# The long term effects of early tracking in schools

## A natural experiment in Hungary

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# ABSTRACT

This thesis contributes to the debate about early student selection by investigating the long term effects of the Hungarian tracking policy throughout the 1990's. My question is whether the selection of students is able to fulfil its goal by increasing the efficiency of education and improving the labor market outcomes of students. The thesis exploits the space and time variation of the selective schools' establishment combining administrative data about the former students' employment status, tertiary education rates and occupational choice. I estimate OLS regressions with municipality and time fixed effects, and compare the birth cohorts from 1976 to 1989 in the same municipality before and after the policy implementation. Using event-study methodology allows to exploit the pattern of the outcome variables year by year. According to my empirical findings, the structural change of the Hungarian school system did not have significant average effect on the long run. Both the fixed effects and event-study estimates resulted in zero effects with small standard errors. The zero tertiary education effect suggests that the Hungarian policy could not fulfil its expectations and prepare the selected children better for higher education then the pre-reform system.

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# Introduction

Many countries have implemented policies that aim at abandoning early tracking in recent years (Finland, United Kingdom or Sweden). This means that they stopped selecting children for different classes or schools according to their skills. The goal of tracking's termination was to decrease the differences between children and provide equal opportunities to everyone. However, the supporters of educational tracking say that the selection of children enhances the efficiency of education and the more targeted curriculum can improve the achievement of all children for the better. Hungary is among the supporters of this idea, and implemented educational tracking nationwide throughout the 1990's. Today, Hungary is one of those countries where the correlation between the student's socio-economic status and their mathematical test points is the strongest (OECD, 2013).

This thesis contributes to the debate about the possible effects of early student selection by investigating the effect of the Hungarian tracking policy on employment and higher education rates. This tracking policy changed the pre-reform system where achievement-based selection took place only after the 8$^{th}$ grade of elementary school at age 14. The new system allows the schools to cherry pick the high achiever students earlier, after the 4$^{th}$ grade at age 10 and the 6$^{th}$ grade at age 12. Thus the selected children leave the normal primary schools and continue their studies in the secondary education in 8-year long and 6-year long tracking classes until the end of high school.

I exploit three different data sets are in order to compare the students who had and did not have the opportunity to attend tracking schools. The eligible cohorts are identified by using a unique survey, based on my own data collection, which contains the specific time and place of the tracking schools' establishment. By merging this dataset with the Hungarian Census from 2011 and the Hungarian Harmonized Wage Survey, I get a database with the eligible birth cohorts and

their labor market outcomes. As left hand side variables I use unemployment, employment and tertiary education rates and imputed wages. I estimate OLS regressions with municipality and time fixed effects and compare the birth cohorts of the same municipality before (control group) and after (treatment group) the policy implementation. Using event-study methodology allows me to exploit the pattern of the outcome variables year by year. I find that the Hungarian tracking policy did not alter the labor market variables of the covered birth cohorts, which implies that this kind of student selection does not necessary improve the overall efficiency of education.

My thesis contributes to the literature in multiple ways. First, contrary to previous policy evaluations (Meghir and Palme (2005), Aakvik, Salvanes and Vaage (2010) and Kerr, Pekkarinnen and Uusitalo (2012)) that investigates the effect of school reforms abandoning tracking, I identify the effect of an opposite direction policy, which made the school system more selective. Second, I examine, the establishment of the early selective tracks in a context that did not overlap with other large-scale educational policy changes, which allows for an accurate identification of early educational tracking's impact.

The thesis addresses a gap in the literature by investigating the long term effects of the Hungarian tracking policy. Horn (2013) analyzed the same policy but he evaluated the short-term impact of it on student achievement. He found that tracking schools have higher value-added than general schools, while they have deteriorating effect on the test scores of students left in normal schools. I continue this idea and ask whether the higher value-added of the tracking schools remains by improving the labor market outcomes as well. The findings of Horn and this thesis suggest that tracking is able to improve the short-term school achievement of the selected students on the expense of other children's achievement, but its impact disappears in the long run.

The thesis is structured as follows: I provide an overview of the theory and evidence about educational tracking by presenting the dilemma of tracking versus comprehensive schools and discussing the results of the connecting evaluations. In order to understand the research design

of my thesis I outline the Hungarian tracking school establishment process, especially the time and place variation of it. After reporting the database to be analyzed, I outline the empirical strategy and regressions to be estimated. Then, I present the results of my empirical research and test them with robustness checks. Finally, I conclude the results and discuss further research directions.

# 1. Background

As more and more data became available worldwide higher interest has been generated in the effect of distinct school systems on student achievement. The difference between selective (tracking) and comprehensive schools is in the focus of the policy makers' and the researchers' interest because the theory and empirical findings are vague. In this chapter I will discuss the theoretical background and the empirical evidence on tracking and emphasize at what point will my paper contribute to the existing literature. In the second part of the chapter I outline the implementation of the Hungarian tracking policy and its features.

## 1.1 Educational tracking. Theory and Evidence

Educational tracking, also called streaming and phasing, separates students into different groups according to their skills and abilities on a certain level of education. Theoretically, higher efficiency can be reached in teaching and learning if students with similar skills are taught together. However, tracking is heavily criticized in the last decades because empirical evidence did not support the theory. The selection of the students affect not only the students who are selected but also that one who are left behind and attend the general schools. The possible channels through which tracking is able to influence both groups of children are peer effects, teacher quality or the curriculum. Peer effect refers to the phenomenon when children influence the behavior of each other simply due to the common activities and time that they spend together. Consequently, when students with different skills are taught together they can motivate and inspire each other but when the high-achiever students exit the classes the remaining parts of the students can lose competition, incentive, etc.

While empirical evidence are not very sound about the mechanism, many of them showed that peers matter (Hoxby (2000), Hanushek, Kain, Markman and Rivkin (2003) or Duflo, Dupas,

Kremer (2011)). The most remarkable and clearest evaluation on the effect of tracking was made by Duflo, Dupas and Kremer (2011). In a Kenyan natural experiment they found that all children can benefit from the selection. If the curriculum is rightly adopted to the needs of the weaker students they can gain from tracking as well as the high achiever students who can learn together with the best peers. However, many times the existing school structures do not apply all requirements of tracking so well. For example, in Hungary Varga (2011) investigated the quality of teachers and found that not properly trained teachers are employed in schools mainly where the share of children with lower socio-economic status and worse grades is higher. Due to tracking the schools that collect lower performer students attract low skilled teachers while the tracking schools attract the better teacher. As a consequence, tracking can have a negative effect on the lower performer students while a positive on the higher performer ones. Furthermore, it seems that specialized curriculum does not benefit all students. Hall (2012) examined the effect of a Swedish comprehensive school reform which generalized the curriculum in vocational schools. She found that after a more academic curriculum, which was similar to the higher tracks' curriculum, was introduced in vocational schools the achievement of the students increased significantly. All in all, the effect of early tracking depends on the details of the implementation and it's not obvious at all.

A number of papers analyzed the effect of selection through de-tracking reforms, when countries postponed the selection of children to a later age, thereby, making the school system more comprehensive. These papers analyzed the effect of selection on inequality which is another potential impact of it. Meghir and Palme (2005) examined the effect of the Swedish comprehensive school reform in the 1950s, which postponed the age of selection, increased the compulsory age of school attendance and introduced new curriculum. They estimates the effect of the reform by comparing the students who were affected and who were not affected by the policy due to the different time of the implementation in different regions. 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. However, because the age of tracking was not the only part of the school system which was changed the real reason of the measured effects is not certainly clear. Kerr, Pekkarinen and Uusitalo (2012) used the same feature of the Finnish comprehensive school reform in the 1970s to measure its impact on the development of cognitive skills. They found that the reform was the most beneficial for students, whose parents had basic education or low income. However, the Finish reform also changed the curriculum of the schools therefore the causal relationship is uncertain as well. In my paper, the only part of the school system which was changed is the age when students were selected into more academic tracks. This feature of the reform allows us to identify the actual effect of early tracking. Moreover, I test this effect of an opposite direction policy, when tracking schools were introduced.

