Estimating primary school effectiveness from secondary schooling data: the case of Hungary[1]

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The paper aims at estimating the effectiveness of individual primary schools in Hungary using secondary schooling data at the individual level. Secondary schooling depends on student performance in the primary school, while student performance is assumed to be affected by school quality and individual characteristics. Thus, estimating the determinants of secondary school type (academic, vocational or mixed academic and vocational) with random school effects provides a measure of school quality. Different estimates of the school effects are compared both to each other and to the unadjusted means of the secondary schooling variables. The size of the estimated school effects are also evaluated relative to the effect of individual family characteristics.

Most of the empirical studies on both school effectiveness and the production of education uses standardised test results as the measure of school output (see the reviews of Teddlie et al., 2000 and Hanushek, 2003 respectively). In state-of-the-art school effectiveness research school effects are usually estimated in terms of student achievement using multilevel models, controlling for individual characteristics and preferably prior achievement, as well. In education economics, when test results are available for at least two points in time, usually the value added specification of the education output is applied, which, under certain assumptions, make the estimation of a production or school function possible without measuring innate ability[4].

However, standardised student achievement data are frequently not available for the analysis and extensive testing may be difficult (it is quite expensive, it requires

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[4] The value added specification is generally regarded as the best available method in empirical research, given the usual data limitations. However, as Todd and Wolpin (2003) show, from a theoretical point of view it is far from ideal, underlying assumptions are not very plausible.

professional staff and good organisational background etc.). Under these circumstances the analysis of school effectiveness using individual school continuation data provides a second best option. Estimated school effects are evidently less accurate than those estimated from standardised test data[5], hence it is not recommended to use these measures when evaluating the performance of individual schools and rewarding or sanctioning school management. Nevertheless, ‘second best’ measures of school effectiveness may well yield reliable information for national or regional level educational policy and useful inputs for further research.

Besides data availability or lower cost of data collection, school continuation is *in fact* a crucial element in the output of schools at the lower levels education. Dustmann et al. (2003) for the UK shows that school continuation decisions play a decisive role in mediating the effect of school inputs on wages. This way school continuation decisions are crucial steps in the human capital accumulation process. It can be argued, that higher admission rates to universities reveals better secondary school quality, though this clearly tells only part of the story; school quality should also enhance labour market success for the students not continuing their studies. At the same time, after completing the primary school (providing primary and lower secondary education in Hungary) students are usually expected to continue in some form of secondary schooling in OECD countries, since they are well within the mandatory education age. Thus, secondary schooling is a more complete measure of the output of primary schools than entering higher education in case of secondary schools. In one word, making the better secondary schools achievable for more students is indeed an extremely important part of the output of primary schools.

The paper provides a method for estimating primary school effectiveness using secondary schooling data and an application of this approach to Hungarian data. In section two this approach is summarised, in relation to human capital theory. Section three briefly describes the Hungarian context, the empirical methodology applied here and the data. Section four compares different measures of school effects with each other and the magnitude of school effects with the estimated impact of family characteristics. Section six concludes the paper.

[5] Additionally, this way quality may be measured only on a limited range, since above a threshold almost every student will choose the most prestigious general secondary schools.

2. Estimating school effectiveness from school continuation data

Human capital theory says that schooling decisions (i.e. investment in human capital) depends on the expected benefits of education relative to its cost. Individual ability may affect both the expected benefits (labour market returns) and the costs (see e.g. Becker – Tomes, 1986). Better ability generally enhances the *expected* returns, since it increases the likelihood of success in school and thus the expected value of a higher wage due to better education (see e.g. Erikson – Jonsson, 1996). At the same time, the subjective costs of learning are assumed to decrease in ability. Altogether, ability can be assumed to positively correlate with the optimal level of human capital investment for the individual[6].

