Swedish Institute for Social Research (SOFI)

Stockholm University

WORKING PAPER 2/2006

DETRACKING SWEDISH SECONDARY SCHOOLS - ANY LOSERS, ANY WINNERS?

by

Krister Sund

Detracking Swedish secondary schools – any losers, any winners?*

Krister Sund‡
Swedish Institute for Social Research
Stockholm University

April 4, 2006

Abstract
Whether or not to differentiate - or track - students according to ability has been debated over the years. In Sweden, secondary schools that practiced tracking and schools that did not practiced tracking existed simultaneously from 1980 to 1997. This variation in tracking status between schools is used in a differences-in-differences approach. I estimate whether tracking math, or not, in Swedish secondary school had any effect on the probability of having graduated upper-secondary school, but also whether tracking had any consequence for the math grade in upper-secondary school. The results show that when considering the attainment of upper-secondary education and the mean achievement in math, there are no effects of tracking. However, there are effects when estimating the probability of receiving a specific grade, i.e. fail, pass, pass with distinction or pass with special distinction. Tracked students, from families with low-educated parents, are more likely to fail math than similar students in a non-tracked environment.

Keywords: Educational economics, Tracking, Ability grouping

JEL classification: I21

---

* I would especially like to thank Anders Björklund and Mikael Lindahl for valuable comments and suggestions. I would also like to thank seminar participants at the Swedish Institute for Social Research and Jesper Antelius at the Swedish National Agency for Education for comments. Financial support from Jan Wallander's and Tom Hedelius' Research Foundation is greatly acknowledged.
‡ Swedish Institute for Social Research (SOFI), Stockholm University, SE-106 91 Stockholm, SWEDEN.
E-mail: krister.sund@sofi.su.se , phone +46816 23 07

1. Introduction

Whether to differentiate students according to ability has been debated over the years. An argument in favor of differentiating – or tracking as it is called in the US context where most work on the subject has been done – is that a narrower ability distribution within the classroom enables the teacher to better reach all students when teaching. If the teacher adapts the level of instruction to the mean or median student, the students situated at the tails of the ability distribution will suffer, to what extent will be dependent on the spread within the classroom. Proponents of differentiated teaching argue that everyone will benefit from tracking. Students at the top end of the ability distribution can be given tasks that are more challenging, but lower achievers can also benefit from instruction better suited to their ability. Predominant critique against differentiating students is that in a heterogeneous classroom there will be positive peer effects,[1] in that stronger achieving peers can help poorer achievers both directly and as role models. But there is also a notion that grouping according to ability can be stigmatizing. Another argument against tracking is that it redistributes resources such that more resources and better teachers are directed towards high-ability students at the expense of low-ability students (Oakes, 1990). Not much empirical evidence has been produced to support the proponents' view. A large share of the studies shows that differentiating students according to ability benefits higher achievers, at the expense of lower achieving students (see Hoffer, 1992; Kerckhoff, 1986).

In Swedish secondary school, tracking has been practiced for decades in math and English and was introduced with the implementation of the 9-year compulsory school in the early 1960s. In the late 1970s, The Board of Education (*Skolöverstyrelsen*) proposed to remove tracking in Swedish secondary schools, but this proposal received opposition from the teachers' organizations. The result was somewhat of a compromise; with the implementation of the curriculum in 1980 (*Lgr* 80), schools that wanted to continue to track their students could do so and, as it turned out, most schools did. With the implementation of a new curriculum (*Lpo 94*) in the school year 1995, tracking in English and math was finally removed, and thus affected those who graduated secondary school in 1998 and onwards.

In this paper, I use the period between these two policy changes to evaluate if tracking math classes in secondary school had any impact on the attainment of upper-secondary education and the math grade in upper-secondary school. I utilize the fact that both tracking and non-tracking were practiced simultaneously in Swedish schools up until the

[1] Regarding classroom peer effects see, for instance, Hoxby (2000).

implementation of a new curriculum in 1995 when tracking was abandoned. Therefore, I have different tracking regimes in math in schools that otherwise have the same curriculum. Further, I should stress that the schools considered are compulsory comprehensive schools, which if they tracked math and English, only did so in the last three grades (7-9), i.e. secondary school. This fact rigs the setting for a differences-in-differences analysis of tracking. I use the probability of having graduated upper-secondary school and the subject grade of the first math course taken in upper-secondary school as outcome variables. Moreover, I also interact tracking with family background to see whether the impact differs depending on family background.

The results show that there are no effects of tracking when considering the probability of graduating upper-secondary education and the mean achievement in math. However, when estimating the probability of receiving a specific grade, there are effects. The math grade is reported in four levels, fail, pass, pass with distinction and pass with special distinction and the results show that tracked students, from families with parents who have at most 9 years of education, are more likely to fail in math than similar students in a non-tracked environment.

The paper proceeds as follows. Next, there is a section on previous studies followed by a description of tracking in Swedish secondary schools. Then, sections dealing with data and empirical strategy are followed by a sensitivity analysis and conclusions.

