

The long term effects of early selection – a quasi natural policy experiment from Hungary

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Using a unique institutional change this paper provides causal estimates on the long-term effects of early educational selection. During the post-socialist transition the structure of the Hungarian education system changed gradually as new early-selective tracks were established. These elite academic tracks have cream-skimmed the best students at ages 10 and 12 - as opposed to the typical age of first selection at age 14 - in order to provide better education and thus better employment chances for the selected. Utilizing the spatial and time variance in the establishment of these early-selective tracks we provide difference-in-difference estimates on the effect of these tracks on probability of employment and other outcomes. Using the 2011 Census data we estimate time and settlement fixed-effect and event-

study models. The paper contributes to the literature in two ways. Firstly, contrary to previous policy evaluations on the effect of comprehensive, de-tracking school reforms we identify the effect from a re-tracking policy, which made the school system more selective. Secondly, the establishment of the early selective tracks did not overlap with other large-scale educational policy changes, which allows for an accurate identification of the effects of early educational tracking. Our results show that contrary to expectations early selection has no effect on the employment and unemployment chances or on tertiary participation probabilities of the young adults. The effects are precisely estimated zeros.

Acknowledgment: Thank you for the useful comments of prof. Daniel Horn and prof. Gabor Kézdi.

INTRODUCTION

During the last 50 decades European countries tended to implement educational policy reforms, which aimed to turn their public education system into more comprehensive. The poli-

cies initiated introduced national curricula, raised the age of compulsory schooling or decreased the first age of tracking into different types of classes or schools. The idea behind these policy reforms was to decrease inequality among students by providing the same opportunities to all children. Sweden, Norway and Finland, were among the first to introduce such policies between 1950 and 1970. Lately Poland and Romania followed the example of the Scandinavian countries and formed their public school system more uniform.

Recently, more and more researches investigate the potential effects of these educational policies. They measure the impact of changing the first age of tracking on academic achievement, university attainment or future earnings. The results of Meghir and Palme (2005), Aakvik Salvanes and Vaage (2010) and Kerr, Pekkarinnen and Uusitalo (2012) regarding academic achievement show quite similar effect. Delaying the age of tracking have positive effect on average test scores through increasing the achievement of students with lower socio economic status and not significantly changing the outcomes of those with higher socio economic status. However, the impact on university attainment and future earnings are less straightforward. While Meghir and Palme (2005) found positive effect on the earnings of students with low socio economic status, Malamud and Pop-Eleches (2010, 2011) found no effect either on university attainment or on labor market outcomes. Naturally the question arises, whether tracking in itself can improve the achievement of the selected children? Abdulkadiroglu, Angrist and Pathak (2011) showed that high achieving students would have performed well without the services of a tracking school and Dustman, Puhani and Schönberg (2014) also found no evidence that more advanced tracks could lead to more favorable long-term labor outcomes.

The structural change of the Hungarian education system during the 1990's is very suitable for investigating the causal effect of the early selection because of its re-tracking feature. While the traditional school system had selected the students on secondary level only at age 14, the new education policies allowed selection at age 10 and 12 from 1989. Children attended selective tracks started their academic secondary education four or two years earlier than the others. The schools changing their structure could select the most able students by taking rigorous admission requirements whereas the unchanged primary schools were left behind by their most talented students. This selection can influence the future achievement of both groups (tracking and non-tracking pupils) through peer effects or the moving of the best teachers. Horn (2013) found that early selective academic tracks improve the test scores of their students but harms those who are left behind. However, we know little about the persistence of these differences. While early selective academic tracks might be beneficial in increasing test scores of students it is not obvious that differences remain at labor market entrance.

To a large extent, previous literature investigated the effect of the comprehensive school reform as opposed to my thesis which aims to measure the influence of a policy allowing early selection. Moreover, I investigate the long-term impact of tracking policy on labor market outcomes, which is a more controversial part of these policies. In order to test the effect of early tracking on labor market outcomes (the probability of employment) I use a unique panel database. I have matched the exact place and time of establishment of the early selective tracks with the individual data from the Hungarian Census 2011, and the TSTAR database which contains information about the settlements. Looking at cohorts between 1976 and 1989, who

were potentially eligible to attend the first classes of tracking schools, and assuming that the time of the tracking schools' establishment is independent from the settlements' characteristics which influence later labor market outcomes, I can estimate the intention to treat effect of early tracking. In other words, I assume that the probability of a student entering an early-selective track – which is 0 if there were no such track at that time and place, when other students were entering early selective tracks, and 1 if s/he had the option to enter – does not correlate with the individual unobserved characteristics, and thus can only have an effect on her/his labor market success through early selection. By using time and place fixed effects this assumption allows us to see the labor market differences resulting from the opportunity to go to a tracking school (intention to treat effect). The preliminary results show that tracking schools had no significant effect on the unemployment, employment and tertiary education participation rate of the individuals who lived in settlements where an early selective school functioned compared to those who lived in the same settlement before the opening of the school.

The paper is structured as follows: First, I provide an overview of the relevant literature and the key policy and measurement issues. I present the dilemma of tracking versus comprehensive schools and discuss the results of connecting evaluations. Then I describe the Hungarian tracking school establishment process focusing on the time and place variation of it. After reporting the database to be analyzed I outline the empirical research design. Then I present the results of the empirical research and my conclusions derived from that. Finally, I discuss the limits of my research and present some ideas for further thinking.

INTERNATIONAL AND HUNGARIAN EVIDENCE ON EARLY STUDENT SELECTION

previous literature

The purpose of this study to measure the effect of early student selection on labor market outcomes by examining the Hungarian educational reform during the post-socialist transition. The paper contributes to the literature in two ways. Firstly, contrary to previous policy evaluations on the effect of comprehensive, de-tracking school reforms we identify the effect from a re-tracking policy, which made the school system more selective. Secondly, the establishment of the early selective tracks did not overlap with other large-scale educational policy changes, which allows for an accurate identification of the effects of early educational tracking. In Hungary, Horn (2013) investigated the short-term effect of early selection as he measured the test score differences between tracking and non-tracking students. His findings showed that while students attending tracking schools perform better due to the school the other children perform worse than without tracking. My paper develops Horn's (2013) findings by examining the long term effects.