If individuals make human capital investment decisions within their schooling career, past achievement has a similar impact than ability, and in fact, these two factors are generally indistinguishable in empirical analysis[7]. Hence we assume, that the schooling decisions depends on three factors:

$$Y = y(A,F,X)$$

where *A* denotes student achievement, *F* family characteristics (e.g. wealth and prefrences) and *X* other determinants of the returns and costs of schooling. At the same time, achievement is determined as the output of the education production process and described by the familiar education production function (see e.g. Hanushek, 2003):

$$A = a(E,S,P,F)$$

where *E* stands for individual endowment (ability), *S* for school inputs, *P* for peer group effects within the school and *F* for family characteristics. Substituting the production function into the equation describing the schooling decision provides the relationship between (prior) school quality and the schooling decision. Assuming that

[6] It has to be noted that Becker and Tomes (1986) argues that ability may in fact have a negative effect on the returns to schooling and thus on human capital investment. If ability is acknowledged by employers independent of schooling, it is in principle possible, that schooling provides less returns in terms of wages to the more able workers.

achievement increases the optimal level of human capital and past school quality contributes to student achievement, school quality is expected to have a positive impact on further schooling decisions. Thus, estimating the impact of either school inputs or individual school effects on the schooling decisions when controlling for the other factors, provides a measure of school quality.

What kind of schooling decisions can be analysed in this context? Clearly, it is necessary for the schooling decision analysed to closely reflect the human capital investment decision. School continuation decisions (schooling v entering the labour market) or the choice between academic and vocational tracks best comply with this requirement.

The analysis is more reliable if the schooling decision analysed involves some form of an entrance exam or the admission of students is related to prior student performance in some other way. In this case another mechanism comes into play: if students are sorted with respect to achievement, school continuation is a direct indicator of better performance. This makes the correlation between school quality and further schooling much stronger and reliable.

Since schooling decisions are described by categorical data, the effect of school quality can be estimated using a logit or probit regression, while in case of more than two outcomes ordered logit or probit can be used.

3. Data and methodology

In Hungary after the primary school (providing primary and lower secondary education in the ISCED terminology in the 1-8th grades) students continue their studies either general secondary schools (hereinafter GSS) or vocational secondary schools (hereinafter VSS) or technical schools (hereinafter TS). GSS is the academic track in secondary education, usually chosen by the most able students, most of them later continuing their studies at universities. TS provides the shortest way through secondary education to the labour market and does not qualify for university studies. TS is generally the option for the students with the weakest achievement in primary school. VSS has a mixed academic and vocational orientation, it provides

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[7] This is not the case only when families are assumed to make a decisions only at one point in time, when their children first enter school, and stick to it during the whole schooling career.

vocational training and also the qualification required for university admission, though with smaller chance of further university studies than GSS.

Since students can complete vocational training sooner in TS than in VSS and they are not qualified for higher education, the expected years of schooling is the least in case of TS[8]. Also, the qualification of TS provides rather poor labour market returns; low wages and a high chance for unemployment. Some researchers even argue, that the level of human capital acquired in TS so much lags behind that of GSS or VSS, that TS rather should be considered as a prolongation of lower secondary education instead of classifying as a part of upper secondary education[9]. At the other extreme, GSS is the option for students with a strong aspiration to enter higher education. VSS is a mixed option, favoured in part by risk-averse families: it leaves the way open towards higher education, but also provides vocational training (and thus an opportunity to enter the labour market immediately after secondary education), at the cost of somewhat lower chances for getting admission to the most prestigious universities than GSS. Altogether, the three tracks correspond to different expected educational careers, different amounts of human capital acquired in secondary education and sort students accordingly. Overall, the school continuation decision after the primary school is a crucial step in the human capital accumulation process.

Secondary schools are allowed to have entrance exams and students are traditionally sorted with respect to prior achievement, though this sorting is not perfect, since the distinct secondary schools may have different requirements and also families are allowed to choose among the three types of schools[10]. Thus, it could happen to be more difficult to get into an extremely prestigious VSS than a less popular GSS. At the same time, some families send their children into a VSS or even a TS even though their achievement would allow to choose a GSS. In the analysis below we assume, that these cases either are related to family characteristics and thus are controlled for or occur randomly within schools.

The analysis aims at estimating individual school effects is the schooling decision context. This is carried out by estimating simple multilevel regressions of the schooling decision with a school random effect, i.e. a primary school specific random

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[8] Though this is the typical case, some students continue their studies after TS in GSS or VSS.
[9] Kertesi – Varga, 2005.

