DIVERGING PERFORMANCES

THE DETRIMENTAL EFFECTS OF EARLY SELECTION ON EQUALITY OF OPPORTUNITY IN HUNGARY

DÁNIEL HORN[1]
Max Weber Fellow
European University Institute
Villa La Fonte, Via delle Fontanelle, 10
50014 San Domenico (FI) - ITALY
Mail: Daniel.Horn@eui.eu
Web: http://mwpweb.eu/DanielHorn

[1] On leave from the Institute of Economics, Research Center for Economic and Regional Studies of the Hungarian Academy of Sciences (CERS-HAS) and from the Economics Department, ELTE, Hungary (ELTEcon)

Acknowledgement

I would like to thank Zoltán Hermann, Balázs Muraközy, Gábor Kertesi, Gábor Kézdi, Edwin Leuven, Fabian Pfeffer, Álmos Telegdy, Balázs Váradi, Júlia Varga and three anonymous referees for their helpful insights. Comments from participants at the SIMlife conference in Mannheim, NEPS seminar in Bamberg, the CERS HAS seminar in Budapest, the annual meeting of the Hungarian Society of Economics in Budapest and the Workshop on Educational Governance and Finance in Oslo are warmly acknowledged. I thank Alyson Price for the English editing. Remaining errors are solely mine.

DIVERGING PERFORMANCES - THE DETRIMENTAL EFFECTS OF EARLY SELECTION ON EQUALITY OF OPPORTUNITY IN HUNGARY

1. INTRODUCTION

Whether or not early selection exacerbates inequality in education is a recurrent question in social science. Most countries, when changing the age at which the first selection in education is made, increase it, rather than decrease it, hoping thus to make the system more equal.[2] There are very few countries in which the age of first selection is decreased, and Hungary is one of them.

Hungary is an obvious choice for a test of the effect of early selection on inequality of opportunity for two important reasons. First, inequality of opportunity in the Hungarian education system is especially high. In 2009, the variance in reading performance, explained by family factors, was the highest among all PISA[3] participant countries (OECD 2010a). In other words, the role of the family is significant in how students perform in schools. The difference in literacy between the academic and vocational routes at secondary level is striking: students on the academic track score close to the best PISA performers, while students in the lowest level track of vocational training score near the bottom of the PISA rank. Naturally, this difference stem mainly from the fact that tracks have very different student intake (Horn et al. 2006). Second, during the transition, Hungary in effect decreased the first age of selection; two new types of academic track were introduced, which select the best children at ages 10 and 12, as opposed to the traditional age of selection at 14. This paper utilizes this unique multi-level selective feature of the Hungarian system, as well as the spatial variation in these *early-selective academic tracks*[4], to test whether early selection has contributed to the high inequality of opportunity in the Hungarian system.

Inequality is usually understood either as inequality of outcome or as inequality of opportunity. While the variance in educational outcomes is unquestionably an important aspect, I focus on the dimension of inequality of opportunity in this paper. I believe that giving a fair opportunity in life is a more commonly accepted goal of educational policy, rather than decreasing the variance in outcomes. Henceforth inequality is understood as inequality of opportunity, unless otherwise noted.

---

[2] see Sweden in the 1950s (Meghir and Palme 2005), UK in the 60s-70s (Galindo-Rueda and Vignoles 2005; Galindo-Rueda and Vignoles 2004; Pischke and Manning 2006), Finland in the 70s (Pekkarinen, Uusitalo, and Kerr 2009) or Poland in 1999 (Jakubowski et al. 2010)

[3] PISA: Programme for International Student Assessment. See the first assessment in OECD (2001).

[4] I will call the two tracks that select at age 10 and 12, somewhat suggestively, early-selective academic tracks. See the third section for details.

But what are the possible reasons behind inequality increasing as an effect of early selection? Why would a lower age of selection increase inequality of opportunity?

The most likely reasons are that teachers matter and peers matter. More precisely: since teachers matter, selecting students and forming them into groups, *and* allowing the quality of teachers to differ across groups, affects the performance of the students differently in the selected groups. If there are differences in teacher quality between schools, this should affect the mean performance of the students differently between schools. This is precisely the starting point of Rivkin, Hanushek and Kain (2005), who test this assumption. They test the effects of teachers on students, using a large, matched, teacher-student dataset from Texas. They arrive at the unsurprising conclusion that, "teachers, and therefore schools matter importantly for student achievement" (p. 449). Another way of looking at teacher quality, besides rich teacher-student matched datasets, is to qualitatively assess the reasons behind superior education performance. McKinsey&Company (2007) looked at the best PISA performers and concluded that the most important reason behind the quality of these education systems is the high quality of teachers. The best systems, "get the right people to become teachers", because the "quality of an education system cannot exceed the quality of its teachers."

Peers also have an important effect, and several papers have attempted to identify the effects of peers on student outcomes. Although this problem is loaded with methodological difficulties (see Manski 1993), some have shown, in an especially convincing way, that peers indeed matter (see Sacerdote 2001 on college roommates). Hoxby (2000), as well as Hanushek, Kain, Markman and Rivkin (2003), in the case of public education, have shown that peer effects are important. Higher achieving schoolmates can improve others, but these effects are likely to be reciprocal, less bright peers can be a hindrance. Thus, again, in a system where students are selected into homogeneous groups, peer effects could increase differences between schools, and thus increase inequality of outcome.[5]

But selection is usually not "status blind"[6]. Higher status students are much more likely to attend academic tracks, and continue on to tertiary education, than their lower status peers. It has been accepted for some time that social origin is one of the most important factors in individual educational attainment (Shavit and Blossfeld 1993). A student's socioeconomic status

[5] Teacher and peer effects are only two examples of why early selection might increase differences between students. There could be other possible reasons, for instance, curricular differences between tracks, or a better school climate in some schools (Dronkers and Robert 2008). But these two alone give enough rationale to take a look at whether selecting students at an early stage increases the gap between students.

[6] Throughout this paper I use the term "status" as shorthand for the "socioeconomic status" of students.

is also much more important in determining educational outcome differences between schools than the measured differences in school resources (Coleman 1966; Hanushek 1998).

Hence, if teacher quality and peer effects increase differences between tracks that have a significantly different composition in socioeconomic status, then the increasing inequality of outcome becomes increasing inequality of opportunity, and the gap between high-status and low-status students increases.

This paper goes through this logic step-by-step to argue that early selection has a causal and detrimental effect on equality of opportunity. The first empirical question is whether the early-selective tracks are status selective. The paper looks at whether the observed differences in socioeconomic status between the different tracks are due to skill selection, or to other non-observed factors, related to individual status, which also determine who gets into the early-selective tracks. The second part of the empirical section looks at the "value-added" of the different tracks. It tests whether the type of track has a significant effect on the student's numeracy or literacy, even if individual status and previous test scores are controlled for. Logically, if the early-selective tracks are composed of higher status students, and they also add to the literacy scores of the individual students, then the inequality of opportunity of the Hungarian system is larger than it would be without the early-selection.

Note, however, that outcome differences between tracks can increase either by increasing performance on the top or by decreasing performance at the bottom. While increasing the performance only of students in academic tracks might be considered a Pareto improvement, if lower status students lose, the process of early selection is clearly undesirable. Early selection skims off the best students and the best teachers from the general schools, and thus decreases the important peer and teacher effects. Thus I expect the performance of general schools to be lower, due to the negative effect of early-selection. The final part of the paper studies this problem. It tests whether unselected students, whose classmates move to early-selective tracks, lose out in numeracy or literacy as compared to students who had no classmates leaving early.

2. THE EFFECT OF EARLY SELECTION ON INEQUALITY – STATE OF THE ART

Studies looking at the effect of early selection (or tracking) on educational inequality use either the geographical variance of educational systems across countries (e.g. Hanushek and Woessmann 2006; Ammermüller 2005), geographical variance within countries (e.g. Bauer and

Riphahn 2006), time variance within countries (e.g. Meghir and Palme 2005; Pekkarinen, Uusitalo, and Kerr 2009; Aakvik, Salvanes, and Vaage 2010), or pilot studies (Hall 2012).[7]

Most of the cross country comparative papers put forward the idea that tracking associates strongly with higher inequality (OECD 2005; Fuchs and Woessmann 2006; Schuetz, Ursprung, and Woessmann 2008; Horn 2009), but few argue that there is a causal link between the two. Hanushek and Woessmann (2006) show that tracking increases inequality of outcome. Using a diff-in-diff analysis they find that secondary level test score variance is much higher in countries where early selection takes place, as opposed to primary level variance and to countries where first selection is made at a later age. Using a similar technique, Ammermueller (2005) argues for a causal relationship between tracking and inequality of opportunity. He shows that a greater number of school types (a proxy for tracking) leads to a greater association between family background and test scores. These results are disputed by Waldinger (2007), who finds no relation between tracking and the effect of parental education on test scores. Brunello and Checchi (2007) show that family background has a greater impact on educational attainment in tracking countries, but tracking doesn't affect the relationship between family background and outcomes at a later stage.