2. Previous Studies

In previous research, two different designs have foremost been used. First, comparing the achievement of students in tracked vs. non-tracked schools was the prevalent method in earlier studies. Slavin (1991) is an example of this. From his review of 29 studies, using this approach, he concluded that ability tracking had no or small effects on achievement. A problem with this approach is that when only comparing mean achievement, differential effects might be overlooked. Tracking might affect students differently depending on ability and/or background. This could result in an overall effect of zero when there might actually be opposing effects depending on where in the ability distribution you look. The second approach used is comparisons of students in different tracks with non-tracked students, i.e. comparing students in high, middle or low ability tracks with students in non-tracked schools. The latter design gives more information but may have problems with selection on omitted variables. When comparing non-tracked students with students in a high track and using earlier grades or test scores as controls for initial ability, the estimates may still be plagued by omitted variable bias. It is likely that, on average, students in a high track are more motivated than non-tracked students. This problem is dealt with in various ways.

Using British data, Kerckhoff (1986) compares students in high, middle and low ability classes in tracked secondary schools with students in non-tracked secondary schools. The outcome variables are test scores in math and reading. The study shows that there are differential effects. Students at the low end of the ability distribution are harmed by tracking whereas high achieving students benefit from the same. Even if Kerckhoff uses many controls in the analysis, omitted variables may still be a problem. How comparable are students in a high track with students in a non-tracked setting?

Hoffer (1992) uses longitudinal data and initially compares growth in tests scores between students in middle schools that track and middle schools that do not. He finds no significant effects. Then, he takes the analysis further by considering different parts of the ability distribution. He uses propensity score matching to compare students grouped in high, middle and low classes with students in heterogeneous classes. The result suggests that there are different effects of tracking, dependent on where in the ability distribution the student is situated. There are positive effects of tracking for students at the high end of the ability distribution, whereas there are negative effects for those at the low end.

Argys, Rees and Brewer (1996) use a two-stage Heckman correction model to adjust for selection bias. They find differential effects of tracking in the tenth grade, dependent on the students' ability. Similar to Hoffer and Kerckhoff, they find that detracking schools in America would benefit lower achievers, at the expense of high achieving students.

Betts and Shkolnik (2000), compare achievement growth between tracked and non-tracked students in high school using US longitudinal data. Their outcome variable is a standardized test score. They find no evidence that ability grouping benefits all students. However, when analyzing the effects of tracking for different parts of the ability distribution they find that tracking has differential effects dependent on initial ability. The results show that students are harmed by tracking if they are situated in the middle of the ability distribution, but that high achievers benefit from the same. They find no effects among low achievers.

In a similar approach, Figlio and Page (2002) compare students in tracked and non-tracked schools conditional on an initial test score. This method hinges on the assumption of there being no systematic difference between tracked and non-tracked schools. Unfortunately, this assumption does not seem to hold. Their result suggests that school tracking status and the socioeconomic composition of the student body are correlated. To address this endogeneity, they use an instrumental variables approach that uses two- and three-way interaction between three variables: the number of academic courses required for state graduation, number of schools in the county and the fraction of voters who voted for President Reagan in the 1984 election. From this, they conclude that low achieving students may experience larger test score gains in a tracked than in a non-tracked environment, but the results are far from precise.

A recent contribution is Hanushek and Wößmann (2005). They use cross-national variation to identify tracking effects using PISA test scores and they find that early tracking increases inequality and reduces mean performance. Other studies on tracking are Zimmer (2003) who finds that tracking have negative effects for low- and average-ability students, Linchevski and Kutscher (1998) finds similarly the tracking have negative effects for the average- and low-performing student.

In sum, none of the studies above found any evidence that supports the proponents' view that everyone benefits from tracking independent of initial ability. However, evidence of differential effects seems prevalent and most of the studies found that tracking benefits higher achievers at the expense of lower achieving students.

In Sweden, some work on tracking was done in the 1960s. Svensson (1962) uses a natural experiment, The Stockholm Study (*Stockholmsundersökningen*), which was put together to evaluate different designs of comprehensive school systems. The systems differed in how long students were in a comprehensive school; some student could choose different educational tracks after 4 years whereas others did so after 6 years. Svensson compares the students who were tracked at different ages to evaluate whether there were any differences in achievement, but finds no long-run effects. Dahllöf (1969), who uses the same natural experiment as Svensson, and two other similar studies concluded that in terms of achievement tests, there was not much difference between the two systems, but that this was probably due to compensation in the time spent teaching. Even though these studies are interesting, other factors besides tracking were present that could bias the result, e.g. teachers' education differed systematically between groups.

There was an attempt at evaluating different tracking regimes in Sweden in the 1980s. The results from this project are presented in Hellström (1987). In short, secondary schools in Sweden were asked if they would like to participate in a project where different tracking designs were to be evaluated. Unfortunately, not many schools did participate, thus making the results not suited for any general conclusion of the issue. Still, based on the results from interviews with teachers, students, and analyses of test scores that gave insignificant results, the author suggested that the differentiated grading practices that have been used in Sweden should be abandoned. Another related Swedish survey is Erikson and Johnson (1993) who concludes that postponed introduction of differentiated instruction reduces socially uneven recruitment to higher education.

In my opinion, there are three main problems that most of the studies above must deal with. I will argue that my approach handles these problems in a more satisfactory way due to better data and method. First, identifying the track status of a school or a student is not without problems in the US context. To categorize which, if any, track a student has attended is based on principal, teacher or self-reported assessment of track status and there is evidence suggesting measurement error in this variable (see Rosenbaum, 1980; Rees et al., 2000). I identify tracking from register data by the reported course in which the student has received the math grade in. Schools that practiced tracking report two different math grades – ordinary or advanced – whereas non-tracking schools only report one, general grade. Therefore, the question of whether a student attended a tracked school is revealed from data.