intercept. For the schooling decision regression we use two specifications. First, ordered logit model is applied, which is consistent with the human capital model of schooling decisions (see Cameron – Heckman, 1998 and Lauer, 2000). This specification builds on the assumption, that the three schooling options corresponds to different levels of human capital investment. In this case we assume that both the family characteristics and the school effects always have a similar effect on the probability of a higher ranked option relative to the others (i.e. GSS v VSS and TS; VSS v TS)[11]. Second, we allow for different school effects for different elements of the schooling decision by assuming a nested structure. We assume, that first students are sorted between technical schools and secondary schools (GSS and VSS), then in the second phase those entering secondary education choose between (or selected by schools into) GSS or VSS. We estimate the determinants of this nested decisions using two separate logit regressions, both with a school specific intercept. Altogether, we have three estimates of the school effects: (1) the ordered logit estimate school effect on secondary schooling, (2) the school effect on TS v GSS/VSS and (3) the school effect on GSS v VSS. For the estimation of the models and the individual school effects we used the GLLAMM module written by Rabe-Hesketh et al. (see Rabe-Hesketh et al., 2001) for the STATA software.

The individual school effects are estimated from the ordered logit and logit parameters by the shrinkage or empirical Bayes estimator, as it is common in school effectiveness research, when the purpose is not evaluating the performance of the individual schools separately (Fitz-Gibbon, 1996, Teddlie et al., 2000). The shrinkage effect is a weighted average of the unadjusted effect and the grand mean of the effects, where the weights are determined by the reliability (i.e. the standard deviation) of the unadjusted effects (see Raudenbush – Bryk, 2002 for further details). Since the mean of the unadjusted effects is zero, the less reliable estimates, i.e. schools with small sample size are shrunken towards zero, resulting in conservative estimates, especially for the smaller schools. However, the variance of this estimator is less than that of the unbiased unadjusted effects (see Raudenbush – Bryk, 2002).

[10] This is also true for the German education system (Schnepf, 2002), which the three track system in Hungary follows.
[11] See e.g. Long, 1997 on this „paralel regression assumption".

The directly estimated school effects are parameters of the logit or ordered logit estimation, thus, besides the sign and the statistical significance these can not be interpreted. For the evaluation of the magnitude of the school effects either odds ratios or marginal effects on the probabilities can be used. The analysis below builds on the latter approach, average and school specific probabilities of the three options are computed for students with typical family characteristics. Estimated probabilities are calculated from the shrunken school specific random error term. School effects are defined in terms of probabilities, i.e. the difference between the estimated probability including the school specific random intercept and the probability without the school specific term, in case of a typical student.

The major difficulty in estimating school effects in the Hungarian context is that prior achievement or ability of students can not be observed, while there is substantial selection in the intakes of primary schools, i.e. we can not assume, that ability is randomly distributed, independently of the schools. In Hungary parents are allowed to choose among primary schools freely. Primary schools are not allowed to hold entrance exams but are able to sort students for example by offering specialised classes as signals of elite education. Another source of selection is that some general secondary schools extended their programme for the upper-cycle of primary schools, attracting the most able students. At the same time, primary schools left by many children with good abilities end up with a less favourable than the average student group. These selection processes can not be directly measured, however, we can assume, that (1) ability correlates with individual family background, especially education of the parents, and (2) selection correlates with the composition of students in the schools. Unfortunately, the remaining part of ability selection may bias the results. Moreover, controlling for the composition of students also raises some concerns, since interpreting the estimated contextual effects is far from straightforward.

Contextual effects may represent at least three different factors: selection according to ability or prior achievement, peer group effects among the students or omitted variables correlating with the composition, especially school inputs (e.g. prestigious schools with more students from better-off families may have a teaching staff of higher quality) (Raudenbush – Bryk, 2002, Meyer, 1997). The pure impact of selection should be removed from the estimated school effects, since it has nothing to do with school quality. At the same time, the impact of school inputs should be regarded as part of the school effect. Peer group effects, however, belongs to the true school effect for the families, when choosing among the schools, but the governemnt should not consider it part of the school effect, since it is not produced by the school inputs; it is not the result of the efforts made by the school management or the teaching staff (Meyer, 1997). Since we can not distinguish these factors, controlling for the contextual effects provides a downward biased estimate of the school effects (part of the true school effect is removed), while incorporating the contextual effects into the school effects results in upward biased estimates (selection effects are mistakenly considered as part of the school effects). Thus we compute each school effect measures both with and without copntextual effects and interpret these as upper-bound and lower-bound point estimates of the school effects respectively[12]. Both upper and lower bound estimates are calculated from the same set of regression parameters[13], since the random intercept model is misspecified if we ignore the contextual effects when these are in fact present (see e.g. Snijders – Bosker, 1999)[14].