Educational tracking, also called streaming and phasing, separates students into different groups according to their skills and abilities on a certain level of education. These groups can be different classes or different schools depending on the specific system. The idea behind

tracking is that the similar skills and knowledge students have the higher effectiveness can be reached during their education. However, this idea is heavily criticized in the last decades due to the raising inequality issues. It is possible that the more ambitious students can perform better when they are taught together and separately from the less motivated ones but the impact of such policy on the other students can be detrimental. Furthermore, the question arises what is the basis of the selection in real. Since many educational policies targeted to change the existing tracking system during the last decade their empirical investigations attempt to shed light on its effects and the mechanisms behind it. However, the results are still ambiguous.

A number of papers analyzed the effect of de-tracking, when countries postponed the selection of children to a later age thereby made the school system more comprehensive. Meghir and Palme (2005) examined the effect of the Swedish school reform by comparing the students who were affected and who were not affected by the policy due to the different time and place of the implementation. They found that the more comprehensive system increased the schooling of students with unskilled father and higher ability beyond the new compulsory level. Moreover, their results showed positive effect on their earnings while slightly negative effects on the earnings of students with higher-skilled father. Kerr, Pekkarinen and Uusitalo (2013) used the same feature of the Finnish comprehensive school reform to measure its impact on the test score of the children. They found that the reform was beneficial at most for students, whose parents had basic education or low income, and on average had a small positive effect only on verbal score, but did not have effect on arithmetic or logical reasoning scores. Hall (2012) presented causal evidence on the effect of de-tracking from Sweden. She found that the policy had positive effect on the number of children who obtained upper secondary school but could not increase the participation in tertiary education. Nevertheless, Malamud and Pop-Eleches (2010) investigated the Romanian comprehensive school reform and found no significant impact of it. My paper contributes these papers by analyzing the opposite direction policy: what is the effect of introducing tracking in a comprehensive system and making it more selective?

While the Hungarian policy was not mandatory and did not covered all students directly, as opposed to the comprehensive policies whereas all students were taught by the new curricula, the influence of losing the best performing peers or the best teachers can have indirect effect on the students left in general schools. Hence lowering the age of selection is very rare phenomenon, literature about this kind of changes is very scarce. Piopiunik (2013) examined the effect of a school policy in German state of Bavaria. This policy moved the timing of tracking in low- and middle-track schools from grade 6 to 4. The author compared the achievement of the students before and after the reform, with states where the policy was not introduced and with students who were not affected (because they were on higher a higher track). He found that the test scores of the students decreased on both tracks. Moreover, the dispersion of the performance increased as well, suggesting greater inequality of opportunity. However, the long term effects of the earlier selection could not have been measured.

Horn (2013) analyzed the effect of early selection on performances and equality of opportunity in Hungary for tracking and non-tracking students. He used the National Assessment of

Basic Competences data from 2008 and 2010. Comparing the performance improvement between 2008 and 2010 he found that 6-yr-academic tracks seems to have higher value added than general academic tracks even if controlling for socio economic status, previous performance and schools fixed effects. Furthermore, students with better socio economic status have higher chances to go to the tracking schools even if it is controlled for their previous performance. However, these results suffer from endogeneity problems and it seems that effect of the selection depends on cohorts and subjects. My paper contributes to Horn (2013) because it question is whether this advantage in performance remains on the long run and appears on the labor market. In this paper the reduced form of this effect is observed because we only know the exact time and place of the selection but not the selected children. However, the panel database, including data for 10 years of the policy, makes it possible to measure the causal effect of the policy.

The potential channels of student selection

The selection of the students affect not only the students who are selected but also that one who attend the general schools because of two reasons: peer effects and teacher quality. When the high-achiever students are selected from the school the other students can lose motivation, competition, etc. While empirical evidence are not very sound many of them showed that peers matter (Hoxby (2000), Hanushek, Kain, Markman and Rivkin (2003) or Duflo, Dupas, Kremer (2011)). Many of these papers found that better performer students can improve from the selection but the peers left behind perform worse after the selection. This effect can aggregate on the long run and cause huge differences among the different tracks. The teacher quality can also depend on the quality of the students. Varga (2011) found that in Hungary the schools where the share of students with lower socio economic status and worse grades is higher employ more likely not properly trained teachers. Rivkin, Hanushek and Kain (2005) found that the quality of the teachers and the school have determining role in the performance of the students.

THE STRUCTURAL CHANGE OF THE HUNGARIAN EDUCATIONAL SYSTEM DURING THE 1990'S

The policy reform

In this chapter I clarify the process of the Hungarian educational system's structural change, the potential channels of its impact. The Hungarian educational reform was unusual in the sense that the schools and the local governments could decide about its implementation. For this reason the early-selective schools were established gradually throughout the 1990's. However, knowing the exact time and the place of these schools we can compare the students before and after the policy implementation. While the policy did not covered all children in the country because it was introduced only in the larger towns, its effect is not negligible for two reasons. Firstly, almost 8 percent of the students had the opportunity to leave their schools and learn together with their high-achiever peers. Secondly, the policy had impact not only on

those students who attended the selective schools, but also on those students who left their most motivated peers.

