured by the grades from compulsory school. There is also some evidence that the math and science oriented tracks also increase transitions to higher education which could be one important mechanism.

However, we find that the huge raw difference in draft test scores between math oriented students and other students is entirely due to selection on unobservables. Our estimates indicate that marginal students with an interest in both math and social science perform equally well on these tests regardless of what they are taught during high school.

Overall the results confirm the findings in Schrøter Joenssen and Skyt Nielsen (2009), and Goodman (2009), of a positive wage premium of taking more math and science in secondary school.

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Appendix A

Table A1 Applicants per slot, by track and year.

	1986	*1987*	*1988*	*1989*	*1990*	*1991*	*1992*
Academic 3-year							
Business	1.17	1.15	1.14	1.12	1.14	1.08	1.13
Social studies	1.06	1.06	1.1	1.12	1.12	1.11	1.13
Humanities	1.06	1.03	1	1.02	1.03	1.07	1.12
Science	1.01	1.01	1.01	1.02	1.03	1.04	1.07
Engineering	1.06	1.11	1.08	1.05	1.06	1.04	1.09

Table A2 Estimated track effects on labour market outcomes RD-OLS and RD-IV, ± 0.5 GPA from the admittance threshold

	RD-OLS			RD-IV		
	Annual earnings	Monthly Wage	Employment	Annual earnings	Monthly Wage	Employment
Science/Engineering	0.0823*	0.0829***	0.00128	0.175*	0.171***	0.0155
	(0.0428)	(0.0280)	(0.0149)	(0.0942)	(0.0620)	(0.0334)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Group fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Nr polyn trend GPA	4	4	4	4	4	4
Observations	4191	2060	4502	3927	1955	4188
Number of groups						

Note: Admission round specific fixed effects and GPA-trends, as defined in the text, are included in all regressions. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A3 Estimated track effects on educational performance and draft test scores RD OLS and IV, ± 0.5 GPA from the admittance threshold

	RD-OLS			RD-IV		
	GPA	Drop out	Draft test scores	GPA	Drop out	Draft test scores
Science/Engineering	-2.255**	0.0226	1.372	-5.534**	-	2.392
	(1.094)	(0.0139)	(1.387)	(2.283)	-	(2.933)
Covariates	Yes	Yes	Yes	Yes	-	Yes
Group fixed effects	Yes	Yes	Yes	Yes	-	Yes
Nr polyn trend GPA	4	4	4	4	-	4
Observations	4446	4676	2960	4346		2778
Number of groups						

Note: Admission round specific fixed effects and GPA-trends, as defined in the text, are included in all regressions. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A4 Estimated track effects on university education RD OLS and IV, ± 0.5 GPA from the admittance threshold

	RD-OLS		RD-IV	
	Start university	University degree at 30	Start university	University degree at 30
Science/Engineering	0.0308	0.0118	0.0828*	0.0170
	(0.0224)	(0.0233)	(0.0484)	(0.0507)
Covariates	Yes	Yes	Yes	Yes
Group fixed effects	Yes	Yes	Yes	Yes
Nr polyn trend GPA	4	4	4	4
Observations	4676	4676	4346	4346
Number of groups				

Note: Admission round specific fixed effects and GPA-trends, as defined in the text, are included in all regressions. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A5 RD-OLS ± 1.0, ± 0.5 and ± 0.3 GPA from the admittance threshold

	± 1.0 GPA		± 0.5 GPA		± 0.3 GPA	
	Annual earnings	Monthly Wage	Annual earnings	Monthly Wage	Annual earnings	Monthly Wage
Admission Rule	0.0514	0.0878***	0.0823*	0.0829***	0.0253	0.0680*
	(0.0387)	(0.0267)	(0.0428)	(0.0280)	(0.0543)	(0.0358)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Group fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Nr polyn trend GPA	4	4	4	4	2	2
Observations	8025	4093	4191	2060	2305	1112
Number of groups						

Note: Admission round specific fixed effects and GPA-trends, as defined in the text, are included in all regressions. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A6 Estimated track effects on labour market outcomes RD-OLS and RD-IV, ± 1.0 GPA from the admittance threshold: GPA trend interacted with type of track

	RD-OLS			RD-IV		
	Annual earnings	Monthly Wage	Employment	Annual earnings	Monthly Wage	Employment
Science/Engineering	0.0837	0.119***	-0.00442	0.151	0.246***	0.00668
	(0.0510)	(0.0325)	(0.0179)	(0.113)	(0.0787)	(0.0410)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Group fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Nr polyn trend GPA	4	4	4	4	4	4
Observations	8025	4093	8579	7526	3878	7999
Number of groups						

Note: Admission round specific fixed effects and GPA-trends, as defined in the text, are included in all regressions. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A7 Estimated track effects on labour market outcomes RD-OLS and RD-IV, excluding students with no secondary school in home municipality.