Second, in Figlio and Page (2002) there is a selection problem in schools that practiced tracking. In the Swedish context, institutional factors will lessen that problem. In Sweden, the possibility of choosing school was introduced in 1992; however, residing close to a school remained the main principle for allocating students. Third, when comparing students in different tracks with non-tracked students, omitted variables are likely to bias the estimates. I use a similar approach to that of Figlio and Page when analyzing tracking, in that I compare students in tracked with non-tracked students instead of comparing students in different tracks with non-tracked students, which will reduce the problem with omitted variable bias. I also have the advantage of a differences-in-differences approach.

Further, if there are different effects in the ability distribution, as previous research has shown, the overall effect might be zero. Unfortunately, I do not have any information of initial ability but use family background as a proxy. I construct interaction terms between tracking status and family background in an attempt at unveiling any differential effects.

3. Tracking in the Swedish school system

Throughout the twentieth century, there has been a motion towards a more comprehensive school system in Sweden. The goal of equality of opportunity has motivated this education policy. However, this has not been without opposition. Forces against argued that with a 9-year comprehensive school, too many students would continue to higher education and that there were simply not enough qualified jobs to go around. Thus, a compromise was reached in order to mitigate the opponents. As a result, quite extensive tracking was introduced from the seventh grade with the implementation of the 9-year compulsory school in 1962 (Marklund 1981).[2] However, this tracking was reduced in the late 1960s with the implementation of the curriculum (*Lgr69*), after that tracking was only practiced in math, English, French and German. Courses were made available at an ordinary or advanced level, for the students to choose between. Students self-selected themselves into any of these courses so there was no formal testing, but it might be suspected that teachers and/or parents assisted the students in choosing the suitable course based on their ability. At first, students were required to have graduated secondary school from the advanced levels of mathematics and English to be eligible to more academic course programs in upper-secondary education; however, this requirement was removed at the end of the 1980s (Wallby et al., 2001).

In the preceding work for the new curriculum for the compulsory school in the late 1970s (*Lgr 80*), the differentiation question surfaced once more. The Board of Education suggested that differentiated instruction should be removed. This proposal received strong opposition from the teachers' organization and the result was a compromise. The differentiated courses in French and German were abandoned, but a peculiar situation arouse concerning math and English. In the final document, the differentiation question was handled in a rather vague way. The differentiated courses are only briefly mentioned in the formal document, but they are not even considered in the actual syllabus. Thus, there was no definition of what should be the course content of an advanced or ordinary level math course. However, the courses were defined by the differentiated literature that had been used over the years, so it was more or less business as usual. Altogether, schools were more or less free to choose whether to keep tracking their students. The result was that most schools kept the differentiated instruction in math and English (Hellström, 1987).

In the early 1990s, the Swedish school system underwent a substantial change with the municipalities receiving full financial responsibilities for secondary and upper-secondary education. Thus, the question of differentiation was removed from the national agenda and was reduced to a local matter. Whether to differentiate or not was now up to the principal at each school. Most schools kept the old system with differentiated instructions in math, some schools abandoned the practice of tracking and a few practiced both systems in parallel, with three different courses: ordinary, advanced and a general course. All systems were accepted at

[2] The implementation of the 9-year compulsory school started in 1949 but it was not until 1972 that all schools in Sweden had students graduating compulsory school after nine years.

a central level, which can bee seen since The Board of Education administered centralized test for both regimes (Wallby et al., 2001).

The way the share of students graduating from detracked secondary schools grew in Sweden from 1980 and onwards is displayed in figure 1. It seems like most schools that abandoned tracking prior to 1995 did so when the question was reduced to a local issue. The share of students graduating from detracked schools was below 2 percent during the 1980s, but started to grow around 1993, which coincide in time with the municipalities' financial commitment over secondary and upper-secondary school. About 40 percent of those students that attended a tracked school were enrolled in the ordinary track. Differentiated courses were finally removed from the Swedish school system with the implementation of the curriculum in 1995 (*Lpo94*).

4. Data

The data used are administrative registers supplied by Statistics Sweden and are based on a 20 percent random sample of individuals in each cohort and their siblings in Sweden. From these, I use those who graduated from secondary school between 1994-1998, with 1998 being the first year that all students graduated secondary school without differentiated math courses. Following the somewhat loosely specified course plan in the curriculum, there were some schools that did not practice differentiated teaching before 1995, thus students graduated from detracked schools prior to 1998. I use these as a counterfactual in a differences-in-differences approach.

Secondary school data are supplied from *The Student Register, Year 9* (*Årskurs 9-registret*) and contain information of grades in specific subjects, year of graduation and a school identifier. I only use information of graduation year and the school identifier, which I match with a special dataset to identify whether a specific school tracked its students that year or not. In the data set used to identify tracking, I have information on grades given in math in each secondary school. Tracking schools are identified in that, for a specific year, they give two different grades in math, ordinary and advanced, while non-tracking schools only give a general math grade. I exclude those schools that give grades according to both systems in a specific year.