Hence lowering the age of selection is a very rare phenomenon, and literature about this kind of changes is 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-non tracking students. He used the National Assessment of Basic Competences data from 2008 and 2010. Comparing the performance

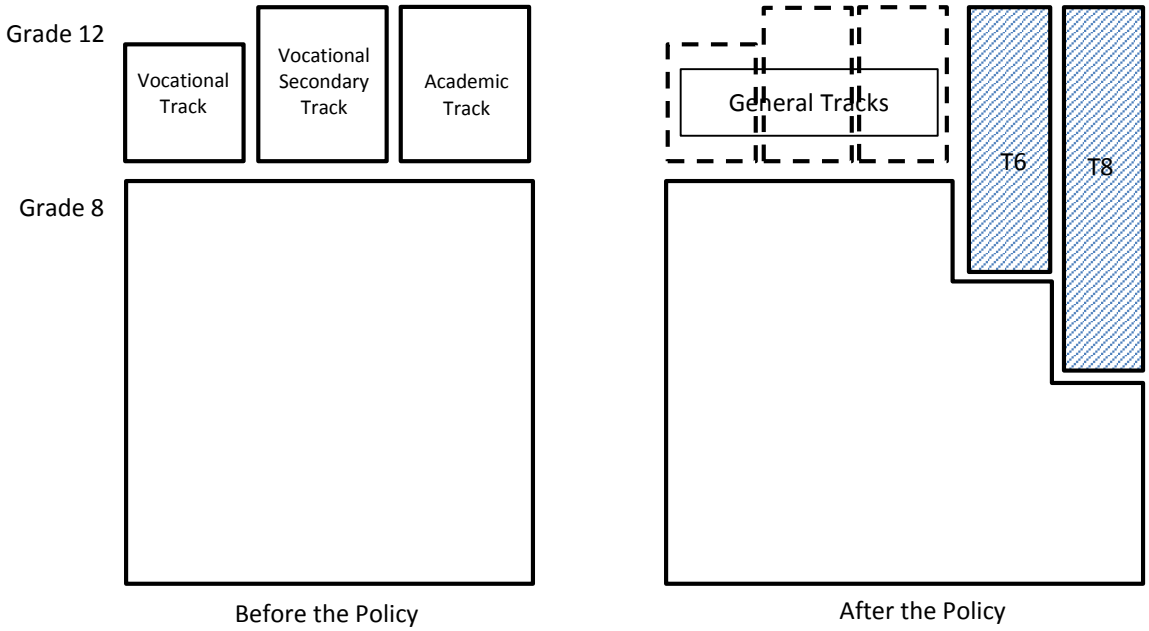
improvement between 2008 and 2010 he finds that selective tracks seems to have higher value added than general academic tracks even if controlling for socio-economic status, previous performance and schools fixed effects. Furthermore, students with better socio-economic status have higher chances to go to the tracking schools even if it is controlled for their previous performance.

However, Horn's results may suffer from endogeneity problems as the children who attend the selective track have systematically different characteristics than those who do not. My paper contributes to Horn (2013) because I identify the effect of tracking schools from the differences between cohorts which attended schools before and after the establishment of tracking schools. My question is whether the advantage in performance remains in 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.

### 1.2 The Hungarian tracking policy

In order to introduce the research design of my thesis this section will outline the structural change of the Hungarian education system during the 1990's due to the tracking policy. After describing the pre-reform school system, I explain the goals of the new, more selective system and the process of its formation. By the end of the decade the foundation of new selective schools were forbidden as a consequence of the new educational policy approach and local conflicts.

**Figure 1. The Hungarian school structure before and after the introduction of tracking policy**




Until the mid-1980's the socialist educational system was highly centralized and uniform in Hungary. The state owned schools adopted a centrally defined curriculum which was uniform all over the country. Children were required to start primary 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 after the 8th grade. On the secondary level students could choose among three 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 school *(szakmunkásképző)*, and provided professional training and practical instruction according to specific occupations as carpenter, mechanic or painter. And there was a third track which combined the features of academic and vocational tracks by ending with schools leaving and providing professional training as well. This kind of school structure can be seen on the left side of Figure 1.

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 with academic track depending on the permission of the Ministry. As part of these plans, two types of early selective tracks appeared in the system. One type of the tracking schools, the8-yr-long tracking schools, selected children at the 5th grade and taught them together for 8 years, until the end of the secondary school (t8). This structural concept was not new in Hungary because it worked before the Second World War. In contrast, the other type of tracking schools, the 6-yr-long tracking schools, was an innovation as it selected students at the 7th grade and taught them together for 6 years (t6). This new structure of the school system can be seen on the right side of Figure 1.

The selective secondary schools aimed to prepare students better for the higher education than general high schools and give them a more detailed and deepened academic knowledge. They launched rigorous submission requirements and exams in order to cream skin the best students. The early selective tracks functioned next to the traditional primary and secondary schools. Those student who were selected by the tracking schools studied the academic curricula for 8 or 6 years instead of the traditional 4 years. The students could decide to leave the general primary school and attend early selective classes from the 5th or 7th grade or stay and go to the general secondary tracks from the 9th grade.

By the time the concept of early selective schools was ready to be implemented, the decision about education got decentralized. As a consequence, the tracking schools were established without central control or supervision. 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. This trend faded out by the late 1990's as the new educational policy trend favored the implementation of common standards instead of the special structures (Balogh, 2001).

The above summary outlines that the impact of educational tracking is quite controversial due to the several channels through which it may influence both groups of students who are and who are not selected for tracking schools. Since the Hungarian education structure turned into a selective from a comprehensive one, it provides a unique opportunity to investigate its effect. The next chapter presents the research design of the thesis and the data to be used.

## 2. Data: Measurement and Sample Selection

The statistical framework developed in this thesis applies to the problem of estimating the labor market variables for those people who could and could have not gone to tracking schools. However, my empirical work assesses the magnitude and pattern of employment and schooling outcomes only for people who lived in such municipalities where tracking schools were operated. I have limited my analysis to these students in order to take advantage of a unique set of data that I collected and contains the exact time and place of tracking schools' establishment. By combining the labor market variables of people whose potential school location can be identified from the 2011 Census and the Hungarian Wage Survey with the school data, I have created a data set that contains employment and schooling of those people who most likely lived in tracking school municipalities. By observing the exact time and place of tracking schools' establishment I am able to separate with relative accuracy those people who could have affected (treatment) and who could have not affected (control) by the students selection.

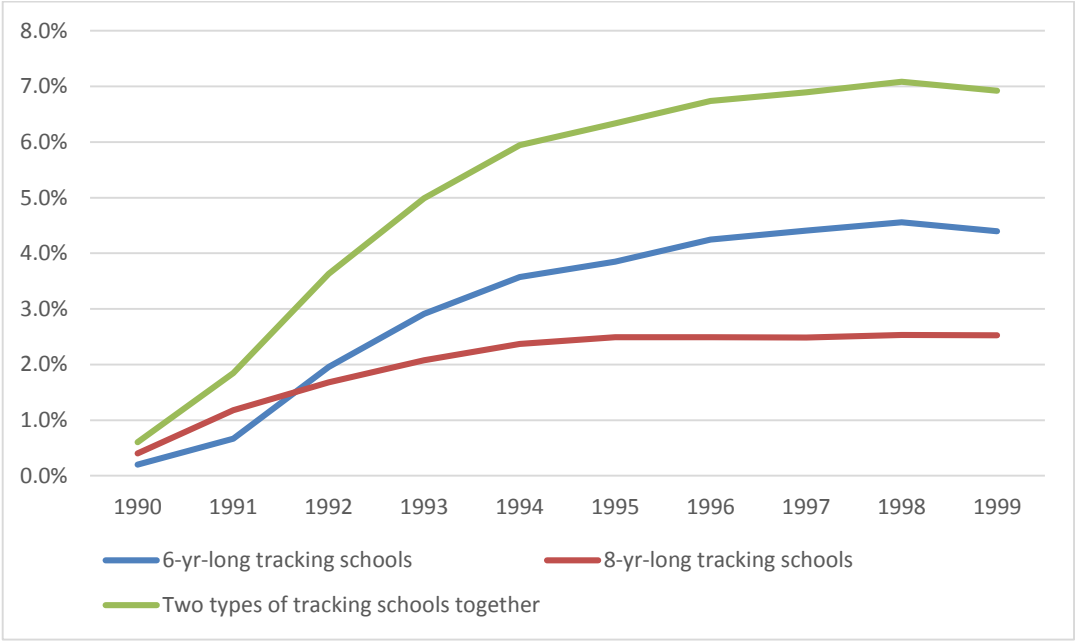
### 2.1 School Survey

Ideally, I could observe the treatment status of the students by knowing what kind of schools people attended. However, the Census data does not contain information about the name or place of the schools attended by the citizens, therefore, I do not know which children went to the tracking schools and which one stayed in the normal primary schools. By knowing where the tracking schools are operated, the treatment can become the existence of a tracking school in the municipality where people most likely went to school. Since tracking does not necessarily influence everyone in the municipality, some classes might have nobody leaving to such school, I am able to measure the intention-to-treat effect. As consequence of not knowing who attended what school the treatment group of the study will be those who lived after the local tracking school was opened.