Two of the within country studies are written by Galindo-Rueda and Vignoles (2005; 2004), who look at the changes in the UK in the 1960s and 1970s. At that time, tracking grammar schools were gradually replaced by non-tracking comprehensive schools. The authors show that, contrary to expectations, the role of family background increased in determining student attainment; however, they also show that the role of ability during this period decreased in determining attainment, due mainly to the increased attainment of lower ability students (Galindo-Rueda and Vignoles 2005). In another paper, the authors investigate the effect of tracking on student achievement (Galindo-Rueda and Vignoles 2004) and find that tracking grammar schools are beneficial for the most able students, while others do not lose out (thus inequality of outcome increases). Pischke and Manning (2006) challenge this conclusion, arguing that self-selection effects were not entirely eliminated by Galindo-Rueda and Vignoles (2004), and that value-added in tracking jurisdictions was larger, even before tracking had taken place.

Bauer and Riphahn (2006) use the geographical variation of the age of tracking between the cantons in Switzerland. They show that the difference in the opportunity of students of higher educated parents attending an academic secondary track, over students of lower educated

[7] See Betts (2011) for a comprehensive review on the economics of tracking in education.

parents, is higher in cantons where the age of tracking is lower, as opposed to cantons with a higher tracking age.

Meghir and Palme (2005) and Pekkarinen, Uusitalo and Kerr (2009) show, in the case of Sweden and Finland, respectively, that comprehensive education reforms help to equalize the system by increasing intergenerational mobility. The authors demonstrate that although increasing the age of selection decreases inequality, it might not be beneficial for higher status families, who tend to lose not only in relative but also in absolute terms. These reforms took place in the 1950s in Sweden and in the 1970s in Finland. Both countries abolished tracking and both imposed a national curriculum on schools, and lengthened compulsory schooling to 9 years from 7 or 8. Meghir and Palme (2005) demonstrate that the reform increased both the attainment and the later earnings of children with low educated parents. At the same time, the reform also decreased the earnings of those with highly educated parents. Pekkarinen, Uusitalo and Kerr (2009) test the effects of the Finnish comprehensive reform and conclude that it had only a small, but an overall positive, effect. It significantly reduced intergenerational income elasticity for boys, and it increased intergenerational income mobility.

On the other hand, Hall (2012), using a pilot study in Sweden, shows that the effects of decreasing tracking (i.e. reducing the differences between academic and vocational tracks) in the 1990s had no long-run effects on labor market outcomes. Similarly, Malamud and Pop-Eleches (2011) argue that simply increasing general training, as opposed to vocational training, did not have a straightforwardly positive effect in Romania, as it did not increase the tertiary enrolment of the disadvantaged.

In sum, there is more evidence in favor of the effect of tracking increasing inequality, than the other way around, but the results are far from being unambiguous.

3. THE EARLY-SELECTIVE TRACKS IN HUNGARY[8]

The Hungarian system selects children quite early, first at age 10. It has not always been like this. Before 1989 the system was a typical "soviet" system, with 8 years of general training and three types of secondary track, in which students could study after the age of 14. There were two vocational tracks, a relatively more academically oriented vocational secondary/technikum (*szakközépiskola*), and a more practical vocational/apprentice training track

[8] There are major changes taking place in the education system of Hungary from the school year 2012/13. Administration and financing of the system is greatly re-centralized, compulsory age of schooling is reduced to 16 from 18, and barriers to enter academic tracks are increased. None of these recent changes affect the analysis shown in the paper.

(*szakmunkásképzés*), and an academic track (*gimnázium*). While these three tracks continue to exist today,[9] there are two additional types of academic track, the 8-year long and the 6-year long academic tracks (see figure 1 below).

(figure 1 around here)

The 8-year long academic tracks (8-yr-ac) select students just after 4th grade, at age 10. Two years later, after 6th grade at age 12, the 6-year long academic tracks (6-yr-ac) select another group of children. And finally after 8th grade, at age 14, each child must choose some secondary level track to continue her/his studies until the age of 18. Only around 3-4% of the whole cohort leaves general schools at age 10, and an additional 4-5% at age 12, to enter the 8-yr-ac and the 6-yr-ac, respectively (see table 2 and 3 below). The introduction of the early-selective tracks was gradual. Their establishment was possible between 1989 and 2000, and most of them were established between 1991 and 1997 (see a more detailed description in Horn 2010). The most important reasons for establishing an early-selective academic track in a given settlement was the demand from the local community. Local citizens – parents, teachers, maybe the school itself – or the church could lobby for such a track. When and where an early-selective track was introduced depended on the local community.[10] The post-transition central governments allowed these educational changes, but did not initiate them. Thus, there are several geographical areas in the country where no such tracks were established.

4. THE NABC DATABASE AND DESCRIPTIVE STATISTICS

The National Assessment of Basic Competencies (NABC) is a standard-based assessment designed similarly to the OECD PISA survey, but conducted annually in May.[11] It measures literacy and numeracy of the 6th, 8th and 10th grade students, and it is standardized to a mean of 1500 with standard deviation of 200. The mathematics and the reading scores are standardized, not only within but also across years. The average score of 6th grade students in 2008 was 1500 (both in math and reading), and each cohort and class is measured to this 2008/6th grade cohort. For instance, if the average mathematics score of the 6th grade students in 2010 is 1550, this means that this cohort's average mathematical literacy is quarter of a standard deviation higher

[9] The apprenticeship training has been renamed and reformed as vocational training (*szakiskola*), but that process is outside the scope of this study.

[10] The reasons why the local community would demand such a track are diverse; the most important of them being the increasing demographic pressure on schools (Liskó 1994).

[11] See Hermann and Molnár (2008) for a more detailed description of the NABC database, in Hungarian, or the OECD Review on Evaluation and Assessment Frameworks for Improving School Outcomes –Hungary Country Background Report (2010b) for a discussion of the whole evaluation system.

than that of the cohort that is two years older. Similarly, one can compare the scores across years, within cohorts.[12] In addition to the mathematics and literacy test scores the database contains extensive information on student background and on the school site.[13]

Table 1 shows who and when was measured within the NABC survey. Unfortunately, up until 2008 the database could only be analyzed on a cross-sectional basis, because it did not contain permanent student level identification numbers. From 2008 onwards the biannual datasets are linked on the student level, thus from 2010 more detailed analyses are possible.

(table 1 around here)

This paper uses data on the 2008/6th grade and the 2008/8th grade cohorts. All of the students from these two cohorts were observed two years later, in 2010. Tables 2 and 3 show the dropout/repeat rates, as well as the number of students changing tracks during these two years. Between the 6th and 8th grades approximately 6.2% of the cohort repeats year(s) or drops out. This number doubles between the 8th and 10th grades.[14] There is also a small but significant number of students that change tracks between the 6th and 8th grades. Approximately 0.2% of the 3.6% leaves or enters the 8-yr-ac during these two years. A somewhat larger fraction leaves the early-selective tracks between the 8th and 10th grades. Students repeating years, dropping out or changing tracks are certainly not typical. In order to eliminate the bias these students might generate I focus my analysis on only those students that finish two academic years within two years and do not change tracks.

(table 2 and 3 around here)

The variable indicating the family status of the students is the socioeconomic status (SES) index. This is generated similarly to the economic-social and cultural status (ESCS) index of the OECD PISA studies. The SES index is a 0 mean 1 standard deviation principal factor of three variables – just as in the PISA database – parental education, parental occupation and home possessions. Parental education is the highest parental education in years (the maximum of the mother's or the father's). Parental occupation is a standardized principal factor of the father's and the mother's employment status. The index of home possessions is a standardized principal

---

[12] See the description of the score generation procedure here (in Hungarian, accessed 07-01-2011): http://www.oktatas.hu/pub_bin/dload/kozoktatas/meresek/orszmer2010/valt_orszmer_skala_110228.pdf

[13] The national and school reports, the questionnaires and all related documents can be downloaded, in Hungarian, from the www.oktatas.hu website.

[14] Official weights, provided by the Education Authority, are used to account for the (minimal) non-response rate.

factor of the following variables: number of rooms, of mobile phones, of computers, of cars, of bathrooms, of books, a home internet connection, a student's own books, own table, own room and own computer. Generally I have used background data from 2008 (if conflicting data was provided), but imputed the SES variable with information from 2010 if data from 2008 was missing.[15]

Tables 4 and 5 below show some basic descriptive statistics of the different track types. It is obvious that the early-selective tracks (both the 6-yr-ac and the 8-yr-ac) are composed of higher status students and have higher than average test scores. That is, they have a selected group of students. It is, however, not at all obvious whether students in these tracks are of a higher status because schools tend to select higher achieving students through entrance exams (which are typical in these tracks), who tend to be of a higher status, or because there are other, status-dependent barriers to entry as well (fees, extra costs, distance from home, discrimination… etc.).