In the sample used, the share of students graduating from schools that did not practice tracking went from almost 4 percent in 1994 to nearly 10 percent in 1997 (see figure 1). In 1998, tracking was completely removed from secondary school.

A basic question when analyzing tracking is what outcome to use, i.e. what measure to use when comparing tracked vs. non-tracked students. Identical tests in math given at the end of the 9th grade would have been ideal, but that is not available. Moreover, students in ordinary or advanced math were graded in such a way that any comparison between the two was made impossible, because the same grade measures different abilities. I use two different outcomes. The first is the probability of graduating from upper-secondary school or, to be specific, that there is information of a graduation year. This is only available if the student has chosen to obtain a final grade from upper-secondary school, thus, students who choose not to do so, due to e.g. poor grades, will not have a year of graduation. Even if tracking status affects the attainment of upper-secondary education, which is interesting in itself, this is not a very precise measure of how tracking affects math achievement. However, this variable paves the way for the subsequent analysis when considering math achievement by clarifying whether there is any selection into upper-secondary school, dependent on tracking status in secondary school. The second outcome used is math achievement in upper-secondary school. If there is an effect of tracking on the subject grade in math in upper-secondary school, this might have policy implications for how to organize class teaching to maximize student achievement.

A new curriculum was implemented in upper-secondary school in 1994, which would affect those students graduating secondary school and continuing to upper-secondary school in the same year and onwards. This new curriculum supplied an outcome variable making this analysis possible. Prior to this date, tracking within subjects had also been practiced in upper-secondary school whereas in 1994 this was removed. Now, Swedish upper-secondary school consists of subjects and courses such as math A, math B and math C. The courses are taken in a sequence and are evaluated and graded separately and independently of each other. There is still some tracking in upper-secondary school, in that students select different programs. Nevertheless, the initial course taken (course A) in the subjects math, Swedish, and English, is compulsory, independent of program enrolment. All students in upper-secondary school take the math A course, so I use the grade from this course as one of my outcomes.

This is not a perfect measure since the teaching in upper-secondary school will influence the grade, but it is the best at hand. Furthermore, this variable is only available for students who have continued to upper-secondary education, which might reduce the validity of the results for a larger population. However, in 2003, 97.8 percent of the students graduating secondary school did continue to upper-secondary education (Statistics Sweden 2005) so this will not be a problem.

All information regarding upper-secondary school is recorded in *The Register of Upper-Secondary School Leavers* (*Register över avgångna från gymnasieskolan*) and similar to *The Student Register, Year 9*, it contains information of course grades, year of graduation, and school identifier. I use the information of graduation year when estimating the probability of graduating upper-secondary school and the course grade of the first math course taken in upper-secondary school, math A. This grade is reported in four levels, fail, pass, pass with distinction and pass with special distinction.

I have used *The Multi-Generation Register* (*Flergenerationsregistret*) to identify students' parents. This enables me to match information from *The Swedish Register of Education* (*Utbildningsregistret*) of parental education to each student. The parents are foremost biological parents when available; otherwise, I use census data to identify the latest residing parents. In households where information of education is available for both parents, I use the average level of education as my measure; otherwise, if there is a single parent or only information about one parent, I use that as the measure of level of education within the household. The variable family education is divided into three levels with the first level consisting of parents having no higher education than secondary school, i.e. no more than 9 years of formal education. The second level consists of parents who have graduated from upper-secondary school and the third level of education is those parents with tertiary education.

From the *Student Register, Year 9*, I also have information regarding school governing. I have excluded schools that are not under the municipal government, mostly because there are some schools among these that practice different pedagogies and I want to abstain from such effects. Moreover, I have also excluded students who have not received any grade in math in secondary school. This choice has been made since they have not been – or only to a very small extent – exposed to tracking for different reasons in any of the two regimes analyzed in this paper.[3]

In short, the two outcomes used are, first, the fact if there is information of graduation year or not to estimate the probability of graduating upper-secondary school and second, the grade from the first course taken in math in upper-secondary school, i.e. math A. Independent variables used are student gender, whether the student is foreign born and family education. School controls and year dummies are also included. Descriptive statistics are presented in table 1 below.

---

[3] Reasons why students have not been graded in math in secondary school are, e.g., dropped out of school, reduced course of studies (*anpassad studiegång*) and too much absence making the basis for grading insufficient.

5. Empirical Strategy

Ideally, one would like to randomly assign students to different tracking status and use standardized achievement tests to analyze the effects of tracking. Since that is not possible, I will try to do the best with data at hand. I consider non-random assignment of students to tracking status to be a minor problem due to admission rules, in that those residing closest to a school will have precedence over students residing further away. Therefore, the problem in previous studies with systematic differences in students between tracked and non-tracked schools due to selection will not be present in a Swedish context. However, there might have been systematic differences between schools that kept tracking and schools that did not. I do not have any information of why the choice to detrack a school was made, but this can be controlled for with the differences-in-differences approach, which implies using school fixed-effects, that will take care of any school-specific effects that are constant over time.