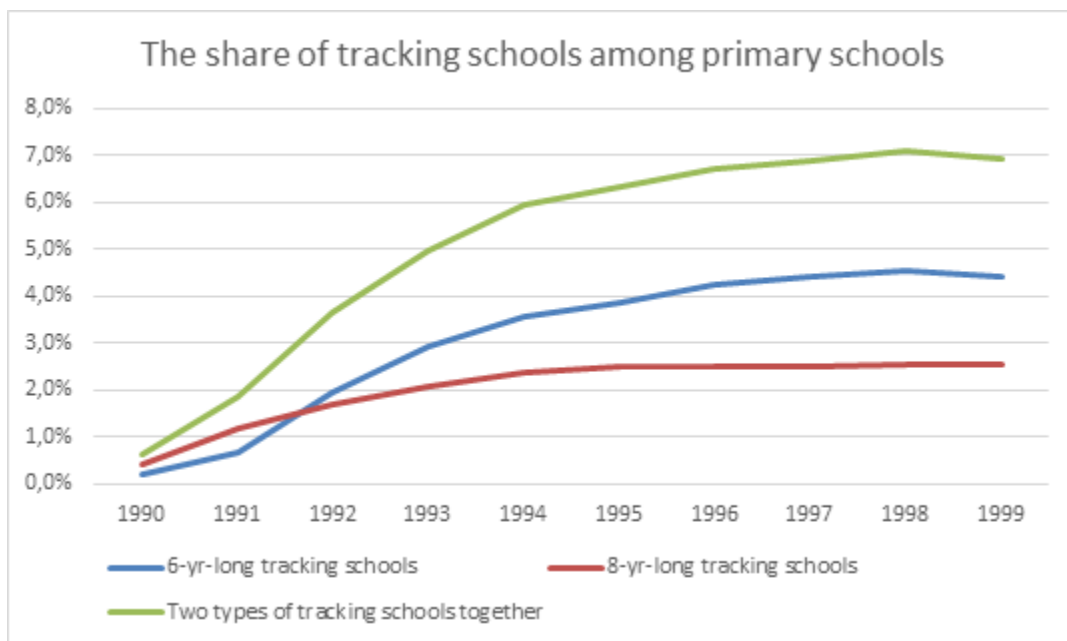
Until the mid-1980's the socialist educational system was highly centralized and uniformed in Hungary. The state owned schools adopted a centrally defined curriculum which was uniform all over the country. Children were required to start school at the age of 6 when they reached school maturity and learned in the same institution until the 8th grade at age 14 which was the end of the compulsory school attendance. Gradually, children had to attend the school assigned to their living place, thus the school system was comprehensive on the primary level and selection among students was carried out only at the secondary level at age 14. On secondary level students could choose among two tracks according to the content of the curriculum. One of them was an academic track (gimnázium) which prepared students for higher education and ended with a school leaving exam which was the requirement of further education. The other was a vocational track, called vocational apprentice school (szakmunkásképző), and provided professional training and practical instruction according to specific occupations as carpenter, mechanic or painter.

This system became more and more decentralized on the level of decision making, curriculum and school structure from the mid 1980's Halász et al. (2001). The Public Education Act of 1985 made it possible to establish new forms of secondary schools depending from the permission of the Ministry, this way widening institutional independence from the centrally defined educational path. As one of these new forms plans about the early selective, eight and six-year-long secondary grammar schools were developed by certain schools assigned by the government. The concept of the eight-year-long secondary school was not new in Hungary because it worked before the Second World War as opposed to the six-year-long tracking school which was an innovation. These early selective secondary school types were aimed to prepare students better for the higher education and give them more detailed and deepened academic knowledge, consequently they targeted the most talented and ambitious students. Moreover, they introduced entrance exams through which they could target the best students. The early selective tracks functioned next to the traditional primary and secondary schools. Thereby the students could decide to leave the general primary school at age 10 or 12 and attend another tracking class or stay and go to the secondary schools at age 14.

By the time the concept of the early selective schools were ready to be implemented, the decision about education got decentralized. The Act LXV of 1990 On Local Governments made the local governments the official owners of public schools and gave them the right to decide about the concerning policy questions such as school structure, curriculum, students' selection etc. Moreover, the Act XXXII of 1991 on Settlement of Ownership of Former Real Properties of the Churches provided the opportunity to get back the control over the institutions which were nationalized after 1946 thereby making the composition of school maintainers more fragmented. As a result, the establishment of tracking schools was practically free in the beginning of the 1990's and the mushrooming of the early selective schools became a tendency. The applying students had to fulfill strict admission requirements based on their previous learning achievement and admission exams. Figure 1. shows this tendency and the proportion of tracking schools to primary schools. In the first time, mostly 8-yr-long secondary schools

were established as opposed to 1992, from which the 6-yr-long schools became dominant. The peak time of this trend was in this year, when 61 tracking schools were established altogether. The trend of early selective schools had faded and the number tracking schools stabilized for 1999. The tendency did not focus on certain areas of the country but it was typical everywhere.

Figure 1. The share of tracking schools among the primary schools throughout the 1990's. Source: Own database, TSTAR



Notes: Data contain only those primary schools which have lower secondary level, because children are selected on this level.

In order to see the relevance of the reform I try to approximate the population concerned. Hence I do not have data about the class sizes of tracking schools I analyzed the amount of the selected population by using the number of relevant schools. I compare the number of tracking schools to the primary schools with lower secondary level, because tracking influences the children who are left in the primary schools and those who start to attend tracking schools. Taking together the amount of the 6- and 8-yr-long tracking schools they reached the 8% of the primary schools and their level became stable for 1997. This data shows us that a not negligible part of the children could choose (and can choose still today) to leave the peers and learn together only with the other high achiever students.

Balogh (2001) specifies the details of educational policy-changes which favored the implementation of common standards instead of the special structures, and this way led to the fade of the new tracking schools' establishment. First, the Educational Act in 1995 declared that the number of classes cannot be increased in primary and secondary schools without the permission of the Ministry out Budapest's boarder. As a consequence of the generally decreasing number of children in Hungary, the tracking schools caused serious tensions between the local tracking and non-tracking schools. When I surveyed the schools about the tracking school establishment, I also asked those schools which have ceased their special tracks about its rea-

sons. Many of them replied that the operating institution (the local government) forbid starting new tracking classes, because of the primary school's pressure. Second, the curriculum got unified again in 1995 and 2000, when the National Core Curriculum and the framework curricula were adopted. These curricula standardized the guidelines and the content of the education for the public schools (and for some extent for the church or private schools as well). Hence the tracking schools had different curriculum than the newly created common guidelines, they had to be revised and the establishment of new ones was further strengthened.