(table 4 and 5 around here)

5. ARE EARLY-SELECTIVE TRACKS STATUS SELECTIVE?

The conditional student composition of the early-selective tracks shows the status selectivity of the early-selective tracks. By controlling for previous test scores the coefficient of the SES index on track choice shows how important status is. I could not look at the 8-yr-ac choice in 6th grade, because controlling for the 6th grade test score would introduce an endogeneity problem: students in 8-yr-ac have already studied there for two years, hence 6th grade test scores are affected by the track, the depended variable in our estimation. On the other hand, point estimates of the 6th grade test scores in the 6-yr-ac choice regression are unbiased.[16] We observe students just before they enter 6-yr-ac. Hence I report only the 6-yr-ac regressions.[17]

(table 6 around here)

The estimation procedure is a simple logit regression with school-site clustered standard errors. In all models shown in table 6 above, the dependent variable is the 6-yr-ac dummy (6-yr-

[15] In a previous version of this paper, instead of the SES index, I included all variables separately in the analysis, with virtually unchanged results (Author 2010).

[16] Note however that if tracking creates incentives before its start – i.e. students work harder to get in 6th grade test scores of early-selective track students might be endogenous as well. This, however, leads to a downward bias in the vale-added measures (Koerselman 2013).

[17] Note that students in 8-yr-ac are dropped. Dropping 8-yr-ac students is necessary, since they are already in their chosen track for two years when they are first observed (at age 12). In order to compare the effect of SES and previous test score on the probability of entering 6-yr-ac, both of these variables must be measured before the track choice. See Author (2010) for an alternative solution to this problem, with similar conclusions.

ac=1 and general track=0), and the independent variables are: SES, test score and gender. Test score here is a mean of the 6th grade mathematics and reading test scores. This variable is standardized to mean 0 and 1 standard deviation, so that the coefficients of the SES and the score can be compared. All variables are on the individual level.

The results indicate that status matters. Indeed the uncontrolled odds of an average student, with one standard deviation higher SES, entering 6-yr-ac is almost 3 times higher (model 1). Test score matters more: the odds of students with one-standard deviation higher test scores are almost 4 times higher than of a student with average scores (model 2). Nevertheless, including both the SES and the test score in the regression drops the size of both effects, but both still remain highly significant (model 3). Taking spatial barriers into account, the distance[18] from home to the nearest 6-yr-ac, does not significantly change the coefficients (model 4). Thus, although students living farther away from the nearest early-selective track are less likely to attend such a track, the reason for the status selectivity of the early-selective tracks is not that lower status students live farther away, and thus cannot afford to choose these tracks. The insignificant interaction effect of SES with test score in model 5 indicates that low status, clever children are just as likely to attend early-selective tracks as others. Including 6th grade school site fixed-effects – i.e. taking into account the general school track effects, where students came from – increases the significance and the size of (almost) all coefficients (model 6). Model 6 is of course very restrictive. It compares those students who are in the same general school in 6th grade. This can only be done in those schools where at least one student has entered a 6-yr-ac, which greatly restricts the sample.[19] Not only are the score and the SES effects highly significant, but also their interaction: it seems that status matters less if one has high test scores, or similarly, test scores matter less if one has high family status. One needs at least one of these – status or skills – to get accepted.

In short, it seems that family status matters. Even if the reasons behind this result are unclear – whether it is due to higher fees, some other income related barriers, pure discrimination of low status students or other factors – higher status students are more likely to attend early-selective tracks, *ceteris paribus* skills.

[18] Distance is measured as linear air-distance from the center of the settlement where the student lives to the center of the school's settlement.

[19] Note that running model 5 on the restricted sample (model 7) results in virtually unchanged coefficients. This indicates that the altered results of model 6 are not due to the changed sample size.

6. DO EARLY-SELECTIVE TRACKS HAVE A HIGHER VALUE-ADDED?

Based on the literature, I expect 8-yr-ac and 6-yr-ac to have a superior performance compared to general tracks. Similarly, 8-yr-ac should perform better compared to the 6-yr-ac track, because students have two additional years of higher quality teachers and peers.

Track differences are estimated by a simple OLS regression for the 2008/6th grade cohort (table 7) and for the 2008/8th grade cohort (table 8). The dependent variable is the 2010 reading and mathematics test scores. I standardized the test scores to mean 0 and standard deviation 1, within cohorts and years. Control variables are the 2008 standardized test scores, the SES index and the gender of the student. All standard errors are clustered on school-site level. Variables of interest are the general track and the 8-yr-ac dummies. 6-yr-ac is the reference; hence differences between early-selective tracks and the general track, and differences between the two early-selective track types, are easy to see.

In an ideal world the following production function could be used to assess the value-added of the different tracks:[20]

$$y_{it} = \alpha_1 + \beta_1 X_i + \gamma_1 Z_t + \delta_1 IQ_i + \varepsilon_1 \qquad (1)$$

where y is the test score of the student, X covers all individually observed time invariant characteristics (here SES and gender) that might affect the test score, Z is the track specific variable that could vary with time (here track type), and IQ is an unobserved time invariant individual characteristic that most likely has an important effect on the test score (here an omitted variable). α, β, γ and δ are the parameters and ε is the error term. The subscript i stands for the individual and t for time. To get rid of the obvious omitted variable bias, I utilized the panel structure of the data and estimated the following equation:

$$y_{it} = (\alpha_1 - \alpha_0) + (\beta_1 - \beta_0) X_i + (\gamma_1 - \gamma_0)(Z_t - Z_{t-1}) + (\delta_1 - \delta_0) IQ_i + \theta y_{i(t-1)} + (\varepsilon_1 - \varepsilon_0) \qquad (2)$$

where 0 subscripts indicate parameters in time t-1 for the same equation as in (1), and θ is a parameter for the t-1 test score.[21] Assuming that the effect of IQ on test score is unchanged through time (i.e. $\delta_1 - \delta_0 = 0$) the equation can be simplified to:

$$y_{it} = \alpha + \beta X_i + \gamma (Z_t - Z_{t-1}) + \theta y_{i(t-1)} + \varepsilon \qquad (3)$$

where $(\alpha_1 - \alpha_0) = \alpha, (\beta_1 - \beta_0) = \beta, (\gamma_1 - \gamma_0) = \gamma,\ and\ (\varepsilon_1 - \varepsilon_0) = \varepsilon$. Thus the estimated parameters indicate the difference between the effects of the independent variables on test

[20] see Todd and Wolpin (2003) or Dolton (2002) for a more detailed review of the production function approach

[21] Note that if θ=1 the equation is a simple difference between equation (1) estimated in t and t-1.

score levels at different points in time. For instance, the coefficients of the 8-yr-ac in table 7 below indicate how much better the 8-yr-ac track students perform compared to the 6-yr-ac in 8th grade relative to the difference between the two tracks in 6th grade.[22]

The first two models in tables 7 and 8 show the uncontrolled differences between tracks. General school students perform approximately 0.7-0.75 standard deviations (s.d.) lower than 6-yr-ac students, who are approximately 0.1 s.d. below 8-yr-ac students in 8th grade.

Controlling for previous test scores dramatically reduces the gaps between 6-yr-ac and general tracks (models 3 and 4). Differences in literacy remain significant (0.144 s.d.), but the gap in numeracy disappears (but note that the coefficients of reading and mathematics test scores never significantly differ). Taking family status into account further reduces the gap in reading (0.03 s.d.) and reverses the sign in mathematics (model 6): it seems that general tracks have a higher value-added in mathematics, compared to the 6-yr-ac tracks (models 5 and 6). However, early-selective tracks are more likely to be run by the capital (Budapest), by the counties, or by private providers. It is reasonable to think that there are substantial differences between schools run by different education providers, as Dronkers and Robert (2004), for instance, have argued.[23] Since I am interested in the difference early selection creates, *ceteris paribus* policy differences, I control for the educational provider. In models 7 and 8 the difference in math between the general tracks and the 6-yr-ac fades away.[24] The strongest test of selection effect is to look at between-track differences within schools. Including the school site fixed-effects in the regression (models 9 and 10) shows these differences. The advantage of the 6-yr-ac is significant in reading, but not significant in math, compared to the general tracks. The performance differences between the two types of early-selective tracks disappear. Note however that there are only four schools in the country where both early-selective track types are present. Thus, differences between the two early-selective tracks in the last two models probably do not reflect the true differences between these tracks. On the other hand, out of the 149 schools, where 6-yr-ac operate, 138 have general track as well (see table A.1 in the appendix). Thus these school site fixed-effect models might offer a stronger test for the difference between general and 6-yr-ac track effects than the previous models.