Since I use the upper-secondary school grade as the outcome and the fact that upper-secondary school is non-compulsory, there might be selection problems. Consider a hypothetical yet plausible situation. If secondary school students from a tracked environment - due more efficient instruction - outperform non-tracked students and since admission to upper-secondary education in Sweden is based on the obtained grade point average when graduating secondary school, tracked students would continue to upper-secondary education to a larger extent than non-tracked students. This would suggest that tracking has an overall positive effect on educational attainment but this selection would also aggravate any further analysis of achievement by underestimating the effect of tracking on the math grade in upper-secondary school. Therefore, I start by estimating the probability of having graduated from upper-secondary school to evaluate whether there are any systematic differences between students in tracked and detracked schools. The equation estimated is

$$y_{ist} = \alpha + \beta_1 TRACKED_{ist} + \beta_2 FAMEDULOW_{ist} + \beta_3 FAMEDUMID_{ist} + \beta_4 FAMEDUHIGH_{ist} + \beta_5 INTERACTION_{ist} + \delta' X_{ist} + \eta_s + \mu_t + \varepsilon_{ist} \quad (1)$$

where y_{ist} is whether individual i, in school s, at time t, has a record of a graduation year from upper-secondary school or not. TRACKED is a dummy variable that takes the value of one if the student could choose level of teaching in math in secondary school. FAMEDULOW indicates that the parents' level of education is no more than 9 years of formal education, FAMEDUMID and FAMEDUHIGH indicate that parents have at most upper-secondary and tertiary education, respectively. INTERACTION is included to see if there are any differences in tracking dependent on family background. It is an interaction between level of family education and the tracking dummy. Since family education is divided into three different levels, the interaction term will also consist of three levels, that is, three different coefficients where the first level of family education is the reference category. In this specification, the dummy variable TRACKED will capture the effect of being tracked for students from families with the lowest level of education as compared to non-tracked students with the same family background. X denotes the two control variables gender and whether the individual is foreign born or not. μ_t and η_s capture year and school-specific effects, respectively, and ε is the error term.

Table 2 reports the estimated coefficients from the differences-in-differences probit estimations of probability of graduating from upper-secondary school. The first column shows the results from estimating equation 1 without interacting tracking status with family background. The effect of tracking is positive but not significant at conventional levels and the inclusion of the interaction does not change the result. Thus, the result provides no evidence of there being any systematic differences between tracked and non-tracked schools concerning students' probability of graduating upper-secondary school.

With that result in mind, I move on to the ordered probit specification using the math grade in upper-secondary school as the outcome. The equation estimated is the same as (1) but with the difference that y_{ist} is the four-scale grade from the first course in math in upper-secondary school; otherwise the controls are the same as in the previous specification. The results are presented in table 3. The first column is, as in table 2, the estimation without interaction terms. The effect from tracking is positive, but not significant and does not change to any considerable extent when the interaction terms are included in the specification. The result from the ordered probit estimation on whether tracking has any overall effect on the upper-secondary math grade does not provide any further evidence. Tracking does not seem to have any effect on the mean achievement in math course A in upper-secondary school, not even when allowing for different effects dependent on family background.

In comparison with Betts and Shkolnik (2000), my estimate of tracking is indistinguishable from theirs. They estimate an effect of being tracked – in terms of a standard deviation of math test scores – of -0.011 with a standard error of 0.024. My equivalent[4] is 0.003 with a standard error of 0.023, which is not significantly different from their effect (using a 95 percent confidence interval).

However, this result could be due to counteractive effects. The above specification does not allow for different effects of tracking within each level of family education. If tracking has positive effects for students in the upper part of the ability distribution, but a negative effect for those situated at the lower end, the overall effect could be zero. In trying to evaluate that, I use the different grades as outcomes and estimate the probability of receiving fail, pass, pass with distinction and pass with special distinction. The same controls are used as in the previous equations. The result is presented in table 4. From these estimations, evidence of different effects of tracking emerges. When estimating the probability of failing math, the interaction term is negative and significant for students from families with a low level of education, thus students from families with at most nine years of education are more likely to fail math if they have been exposed to tracking than their non-tracked peers. The same students are also less likely to pass the math A course as compared to their non-tracked equivalents. For higher grades, there are no significant effects.

For students from academic families, the effect of being tracked on the probability of failing math is negative significant but positive significant when considering the probability of receiving the subject grade pass. There are no significant effects on the probabilities of passing with distinction and passing with special distinction.

6. Sensitivity Analysis

How robust are the results? Since most schools abandoned tracking with the implementation of the new curriculum in 1995, there could have been other confounding effects from the overall change. I have done the analysis on a restricted sample where I have excluded all those graduating secondary school in 1998 since not only was tracking removed, but a new curriculum was also introduced altogether. The only thing that changes is that the coefficient of tracking in the ordered probit estimation becomes negative and significant, indicating that, on average, students from low-level education families will get a lower grade in math in upper-secondary school as compared to their non-tracked peers. Otherwise, the results are the same.

[4] This is the estimated coefficient of tracking when using OLS estimation, without the interaction term. To get comparable estimates I simply divide the estimated coefficient and standard error with the sample standard deviation of the variable of interests, that is math grade in my sample and math tests scores in Betts and Shkolnik (2000).