DATA

The Census data from 2011 – Eligible groups of the policy

Due to the gradual implementation of the policy and the lack of precise information about the place of attended school I analyze the characteristics of settlements and cohorts in order to identify the treatment and control group of the policy. The 2011 Census data contains information about the relocation of the individuals therefore I can approximate the place of attended schools. I surveyed the schools about the accurate time of the tracking school establishment on the phone, therefore I can identify which cohorts were covered by the policy. Combining these data with the 2011 Census data we can select those children who were influenced by the selection. Those people who lived in a settlement where one or more tracking schools were established and were in tracking school starting age after the local implementation of the policy are the treatment group of this study, while those who were too old to attend these schools are the control group. The descriptive statistics indicated that significant differences characterize the settlements which did and which did not establish tracking schools. Moreover, there are dissimilarities among tracking settlements which introduced the selection earlier and later. Hence fixed effects will be used in the estimations to eliminate the potential endogeneity.

As the time of the policy implementation was between 1989 and 1999 I restricted the sample to the cohorts between May 31, 1976 and May 31, 1989. The first 6-yr-long tracking school were established in 1989, consequently the first cohorts to be influenced by the policy were those who were born after May 31, 1976. The individuals who belongs to the 1999 cohorts are included in the analysis because they are the youngest ones who could have been on the job market in 2011. I used the cutoff date May 31 because the Education Act of 1985 declared that children reach school maturity in the year of their 6th birthday. If they reach the sixth age after May 31, they are suggested to start the school in the next year. Approximately 1.75 million people belongs to these cohorts.

The Census does not include data about the individuals' exact place of schooling. In order to have an approximation of the school location I used the information about moving from one residential place to another one. We have exact information about the place of birth, the current living place and the former residential location. As the Hungarian people are not very mobile (Cseres-Gergely, 2003) I restricted my sample on some patterns. I kept only those who have never moved and who moved only in their place of birth. I supposed that if the place of

birth and former location are the same, and moved only after the age 18, the individual went on school in the birth location. I also included in the sample those who moved to their current living place before the age of 10. After excluding those who do not correspond with the groups identified these ways 1023439 people remained in the sample.

Table 1. The amount and the characteristics of the different groups in population by the status of relocation.

	Number of people	Average Age	Share of Unemployment	Share of Employment	Share of Inactive	Average Schooling
Unidentified	540263	29.39	0.10	0.68	0.22	16.3
No relocation	393081	29.52	0.12	0.66	0.21	14.19
Moving only in the place of birth	307844	27.37	0.14	0.69	0.18	14.87
Moved to the current place of living after age of 10	220959	27.37	0.14	0.68	0.18	15.28
The place of birth and the former place of location were the same and moved after the age 18	290175	29.24	0.08	0.69	0.23	16.15
Total	1752322	28.578	0.11	0.67	0.21	15.42

Source: Hungarian Census, 2011

I checked the characteristics of the people who were dropped out from the sample in order to see whether there is any systematic differences between that group and the remaining sample. Table 1. shows the characteristics of the different groups by the status of resettlement. The unidentified group has somewhat higher years of schooling than all the other groups, the second highest employment rate and inactivity rate and the second lowest unemployment rate. Altogether, people who cannot be identified regarding their potential place of school have quite similar attributes than the other groups.

The characteristics of the settlements to be compared

As a consequence of the lack of data about the socio economic status of the individuals the characteristics of the towns have to be analyzed. The specific characteristics of the settlements could cause endogeneity by influencing the resident children's future labor market outcomes. The individuals who lived in settlements where there were no primary schools were left out from the analysis because I could not identify the settlements where they probably went on school. People living in Budapest are hard to be identified regarding to their potential school place because they are more likely to enter their living district to attend a preferable school. As a result they are not involved in the analysis. In Table 2. I compared the remaining settlements which have ever established early selective tracks compared to those which have not established, have very different characteristics before the treatment, in 1990. My expectation was that tracking schools were established in larger and wealthier settlements because tracks were made mostly by secondary grammar schools, which generally can be found in larger cities. This piece of statistic is relevant from the point of view of my research design since I exclude the

non-tracking settlements from my analysis because of their dissimilarity and focus only on the tracking settlements.

Table 2. The population and tax income difference between tracking and non-tracking settlements in 1990.

Municipalities	variable	mean	min	max	N
Non-Tracking	Average Population Size	1816	20	34583	2372
	Per Capita Personal Income Tax Base (logarithm)	51.58	6.52	107.16	2372
Tracking	Average Population Size	27182	2347	212110	125
	Per Capita Personal Income Tax Base (logarithm)	70.91	43.47	125.16	125

Source: TSTAR, own database

The structural change of the schools covered the whole country because they were established proportionally in all part of the country (Figure 1. in Appendix). While in space there is no difference between the schools, in other characteristics great dissimilarities can be found between those who adopted structural change earlier and later. Table 3. shows us that out of the 125 tracking settlements (excluding those 14 having tracking school already in 1990), 67 were established in or before the year 1992 and 58 were after. Because the tracking school establishment was the autonomous choice of the schools we can expect, that there is difference between the early and late adopters which is supported by the data. The early adopters are in larger and richer settlements while the late adopters are in smaller and poorer ones. This difference shows us a tendency that after the more developed areas the less developed ones also created tracking schools. Taking into consideration that there is systematic difference between the early and late adopters it is essential to control for them during the later estimations.

Table 3. The population and tax income difference between early and late adopter tracking settlements in 1990.