	RD-OLS			RD-IV		
	Annual earnings	Monthly Wage	Employment	Annual earnings	Monthly Wage	Employment
Science/Engineering	0.0662	0.0584**	0.00999	0.142	0.0993*	0.0304
	(0.0427)	(0.0262)	(0.0157)	(0.0873)	(0.0581)	(0.0326)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Group fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Nr polyn trend GPA	4	4	4	4	4	4
Observations	6637	3371	7108	6285	3222	6691
Number of groups						

Note: Admission round specific fixed effects and GPA-trends, as defined in the text, are included in all regressions. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A8 Estimated track effects on labour market outcomes RD-OLS and RD-IV, excluding the humanities track

	RD-OLS			RD-IV		
	Annual earnings	Monthly Wage	Employment	Annual earnings	Monthly Wage	Employment
Science/Engineering	0.0649*	0.0868***	0.00405	0.0772	0.155**	0.00303
	(0.0393)	(0.0273)	(0.0145)	(0.0785)	(0.0616)	(0.0290)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Group fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Nr polyn trend GPA	4	4	4	4	4	4
Observations	7691	3925	8225	7213	3719	7674
Number of groups						

Note: Admission round specific fixed effects and GPA-trends, as defined in the text, are included in all regressions. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A9 Estimated track effects on educational performance and draft test scores RD OLS and IV, excluding the humanities track

	RD-OLS			RD-IV		
	GPA	Drop out	Draft test scores	GPA	Drop out	Draft test scores
Science/Engineering	-2.782	0.0175	1.382	-8.015***	-	3.001
	(1.082)	(0.0127)	(1.270)	(1.917)	-	(2.595)
Covariates	Yes	Yes	Yes	Yes	-	Yes
Group fixed effects	Yes	Yes	Yes	Yes	-	Yes
Nr polyn trend GPA	4	4	4	4	-	4
Observations	8248	8572	4769	7996	-	4497
Number of groups						

Note: Admission round specific fixed effects and GPA-trends, as defined in the text, are included in all regressions. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A10 Estimated track effects on university education RD OLS and IV, excluding the humanities track

	RD-OLS		RD-IV	
	Start university	University degree at 30	Start university	University degree at 30
Science/Engineering	0.0299	0.00425	0.0955**	0.0155
	(0.0205)	(0.0207)	(0.0420)	(0.0439)
Covariates	Yes	Yes	Yes	Yes
Group fixed effects	Yes	Yes	Yes	Yes
Nr polyn trend GPA	4	4	4	4
Observations	8572	8572	7996	7996
Number of groups				

Note: Admission round specific fixed effects and GPA-trends, as defined in the text, are included in all regressions. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Figure A1 Coefficients and 95% confidence interval from separate regressions for each outcome year, Log monthly wage at age 30, RD-OLS, +-1.0 GDP

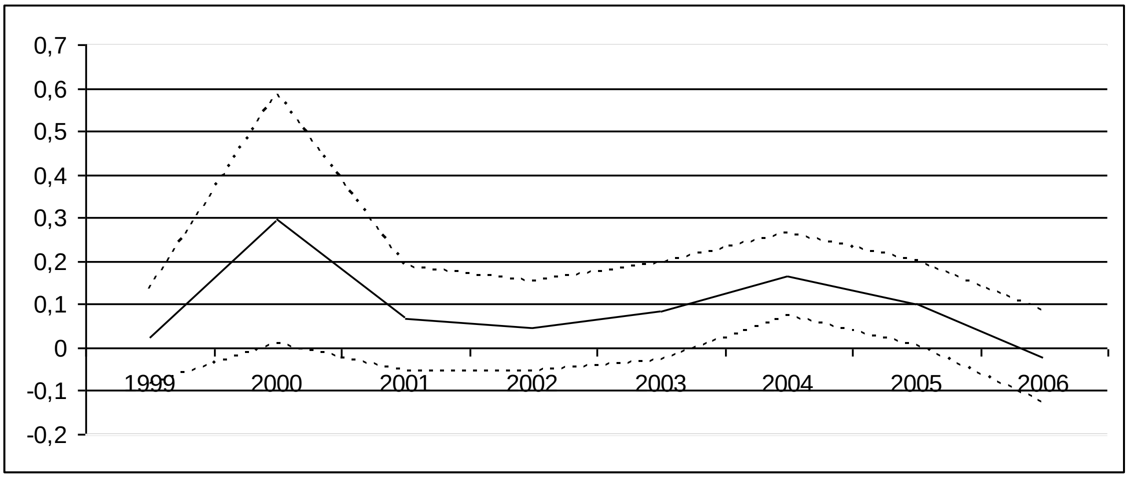

Figure A2 Coefficients and 95% confidence interval from separate regressions for each outcome year, Log annual income at age 30, RD-OLS, +-1.0 GDP