The analysis uses data from yearly school statistics of the Ministry of Education and student level data from the 2003 9th grade student survey of the Institute of Public Education. The latter contains data collected in secondary schools. The survey encompassed all of the secondary schools, 15% of the school refused participation. Overall, more than a hundred thousand students, 77,5% filled the questionnaire. School quality was estimated only for those primary schools, of which at least 80% of students in the 8th grade in the previous year responded for the 9th grade survey (1852 schools, 63%). Primary schools operating as part of a secondary school, or together with a student hostel or elementary school of arts were excluded from the sample, since the teacher staff can not be unambiguously matched with students in distinct branches of the schools.

Individual and family characteristics are the education of parents (coded into five dummies for each parent), one or both of the parents being unemployed in the previous year, gender and a dummy for attending private foreign language classes

[12] In a strict sense these are not upper bound and lower bound estimates for the individual schools, since the student composition and the school effect beyond this may happen to offset each other in certain cases. However, for the entire population of schools, the variance of the school effects including the contextual effect is larger than that of the „narrow" school effect.
[13] The estimated effects of the contextual variables are added to the school specific random effects.

(as an indicator of aspirations). Contextual variables (i.e. school means) are used for each variable. The special characteristics of primary schools in the capital city are also controlled for a dummy (due to the outstanding supply of both secondary schools and universities in Budapest, and the different labour market situation the schooling decisions can be expected to differ from those made in the countryside).

4. Results

The results of the secondary school decision regressions are shown in Table 1. Family and indivudal characteristics are highly significant both at the individual and at the contextual level. The variance of the school specific interceps is also significantly different from zero, i.e. a significant part of the variance left unexplained by the right hand side variables belongs to the school level. Note that in this respect the GSS v VSS model is outstanding; the variance of the random intercepts is almost twice as large as in the two other models.

It is interesting to examine the distribution of the random intercepts. Table 2 shows the percentage of schools with significantly (at the 5% level) higher and lower random intercepts than zero. Overall in two cases out of the three models one third of the schools are significantly different from the average, in the third case just one in four schools can be distinguished from the others. These figures seem to be quite low, however, Balázs and Zempléni (2004) found a similar distribution analysing standardised test results in Hungary. Moreover, the results in Table 2 are more conservative than those of Balázs and Zempléni (2004) in two senses: these are shrinkage estimates (i.e. the values are shrunken towards zero) and lower bound estimates (i.e. contextual effects are removed).

Comparing settlement categories reveals an intriguing pattern. Village schools form the less, and town schools the most heterogeneous group. The former is probably in part due to the shrinkage estimator: since village schools are smaller, the effects are more shrunken towards zero. But it is surprising, that there are larger differences among primary schools in towns than in Budapest, where the mere

[14] If contextual effects are ignored, but these are present in fact, the individual level right hand side variables and the school specific error terms are correlated, producing biased estimates (similar to omitted variable bias in OLS regressions).

number of schools suggests subtantial heterogenity. However, in this comparison contextual effects are ignored, this can modify this first picture.

As opposed to the school specific intercepts, the size of the school effects measured in probabilities can be directly interpreted. In order to compare the ordered logit and the two logit estimates, the school effects from the former were caslculated for the same outcomes as in the latter case. School effects are computed for a hypothetical student with close to the average family background (both parents had secondary education, neither of them were unemployed in the past year, no extra foreign language classes). When contextual effects are not included, schools are assumed to have an average composition of students.

The size of the school effects are depicted by Table 3 and Table 4. The same features of the school effects emerge from both tables. First, school effects have a larger impact for the GSS v VSS decision than on the TS v GSS/VSS decision. This distinction is clearly disguised by the logit estimates, while the ordered logit model somewhat covers it. Second, the contextual effects seem to matter more for the TS v GSS/VSS decision. Including the contextual effects induces more changes in the effect size here, than in the GSS v VSS decision. Third, regarding the TS v GSS/VSS measures, primary schools in towns show the largest heterogenity. For the GSS v VSS decision the differences between settlement categories are more ambiguous.

Table 5 presents the correlation matrix of the school effects. The most striking fact is the virtually missing correlation between the logit estimates of the TS v GSS/VSS and GSS v VSS school effects. This suggests that two relatively independent elements of school effectiveness are exhibited here. Again, the ordered logit model in part levels the difference, with medium size correlation with both type of logit estimates.