Tracking Settlements	Variable	mean	min	max	N
Early adopters	Average Population Size	35024	2347	212110	67
	Per Capita Personal Income Tax Base (log)	73.13	50.00	125.16	67
Late adopters	Average Population Size	18124	3480	63459	58
	Per Capita Personal Income Tax Base (log)	68.34	43.47	104.46	58

Source: TSTAR, Own database

We saw that settlements which have ever and never established tracking schools are systematically different and we also found dissimilarity between early and late adopters. However, as we know the exact time of the early selective schools' establishment, we can separate the population who could have been affected by the reform and those who couldn't due to their age. I assume that people living in the same town but belonging to different cohorts were influenced by the same town specific features which could affect their future labor market outcomes except the existence of the selection. If a tracking school was founded in 1993, the 1992 and 1993 cohorts are different because of the tracking school but similar otherwise. For this

reason people who lived in the tracking school towns in the countryside at school age and belong to the 1977 to 1989 cohorts are the eligible group of the school structural change policy. There is 509162 such people in the sample.

After giving the precise definition of the eligible group the treatment and the control group can be divided. Table 4. shows the size of the remaining sample throughout the years by the treatment and control groups. The treatment group is divided into three groups regarding the type of tracking school operating in the settlement. The third group involves those people who lived in a settlements where both type of the tracking schools were run in the same time. 37 percent of the sample is in the control group because they are too old to be affected by the student selection and 63 percent is in the treatment group. The 1977, 1978 and 1979 cohorts does not contain the 8-yr-long group because these people were 10 years old (in the age of 8-yr-long tracking school starting age) when these school were not founded yet. Similarly, the 1988 and 1989 cohorts do not contain people belonging to the 6-yr-long group because they could start these school in 2000 and 2001.

Table 4. The treatment and the control groups of the early selection policy by the types of the local tracking schools.

	Non-Treated	6-yr-long	8-yr-long	Both	Total
1977	48018	289	0	0	48307
1978	42270	3529	0	0	45799
1979	31080	12325	0	0	43405
1980	16735	20931	3071	1311	42048
1981	8886	19837	5458	5763	39944
1982	4445	18132	7152	7621	37350
1983	2355	16877	7435	8756	35423
1984	1314	14402	7297	11319	34332
1985	844	14931	6870	12371	35016
1986	1175	14726	6794	13791	36486
1987	2808	12675	7182	13860	36525
1988	15432	0	22097	0	37529
1989	15346	0	21652	0	36998
Total	190708	148654	95008	74792	509162
Fraction	0,37	0,29	0,19	0,15	

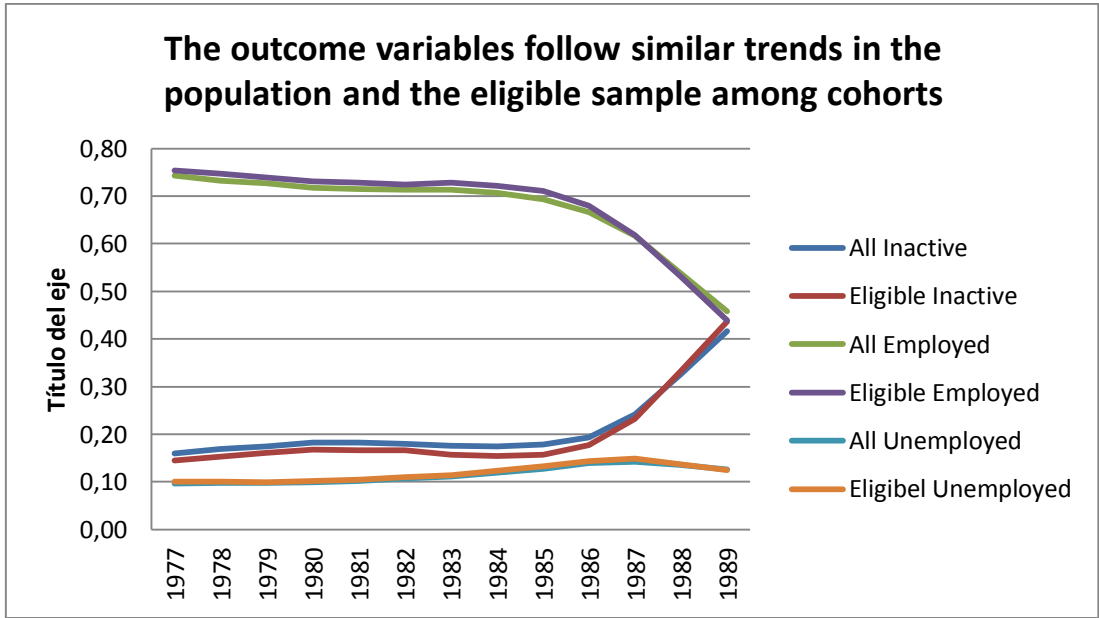
Source: Own database, Census 2011

The Outcome Variables

After dropping more than half of the population it is essential to compare the values of the outcomes between the population and the remaining sample. The outcome variables are the unemployment, the employment and the tertiary education participation rate. Figure 2. describes the inactive, employed and unemployed shares of the population and the sample. The unemployment rates of the sample and the population follow similar trends align the cohorts. However, the employment and inactivity rates slightly alter among the youngest cohorts.

Hence the younger cohorts are more likely to be students, the inactivity rate increases with time sharply. In line with the higher inactivity rate among the younger cohorts, the rate of employed individuals is lower in the sample than in the cohorts with approximately 2 percent.