While the control group will be those people who lived there before the opening. To separate these cohorts we only have to know the exact time and place of the tracking school establishment.

Due to the decentralized implementation of the policy, that I described earlier, the exact time and place of the tracking schools' establishment were not registered by any central database. The Hungarian School Survey registered the tracking classes only from 1997, almost 8 years after the introduction of the first selective classes. In order to get the data necessary for the analysis I conducted an own survey. I surveyed the schools about the accurate time of the tracking classes' establishment on the phone. I also asked them about the continuity of the selection which allows us to observe precisely which cohorts were covered by the policy and which ones were not. Because the official Hungarian School Survey registered only the currently existing tracking classes, I also asked the last time when they had selective classes. Thus I could also document the schools which eased their tracks meanwhile. Finally, I had a database with all schools which ever had selective tracks and the time of their establishment.

**Figure 2. The share of tracking schools among the primary schools throughout the 1990's**




Source: Hungarian Settlement Level Database (TSTAR), School survey

Notes: Table shows only those primary schools which have lower secondary level (grade 5– 8), because children were selected on this level.

Figure 2. shows the tendency of tracking school establishment and the proportion of tracking schools to primary schools. In the first time, mostly 8-year-long tracking schools were established but from 1992 the 6-year-long ones became dominant. The peak time of the opening of new tracking schools was in this year, when 61 new tracking schools were established nationwide. The trend of early selective schools had faded and the number of tracking schools stabilized for 1999. The tendency did not focus on certain areas of the country but it was typical in all parts of the country (Figure 11. in the Appendix).

In order to see the relevance of the reform I try to approximate the population concerned (Figure 2.). Hence I do not have data about the class sizes of tracking schools I use 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. The amount of tracking schools 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.

**2.2 Census 2011**

After identifying the place and time of the tracking schools establishment, I use the 2011 Census to get the data of the birth cohorts who could have been affected by the policy. The time of the policy implementation was between 1989 and 1999, therefore I restricted the sample to the birth cohorts between May 31, 1976 and May 31, 1989[1]. The first 8-year-long tracking schools were established in 1989, consequently the first cohorts influenced by the policy were born after May 31, 1976. The youngest people who are included in the analysis belong to the 1999 cohorts

[1] 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.

because they just entered the labor market in that year when the Census was conducted. Approximately 1.75 million people belongs to the cohorts between 1989 and 1999.

The Census does not register where citizens went to school but it contains data about their residency, therefore I use the relocation of the citizens in order to approximate where they could have gone to school. The Census register three types of information according to the mobility of the citizens': the place of birth, the current living place and the former residential location. My goal was to find the municipality where the people in the sample most likely attended school and determine whether there was a tracking school available in their primary school age. By using relocation data I can approximate in which municipality the people in the sample group went to school. I made three groups of the people. First, I selected those who have never moved from their place of birth (or moved only in their town of birth). Then I selected those who moved to their current location before age 10, because they could likely attend primary school in this place. Finally, I picked out those people who lived in their place of birth until age 18. Supposing that these people can represent the population is not unrealistic, because mobility is a fairly infrequent phenomenon in the life of the Hungarian people (Cseres-Gergely, 2003). They mostly move after the secondary school and the most mobile group is the singles (Illés, 2000). Not knowing where people in the Census went to schools, I am unable to determine the treatment status of the people and the approximation of the school location may cause sample selection problems what will test later.

**Table 1. Characteristics of the different groups in the population by the status of their school location**

| Variables | Identified school location | Not identified school location | Difference |
|---|---|---|---|
| **Average Age** | 28.53 | 29.39 | -0.86 |
| *Std.Err* | *0.0035* | *0.0051* | *0.0064* |
| **Unemployment rate*** | 0.149 | 0.131 | 0.018219 |
| *Std.Err* | *0.00036* | *0.00052* | *0.00065* |
| **Employment Rate** | 0.679 | 0.676 | 0.0026 |
| *Std.Err* | *0.000424* | *0.000636* | *0.00076* |
| **Inactivity Rate** | 0.202 | 0.2214 | -0.0197 |
| *Std.Err* | *0.000424* | *0.000636* | *0.00066* |
| **Tertiary Education Rate** | 0.295 | 0.3916 | -0.096 |
| *Std.Err* | *0.00041* | *0.00066* | *0.00076* |
| N | 1,212,059 | 540,263 | |

Source: Hungarian Census, 2011

*Notes: The unemployment rate is calculated among the active part of the population. This is 967224 people in the identified and 420,865 people in the not identified groups.

In order to see the whether there are any systematic differences between the people who are and who are not involved in the sample of the analysis at that point due to their potential school location, I compared their characteristics in Table 1. above. The two groups have slightly different ages because the people who are excluded from the sample are older than the identified ones. In respect of the unemployment rate larger dissimilarity can be seen as 13.1% of the not identified group is unemployed while the same rate is 14.9% in the identified group. Firstly, this can be the reason of the higher age of the unidentified groups. Secondly, those people who moved during their childhood can belong to the wealthier part of the population. If we take into consideration the active and the inactive people together, the differences in employment rate and inactivity rate are much smaller but significant. Another huge difference can be seen in the participation in tertiary education. Almost 40% of the not identified group attained higher education while this rate is only 30% in the identified groups. The reason of this huge difference can be the same as the reason of lower unemployment rate. These differences can be interpreted regarding the future results as they are limited to those people who were not very mobile during their childhood. In the future, larger districts can be examined as potential school locations

(townships or schools districts). After excluding those people whose school location cannot be identified 1,023,439 people remained in the sample.

Since the Census is in lack of data about the wages I used the Hungarian Harmonized Wage Survey which contains the wages of 100,000-200,000 people based on a yearly survey. Besides the wages, the Hungarian version (FEOR) of the International Standard Classification of Occupation (ISCO) codes are registered on the dataset. "ISCO is a tool for organizing jobs into a clearly defined set of groups according to the tasks and duties undertaken in the job" (ILO, 2015) and can be used to compare the occupational choice of the people. ISCO has 4 digits that shows the employment category and its subcategories respectively, and it has nine main categories altogether

**2.3 Sample Selection**

To construct sample of former students analyzed in this thesis, in the previous part of the thesis I have identified those people whose potential school location can be approximated at the time of the policy implementation. In lack of individual level data about the background characteristics of people, I use municipality level data of the cohorts to select my sample. In this part of the thesis, I restrict my sample to people who most likely lived in municipalities where tracking schools were operated, to avoid complications associated with systematic differences between municipalities that did and did not establish tracking schools.

According to the Hungarian Central Statistical Office's data (2011) in Hungary there are 3154 municipalities. These municipalities can be villages, towns or cities depending on their population and the available services. Regarding this classification, besides villages there are one city (the capital) and 327 towns in Hungary. I leave out from the analysis those settlements which do not have a primary school with lower secondary level. The reason for excluding them is that I cannot approximate where the children who live there go to school because they probably commute to one of the neighboring schools and I cannot approximate the place of school by using

the place of living at that time. I also exclude Budapest and its districts because many of the children living in Budapest leave their place of living in order to attend a preferable school. Table 2. compares the remaining municipalities and separates them into two groups by the existence of tracking school.

Table 2. shows that there are great dissimilarities between the municipalities that did and did not establish tracking schools in their population size, their per capita income tax base and their types of municipalities in 1990. I don't show those municipalities which introduced the student selection in or before 1990, because the tracking schools could have affected their characteristics already. Tracking schools were founded in larger and wealthier municipalities. Moreover, the most frequent type of municipalities is village among the non-tracking settlements and town among the tracking ones. I already showed (Figure 1. appendix) that in space there was no difference in the policy so the establishment of the tracking schools did not restricted only certain parts of the country. However, since the non-tracking municipalities are significantly different in several features from the tracking ones, I exclude them from the analysis.