Another issue is that tracking and class size are somewhat related. Consider tracking as a way of efficiently organizing students according to ability when dealing with larger student bodies, so that instruction will be held at a level most suited to their ability. Then, small detracked schools do not – to the same extent – have to deal with that problem, since the mere size of the class gives the teacher more time for individual instruction suited for each student's ability. Since the objective of this paper is to compare students who have been exposed to tracking with those who have not, small detracked schools might bias the result.[5] I have done the analysis conditional on the size of the graduating student body in every secondary school being larger than 75 students. The results are not affected by this restriction; they are the same but with somewhat larger standard errors, though still significant.

Since tracking was practiced in math and English, the impact on other subjects should be assumed to be small; for that reason, I also use Swedish as an outcome. The result shows that there are effects, however, not as strong as in the main analysis, but still negative significant effects on the probability of receiving the subject grade pass with distinction for students with a low level of family education. The direction of the effect is opposite and significant for students with a higher level of family education. Thus, it seems that even in Swedish, there are negative effects of tracking for students from families with a low education. This could be for a number of reasons, maybe tracking causes stigma among students in lower tracks, schools could have been practicing tracking in other subjects besides math and English. More likely, though, is that this is due to peer effects, in that the groupings formed in math and English carried over to other subjects, simply due to friendships formed within the tracked classrooms.

I have also used a restricted sample of native students, that is, Swedish-born students with Swedish-born parents. The results are similar to those of the main analysis, but with larger standard errors, resulting in the estimations only giving significant effects when estimating the probability of receiving the subject grade pass.

7. Conclusions

In this paper, I use the fact that different tracking regimes were practiced simultaneously in Sweden to evaluate effects in school outcomes for students who attended tracked or non-tracked schools. I use a differences-in-differences approach to estimate whether tracking had any impact on the probability of graduating upper-secondary school, but also whether it had

[5] There are significant differences in the size of the student body in secondary school, in that tracked schools have a somewhat larger student body than detracked secondary schools. These tables are not presented in the paper, but can be obtained from the author upon request.

any effect on the grade in the first math course taken in upper-secondary school. When comparing tracked and non-tracked schools, I find no overall effects of tracking. Therefore, there is no support for the proponents' view that tracking has positive effects, notwithstanding ability or background.

Then, I evaluate whether there are any differential effects by estimating the probability of receiving a specific grade, i.e. fail, pass, pass with distinction and pass with special distinction using interactions between tracking status and family background. The result shows there to be small, negative significant effects from attending a tracked environment for students from families with a low level of education. Tracked students with parents who have at most 9 years of formal education are more likely to fail math than their non-tracked peers. They are also less likely to pass math than their non-tracked equals. This could be the result of not having positive peer effects in a tracked environment. Students originating from families with a low level of education do not have the complementary support that students from academic families have. Thus, if placed in a non-tracked environment, the deficient home support could be compensated in a heterogonous environment by peers.

Tracking in Swedish secondary schools also seems to have effects beyond math and English. I find negative effects for students from families with a low level of education when using Swedish as the outcome. A possible explanation is that study groups formed in math and English with similar peers - motivation and ability vise - carries over to other subjects. Therefore, there seems to be externalities of tracking that affect the student outcome in other subjects than just the direct effects of grouping students in math and English.

So, to answer the question asked in the title of this paper, I would say that the detracking of Swedish secondary schools seems to have benefited students from families with a low level of education, but at the expense of students from families with a higher level of education. Even if the effects are small, the result is in line with previous research with evidence of differential effects.

References

Argys, L. M., Rees, D. I. & Brewer, D. J. (1996), "Detracking America's Schools: Equity at Zero Cost?", *Journal of Policy Analysis and Management*, 15 (4), 623-645

Betts, J. R., Shkolnik, J. L. (2000), "The effects of ability grouping on student achievement and resource allocation in secondary schools", *Economics of Education Review*, 19, 1-15

Dahllöf, U. S. (1969), "Ability grouping, content validity and curriculum process analysis", Report from The Institute of Education University of Göteborg, Project Compass 13

Figlio, D. N, Page, M. (2002), "School choice and the distributional effects of ability tracking: does separation increase equality?", *Journal of Urban Economics,* 51, (3), 497-514

Hanushek E. A., Wößmann L. (2005), "Does Educational Tracking Affect Performance and Inequality? Differences-in- Differences Evidence Across Countries", NBER Working Paper W11124

Hoffer, T. B. (1992), "Middle School Ability Grouping and Student Achievement in Science and Mathematics", *Educational Evaluation and Policy Analysis*, 14, (3), 205-227

Hellström, L. (1987), "Olika lika: Försök att hantera differentieringens problem i matematikundervisningen på högstadiet. Slutrapport från GEM-projektet" , Malmö: Lärarhögskolan Malmö

Hoxby, C. (2000), "Peer Effects in the Classroom: Learning from Gender and Race Variation", NBER Working Paper W7867.

Erikson, R., Jonsson, J. (1993), "Ursprung och utbildning. Social snedrekrytering till högre studier.", SOU 1993:85

Kerckhoff, A. C. (1986), "Effects of Ability Grouping in British Secondary Schools", *American Sociological Review*, 51, (6), 842-858

Linchevski, L., Kutscher, B. (1998), “Tell Me With Whom You’re Learning, and I’ll Tell You How Much You’ve Learned: Mixed-Ability Versus Same-Ability Grouping in Mathematics”, *Journal for Research in Mathematics Education*, 29, (5), 533-554

Marklund, S. (1981), “Skolsverige 1950-1975: Försöksverksamheten.” Stockholm, Liber Utbildningsförlaget.