[22] Note also that a significant β indicates that there are significant differences in the effects of time invariant SES and gender on test score levels in the two observed periods.
[23] The system up until 2012 was highly decentralized: curriculum, financing as well as personnel policy, depended greatly on the provider.
[24] It would, of course, be interesting to see why there are such differences between the different providers, but this question falls outside the scope of this study.

Differences among the two early-selective track types are not significant between 8th and 10th grade (see table 8).[25] On the other hand, differences between 4-year long academic tracks (normal *gimnázium*, henceforth 4-yr-ac), and the early-selective academic tracks, are significant and sizeable in all specifications. Early-selective tracks have about 0.1 s.d. higher value-added in reading and 0.2 s.d. higher value-added in mathematics.

In sum, the differences between 8-yr-ac and 6-yr-ac remain significant and sizeable in both subjects in almost all specifications between 6th and 8th grade, but not between 8th and 10th grade. 6-yr-ac seem to perform better than the general track in reading but not in math, between 6th and 8th grade, but 6-yr-ac does better in both subjects between 8th and 10th grade than the 4-yr-ac.

Although these results comply with the expectations outlined in the literature – that selective tracks fare better than the general ones – the OLS specifications are likely to be biased.

(table 7 and 8 around here)

6.1. ENDOGENEITY PROBLEMS

In both sets of regressions I observe students a few years after they have been accepted to their track. 8-yr-ac and general track students in 6th grade have been in their track for two or six years, respectively. Similarly, 8-yr-ac and 6-yr-ac students in 8th grade have been attending their track for either four or two years. Hence these tracks are likely to have an effect on the test score in all grades. In other words if $Z_t=Z_{t-1}$, the track variable in the estimated OLS regressions is endogenous. Therefore the estimated track coefficients tend to be biased.[26]

Also, even though the two important variables of skills and socioeconomic status are taken into account, there are various other unobserved factors that might have an effect on both the track variable and the later test scores (e.g. motivation of students). These omitted variables might also bias the estimated parameters.

In order to get a glimpse of the unbiased differences between tracks, I introduce an instrumental variable: distance from home to the nearest 6-yr-ac and 8-yr-ac.[27] These variables will provide the exogenous variation in the model (see Card 1993). I argue that distance from

[25] This observation is also apparent in the cross sectional tables and charts of the National Report of the NABC study (see Oktatási Hivatal 2010, 25).

[26] Note that in the regressions I assume that the track variable changes, if it is not changed, Z_t-Z_{t-1} would simplify to 0 in equation (3). Also note that the coefficients of the 6-yr-ac (reference) in the 6th to 8th grade panel, and the coefficients of the 4-yr-ac, technikum and vocational training tracks are unbiased, because we observe students before they enter these tracks, i.e. $Z_t \neq Z_{t-1}$

[27] Distance is measured, as above, as linear air-distance. I have run alternative, non-linear specifications on the effect of distance on track choice without significantly improved fit.

home to the nearest early-selective track does not have a direct effect on the test score, but it signals the chance to get accepted to an early-selective track. Any observable association of distance and test scores are due to the different chances in attending early-selective tracks. As I have shown before, distance from the nearest academic track has a negative effect on attending the given track type (see table 6). However, distance is unlikely to have an effect on how students perform in the given track, assuming that s/he has already been accepted.[28] The average student lives 9.9 kms away from the nearest 6-yr-ac, and 12.5 kms away from the nearest 8-yr-ac. Students already in 8-yr-ac live on average 3.3kms away from the school (assuming they went to the nearest 8-yr-ac), and 6.8 kms away from the nearest 6-yr-ac. Similarly, students in 6-yr-ac live 3.5kms away from their school, but 9.7kms away from the nearest 8-yr-ac. Students in general tracks have to travel 10.3 kms or 13 kms to the nearest 6-yr-ac or 8-yr-ac, respectively.

Using the distance from the nearest 6-yr-ac, and the distance from the nearest 8-yr-ac, to predict the probability of entering 8-yr-ac *and* general tracks in one regression, produces highly correlated track probabilities. Thus I split the sample into two, to gain stronger estimates and to avoid multicollinearity in the pooled model. First, I limit the sample to only those who attend either of the two early-selective tracks. Second, I test the difference between the 6-yr-ac and the general track, between 6th and 8th grade, and the 6-yr-ac and the 4-yr-ac, between 8th and 10th grade.

Tables 9 and 10 below show the 2SLS IV estimation on a sample of all settlements, where either 6-yr-ac or 8-yr-ac is available, and on a sample where both track types are available. The first stage estimations show that distance is still a strong predictor of track choice, even within this restricted sample. Distance from the nearest 8-yr-ac decreases the chance of entering an 8-yr-ac track, while distance from the 6-yr-ac increases the same probability. The 2SLS estimation does not contradict the OLS results. 8-yr-ac tracks have a greater value-added in math between 6th and 8th grade, and in reading between 8th and 10th grade. These results underline the expectation that a two year advantage in a selected track further increases the differences in test scores between students.

(table 9 and 10 around here)

[28] Of the total 174 small regions there are 37 small regions in the country where no early-selective tracks are present. There are 72 where both types, 23 where only 8-yr-ac and 42 where only 6-yr-ac is available.

The second set of analyses concerns the 6-yr-ac and the general track (tables 11 and 12). As I have shown before, these two track types are usually operated within the same school. This allows for a within-school estimation of the track effects, as above, using the instrumental variable. Because we test differences within school, the problem of unobserved factors is minimal. On the other hand, for the same reason the instrument is also much weaker. In fact, between 6th and 8th grade the instrument does not work, it does not explain within-school track choice at all. Similar students coming from greater distances, entering a given school, are just as likely to enter the general track as the 6-yr-ac track. Thus the school-site, fixed-effect, instrumental estimation is not conclusive. The instrumental estimation without school-site fixed-effects, on the other hand, underlines the expectation that 6-yr-ac tracks have a greater value-added, but the difference is significant only in reading.

Looking at the difference between the 6-yr-ac and the 4-yr-ac, the effects are rather weak (table 12). The instrument seems to work better for this set of secondary school students. That is, students living further away from early-selective tracks are less likely to attend 6-yr-ac than 4-yr-ac. However, instrumented track effects are not significant in the fixed-effect regression. Without the fixed-effects, 2SLS results are similar to the OLS results. Students in 6-yr-ac tracks perform better compared to students in 4-yr-ac, but only in math. In brief, it seems that 6-yr-ac tracks are slightly better in reading between 6th grade and 8th grade, and in math between 8th and 10th grade, but the results are not robust to the fixed-effect specification.

Taking everything together, it seems that early-selective tracks have higher value-added, but the size and the significance of this effect varies across cohorts and subjects. Longer 8-yr-ac has superior performance than 6-yr-ac in both of the observed periods, but the significance of the effect varies by time and by subject. Also, both of these early-selective tracks tend to outperform the “majority”, but only in reading between 6th and 8th grade, and in math between 8th and 10th grade. So the results suggest that early selection increases inequalities. Not only do the early-selective tracks fare better through the observed four grades in both subjects than the majority tracks, but the two virtually identical early-selective tracks also show differences due only to their different age of selection.

(tables 11 and 12 around here)

But one question still remains. Even though it seems that early selection increases differences between students, and thus increases inequality, society as a whole could benefit from the early-selective tracks, unless students left in general schools lose because of this early selection.

7. DO OTHERS LOSE?

The spatial variation in early-selective tracks allows for a comparison of those students who remained in general tracks, in regions where early-selective tracks are available, and those who had effective spatial barriers to entry. As above, I assume that performance differences between tracks are due to differences in teacher quality or differences in peer effects. I hypothesize that in areas where 6-yr-ac or 8-yr-ac tracks skim off the best students, those left in general schools have a relatively lower increase in literacy scores. This is because in schools where the best students *and* teachers are unable to opt out from the general tracks, the average teacher and peer effect is higher.

I use propensity score matching to test the differences between general track students in areas with early-selective tracks (treatment), and without such tracks (control). Propensities are calculated using the above utilized individual characteristics: 6th grade math and reading score, SES and gender. The treatment group will be those who live in a settlement with an early-selective track. That is, I assume that students living near to early-selective tracks have no serious barriers to entry. The control group will be those who live at least 15, or 20, or 25 kilometers away from these tracks. These distances are underestimations of the real distances that have to be traveled, because they are measured as straight lines between settlements. For the estimation of the average treatment effects, I use nearest neighbor as well as stratification matching. Table 13 below shows the number of treated, and the number of controls, in each specification, and table 14 shows the average treatment effects and their standard error.

(tables 13 and 14 around here)

It seems that students lose in numeracy between 6th and 8th grade, due to the early selection, but effects are not significant in literacy. In other words, general track students, in areas where any of the two early-selective tracks is available (treatment), are doing worse in mathematics than students in areas without early-selective tracks (control). The same effect is not apparent in reading. The treatment effect is not very sensitive to the definition of treatment and control groups, although when comparing students outside the 25 km radius to the treatment group the effects become less significant; but this is probably due to the relatively small number of students in the control group.