**Table 2. The characteristics of the municipalities by the existence of tracking schools in 1990**

| Variables | Non-tracking | Tracking | Difference |
|---|---|---|---|
| **Population Size (Mean)** | 2,086 | 27,182 | -25,095 |
| *Std.Err* | *53.48* | *3016* | *793.56* |
| **Per Capita Income Tax Base (Mean)** | 52.47 | 70.91 | -18.44 |
| *Std.Err* | *0.27* | *1.29* | *1.13* |
| **Type of municipalities (Mode)** | Village* | Town | |
| **N** | 1928 | 125 | |

Source: Hungarian Settlement Level Database, School survey

I also compared the tracking municipalities regarding to the time of the tracking school establishment. Table 3. shows us that out of the 125 tracking settlements 67 were established in or before the year 1993 and 58 were after (those municipalities which had tracking school in 1990 are excluded again). 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. Data support this hypothesis because 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 in the equations to be estimated.

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

| | Early adopters | Late adopters | Difference |
|---|---|---|---|
| **Population Size (Mean)** | 35,024 | 18,124 | 16,900 |
| *Std.Err* | *5280* | *1633* | *5878* |
| **Per Capita Income Tax Base (Mean)** | 73.13 | 68.34 | 4.79 |
| *Std.Err* | *1.86* | *1.72* | *2.56* |
| **N** | 67 | 58 | |

Source: Hungarian Settlement Level Database, School survey

Notes: The table shows the 1993 data of the municipalities.

We saw that settlements which have ever and never established tracking schools are systematically different and we also found dissimilarities between the 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. For instance, if the student selection was introduced in 1993 in a certain municipality, the 1992 and 1993 cohorts are different resulting from the existence of the tracking school but similar otherwise.

The final sample of analysis is constructed by merging the School survey, the Census and the Harmonized Wage Survey and has 509,168 observation. It contains 139 Hungarian municipalities where tracking schools were established. The Harmonized Wage Survey is merged to the ISCO codes of the people and not individually by the actual wages. People in the sample of

the analysis most likely lived in these municipalities at the time sphere of the policy implementation, from 1989 to 1999, and could have been selected by the tracking classes or remained in the normal nonselective classes.

Because we know the exact time of the adoption of student selection (the establishment of the first local tracking school) and we can approximate where people went to school, the treatment of the evaluation is the existence of at least one tracking school in the place of living. We have seen that the available data give us municipality level time variation in treatment as opposed to the individual level variation. Therefore, the treatment of this evaluation is living in a municipality at the age of ten or twelve in the year of or after the first tracking school was established. The eligible group lived in tracking municipalities and belongs to the birth cohorts between 1977 and 1989. The treatment group attended school after the establishment of the local tracking school. The control group was too old to be affected by the selection of the student.

After giving the precise definition of the treatment, the treatment and the control groups can be divided. Table 4. shows the size of the remaining sample throughout the years by the treatment and control groups. The first column show the control group, who were too old to attend the local tracking school. 37 % of the sample belongs to this group. The treatment group is divided into three groups regarding the type of tracking school operating in the settlement. In the second column those people can be seen who lived in a municipality where at least one 8-year-long tracking school was available and who were young enough to attend it. 19% of the sample could have affected by 8-year-long tracking schools. In the third column similar group can be seen but they lived in a municipality where at least one 6-year-long tracking schools was available. 29% of the sample belongs to this group. Finally, in the fourth column those municipalities' people can be found where both types of the tracking schools were operated. This group made of 15% of the sample. 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-ling tracking school starting age) when these school

were not founded yet. There is no observation in the third group for the 1977, 1978 and 1979 cohorts because they started the 5th grade earlier than the first 8-year long tracking school was established. Similarly, the 1988 and 1989 cohorts do not contain people who belongs to the 6-year long-tracking school 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**

| | Control | Treatment | | | |
|---|---|---|---|---|---|
| Years | Non-tracking | 8-yr-long Tracking | 6-yr-long Tracking | Both | Total |
| 1977 | 48018 | 0 | 289 | 0 | 48307 |
| 1978 | 42270 | 0 | 3529 | 0 | 45799 |
| 1979 | 31080 | 0 | 12325 | 0 | 43405 |
| 1980 | 16735 | 3071 | 20931 | 1311 | 42048 |
| 1981 | 8886 | 5458 | 19837 | 5763 | 39944 |
| 1982 | 4445 | 7152 | 18132 | 7621 | 37350 |
| 1983 | 2355 | 7435 | 16877 | 8756 | 35423 |
| 1984 | 1314 | 7297 | 14402 | 11319 | 34332 |
| 1985 | 844 | 6870 | 14931 | 12371 | 35016 |
| 1986 | 1175 | 6794 | 14726 | 13791 | 36486 |
| 1987 | 2808 | 7182 | 12675 | 13860 | 36525 |
| 1988 | 15432 | 22097 | 0 | 0 | 37529 |
| 1989 | 15346 | 21652 | 0 | 0 | 36998 |
| Total | 190708 | 95008 | 148654 | 74792 | 509162 |
| Fraction | 0.37 | 0.19 | 0.29 | 0.15 | 1 |

Sources: Census 2011, School survey

Given that, as the School Survey's analysis presented above demonstrated, that there is time and place heterogeneity in the Hungarian tracking schools' establishment, we can exploit this variance in order to compare the people in different time and place. Moreover, the Census provides abundant data in both the treatment and the control group, which may allow us a quite precise estimation on the impact of the policy. The methodology that will be used for this purpose is presented in the next chapter.

# 3. Methodology

To evaluate the impact of the reform, I consider unemployment and employment rates and level of education measured by binary outcomes as well as occupational choice measured by ISCO codes. To test representativeness of the sample I compare the outcome variables between the sample of the analysis and the population of people who were 10 or older throughout the time of the policy. Since I cannot see systematic difference between these groups I specify the equations of the regressions to be estimated. My specification is intended to two exploit two of the principal strength of my data, that it covers a long period of time and contains the data of a huge number of people. I use fixed effects regressions with time and place fixed effects to measure the intention-to-treat effect. By using event-study methodology, I can examine the magnitude and the pattern in the outcome variables year by year.

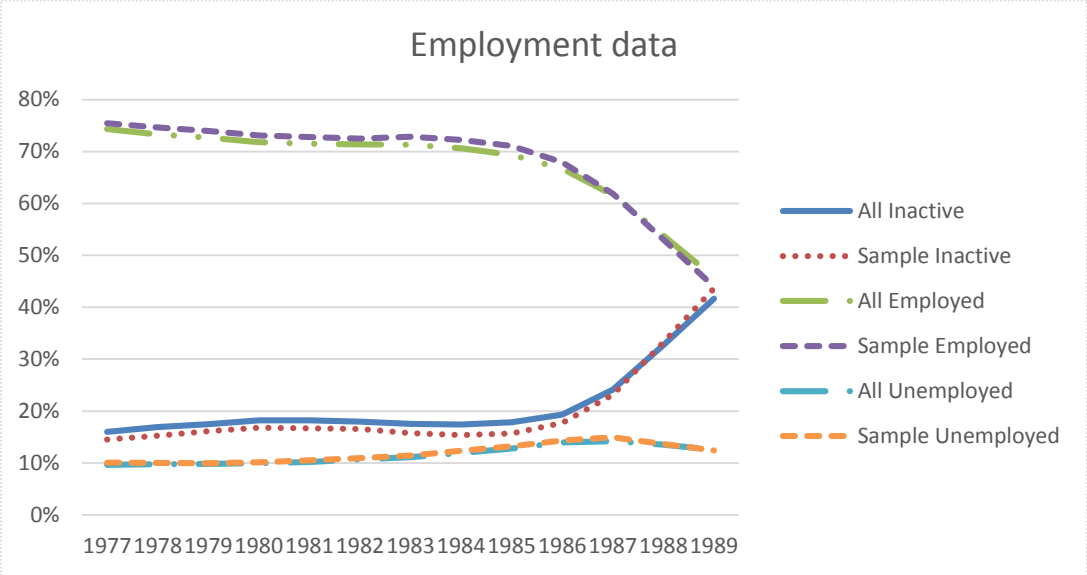
### 3.1 The Outcome Variables

The Hungarian Census register data about the labor force activity, the type of employment and the educational attainment but it does not ask register direct income data. For this reason, first I use the employment and schooling by creating two binary variables for the unemployment rate and employment rate. The unemployment rate is calculated for citizens who actively seeks for job and thus they are part of the labor force. The employment rate takes is calculated for all people in the sample. The dummy variable for unemployment rate is 1 if the individual is unemployed and zero otherwise while dummy for the employment rate is 1 if the individual is in employment and 0 otherwise.

After dropping more than half of the population it is essential to compare the values of the outcomes between the sub-population and the remaining sample to test sample selection bias.