Finally, school effects excluding school composition, contextual effects and the impact of family characteristics at the individual level are compared on Graph 1. The approach is is somewhat different here than above. The individual level effects are depicted by the estimated schooling probabilities for students with different parental background. School effects are represented by the average effects for the quintiles of school (with respect to school effect). Here student characteristics are assumed to be the typical case (see above) and school composition is fiixed at the average. Contextual effects are estimated for the typical student, with no additional school effect. In order to measure this effect, five types of school composition is defined in

Table 6 (close to the 10^{th}, 25^{th}, 50^{th}, 75^{th} and 90^{th} percentiles), and estimated probabilities were calculated for these cases. The results show, that the family characteristics at the individual level dominate both the contextual and the additional school effects. The latter two seems to have a similar magnitude. This suggest, that inequalities between schools a much smaller than inequalities of family background. Though segregation is a dangerous tendency in Hungarian public education, its adverse effect seems to be smaller than the ubiquitous inability of schools to compensate the disadvantages of family background.

6. Conclusion

The paper analysed school effects estimated from secondary schooling data. The most importan conclusion is, that estimation method and the technique of generating interpretable measures matters a lot. The estimated variants of the school effects are sometimes produce rather different results. Both distinct definitions of the outcome to be analysed and dealing with contextual effects are crucial. Since the estimated model is essentially nonlinear in nature, the interpretation of the results requires carefully designed comparisions. Measures based on decisions on different subsets of the secondary schooling options seem to represent distinct dimensions of the school effects in the Hungarian case. Nevertheless, when standardised test results are not available, the analysis of school continuation data can provide sensible results, though for interpretation of these special caution is needed.

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Tables and graphs

Table 1 Estimates of secondary school choice with school specific random intercepts

	Ordered logit +			Logit (GSS/VSS v TS)			Logit (GSS v VSS)		
	coefficient	Std. error		coefficient	Std. error		coefficient	Std. error	
individual level variables									
mother's education									
primary school	1,1042	0,0275	***	-1,1324	0,0313	***	-0,5840	0,0431	***
technical school	0,6763	0,0217	***	-0,6825	0,0267	***	-0,5500	0,0311	***
college	-0,6355	0,0266	***	0,6835	0,0489	***	0,5975	0,0302	***
university	-0,8580	0,0471	***	0,8192	0,1042	***	0,8654	0,0514	***
missing	0,4690	0,0517	***	-0,6416	0,0614	***	-0,0634	0,0735	
father's education									
primary school	0,9716	0,0344	***	-0,9079	0,0389	***	-0,6165	0,0571	***
technical school	0,4032	0,0199	***	-0,3306	0,0261	***	-0,4417	0,0265	***
college	-0,4115	0,0320	***	0,5056	0,0595	***	0,3841	0,0363	***
university	-0,7250	0,0415	***	0,7872	0,0910	***	0,7273	0,0454	***
missing	0,3797	0,0387	***	-0,3665	0,0492	***	-0,3313	0,0530	***
parent(s) unemployed in previous year	0,4306	0,0193	***	-0,5277	0,0231	***	-0,1461	0,0276	***
learning foreign language outside school	-0,6577	0,0261	***	0,8250	0,0468	***	0,5410	0,0298	***
gender (male=1)	0,8316	0,0162	***	-0,7647	0,0214	***	-0,8009	0,0216	***
contextual variables									
mother's education									
primary school	0,3899	0,1832	**	-0,7384	0,2067	***	0,4795	0,2664	*
technical school	0,1265	0,1614		-0,2767	0,1845		-0,0556	0,2330	
college	-1,0676	0,2337	***	0,6811	0,2785	**	1,4612	0,3211	***
university	-2,1908	0,4012	***	1,3980	0,5133	***	2,7120	0,5367	***
father's education									
primary school	0,7061	0,2348	***	-0,7587	0,2633	***	-0,1010	0,3500	
technical school	0,0045	0,1490		-0,1193	0,1714		0,1613	0,2109	
college	-0,1591	0,2901		0,6004	0,3512	*	-0,1926	0,3959	
university	-1,1465	0,3455	***	1,4963	0,4392	***	0,9883	0,4605	**
parent(s) unemployed in previous year	-0,8838	0,1158	***	0,7654	0,1313	***	0,9884	0,1657	***
learning foreign language outside school	0,2330	0,1678		-0,2409	0,2008		-0,1723	0,2273	
gender (male=1)	0,5132	0,1250	***	-0,5361	0,1450	***	-0,2792	0,1772	
Budapest dummy	0,0376	0,0502		0,2639	0,0651	***	-0,2228	0,0665	***
constant	-			2,3049	0,1337	***	-0,6363	0,1578	***
cut-off point 1	-0,4136	0,1137	***	-			-		
cut-off point 2	2,1384	0,1141	***	-			-		
variance of the school specific random intercept	0,1601	0,0101	***	0,1724	0,0134	***	0,3055	0,0196	***
log likelihood	-59261,97			-30034,15			-28609,27		
N (student)	66165			66165			49792		
N (school)	1903			1903			1878		