Oakes, J. (1990), “Multiplying inequalities: the effect of race, social class, and tracking on opportunities to learn math and science”, RAND, Santa Monica, CA.

Rees, D. I., Brewer, D. J. & Argys, L. M. (2000), “How should we measure the effect of ability grouping on student performance?”, *Economics of Education Review*, 19, 17-20

Rosenbaum, J. E. (1980), “Track Misperceptions and Frustrated Collage Plans: An Analysis of the Effects of Track and Track Perceptions in the National Longitudinal Survey”, *Sociology of Education*, 53, (April), 74-88

Slavin, R. (1990), “The effects of ability grouping in secondary schools: a best-evidence synthesis”, *Review of Educational Research*, 60, 471-499

Statistics Sweden (SCB) (1985-2004), “Utbildningsstatistisk årsbok ”, Statistiska Centralbyrån.

Statistics Sweden (SCB) (1993-1998), “Statistisk årsbok för Sverige”, Statistiska Centralbyrån.

Svensson, N-E. (1962), ”Ability Grouping and Scholastic Achievement. Report on a five-year follow-up study in Stockholm”, Stockholm: Almqvist & Wiksell

Wallby, K., Carlsson, S. & Nyström, P. (2001), ”Elevgrupperingar- en kunskapsöversikt med fokus på matematikundervisning”, Stockholm, Skolverket

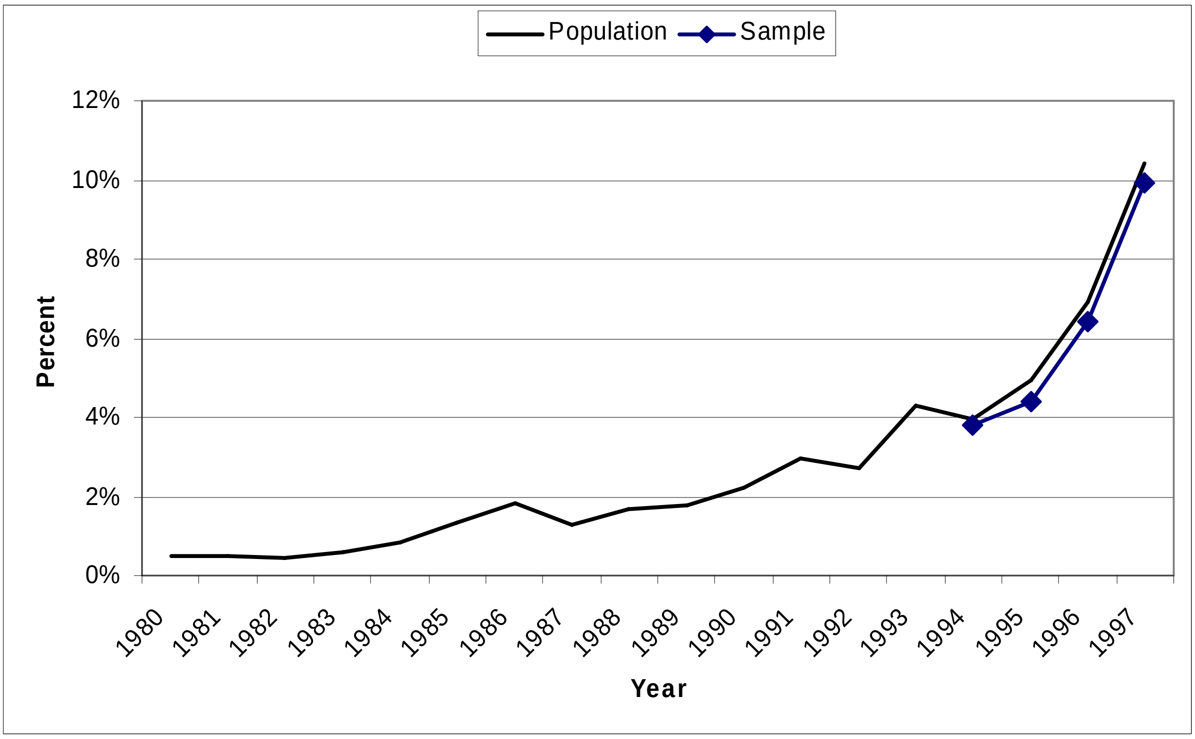
Zimmer, R. (2003), “A new twist in the educational tracking debate”, *Economics of Education Review*, 22, 307-315

Table 1. Descriptive statistics, mean values. Standard deviation in parentheses

	1994	1995	1996	1997	1998
Graduated Secondary School					
Tracked	.962 (.192)	.956 (.205)	.936 (.245)	.901 (.299)	0 (--)
Graduated upper-secondary school	.786 (.410)	.782 (.413)	.764 (.424)	.781 (.413)	.738 (.440)
Male	.507 (.500)	.506 (.500)	.506 (.500)	.510 (.500)	.510 (.500)
Foreign born	.085 (.279)	.089 (.285)	.097 (.296)	.103 (.304)	.114 (.318)
Family education level	2.271 (.670)	2.283 (.668)	2.301 (.661)	2.299 (.657)	2.297 (.662)
Number of students	30933	33395	33740	30876	33163
Graduated Upper-Secondary School					
Course Grade Math A	12.004 (4.570)	12.356 (4.601)	12.494 (4.636)	12.543 (4.921)	12.822 (4.759)
Fail	.064 (.245)	.057 (.231)	.056 (.230)	.068 (.252)	.052 (.221)
Pass	.511 (.500)	.488 (.500)	.471 (.499)	.441 (.496)	.456 (.498)
Pass with distinction	.321 (.467)	.327 (.469)	.336 (.472)	.337 (.473)	.317 (.465)
Pass with special distinction	.104 (.306)	.129 (.335)	.137 (.344)	.154 (.361)	.175 (.380)
Number of students	24109	25890	25562	23952	24345