In the sub-population there are 1,752,332 people while the in the sample 509,162. 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 3. Comparing the outcome variables between the population and the sample by cohorts**

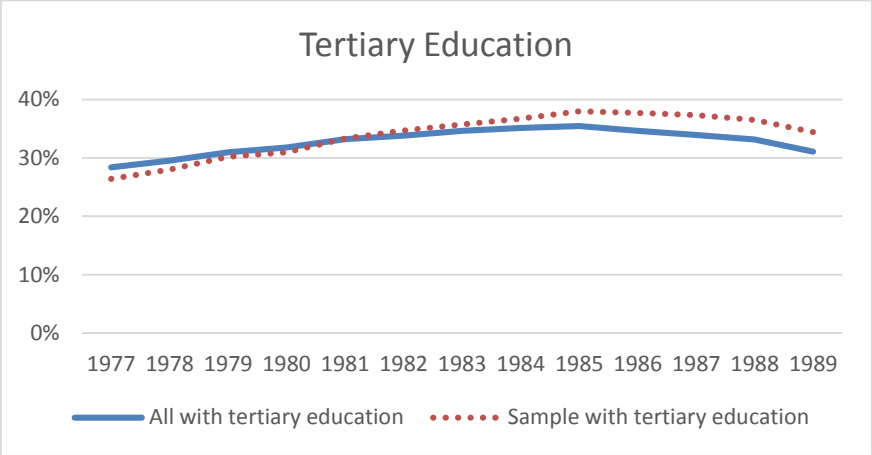



Sources: Census 2011

The original goal of the student selection was to prepare the children better for further education than normal schools. Therefore, I also attempted to measure whether the tracking schools could fulfill this expectation and increase the probability of attending higher education. I created a dummy variable for the participation in tertiary education which has one value if the certain individual spent at least one or more years in the higher education. Figure 3. shows the participation rates in tertiary education between 1976 and 1989. Here we can see very small differences between the sub-population and the sample again which means that we cannot expect for bias as a result of the sample selection. The participation rate in tertiary education is about 30%

for the older cohorts then it started to increase, which follows the trend of the higher education expansion in Hungary. This trend faded out for the end of the 1990s which can be shown in our graph as the share of people with tertiary education start to decrease slightly.

**Figure 4. Comparing the tertiary education rates between the population and the sample by cohorts**




Sources: Census 2011

The Census does not involve data about wages, therefore I used the Hungarian Harmonized Wage Survey for 2011. However, the simple analysis of wages is not possible because the Wage Survey is in lack of data about the place of birth. So I merged the Census and the Wage Survey according to three digits of the ISCO codes, gender and the region of workplace. I can investigate the occupational choice because I know the wages connected to the ISCO codes. After merging the original sample with the Wage Survey 450833 people remained in the new sample. The inactive part of the population did not have ISCO code in the Census consequently they were left out from the sample.

### 3.2. Empirical Strategy

To compare the cohorts before and after the implementation of the student selection locally I used time and municipality fixed effects. The virtue of this strategy is that it can make use of policy reforms implemented in different time periods and places. Because of the gradual implementation of the student selection in Hungary, I can compare the birth cohorts of the same municipality before and after the policy implementation.

I make the following assumption: the unobserved characteristics of the municipalities which can influence the future labor market outcomes of the students who live there are fixed in time. This assumption implies that people who belonged to birth cohorts started the school before and after the first local tracking school 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 municipalities. By controlling for the time and municipality fixed effects, the binary variable for having a tracking school at the time of school start 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. Binary outcome variables are estimated with linear probability models. The linear regression that implements the fixed effects estimator are

$$1.\ y_{icm} = \alpha_m + \beta t6_{cm} + \gamma_c + u_{icm},$$

$$2.\ y_{icm} = \alpha_m + \beta t8_{cm} + \gamma_c + u_{icm},$$

$$3.\ y_{icm} = \alpha_m + \beta t6_{cm} + \delta t8_{cm} + \gamma_c + u_{icm}.$$

Where $y_{icm}$ is the labor market outcome of individual i belonging to cohort c and municipality m. $\alpha_m$ is the municipality fixed effect, $\gamma_c$ is the time fixed effect and $u_{icm}$ is the error term uncorrelated with $t6_{cm}$ and $t8_{cm}$ conditional on the other regressors. In equation 1. variable $t6_{cm}$ was 1 if the individual lived in m municipality where 6-year-long tracking schools were operated and belonged cohorts c old enough to attend it. $T6_{cm}$ had 0 value if no tracking school was operated when the individual was 12 years old. In equation 2. I measured t8 in the same way but it was 1 for 8-year-long tracking schools. By using both t6 and t8, the third equation shows the effect of the tracking schools by their types and together as well. Clustered standard errors are used in the estimations because the error terms are likely to be correlated within the groups (municipalities). Under the assumption described above I can evaluate the impact of the tracking

policy using fixed effects model; that is, I compare the average outcomes between the cohorts for individuals living in the same municipality before and after the tracking schools' establishment.

We might think that the effect of tracking is not homogenous in time but it varies between those who got the first years of the treatment when the new institutions may have been less well operate from later participants who attended more established programs. To uncover this pattern, I use event-study methodology. Jacobson, LaLonde and Sullivan (1993) used the same methodology to measure the earnings loss of displaced workers along time. The advantage of this methodology is that it shows the effect of the policy separately by years. 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:

$$4.\ y_{icm} = \alpha_m + \beta t6_{mc-3} + \beta t6_{mc-2} + \beta t6_{mc-1} + \beta t6_{mc} + \beta t6_{mc+1} + \beta t6_{mc+2} + \beta t6_{mc+3} + \beta t8_{mc-3} + \beta t8_{mc-2} + \beta t8_{mc-1} + \beta t8_{mc} + \beta t8_{mc+1} + \beta t8_{mc+2} + \beta t8_{mc+3} + \gamma_c + u_{icm},$$

where $t6_{mc-3}$ equals 1 if the individual living in municipality m was 12 years old 3 years ago the establishment of the local tracking school. $T8_{mc-3}$ equals 1 if the individual living in municipality m was 10 years old 3 years before the establishment of the local tracking school. The further variables can be interpreted by this scheme. The event window of this study is 7 years long.

In these equations the explanatory variable is the treatment assignment instead of participation because we do not know who attended a tracking class. For this reason, I am able to measure the intention-to-treat effect. As I am measuring a heterogeneous effect, the policy had different effect on those who attended tracking schools then those who were left behind in normal schools, the interpretation of the resulting parameters are not obvious. If the policy had a negative effect on those who were left behind and a positive effect on those who were selected for tracking, that we expect, then I will measure zero effect.

# 4. Results: The Effect of the Hungarian tracking policy

The models developed in the previous chapter defines the difference between the labor market variables of former students who could and couldn't have been influenced by tracking schools. In this chapter, I present the empirical findings of these models by using the data of 139 tracking municipalities and their former or current inhabitants respectively. The results of the fixed effects models are shown in tables by the outcome variables. To highlight the results of the event-study model I plot these estimated effects against the time of the intervention.

### 4.1 Labor Market Outcomes

Table 5. shows the results of the fixed effects OLS regressions measuring the impact of the policy on unemployment rates. Unemployed status is calculated for people who actively sought for job in 2011, hence the sample contains only 408,315 observations. The columns provide the results of the equations presented above (1), (2), (3), respectively. In all equations cohort and municipality fixed effects are used and the standard errors are clustered by the municipalities. The coefficients of the explanatory variables (t7and t8) show the difference in the probability of being unemployed between the people who could and couldn't have been affected by the early student selection.