***: significant at the 1% level, **: significant at the 5% level, *: significant at the 10% level
+: note that in the ordered logit specification the three outcomes are in a reverse order (GSS=1, VSS=2, TS=3) compared to the logit specifications

Table 2 The distribution of the school specific random effects compared to the average (zero) at 5% level of significance, %

	Ordered logit	Logit (TS v GSS/VSS)	Logit (GSS v VSS)
overall			
below the average	16,4	13,0	16,6
not different from the average	66,6	76,4	65,4
above the average	17,0	10,7	17,9
Budapest			
below the average	15,5	10,3	25,2
not different from the average	67,1	82,6	51,0
above the average	17,4	7,1	23,9
Towns			
below the average	24,7	18,1	22,7
not different from the average	52,8	66,8	52,6
above the average	22,5	15,1	24,7
Villages			
below the average	11,6	10,3	11,7
not different from the average	74,9	81,2	75,3
above the average	13,6	8,5	13,0

Table 6 Defining school types by the contextual variables, %

	type 1	type 2	type 3	type 4	type 5
mother's education	20	15	10	0	0
primary school	50	40	30	15	0
technical school	30	40	40	55	60
secondary school	0	5	10	15	20
college	0	0	10	15	20
university					
father's education	20	15	10	0	0
primary school	50	40	30	15	0
technical school	30	40	40	55	60
secondary school	0	5	10	15	20
college	0	0	10	15	20
university	45	30	20	15	10
parent(s) unemployed in previous year	0	5	10	20	25
learning foreign language outside school	50	50	50	50	50
gender (male=1)	20	15	10	0	0

Table 3 Interquartile range and difference of 90th and 10th precentiles of the estimated school effects

	logit				ordered logit			
	GSS v VSS		GSS/VSS v TS		GSS v VSS		GSS/VSS v TS	
	no context. eff.	incl. context. eff.	no context. eff.	incl. context. eff.	no context. eff.	incl. context. eff.	no context. eff.	incl. context. eff.
interquartile range								
overall	0,1233	0,1448	0,0411	0,0702	0,0762	0,1014	0,0432	0,0642
Budapest	0,1414	0,1756	0,0310	0,0569	0,0665	0,1255	0,0378	0,0557
town	0,1312	0,1529	0,0461	0,0679	0,0849	0,1086	0,0478	0,0603
village	0,1150	0,1281	0,0386	0,0583	0,0716	0,0814	0,0409	0,0586
difference of 90th and 10th precentiles								
overall	0,2343	0,2713	0,0796	0,1351	0,1450	0,1948	0,0827	0,1212
Budapest	0,2484	0,3930	0,0707	0,1156	0,1406	0,2889	0,0800	0,1118
town	0,2633	0,2832	0,0913	0,1387	0,1764	0,2248	0,1002	0,1251
village	0,2168	0,2531	0,0732	0,1147	0,1326	0,1572	0,0763	0,1114

Table 4 Tha variances of the estimated school effects on the estimated probabilites of secondary schooling

	logit				ordered logit			
	GSS v VSS		GSS/VSS v TS		GSS v VSS		GSS/VSS v TS	
	no context. eff.	incl. context. eff.	no context. eff.	incl. context. eff.	no context. eff.	incl. context. eff.	no context. eff.	incl. context. eff.
overall	0,0085	0,0125	0,0011	0,0030	0,0034	0,0079	0,0011	0,0025
Budapest	0,0101	0,0243	0,0009	0,0022	0,0035	0,0160	0,0010	0,0021
town	0,0103	0,0134	0,0014	0,0030	0,0047	0,0088	0,0015	0,0026
village	0,0072	0,0095	0,0009	0,0023	0,0027	0,0043	0,0009	0,0021