Notes: The upper part of the table reports descriptive statistics for the entire sample, whereas the bottom part is only those who graduated upper-secondary school. Family education level is reported in three levels, the first level consists of parents with no more than 9 years of formal schooling, the second level consists of parents with at most upper-secondary school and the third level those with tertiary education.

Figure 1. Share of 9th grade Students in Detracked Secondary Schools



Notes: Up until 1987, students are reported as have received the subject grade in advanced course, ordinary course or other. For this period, I define those students reported as other as detracked, i.e. students not receiving subject grades from an ordinary or an advanced course. From 1987, students are reported as having received the subject grade in ordinary courses, advanced courses or no alternative course. I define the last as detracked students.
Source: Statistics Sweden

Table 2. Differences-in-Differences probit estimations of probability of having a degree year from upper-secondary school, marginal effects. Robust standard errors in parentheses.

	(1)	(2)
Tracked	0.004 (0.010)	0.008 (0.012)
Family education Middle	0.069 (0.003)***	0.069 (0.006)***
Family education High	0.169 (0.003)***	0.176 (0.006)***
Man	-0.058 (0.002)***	-0.058 (0.002)***
Foreign born	-0.090 (0.005)***	-0.090 (0.005)***
Family education Middle X Tracked		0.000 (0.007)
Family education High X Tracked		-0.011 (0.008)
Number of students	162012	162012
Number of schools	1179	1179

* significant at 10%
** significant at 5%
*** significant at 1%

Notes: The dependent variable is whether there is a record of graduation year from upper-secondary school. All specifications include controls for school and year. The omitted category of family education level is family education level low i.e., the comparison group is parents with at most 9 years of formal education. Thus, in the specification with interaction, the coefficient of the single tracking variable captures the effect of being tracked for students with parents who have at most 9 years of education. Standard errors are clustered on school.

Table 3. Differences-in-Differences ordered probit estimations, dependent variable math course A grade upper-secondary school. Robust standard errors in parentheses.

	(1)	(2)
Tracked	0.009 (0.027)	0.009 (0.037)
Male	0.011 (0.007)	0.011 (0.007)
Foreign born	-0.465 (0.013)***	-0.465 (0.013)***
Family education Middle	0.210 (0.012)***	0.209 (0.023)***
Family education High	0.722 (0.012)***	0.724 (0.024)***
Family education Middle X Tracked		-0.002 (0.027)
Family education High X Tracked		-0.003 (0.027)
Number of students	123858	123858
Number of schools	1192	1192

* significant at 10%
** significant at 5%
*** significant at 1%

Notes: The dependent variable is subject grade in math course A in upper-secondary school. The grade is reported in four levels, fail, pass, pass with distinction and pass with special distinction. All specifications include controls for school and year. The omitted category of family education level is family education level low i.e., the comparison group is parents with at most 9 years of formal education. Thus, in the specification with interaction, the coefficient of the single tracking variable captures the effect of being tracked for students with parents who have at most 9 years of education. Standard errors are clustered on school.

Table 4. Differences-in-Differences probit estimations of probabilities of receiving the grade fail, pass, pass with distinction or pass with special distinction in math, course A, in upper-secondary school, marginal effects. Robust standard errors in parentheses.

	Fail	Pass	Pass with distinction	Pass with special distinction
Tracked	0.011 (0.005)**	-0.041 (0.016)***	-0.009 (0.016)	0.014 (0.011)
Male	0.007 (0.001)***	-0.015 (0.003)***	-0.006 (0.003)**	0.012 (0.002)***
Foreign born	0.050 (0.003)***	0.137 (0.006)***	-0.107 (0.005)***	-0.074 (0.003)***
Family education Middle	-0.016 (0.004)***	-0.052 (0.011)***	0.045 (0.010)***	0.052 (0.008)***
Family education High	-0.058 (0.004)***	-0.214 (0.011)***	0.122 (0.010)***	0.163 (0.009)***
Family education Middle X Tracked	-0.007 (0.004)	0.023 (0.012)**	0.012 (0.011)	-0.013 (0.009)
Family education High X Tracked	-0.010 (0.004)**	0.031 (0.012)***	0.020 (0.012)	-0.006 (0.009)
Number of students	121156	123820	123810	123354
Number of schools	1073	1171	1170	1140

* significant at 10%
** significant at 5%
*** significant at 1%

Notes: All specifications include controls for school and year. The omitted category of family education level is family education level low i.e., the comparison group is parents with at most 9 years of formal education. Thus, in the specification with interaction, the coefficient of the single tracking variable captures the effect of being tracked for students with parents who have at most 9 years of education. Standard errors are clustered on school.