**Table 5. The effect of tracking on unemployment rate**

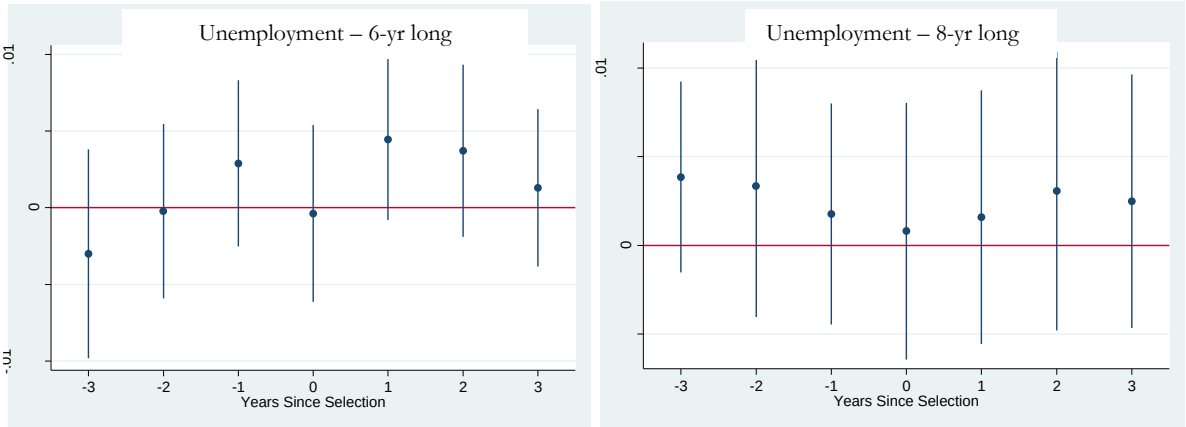
| VARIABLES | (1) | (2) | (3) |
|---|---|---|---|
| t6 | 0.00263 | | 0.00230 |
| | (0.00217) | | (0.00220) |
| t8 | | -0.00284 | -0.00251 |
| | | (0.00236) | (0.00239) |
| Constant | 0.119*** | 0.119*** | 0.119*** |
| | (0.00160) | (0.00157) | (0.00157) |
| Time Fixed Effects | Yes | Yes | Yes |
| Municipality Fixed Effects | Yes | Yes | Yes |
| Observations | 408,315 | 408,315 | 408,315 |
| R-squared | 0.008 | 0.008 | 0.008 |
| Number of municipalities | 139 | 139 | 139 |

Note: Robust standard errors clustered by municipalities in parantheses and ***, ** and * indicate statistical significance at the 1%. 5% and 10% level, respectively. The sample is constructed from the 2011 Census and Shool survey. Binary outcome variables are estimated using linear probability model.

The average unemployment rate of the people who lived in a municipality before the selection is about 11.9%. The

tracking school variables explain very small part of the variance in unemployment since the value of $R^2$ is very low about 0.008. The first column shows the differences between people who were in the 7th grade before and after the selection was introduced. The point estimation is insignificant and very close to zero with small standard errors implying there is no significant effect of the tracking school on the probability of unemployment. The results for the schools which track from grade 5 through 8 years are similar, the point estimation is -0.00284 and insignificant. If the two types of the tracking schools are involved in the equations together, the results remain the same. We cannot see significant effect of the tracking school on the probability of unemployment.

**Figure 5. The effect of the reform on unemployment**




Sources: Census 2011, School survey

The simple fixed effects estimations can show zero effects of the policy because we compare the average unemployment rates of all years before and after the tracking school establishment. The event study estimations can handle this problem by measuring the effects for all years separately. Figure 5. shows the results of the event study estimations. The first figure displays the effects of the 6-year-long tracking schools and the second shows the effect of the 8-year-long tracking schools. The 0 value on the x-axis shows the opening of the local tracking schools and the lines shows the point estimations with the confidence interval. We can see that confidence intervals contain zero values for all the point estimates so we cannot reject the null

hypothesis that the impact of the policy was zero. I tested the joint significance of the point estimations and I cannot reject the null hypothesis at 66.34% significance that all of the coefficients are zero. 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 this level does not have significant effect on the children.

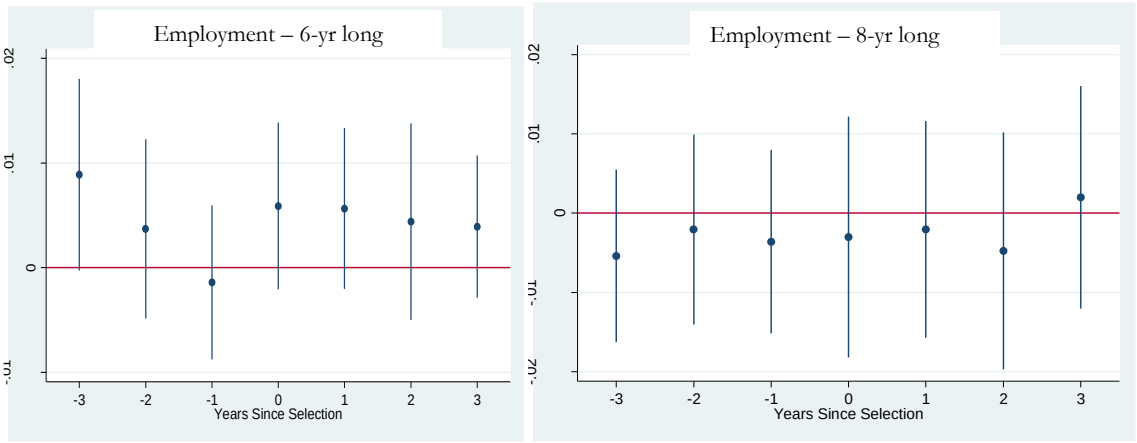
Table 6. shows the results of the fixed effects regressions measuring the impact of the reform on employment rate. The number of observations is 509,168 because all people who were active or inactive on the labor force in 2011 are involved in the sample. The average employment rate of the people who lived in a town before selection was adopted is 75.3 percent. The tracking school variables explain a larger part of the variance of the employment than the unemployment, but the $R^2$ is still quite small 0.038. The point estimates had precise zero values similarly to the estimates of unemployment. Figure 6. describes the results of the event study estimates, which values are around zero and the confidence intervals contains the zero values. The corresponding p-value of the joint significance test was 0.396 therefore I cannot reject the null hypothesis that all of the lag and lead variables are zero. All in all, we cannot see that the effect of the policy become significant as time passes.

**Table 6. The effect of tracking on employment rate**

| Variables | (4) | (5) | (6) |
|---|---|---|---|
| t6 | 0.00496 | | 0.00506 |
| | (0.00400) | | (0.00424) |
| t8 | | 0.000165 | 0.000831 |
| | | (0.00528) | (0.00545) |
| Constant | 0.753*** | 0.753*** | 0.753*** |
| | (0.00278) | (0.00272) | (0.00278) |
| Time Fixed Effects | Yes | Yes | Yes |
| Municipality Fixed Effects | Yes | Yes | Yes |
| Observations | 509,168 | 509,168 | 509,168 |
| R-squared | 0.038 | 0.038 | 0.038 |
| Number of municipalities | 139 | 139 | 139 |

Note: Robust standard errors clustered by municipalities in parantheses and ***, ** and * indicate statistical significance at the 1%. 5% and 10% level, respectively. The sample is constructed from the 2011 Census and the School survey. Binary outcome variables are estimated using linear probability model.

**Figure 6. The effect of the reform on employment**




Sources: Census 2011, School survey

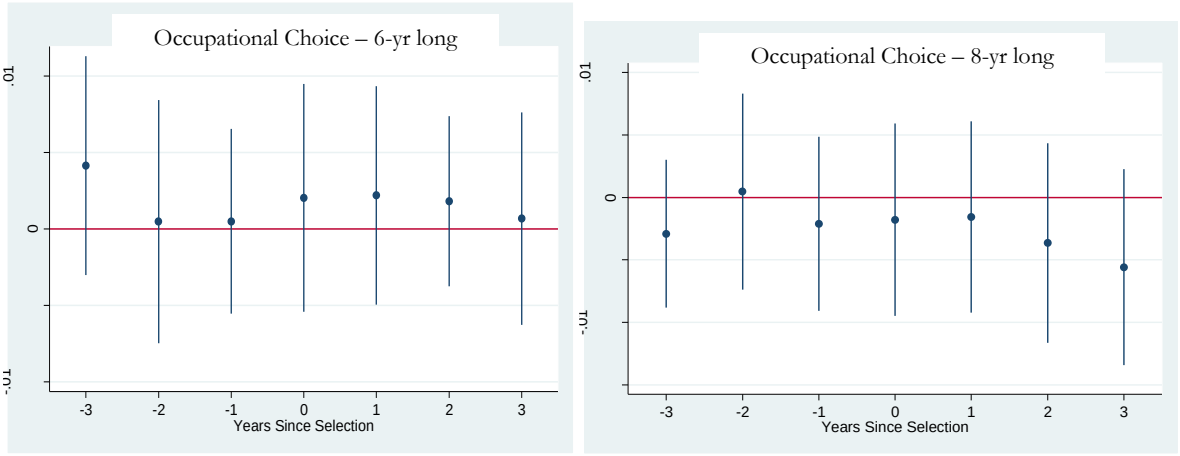
Table 7. shows the impact of the reform on occupational choice. As I have only the average wages according to three digits of the ISCO codes, I cannot observe the actual wages of the individuals. Nevertheless, I can compare whether the treatment group chose occupations which have higher average wages than the control group, in other words their occupational choice. The point estimations have precise zero values again. The coefficients of the event-study estimations do not vary significantly from zero either (Figure 7.) and the p-value of the joint significance test had 0.73 value.