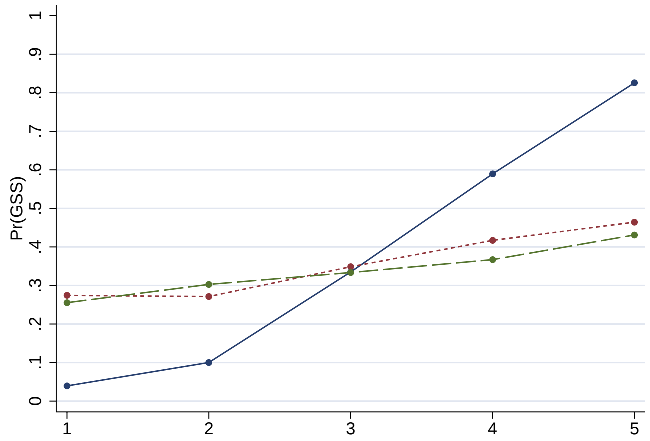
Table 5 The correlation matrix of the estimated school effects

			logit				ordered logit			
			GSS v VSS		GSS/VSS v TS		GSS v VSS		GSS/VSS v TS	
			no context. eff.	incl. context. eff.	no context. eff.	incl. context. eff.	no context. eff.	incl. context. eff.	no context. eff.	incl. context. eff.
logit	GSS v VSS	no context. eff.	1,00							
		incl. context. eff.	0,81	1,00						
	GSS/VSS v TS	no context. eff.	0,08	0,07	1,00					
		incl. context. eff.	0,04 +	0,33	0,64	1,00				
ordered logit	GSS v VSS	no context. eff.	0,68	0,55	0,75	0,48	1,00			
		incl. context. eff.	0,45	0,75	0,47	0,83	0,64	1,00		
	GSS/VSS v TS	no context. eff.	0,65	0,51	0,79	0,49	0,97	0,60	1,00	
		incl. context. eff.	0,43	0,68	0,57	0,91	0,66	0,93	0,67	1,00

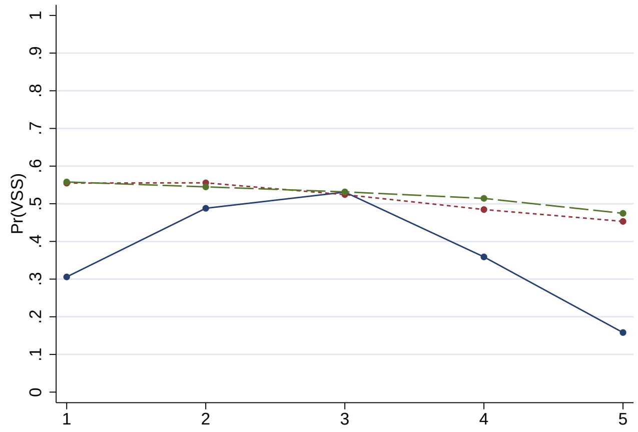
+: significant at 10% level. All the other correlation coefficients are significant at 1% level.

Graph 1 The estimated effect of family background at the individual and the contextual level and the lower bound school effect on secondary schooling (calculated from the ordered logit estimation of Table 1)

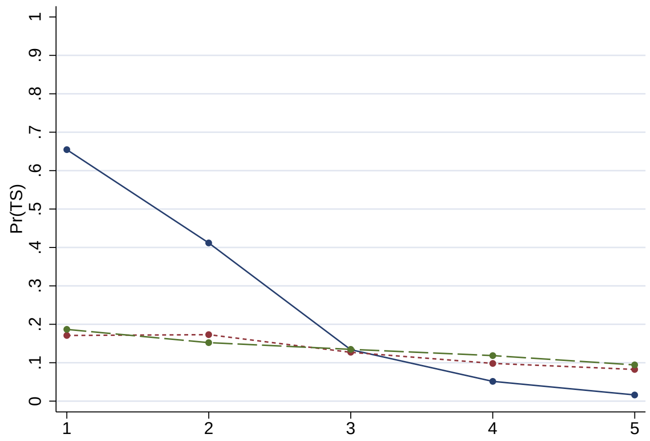
a. probability of general secondary school



b. probability of vocational secondary school



c. probability of technical school



________ : family backround, individual level (parental education)

- - - - - - - : family background, contextual level (see Table 6)

__ __ __ : lower bound school effects (quintiles)