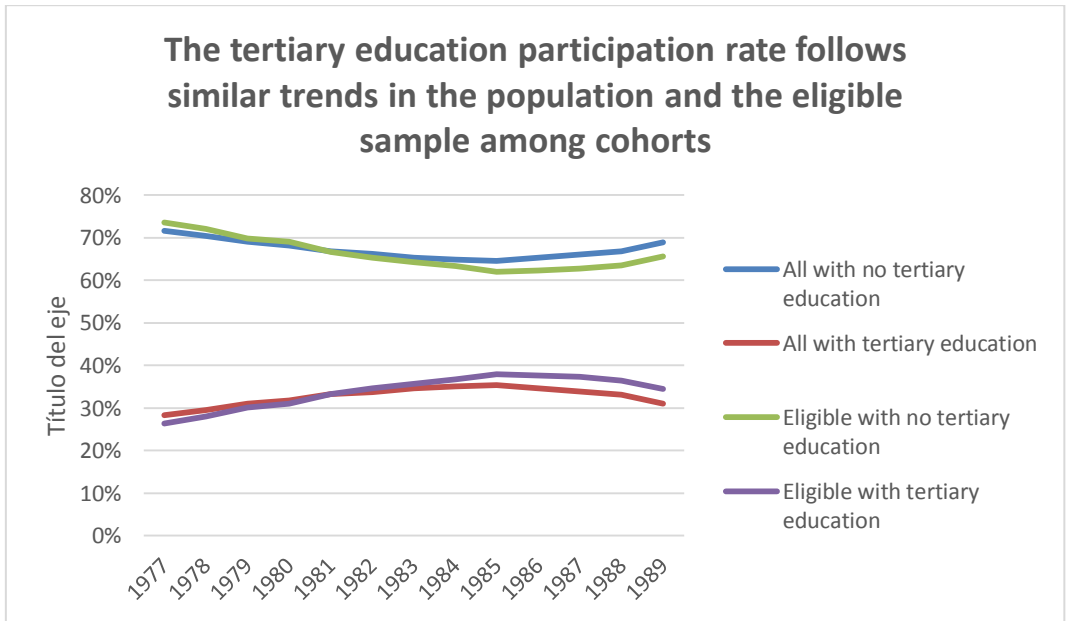
Figure 2. Comparing the outcome variables between the population and the eligible sample by cohorts.



Sources: Census 2011

Figure 3. shows the tertiary education participation rate for the whole population between 1976 and 1989. Here we can see slight differences but they are not significant. We can conclude that the outcome variable on the sample of my study follow very similar trend as the population.

Figure 3. The tertiary education participation rate follows similar trends in the population and the eligible sample among cohorts. Sources: Census 2011



4. ESTIMATING THE IMPACT OF THE REFORM ON UNEMPLOYMENT, EMPLOYMENT AND PARTICIPATION IN TERTIARY EDUCATION

4.1 IDENTIFICATION STRATEGY

To identify a causal effect we require a natural experiment which randomizes the students with the same ability into different tracks. Assuming that the establishment of the tracking schools was independent from those characteristics of the students which can influence their future labor market outcomes a quasi-natural experiment is provided. Utilizing the spatial and time variance in the establishment of these early-selective tracks I provide difference-in-differences estimates on the effect of these tracks on probability of employment and other outcomes. I first use fixed effects approach which compares the labor market outcomes in 2011 of those people who reached tracking school-starting age in the year or after the year of the tracking school's establishment with those who reached before and living in the same towns. Since the selection can have diverse effect getting away from the policy implementation event study approach is used.

I make the following assumption: the unobserved characteristics of the settlements which determine whether they establish early selective school or not are fixed in time. This assumption implies that people who belonged to cohorts started the school before and after the structural change are not systematically different regarding their future labor market outcomes. This strong exogeneity means that the establishment of tracking school is not related to any time variant unobserved heterogeneity of the settlements. By controlling for the time and settlement fixed effects, the dummy for having a tracking school at the time of starting school shows the estimated treatment effect. The fixed effect model identifies the effect of the tracking schools from those who lived in the settlement before and after the structural change. The fixed effects equations to be estimated are the following:

$$1. y_{icm} = \alpha_m + \beta Six_{cm} + \gamma_c + u_{icm}$$

$$2. y_{icm} = \alpha_m + \beta Eight_{cm} + \gamma_c + u_{icm}$$

$$3. y_{icm} = \alpha_m + \beta Six_{cm} + \delta Eight_{cm} + \gamma_c + u_{icm}$$

Where y_{icm} is the labor market outcome of individual i , who belonged to cohort c and lived in municipality m . α_m is the settlement fixed effect, γ_c is the time fixed effect and u_{icm} is the error term. In equation 1. variable Six_{cm} was 1 if the individual lived in m municipality where only 6-yr long tracking school operated and belonged cohorts c which was old enough to attend the tracking school. Six_{cm} had 0 value if no tracking school operated when the individual was 12 years old. Municipalities where 8-yr-long tracking schools operated were excluded. In equation 2. I identified T in the same way but it was 1 for 8-yr-long tracking schools. In the third equation Six and $Eight$ were 1 if individuals lived in a municipality where certain type of tracking schools existed, independently from the other type of school. This way I could incorporate all people from the sample. Clustered standard errors are used in the estimations because the standard errors are likely correlated within the groups (settlements).

The problem can arise regarding this specification that those attended the tracking schools some years after their establishment are systematically different in their future labor market outcomes than those who lived before and right after the establishment. To eliminate this problem event study methodology can be used. I generated lag and lead variables which measure the distance between the year of tracking school establishment and the year of certain individual's potential tracking-school-start. The equation to be estimated is the following:

$$4. y_{icm} = \alpha_m + \beta Six_{mc-2} + \beta Six_{mc-1} + \beta Six_{mc} + \beta Six_{mc+1} + \beta Six_{mc+2} + \beta Eight_{mc-2} + \beta Eight_{mc-1} + \beta Eight_{mc} + \beta Eight_{mc+1} + \beta Eight_{mc+2} + \gamma_c + u_{icm},$$

where Six_{mc-2} equals 1 if the individual living in municipality m was 12 years old two years before the establishment of the local tracking school. $Eight_{mc-2}$ equals 1 if the individual living in municipality m was 10 years old two years before the establishment of the local 8-yr-tracking school's establishment. The further variables can be interpreted by this scheme.