8. CONCLUSION

Educational selection is not "status blind". Higher socioeconomic status students are more likely to attend academic tracks than their lower status peers, thus academic tracks tend to have higher average status. Higher status students also attract higher quality teachers (Rivkin, Hanushek, and Kain 2005; Varga 2011). Peer effects and teacher quality are two very important factors that affect student performance (Hanushek et al. 2003; Hoxby 2000; McKinsey 2007). If academic tracks select students based on their socioeconomic status, and these tracks also have higher peer and teacher effects, the differences between academic and non-academic tracks will increase through time. The earlier this selection takes place, the larger the difference between low and high status students will be. That is, early-selection will increase inequality of opportunity.

The Hungarian system is an ideal one in which to test this logic, since students are selected at three different ages. First, at age 10 the 8-year long academic tracks skim off the best students, then at age 12 the 6-year long academic tracks add to this selection, and finally at age 14 all students enter secondary level, choosing between three types of 4-year long tracks: academic, vocational secondary and vocational training (see figure 1).

The paper first shows that Hungary is not different from other countries: higher status students are more likely to attend academic tracks, even if previous test scores are controlled for. The second part of the empirical analysis looks at the effectiveness of the separate tracks and shows that their performance diverges, even if skill and status selection is taken into account. This result is robust to specifications. It is shown that performance differences between the longer 8-year-long academic track and the shorter 6-year-long academic track increase between 6th and 8th grade. In other words, two additional years in a selective track increases individual performance *ceteris paribus* skills and status. Similar differences are shown between the 6-year-long academic track and the general track between 6th and 8th grade, and the 6-year-long academic track and the 4-year-long academic track between 8th and 10th grade. Taking everything together, it seems that early-selective tracks have higher value-added, but the size and the significance of this effect varies across cohorts and subjects.

The third section of the empirics looks at whether this process is a Pareto improvement, or whether there are groups in society that lose by the early-selection. It is shown that those who are left in general schools in areas where the best students can opt-out to early-selective tracks perform worse in mathematics than similar students in general tracks with no option of leaving. That is, selection is not a Pareto improvement as it harms those who are left behind.

In sum I conclude that early selection in Hungary increases differences between students of different status and thus adds to the not-small inequality of opportunity of the Hungarian education system, and this process harms the lower status students.

References

Aakvik, Arild, Kjell G. Salvanes, and Kjell Vaage. 2010. 'Measuring Heterogeneity in the Returns to Education Using an Education Reform'. *European Economic Review* 54 (4): 483–500. doi:10.1016/j.euroecorev.2009.09.001.

Ammermüller, Andreas. 2005. *Educational Opportunities and the Role of Institutions*. Maastricht: ROA, Research Centre for Education and the Labour Market.

Bauer, Philipp, and Regina T. Riphahn. 2006. 'Timing of School Tracking as a Determinant of Intergenerational Transmission of Education'. *Economics Letters* 91 (1): 90–97. doi:10.1016/j.econlet.2005.11.003.

Betts, Julian R. 2011. *The Economics of Tracking in Education*. Handbook of the Economics of Education. Elsevier.

Brunello, Giorgio, and Daniele Checchi. 2007. 'Does School Tracking Affect Equality of Opportunity? New International Evidence'. *Economic Policy* 22 (52): 781–861. doi:10.1111/j.1468-0327.2007.00189.x.

Card, David. 1993. *Using Geographic Variation in College Proximity to Estimate the Return to Schooling*. Working Paper. National Bureau of Economic Research.

Coleman, James S. 1966. *Equality of Educational Opportunity*. United States Department of Education.

Dronkers, Jaap, and Péter Róbert. 2004. 'Has Educational Sector Any Impact on School Effectiveness in Hungary? A Comparison of the Public and the Newly Established Religious Grammar Schools'. *European Societies* 6 (2): 205–236. doi:10.1080/14616690410001690727.

Fuchs, Thomas, and Ludger Woessmann. 2006. 'What Accounts for International Differences in Student Performance? A Re-examination Using PISA Data'. *Empirical Economics* 32 (2-3) (August): 433–464. doi:10.1007/s00181-006-0087-0.

Galindo-Rueda, Fernando, and Anna F. Vignoles. 2004. 'The Heterogeneous Effect of Selection in Secondary Schools: Understanding the Changing Role of Ability'. *SSRN eLibrary* (August).

———. 2005. 'The Declining Relative Importance of Ability in Predicting Educational Attainment'. *Journal of Human Resources* 40 (2): 335.

Hall, Caroline. 2012. 'The Effects of Reducing Tracking in Upper Secondary School Evidence from a Large-Scale Pilot Scheme'. *Journal of Human Resources* 47 (1) (December 21): 237–269.

Hanushek, Eric A, John F Kain, Jacob M Markman, and Steven G Rivkin. 2003. 'Does Peer Ability Affect Student Achievement?' *Journal of Applied Econometrics* 18 (5) (September 1): 527–544. doi:10.1002/jae.741.

Hanushek, Eric A. 1998. 'Conclusions and Controversies About the Effectiveness of School Resources'. *FRBNY Economic Policy Review* (March): 11–27.

Hanushek, Eric A., and Ludger Woessmann. 2006. ‘Does Educational Tracking Affect Performance and Inequality? Differences-in-Differences Evidence Across Countries’. *The Economic Journal* 116 (510) (March): C63–C76. doi:10.1111/j.1468-0297.2006.01076.x.

Hermann, Zoltan, and Tímea Laura Molnár. 2008. ‘Országos Kompetenciamérési adatbázis’. http://adatbank.mtakti.hu/files/dokum/7.pdf.

Horn, Daniel. 2009. ‘Age of Selection Counts: a Cross-country Analysis of Educational Institutions’. *Educational Research and Evaluation: An International Journal on Theory and Practice* 15 (4): 343. doi:10.1080/13803610903087011.

———. 2010. ‘The Political Background of Structural Changes in the Educational System of Hungary, 1985-1994’. *Center for Policy Studies Working Paper* (2010/1): 45.

Horn, Daniel, Ildikó Balázsi, Szabolcs Takács, and Yanhong Zhang. 2006. ‘Tracking and Inequality of Learning Outcomes in Hungarian Secondary Schools’. *Prospects* 36: 433–446.

Hoxby, Caroline. 2000. ‘Peer Effects in the Classroom: Learning from Gender and Race Variation’. *National Bureau of Economic Research Working Paper Series* No. 7867 (August).

Jakubowski, Maciej, Harry A. Patrinos, Emilio Ernesto Porta, and Jerzy Wisniewski. 2010. ‘The Impact of the 1999 Education Reform in Poland’. *World Bank Policy Research Working Paper* (5263).

Koerselman, Kristian. 2013. ‘Incentives from Curriculum Tracking’. *Economics of Education Review* 32 (0) (February): 140–150. doi:10.1016/j.econedurev.2012.08.003.

Liskó, Ilona. 1994. ‘Átalakuló Iskolák’. *Educatio* 3 (1): 77–88.

Malamud, Ofer, and Cristian Pop-Eleches. 2011. ‘School Tracking and Access to Higher Education Among Disadvantaged Groups’. *Journal of Public Economics* 95 (11–12) (December): 1538–1549. doi:10.1016/j.jpubeco.2011.03.006.

Manski, Charles F. 1993. ‘Identification of Endogenous Social Effects: The Reflection Problem’. *The Review of Economic Studies* 60 (3): 531–542. doi:10.2307/2298123.

McKinsey. 2007. *How the World’s Best-performing School Systems Come Out on Top?* McKinsey and Company.

Meghir, Costas, and Marten Palme. 2005. ‘Educational Reform, Ability, and Family Background’. *The American Economic Review* 95 (1): 414–424.

OECD. 2001. ‘Knowledge and Skills for Life - First Results from PISA 2000’. OECD: PARIS.

———. 2005. ‘School Factors Related to Quality and Equity’. OECD: PARIS.

———. 2010a. ‘PISA 2009 Results: Overcoming Social Background - Equity in Learning Opportunities and Outcomes Vol. II.’ OECD.

———. 2010b. ‘OECD Review on Evaluation and Assessment Frameworks for Improving School Outcomes - Hungary Country Background Report’. OECD: PARIS.

Oktatási Hivatal. 2010. *Orszagos Kompetenciamérés 2010 - Országos Jelentés*. Budapest: Oktatási Hivatal.

Pekkarinen, Tuomas, Roope Uusitalo, and Sari Kerr. 2009. 'School Tracking and Intergenerational Income Mobility: Evidence from the Finnish Comprehensive School Reform'. *Journal of Public Economics* 93 (7-8) (August): 965–973. doi:16/j.jpubeco.2009.04.006.

Pischke, Jörn-Steffen, and Alan Manning. 2006. *Comprehensive Versus Selective Schooling in England in Wales: What Do We Know?* Working Paper. National Bureau of Economic Research.