**Table 7. Effect of the Early Tracking on Occupational Choice**

| VARIABLES | (1) | (2) | (3) |
|---|---|---|---|
| t6 | 0.00197 | | 0.00179 |
| | (0.00267) | | (0.00275) |
| t8 | | -0.00156 | -0.00131 |
| | | (0.00332) | (0.00338) |
| Constant | 11.91*** | 11.91*** | 11.91*** |
| | (0.00237) | (0.00235) | (0.00237) |
| Time Fixed Effects | Yes | Yes | Yes |
| Municipality Fixed Effects | Yes | Yes | Yes |
| Observations | 450,833 | 450,833 | 450,833 |
| R-squared | 0.010 | 0.010 | 0.010 |
| Number of slocation | 139 | 139 | 139 |

Note: Robust standard errors clustered by municipalities in parantheses and ***, ** and * indicate statistical significance at the 1%. 5% and 10% level, respectively. The sample is constructed from the 2011 Census, the School survey and the Harmonized Wage Survey. The logarithm of the wages are calculated.

Figure 7. The effect of the reform on occupational choice




Sources: Census 2011, School survey

## 4.2 Schooling

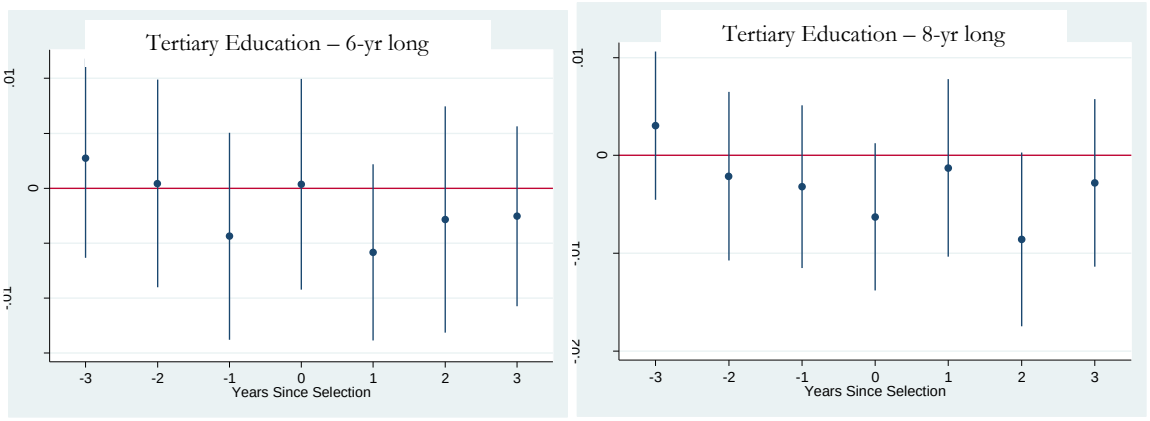
Finally, the tertiary education attainment is examined because the main reason of the tracking school establishment was to prepare students better for the higher education and increase their university submission rate. Table 8. includes the results of fixed effect estimates. The outcome variable had 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 estimations are precise zero again and the event study estimates provided the same results (Figure 8.). These results indicate that as opposed to the initial purpose of the schools' structural change they could not improve their students' chances for further education.

Table 8. The effect of tracking on tertiary education

| Variables | (1) | (2) | (3) |
|---|---|---|---|
| t6 | -0.00101 | | -0.00142 |
| | (0.00320) | | (0.00322) |
| t8 | | -0.00310 | -0.00328 |
| | | (0.00486) | (0.00487) |
| Constant | 0.262*** | 0.262*** | 0.262*** |
| | (0.00252) | (0.00254) | (0.00254) |
| Time Fixed Effects | Yes | Yes | Yes |
| Municipality Fixed Effect | Yes | Yes | Yes |
| Observations | 509,168 | 509,168 | 509,168 |
| R-squared | 0.007 | 0.007 | 0.007 |
| Number of municipalities | 139 | 139 | 139 |

Note: Robust standard errors clustered by municipalities in parantheses and ***, ** and * indicate statistical significance at the 1%. 5% and 10% level, respectively. The sample is constructed from the 2011 Census and the School survey. Binary outcome variables are estimated using linear probability model.

**Figure 8. The effect of the reform on schooling**




Sources: Census 2011, School survey

According to my empirical findings, the Hungarian early student selection may not fulfil its goals and improve the efficiency of education as it has had precise zero impact on the sample of the analysis. In this section I have tested three types of labor market outcomes according to the effect of the early student selection. Both the fixed effects and event-study estimates resulted in zero effects with small standard errors. The zero tertiary education effect suggests that the Hungarian policy could not fulfil its expectations and prepare the selected children better for higher education then the pre-reform system. It is sure that the selection did not have larger effect on one group than another. The zero effect can be resulted from the fact that the selection had significant and identically large effect on both groups. However, the results of the previous literature have never found this pattern on the effect of the student selection. Meghir and Palme (2005) showed that the comprehensive schools reform had large positive effect on the underprivileged and slight negative on the wealthier children. Hall (2012) found no effect on the labor market outcomes. While Duflo, Dupas and Kremer (2011) found positive effects. All things considered, these point estimations can be interpreted in the following way; the structural change of the Hungarian school system did not have significant average effect on the long run.

## 5. Robustness Checks

The fundamental difference between the two types of the tracking schools, the 6-year-long and the 8-year-long, is the length of the program due to the age of the selection. The 8-year-long tracking schools select the children earlier and teach them together on a longer period of time. Therefore, it is possible that the two types of tracks have different effects. Horn (2013) found that the longer track has larger effect on the academic achievement of the students than the shorter one. As robustness checks I investigate the long term effects of the two types of tracking schools separately.

**Table 9. The effect of early tracking in municipalities where 6-yr-long tracking schools were operated**

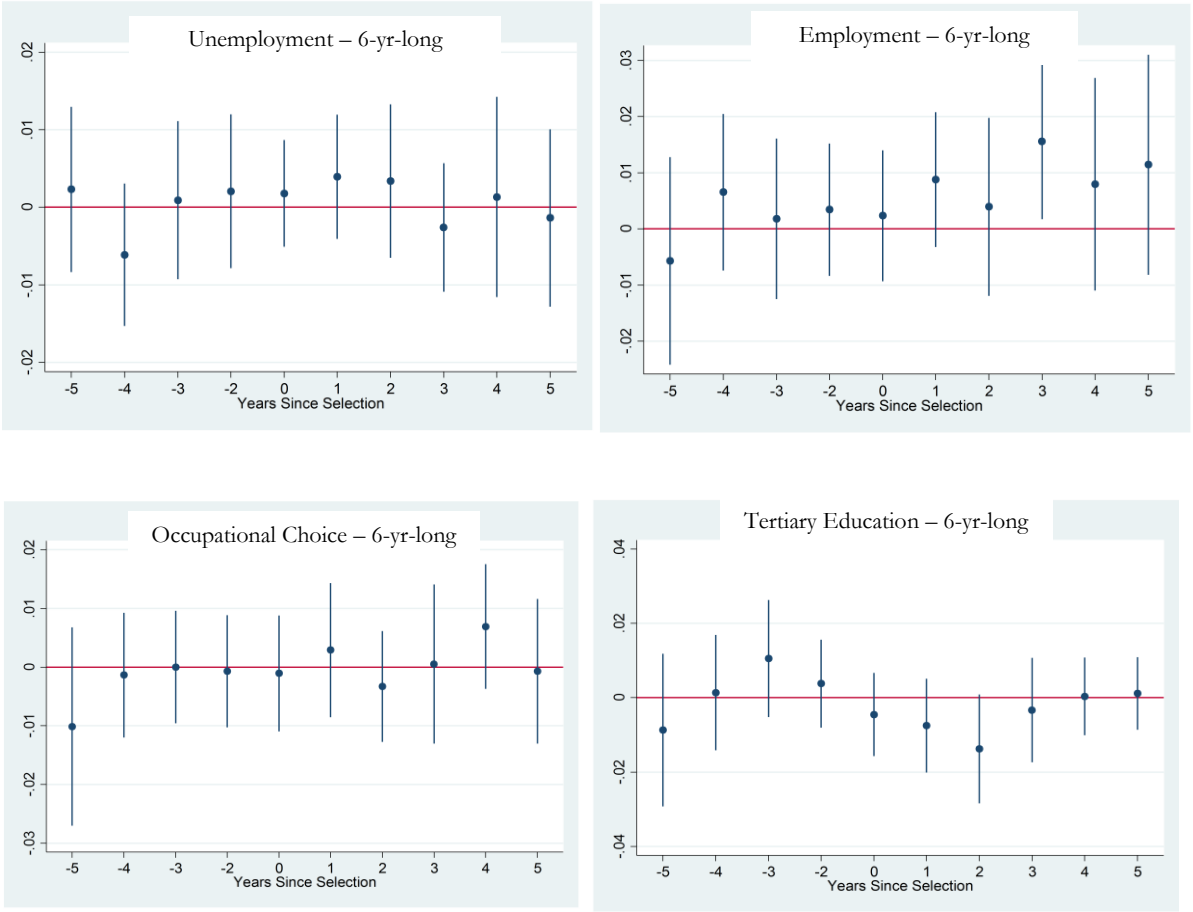
| Variables | Unemployment | Employment | Higher Education | Occupational Choice |
|---|---|---|---|---|
| t6 | 0.000827 | 0.00946 | -0.00797 | 0.00173 |
| | (0.00322) | (0.00574) | (0.00499) | (0.00334) |
| Constant | 0.122*** | 0.745*** | 0.246*** | 11.87*** |
| | (0.00248) | (0.00525) | (0.00491) | (0.00441) |
| Time FE | Yes | Yes | Yes | Yes |
| Municipality FE | Yes | Yes | Yes | Yes |
| Observations | 164,007 | 205,435 | 205,435 | 181,311 |
| R-squared | 0.009 | 0.036 | 0.007 | 0.010 |
| Number of slocation | 78 | 78 | 78 | 78 |