RESULTS

The Effect of the Reform on Unemployment

The estimates of the impact of the reform on unemployment are shown in Table 5. These are presented for people who were active on the labor force in 2011, hence the sample contained only 408315 observations. The columns provide the results of the equations presented above. In all equations time and municipality fixed effects are used and the standard errors are clustered by the municipalities. For the unemployment differences the results of OLS estimation are reported from equation 1. which represents the effect of the reform measured in the probability of being unemployed. The unemployment rate of the average people who lived in a municipality before the selection is about 11.86%. The tracking school variables explain very small part of the variance in employment since the value of R^2 is very low, about 0.008. The first column shows the differences between people who were in 6-yr-long tracking school age before and after the selection was introduced. The point estimation is insignificant and very close to zero with small standard errors implying that there is no significant effect of the tracking school establishment on the later unemployment rates. The results for the 8-yr-long schools and the two types together give the same results. The event study estimations show very similar results. We can see a significant alteration among those who started school after an 8-yr-long selective school was opened, but the amount of the point estimation is 0.7 percent which is economically negligible effect and it disappears for the second year. This result is quite surprising because the decline of the labor market chances of the normal school's student was expected. However it seems, that selection in that level does not have significant effect on the children.

Table 5. The impact of the reform on unemployment.

Outcome variable: Unemployment. 0 – employed 1 – unemployed						
	1)	2)	3)			4)
six	0,0026		0,0023		slag2	-0,0008
	0,0022		0,0026			0,0028
eight		-0,00283	-0,0026		slag1	0,0031
		0,00236	-0,0024			0,00261
constant	0,1186	0,1186	0,1185		slag0	-0,00024
	0,0016	0,0016	0,001567			0,002895
Municipality fixed effect	Yes	Yes	Yes		slead1	0,0037
Time fixed effect	Yes	Yes	Yes			0,002646
R2	0.008	0.008	0.008		slead2	0,0029
Number of observations	408315	408315	4083015			0,00771
Groups	139	139	139		elag2	0,001781
						0,00355
					elag1	-0,00145
						0,00267
					elag0	-0,00047
						0,00344
					elead1	0,0072
						0,003247
					elead2	0,000169
						0,003754
					cons	0,1186
						0,00162
					Municipality fixed effect	Yes
					Time fixed effect	Yes
					N	408315
					groups	139

Sources: Census 2011, Own dataset

As robustness checks I also run these regressions on two restricted sample: first I analyzed people who started school when no tracking school was available at all or only certain type of school was available. Secondly, I restricted my sample only on those who lived in a settlement where only one type of school was available ever. Table 1. and 2. also exhibit the results for these samples and the point estimations are precise zero.

The Effect of the Reform on Employment

The estimates of the impact of the reform on employment are shown in Table 6. These are presented for people who were active or inactive on the labor force in 2011, hence the sample

contains 509168 observations. The employment rate of the people who lived in an average town when selection was not employed is 75.3 percent. The tracking school variables explain a larger part of the variance in employment than the unemployment, but the R^2 is still quite small 0.038. The results lead to the same conclusion, that the early selection did not have significant impact on the employment rates of the eligible people. Moreover, the point estimates have precise zero values. The coefficients of the event study do not have significant values anywhere. The findings of the restricted samples do not show significant effects.

Table 6. The impact of the reform on unemployment.

Outcome variable: Employment. 0 – Inactive or Unemployed 1 – Employed					
	1)	2)	3)		4)
six	0,00496		0,0051	slag2	0,00542
	0,003995		0,0042		0,0039
eight		0,00017	0,00083	slag1	-0,0009
		0,0053	0,0055		0,0031
constant	0,753	0,753	0,753	slag0	0,00662
	0,0027	0,0027	0,0028		0,00377
Municipality fixed effect	Yes	Yes	Yes	slead1	0,00662
Time fixed effect	Yes	Yes	Yes		0,00365
R^2	0.038	0.038	0.038	slead2	0,00502
Number of observations	509168	509168	509168		0,00446
				elag2	0,00066
					0,00476
				elag1	0,00399
					0,00401
				elag0	0,0043
					0,00524
				elead1	0,00168
					0,00529
				elead2	-0,0019
					0,00588
				cons	0,752
				Municipality fixed effect	Yes
				Time fixed effect	Yes
				R^2	0.038
				N	509168
				groups	139

Sources: Census 2011, Own dataset

The Effect of the Reform on Participation in Tertiary Education

Finally, the tertiary education attainment is examined because the main reason of the tracking school establishment was that it can enhance the chances of successful further education submission. The outcome variable had 1 value for all people who ever participated in higher education. The tertiary education attainment rate of the people who lived in a municipality where no tracking school was available at school age is about 26%. The values for the R^2 are still very low, about 0.0065. The point estimation are precise zero again. These results indicate that as opposed to the initial purpose the structural change of the school could not improve the chances for further education of the students attended the school.

Table 7. The impact of the reform on tertiary education participation.

	1)	2)	3)		4)
six	-0,00101		-0,0014	slag2	0,00033
	0,0032		0,00322		0,0045
eight		-0,003	-0,0033	slag1	-0,0046
		0,0049	0,0049		0,0047
constant	0,26	0,262	0,2625	slag0	-0,00048
	0,0025	0,0025	0,0025		0,0046
R^2	0.0065	0.0064	0.0064	slead1	-0,00448
Municipality fixed effect	Yes	Yes	Yes		0,0036
Time fixed effect	Yes	Yes	Yes	slead2	-0,0027
Number of observations	509168	509168	509168		0,0048
				elag2	-0,0035
					0,004
				elag1	0,000418
					0,0042
				elag0	-0,005
					0,00381
				elead1	-0,00188
					0,0044
				elead2	-0,0061
					0,00413
				cons	0,263
					0,00226
				Municipality fixed effect	Yes
				Time fixed effect	Yes
				R^2	0.0063
				N	509168
				Groups of clusters	139

Sources: Census 2011, Own dataset

CONCLUSION AND FURTHER RESEARCH QUESTIONS

My paper aimed to measure the effect of the structural change of the Hungarian education system which introduced selection among students at 10 and 12 age. According to the previous literature of comprehensive school reforms (de-tracking) seemed to have positive effect on the students with lower socio economic status and slightly negative or no effect on children who were formerly selected. However, the results of this paper suggest that early selection did not have long-term effect on the population concerned. Utilizing the spatial and time variance in the establishment of these early-selective tracks we provide difference-in-difference estimates on the effect of these tracks on probability of employment and other outcomes. Using the 2011 Census data the results of the fixed effect and event-study models gave precise zero effects. This effect can be the consequence of the earlier selection in the Hungarian school system as very homogenous groups of the children are formed even in the first class of the elementary school.