Rivkin, Steven G., Eric A. Hanushek, and John F. Kain. 2005. 'Teachers, Schools, and Academic Achievement'. *Econometrica* 73 (2): 417–458.

Sacerdote, Bruce. 2001. 'Peer Effects with Random Assignment: Results for Dartmouth Roommates*'. *Quarterly Journal of Economics* 116 (2): 681–704.

Schuetz, Gabriela, Heinrich W. Ursprung, and Ludger Woessmann. 2008. 'Education Policy and Equality of Opportunity'. *Kyklos* 61 (2) (May): 279–308. doi:10.1111/j.1467-6435.2008.00402.x.

Shavit, Yossi, and Hans-Peter Blossfeld. 1993. *Persistent Inequality: Changing Educational Attainment in Thirteen Countries. Social Inequality Series.* Westview Press, 5500 Central Avenue, Boulder, CO 80301-2847 ($45).

Todd, Petra E., and Kenneth I. Wolpin. 2003. 'On the Specification and Estimation of the Production Function for Cognitive Achievement*'. *The Economic Journal* 113 (485): F3–F33. doi:10.1111/1468-0297.00097.

Varga, Júlia. 2011. 'A Tanárok Elosztása a Különböző Szociokulturális Hátterű Tanulókat Tanító Iskolák Között'. In *Oktatás És Foglalkoztatás*, 65–82. KTI. Budapest: KTI könyvek.

Waldinger, Fabian. 2007. 'Does Ability Tracking Exacerbate the Role of Family Background for Students' Test Scores?'

Missing from list: Author (2010)

APPENDIX

(table A.1 around here)

Table 1 – The official NABC database

	4th grade	6th grade	8th grade	10th grade
2003	0	20 students from every school	0	20 students from each track from each school
2004	0	20 students from every school	20 students from every school	20 students from each track from each school
2006	full cohort	every student from a sample of 195 schools	full cohort	30 students from each track from each teaching site
2007	full cohort	every student from a sample of 200 schools	full cohort	30 students from each track from each teaching site
2008*	every student from a sample of 200 schools	full cohort	full cohort	full cohort
2009*	every student from a sample of 200 schools	full cohort	full cohort	full cohort
2010*	every student from a sample of 200 schools	full cohort	full cohort	full cohort

* Permanent individual identification numbers are available.

Table 2– Number (and percentage) of students in different tracks, 2008/6th – 2010/8th

		2010/8th grade				
2008/6th grade	Type of track	General	8-yr-ac	6-yr-ac	Missing (dropout/repeat)	Total
	General	91200	194	5318	6821	103533
		(81,9%)	(0,2%)	(4,8%)	(6,1%)	(93,0%)
	8-yr-ac	136	3839	80	66	4121
		(0,1%)	(3,4%)	(0,1%)	(0,1%)	(3,7%)
	Missing (dropout/repeat)	3331	42	126	149	3648
		(3,0%)	(0,0%)	(0,1%)	(0,1%)	(3,3%)
	Total	94667	4075	5524	7036	111302
		(85,1%)	(3,7%)	(5,0%)	(6,3%)	(100,0%)

Table 3 – Number (and percentage) of students in different tracks, 2008/8th – 2010/10th

		2010/10th grade						
2008/8th grade	Type of track	8-yr-ac	6-yr-ac	4-yr-ac	techn.	vc. training	Missing (dropout /repeat)	Total
	General	139	293	28715	35948	17281	16097	98473
		(0,1%)	(0,2%)	(23,2%)	(29,0%)	(13,9%)	(13,0%)	(79,4%)
	8-yr-ac	3024	32	488	191	23	177	3935
		(2,4%)	(0,0%)	(0,4%)	(0,2%)	(0,0%)	(0,1%)	(3,2%)
	6-yr-ac	20	4617	599	249	24	277	5786
		(0,0%)	(3,7%)	(0,5%)	(0,2%)	(0,0%)	(0,2%)	(4,7%)
	Missing (dropout/ repeat)	83	220	2627	6716	5979	153	15778
		(0,1%)	(0,2%)	(2,1%)	(5,4%)	(4,8%)	(0,1%)	(12,7%)
	Total	3266	5162	32429	43104	23307	16704	123972
		(2,6%)	(4,2%)	(26,2%)	(34,8%)	(18,8%)	(13,5%)	(100,0%)

Table 4 – Test scores means and standard errors by track type and grade

Math	2008/6th grade	2010/8th grade	2008/8th grade	2010/10th grade
General (other)	1491	1615	1604	1615*
	(0,67)	(0,70)	(0,66)	(0,73)
8-yr-ac	1673	1782	1772	1821
	(2,74)	(3,06)	(2,96)	(3,52)
6-yr-ac	1675	1756	1755	1807
	(2,42)	(2,66)	(2,49)	(2,96)
4-yr-ac			1693*	1704
			(1,01)	(1,15)
Technikum			1598*	1608
			(0,85)	(0,94)
Voc. train.			1450*	1452
			(1,24)	(1,29)
Read	2008/6th grade	2010/8th grade	2008/8th grade	2010/10th grade
General	1494	1576	1583	1628*
	(0,67)	(0,67)	(0,66)	(0,75)
8-yr-ac	1667	1746	1754	1817
	(2,42)	(2,44)	(2,57)	(2,78)
6-yr-ac	1661	1726	1739	1806
	(2,15)	(2,18)	(2,17)	(2,32)
4-yr-ac			1682*	1744

			(0,93)	(0,98)
Technikum			1578*	1622
			(0,82)	(0,89)
Voc. train.			1408*	1410
			(1,31)	(1,48)

Note: Starred numbers are generated using the panel structure of the data. No such tracks exist in the given years. Standard errors in parentheses.

Table 5 – SES index by track type and grade

SES	2008/6th grade	2008/8th grade
General (other)	-0,059	
8-yr long	0,85	0,8
6-yr long	0,82	0,8
4-yr-ac		0,39
Technikum		-0,1
Voc. train.		-0,81

Table 6 – 6-yr-ac track choice, logit

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES				6-yr-ac			
Standardized mean score (math & read) 6th grade		3.889***	2.884***	2.788***	2.927***	4.596***	2.957***
		(0.287)	(0.199)	(0.192)	(0.211)	(0.166)	(0.217)
SES	2.961***		2.039***	1.850***	1.947***	2.252***	1.820***
	(0.161)		(0.0919)	(0.0846)	(0.109)	(0.0757)	(0.104)
Score * SES					0.927	0.895***	0.937
					(0.0455)	(0.0276)	(0.0477)
Female	1.189***	1.036	1.089	1.100	1.099	1.097**	1.106
	(0.0758)	(0.0672)	(0.0695)	(0.0708)	(0.0706)	(0.0408)	(0.0698)
Distance bw. home and closest 6-yr-ac (km)				0.930***	0.930***	0.972***	0.978***
				(0.00528)	(0.00528)	(0.00633)	(0.00668)
Constant	0.0339***	0.0317***	0.0257***	0.0429***	0.0421***		0.0525***
	(0.00359)	(0.00352)	(0.00295)	(0.00510)	(0.00509)		(0.00647)
School-site FE	n	n	n	n	n	y	n
Observations	94,024	93,863	89,506	89,352	89,352	48,057	48,057
Number of school-sites						1,162	

Robust site-clustered se in parentheses, ORs reported, *** p<0.01, ** p<0.05, * p<0.1

Table 7 – Track differences in value added, 2008/6th - 2010/8th grade panel, OLS

VARIABLES	(1) Read	(2) Math	(3) Read	(4) Math	(5) Read	(6) Math	(7) Read	(8) Math	(9) Read	(10) Math
General school	-0.768***	-0.696***	-0.144***	-0.00815	-0.0327**	0.0495**	-0.0383***	0.0221	-0.201***	-0.128
	(0.0353)	(0.0509)	(0.0152)	(0.0215)	(0.0139)	(0.0211)	(0.0145)	(0.0208)	(0.0398)	(0.0886)
8-yr-ac	0.101**	0.128*	0.0775***	0.138***	0.0759***	0.136***	0.0732***	0.138***	0.00458	0.158
	(0.0490)	(0.0754)	(0.0202)	(0.0321)	(0.0186)	(0.0312)	(0.0188)	(0.0304)	(0.0598)	(0.0998)
Standardized reading score, 6th grade			0.778***		0.565***	0.165***	0.565***	0.165***	0.555***	0.174***
			(0.00413)		(0.00496)	(0.00555)	(0.00496)	(0.00555)	(0.00414)	(0.00401)
Standardized math score, 6th grade				0.769***	0.230***	0.630***	0.230***	0.630***	0.249***	0.624***
				(0.00559)	(0.00492)	(0.00644)	(0.00493)	(0.00645)	(0.00367)	(0.00488)
SES					0.0825***	0.0579***	0.0832***	0.0597***	0.0675***	0.0644***
					(0.00380)	(0.00441)	(0.00379)	(0.00441)	(0.00277)	(0.00282)
Female					0.222***	-0.131***	0.222***	-0.131***	0.222***	-0.136***
					(0.00493)	(0.00537)	(0.00493)	(0.00537)	(0.00470)	(0.00471)
Education provider FE.	n	n	n	n	n	n	y	y	n	n
School-site FE	n	n	n	n	n	n	n	n	y	y
Constant	0.757***	0.681***	0.108***	-0.0180	-0.106***	-0.00754	-0.100***	0.0227	0.0480	0.154*
	(0.0342)	(0.0501)	(0.0148)	(0.0210)	(0.0138)	(0.0211)	(0.0148)	(0.0215)	(0.0367)	(0.0835)
Observations	85,550	85,549	85,550	85,549	82,211	82,210	82,211	82,210	82,211	82,210
R-squared	0.062	0.051	0.598	0.577	0.638	0.595	0.638	0.596	0.696	0.711