Notes: Robust standard errors clustered by municipalities in parentheses and ***, ** and * indicate statistical significance at the 1%. 5% and 10% level, respectively. The sample is constructed from the 2011 Census, the School survey and the Harmonized Wage Survey. Binary outcome variables are estimated using linear probability model.

First, I selected those municipalities which had only 6-year-long tracking school. There are 78 such municipalities. Then, I run the same fixed effects and event-study regressions on these municipalities respectively. Table 9. includes the results of the fixed effects regressions and the point estimations have zero values again. Figure 9. contains the results of event-study estimates with a eleven years long time window. I increased the time window from seven to eleven years as

another type of robustness checks. I was interested in what will happen with my point estimates if I examine a larger time period before and after the opening of the local tracking schools.

**Figure 9. The effect of tracking in municipalities where only 6-years-long tracking schools were established by using event-study estimates**




Sources: Census 2011, School Survey, Harmonized Wage Survey

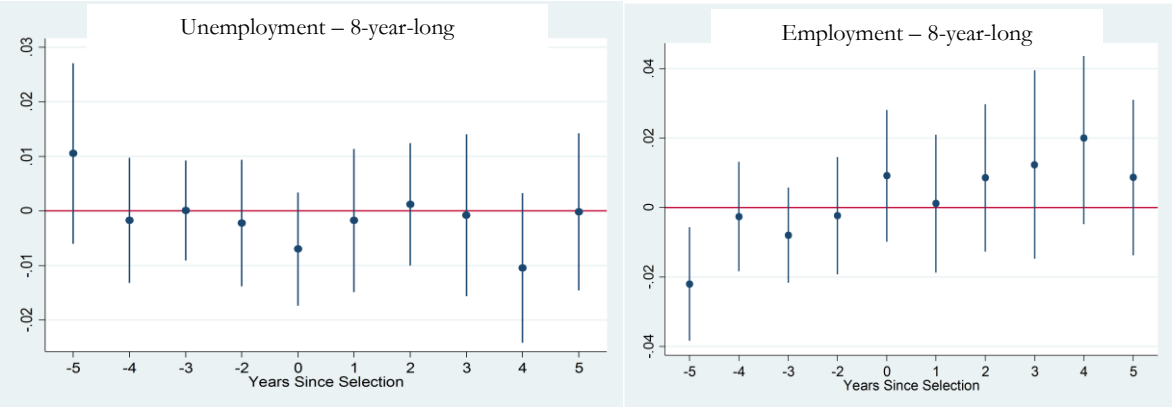
Then, I selected the municipalities which established only those tracking schools which select from grade 5 through 8 years. The results of the fixed effects estimates are in Table 10. Here we cannot see significant effects again. These results show that the zero value are not caused by the heterogeneous effect of the different types of tracking schools.

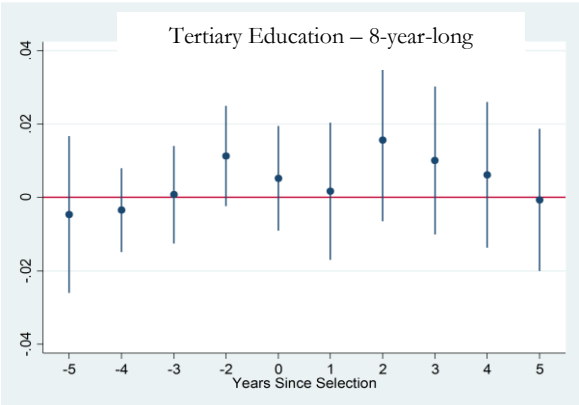
**Table 10. The effect of early tracking in municipalities where 8-yr-long tracking schools were operated**

| Variables | Unemployment | Employment | Higher Education | Occupational Choice |
|---|---|---|---|---|
| t8 | -0.00118 | 0.00595 | 0.000674 | 0.00904 |
| | (0.00401) | (0.00910) | (0.00744) | (0.00742) |
| Constant | 0.116*** | 0.758*** | 0.249*** | 11.93*** |
| | (0.00264) | (0.00346) | (0.00463) | (0.00437) |
| Time FE | Yes | Yes | Yes | Yes |
| Municipality FE | Yes | Yes | Yes | Yes |
| Observations | 77,576 | 95,736 | 95,736 | 85,499 |
| R-squared | 0.007 | 0.033 | 0.007 | 0.009 |
| Number of slocation | 33 | 33 | 33 | 33 |

Notes: Robust standard errors clustered by municipalities in parentheses and ***, ** and * indicate statistical significance at the 1%. 5% and 10% level, respectively. The sample is constructed from the 2011 Census, the School survey and the Harmonized Wage Survey. Binary outcome variables are estimated using linear probability model.

**Figure 10. The effect of tracking in municipalities where only 8-years-long tracking schools were established by using event-study estimates**




Sources: Sources: Census 2011, School Survey, Harmonized Wage Index

# Conclusion

My thesis analyzed the effect of the Hungarian tracking policy which selected the best students into different classes in 139 municipalities. After conducting a survey about the exact time and place of the tracking schools establishment, I had a natural experiment design. By using fixed effects and event-study models, I compared the labor market outcomes of those cohorts which went to school before and after the implementation of the policy. As outcome variables I used the probability of unemployment, employment, participation in higher education and occupational choice. My estimations had precise zero values which indicates that the tracking policy had no long term effects.

These results are interesting in academic and policy aspects, as well. My thesis supports the findings of those policy evaluations which found that abandoning country level tracking has positive or no effect on the long-run. However, it measures the effect of the introduction of tracking and in this sense it is similar to the paper of Duflo, Dupas and Kremer (2011) who found that tracking can be advantageous for all children in a Kenyan experiment. It seems, that the implementation of tracking in developed countries was carried out in a way which cannot provide this beneficial effect of it. It would be interesting to examine the details of the differences through the implementation in order to understand the causes of the contrasting policy effects. Furthermore, my results imply that the tracking schools cannot be the reason of the Hungarian education system's inequality strengthening feature. While I could not separate the children according to their socio-economic status, the precise zero values prove indirectly that the impact of tracking couldn't vary between the different groups of people.

My thesis is only the first step of a more detailed analysis of the tracking schools' long term effect. In the future I am going to test the different groups of the municipalities by the

exposure to the selection because tracking might have higher impact in settlements where more children are selected by the tracking schools (due to the low population in the town or the high number of tracking schools). Furthermore, the effect of the policy on inequality can be measured directly if the variance of the wages is used as the outcome variable. If the variance of the wages is larger after the selection it can be the evidence on the unequal effect of tracking. The extension of this project in this direction would be essential, because inequality is the most important policy issue regarding tracking.

# Appendix

Figure 11. The distribution of tracking schools in space throughout Hungary

Source: TSTAR and School Survey

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