In the future it is worth to analyze the different groups of the settlements by the exposure to the selection. Maybe in the settlements where more children were selected by the tracking schools (because of the low population in the town or the high number of tracking schools) higher impact can be observed. Moreover, wages can be imputed from the Hungarian Wage Survey in order to test the effect on occupational choice. Using the wages as outcomes allow us to test changes in inequality as the variance can be measured. Finally, more robustness checks are required because of the assumption about the covered cohorts and school location.

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APPENDIX

Figure 1. The distribution of tracking schools in space. Source: TSTAR and Own database

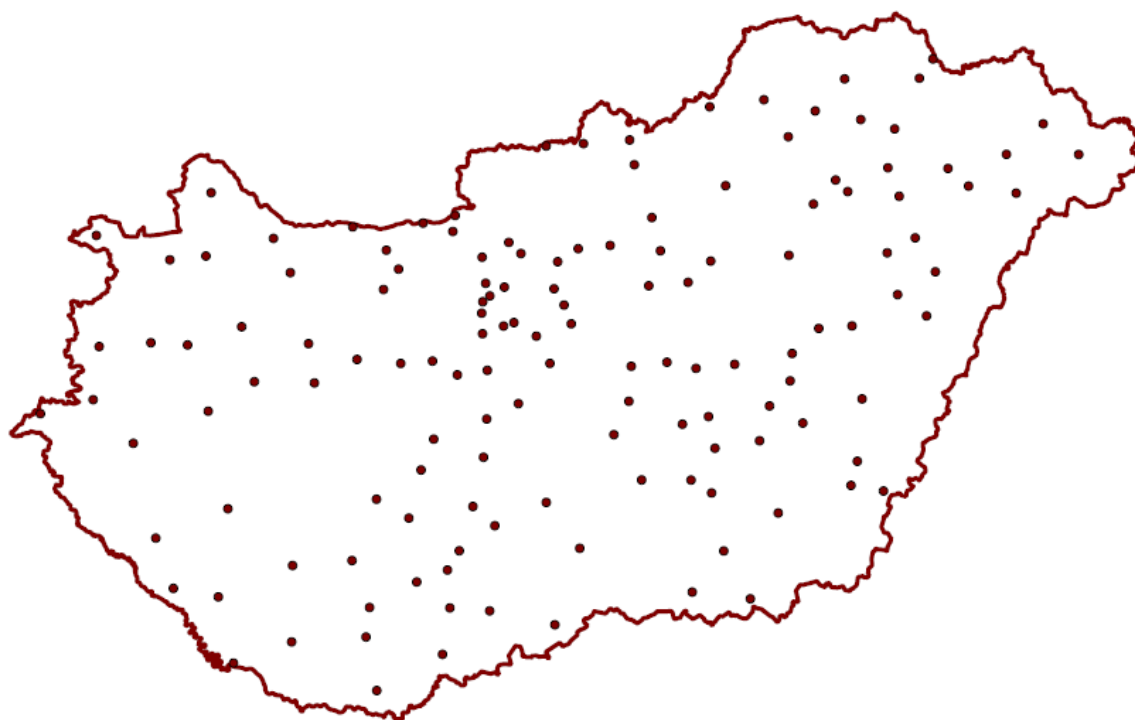


Table 1. OLS Regressions for restricted samples

Depndent variable: unemployment						
Only certain type of tracking school at school starting age			Only certain type of tracking schools			
	1)	2)		1)	2)	
six	-0,00091		six	0,000827		
	0,00245			0,0032		
eight		-0,00847	eight		-0,00118	
		0,0036			0,004	
constant	0,1209	0,1177	constant	0,1224	0,1155	
	0,00147	0,0036		0,0025	0,0026	
Municipality fixed effect	Yes	Yes	Municipality fixed effect	Yes	Yes	
Time fixed effect	Yes	Yes	Time fixed effect	Yes	Yes	
R2	0.0058	0.0112	R2	0.008	0.0073	
Number of observations	277108	223321	Number of observations	164007	77576	
Groups of clusters	139	139	Groups of clusters	78	33	

Table 2. OLS Regressions for restricted samples

Dependent variable: employment					
Only certain type of tracking school at school starting age			Only certain type of tracking schools		
	1)	2)		1)	2)
six	0,00726		six	0,0095	
	0,0039			0,0053	
eight		-0,0026	eight		0,00595
		0,00852			0,0091
constant	0,7496	0,7562	constant	0,753	0,756
	0,00279	0,0037		0,0027	0,0035
Municipality fixed effect	Yes	Yes	Municipality fixed effect	Yes	Yes
Time fixed effect	Yes	Yes	Time fixed effect	Yes	Yes
Number of observations	339362	285716	Number of observations	205435	97375

Table 3. OLS Regressions for restricted samples

Dependent variable: Tertiary education participation					
Only certain type of tracking school at school starting age			Only certain type of tracking schools		
	1)	2)		1)	2)
six	-0,0041		six	-0,0079	
	0,00377			0,0049	
eight		-0,00193	eight		0,00067
		0,0054			0,0074
constant	0,2562	0,2534	constant	0,245	0,249
	0,0027	0,0024		0,0049	0,0046
Municipality fixed effect	Yes	Yes	Municipality fixed effect	Yes	Yes
Time fixed effect	Yes	Yes	Time fixed effect	Yes	Yes
Number of observations	339362	285716	Number of observations	205435	