Robust school-site clustered standard errors in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 8 –Track differences in value-added, 2008/8th - 2010/10th grade panel, OLS

VARIABLES	(1) Read	(2) Math	(3) Read	(4) Math	(5) Read	(6) Math	(7) Read	(8) Math	(9) Read	(10) Math
8-yr-ac	0.0577	0.0728	0.00593	0.00913	-0.00340	0.00828	-0.00647	0.00782	-0.000953	-0.00652
	(0.0491)	(0.0761)	(0.0261)	(0.0345)	(0.0226)	(0.0334)	(0.0224)	(0.0318)	(0.0321)	(0.0395)
4-yr-ac	-0.300***	-0.510***	-0.116***	-0.284***	-0.0894***	-0.235***	-0.0871***	-0.244***	-0.0830***	-0.234***
	(0.0362)	(0.0515)	(0.0194)	(0.0234)	(0.0170)	(0.0215)	(0.0170)	(0.0211)	(0.0206)	(0.0225)
Standardized reading score, 8th grade			0.620***		0.499***	0.179***	0.499***	0.178***	0.465***	0.154***
			(0.00489)		(0.00484)	(0.00511)	(0.00485)	(0.00509)	(0.00430)	(0.00441)
Standardized math score, 8th grade				0.676***	0.164***	0.534***	0.162***	0.531***	0.132***	0.491***
				(0.00811)	(0.00460)	(0.00823)	(0.00459)	(0.00816)	(0.00385)	(0.00749)
SES					0.0555***	0.0558***	0.0567***	0.0572***	0.0272***	0.0335***
					(0.00363)	(0.00403)	(0.00361)	(0.00398)	(0.00265)	(0.00293)
Female					0.155***	-0.366***	0.154***	-0.367***	0.130***	-0.358***
					(0.00682)	(0.00755)	(0.00679)	(0.00753)	(0.00545)	(0.00628)
Technikum	-0.897***	-0.986***	-0.362***	-0.415***	-0.273***	-0.359***	-0.262***	-0.367***	-0.247***	-0.342***
	(0.0394)	(0.0579)	(0.0213)	(0.0270)	(0.0190)	(0.0243)	(0.0192)	(0.0242)	(0.0259)	(0.0296)
Voc. Training	-1.935***	-1.763***	-0.841***	-0.657***	-0.676***	-0.546***	-0.669***	-0.558***	-0.628***	-0.558***
	(0.0413)	(0.0565)	(0.0254)	(0.0297)	(0.0238)	(0.0278)	(0.0239)	(0.0278)	(0.0296)	(0.0345)
Ed. Provider FE	n	n	n	n	n	n	y	y	n	n
School-site FE	n	n	n	n	n	n	n	n	y	y
Constant	0.826***	0.894***	0.338***	0.374***	0.187***	0.502***	0.199***	0.536***	0.182***	0.497***
	(0.0352)	(0.0539)	(0.0188)	(0.0234)	(0.0172)	(0.0224)	(0.0184)	(0.0229)	(0.0217)	(0.0243)
Observations	76,530	76,521	76,530	76,521	73,256	73,253	73,256	73,253	73,256	73,253
R-squared	0.375	0.255	0.639	0.592	0.659	0.633	0.660	0.634	0.699	0.677

Robust school-site clustered standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 9 – Track differences in value-added, 2008/6th – 2010/8th grade panel, 8-yr-ac vs. 6-yr-ac tracks, 2SLS

	1st stage	2nd stage		1st stage	2nd stage	
	All settlements			Settlements where both tracks available		
VARIABLES	8-yr-ac	Read	Math	8-yr-ac	Read	Math
8-yr-ac		0.0927**	0.169***		0.113	0.314***
		(0.0435)	(0.0635)		(0.0743)	(0.102)
Distance bw. home and closest 8-yr-ac	-0.0194***			-0.0193***		
	(0.00152)			(0.00269)		
Distance bw. home and closest 6-yr-ac	0.0176***			0.0139***		
	(0.00160)			(0.00355)		
Standardized reading score, 6th grade	0.0100	0.545***	0.189***	0.0167	0.533***	0.176***
	(0.0137)	(0.0118)	(0.0138)	(0.0181)	(0.0146)	(0.0160)
Standardized math score, 6th grade	-0.0194	0.218***	0.701***	-0.0195	0.224***	0.719***
	(0.0181)	(0.0105)	(0.0135)	(0.0237)	(0.0118)	(0.0158)
SES	-0.00682	0.0482***	0.0751***	-0.0113	0.0505***	0.0721***
	(0.0112)	(0.00839)	(0.00872)	(0.0156)	(0.00917)	(0.0107)
Female	0.0210	0.190***	-0.242***	0.0229	0.194***	-0.241***
	(0.0165)	(0.0137)	(0.0142)	(0.0214)	(0.0154)	(0.0172)
Constant	0.443***	-0.0281	-0.0217	0.421***	-0.0400	-0.0825*
	(0.0586)	(0.0261)	(0.0379)	(0.0715)	(0.0354)	(0.0486)
Ed. provider FE	y	y	y	y	y	y
Observations	7,986	7,989	7,986	5,878	5,881	5,878
R-squared	0.209	0.536	0.646	0.064	0.522	0.636

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 10 – Track differences in value-added, 2008/8th – 2010/10th grade panel, 8-yr-ac vs. 6-yr-ac tracks, 2SLS

	1st stage	2nd stage		1st stage	2nd stage	
		All settlements			**Settlements where both tracks available**	
VARIABLES	8-yr-ac	Read	Math	8-yr-ac	Read	Math
8-yr-long		0.0491	0.0942*		0.238**	0.0725
		(0.0474)	(0.0510)		(0.0936)	(0.0965)
Distance bw. home and closest 8-yr-ac	-0.0174***			-0.0185***		
	(0.00166)			(0.00274)		
Distance bw. home and closest 6-yr-ac	0.0175***			0.0168***		
	(0.00216)			(0.00363)		
Standardized reading score, 8th grade	0.00938	0.458***	0.117***	0.0180	0.444***	0.113***
	(0.0162)	(0.0128)	(0.0132)	(0.0206)	(0.0163)	(0.0149)
Standardized math score, 8th grade	0.00955	0.212***	0.756***	0.00760	0.221***	0.755***
	(0.0199)	(0.0118)	(0.0136)	(0.0249)	(0.0155)	(0.0158)
SES	-0.0245*	0.0311***	0.0393***	-0.0384**	0.0380***	0.0406***
	(0.0133)	(0.00895)	(0.00941)	(0.0171)	(0.0115)	(0.0110)
Female	0.00678	0.177***	-0.296***	0.00971	0.179***	-0.281***
	(0.0186)	(0.0141)	(0.0157)	(0.0236)	(0.0174)	(0.0185)
Constant	0.424***	0.182***	0.354***	0.413***	0.109**	0.365***
	(0.0594)	(0.0294)	(0.0302)	(0.0696)	(0.0474)	(0.0466)
Ed. Provider FE	y	y	y	y	y	y
Observations	6,375	6,379	6,375	4,782	4,785	4,782
R-squared	0.189	0.500	0.687	0.059	0.464	0.678

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 11 – Track differences in value-added, 2008/6th – 2010/8th grade panel, 6-yr-ac vs. general tracks, 2SLS

	1st stage	2nd stage		1st stage	2nd stage	
	Education provider fixed effects			**School-site fixed effects**		
VARIABLES	6-yr-ac	Read	Math	6-yr-ac	Read	Math
6-yr-ac		0.466**	0.115		0.249	10.09
		(0.215)	(0.301)		(7.394)	(9.202)
Distance bw. home and closest 8-yr-ac	0.000633*			6.74e-05		
	(0.000333)			(6.22e-05)		
Distance bw. home and closest 6-yr-ac	-0.00226***			-7.58e-05		
	(0.000271)			(7.25e-05)		
Standardized reading score, 6th grade	0.0103***	0.562***	0.163***	0.000883	0.562***	0.167***
	(0.00184)	(0.00553)	(0.00632)	(0.000572)	(0.00725)	(0.00902)
Standardized math score, 6th grade	0.0284***	0.217***	0.622***	0.00154***	0.253***	0.609***
	(0.00363)	(0.00815)	(0.0111)	(0.000568)	(0.0118)	(0.0147)
SES	0.0208***	0.0738***	0.0562***	0.000389*	0.0656***	0.0619***
	(0.00239)	(0.00595)	(0.00810)	(0.000205)	(0.00381)	(0.00474)
Female	0.00626**	0.219***	-0.128***	-0.000126	0.225***	-0.129***
	(0.00285)	(0.00536)	(0.00589)	(0.000254)	(0.00414)	(0.00515)
Constant	0.0454***	-0.152***	0.0381***	0.0570***	-0.160	-0.543
	(0.00689)	(0.00886)	(0.0119)	(0.000768)	(0.422)	(0.525)
Ed. provider FE	y	y	y	n	n	n
School-site FE	n	n	n	y	y	y
Observations	78,744	78,743	78,744	82,051	82,052	82,051
R-squared	0.187	0.621	0.583	0.002		
Number of school-sites				2,762	2,762	2,762

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 12 – Track differences in value-added, 2008/8th – 2010/10th grade panel, 6-yr-ac vs. general tracks, 2SLS

	1st stage	2nd stage		1st stage	2nd stage	
	Education provider fixed effects			**School-site fixed effects**		
VARIABLES	6-yr-ac	Read	Math	6-yr-ac	Read	Math
6-yr-ac		-0.0434	0.225**		-0.252	0.0667
		(0.103)	(0.102)		(0.157)	(0.170)
Distance bw. home and closest 8-yr-ac	0.000452			-0.00109***		
	(0.000647)			(0.000353)		
Distance bw. home and closest 6-yr-ac	-0.00592***			-0.00202***		
	(0.000554)			(0.000391)		
Standardized reading score, 6th grade	0.0252***	0.503***	0.169***	0.0166***	0.472***	0.143***
	(0.00495)	(0.00807)	(0.00816)	(0.00391)	(0.00574)	(0.00620)
Standardized math score, 6th grade	0.0185***	0.192***	0.646***	0.0139***	0.163***	0.613***
	(0.00659)	(0.00655)	(0.00987)	(0.00446)	(0.00524)	(0.00567)
SES	0.0347***	0.0579***	0.0475***	0.0226***	0.0349***	0.0303***
	(0.00453)	(0.00568)	(0.00627)	(0.00302)	(0.00542)	(0.00586)
Female	-0.0137*	0.139***	-0.358***	-0.00239	0.127***	-0.358***
	(0.00708)	(0.00827)	(0.00879)	(0.00433)	(0.00664)	(0.00718)
Constant	0.132***	0.114***	0.238***	0.143***	0.183***	0.277***
	(0.0163)	(0.0160)	(0.0153)	(0.00524)	(0.0185)	(0.0200)
Ed. provider FE	y	y	y	n	n	n
School-site FE	n	n	n	y	y	y
Observations	28,354	28,353	28,354	28,577	28,576	28,577
R-squared	0.084	0.531	0.633	0.020		
Number of school-sites				512	512	512

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 13 – Propensity score matching, number of treated and controls

		Nearest neighbor	Stratification	
kilometers from nearest early-selective track	15	34438	34438	Treated
		11596	16119	Control
	20	34438	34438	Treated
		5913	6679	Control
	25	34438	34438	Treated
		2546	2642	Control

Table 14 – Propensity score matching, average treatment effects

		Nearest neighbor	Stratification	
	Mathematics			
kilometers from nearest early selective track	15	**-0,034**	**-0,032**	Mean
		(0,013)	(0,009)	(se)
	20	**-0,045**	**-0,030**	Mean
		(0,018)	(0,012)	(se)
	25	-0,038	-0,032	Mean
		(0,028)	(0,020)	(se)
	Reading			
	15	0,000	-0,003	Mean
		(0,013)	(0,008)	(se)
	20	-0,007	0,004	Mean

		(0,018)	(0,011)	(se)
	25	-0,023	0,005	Mean
		(0,028)	(0,016)	(se)

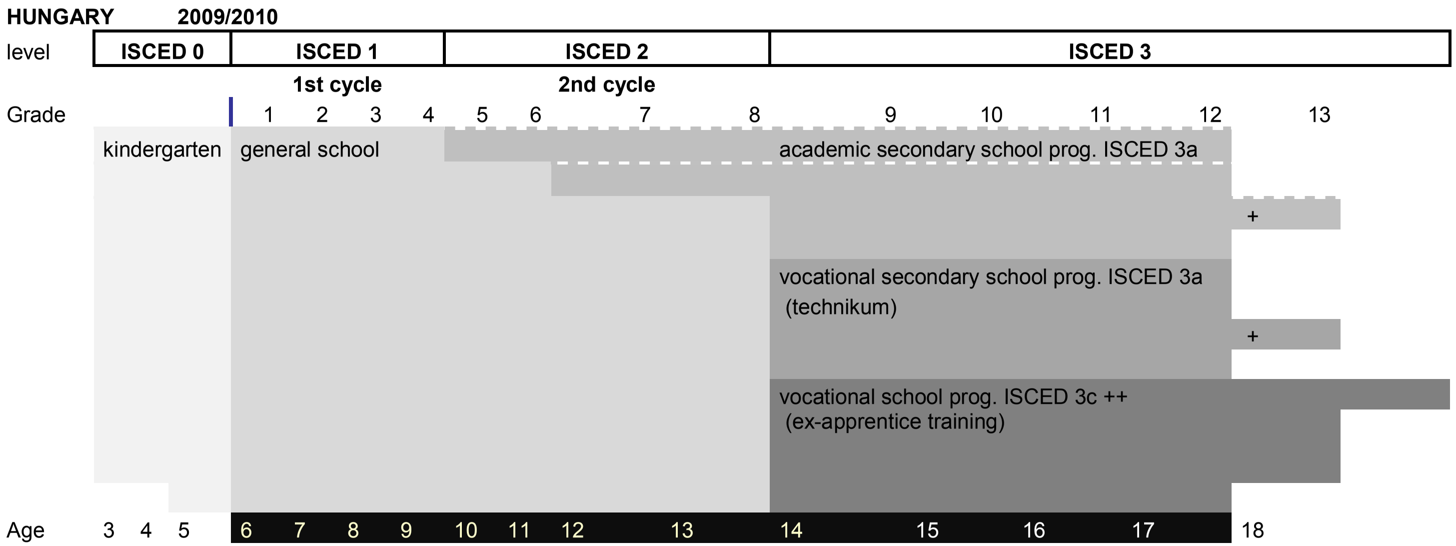
Note: significant effects in bold.

Table A.1 – Track type combinations within school-sites, 2010

Number of sites	general	4-yr-ac	6-yr-ac	8-yr-ac	technikum	voc. train.
2463	+	-	-	-	-	-
228	-	-	-	-	+	+
162	-	-	-	-	+	-
95	-	+	-	-	+	-
91	-	+	-	-	-	-
82	+	+	+	-	-	-
78	-	-	-	-	-	+
72	+	+	-	-	-	-
52	+	+	-	+	-	-
50	-	+	-	-	+	+
43	+	-	-	-	-	+
23	+	-	-	+	-	-
23	+	-	+	-	-	-
23	+	+	+	-	+	-
15	+	+	-	+	+	-
14	+	-	-	-	+	-
8	-	+	+	-	-	-
8	+	-	-	-	+	+
8	+	+	-	-	+	-
7	+	+	-	-	-	+

7	+	+	-	-	+	+
5	-	+	-	-	-	+
3	+	-	+	-	+	-
3	+	+	+	+	-	-
2	-	+	-	+	-	-
2	-	+	+	-	+	-
2	+	+	+	-	+	+
1	-	+	+	-	+	+
1	+	-	-	+	+	-
1	+	-	+	+	-	-
1	+	+	-	+	-	+
1	+	+	-	+	+	+
1	+	+	+	+	+	-

Figure A.1 – The Hungarian compulsory education system



compulsory education until the age of 18 applies for the 1st graders in 1998 and later (previously and from 2012 September: until the age of 16)

vocational secondary school programmes curriculum includes vocational subjects and many students progress to PS voc to get a VQ

+ : some schools offer an extra grade teaching a foreign language before secondary school educ. (i.e. between grade 8 and 9)

++: some programmes are also available for elementary school drop-outs

ISCED	English	national language	share
0	kindergarten	óvoda	
1,2a	general school	általános iskola	100%
3a	secondary grammar school prog.	gimnázium	
3a	vocational secondary school prog.	szakközépiskola	
3c	vocational school prog.	szakiskola	